IN SEARCH OF THE TWO-HANDED ECONOMIST

IDEOLOGY, METHODOLOGY AND MARKETING IN ECONOMICS

Craig Freedman

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Palgrave Studies in the History of Economic Thought Series
ISBN 978-1-137-58973-6 ISBN 978-1-137-58974-3 (eBook)
DOI 10.1057/978-1-137-58974-3

Library of Congress Control Number: 2016939603

Printed on acid-free paper

This Palgrave Macmillan imprint is published by Springer Nature
The registered company is Macmillan Publishers Ltd.
The registered company address is: The Campus, 4 Crinan Street, London, N1 9XW, United Kingdom

For Emily and Nicola, my two beloved terrors.
And for both Peter Kriesler and Geoff Harcourt, who have kindly read too many of my questionable papers over the years.

The Inevitable Preface

In Search of the Two-Handed Economist: Ideological Biases, Methodological Pitfalls and Marketing Initiatives in Economics[1]

Writing a preface is perhaps a secret and largely unacknowledged pleasure unintentionally bestowed upon an academic author, or at least this one. Buttressed by the knowledge that few, if any, readers do more than skip over the words composing such efforts, a writer is left to his or her own devices. These prefatory words are a realm where almost anything goes. The freedom accorded is heady, to say the least.

In any case, for those careful or obsessive readers, I intend to use this limited space to quickly sketch out the direction and purpose of this volume. What follows are meditations or extended essays that explore a central theme. They of course fall short of the Meditations offered by a supreme essayist like Montaigne.[2] However, so very few authors ever manage to scale those heights that I am not embarrassed by presenting

[1] This title represents the original and more descriptive sense of the volume. However, book titles (even subtitles) should not be unnecessarily long, becoming an unwelcome challenge to the potential reader.

[2] The essays of Michel de Montaigne were first published in 1580. Few, if any, since then have been able to match the style, wit or precision displayed by his writing. Even a humble economist would be well advised to enjoy the bounties offered by these efforts.

my own efforts, even if I fail to reach Montaigne's level of pithiness in each one of my chapters.

My concerns lie with the operation of the marketplace for economic ideas, in particular the way in which they might be shaped by the forces of marketing, ideology and methodology, a trio that, though highly interdependent, seldom find themselves as the focus of any intensive investigative spotlight. To explore this topic, the type of "love that dare not speak its name"[3] that exists at least in the field of economics, the reader must first choose between two possible understandings of such markets. The first is perhaps the way in which most economists would prefer such a marketplace to operate: the understanding articulated by Edwin Canaan (1903) and subscribed to by luminaries such as George Stigler (1985:174).

> However lucky Error may be for a time, Truth keeps the bank and wins in the long run. (Cannan 1903:392)

Were this the case, then questions concerning the ethics, ideology and assorted marketing ploys beloved of economists would largely be beside the point. If the truth must out no matter what obstacles are presented, then the motivations behind an economist's output, though interesting, are simply an entertaining sideshow or even a distraction. Unfortunately, evidence for such a cheerful and encouraging assumption seems somewhat lacking. For those who are more attracted to a harder-nosed and perhaps more worldly view, Gary Becker's formulation may seem a bit more convincing.[4]

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[3] The phrase is due to Lord Alfred Douglas (1894), though uttered in a somewhat different context. These words were later used at Oscar Wilde's indecency trial. Wilde himself had his insufficiently noted, but significant, moments of economic insight: "Nowadays people know the price of everything and the value of nothing" (Wilde 1890: Chap. 3).

[4] Becker formulated this viewpoint in a conversation with the author. The discussion at this point centered on the joint work of Gary Becker and his friend and colleague, George Stigler. Following the Chicago view ("a market is a market is a market"), a market for ideas must operate in accordance with those ideas constructed in the seminal Becker/Stigler article (1977). In which case, the evolutionary result of such a model provides a match between a given environment and a specific product without any guarantee that such products are optimal in any absolute sense of the word. The marketplace for ideas then reflects the *Fox News* objective, "all the news that fits" rather than the more genteel goal extolled by *The New York Times*, "all the news that is fit to print."

> It may be that in the long run good ideas do surface but they surface faster, if written in a persuasive fashion. Moreover, bad ideas may be put persuasively. And they may gain the necessary threshold. However, taking that same analogy in competition among ideas, there is a presumption, although not a certainty, that in the longer run, the good ideas are going to compete out the bad ideas. But that may take a long time and may not even always operate. There's nothing necessary about that. Nothing guaranteed about that. (Conversation with Gary Becker, November 1997)

The question explored in the following chapters, then, is whether in the type of marketplace described by Becker, questions of marketing, ideology and methodology come into their own. This potential stumbling block remains an open issue even after the exploration provided by this volume. Marketing and ideological slants are certainly evident in the profession, but whether examining their impact is only a fascinating digression, even in this imperfect world in which we live, remains an open question. Articles whose methodology is heavily moulded by marketing and ideological concerns may nevertheless be interesting and provide useful contributions to the profession. Even the strategy of assuming one's conclusion at the commencement of a given article may still contribute thought experiments that provoke fruitful debate.

To some degree, then, the Chicago School of Economics,[5] manned by the likes of Milton Friedman and George Stigler, is sometimes forcibly drafted into the examinations performed within this volume, not to cast scorn on its enviable output (or the inevitability of its conclusions), but because within the profession itself, the School's ideological constraints have been well documented. Perhaps even more to the point, marketing, a word shied away from by economists with dreams of becoming a sparkling version of the cinematic scientist (smartly draped in a white lab coat), has been a mainstay of the School's work. Friedman and Stigler themselves were extraordinary marketers, as has been the case for many

[5] The Chicago School of Economics is used as a shorthand that incorporates the economists assigned to the Business School (and Law School) as well. Such terminology might be extended to encompass many other economists who explored similar lines of research and modes of analysis. A number of members of the UCLA department come to mind here.

other faculty members associated with the Chicago mindset.[6] The focus on the Chicago School is thus more of an instance of self-selection than anything resembling an intentional attempt at targeting. No other group of noted academics are capable of providing such strong examples, or even case studies, within the constraints of my own analytical framework.

Methodology, at least in the practical sense of the word, conforms to specific marketing and ideological objectives.[7] Even if we accept that the methods employed by economists may become warped by ideological imperatives that are wrapped in marketing glitter, their value may still remain relatively undiminished even given such manipulative strategies. The idea that economists should carefully avoid one-handed approaches, striving instead for transparency and openness in their presentations, may be no more than the reflection of individual subjective preferences. In which case, both ideology and marketing should be simply recognized for what they represent, rather than attracting any summary disapproval. The author of this volume, for example, may regard, with considerable distaste, the practise of constructing an article by employing 'reverse engineering' without being able to reject conclusively the result achieved by employing such methods.[8]

Accordingly each chapter, though self-contained, is arranged to come at the same set of problems, but from somewhat different directions. While realizing that there is no way to enforce a request as to the order in which the chapters are read, the volume has actually been carefully structured and ordered in a deliberate sequence to best raise issues and provoke discussion, rather than attempting to provide any authoritative conclusions.[9]

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[6] This skill at selling ideas may quite naturally be due to the quality of the theories on offer. But while quality may contribute to any successful marketing campaign, the ability to persuade is undoubtedly a necessary factor as well.

[7] Throughout the volume I will shy away from the prescriptive flavour of methodology. This approach has made me nervous ever since I was subjected to the stylization produced in Descartes' "Rules for the Direction of the Mind." Prescriptive methodology seems to breathe an air of near-suffocating smugness. It is contemporaneous with a stealthy infusion of ideological concerns driven by an unnerving display of clever marketing. Thus Friedman, arguably, closed off future methodological discussions (or sought to do so), especially focussing on those antithetical to Chicago price theory, by skilfully marketing his own brand of methodological reasoning.

[8] Reverse engineering starts with a desired conclusion and then works backwards in order to discover the assumptions which are required to reach such an inevitable end.

[9] The temptation is to skip randomly through the chapters according to whim and inclination. Nothing can stop readers from doing so. The ordering, however, is essential and should not be lightly dismissed.

By undertaking this uncertain journey, with at times a certain degree of diffidence, this volume hopes to stay true to the exemplar represented by the two-handed economist.

Sydney, Australia
Craig Freedman

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A Random Walk Through Economics: Notes from a Curiously Spent Career in the Profession

> If that's what the evidence is, that is what it is and we have to, we *have* to, you know at the end of the day, if we are convinced that that is what the evidence is, that is the truth. Not our pride. Our pride is not the truth. (Sam Peltzman, conversation with Craig Freedman, October 1997)

The invisible hand may sometimes appear to be absent without leave from the topology defined by the marketplace. But it has played an often unacknowledged role in my own stroll through the thickets of economics. Much of what I've attempted to accomplish turns out to be almost unintentionally connected, perhaps tied together at some more subliminal level. The result of these unplanned rambles has been collections much like the one now eagerly awaiting your less than meticulous scrutiny. These individual chapters have come about for a variety of often obscure reasons over recent years. The piece inspired by Richard Bronk's book, for example, is the result of a simple request for a written review (though hardly for the convoluted meditation that appears here). In a similar cross-purpose manner, the overview of Chicago-style economics branched off from an early draft of a paper written with David Colander, an interesting article that is still in search of a home. The joint work soon took a somewhat different direction and the draft was left waiting for some future gentle resuscitation (and left in that moribund state for a considerable period). The novella delineating George Stigler's winter campaign against the ravages brought forth by

Chamberlin's vision of the economy was instigated by a request to present something at a very enjoyable History of Thought workshop held at the University of Richmond. (The length of the paper eventually presented an almost comical challenge to other workshop participants.) Nor is it possible to discover some carefully premeditated scheme behind the accumulated notes that resulted from employing two classic, but completely contradictory papers, to serve as vehicles to hone critical reading skills in upper-level industrial organization classes.

Almost alarmingly, as though driven by some unknown elective affinities, all of these papers seem to adhere to one another in distinctly different, but clearly connected, ways even when dealing with quite distinguishable issues. Though methodology has long inhabited the backwaters of the profession, the pervasiveness of marketing and the scourge of ideology in moulding methodological approaches remains something of a badly kept secret. The essays found in this volume raise the issue of marketing and ideology without in any way resolving it. For an economist like George Stigler, this complication, winding its way through the subject matter like a red thread inextricably linking all economic analysis, posed no problem. For him, such considerations existed as nothing better than fabricated distractions, side shows for the feeble-minded. Competition within the marketplace for ideas would act as a perfect scourge, eliminating over time all ideas and pseudo theories that fell short of the truth. This invisible hand compelling one economic model to vie with another would eradicate any serious concerns focused on the problems presented by marketing distortions or ideological biases. To conclusively refute this assertion remains surprisingly difficult. Unfortunately, evidence that might ultimately resolve this matter still eludes our grasp (or at least my feeble one).

Sydney, Australia
Craig Freedman
November 2015

Marketing Truth: Why We May Get What We Deserve, Rather Than What We Want

> To market, to market to buy a fine pig;
> Home again, home again, jiggety jig. (Traditional nursery rhyme)

If I were to try for something approaching a strict truth, I would be forced to admit that I simply adore writing prefaces and other pieces of prefatory writing. Unlike an introduction, which some obsessive souls may feel obligated to read, no one in their right mind reads a preface. Maybe an author's mother, moved by the spirit of altruistic love, might be so tempted, but the list of potential readers diminishes at an alarming rate when pinpointing those who will read whatever you scribble no matter how woeful. What makes preface writing so alluring is the sheer freedom attached to doing so. An author can essentially write whatever comes to mind without fearing any noticeable backlash from easily offended readers, those desperately in search of a reason to be so. This absence of constraint provides me with the opportunity to just ramble for a while.

Back in 1997, during a conversation with Gary Becker, we wandered into an interesting discussion concerning the market for ideas. Having recently read Ronald Coase's (1995:68) insistence that the market for ideas was in no way significantly different from any other functioning market, I was almost naturally compelled to question Becker as to whether the ideas expressed in his classic co-authored article (1977) either could or should be extended to the realm of ideas. What interested me was that the logic of the article dictated an inescapable conclusion. Markets at their best deliver a classic variation on the *The New York Times*' well-known tag: it is not all the news that's fit to print that succeeds in the marketplace, but rather all the news that fits. Thus ideas that succeed in a given market are not necessarily those which are optimal or even in some sense true.[1] Markets do not select for any such characteristic. Rather, success comes to those ideas that can best be marketed and made to appeal to the economics profession as a whole, the potential targeted consumers. As Becker honestly admitted by extending his basic model, we might hope that over time we approach something resembling this ideal goal, but there remains absolutely no guarantee of that result. Given the importance of this issue, we need to realize that market forces do not necessarily eradicate the bogus or the ideologically stimulated construction. Given recent insistence on disclosing any potential conflicts of interest when presenting a piece of research, on allowing ethical considerations to creep into the fabric of the profession, the very nature of the markets in which economic ideas are bandied about needs to be reconsidered. This book, then, perhaps foolishly tries to make an initial attempt, at least in a preliminary way, to explore these issues, even if only peripherally.

Whether such conflicts of interest are simply a diversion rather than a significant issue notable largely for a failure to properly grapple with the inherent issues has yet to be definitively discussed. The profession itself knows that many of its practitioners, even those of the highest repute, are in fact classic one-handed economists. But whether this poses a significant problem is not transparently obvious. There may in fact be nothing even approaching close to ethical or professional transgressions in constructing an argument with a foregone and preconceived conclusion. In which case, any serious search for those elusive two-handed economists may be reduced to a simple waste of time. As in most things, when judging this matter we need to back our preferences with more than mere emotional biases. The following essays go some way towards shedding something more than a feeble light on this particular issue.

[1] The political marketplace seems determined to insist upon this unwelcome fact. If anything, the art of making outrageous and at times shameful assertions all too often bring rewards to these ambitious malefactors.

As one of the few serious preface readers, I know that this is an appropriate place for acknowledging all the people who have assisted me in producing this unasked-for volume. On previous occasions I have been excoriated for not doing so with a sufficient degree of enthusiasm. Accordingly, I note the following. Clearly this book could not have been written without the generous help of all of those I interviewed in connection with my research on George Stigler. For that matter, it would have been difficult to be inspired by and to write a lengthy review of *The Romantic Economist* had it never been written. Nor would talking about the Chicago School of Economics make much sense had not John D. Rockefeller contributed the funds to establish the University of Chicago. Clearly acknowledgements can continue and extend interminably. But quite reasonably, friends who have helped and those others who have been less than a help know who they are. By enumerating them all, I would inevitably be guilty of overlooking someone. Although I am sorely tempted to generously share the blame for any, if not all, of my various errors and mistakes, most are probably due to my own stubborn insistence on trying to protect too many of my own misguided views. But I do wish to thank in advance each and every reader who proves patient enough to make his or her way through this volume.

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Contents

Elephant Stalkers: Fixed Perspectives and Required Results[1]

The dog wags the tail when we compare direction of causal influence from the lay public to academic economists' ideology—despite Keynes's assertion that men in authority are merely slaves of past zealots of the chair. (In my observations princes always get the advisers they deserve, as they select from a varied menu of flat-earthers. President Truman was dead wrong: It is the prince as much as the brain trusters who needs two arms. In our guild, one-armed economists divide between those with only a right-arm or those with a left arm only; and then the wise prince will have a need for the eclectic two-armer to adjudicate between them.

[1] The title refers to a well-known poem, at least well known to all those baby boomers who were obliged to memorize and recite it in primary school. "The Blind Men and the Elephant" by John Godfrey Saxe describes how six blind men jump to conclusions based on limited knowledge, namely the precise part of the elephant each one perceives.

And so these men of Indostan
Disputed loud and long
Each in his own opinion
Exceeding stiff and strong
Though each was partly in the right
And all were in the wrong! (Saxe 2014)

C. Freedman, *In Search of the Two-Handed Economist*,
DOI 10.1057/978-1-137-58974-3_1

> "One man with the truth is a majority." That is not a wise truth. It is an expensive joke.) (Samuelson 2011: 428)[2]

Economics is what economists do.[3] A definition so closely resembling a tautology can be easily dismissed. Yet there is a gem of insight lurking in this claim that needs further investigation. In essence, the statement reflects the fact that few economists are self-reflective. They have more urgent objectives at hand. Such professionals are far more concerned with conducting research than with distancing themselves sufficiently to ponder deeply on the way they proceed with their work and whether their operational algorithm might contaminate their eventual results. Despite this common practice of affording such introspective examination a deservedly benign neglect, methodology remains important, though not in the irritatingly prescriptive sense adopted by Descartes or, in the case of economics, fashioned by Milton Friedman and his close ally George Stigler.[4]

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[2] This sad indictment of the economics profession, though hardly unique to it, originated in a speech delivered by Paul Samuelson in honor of Robert Heilbroner at the New School for Social Research, New York, November 12, 1998. Subsequently, the article appeared in *Challenge* (1999) 42(5): 29–33. The speech itself warned of the consequences when, in Samuelson's words, "the failed god of Marxian socialism gets replaced by the ancient god of libertarian laissez-faire" (Samuelson 2011:426). In the same article Samuelson also looks back on the folly of left-wing economists.

> As Keynes said about another system, the market system, in its moment of worst 1930s pathology: *It is not aesthetic and it does not deliver the goods*. Exactly that held for pre- and post-Stalin Soviet Union, Maoist Cultural Revolutions, Tito's Yugoslavia, North Korea, Albania, Cuba, and all the utopias that transiently enchanted so intelligent and humane an expert as the late Joan Robinson. (Samuelson 2011: 427)

[3] This phrase has been attributed to Jacob Viner. The same wording was also employed by Kenneth Boulding in his breakthrough, but subsequently ignored, textbook (1941:3). Boulding did spend 1932–3 on a Commonwealth Fellowship at Chicago choosing Viner as his adviser, mentioned in passing (not without the requisite sardonic, offhand remark) by George Stigler in his bare-bones autobiography.

> Kenneth Boulding came to Chicago from England as a Commonwealth Fellow. (Commonwealth Fellows usually spent some of their time, and perhaps more of their fellowship grants, speculating on the English pound.) He was both clever and so brave as to engage in disputes with Frank Knight about the nature of capital. (Stigler 1988:25)

Whether Boulding was simply repeating a phrase uttered by Viner that caught his fancy must remain a mystery. Both have long since ceased to communicate on such matters.

[4] Descartes (1961) in 1623 set down a didactic series of steps, a veritable pathway to discovering the truth (*Rules for the Direction of the Mind*). Milton Friedman (1953) became famous, if not notorious, for attempting to squelch further methodological discussion in economics with his much-cited critical essay. Friedman's thinking was in part shaped by his discussions with George Stigler. An early version of Friedman's guidelines appears in Stigler's (1949) attack on monopolistic competition.

> When I was a graduate student we were taught a paradigm of how you do research. I've got to tell you it's all wrong. It's not the way we operate. We don't sit up here and develop hypotheses and go out and test them. That's just not what we do. George taught me that. Milton taught me that. They're wrong! And I understand that. I'm older enough now to figure out that's not the way we do work. There's a lot of salesmanship, there's a lot of taking positions, defending them. Right. The facts will win out. I'm not saying that we're not in that sense correct. The facts do win out. But the process by which that happens is not the clean one of scientific method rigorously applied all the time. (Conversation with Sam Peltzman, October 1997)

The methods employed by the ranks of economists are far from pure, often running along practical lines much to the advantage of the discipline itself. Unfortunately, the habit of maintaining a largely unacknowledged methodology provides an unconscious opening for such methods to be at least partially shaped by the bias of ideology and the demands of marketing. Researchers can take comfort of course in casually dismissing such claimed distortions in as cavalier a fashion as they wish.[5] This particular attitude was not always dominant, but owes more to the post-war period, a transformative stretch for the profession.[6]

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[5] The presence of ideology can be detected by a simple litmus test. In the case of George Stigler, this volume's author employed a very simple-minded method. He posed a seemingly unsophisticated question to those who knew and worked with this eminent economist: "What evidence would succeed in convincing George Stigler that markets were not efficient?" (George Stigler had been an early proponent and vigorous crusader for empirical testing of economic propositions.) No one was able to imagine the shape or content of such evidence. For ideologues, whether politically leaning to the left or right, all data, whatever shape or form that data might take, serves consistently to bolster their unyielding convictions.

[6] Gary Becker firmly believed that in turning away from Frank Knight, George Stigler was ultimately more influenced in terms of his conceptualization of price theory by his other teacher, Jacob Viner. However on the issue of ideology, Stigler manages to substantially part company with both his teachers.

> Yes, I'm saying he becomes more like Viner. Look at Viner, early on, in Viner's own dissertation on international trade, he is already testing the Canadian and American data. George became a big, empirical testing guy. As with Friedman, Viner was the greater influence. Not Knight. (Conversation with Gary Becker, October 1997)

Stigler continued to believe that quantification and testing had allowed the profession to rise above mere assertion. Moreover, the motivation for a theory seemed irrelevant to him. What counted was the test of time, the competitive battle to accumulate evidence one way or another.

> As I said the other day, he built up evidence and other people contributed their evidence and when you have a *mountain* of evidence you have more confidence in a theory. And if you don't have a mountain, forget about any conclusive test. George was adding something to that pile (or starting a new one). (Conversation with Claire Friedland, October 1997)

> In so far as possible, value judgements should be labelled as such, but their systematic exclusion is, I am convinced, not in practice either possible or desirable. They should not be concealed, they should not be eccentric, and they should not be elaborated or didactically pressed. (Jacob Viner quoted in Van Horn 2011: 291ftn. 41)

George Stigler's strong denial of any role for ideology seems to have arisen from his inherent aversion to employing an explanatory factor that appeared too slippery to quantify. If the rational choice model promoted in tandem with his close friend, Gary Becker, was to be the key analytical measure, clearly driving individual decision making, then according serious consideration to ideological concerns could only be viewed as an unwelcome and disruptive intrusion.[7] For Stigler, such models did not represent a mere tool of investigative convenience, but an accurate representation of observable economic behavior. It can be argued that by adopting this uncompromising approach, Stigler had more than edged away from the methodological position he had helped develop with his compatriot, Milton Friedman, in the immediate post-war period. Certainly his strong concurrence with Becker's expansionary project, one that sought to embrace other disciplines, would seem to indicate as much.[8] Stigler nourished an unwavering stance that ceded no ground to any of his numerous critics, preferring instead to view the rational choice

[7] Becker, supported strongly by Stigler, believed a rational choice approach to be applicable to any observed decision-making process. That all-inclusive quality was exactly what both found to be particularly appealing, when applying this particular mode of analysis. The method was universal because it was:

> Applicable to all human behavior, be it behavior involving money prices or imputed shadow prices, repeated or infrequent decisions, large or minor decisions, emotional or mechanical ends, rich or poor persons, patients or therapists, businessmen or politicians, teachers or students. (Gary Becker quoted in Medema 2011:162)

[8] Medema (2011) is useful in considering this type of refocusing on Stigler's part. In a sense, his original approach to methodology was not spurned, nor was an alternative substituted. Rather, the issue of methodology became largely irrelevant in later years for Stigler. However, one can only suspect that like Friedman (in a 50th-anniversary ASSA session celebrating his essay) Stigler would have still adhered to the same methodological position if pressed.

model as an insightful blueprint revealing concrete human decision-making processes.[9]

> The paper is technically sound, ingenious, clever, the work of a born economist. But I don't believe it has taught me a thing about marriage. … Why? Because, I think, the model of the marriage market is way off in at least two respects. First, the motivations of the participants are quite different from the maximization of Z or of something related. Second, because the structure of the mating process is far more complicated in institutional terms than that postulated in the paper. … The usual response to this kind of criticism is to say that it is enough if the data behave "as if" individual motivations were as postulated and if the marriage market did the same. In some contexts, e.g., profit maximization, there is a powerful additional argument, that in a competitive world profit-maximizing firms will survive better than others, so in time will displace others. That reason seems lacking here. (Letter from Robert Solow to George Stigler concerning Solow's referee report on Becker's "A Theory of Marriage: Part I", November 13, 1972)

Given the general stance of the profession, according any reputable degree of credence to such a self-critical focus, by taking the influence of ideology seriously, might possibly undermine the ultimate validity of opting for a preferred universal model of behavior. The choice is perhaps even more critical for George Stigler's fundamental concerns. From his perspective, acknowledging such departures from strict rational decision-making requirements could, and would, be categorized as a distinctly backward movement. Such a retrograde slide into the morass of pre-war

[9] Stigler was not satisfied with using self-interest as a useful assumption, a mere platform from which to launch productive economic analysis. As he makes clear in his 1980 (April) Tanner Lectures, self-interested motivation should instead be understood as an economic hypothesis that if tested would prove to be correct.

> I predict this result because it is the prevalent one found by economists not only within a wide variety of economic phenomena, but in their investigations of marital, child-bearing, criminal, religious, and other social behavior as well. We believe that man is a utility-maximizing animal – apparently pigeons and rats are also – and to date we have not found it informative to carve out a section of his life in which he invokes a different goal of behavior. In fact, the test I have just proposed has very little potential scope, I shall argue, because most ethical values do not conflict with individual utility-maximizing behavior. (Stigler 1982a:26)

ambiguity would represent a deliberate step backward. Stigler would most likely identify this antiquated approach more closely with the analytics typifying the work of his teacher, Frank Knight, than with any of the positive empirical advancements made since that time.[10] Quantification, which alone was capable of providing the basis for extensive empirical testing, was his preferred mode of research.[11] Moreover, Stigler was never interested in detailing the limitations of the discipline, but rather in extending its reach.

> You face a difficult problem with ideology … If the behaviour is non-rational, then any behaviour can be presumably taught. To say that the existing theory has serious problems in explaining some behaviour (e.g. voting) and <u>therefore</u> ideology is the explanation, is like saying ideology

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[10] Ronald Coase takes quite a different position. He prefers to adhere more closely to Knight's behavioral approach rather than subscribing to the less flexible world of incontrovertible rational choice.

> When you say it is un-Chicago, you mean that it is an unmodern Chicago view. Because Frank Knight was at Chicago, and I was brought up more on Knight than I was on any of the others. And my views were quite consistent with what he says. They're not consistent with what George Stigler, Gary Becker and Richard Posner say. Posner condemns me because I don't think people maximize utility. … I don't say people are wholly irrational. … but it doesn't follow that because a person does less foolishness when the price is high for foolishness that you don't have foolishness. The foolishness follows the universal law of demand. The greater the price you have to pay for being foolish, the less you do. (Hazlett 1997:3)

[11] There is no point in denying that Stigler himself was a master at selling his ideas, much in the fashion delineated in Stigler and Becker (1977). He knew how to appeal to the existing preferences of his audience. His genius involved putting just the right spin on any given argument or empirical analysis. He was a skilled rhetorician despite the fact that he dismissed the McCloskey-style (1983) focus on the way in which economists persuade. His insistence on presenting economists as impartial observers, unmoved by self-interested or partisan concerns, led to him assert in a 1985 NSF report that persuasion in economics was a simple function of the facts. For Stigler the task was always to put together a clear chunk of analysis supported by empirical evidence. The ensuing result would sell itself. (Such thoughts are reminiscent of the optimism embodied in the oft-misquoted phrase dubiously attributed to Ralph Waldo Emerson, "build a better mousetrap, and the world will beat a path to your door.") However, Stigler's own professional strategy tended to belie this idealized position.

> He had confidence in his theory and he felt that his data supported it. The defects, they are always mentioned somewhere, not in strong terms, but they are always there. Maybe they're in a footnote or an appendix, but the defects in the data, as far we were aware of them, are there. We always tried, of course, to find a way to overcome them. But I think it was really a personality thing that saw him always presenting everything with great confidence. He was always a kind of rhetorician, although, he did give that bad review of the McCloskey book on rhetoric. (Conversation with Claire Friedland, October 1997)

> is what you name the residual from a (crude) self-interest theory. There is always a residual, so all is well? (Letter from George Stigler to Douglas North, March 17, 1981)

For Stigler there is essentially no conflict of interest involved in ideological considerations, since the power of competition must inevitably cleanse accepted doctrine of any bias.[12] In the marketplace of ideas, a concept adhered to by those at Chicago; it is observable results rather than the motivation of the author that must ultimately carry the day.[13] More widely, the same must hold in the realm of public policy. What matters most in this case must be the worth of the proposal rather than the background of its formulation.

> I do not intend by this argument to urge the adoption of full disclosure rules for private organizations which seek to influence public opinion. Arguments should be valued on their logic and evidence, not on the possible bias of their authors. Public requirements of disclosure of interest would lead to infringements of freedom of expression of which the Securities and Exchange Commission's insider trader policies already provide bizarre examples. The proper inference to be drawn from my argument is that official conflicts of interest are vastly over-emphasized and over-publicized, and this exaggeration seriously hampers the recruitment of public officials (Stigler "Conflicts of Interest" draft, February 24, 1989:2).

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[12] For George Stigler, the power of market competition made much of government intervention and regulation not only unnecessary, but downright counter-productive. Certainly the Securities and Exchange Commission fit this profile.

> The capital markets work best when they are competitive, because people work best when they are subjected to competition. Competitors simultaneously raise the dividends and interest payable to savers and lower the cost of capital to investors. Competitors provide securities offering all degrees of safety or risk that different investors desire. (Stigler "The Economy", *Business Month* January 1988:8)

[13] Undoubtedly Stigler would regard the decision by the American Economic Association to have the authors of articles published in its journals openly declare any potential conflicts of interest as no more than an irritating distraction, lacking any discernible shred of merit whatsoever. (See the AEA disclosure policy, http://www.aeaweb.org/aea_journals/AEA_Disclosure_Policy.pdf) The position and ideas articulated in any such article would of necessity face the persistent competition generated within the market for economic ideas and would fall or rise on their own merits, no matter what the author's funding source might be. Thus focusing on any such perceived conflicts of interest was essentially an irrelevant diversion and an outright waste of time.

As economists we all relish an image of belonging to a profession that is inhabited by clinical scientists carefully weighing all available evidence before issuing a tentative finding. (Even academics are capable of imagining that they would look particularly fetching clothed in Hollywood-style starched white lab coats.) But it seems a human trait to suffer from the burden of deeply entrenched a priori beliefs that influence our thinking and thus our interpretation of any forthcoming data. Facts refuse to speak for themselves, preferring to remain mute. It falls to the economist's lot to perform as stumbling interpreter in their presence. The problem arises if economists in a state of unconscious denial refuse to recognize and at least tamp down their ideological proclivities. The danger comes not in the ideology itself, but in a refusal to recognize it.

> I doubt there is a truly unbiased academic … If you think the [Chicago] GSB is an unbiased environment, think again. They are recruited for their views. I wonder how many free marketers would get jobs in anthropology or sociology. (James Heckman quoted in Nik Khah 2011b:380)

This very human quirk, identifiable within the economics profession (only the dead are reliably and completely unbiased), allows vested interests in all shapes and forms to cherry-pick their desired stance from among the multitude of available economists.[14] If searched for with sufficient diligence, someone in the profession will be found to honestly and perhaps naïvely hold the sought-after position without any apparent prior inducement being applied. Dishonesty is not, then, the abiding problem in economics, or at least significant evidence of such calculated misdirection seems largely lacking. A general absence of self-reflection, overwhelmed by a need to "get on with the work at hand," is a more insidious and destructive force that badly needs serious and even-handed attention to keep its dangers at bay. The key to a more useful economics in that case should begin with an artful attempt to disentangle ideology from methodology, tamping down

[14] Nik Khah (2011a) discusses George Stigler's relationship with corporate funding. However it would be a stretch bordering on exaggeration and perhaps dishonesty to see him as some sort of shill for corporate interests or even influenced heavily by them. It is more accurate to say that Stigler was very successful in raising such private backing because of the views he held, rather than thinking that he shaved his position to attract such funding.

the temptation to employ those methods which ensure a desired result. This volume, containing a series of linked, but distinct, chapters, makes a hopeful, if only initial, start on examining this issue.

Posing the problem must begin by clearly demonstrating its precise nature: starting by acknowledging that a concrete example, a pertinent story, will always have a greater impact than an abstract discussion circling the relevant key elements composing an issue.[15] In this spirit, one of the initial chapters ("A Tale of Two Cities") seeks to meticulously explain how initial assumptions inevitably determine desired conclusions. Two articles, when placed under a critical microscope, reveal themselves to be essentially mirror images of one another, persuasively attempting to model the exact same phenomenon.[16] The respective authors need only to reverse the essential starting assumption of their analysis to transform solutions into problems and vice versa. The two comparative approaches signal a known but not so widely acknowledged reality, namely that a clever economist (and all three of the relevant economists here are clever in the extreme) can argue and prove any pre-ordained conclusion whatsoever, simply by following a thread of thought backwards to its beginning.

The most effective strategy for combating the problem of constructing an argument that necessarily arrives at a pre-ordained result does not, despite contrary inclinations, suggest any categorical requirement to refocus on methodological considerations. Substituting one methodology for another, as Richard Bronk (2009) so painstakingly attempts to do, hardly provides the profession with anything resembling a cure or even a temporary palliative. (This problem is dutifully examined in "Love among the Ruins" in this volume.) Replacing one set of difficulties with another manages to change the decorative theoretical backdrop while still

[15] For example, somewhat notoriously, government budgets are more often than not erroneously presented in the cosy terms of household spending dilemmas, a story that is immediately comprehensible to the casual voter, but misleading in quite a deliberate fashion.

[16] Given the diametrically opposed political stances embedded in these articles, the examples chosen reinforce what should be obvious from the start. Ideology doesn't only affect the methodological approach taken by a uniquely specific political allegiance. The Chicago School of the post-war era was certainly far from alone in this respect. This particular tribe of economists does loom large in this volume, not because it deserves any special brickbats, but simply because of the skill and success the School manifested in marketing its ideas. Chicago provides a more cogent, and perhaps entertaining, example than those with more left-leaning ideologies. The issue is best illustrated by the most adept as opposed to the many other also-rans available.

falling into the trap of requiring a stepped path to effective research.[17] The underlying problem that remains largely untouched lies in the persistent effort to reduce analysis to a universal algorithm. What may, however, present a more constructive path in this case would be an acute observation of what economists actually do, where their biases and pre-judgments arise, and the effect of such influences. Perhaps the basis for unconsciously glossing over such proclivities lies with the urge, or even necessity, attached to marketing one's research and findings. This imperative helps explain, to some extent, Chicago's strategy and that of those economists with competing agendas. The Romanticism offered by Bronk then constructs nothing more than an escape from this intrinsic problem, an attempt to flee from rather than grapple with the issues at hand. As a result, this apparent alternative posture provides little that cannot be similarly offered by an intrinsic preference for market rationalism. The only intractable impediment seems to arise from a need to adopt a rigid lens through which to process observational data, turning that framework into something resembling a Procrustean bed upon which all findings are shaped to fit. In terms of the market sensibilities delineated by Stigler and Becker (1977), the marketplace for ideas, under these conditions, yields only the "all the news that fits" category of results, rather than the more lofty heights encapsulated by "all the news that is fit to print."[18]

Ideology induces a researcher to see the world the way he or she would like it to be, rather than as it is.[19] As previously emphasized, a useful way to measure the ideological content of any policy is to ask what possible evidence would effectively change a given economist's mind. Too often

[17] The resemblance to the sort of twelve-step program subscribed to by Alcoholics Anonymous or other offshoots of that original model is striking. The essence of the predicament may lie in a human proclivity to want to direct the lives of others, their undeniable enjoyment in telling others what they should do and how they should do it.

[18] The key to marketing in this academic arena is to convince other professionals that your idea provides them with what they want or think they need. This process has only a sideways relation at best to an idealized path toward some greater truth. There remains a distinct difference between the types of evolutionary result we would like to confirm as opposed to the reality that may in fact evolve. The consequence of skillful marketing is not the high-minded idealism represented by the *New York Times* tag of claiming that what is sustained consists only of the news that is fit to print. Evolution promotes a reality which encourages all the news that fits the environment, rather than insisting on a more ideal result.

[19] Too many economists seem unaware of the particular Kantian blinders limiting their vision.

the answer is: none that is even remotely conceivable. Keynes may have said "When the facts change, I change my mind. What do you do?"[20] But at least in the case of some economists, when confronted by this quandary, they react either by re-interpreting the facts to suit their preconceived notions or by finding a more congenial data set. A recent event can serve to explicate this point. With a number of governments facing economic wreckage in the aftermath of the global financial crisis, at least a few attempted desperately to salvage the situation by administering various forms of Keynesian-like stimulants to boost aggregate demand. Opposition arose from some economists who knew a priori that such policies inherently must and therefore would fail.[21] Their confidence needed no embellishment derived from or associated with any careful consideration of empirical results. Other economists who foresaw the inherent futility of any type of fiscal policy would subsequently employ empirical results to substantiate their instinctive revulsion, but in some cases were driven to torture the data whenever it proved intransigent, until ultimately, as expected, the data despite its initial recalcitrance simply confessed its sins and surrendered. The core problem presented by an unacknowledged, but unshakable, ideology revolves around the indisputable fact that any sufficiently clever economist can always arrive at a desired result through logical argument and statistical evaluation. Those who are adept at marketing their irrefutable beliefs can discover a successful strategic path that will allow them to convince others and by doing so widen their influence. Unfortunately, such market prowess seems to reduce economists to so many warlords defending their castle keeps against all and any invader.[22]

[20] The statement is attributed to Keynes and has become part of a very hard-to-change oral tradition in economics. Since this is a reported verbal utterance, it will always be difficult to pin down. However, if Keynes did not actually make such a statement, then he certainly was remiss and should have adroitly fashioned one along those lines.

[21] John Cochrane (2010), ensconced at the Booth School of Business (Chicago), seems intent on continuing this Chicago a priori tradition in regard to fiscal stimulus.

[22] A reading of Coetzee's (1980) *Waiting for the Barbarians* might in this instance be of some use even for veteran economists.

Marketing Mayhem: The Importance of Constructing Convincing Stories

> *Craig Freedman: He [George Stigler] did want to convince people when he wrote.*
> Milton Friedman: Yes,
> *Otherwise, why would you write?*
> Milton Friedman: Yes.
> Aaron Director: I'm not so sure about that rash statement. [laughter] You would want to write just to first make sure that you yourself understood and secondly to show other people.
> *Well, you have to convince yourself first.*
> Aaron Director: Well, yes you have to respect it seriously.
> *Absolutely. But if you're going to convince somebody else, or to explain it to somebody else …*
> Aaron Director: Well, you want to do it even if you didn't want to explain it to anybody else, even if you only wanted to explain it to yourself … and maybe just a few students. [laughter]
> Milton Friedman: Now you're already giving up some ground.
> Aaron Director: I'm giving up my ground having paid for it.
> (Conversation with Milton Friedman, Rose Friedman and Aaron Director, August 1997)

Economists have always been in the business of selling their ideas. But during more recent decades, as the quantity of published articles has increased and new journals have sprung up mushroom-like to accommodate them, the importance of marketing has grown apace.[23] Technological

[23] Before the 1970s, most academic economists published only sporadically, their responsibility residing mainly within the realm of teaching. According to Stigler and Friedland (1982b:198), more than one-third of economists granted doctorates from the top economics departments in the USA failed to publish a single article during the 1950–1968 period. A mere 5 percent managed to publish sixteen or more articles. Clearly less competitive pressure existed during that era. Nowadays, economists, even those employed at institutions verging on the obscure, and only equipped with degrees from schools sporting less than prestigious banners, would barely be treading water if adorned with similar publishing records. Conceivably, such added competition may have ultimately generated more dross than gold, since in that long-lost era, most published research was the result of self-motivated individuals. If we accept basic economic assumptions concerning motivations and incentives, only those with a true interest and vocation would have bothered investing time in what was then a seemingly optional practise. The opportunity cost of not publishing simply was much lower than it has become in more recent times.

changes in the form of computing power and the internet have transformed the nature of finding source material and data, as well as reshaping the boundaries of statistical work and even simple typing.[24] Simultaneously, competition for recognition has increased the need to market in an almost obsessive fashion. Despite its importance, to focus too much attention on salesmanship, to pay a deliberate degree of consideration to it, has been implicitly considered as being somewhat crass. This activity, like that of the nineteenth-century body snatcher, is deemed to be a necessary one, but not something that is likely to be acknowledged or discussed in polite company. Ideas are supposed to sell themselves, in the clichéd sense of Emerson's better mousetrap.[25] Under this somewhat fanciful construction of the economics profession, competition must inevitably ensure the survival of the best and truest theoretical models. Yet competition, even when operating effectively, only promises a match between the ideas and the market in which they are being hawked. Optimality, let alone truth, no more plays a necessary role under these constraints than it does in the case of any other product or service. (Few economists would insist that people demand consumption products because they lead to obesity. The market responds to what these consumers desire, even if the results of meeting such demands are not themselves desirable.)

Yet clearly the ability to market, to intimately know your audience and to know how to persuade such targeted consumers, is not equally distributed throughout the economic profession. Such tactics are not explicitly

[24] Running even simple regressions in the era of punch cards required a degree of patience outshone only by the dark days prior to such access. Without the assistance of skilled research librarians, finding articles and data was extremely time-consuming and often unrewarding as well. Typing itself was yet another daunting hurdle which only those able to access secretarial assistance could easily overcome. The associated opportunity cost of writing an article implied that only the most self-motivated, or masochistic, succeeded in producing anything resembling frequent output.

[25] Perhaps ironically Ralph Waldo Emerson may not himself have written such a moldy chestnut (or more kindly, an epigram) to encapsulate this thought. At best it might have been uttered by him during an address or speech. The original printed version read, "If a man can write a better book, preach a better sermon, or make a better mousetrap than his neighbor, though he build his house in the woods, the world will make a beaten path to his door." This dubious quotation became transmuted and marketed as "Build a better mousetrap, and the world will beat a path to your door." (For those wishing to indulge an unslakeable idle curiosity, the place to begin this rather fruitless exploration can be found at http://todayinsci.com/E/Emerson_Ralph/EmersonRalph-MousetrapQuotation.htm. However, whether such a statement ever contained much in the way of practical wisdom is another matter, one perhaps not worthy of consideration.

the subject of any concentrated graduate work, nor are such techniques commonly discussed. Though much in recent years has been written on the rise of the Chicago School economists, little attention has been paid to the undeniable fact that at least some of the undoubted success of the counter-revolution through which they managed to conquer the discipline of economics was due to the marketing skills exhibited by its two major proponents. Both Milton Friedman and George Stigler raised the artistry of their advertising within the presumed marketplace for ideas to levels previously unknown. In response, or even in self-defense, other competitors within the profession had to attempt to raise their game to these new heights or be swamped by the skill with which this particular duo managed to package their theories and proposals. Those holding diametrically opposite views would find themselves hard put to match such abilities. Though the contest raged during the post-war decades, few explicitly acknowledged the role played by the deft packaging that enticed others to sample those ideas, with attention focused instead exclusively on the theories contained within those well-wrapped bundles.

> I naturally consider you, on balance, a bad economist, a thought from which you will doubtless recover. You are an unduly predictable exponent of the stereotypes. You would have had no influence whatever save that you are able to write with clarity, some grace and occasional humor. I can think of no economist from Smith to Milton Friedman who has earned any reputation in the field without some competence as a writer. I've said on many occasions that Marx and Ricardo both owed something to the obscurity of their prose and the ability of their disciples to find therein what they want to believe. But both had a stunning capacity to make themselves heard when they wished. (Letter from John Kenneth Galbraith to George Stigler, June 2, 1977)[26]

[26] Galbraith felt compelled to respond to Stigler's reflexive evisceration of his television effort "The Age of Uncertainty". The two had been sparring since the post-war era and Galbraith would have been disappointed without a slashing review from Stigler.

> That the television series [*The Age of Uncertainty*] and the accompanying book, contain few ideas new even to Galbraith is therefore no occasion for surprise, let alone complaint. So a well-known preacher has returned with his customary sermon, and if it is beginning to be delivered in a somewhat garrulous fashion, that is a small sin and one customarily indulged in by people of Galbraith's (and my!) age. (Stigler 1986:352)

As detailed in the chapter sketching out the rise of Chicago under the triumvirate of Friedman, Stigler and Director ("The Heart is a Lonely Hunter"), this trio mounted their post-war counter-revolution by focusing determinedly on the balancing effect that competition provided in coordinating economic activity. Counter-balancing checks prevented any serious market transgressions by providing self-interested incentives for participants to toe the more mundane straight and narrow line. This rationale for encouraging competitive markets proved over the long haul to be a powerful line worth pursuing, since its unswerving repetition eventually shifted focus away from the issue of private economic power (the problem of monopolies) and fixed attention instead on the consequences of government intervention. The persistence with which this was marketed, and the degree of what almost amounted to intransigence in considering alternatives, ultimately proved to be a successful sales strategy. (Aided of course by the historical context in which they were propounded.) Fortunately the progenitors of this variant of market fundamentalism proved impervious to successive early attempts to dismiss these theories out of hand by heaping them with veritable torrents of scorn and ridicule.

> All of us here are accustomed to being members of an intellectual minority, to being accused by fellow intellectuals of being reactionaries or apologists or just plain nuts. (Friedman quoted in Cherrier 2011:359n)

When trying, then, to understand Chicago as an ongoing project, as well as a settled doctrine, a mortal sin would be committed if one were to misunderstand the unintentional ideological bias threaded throughout the School as a deliberate attempt by these mainstays of Chicago

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Stigler would not publicly acknowledge the importance of rhetoric, preferring the pose of the objective scientist operating above the affray. Galbraith, however, was always the public figure, the showman who enjoyed selling his wares. He understood and never denied the importance of uncommon marketing.

> On balance, I think it more important to be known as a good writer than a good economist. There must be hundreds of our profession … I could name a dozen … with an original, perceptive or otherwise interesting view of matters, large or small, who have been unable to communicate it beyond at least the narrowest circle, often not that. (Letter from John Kenneth Galbraith to George Stigler, June 2, 1977)

to promote vested interests.[27] They may well have promoted an ideological position against what they clearly saw as the dangers of an ever-encroaching collectivism. This vision in turn may have in fact paralleled a number of given corporate interests. But there remains a great deal of doubt that either Friedman, or Stigler, let alone Director, saw themselves as either promoters or defenders (and certainly not paid flacks) of the private business sector. They did see in an almost obsessive fashion the continuing dangers of government intervention in any given market. The need to sell such a vision conclusively, then, would necessarily require that corporate power, in whatever form it was conceived, had to be trivialized by emphasizing the persistent presence of effective competition. Markets, and the benefits flowing from them, given this approach, could only be substantially throttled by the heavy hand of government.[28] For any seeming defense of corporate interests to be at all palatable, a robust business agenda must instead be transformed into, and identified with, an inescapable skepticism concerning the exertion of government power.[29]

> How can we keep the government we create from becoming a Frankenstein that will destroy the very freedom we establish it to protect? (Friedman 1962:12)[30]

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[27] To repeat the obvious, Chicago isn't the only school or department with an unacknowledged and unintentional ideological bias.

[28] Republicans have for many decades profited from the facile quip slipped into Ronald Reagan's first Inaugural Address (January 20, 1981), "Government is not the solution to our problem; government is the problem."

[29] To emphasize again, the purpose was not to deliberately promote the vested interests of the corporate sector. That the end result closely resembled an intentional lobbying effort is largely beside the point. An unshakeable belief in competitive markets reduced economic power to a level of inconsequence. The danger then became not any attempted conspiracy by corporate interests, but rather the unwelcome intrusion of government administrators.

[30] Notice that Friedman's statement is defined by a distinct marketing spin, however trivial it may seem. He chooses to opt for effect over accuracy, a perhaps unintentional sloppiness that can prove addictive. The rush to sell a point can leave precise specification in the dust. The issue Friedman fixes upon is not the dilemma of creating a Frankenstein but of creating Frankenstein's monster. The common error is to confuse the scientist with his creation. Undoubtedly if Friedman chose to be more careful he would warn against the collectivist scientist, Victor Frankenstein, who like the central planners of the post-war era has the audacity to think that he could be god-like, achieving ends that would benefit all humanity. Instead he creates a life-threatening monster. In contrast, self-regulating markets, at least in textbooks, are able to avoid such unintended consequences.

Epicenter Chicago: The Model of Skill and Grace

> I don't think he [George Stigler] was trying to put anyone down by making people out to be idiots and showing himself to be their intellectual superior. That wasn't his intention. He just was very critical. He said exactly what he thought and he thought that most things were probably wrong! And you know what? He was probably right! [laughter] But that's part of the culture at the University here also. This is a *very* critical place. So, you need to place him in that culture. I don't know what he was like when he was younger. He was a student here, so he grew up in and was moulded by this culture, here at Chicago. Friedman, who was George's very close friend, was in this Department for years. He is a very critical fellow too. He also has very strong opinions and is not shy when it comes to expressing them. And both of them were often right. [laughs] So I guess that's where their reputation and approach was formed. (Conversation with Sherwin Rosen, October 1997)

Chicago existed as an almost classic coincidence of serendipity and design. The relentless post-war rise of the evolving School in South Chicago can best be categorized as driven by talent, ambition, ideology, time and place, making it little different than other academic departments. The peculiar talents displayed, however, created a movement that carried this band of determined economists from a place at the very fringes of the profession to a determinative force shaping the nature of how economics is practiced. The very epitome of the single-minded drive and creative insight behind this success was the figure of George Stigler, despite his lengthy exile from Chicago through most of the 1950s.[31] (Though geographically parted, he never spiritually left.) Stigler, the Knight of the Woeful Countenance,

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[31] It is difficult to keep the refrains from Gilbert and Sullivan's famous patter song "A Modern Major General" from floating through one's mind when considering Stigler as a very epitome of the Chicago School and style.

> I'm very good at integral and differential calculus;
> I know the scientific names of beings animalculous;
> In short, in matters vegetable, animal, and mineral,
> I am the very model of a modern Major-General. (Gilbert and Sullivan, *The Pirates of Penzance*, 1879)

tilted against real giants even during those early years when others were all too ready to dismiss them as mere windmills.[32] Thus taking on Edwin Chamberlin and his *deus ex machina* of monopolistic competition, in some sense one of the mainstays of East Coast (Harvard) economics, displays an early fearlessness and precocity. As carefully depicted in the volume ("The Chicago School of Anti-monopolistic Competition") the force of Stigler's attempt at demolition economics rested more on his turn of phrase and rhetorical strategies than on the strength of any detailed argument, let alone empirical foundation.[33] Choosing the proper opponents, and the timing of one's attack, forms an often overlooked, but essential component of succeeding in this academically wainscoted marketplace of ideas.

In his attempt to demolish Chamberlin, and all that he represented, the core of the joust is a fixed determination to defend the honor of price theory no matter what the cost. The result is a battle, rather than an attempt to understand or carefully evaluate Chamberlin's work. Pointing out Stigler's strategy, though, should not be taken to constitute anything resembling an exculpatory defense of the theory of monopolistic competition, no matter what the version or vintage might be. This exercise, then, in no way constitutes an attempt to pick favorites, since what is at stake is in no way an either/or situation. Rather, the aim is to try to understand the context within which George Stigler was tempted to launch his devastating attack and the driving motivation that sustained it. As always in Stigler's attacks against challenges to price theory, there were more than simple theoretical issues at stake. The urgency defining the

[32] The reference here is to Cervantes' *Don Quixote*. Photos of Stigler, especially when standing beside the Sancho Panza-like figure of Milton Friedman, make such an analogy inescapable. As Stigler's long-time research assistant Claire Friedland noted:

> His loyalty even extended to abstractions: the Chicago School or neoclassical economics. Much of his work centered around saving the damsel in distress, neoclassicism, from her attackers: hence his work on the economics of information and his enthusiasm for the Coase theorem. (Friedland 1993:780)

[33] I am indebted for the idea of employing a rhetorical demolition derby to ritually disembowel opposing theories to Thomas Sowell's recollections of Stigler, from his position as his student. The approach was a scorched-earth strategy or perhaps a campaign reminiscent of Rome salting the very earth on which Carthage once stood to prevent the city-state from ever again rising to prominence.

> It was the perfect place for Stigler to conduct a Demolition Derby. Nor was he hesitant about the task. Theories like "monopolistic competition" and "countervailing power," which were treated reverently at Harvard (where they originated), were eviscerated by Stigler. (Sowell 1993:787)

rhetoric of such an attack perhaps reflects the then overwhelming need to market those innate ideas that were sustaining the Chicago version of price theory. Thus a degree at least of ideology is entwined in the methodology employed. Stigler strongly suspected that despite Chamberlin's inherent political conservatism, his theory could be employed to support aims distinctly tinged by collectivist leanings. In these early Cold War years, those who fought against the tide of a leftward political drift, one which advocated more planning and government intervention, viewed such economic theories as carrying the germs that would only further limit freedom and eventually curtail the notion of liberty. It can hardly be purely coincidental, then, that Stigler's lecture at the London School of Economics in which Chamberlin was so summarily dismissed, came shortly after the founding of the Mont Pelerin Society (1947). This newly founded association, formed battle ready, can only be viewed as a clear attempt by intellectuals on the right to redirect public discourse into channels that men such as Stigler, Friedman and Director considered far more amenable. (This strategy constitutes a classic attempt to shift the grounds of debate onto a more congenial terrain.)

If we trace out the development of economics, as it came to be practiced at Chicago, we see in miniature the same war to reconquer the heart of the discipline from the Keynesians and others with a collectivist bent who appeared to be the dominant academic force in those early post-war years. By bringing George Stigler back into the fold, ending his exile at Columbia in 1958, and with Aaron Director excavating an unshakable foundation for Law and Economics at the Law School, the place of price theory and the ideology attached to it was now firmly established at Chicago.

Tangled: How Methodology Has Become Increasingly Intertwined with Marketing and Ideology

> Part of it is the persuasion. There's no question. George Stigler, I remember when I was a young person, wired and said "Selling is very important in your research. So write better. Work on writing because that is important. You've got to sell what you are doing." I think he's exactly right. You've got

> to sell what you are doing. It may be that in the long run good ideas do surface but they surface faster, if written in a persuasive fashion. Moreover, bad ideas may be put persuasively. And they may gain the necessary threshold. However, taking that same analogy in competition among ideas, there is a presumption, although not a certainty, that in the longer run, the good ideas are going to compete out the bad ideas. But that may take a long time and may not even always operate. There's nothing necessary about that. Nothing guaranteed about that. (Conversation with Gary Becker, October 1997)

Economists rightly believe in the power of incentives, at least to varying degrees. If choices are founded on a bedrock of self-interest, economists cannot excuse themselves from also being so motivated. In fact, being conscious of this mainspring of choice, they should be even more aware of its consequences for them personally. As a starting point, individual economists seek widespread recognition of their work for a number of interrelated reasons: career advancement, financial gain, preferred positions and ego satisfaction. But to be recognized in such a crowded field, to stand out from the faceless mob, demands skilful marketing. This requirement is a simple reflection of reality rather than something approaching a sordid secret. Certainly a necessary imperative is not to come to the marketplace for ideas with an empty basket, but at least equally important is an ability to spin those delivered ideas so that they glitter when placed before the profession. Following the lead of Stigler and Becker (1977), the aim is to convince this gaggle of purported colleagues that these painstakingly constructed ideas best satisfy their given and fundamental preferences.

> Because what meets market demand may have an element of so-called truth in it in the sense that what people think better organizes the world for them, about how do we know what shapes events and so on, would presumably be one of the factors (and we hope an important factor) in determining legitimacy. But it wouldn't be the only factor. We would need other factors. And where these other factors dominate you'll get other results. (Conversation with Gary Becker, October 1997)

The Chicago method was unquestionably built upon hypothesis testing and bound to empirical evidence. As one of the founding post-war members, George Stigler (1965) was certainly committed to this methodology. Yet given the undoubted imperative to market one's ideas, perhaps what an economist intuitively knows must be the case, would begin to shape the way that economist's finding were marketed and perhaps encourage such an economist to tickle any strict methodological barriers into something resembling concordance.

> People take particular stands early in their career and they're steadfast, I have to admit that. They are not wishy-washy. No amount of empirical evidence will persuade them to change to a different point of view. Would there be some evidence that would have persuaded George on this? I'm not sure. He believed, he really believed that prices responded to short-run market demand. Was there any evidence that would have caused George not to believe that prices responded to short-run market demand? I can't help you on this. I don't have any reason to believe that he did believe there was any evidence for that. (Conversation with James Kindahl, November 1997)

In the case of the second generation of post-war Chicago economists, both Becker and Peltzman consistently demonstrated a clear dedication to this adamantine Chicago method. However, Peltzman's certainty was perhaps not quite as nuanced as Becker's, but was more strictly bound by what he recognized as the nature of the available evidence.

> If that's what the evidence is, that is what it is and we have to, we *have* to, you know at the end of the day, if we are convinced that that is what the evidence is, that is the truth. Not our pride. Our pride is not the truth. But he was complex… Who was it, Mel Reder (1982) who wrote a piece in which he said "the distinguishing characteristic of Chicago economics is 'the tight prior equilibrium'." That was wrong. I told Mel it was wrong, but it is easy to see how somebody could come to that conclusion. (Conversation with Sam Peltzman, October 1997)

In one sense, this is at least what Chicago claims as its non-negotiable foundation. But underlying Peltzman's admirable claim is a vision that in the marketplace for ideas, truth continues to ultimately decide what is accepted (certainly in the long run). However, in an intriguing twist of this more standard methodology, Gary Becker chooses instead to push the logic of his own theoretical position to its inevitable conclusion. Evidence does not lead to an unambiguous result. Given this analysis, when aided and abetted by the ultra-competitiveness characterizing Chicago economics, reality need not be successful in steering clearly away from the twin chasms of marketing and ideology. Denying this possibility seemingly requires that we are either unduly optimistic or sufficiently naïve to deny the importance that incentives exert on academic pursuits, driven as they must be by prior beliefs. Given such a reality, it is an intuitive leap to claim that the marketplace for ideas must inevitably yield anything resembling a kissing cousin to an irritably elusive truth. As a result, an obligation to temper the enthusiasm with which theories are presented and results hailed would seem to be based on the professional history of the discipline for at least the past three decades. But balancing the competitive rewards allotted to those who vie for recognition against the incentives created by this process, expecting different results to accrue would appear to be no more than an instance of ingenuous intransigence. The marketplace for economic ideas remains to some degree terra incognito. "Here be dragons."[34]

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[34] English mapmakers were wont to place the legend, "here be dragons" to denote the edges of the known world.

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A Tale of Two Cities: A Priori Assumptions and A Priori Conclusions

> Both of them, both of these men had taken strong positions on this ... Maybe - I almost come close to saying, that they had taken these positions before I was born. That's not true, but it certainly was before I was born as an economist. I could have predicted George Stigler's predictions before I signed up from what I'd learned in graduate school of his writings. And I could have predicted, to a great extent, Gardiner Means' predictions and you could guess what I learned at graduate school about Gardiner Means (Conversation with James Kindahl, October 1997).

In 1964, George Stigler chose the occasion of his AEA presidential address to challenge the assembled battalions of economists, entranced by his words, to engage in the moral equivalent of a crusade within the profession.[1] He

[1] As was his wont at such moments, Stigler pushed his vision to the precipice's edge (and perhaps over). Barely concealed in his rhetoric are images of a bright new dawn breaking over the previously benighted profession. The crusading tone of his language is almost inescapable.

> It was just that he was so enthusiastic about quantitative measures. He thought that he was going to change the world. … I was sitting with Aaron Director at the time when he gave his presidential address and we did look at one another at the time to try to see what each one thought about all of this. (Conversation with Ronald Coase, October 1997)

C. Freedman, *In Search of the Two-Handed Economist*,
DOI 10.1057/978-1-137-58974-3_2

called for the eradication of economics by assertion.[2] From this time onward, no economist would be worth that name unless each and every hypothesis put forward was supported and tested by available evidence. No longer would economics be simply stated or buttressed by "just so" stories. The putting forward of testable hypotheses and evaluating each one with given available data is what would become known, in the best sense of the term, as the Chicago method.

But Stigler and other of his colleagues would come to cultivate an unfortunate tendency when employing this method. Absent were some key characteristics of this approach, in particular the ability to be surprised by one's results.[3] In the absence of this quality, the method simply creates a quantitative rhetorical device rather than an incisive tool of discovery. But just as it is possible to interpret quantitative results in a manner guaranteed to verify any a priori conclusion, in a parallel fashion, any clever economist can set up a priori assumptions that, once accepted, inevitably lead to a foregone conclusion. Again, this is a

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[2] In a 24 November 1947 letter to Arthur Burns, Stigler inveighed against the widespread sloppiness endemic to the profession in its use of empirical data (Hammond and Hammond 2006:68–71). Statistics, often without cited sources, were sprinkled throughout papers like so many colored jimmies on a frosted cupcake. Stigler detailed the way that sources of quotes remained unidentified, along with calculated results and random statistics presented without the courtesy of explanation or verification. Instead, assertions dominated and were rife throughout all published work, even that appearing in well respected journals. Stigler's letter would later be transformed into an expanded essay appearing some sixteen years later (Stigler 1963), just prior to his prophetic presidential address.

[3] Testimony citing a pronounced tendency to know the answers to their posed questions prior to any empirical investigation can be supported by a number of sources who were not antipathetic to the Chicago approach. James Kindahl, who along with George Stigler (1970) co-authored a significant piece of empirical research, had few doubts about this pronounced inclination. Stigler, who was intent on destroying any credibility attached to Gardiner Means' framework of administered prices, could foresee only one result issuing from their carefully gathered price surveys and subsequent reconstruction of industrial prices.

> He was absolutely convinced that prices were flexible from Day 1. That was clearly his a priori intention. It was more than an intention. It was his belief, in the true believer sense. He really believed that. (Conversation with James Kindahl, November 1997)

This same mindset has also been noticed by some of those who possessed an insider's ability to observe the Chicago School in practise from its earliest Friedman/Stigler/Director beginnings. See Reder (1982) for an insightful analysis.

rhetorical device that entails assumptions being presented as self-evident and thus hardly worth the effort involved in staging a debate. The skilled rhetorician markets his conclusions by casually slipping in these essential assumptions. Such economists successfully costume these suppositions to resemble logically consistent entry points, indisputable and beyond reproach. But once the reader swallows this palatable bait, he or she is inevitably drawn to accept the foregone conclusion provided by the author. The lingering suspicion attached to such methodology is that these authors retrofit their arguments to reach a set of desired conclusions.[4] The economic method then becomes not a tool of discovery but rather a form of rhetorical persuasion where, at its worst, arguments motivated by biased prejudgments compete for the hearts and minds of a profession's practitioners. In such an environment, the ability to market convincingly tends to infiltrate and subsequently dominate every academic exchange.

> If you happen to have a product that is no good you might market it well but it's going to fail. So I do think marketing is very important when undertaking an intellectual activity. It's probably more important *now* than it was before. The market is much bigger [laughs] for one thing. You have people making serious moneyout of marketing ideas and getting reputations from successfully doing that. But it's not only the money. It's the associated prestige and all kinds of other things as well. It seems to me that there's a lot more score keeping these days. Unlike in previous days, Economists are now reckoned to be in a pretty interesting discipline. So to succeed, you've got to be able to sell your ideas somehow. How do you do that? I don't really know. Maybe you do attack other people. That's been done over the years, but it doesn't always work. It doesn't work if there's nothing behind your attack. (Conversation with Sherwin Rosen, October 1997)

[4] Perhaps we might make cautious use of the "as if" device pioneered at Chicago by Milton Friedman (1953) with vital assistance from George Stigler.

> I think there's no doubt that my work would have been different if I hadn't been influenced by George and George's work would have been different if he hadn't been influenced by me. (Conversation with Milton Friedman, August 1997)

To demonstrate that such an approach can operate in a somewhat insidious manner, even in the best of papers, it is useful to compare two articles which ostensibly claim to explore almost identical issues (though the later of the two investigations makes scant mention of the earlier one).[5] Both papers focus on a moral hazard predicament attached to labor production, while simultaneously interrogating the appropriate assignment of property rights. Though some thirteen years separate their respective publication dates, what sets these two papers apart (while implicitly attaching them at the hip) is the way in which they each inhabit a peculiar looking-glass world where one paper mirrors the other in eerily alarming ways.[6] At key points their assumptions and claims are directly and irrefutably opposite. It then comes as no surprise to discover that the conclusions and possible policy implications reached support very different a priori viewpoints. The two when considered individually remain interesting and original papers, but when examined together, they both appear to be constructed following pre-set and pre-existing agendas.[7] We need to ask ourselves, then, whether pursuing clearly identifiable ideological objectives is incompatible with arriving at a reliable structure for economic analysis. The answer is far from obvious, given the thought-provoking nature of both cleverly constructed articles.

[5] I deliberately opted for theoretical rather than empirical papers in making my choice. In a sense, empirical examples pose little, if any, challenge. Moreover, I focused on papers that, at least in my estimation, were interesting and well argued. Targeting papers that are not worthy of respect is equivalent to the proverbial pastime of shooting fish in a barrel. Choosing two papers that deal so similarly with the same topic, yet come to drastically opposite conclusions, have the inherent potency to display this ideological issue in shades that stand more starkly opposed to one another than possible empirical alternatives could achieve.

[6] Precisely speaking, a mirror image reverses right for left in the original. From start to end, the two papers under consideration consistently assert directly opposite claims, assumptions and conclusions. If we extend the metaphor to encompass political considerations, substituting right for left becomes a more than apt description.

[7] The term I prefer to employ for such methodology is "reverse engineering." Some economists have understood this to be a derogatory label, imbuing the two works with at least a whiff of scandal or deceitful behavior. Yet the term seems appropriate and need not be associated with any notions of wrong-doing. The description, as applied in this paper, simply reflects a practise of working backwards, starting with the finished result and constructing a strategy to arrive at that preconceived conclusion.

Ariadne's Thread: Camouflaging A Priori Conclusions by Means of Deductive Reasoning

> He [George Stigler] was interested, I would say primarily, in a particular sort of puzzle and it's a typical Chicago puzzle. And I don't mean that in any bad way, it's the sort of puzzle that the Chicago School's presuppositions require. Show me an apparent anomaly, something that does not seem to be explicable using the Smithian apparatus and the Marshallian apparatus and I will show you that it can be explained that way. That was exactly the sort of thing that George went looking for. And that's not a bad thing. I'd have to say that it can actually be very good. (Conversation with Robert Solow, November 1997)

A technique slyly employed for solving the mazes beloved by many children is to work backward. Start at the end and find your way to the convenient starting point. This perhaps runs counter to the intention of those who construct such puzzles, but it is the low-risk alternative for those unwilling to flirt with potential failure. Perhaps it is some unarticulated memory of their shared delights at maze-like reasoning that drives the authors of these two mirror-like articles to adopt a similar strategy. Both Alchian and Demsetz (1972) and Sam Bowles (1985) are all too aware of the respective conclusions they wish to achieve.[8] Their only challenge is to figure out an elegant and convincing argument that transports the reader from their preselected starting points to their desired goals. This approach is quite reminiscent of the tactics used so successfully by commanding figures at the University of Chicago for decades. Those forming the shock troops of this school, legendary scholars like George Stigler and Milton Friedman, possessed a remarkably persistent and uncanny knack of asking the right questions, but the equally unfortunate habit of knowing the answers beforehand.

[8] For those with a cast-iron, literal turn of mind, it is inevitably impossible to know what any of these authors might have been thinking while constructing these two papers. Even a detailed discussion with the authors themselves may lead to unreliable evidence. The claim that the authors were aware of their desired conclusions a priori is based on the known ideological bent of each author, a familiarity with their research and the evidence provided by the papers themselves. If they did not have some predetermined conclusion in mind, explaining the choice of the fundamental assumption on which their arguments are based becomes difficult, if not impossible.

> It is always necessary to begin somewhere, but where one begins can have great influence on where one ends up. (Jacob Viner quoted in Van Horn 2011: 291ftn.41)

However, the fact that the same approach can work so well regardless of one's political sympathies demonstrates the power of a priori reasoning in economics, as well as the shadowy role ideology continues to play. The authors of these two papers both seemingly tackle the issue of the firm as a solution to a specific moral hazard problem. But since their political leanings are so diametrically opposed, they aim at achieving completely opposite results. In the case of Alchian and Demsetz, firms turn out to be not only an efficient solution to the coordination problem, but unarguably one delivering equitable results as well. In contrast, for Bowles, firms are not only inequitable but also surprisingly inefficient, failing to accomplish their presumed objectives. To achieve these starkly conflicting results, the authors enter looking-glass-type worlds where the dictum of Professor H. Dumpty seems to rule.[9] What is intriguing is that Bowles reaches his results more than ten years later by simply latching on to the opposite of whatever Alchian and Demsetz propose. Though the participants at times seem to be revelling in a Mad Hatter's Tea Party of an argument, there is a clear method (or even slipping into the grandiose, a methodology) that consistently controls what they are doing. Both demonstrate that if one is simply clever enough, it is possible to find one's way to any desired conclusion. The secret is the same as that which guided Theseus through the Minotaur's maze. Simply follow the thread of reasoning back from the core con-

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[9] Humpty Dumpty famously declared that meanings are arbitrary, existing to be bent to the will of those bold enough to exert their rightful prerogative. In the topsy-turvy world of economics, firms may be efficient, equitable or bear neither of those characteristics, according to the perspective staked out by the relevant researcher.

> "When *I* use a word," Humpty Dumpty said in rather a scornful tone, "it means just what I choose it to mean—neither more nor less."

"The question is," said Alice, "whether you *can* make words mean different things."

"The question is," said Humpty Dumpty, "which is to be master—that's all." (Carroll 1974:193) clusion to its seemingly innocuous beginning. (Any useful Ariadne-like devices can be employed along the way and then abandoned at one's convenience.)

Implicit ideology structuring the shape and direction of an argument simply discovers what it already knows to be a self-evident truth. The two articles are chosen as stark examples of this inherent problem. The intention here is not to evaluate these two frankly fascinating articles with the intention of awarding prizes or apportioning condemnation. Rather, the goal is to understand how the authors strategically constructed and marketed their core belief, as well as exploring their fundamental objective in doing so.[10]

> He [Stigler] was absolutely sure the economy was on his side and if research was properly done it would show this. He really believed that he understood how the world works. And the way the world works had been shown to him by the theory of price. (Conversation with James Kindahl, November 1997)

[10] Making such a concise declaration of one's intent would not be necessary in an academic environment where authors are accorded a generous measure of goodwill, and which incorporates and extends an appropriate "benefit of the doubt" to the work in question.

> This means, on the one hand, that an economic writer requires from his reader much goodwill and intelligence and a large measure of co-operation; and, on the other hand, that there are a thousand futile, yet verbally legitimate, objections which an objector can raise … (Keynes 1973: 469–470)

Defensively declaring one's aims to deflect the inevitable brickbats tossed by antagonistic readers reflects the tendencies of ideologues of all shapes to almost purposely misunderstand objectives. In such cases, there seems to be an almost frantic scramble to locate a rationale for taking offence at the written word. Those most ideologically committed appear more likely to decry ideological bias when confronted by a viewpoint that conceivably runs strongly counter to the one they cherish. Authors then feel forced to telegraph their impetus for writing, in the vain hope that the inevitable white noise elicited might be kept to a minimum. Doubtless this shows an unwarranted degree of naïvety, a hope born to be disappointed.

It Was the Best of Times; It Was the Worst of Times[11]

> It was the best of times, it was the worst of times, it was the age of wisdom, it was the age of foolishness, it was the epoch of belief, it was the epoch of incredulity, it was the season of Light, it was the season of Darkness, it was the spring of hope, it was the winter of despair, we had everything before us, we had nothing before us, we were all going direct to Heaven, we were all going direct the other way—in short, the period was so far like the present period, that some of its noisiest authorities insisted on its being received, for good or for evil, in the superlative degree of comparison only. (Dickens 1859:1)

Academic papers, especially in a discipline that longs so painfully to be recognized as a near relative to the precise sciences, should ideally be able to escape from any and all of the historical contexts to which they are condemned. George Stigler, something of a godfather to both Alchian and Demsetz, argues categorically, as he was wont to do, that economic theory does not reflect specific historical events.[12] However, the Alchian

[11] As Dickens' Ebenezer Scrooge realized, nothing is ever quite straightforward. What one sees and how that vision might be interpreted is largely a function of perspective. Different economists can, in this fashion, quite honestly view a given market in a polar opposite manner. Consequently, reaching a specific conclusion depends on choosing appropriate initial assumptions.

[12] Stigler claimed that too much was made of isolated instances such as Ricardo and the campaign to rescind the Corn Laws. Economists operated largely insulated from the historical context in which they lived. Given this desire to analyse at an abstract and universal level, one of George Stigler's long-term objectives was his intention to transform and present economics in the form of an empirically based science. He deliberately and decisively broke with his teacher, Frank Knight, in this regard. Stigler seemed to envision an economic scientist resembling some variant of a neutral observer searching for truth, a professional remaining bias free, lacking any taint of self-interest. Assuming a critical absence of self-interest was painfully at odds with Stigler's core assumption that all human actions could be encompassed with the glaze composing simple, rational self-interest. Accordingly, given this depiction of the economist as scientist removed from the petty concerns of daily life, such researchers could not simultaneously function as prisoners enchained within their historical environment. Their theories could not then, for the most part, reflect a discernible response to the economic issues of the day. Nor could their ideological response to pressing policy issues peek through. The generation of, and progress made by, economic science, in Stigler's view, must instead reflect the inevitable resolution of interminably rational and internalized debates. Stigler makes this clear in his response to a letter from his close friend Robert Solow. Solow (October 20, 1959), had detailed a few prickly issues arising from a draft article by Stigler that was subsequently published (Stigler 1960). Stigler, while conceding a bit of ground to his friend, was not to be turned away from his inconvertible judgment.

and Demsetz paper appeared, with timing that belies coincidence, during an unusual period of turmoil on academic campuses worldwide. Clearly these ever-present conflicts represent an uprising that touched Chicago and other major universities, and one which could not fail to escape the notice of these two UCLA professors. Certainly the perception held by conservative faculty members, those most closely aligned to the Chicago project, such as the cohort based at UCLA, led them to feel under threat from the distinct left-wing surge prevailing not only among students, but also widely scattered throughout the younger academics of these institutions. In a looking-glass fashion, this perception paralleled an earlier threat to left-leaning academics during the McCarthy period of the 1950s.[13] (Though it could easily be claimed that during the Red

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> The trouble with your fine letter is that with characteristic subtlety you disagree with me. This unnerves me. You really want current events to have a larger effect than I do. Partly you evoke them as the original factor of production, and here I don't really disagree with the way you do it, but partly you are more immediate, as in the case of Keynes. He isn't ideal for my argument and I consider this a count against him, but wasn't he more influenced by the depressed twenties than the great depression? (Receptivity of the profession to new theories is affected by events but this is an almost separate question), and I can concede Keynes to you since I don't argue the never-never case: I would also certainly concede Ricardo on rent, and possibly Dupriet and the opening of the Vin Rouge Creek bridge. (Letter from George Stigler to Robert Solow, November 6, 1959)

[13] Ideology tends to color one's perspective. In some respects, it seems that those who are most motivated to defend freedom of speech are more likely to interpret that right as pertaining largely to speech with which they have prior and sympathetic leanings. A recent study by Epstein, Parker and Segal (2013) indicates the presence of, at times, a distinct bias in the judicial decisions of specific Supreme Court Justices. Freedom of speech may be championed according to a given predilection for the flavors of such speech (who is speaking and on what topic). Correspondingly, in the academic world, those staunch libertarians and conservatives who were most vociferous in castigating radical students and faculty for trying to impede contrary views in the 1960s were often quite complacent during the McCarthy period when left-wing colleagues were ejected from their tenured positions.

> But I once did a little informal investigation of whether people who were economic libertarians and tended to favour low taxation and low regulation and laissez-faire, were also people interested in civil liberties and freedom of expression, and that sort of thing. So I would ask innocent questions. "Now what do you think of this group?" Of course, I had a placebo question control group. "What do you think of the fact that this professor at the University of New Hampshire, the one who invited Paul Sweezy in the McCarthy era to give a lecture, is losing his job because neither he nor Paul Sweezy will testify as to what was the content of the lecture?" And Milton said, "Gee, it's a simple case. It's a free speech society. If a man will not do what he should, this professor should be fired. Society has a right to know." I said, "You don't understand. They've got the notes on the lecture, verbatim. It's not a question of information." (Conversation with Paul Samuelson, November 1997)

Scare, the threat was more of a reality than it ever would become during the years when would-be student revolutionaries widely roamed university campuses.) As Hegel pointed out centuries ago, it is impossible to escape the given environment in which one operates and into which one is thrust.[14] Academic economists need not be explicitly, let alone continuously, aware of this ever-present constraint. Nevertheless, acquired obtuseness might lead them, on occasion, to operate as though perfectly oblivious to their own entrenched ideological biases. However apparently perspicacious they might be, issues of this ilk will still manage to permeate, by some sort of poorly guarded back door, into their research and writing unless a scrupulous guard is cautiously posted.[15]

Specifically, groups like Students for a Democratic Society, and especially more radical organizations, toyed with Marxism or at least some easily digestible version of that doctrine. The evils of capitalism were dramatized. The plight of exploited workers and the greed of capitalists were lovingly depicted. The exploitation of the working class was evoked, employing emotive, if not downright colorful, tones.[16] Undoubtedly capitalism was under an intense internal attack in the US during this period, a thrust comparable only to the upheavals of the 1930s. Ironically during that earlier era, the loss of trust in the capitalist system was driven by a major economic depression. The 1960s featured a period of unanticipated wealth and well-being for a burgeoning middle class. Students seemed willing to protest against a system whose very success had apparently provided them with the ability to do so. The response from Chicago-type economists, and those allied at UCLA (teasingly

[14] Hegel of course put it more elegantly:

> Whatever happens, every individual is a child of his time; so philosophy too is its own time apprehended in thoughts. It is just as absurd to fancy that a philosophy can transcend its contemporary world as it is to fancy that an individual can overleap his own age, jump over Rhodes. (Hegel 1967: 11)

[15] Godzilla didn't randomly appear in Japanese films of the 1950s, nor did alien invasions in Hollywood films during that same era represent a piece of unexplained serendipity.

[16] In Marxian theory, the rate of exploitation is purely a technical term to explain the relation of the creation of surplus value and the disparity between the actual embedded labor in a given output and the compensation required to produce that output. However, it is a stretch to suppose that Marx did not choose the terminology fully aware of its emotive baggage, despite the neutrality of its actual exposition.

referred to as Chicago West) was to man the barricades.[17] Not only did the market system need to be defended against what they deemed spurious charges, but the positives needed to be refurbished and polished to a highly gleaming burnish. Spin was in the air. Given the conclusion to which Alchian and Demsetz inevitably arrive in their much respected article (if capitalism didn't exist, workers would rise up, demanding its creation and establishment), divorcing their arguments and the associated conclusions (a highly charged defense of private property) from that era's debates becomes an exercise requiring increasing dollops of self-delusion.

While appreciating the thrust and persuasiveness of their specific efforts, it is useful to remember that Alchian's Gertrude Stein stage (a market is a market is a market) neither begins nor terminates with this joint paper. Its roots go far back to an insistence that economic analysis need not, and should not, become distracted by what differentiates one market from another. All markets exist to facilitate exchange. For Alchian that is the intrinsic starting point (and perhaps the only point that matters).[18] Unlike his writing partner Harold Demsetz, Alchian was never officially associated with the University of Chicago, either as a student or faculty member (excluding his role as a visitor), but his positions, at times, were even more unyielding than his implicit role model, George Stigler.[19] After reading a copy of one of Stigler's papers, Alchian pointed out critically if not acerbically:[20]

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[17] One of the odder reactions to the student unrest of the 1960s was formulated by Milton Friedman. Noting that many of the student leaders were Jewish, he worried that this would somehow create a later blowback for the Jewish community as a whole. The parallel he was imagining here lay with Germany in the 1920s and early 1930s when communist supporters battled, sometime openly, with their Nazi counterparts. Despite this undeniable fact, it does seem a far reach to claim that anti-Semitism was significantly exacerbated by Jewish involvement in leftist movements. To maintain perspective, however, it might perhaps be best to remember that this was just a passing remark in a letter rather than a well-maintained position. Usually unarticulated paranoia has a propensity to make cameo appearances given an appropriate set of circumstances.

[18] The urge to reduce everything to a matter of markets and exchange is reminiscent of Robert Solow's reported quip about Milton Friedman and what might at times seem an obsessive focus on money. "Everything reminds Milton Friedman of the money supply. Everything reminds me of sex, but I try to keep it out of my papers." http://www.econlib.org/library/Enc/bios/Solow.html

[19] Paul Samuelson noted, "I think like Armen Alchian, who is more Catholic than the Pope, who never went to University of Chicago but is a real Chicagoan, he [George Stigler] does end up doing some simple macro theory (Conversation with Paul Samuelson November 1997).

[20] The paper in question (Stigler 1967) appeared in *Barron's* magazine, a business publication.

> But I do admit to an inability to detect the "moral responsibility to society" that you attribute to a university. I also admit to no privileged position for a shoe producer, a butcher or an advertising agency, the university has a product to sell. I care not if some of these universities propose to suppress certain ideas of their employees (who can go elsewhere) nor if the university has an ax to grind. I care not so long as the university is not a monolithic monopoly of the state—as it threatens to be. The access to the market for education, truth, falsity, shoes, wax, and what you will is what gives truth a chance to survive. Nor is it necessary—not even desirable possibly?—that truth be the goal of the agents that get it revealed. Your position strikes me as perilously close to advice to a paternalist agency. On the same grounds I see no reason to expect newspapers to have a responsibility to be honest. Let them lie as they will. I rely on access of others to expose the lie and to cater to the public's desire for truth—to whatever extent the public and individuals wish it. (Letter from Armen Alchian to George Stigler March 25, 1967)[21]

Though he would later move closer to Alchian's position, Stigler does feel moved to bring up one contentious issue in a short follow-up note. Clearly, Alchian is not bothering to question the likely presence of the required level of market competition that might make his position viable.

> The man who puts all his reliance in competition has to be sure he will get competition. In the absence of some kind of moral code, how do you prevent an Adolf Hitler from winning the election and excluding all competitors? (Letter from George Stigler to Armen Alchian, April 3, 1967)

Alchian's almost obsessive focus on competitive markets makes his work highly consistent, but also quite provocative. Much as was the case with

[21] There are a few interesting points and reflections here that should not go unremarked. As in his paper with Demsetz, switching costs go largely unnoticed by Alchian. Faculty members, like any other employee in the article under discussion, are free to find work elsewhere if dissatisfied with their current position. The choice is simply between exit and loyalty with no place left for Hirschman's (1970) third option of voice. Like the market for canned tuna fish, you either buy the can or you exit the market. The other striking point, which first appears in Alchian's breakthrough paper (1950), uses an evolutionary metaphor to evince the idea that competitive processes yield an optimal outcome.

Becker, conclusions are known a priori, but the journey to any of the predictable outcomes is usually fascinating and well worth the ride.[22] Though he is not the first economist to skilfully fit any data, theory or observation onto a convenient Procrustean theoretical framework, his very cleverness, when accomplishing this self-allocated task, leaves many readers unaware of the previously constructed ideological bed that underlies his basic analysis. This impulse to see the world through categorical lenses extends, unsurprisingly, to daily decisions as well as political policies. Thus during the student uprisings at UCLA in the 1960s, the issue for him was not one of academic freedom, moral codes or freedom of speech. For Alchian, the contending forces at play boiled down to an unresolved conflict over property rights.[23] Student protestors had been violating basic university property rights by seizing buildings and conducting unauthorized sit-ins. University administrators, in Alchian's adamant opinion, should not equivocate, but rather defend the centuries-old doctrine of private property rights that substantiated, as well as validated, markets, capitalism and civilized life. Without a willingness to display an appropriate backbone, authority would necessarily crumble, eviscerating the fragile basis underpinning the university as a functioning system.

> It is important the issue be clearly understood to <u>not</u> be how to meet the particular demands being expressed, but how to stop the profitability and growth of force and trespass and interference with university operations. The Regents I hope are aware of the importance of getting faculty support in advance; the faculty must be committed ahead of time to the acceptance of the use of the police to deny trespass, coercion and force in affecting

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[22] This, of course, is the problem posed in the paper. All three authors, whether consciously or not, seem to have started off with their conclusion and spent the rest of their efforts in constructing an argument to bridge the theoretical gulf impeding the desired path to their goal.

> It seems to me that when you get to his [Stigler's] later work, say with Becker, you know what the conclusion is going to be before you start the argument. In a sense, you're assembling arguments to support a conclusion. I mean, that may be unkind and untrue, but it's an impression. (Conversation with Ronald Coase, October 1997)

[23] Alchian was among the first economists to recognize the necessary link tying market exchange to property rights. His 1965 article remains a seminal addition to the field.

> university operations. (Letter from Armen Alchian to Glenn Campbell,[24] January 20, 1969)

Alchian also shares with Stigler an unwavering belief in, or at least willingness to assert, the total absence of bias in his own Economics department and a lack of ideological persuasion in the discipline itself. (It is possible that other more blighted departments have strayed from the approved scientific road, but certainly not those in Chicago or Los Angeles.) Though it is more than a bit ingenuous to assume that such bias, when it is operative, is always pursued at a conscious or deliberate level. Certainly discrimination in hiring practises is not always part of an evident strategy. It remains a quite unremarkable proclivity that economists would tend to find more to praise in work that accords with their own predilections rather than in research that challenges their fundamental beliefs and insights. Given two comparable candidates, the one whose work is more closely contiguous with the values of the existing department has a greater probability of being hired. Decisions are made in part on how convivial the person would be to have in one's department. It is likely that people holding similar positions on a number of fronts would be deemed a better fit than those more perpendicular to the programmatic thrust of the hiring committee. This reaction would not need to be recognized as a deliberate or conscious nugget of discrimination, but would come about quite naturally, operating at a more concealed level.

> Just as I have an instinctive doubt of the scientific integrity (or critical intelligence) of a Marxist economist, so economists with conventional views find it difficult to separate completely the scientific ability and policy

[24] W. Glenn Campbell served as director of the Hoover Institute on the Stanford campus from 1960 to 1989. He was selected for that position by former President Hoover who wanted his Institute delivered from the hands of left-wingers. Campbell, a Harvard-educated economist, proved to be the embodiment of a pair of safe hands. Not only were those straying from the more conservative line generally not found within its precincts, but Campbell re-established its finances, raising its endowment from $2 million to $135 million. The Institute's policy influence and reputation increased as well, especially during the Reagan years. (The two were in fact friends.) Under Campbell, the Institute became something of an extension of the Chicago School, with a number of its faculty members having some sort of affiliation or position at the Hoover Institute as well (Friedman, Stigler, Director and Becker, just to name a few).

> views of a conservative. This is no violent prejudice, and I know no good conservative economist who is vastly underrated. But I think a fair number are given somewhat less than their due. (George Stigler, letter to The President, Board of Trustees of Stanford University, 600 California Avenue, San Francisco, California, March 28, 1966)

The hiring committee can in turn rightfully claim that merit was, is and always will be the only basis for any decision without being grossly incorrect. Moreover, a bit of self-selection may also take place. Candidates recognize the differing characters displayed by a variety of departments and only choose to apply to ones they think will be conducive to their future work. In this way, without any deliberate intention, departments may end up with certain uniform views. Despite this pronounced tendency, departments will not be transformed into pockets composed of identical drones. Even during the height of the Chicago School under the yoke of Friedman, Director and Stigler, the hearts of the department failed to beat as one. But there were readily discernible elements held in common that became more evident as the years went by. Nonetheless, this insistence on mannered neutrality may have been deleterious in the long run. None of us can escape our ingrained ideology. But by recognizing it, we can hope to tame it sufficiently for it to only lightly color our output.

> In Economics, we have a positive science, one completely devoid of ethics or normative propositions or implications. It is as amoral and nonethical as mathematics, chemistry and physics. Whether a man believes in more government regulation of in socialism or in compulsory social security or in union strikes or opposes them does not in any way enter into the balance of his competence as an economist or teacher….
>
> Yet, and this is the point I would emphasise, we have been accused of being imbalanced because we do not have a strong vociferous group favouring certain social policies—say such as free tuition or the grape strike or greater public support for rapid transit, etc. etc. Because of that accusation of imbalance, men like Nutter are denied appointment to present even greater "imbalance". You can see why I dislike the epithet imbalance. It enables a coward to hide behind a term that means all things to all men and enables him to deny the pejorative meaning, when in fact that

> is precisely what he meant. (Letter from Armen Alchian to Glenn Campbell, January 20, 1969)[25]

Harold Demsetz was to a significant degree a reflection of the indisputable success of the construction of what he himself termed "Team Stigler."[26] Stigler looms large in both writers' academic careers. In fact, Alchian's recommendation no doubt helped encouraged Chicago to recruit this once and future colleague of Alchian.[27] Demsetz was to spend a formative period (1963–1971) as a member of the Graduate School of Business, home also to George Stigler.[28]

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[25] The question arises as to where Alchian's pointed, if not bitter, accusations were during the McCarthy era. Despite his professed neutrality, it would be interesting to uncover his (unwritten) analysis of the dynamics that occurred during that era. Perhaps, like Friedman and Stigler, his agreement with the need to defend liberty (embattled by the communist threat) overrode all other hesitations.

> But it wouldn't surprise me that he [George Stigler] had a schizophrenic attitude to attacks on intellectual leadership depending on which direction it came. A lot of right-wing people must have found McCarthy to be vulgar, brutal but less of a problem than the alternative. Basically he was on the right side of the issues. Not unlike what you read that people said about Hitler, in the early 30s, when he was on the rise. Yes, he's a gangster. Yes, he's a hooligan but he's anti-communist. He's going to keep back the communists and control trade unions. These are benefits. And they didn't realize that they were rearing a Frankenstein's monster who would eventually chop off their own heads. A lot of people were sympathetic, who would have been horrified if they had foreseen what would happen. (Conversation with Mark Blaug, April 1998)

[26] Stigler used Walgreen Foundation funds to bring Demsetz to Chicago from UCLA. Stigler, who was appointed Walgreen Professor in a successful effort to lure him back from his Columbia University exile in 1958, was at the time on one of the largest academic salaries in the USA. Walgreen funds enabled him to assemble a group of like-minded, but also highly skilled and accomplished academics, in Chicago. This is recounted by Nik-Khah: "… it brings to mind Demsetz's reference to the 'Stigler Team': and suggests just how important was his desire to build a research team" (Nik-Khah (2011).

[27] Like his colleague Armen Alchian, Demsetz (1967) would also write a key early article on the economics of property rights while at Chicago. The article (1972) examined here is an almost natural outcome of their mutual interests and the time at which the joint work was authored.

[28] Stigler held a joint appointment in both the economics department and the graduate business school. The author has written extensively on George Stigler, leading to the suspicion that everything he reads reminds him of Stigler. Consequently, he sincerely hopes this focus hasn't led to some unseemly, and possibly, obsessive behavior. However in the case of Alchian and Demsetz, to deny Stigler's lasting influence would be an exercise in sheer contrariness. As Demsetz himself admits:

> I have just received a copy of The Citizen and the State. In your introduction you give thanks to your colleagues including me now at UCLA. Clearly, thanks should go in the reverse direction, and I convey mine now. Whatever I may accomplish as an economist in

I was teaching here at UCLA from 1960 to 1963. Then my colleague who occupied the next office, Armen Alchian, apparently recommended me—said some nice things about me—to the people at Chicago. At the AEA meeting in Chicago, I met one of the people who were to become a colleague of mine and that was Reuben Kessel, who is no longer alive. Reuben asked me whether I was happy at UCLA. At that time I happened to give him just the right kind of answer for Reuben Kessel and maybe the people in Chicago, I said, "Make me unhappy."

So, they made me unhappy and I was invited to come to Chicago—to try to make me unhappy—and my first meeting with George Stigler was at a dinner I was invited to attend at Merton Miller's place. At that dinner, there were several other people there. I can't remember who was there at the time, but George Stigler wasn't there when I first arrived. There was a robust conversation going on around the table, and then this tall man, George Stigler, walks into the room. From thence forward the entire conversation at the table was from and focussed on George Stigler and what he had to say. I had never before been exposed to a personality who was so overpowering in conversation as he was. So that was my first meeting with George Stigler. (Conversation with Harold Demsetz, October 1997)

In addition to an abiding interest in neoclassical price theory, property rights and efficient markets, Demsetz shared with Alchian a deep-rooted contempt for what they both saw as the spineless administrative response to the student protests of the 1960s. Compare a Demsetz letter to George Stigler with the previous correspondence between Alchian and Campbell.

I can understand the faculty who object to bringing in the police for silly ethical reasons or because they fear the aftermath. But there are at least 15 per cent of the faculty who favour amnesty! And a significant percentage probably more than 15 per cent, who doubt the legality of identifying and disciplining the students through University machinery. (Letter from Harold Demsetz to George Stigler, February 7, 1969)

large part will be due to a certain stubbornness born of a childhood in the Chicago west side ghetto mixed with the very generous treatment accorded me over a seven year period by the brightest intellect I have ever known. (Letter from Harold Demsetz to George Stigler, October 14, 1975)

In contrast, Sam Bowles embodies, perhaps all too starkly, exactly what Alchian and Demsetz most despised during the student protest movement.[29] Younger than either of them, yet too old to be one of the student protestors, Bowles represents one of those young instructors who possessed the temerity to side with the student radicals. With his frequent co-author Herb Gintis and other fellow travellers, he was to reconstruct the economics department at the University of Massachusetts (Amherst) in 1973 by redefining its essence as a radical Marxist (or post-Marxist) enterprise. Trained in mainstream economics at Harvard, Bowles wished to deliver his more radical message in terms, and by using tools, that were impeccably neoclassical.[30] Bowles did not wish to be casually dismissed by the economic profession as someone constructing models that were too dependent on special cases or being attacked for a fatal lack of rigor.[31] Deploying the standard tools of microeconomic analysis to deliver a fatal twist on traditional neoclassical results allowed Bowles to speak to potential opponents in their own terms. This strategy also pre-empted the classical rhetorical ploy of shifting the terms of debate to more conducive geography.[32]

Given the background of the protagonists and the geography in which they operated, little room remains for surprise when we closely examine the two papers.[33] As we would expect, both take issue with what at the

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[29] On a more whimsical note, it sometimes seems as if a male's memory of those turbulent days of the 1960s is a strange function of the amount of sex enjoyed during that period.

[30] In essence, Bowles wanted to beat the economic establishment at its own game. You can almost hear Ethel Merman belting out "Anything you can do, I can do better" (from the 1946 musical *Annie Get Your Gun*) when reading his work.

[31] A strong argument can be offered that John Maynard Keynes assumed perfect competition in *The General Theory* to prevent any subsequent discussion from becoming side-tracked in the morass created by a charge of special conditions. Assuming oligopolistic markets, for instance, would most likely have created a heated debate over the applicability of such an assumption. Keynes, however, wanted to emphasize that even optimally efficient markets had a tendency, in aggregate, toward periodic downturns and recessions. In much the same way, Bowles did not want to depend on flawed markets as the core of his argument. This alternative path was taken by Gardiner Means who persistently started with an assumption of market and pricing power.

[32] Stigler became a master of this technique. Launching counter-attacks on non-orthodox pricing models, Stigler created enough straw men to populate dozens of revivals of *The Wizard of Oz*.

[33] Escaping the issue of ideology is probably impossible. These days, admitting to being an ideologue is roughly on a par with declaring membership as a card-carrying paedophile. To be more exact, it is always some opposing academics who are chained by their ideological beliefs. Few economists are motivated to perform such a *mea culpa*. Confession, though arguably good for one's

time were widely held beliefs. Both offer tightly argued models, whether formal or not. But the results of their respective frameworks are reliably predictable given their creators. Despite the rigor of their arguments, neither paper would sit comfortably if the authors were forced to swap their positions. Doing so would be equivalent to forcing a devout carnivore to become a vegan or vice versa.

Alchian and Demsetz were writing at a time when capitalism and corporations were being condemned, especially by the student population, for immorality and exploitation of their workers and of the environment. The two authors demonstrate, with cutting logic, that firms not only achieve a level of textbook efficiency, but they do so while also providing a surprising degree of equity for the system's participants. Had it not already existed, workers of the world would have united to demand the creation of capitalism. Conversely, a decade later, swimming against the tide coming in during the Reagan years,[34] Bowles takes aim at what is already a much cited paper.[35] Without explicitly naming his target, he turns the work of Alchian and Demsetz on its head. Under Bowles' strictures, capitalist firms are naturally unfair, but astonishingly inefficient as well. Bowles then puts forward a definitively mirror image of the Alchian and Demsetz world that his more conservative brethren so carefully construct. Neither paper provides an illegitimate argument, though

soul, is not similarly beneficial to one's career. However, a grievous mistake would be committed, if either or both of these papers were breezily dismissed as being simply ideologically motivated. Both are thought-provoking and make a genuine contribution to the discipline. Nor is it surprising that the Alchian and Demsetz paper, despite possible faults or shortcomings, is one of the most cited articles in the discipline. Papers are of interest not because they simply concur with one's own predilections. To categorize publications according to such a biased classification is to fall into an abyss of intellectual masturbation.

[34] In many ways the Reagan years represent the final triumph of the Chicago counter-revolution that had its seeds in the first meeting of the Mont Pelerin Society. For a feel of the self-congratulatory tone of that time, a lengthy, edited session of the stalwarts of that brigade discussing the growth of law and economics at Chicago makes for riveting reading (see Kitch 1983).

[35] In its 100th anniversary edition, *The American Economic Review* assembled a distinguished team of economists (all of whom were still seemingly alive) to compile a list of the top 20 articles to have appeared in that highly rated journal during its century of publication. The Alchian and Demsetz paper made that extremely exclusive list.

> In any case we were expected to use our judgment about quality and significance. So we used the citation and JSTOR data only to give us a large group of eligible. (Arrow et al. 2011:1)

both are flecked with potential flaws. The key factor in each paper is the designated initial assumption, the entry point, which serves as a jumping-off point for the rest of the delineated route of analysis. Swallow that one assumption, and the reader becomes a mere passenger on a logical express. The carefully crafted one-way ticket inevitably delivers you to the authors' pre-ordained destination. Given this detailing of the preliminary landscape, the task remaining is to forensically analyse exactly how each author accomplishes this sleight of hand.

The Zen of Tuna: A Shopkeeper's World View

> My personal view is that if someone holds a view, it cannot be dislodged by any conceivable empirical data. Evidence from a data system doesn't convince them. These people have made their decisions already. They've become true believers and no amount of empirical evidence will ever convince them by definition. (Conversation with James Kindahl, November 1997)

The two articles examined share, surprisingly, some common elements and points of agreement. Each intends to analyse the labor market as a means of ultimately evaluating capitalist firms. (Firms are formed to utilize labor in ways otherwise unobtainable.) In each case, the central players, employers and employees, respond to market incentives, namely relative prices. Within these two articles, we can easily identify the key price distortion lying at the heart of all the subsequent analysis performed by the respective authors. The nature of this distortion is what dramatically separates the two papers, dividing them as if by an unbridgeable gulf. However, in neither exposition is there any resort to opportunism as a motive, or creator, of incentives. Every economic player performs honestly within the role allocated, bereft of any desire to intentionally mislead. In neither the original exposition by Alchian and Demsetz, nor in the deft jujitsu performed by Bowles on this same approach, do these authors stoop to vilify either employees or even employers. Again, none of these authors are interested in becoming side-tracked by the introduction of extraneous qualifications into their essential analytical mixture. Each and every decision, then, must be, and in fact is, above board. The commonplace idea often attributed to mainstream economics that workers want to maximize their pay by doing minimum work, never rears its disagreeable head. In each case, workers are understood as honestly responding to the incentives provided by the relevant labor market. Bringing in the issue of opportunistic behavior, or maximizing by using guile, would simply muddy the argument each wants to make.[36] None of the authors in fact need to employ this sort of assumptive crutch to reach their desired conclusion.[37]

Whenever an analytical focus is concentrated on a specific market, a decision has to be made as to whether the specifics that differentiate this market from others might be of greater significance than the shared characteristics that equates a given market facilitating exchange with any other. Alchian and Demsetz adopt the Chicago approach, which fixedly focuses its gaze on that which binds markets together. For these authors, the labor market is no different than any other market where an exchange is struck between a buyer and seller, each of whom are equally

[36] Without this understanding, the role of the monitor in each paper tends to get muddled with the more common Williamson (1975)-style idea of preventing opportunism. Resorting to more standard mainstream tropes, all economic actors performing in these two papers are essentially honorable (in terms of marketplace conventions). They pursue their self-interest, but without guile or any deliberate intention to deceive. The idea of getting "something for nothing," or of being overpaid, does not enter into any individual calculation.

[37] For some unknown reason, whether through innocent carelessness or from more nefarious objectives, Bowles lumps the Alchian and Demsetz (1972) paper together with that of Williamson (1980). The result is a jumbled category he designates as being a conclave of neo-Hobbesians, after the dour seventeenth-century political philosopher. This is an easy, but unhelpful generalization to make. The authors may all employ the convenient idea of shirking, but hardly in a shared sense or for the same purposes. For Williamson everyone is inherently opportunistic, always on the look-out to discover, or create, a rent-seeking opportunity. He amends the standard assumption of self-interest to "self-interest seeking with guile," (Williamson 1975:255). Shirking then occurs because of inherent and unchanging human behavior. This psychological assumption forms something of a bedrock foundation for most of Williamson's theory, much as the concept of original sin is intrinsic to Christianity. But for Alchian and Demsetz, no condemnation should attach to cases of apparent shirking. Workers can hardly be vilified for responding to market incentives. (They would fail to be economically rational if they did otherwise.) The appearance of shirking is simply a function of distorted relative prices, not evidence of the "Fall of Man." Correct the imbalance and the shirking effortlessly falls away.

> I refer to these models as neo-Hobbesian because according to them the key to understanding the internal structure of the firm is the concept of malfeasance. Also known as shirking or free riding, malfeasance gives rise to the archetypal Hobbesian problem of reconciling self-interested behaviour on the part of individuals with collective or group interests. (Bowles 1985:17)

capable of accepting or rejecting any potential contractual arrangement. Neither party exerts any power over the other given a total absence of any authoritarian control. This assumption is presented as unarguable, something that goes without saying and needs no further explanation, a total of three times throughout the paper. Most authors are unlikely to feel compelled to present the same assumption that many times by sheer coincidence alone. The surprising repetition of this identical claim provides more than a clear hint of the essential role that this key contractual relation plays in the architecture of the authors' argument. It first appears on the article's initial page as:

> Telling an employee to type this letter rather than to file that document is like my telling a grocer to sell me this brand of tuna rather than that brand of bread. I have no contract to purchase from the grocer and neither the employer nor the employee is bound by any contractual obligations to continue their relationship. Long-term contracts between employer and employee are not the essence of the organization we call a firm. My grocer can count on my returning day after day and purchasing his services and goods even with the prices not always marked on the goods—because I know what they are—and he adapts his activity to conform to my directions to him as to what I want each day … he is not my employee. (Alchian and Demsetz 1972:777)

The essence of the labor contract is clarified and emphasized some six pages later:

> The relationship of each team member to the *owner* of the firm (i.e., the party common to all input contracts *and* the residual claimant) is simply a "quid pro quo" contract. Each makes a purchase and sale. The employee "orders" the owner of the team to pay him money in the same sense that the employer directs the team member to perform certain acts. The employee can terminate the contract as readily as can the employer, and the long term contracts, therefore, are not an essential attribute of the firm. Nor are "authoritarian," "dictational," or "fiat" attributes relevant to the conception of the firm or its efficiency. (Alchian and Demsetz 1972:783)

In case our minds have wandered during the course of a lengthy, complex and wide-ranging argument, the same point makes a return appearance as the paper nears its conclusion. The assumed undeniable logic of the statement is for the third time etched into the reader's mind:

> The central agent is called the firm's owner and the employer. No authoritarian control is involved; the arrangement is simply a contractual structure subject to continuous renegotiation with the central agent. (Alchian and Demsetz 1972:794)

The argument for this assumption is presented so casually and at the same time so confidently that few readers would pause to weigh its validity.[38] However, this seemingly innocuous assumption forms the unyielding base upon which rests the logic of their economic model. Acceptance of this contention makes the conclusion become something of an inescapable anti-climax. On closer examination, it is not clear that this key claim holds any water, or that it might only be valid under very restrictive conditions. In fact, though forcefully presented, the claim is far more problematic than it would appear after an initial reading. Hidden within this simple assumption is a claim about the opportunity cost attached to the decision made. In each case, whether for the employer or employee, switching costs are minimal. Therefore each faces the other armed with an equal degree of bargaining ability. Why this particular contractual aspect should exist as a universal feature attached to all labor markets is neither considered nor properly explained.

Alchian and Demsetz become almost obsessive in their insistence that the employer/employee relationship is not defined by a power nexus, but is rather an uncontroversial market exchange between equals. Unfortunately without this assumption, their core argument starts to crumble. So it is no surprise, as previously noted, that they not only emphasize this intrinsic balance in the labor market at the beginning of

[38] Having used both papers in seminars and tutorials featuring both undergraduate and graduate students, I can attest that this particular assertion tends to whizz by under the radar for almost all of these readers. They neither see the essential role played by such a claim nor initially find anything startling or contestable about it. This of course is the rhetorical role such an assumption was designed to play.

the article, but then feel compelled to dredge it up toward the halfway mark only to make, almost reflexively, one last repetition as they bed down the argument. Notice the rhetorical ploy they utilize to advance the plausibility of their contention. What they might mean by employing the term "power" is never properly defined. In other words, the authors surreptitiously suggest that what is meant by power is known so obviously to each and every reader that there is no exigent need to waste any additional precious time by explicating. They come close to implying that spending time defining or analysing the concept would be something of an insult to the intelligence of those self-selected readers.

Rhetorically this strategy enables the authors to make an end run around a topic which actually turns out to be quite complex. If we examine the issue critically, in economic terms at least, "power" becomes a term fraught with difficulties. Loosely defined it would seem to indicate the control by one party over the choices made by another. Choice, however, is expressed in economic terms by the opportunity cost attached to each specific decision. Thus although the opportunity cost of foregoing an alternative may be very high, a choice still exists. Constructing a set of circumstances where an economic agent has literally no choice is not so easily imagined.[39] All that an economist can honestly reply, then, is to admit that certain choices have such high opportunity costs attached that few would rationally opt for those alternatives.

Therefore, since it is impractical to think of someone completely controlling another's choice, the authors must logically associate power with the idea of being able to influence the decision made by another. Following this particular line of thought, it would then become more likely that one person would make a choice that benefits another if a linked high opportunity cost was somehow attached to the alternative. In these terms, a power relationship in the labor market might occur when

[39] These circumstances can be illustrated, if in a somewhat flippant fashion, by referring to a classic comedy sketch. Jack Benny was a noted comedian (vaudeville, radio, television) in mid-century America. The comedy character he created was that of an extreme skinflint. This fictional trope was the basis for a number of gags including an instance where Benny is held up one night. The crook brandishing a gun mutters, "Your money or your life!" This is followed by an excruciatingly long pause, dead air on the radio, the listening audience imagining Benny looking reflective. The crook, running out of patience, finally grumbles, "Didn't you hear what I said?" Benny then replies, "Don't rush me. I'm thinking."

the opportunity cost to an employer of having an employee quit is notably lower than the cost to the employee of being fired by the employer. This reasoning, though, skirts the issue of how great the disparity has to be before it represents a power relationship. The Alchian and Demsetz solution is to make the opportunity cost of switching near zero for both employer and employee. Implicitly we are operating in a world where the transaction costs associated with the most essential labor market operations have for practical purposes nearly vanished.[40] Without explicitly specifying the circumstances, Alchian and Demsetz have forcibly located their readers in an environment where markets can perform in an optimal fashion. The unresolved issue, then, is how closely such an abstract specification adheres to observable labor markets. This train of logic might represent a leap of faith that ultimately obscures some essential qualities characterizing labor markets and differentiating them markedly from simple product-style alternatives.

Certainly, by adopting the Chicago style of discourse, we can claim that what is important is not the reality of this key assumption, but the results of testable hypotheses derived from such a starting point.[41] But while theoretically viable, in practise, observable results may often

[40] Demsetz (1969) himself has inveighed, if not scoffed, at this variety of idealization elsewhere. It is difficult to determine whether the version presented represents what the authors see as a useful approximation of reality.

> The view that now pervades much public policy economics implicitly presents the relevant choice as between an ideal norm and an existing "imperfect" institutional arrangement. This *nirvana* approach differs considerably from a *comparative institution* approach in which the relevant choice is between alternative real institutional arrangements. (Demsetz 1969:56)

[41] This somewhat instrumental "as if" approach is usually attached to the methodology propounded by Milton Friedman (1953). However, the largely silent partner in this attempt, not so much to investigate, but to curtail future debates centered on the reality of assumptions, was Friedman's close friend George Stigler (1949).

> I had written the methodology paper, which was later formally published. This preceded, by three or four years, the earlier versions. And he refers in one of those lectures to the fact that we had been talking about it. … I think there's no doubt that my work would have been different if I hadn't been influenced by George and George's work would have been different if he hadn't been influenced by me. (Conversation with Milton Friedman, August 1997)

diverge from this ideal. Data inevitably must be interpreted.[42] It never speaks for itself. If an assumption exists to lead an argument to a desired end, it seems unlikely that data cannot also be brought to heel, though not in any fraudulent manner, with that self-same result.

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[42] Empirical results which clash with generally accepted theories are often strenuously resisted given the potentially heavily ideological position attached to any given outcome. Added to this is the standard issue of sunk costs. For senior members of the profession, a not inconsiderable sunk-cost investment has been committed in terms of both research and teaching. A classic example is revealed by the seemingly unresolvable debate forever circling the issue of setting a minimum wage. The ideological issues here involving the nature of the labor market and the role of government are manifold. Those providing empirical research that seemingly shows that raising minimum wages does not necessarily yield substantial or even any job losses inevitably infuriate parts of the discipline. Certainly David Card underwent a test by ordeal when his empirics indicated that an increase of that type was consistent with an actual increase in employment.

> To what extent does the simplest model of supply and demand actually describe how employers operate in the labor market? That model says that if an employer wants to hire another worker, he or she can hire as many people as needed at the going wage. Also, workers move freely between firms and as a result, individual employers have no discretion in the wages that they offer. In contrast to that highly simplified theoretical model, there is a huge literature that has evolved in labor economics over the last 25 years, arguing that individuals have to spend time looking for job opportunities and employers have to spend time finding employees. In this alternative paradigm a range of wage offers co-exist in the market at any one time. That broader theory is, I think, pretty widely accepted in most branches of economics. The same idea is used to think about product markets where two firms that sell very similar products may not charge exactly the same price. The theory explains a lot of things that don't seem to make sense, at least to me, in a simple demand and supply model. (Clement 2006:2)

The resulting fury and backlash against Card made any further balanced discussion difficult, if not impossible. Some economists reacted as if their daughters had been defiled.

> I've subsequently stayed away from the minimum wage literature for a number of reasons. First, it cost me a lot of friends. People that I had known for many years, for instance, some of the ones I met at my first job at the University of Chicago became very angry or disappointed. They thought that in publishing our work we were being traitors to the cause of economics as a whole. (Clement 2006:3)

Sherwin Rosen, perhaps Chicago's premier labor economist during the height of this controversy, reflects an attitude within the profession that simply dismisses any evidence contradicting some firmly lodged belief.

> What that means, I don't know. I can't tell you why he [George Stigler] changed his mind. I don't know how his mind worked. How would anyone who didn't know him very well in those days be able to make even an intelligent guess? [laughs] I don't know how people change their minds. I don't even know how I change my mind. I'm an old man now. I know a lot of stuff I've learnt over the years. So, if someone comes up and tells me now that everything I know is wrong I tend to be defensive. I naturally believe that the claim is probably erroneous … given my investment, given what I've read over the years. When somebody tells me now that an increase in the minimum wage increases employment, there's just been a study out on that, I'm very skeptical of that claim. I don't believe it! (Conversation with Sherwin Rosen, October 1997)

> The interesting thing is he [George Stigler] was a great enthusiast for quantitative methods. … So, it doesn't seem altogether consistent. But he certainly was. On the other hand, he knew what the answer was going to be. He just regarded it, as I say, as a way of persuading other people. (Conversation with Ronald Coase, October 1997)

One key implication of erasing any trace of economic power or authority from the labor market is the dissolution of each and every potential conflict of interest between the two parties.[43] In other words, both buyer and seller in this contrived market can strive to achieve their objective without interfering with any opposing pursuit. This case clearly prevails when purchasing a can of tuna fish from the local grocer or supermarket. Failure to meet the implied promises of the exchange on the part of the seller (inferior quality or damaged goods) would lead the buyer to cancel any future contracts with that grocer. In a competitive marketplace (as assumed here) such a grocer would quickly arrive at an unsustainable position. Equally, a buyer who attempted to avoid the payment agreed on would also not be welcomed back. Future contracts would be terminated based on a lack of good faith. In this sense, markets are self-regulating given the fundamental absence of any serious conflict of interest in this contractual relationship.

Again, equating labor contracts to those incurred in a weekly shopping expedition translates into a total absence of any long-term contractual arrangements. "Long-term contracts between employer and employee are not the essence of the organization we call a firm" (Alchian and Demsetz 1972:777). Employer/employee relations are reduced to this essential

[43] If in fact private economic power is denied, or at least reduced to an undoubted periphery of concern, then attention is not distracted from government power, which for Chicago tacticians represents the seat of essentially all economic malfunctions. Though rightfully focusing on government failures, these practitioners seem to employ something akin to a one-sided scepticism. Unfortunately, like governments, markets are also a human creation and almost by definition similarly flawed. Sweeping generalizations are more often bound to mislead than to instruct. This tendency to ignore the inherent flaws in a favored approach is naturally not restricted to those on the political right. Left-leaning economists are often prone to make an equal, if opposite, error of one-sided skepticism.

arrangement, namely an almost continuous series of spot contracts, each attached to a potential termination agreement. Either party is able to end the ongoing tie with equal ease. The position presented is at the very least superficially persuasive and certainly tactically effective in delivering the targeted objective.

Before delving into the fundamental conflict that almost spontaneously leads to the development of a capitalist firm, we need to establish what is driving each of those employees when entering into a contractual arrangement.[44] In economic terms, then, it is crucial to grasp the exact choice each worker faces and how he or she manoeuvres to maximize the utility to be gained from working. In this contractually determined environment, employees voluntarily adjust their work, in terms of effort expended, according to the opportunity cost of pursuing leisure on the job (shirking). In both articles this pursuit of leisure is articulated as a decision to expend something less than their potential effort (the amount of labor these employees would produce under a different price-setting regime).[45] Given this mindset, shirking for Alchian and Demsetz is not symbolic of any degree of laziness, dishonesty or irresponsibility, but rather exists as an expected (and fair) response to a set of distorted price signals.[46]

In standard labor market analysis, workers, as mentioned, adjust the effort they contribute according to the opportunity cost of enjoying leisure. More formally, they set their individual marginal rate of substitution of leisure for consumption at the prevailing market wage offered.[47]

[44] Readers can view this preliminary framework as establishing the article's ground rules concerning labor markets and exchange cultures. Doing so enables the authors to start their long ascent of clarifying (or perhaps transforming) the relation between specific property rights and their resulting production outcomes. Essentially the aim is to create something akin to a useful fable (or even a likely story), one that supposedly embodies the origins of the firm under market capitalism.

[45] Employees shirk by "goofing off," to use a vernacular term. In any given hour, part of it is spent not doing one's assigned work.

[46] The simple rule of thumb is that it is foolish to blame individuals for responding correctly to a given set of price signals. (If workers tended to ignore the relevant opportunity cost of leisure, economists would be compelled to classify them as irrational and thus outside the analysis offered by the discipline.) The issue is whether or not such signals are in fact distorted.

[47] By assumption, labor itself produces only disutility. The advantage of working lies in the income earned, which can be easily transformed into consumption. The stored consumption that income represents measures its utility, rather than the pure monetary value of the associated earnings at any given moment.

At this margin, forgoing an additional dollar of wages yields as much additional utility in increased leisure as does the additional utility of earning that wage and using it to increase that individual's consumption.

$$\text{Mathematically: } (\partial U / \partial \mathrm{H}) / (\partial U / \partial C) = w$$
$$\text{where: } U = \text{utility; } H = \text{leisure; } C = \text{consumption and } w = \text{wage}$$

However, a competitive wage must in equilibrium be equal to the value of the marginal product produced by labor: $w = \mathrm{MP_L}$. From this equilibrium condition we can conclude, in the absence of any external price distortion, that workers will put in a level of effort up to the point where their marginal rate of substitution of leisure for consumption is equal to the value of the marginal product produced by their labor. From here it is simple to comprehend the circumstances under which either more or less effort is expended than this optimal level. (Notice that in both articles this result represents a fair repayment for labor. The worker is repaid the market value of his or her output.) Departures from this relative price or opportunity cost of leisure will lead to either more or less effort being expended. By making the opportunity cost of leisure cheaper, maximizing workers will have no acceptable alternative other than to expend less effort by consuming more leisure. Alchian and Demsetz label this condition "shirking." In direct contrast, by raising this opportunity cost higher than the undistorted level, workers will optimize by expending more effort than they would otherwise choose to commit.

Given these basic fundamentals of price theory, the depth of the virtual abyss dividing these two papers will ultimately depend on establishing whether firms distort or correct the relative opportunity cost of leisure. Making such an assignation must then determine whether these capitalist firms are the heroes or villains of the stories told. Having universalized labor contracts as being no different than any other market exchange, Alchian and Demsetz are obliged to explain the derivation and existence of firms as an evolved institutional response to a distorted labor market, an innovation that corrects an otherwise intrinsic price distortion.

What they must demonstrate is the way in which an assumption of equal exchange between employer and employee leads to the required correction instituted by the capitalist firm. In this transition, property rights, particularly private property rights, represent the lynchpin of the model presented. The specific device proven capable of accomplishing this objective is what the authors define as a "centralized contractual agent." Using common parlance, this individual would more normally be designated as the owner/manager of a firm. The challenge facing the authors is to show how establishing the role of a boss (the owner/manager) is beneficial to all parties concerned.

> Wherein then is the relationship between a grocer and his employee different from that between a grocer and his customers? It is in a *team* use of inputs and a centralized position of some party in the contractual arrangements of *all* other inputs. It is the *centralized contractual agent in a team production process*—not some superior authoritarian directive or disciplinary power. (Alchian and Demsetz 1972:777–78)

To cement this far from obvious connection between team production and a central contractual agent, the authors resort to spinning an ingenious "just so" story.[48] Here, the striking parallel with what might be categorized as Hollywood history (the past as re-imagined by screenwriters) may instantly come to mind.[49] However, while screenplays sacrifice historical accuracy on the altar of cinematic pace and potential ticket sales, economists construct less than accurate fables in order to produce anticipated conclusions. Since the early steps of any theory are perhaps the most crucial, forging ahead requires a look at how the nature of property rights prevailing in any exchange determines the efficiency and equity of a given outcome.

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[48] Economists have employed these illustrative stories from the time of Adam Smith and his imagined "deer and beaver" economy. The temptation to do so can be at times overwhelming.

[49] Examine, for instance, the Tom Cruise version of Japan's Meiji Restoration depicted in *The Last Samurai* (2003). Samurai, basically sword-carrying accountants, are somehow romantically equated with the Sioux tribes inhabiting the nineteenth-century American West. A number of other historical fantasies can also quite easily be spotted by those wishing to make the required effort. (For the idly curious, take a quick glance at http://www.hnn.us/article/2746 for a collection of some of the more amusing howlers.)

Property Is Theft: The Essential Role of Property Rights in Economic Outcomes

> Economists are, I think, too prone to examine exchange as a cooperative act whereby the buyer and seller each act in an effort to reach a more desired position. Yet I find it more interesting … to examine the competitive, or property, aspect of exchange. The act of exchange is a means whereby the buyer is able to compete against other claimants for the goods being obtained from the seller. The kinds of offers, forms of competition and behavior that the members of society can employ in an endeavor to get more of the goods that would otherwise go to other people, is brought more into the focus of attention. More directly, the forms and kinds of property rights sanctioned in a society define or identify the kinds of competition, discrimination, or behaviour characteristic of that society. (Alchian 1965:817)

The pivot of their argument ultimately depends on connecting team production with property rights. What the authors propound has at least a superficial degree of plausibility. Firms do employ a number of workers who cooperatively produce a given output or service. But Alchian and Demsetz pull off something of a sleight of hand by defining team production in such a way that property rights lie at the structural heart of an efficient firm. In their world, team production is a specific form of joint production. The simultaneity of the effort expended essentially entails an inability to assign a given output to a given effort. This quandary is strategically wrapped and presented as an intractable measurement problem. An individual cannot receive his or her marginal product, if it is impossible to accurately measure that effort by observing the final outcome.

> With team production it is difficult, solely by observing total output, to either define or determine *each* individual's contribution to this output of cooperating inputs. The output is yielded by a team, by definition, and it is not a *sum* of separable outputs of each of its members (Alchian and Demsetz 1972:779).

The authors cleverly provide a baby step toward this intuition with the example of two men lifting heavy cargo onto a truck. The cargo has to be lifted together and all one can observe is a result, namely the cargo on the truck. Viewing that final result of course says nothing about how much effort each individual exerted toward that final end. According to this logic, it would be impossible to provide the two men with compensation that exactly rewarded each according to the effort expended. But this problem seems something of an artificial construct. With only a two-man team involved, each partner would be well aware if the other individual was not pulling his weight. Perhaps each individual contribution wouldn't be precisely equal, but probably sufficiently close that pragmatically such purported measurement issues would fail to matter.[50] In fact, as the article develops, the measurement problem arises only when the team is sufficiently large that members of the team are incapable of determining who is doing what. This then poses the real problem with their core idea of team production, namely the applicability of this concept. For instance, assembly lines and many other types of production are sequential rather than simultaneous, which would seem to limit the reach of the Alchian and Demsetz argument. The authors deftly sidestep this issue and instead at least create the impression that their concept of team production tends to dominate economic activity. Without that implied conceit, the artfully constructed fable that efficiently links team production to a central contractual agent (and plunges us into the realm of private property) simply crumbles away.

Ceding, however, their basic presumption focused on this metering issue, the set-up of the problem then flows quite easily. A team of workers must cooperate to produce a given output. Property rights of any production are owned collectively by the given team. Without any way of properly measuring individual contributions, returns are simply divided equally between the given team members. This approach essentially generates a classic moral hazard problem whereby individual partners in this enterprise fail to meet their implied obligation. However, this failure

[50] When the article later turns its gimlet-like focus to other team structures, partnerships, under certain circumstances (small numbers, skilled observation required), are deemed a superior alternative compared to firms. Otherwise, their own article (a partnership) would have inevitably become bogged down in unseemly disputes over who was carrying most of the productive load and come to nought. (Academics are in fact well acquainted with such squabbles. However those partners who are not seen to pull their weight, one way or another, are seldom asked to embark on any future joint work.)

simply stems from the inevitable under-pricing of leisure time (in this case time spent slacking off). If individual effort could be accurately measured, then team members would produce a level of effort up to the point where their marginal rate of substitution of leisure for consumption is equal to the value of the marginal product resulting from their labor (as explained above).

However, because of the prevailing property rights, any drop in total output due to an individual measure of slacking off will be borne by each and every member of the team (less output means lower returns for their labor). But, the utility gained from this slacking off will be enjoyed only by the individual who decides to reduce his or her effort. With the opportunity cost of procuring additional leisure under-priced in this way, a team member will rationally respond by consuming extra leisure. With each member acting accordingly, output will fall considerably. As a result, collective property rights will turn out to be inherently inefficient under a wide range of circumstances. Team members themselves are assumed to view the situation as unsatisfactory and inequitable. The authors assume that these individual workers would prefer to put their effort into an environment where relative prices are not distorted in this fashion. They just desire a fair return for their labor. In the fable created, a solution would inevitably be sought which would satisfy all concerned. Doing so requires an institutional structure that would remove these distorting incentives. Since the problem lies in an instance of asymmetrical information, only the individual worker knows precisely what his or her effort might be; a viable solution would require a resolution of this underlying information problem. Fortunately, all that is essentially required is a change in property rights, according to the two authors. Individual effort must be observed independently, coupled with the appropriate incentive to perform such observational duties effectively.

> One method of reducing shirking is for someone to specialize as a monitor to check the input performance of team members. But who will monitor the monitor? One constraint on the monitor is the aforesaid market competition offered by other monitors, but for reasons already given, that is not perfectly effective. Another constraint can be imposed on the monitor: give him title to the net earnings of the team, not of payments to other inputs (Alchian and Demsetz 1972: 781–782).

The logic is simple, but more subtle than it would appear. A team must shift from being defined by collective property rights to falling under the designation of private property rights. In other words, someone must serve as a monitor to observe each member's individual accomplishments. The monitor in turn must be provided with a sufficient incentive to perform this task effectively. This objective involves assigning certain property rights to that self-selected individual.

> It is this entire bundle of rights: (1) to be a residual claimant; (2) to observe input behaviour; (3) to be the central party common to all contracts with inputs; (4) to alter the membership of the team; and (5) to sell these rights, that defines the *ownership* (or the employer) of the *classical* (capitalist, free-enterprise) firm (Alchian and Demsetz 1972:783).

The obvious contention is that the assigned monitor would wish to see the hired workers of the team put in extra effort (not to slack off) since doing so would produce more output and thus a greater surplus to which he or she is the residual claimant. The point of confusion is why this set of private rights induces workers to stop their opportunistic behavior or at least reduces the frequency of such a strategy. The usual response of those who fail to read the article carefully is to conclude that workers now are in fear of losing their jobs (a resort to the "big stick" as a compelling incentive). Yet as we read further and more carefully, it becomes clear that this cannot be the case since no real cost of job loss is posited. Neither party to the labor contract is assumed to possess any negotiating power (remember the tuna fish example). Workers can exit a team, either of their own volition or at the request of the owner/monitor, and simply shift to another team, much as a tuna-fish fancier could opt for another supermarket or the grocer find another customer. The key here is not fear, but rather the demise or drastic reduction of asymmetrical information. The newly self-appointed monitors can now observe the ability of each team member and allocate positions within the team so that output is maximized. The monitor is also obliged to reward each team member for the observed work performed. If less is offered, the team member has the option to leave and join a team where his or her full worth is properly valued or perhaps become part of a team that is more conducive to his or her skills. The team member who demands more than he or she is worth (according to performance) will encourage the monitor to exercise the available option of replacement.

Given a world where switching costs are either zero or negligible, a sorting process will proceed in such a fashion that teams will rearrange themselves until each has the optimal mix of members producing at a cost-efficient level of output. Otherwise, there would be an incentive for owner/monitors to rearrange their teams and/or for workers to migrate to better opportunities. Improved information thus facilitates market efficiency. Shirking-type behavior rapidly dwindles given the change in incentives. With leisure no longer undervalued, workers lack any reason to indulge in excess consumption of that particular good. The firm then, in the form of private property, corrects the inherent price distortions produced by the original presence of collective property rights. One other result is undeniable. Since all are rewarded according to their results, each worker is allowed to put in the degree of effort that would produce a desired level of reward. The system is without question equitable, since each individual only seeks to be paid for his or her actual contribution.[51] The startling result then is that not only does the firm, by correcting the pre-existing price distortion, create a world of efficient production, but also manages to achieve an equitable outcome as well.

Clearly the authors assume that:

- Leisure will decrease as the real wage forgone by such slacking increases. (In the model they can no longer spread the loss to other team members.) This result will hold as long as the substitution effect of the real wage forgone is greater than the attendant income effect. Such a requirement would need to be demonstrated empirically rather than argued theoretically.

[51] Here it is wise to skip discussions centered on whether market valuations of an array of productive outputs are fair or not. If one accepts that, in a competitive market with a given distribution of income, consumer responses are the best economic guide to value, the equity conclusion underlying the Alchian and Demsetz article must follow.

- Wages are market determined in a harmonious manner with employers and workers (at least over time) reacting correctly to market signals. The existence of competitive labor markets with minimal switching costs are a necessary (though not sufficient) condition for the Alchian and Demsetz analysis to hold. Otherwise, an optimal labor market equilibrium cannot be guaranteed.
- Equitable and efficient outcomes in the labor market require competitive markets where wages reflect productivity.
- There is no place for government intervention in labor markets since capitalist firms deliver results that are both efficient and equitable. Though economic agents are intent on only pursuing individual self-interest, unimpeded labor markets resolve equity issues dealing with income distribution in a manner that precludes the need for labor market policies.

An argument that would provide a stronger defense of capitalism and the capitalist firm would be difficult to imagine. The popular rants against "the system" almost ubiquitous at the time of publication would then appear to be shallow and illogical, based on some flimsy anecdotal evidence at best. The appeal of such arguments is reduced to the emotional rather than resting on any solid logic. The authors, however, do not stop there. At the conclusion of the article they allow themselves to indulge in what might seem to be no more than a bit of whimsical speculation, but on consideration does have much deeper implications. The firm has appeared in the guise of an internal private market. Individuals pursue accurate information in such a market because they are fully rewarded for any such efforts.[52] The fruits of these endeavors need not be shared with others as may often be the case within public markets. Therefore the superiority of the firm over the market may lie in the fact that in one case, information remains private property, while in the other, such information must be shared. Thus their near obsession with the superiority of private property is now entirely complete.

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[52] Under a private property regime, information garnered remains private as well with benefits fully limited to those gaining and controlling such information. The leakages representative of a public market fail to occur.

> We conclude with a highly conjectural but possibly significant interpretation. As a consequence of the flow of information to the central party (employer), the firm takes on the characteristic of an efficient market in that information about the productive characteristics of a large set of specific inputs is now more cheaply available. Better recombinations or new uses of resources can be more efficiently ascertained than by the conventional search through the general market. In this sense inputs compete with each other within and via a firm rather than solely across markets as conventionally conceived. … Could it be that the market suffers from the defects of communal property rights in organizing and influencing uses of valuable resources? (Alchian and Demsetz 1972: 794–795).

Waiting for the Revolution: The Attempt to Reconstitute the Legacy of Marxism

> "It is possible … that it may not come, during our lives … We shall not see the triumph."
>
> "We shall have helped it," returned madame. (Monsieur and Madame Defarge in Dickens 1859:357)

For Sam Bowles, the key to denouncing capitalism, specifically when the system is encapsulated by the privately owned firm, is to demonstrate conclusively that leisure, when viewed correctly, is in practise deliberately overpriced under a regime of private property. Although, as in the previous article, property rights do not command center stage, gathering the full glare of attention, they consciously remain the underlying basis for all subsequent analysis. Under Bowles' guidance, this very same private property regime is reflected in such a way that firms shift from being saviors, in the world constructed by Alchian and Demsetz, to being something approximating the villains of classic melodrama. The cleverness of Bowles' (1985) constructed universe lies in how he stage manages his interlocking scenes to reach, in an inevitable fashion, his duly designated destination.[53] The architecture of his argument reveals a clever bit of effective navigation. As reflected by the methodology guiding Alchian and Demsetz, Bowles' model pivots on one crucial assumption that drives the dynamics of the paper. Given the contrasting agendas of these authors, the revelation of diametrically opposed assumptions carries with it little, if any, surprise.

Remember that it was the imposition of collective property rights that generated the persistent problem of moral hazard for Alchian and Demsetz. For Bowles, it is the institution of private property, the very foundation of the capitalist firm, that generates similar difficulties. This property relation, in the world of Bowles, creates a classic agency relationship between the two classifications that lie at the heart of any given firm, namely owner/managers (employers) and workers (employees). That such an arrangement must inevitably create a clash is Bowles' starting assertion, a mirror image of the Alchian and Demsetz insistence on a non-authoritarian contractual relationship. Without a shared objective, a common point of self-interest, conflicts of interest will repeatedly occur.

For Bowles, as was the case for Alchian and Demsetz, workers seek to maximize their utility according to the marginal conditions previously stipulated. These prevailing conditions provide a core building block for any microeconomic analysis of the labor market. Unfortunately, there is no reason to expect that the utility-maximizing effort desired by workers will coincide with the equivalent profit-maximizing level demanded by the corresponding owners. The only constraint, from the viewpoint of employers, limiting these demands for increased effort (aiming at producing additional profit) is simply the cost of achieving that extra

[53] As previously cited, Ronald Coase notes this habit among some of his Chicago colleagues. Clearly a strategy that backtracks in this manner can be found in the work of economists who embrace the entire range of ideological positions.

> It seems to me that when you get to his [George Stigler's] later work, say with Becker, you know what the conclusion is going to be before you start the argument. In a sense, you're assembling arguments to support a conclusion. I mean, that may be unkind and untrue but it's an impression. And, it's even more so in the work of Richard Posner. Have you read any of that? It seems to me that the plot is always the same, and the characters stay fixed. It's the same thing, to some extent, one might say about Becker. But his work is so very good. And you learn so much from studying it, that that element in it is not a problem. (Conversation with Ronald Coase, October 1997)

effort. In the workplace that Bowles constructs, only a surprising and rather random piece of good fortune would identify that sought-after utility-maximizing level of effort with an intensity which simultaneously maximizes profit.[54] This irreducible conflict, then, is based on the institution of private property (an owner/employer hiring workers). For Bowles, logically and in practise, ownership must inevitably produce a power relationship.[55] Labor is hired to maximize profit in this analysis, without any necessary responsibility to maximize the welfare of the labor so employed.

> The distinctiveness of the Marxian microeconomics with respect to the neo-Hobbesian and Walrasian approaches, as we shall see, has little to do with the labor theory of value, however. Its primary focus is on the interactions between the voluntary relations of the marketplace and the command relationships of the workplace. Thus Marxian economists take strenuous exception to Paul Samuelson's assertion that "in the competitive model it makes no difference whether capital hires labor or the other way around". (Bowles 1985:17–18)[56]

This underlying conflict creates a discernible difference in comprehending the nature of the labor contract and the actual application of that contract. For Bowles, borrowing (or at least dressing up his analysis by employing) some Marxian vestments allows a distinction to be made between labor power and labor itself. The employer purchases labor power (the potential to deliver work) and must induce the worker to actually produce the required effort. From this perspective, labor is the desired output and like any other production process requires a set of requisite inputs to yield a chosen objective. In the absence of any conflict (in the world of Alchian and Demsetz), no separation between striking a labor contract and enforcing it exists. In both papers a moral

[54] Quite simply, Bowles denies that there is any market mechanism that would reliably resolve this inherent conflict.

[55] The insistence by Alchian and Demsetz that no power relationship can exist between employer and employee is matched in intensity by Bowles' assertion of the directly opposite case.

[56] Both articles assume competitive markets, so the opposing viewpoints are never confounded by the distractions produced by the presence of market imperfections. All three authors are intent on packaging their argument in the most marketable fashion possible. The focus of their difference lies in whether or not labor markets are based on an intrinsic conflict of interest.

hazard problem arises due to a property rights issue, but in one case it is a collective property rights regime that causes the subsequent distortions, while for Bowles the misfortune of private property produces the unequal balance of power that generates this fatal conflict. The consequence of this incommensurability disturbing the heart of capitalism is clearly announced as one of three propositions nailed to the proverbial ideological church door, allowing Marxist terminology the leeway to use such metaphorical ports of entry.

> … capitalists (owners of firms or their representatives) will generally select methods of production which forego improvements in productive efficiency in favour of maintaining their power over workers. For this reason, the technologies in use in a capitalist economy, as well as the direction of technical change, cannot be said to be an efficient solution to the problem of scarcity, but rather, at least in part, an expression of class interest. (Bowles 1985:18)

Clearly, raising the price (the opportunity cost) of leisure (reduced effort or slacking off) will produce more work effort.[57] Achieving this, then, is intrinsic to the process of producing a profit-maximizing level of actual labor. Bowles carefully delineates the two basic inputs that will achieve such an outcome. (In other words, like any other output, labor must also have its own specific production function.) The two factors identified by Bowles both depend on the creation of involuntary unemployment or, in Marxist terms, a reserve army of the unemployed.[58]

> … involuntary unemployment is a permanent feature of capitalism central to the perpetuation of its institutional structure and growth process. In a capitalist economy, product and labor markets will not function so as to eradicate Marx's familiar "reserve army of the unemployed." Moreover, even public policy towards this objective will be unable to maintain full employment. (Bowles 1985:18)

[57] Notice that on this point both articles agree. The unresolved issue is whether increasing the opportunity cost of leisure decreases or increases any effective price distortion.

[58] For Alchian and Demsetz there may be the possibility of some brief, transitory unemployment due to search costs, but this would specify an instance of being between jobs and more likely to fall into the voluntary category.

The lack of any serious opportunity cost attached to losing one's job rendered the employment contracts representative of the Alchian-Demsetz world devoid of any attendant power relations. However, if losing current employment represents a definite loss of income, for either a short or extended period, as is the case in the world constructed by Bowles, contractual relations must undoubtedly change. Transforming unemployment into a reality creates a world driven by fear. Workers are now prodded to produce additional effort since the cost of being discovered slacking off has become increasingly serious. At this point in his argument, Bowles is obliged to locate and explain the inputs required to produce labor as an output. He reveals these factors to be the way in which unemployment is created, and the method for discovering whether a worker is attempting to avoid putting in the requisite level of effort. (Taken together, the probability of losing one's job combined with the probability of finding another essentially explains the method by which employers artificially inflate the price of leisure. Confronted by this modern day Scylla and Charybdis, these employees opt to produce additional labor in order to secure their jobs. By using such methods, capitalist employers operating in this Bowlesean world manage to squeeze more effort out of each and every employee.)

Wages that are set above the market clearing level must create an excess supply of available and willing labor.[59] The higher the wage, the larger the pool of unemployed workers will be, thus raising the potential cost of job loss.[60] Workers will then exert greater effort since the price of leisure rises

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[59] In the model carefully calculated by Bowles, an ineradicable conflict of interest between employer and employee is the inevitable partner of a capitalist labor market. Such a market is defined by a deliberate level of involuntary unemployment.

> Given a positive cost of surveillance and a conflict of interest between employer and worker over work effort, the wage rate offered by the competitive profit-maximizing employer will exceed the worker's next best alternative. This is possible in general only if the probability of reemployment is less than one. Therefore, labor market competition cannot clear the labor market. Correspondingly, market clearing—the absence of involuntary unemployment—implies labor market disequilibrium. (Bowles 1985:31)

[60] The greater the pool of unemployed workers, the smaller the probability of re-employment will be, given the loss of one's job.

with every increase in the level of involuntary unemployment.[61] This single factor alone is, however, insufficient. A potential loss is not an effective threat unless there is the possibility of being observed putting in less than the required level of effort. The cost of job loss needs to go hand in hand with the actual potential for losing one's job. This necessity brings in the second factor of labor output production, namely the surveillance that will yield the requisite information on work performance.[62] Thus the more monitoring of this sort is performed (by investing in equipment and surveillance staff), the greater the probability that if a worker attempts to slack off, that worker will be discovered not fulfilling his or her contractual duty. Thus any combination of surveillance with a given cost of job loss will increase the price of leisure and as a result produce greater labor effort. In this way the relative price of leisure is distorted to produce higher profits for the employer.

The train of logic in Bowles' model becomes inescapable at this point and very familiar to anyone conversant with second-year microeconomics. Different combinations of surveillance and cost of job loss will yield

[61] Described here is a version of the "efficiency wage theory" which developed in the 1980s. Fear is the driving force in Bowles' model, as it is in many of the other variants. However, Akerlof (1982) presents an alternative which at least seems founded on a recognizable empirical foundation. His approach is firmly based on what is an undeniably observable bit of human psychology (though not the sort usually spotlighted in economics). Treating employees well, including providing reasonable remuneration, creates an obligation on the employees' part to deal fairly with their erstwhile employer. This sort of reciprocity differs sharply from the usual fear-driven story relayed by other economists. Both contentions would appear to have a role to play in operative labor markets.

[62] Owner/monitors observe workers to gain information in the world depicted by Alchian and Demsetz. However, the nature of the information sought is different than its role in the Bowles alternative. In the world defined by Alchian and Demsetz, it poses no threat since the cost of losing one's job remains minimal. Instead, information gathering is a vital component of knowing how to form the most efficient team (who belongs and exactly where they belong). Firing becomes simply part of a greater sorting process equivalent to the right of an employee to quit one team and join another. Thus in both depicted versions of the labor market, information is crucial, but for very different and almost diametrically opposite reasons. For Bowles, monitoring is not really driven by any informational imperative. To directly negate the Alchian and Demsetz purposive world, Bowles assumes all employees are identical, endowing each one with the same set of inherent trade-offs and propensities. Monitoring is done simply to attach a cause-and-effect impetus to the firing process. Otherwise, the fear of job loss would be entirely nugatory. The firing of interchangeable workers could be logically performed entirely at random. (No justifiable reason exists to fire one and not another.) Doing so, however, would fail to provide any rationale to put in additional effort on the factory floor. Individual employee decisions would cease to affect the probability of job loss. The connection between performance and job retention would be effectively shattered.

the same output of work effort. A lesser possibility of being observed slacking can be counter-balanced by a greater cost of job loss and vice versa. This line of thought enables the theorist to construct isowork (isoquant equivalent) curves as well as combinations that will attract the same cost for the employer (isocost curves). To maximize profit, no matter how much labor will ultimately be needed, the employer needs to produce that given level of effort at the minimum cost possible. Lining up all minimum cost combinations for every level of potential labor effort produces the familiar expansion path that torments every undergraduate student. Bowles produces a simple graphical representation (Fig. 2.1) as a compelling visual aid. Remember that his objective, as stated initially at the beginning of the article, is to provide a rigorous condemnation of capitalist firms based on private property. The figure is meant to offer substantiation of his claim.

Bowles constructs his theory by initially assuming the basic injustice of labor relations under the sway of the capitalist firm. (Everyone knows capitalist firms are exploitative.) The fundamental conflict of interest must mean that owners prosper at the expense of their employees.

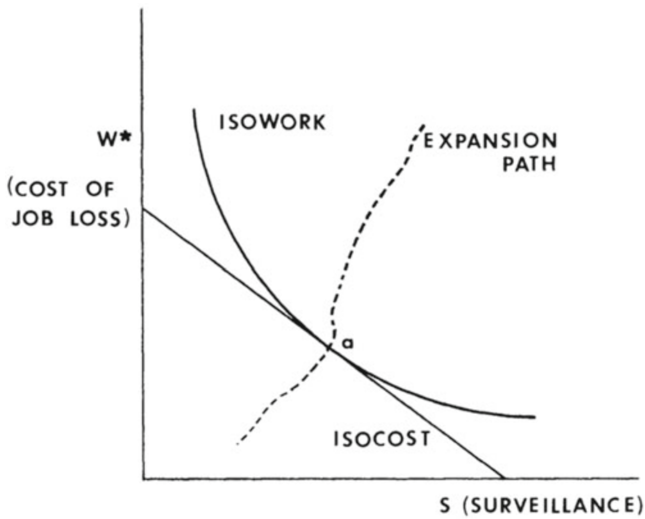

Fig. 2.1 (Bowles 1985:28)

Forcing them to put in more effort than would maximize their utility defines a world of inequity for Bowles. Prevailing property relations yield an environment where workers are squeezed so that capitalists may profit. Bowles' proposed ineradicable conflict of interest manages to encapsulate these sentiments. Where his cleverness shines is by demonstrating the implications of a model built on conflict. Not only are the property relations that provide the foundation of such firms inequitable, but they are also inefficient. Thus Bowles manages to deftly reverse the Alchian-Demsetz formulation. In their world, lacking any implied conflicts, and devoid of any resulting power relationships, firms produce outcomes that are not only efficient but also equitable for owners and workers alike.

How then does Bowles manage this logical twist? To reach his preferred objective, he must focus on the idea of technical efficiency, namely producing a given output using the least amount of scarce inputs. The graphic model above clearly demonstrates that capitalist firms sacrifice efficiency for profit, at least at this micro level. Thus due to the primacy of private property rights, a capitalist economy fails to reach its productive capacity. As Fig. 2.1 reveals, given the costs of spending another dollar by increasing surveillance inputs, or spending another dollar to increase the cost of job loss by increasing wages, a capitalist owner will seek to minimize the cost of producing any level of work effort by choosing the combination represented by point *a* on Fig. 2.1. Note that this minimizing combination simply exhibits the usual characteristics observed in standard microeconomic theory. The marginal rate of substitution of the two inputs equals their relative costs. (Visually this is represented by that tangency of the isocost curve to the isowork curve.) Unquestionably such a choice would minimize costs. But Bowles, having set up his knock-out punch, now strikes by claiming that though cost efficient, such a choice is not technically efficient.

> I will say that the capitalist has chosen an inefficient technology when there exists some other method of production that, per unit of output, uses less of at least some input and not more of any. The logic of the concept of capitalist technology is that a technology that is inefficient in the above sense may nonetheless be cost minimizing if it allows the capitalist to lower the cost of some input. (Bowles 1985:33)

All he has to do to achieve this objective is to demonstrate that it is possible to achieve the same level of output using fewer inputs. Accomplishing this particular agenda turns out to pose no problem. If we simply shift left from the combination represented by point *a* on the visualized isowork curve by using fewer surveillance inputs and instead providing higher wages, we would be able to produce the same output, but instead employ fewer physical inputs (fewer real resources). Thus the cost-minimizing combination *a* cannot also represent a technically efficient choice.

> Starting at point *a*, were the firm to move along this isowork locus by raising wages and cutting surveillance inputs, the cost of labor would rise and hence the profit rate would fall, but output per unit of input would rise … This result arises because there is a tradeoff between surveillance and the wage rate in the labor extraction function, and while surveillance inputs are resource-using, the wage rate is not; hence raising wages and lowering surveillance may be efficient but not profitable. Thus cost minimization and efficiency do not coincide: the tradeoff in this case is not efficiency vs. equity, but efficiency vs. Profitability. (Bowles 1985:34)

The logic here is that a reduction of surveillance inputs frees up physical factors that can be used elsewhere, while paying higher wages is simply shifting the prevailing distribution of income. Thus the drive by capitalists to maximize profits implies that it is inherently impossible for these firms to produce efficiently. This surprising result is both audacious and clever, whether or not a reader is ultimately convinced.[63]

After so much clever footwork, Bowles' conclusion does lack punch. Technical efficiency provides the platform that allows him to claim that institutional change in the form of collective property rights would

[63] Bowles in the article fails to explain how, if firms choose not to maximize profits, factors would be allocated between existing firms, or more broadly how factors of production would be allocated within a given economy. Nor is the basis for deciding between raising wages and employing surveillance inputs completely clear. Though Bowles continues to suggest that significantly less monitoring would be needed given an alternative set of institutions (other property rights regimes), he never goes so far as to claim that no monitoring would be required or that the possibility of a cost of job loss would cease to exist. Doing so would forcibly enrol Bowles in the world constructed by Alchian and Demsetz, a world where no conflict of interest would prevail. However in Bowles' case this would be achieved through collective property rights, not by the domination of a private property regime.

accordingly shift each isowork curve closer to the origin. A firm could then produce the same output while using fewer surveillance inputs (as well as creating a lower cost of job loss for that matter).[64] This outcome, according to Bowles, would redeploy those workers who would otherwise be employed in surveillance, increasing total output. Yet it does seem that even accepting this particular scenario, Bowles would still require some cost of job loss (involuntary unemployment) under his preferred institutional arrangement. Certainly on a macro level, the inherent requirement for involuntary employment cannot be evaluated as being efficient.[65] So, though striking, Bowles in his concluding remarks seem to be pulling his punches. He might be interpreted as saying that as we move ever closer to ideal institutional arrangements with ever improved collective property rights the need for surveillance and thus the cost of job loss dwindles ever close to zero. He does refer to a few empirical studies, but here Bowles may be resorting to the type of *nirvana* economics (or wishful thinking) previously described and dismissed.

Whether a reader fully embraces either position, it should be clear that Bowles presents a perfect mirror image of the model so carefully constructed by Alchian and Demsetz. For the latter, collective property ownership distorts the relative prices of labor and leisure. Establishing private property rights (and thus establishing a private market place) corrects this distortion and yields an environment where both employers and employees are free to choose. The result is an economy that is both efficient and equitable. Monitoring is done to gather information, not to exert control, since under private property arrangements the labor contract does not

[64] In an extended footnote Bowles grapples briefly with the question of why the clear advantages of worker cooperatives are not viewed as opportunities in a given economy. He does however indicate where his sympathies lie.

> To the extent that attitudes toward work are determined by an entire nexus of social institutions which change slowly, the opportunities for the atomistic movement towards a less socially irrational form of production are quite limited. Perhaps more important, because workers' own assets are not extensive, their access to credit is limited or costly by comparison to that enjoyed by the owners of firms … And it might be added that, perhaps for some of the reasons outlined in this paper, and despite the obstacles outlined in this note, the last decade has witnessed a substantial growth of workers' co-ops and worker managed firms in the United States. (Bowles 1985:41ftn.32)

[65] Bowles is providing an improved system of property rights, but not a perfect one. In this sense, at least, the unwelcome whiff of *nirvana* economics is partially forestalled.

embody any fundamental conflict of interest. Given their beginning, Alchian and Demsetz can only arrive at this pre-ordained conclusion.

For Bowles, however, private property rights remain the intrinsic problem, creating an unavoidable conflict of interest that distorts the relative prices of labor and leisure. The result is a system that is neither equitable nor efficient. Institutional change, shifting to collective property rights, could correct the relative price distortion and yield improved results whether viewed from a technical or equity position. What is striking, and perhaps tips the hand of the respective authors, are the inevitable policy implications. If labor markets largely reflect the conditions constructed by Alchian and Demsetz, any government intervention in the bargaining process could only lead to regrettable results. If the market is efficient and equitable, improving it through regulation seems improbable. In contrast, if lodged within the heart of all labor markets is an indigestible conflict of interest; the argument for government agencies to intervene in order to rebalance bargaining power becomes much more compelling.

Policy Patterns: Why Ideology Matters

> Among all forms of mistake, prophecy is the most gratuitous. (Eliot 1872:85)

Ultimately these papers provide very different versions of the labor market while meandering through the thickets surrounding matters of property rights. Which vision may veer closer to an observed version of reality would seem to be a purely empirical matter. In the Gertrude Stein model offered by Alchian and Demsetz, labor markets tick along in an efficient manner, satisfying each and every constituent. For Bowles, in his deployment of a more Dickensian vision, labor not being directly purchased must therefore be produced. In this production process, inequities and inefficiencies arise. Clearly one view of how labor markets work precludes government intervention in any shape or form while the other seems to plead for it. Policy then pivots around the interpretation chosen. Unfortunately, it is questionable whether any set of empirics will ultimately settle this issue, despite the compelling logic of doing so.

Ideological viewpoints inevitably color the way in which such markets are viewed, observed and analysed. Policies, when ultimately announced by a given government, will rely on economic research when an effort is actually made that is conducive to its ideological leanings. If the market place for ideas worked effectively in matters economic, ideology would not of course matter. In the absence of such reliable market outcomes, ideology can be decisive, posing problems for policy makers, let alone voters.

Constructing arguments in reverse, making the appropriate assumptions to reach desired results, can be dismissed as just another convincing form of rhetoric. Selling logically tight arguments is the essence of effective marketing. Or from a slightly different perspective, both papers are producing the strongest cases possible and allowing the market place for ideas to adjudicate the optimal resolution of these conflicting visions. There is, unfortunately, a less sanguine way to look at the dilemma posed by these two papers. They merely provide ammunition for those with a priori positions, failing to cause any serious reconsideration due to the partisan nature of their contrived initial assumptions. Were economists less concerned with packaging their articles to gain maximum recognition, more doubts could creep into their work, together with a possible acknowledgement of any limitations due to pre-determined outlooks.

> Now what they [Fuchs, Poterba and Krueger (1998)] found was the difference in their policy recommendations were—I'm using your language, not their language—ideologically premised values. They were not fact driven. Now there are a few cases like the minimum wage or Ricardian comparative advantage where you can almost get certain unanimity, free of ideology. But these are exceptions in my opinion. (Paul Samuelson, conversation with Craig Freedman, November 1997)

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The Heart Is a Lonely Hunter: Chicago's Climb to Glory[1]

It might not be a perfect system; nothing was perfect; but what he objected to, was, the insertion of the wedge. Under the Prerogative Office, the country had been glorious. Insert the wedge into the Prerogative Office, and the country would cease to be glorious. He considered it the principle of a gentleman to take things as he found them; and he had no doubt the

[1] I feel obliged to acknowledge a number of individuals who have helped to shape this chapter and made it possible. Many notable academics, including Milton and Rose Friedman, Aaron Director, Armen Alchian, Harold Demsetz, Arnold Harberger, Claire Friedland, Gary Becker, Ronald Coase, Sherwin Rosen, Stephen Stigler, Lester Telser, Sam Peltzman, Paul Samuelson, Robert Solow, John Kenneth Galbraith, James Kindahl, Mark Blaug and Paul Sweezy, were willing to surrender their valuable time to answer my questions and to reminisce. Too many are no longer with us. Wisely, none of them have been asked to endorse the author's scattered thoughts. More than a few would undoubtedly find fault with the ideas contained in this chapter. I would also like to thank David Colander and David Levy for many valuable conversations over the years. They cannot unfortunately be held responsible for failing to shake any delusionary ideas I still cherish. I would also like to recognize two somewhat anonymous referees for reminding me of how difficult it is to communicate with readers. Failure in this regard always remains mutual. Lastly, and most importantly, I would like to acknowledge the essential role played by John D. Rockefeller. Without his philanthropy in establishing the University of Chicago, this chapter itself would have been clearly pointless. However, I seriously doubt that were he alive today, he would have any interest in reading the results of my analysis.

C. Freedman, *In Search of the Two-Handed Economist*,
DOI 10.1057/978-1-137-58974-3_3

> Prerogative Office would last out time. (Charles Dickens (1850) *David Copperfield*)[2]

These days, economists talk about the Chicago School as though the meaning behind that phrase was both common knowledge and unquestionably understood.[3] But if those words represent anything more than

[2] The Dickens quotes throughout this chapter reflect the stylized perception, if not caricature, that the profession has nurtured of the Chicago School these many years. In his novels, Dickens' romantic focus on social injustice inevitably finds the greed and callousness of the business place to be at the heart of human misery. Martin Bronfenbrenner, a contemporary of Milton Friedman and George Stigler, provides a useful depiction of how the profession viewed Chicago during those decades following World War II.

> I never heard of any "Chicago School" until I left Chicago. I thought of my teachers and my older fellow students as good economists, not as members of a sect, or cult, or clique. Shortly after leaving the Midway however, I encountered the term full force. It was usually used pejoratively, especially when I was included in the membership. On the banks of Lake Mendota [University of Wisconsin, Madison], for example, "the Chicago School" meant Pangloss plus Gradgrind with touches of Peachum, Torquemada plus the Marquis de Sade thrown in as "insulter's surplus." (Bronfenbrenner 1962:72)

[3] "Chicago School" throughout this article refers solely to its post-war manifestation, particularly in the period from the late 1950s to the end of the 1970s. Prior to this period, and certainly during the interwar years, it would be much more difficult to categorize such a heterogeneous group of academics by employing a single label. In fact, when it appeared that Paul Samuelson was going to move to Chicago in the late 1940s, there were those in the department who feared that the Keynesian influence would soon dominate.

> The Samuelson matter was again forced to a head by Douglas—& thanks mainly to his efforts we lost badly. The dep't has voted to make Samuelson an offer. (Letter from Milton Friedman to George Stigler November 27, 1946 in Hammond and Hammond 2006:46)

The actual formulation specifying, the "Chicago School" was more of an oral tradition that didn't explicitly make an appearance within the profession until a controversial 1962 article by Miller was appropriately published in the *Journal of Political Economy*. Miller distinguished between the interwar group of Knight, Simons and Viner, and the post-war group led by Friedman and Stigler. This distinction has generally been accepted by those who have since dissected this school of thought. However, in the same issue, George Stigler (1962) provided a tart, and deliberately ingenuous, two-page reply that essentially dismissed this categorization as somewhat less than useless.

> My basic objection to Miller's statement, however, is that it does not really investigate the thesis that there is a "Chicago School." He has not described either a unifying ethical or political philosophy or an articulate and reasonably specific policy program. Instead he has merely sketched, less than completely, the views of my friend Milton Friedman. Friedman is clearly one of the most influential economists in the United States, if we mean by influence the affecting of people's thinking rather than merely its popular expression. He has not been ignored at Chicago, but I believe his influence on policy views has been greater elsewhere than here. If he is to be given a geographical base, a practice for which there is a good English precedent, I suggest that he be called the leader of the Berkeley-Cambridge axis, better to identify his locus of influence. (Stigler 1962:71)

an empty formulation, the assured attribution buttressing that meaning lies in the ghosts of theories now well past their shelf-life. The type of rock-ribbed price theory that formed the wedge Milton Friedman and George Stigler adroitly used to shatter the immediate post-war consensus no longer sufficiently differentiates the clear-water approach of Chicago from the salt-water recipe characterizing Harvard or Berkeley. Like any other discipline or profession, economics moves on. After years of cross-breeding and mutually dependent influences, such a clear-cut distinction belongs to the past rather than the present state of the profession. Bloody battles and past triumphs are left to historians of thought to dissect. However, what stubbornly persist are labels and oral traditions that are often built upon misunderstandings and fractured fairy tales handed down from one generation to the next.

> … we know little of how traditions get established, while it seems clear that once established, a tradition does not get changed through calling attention to its absurdity, or that of the factual assumptions upon which it rests. (Knight (1955) as quoted in Coats 1963:492)

Now, with most of the key antagonists residing permanently in the long run that befalls all economists, even those residing on the South Side of Chicago, it is time to take stock and understand the lasting effect that the Chicago School, with Friedman and Stigler[4] forming its

Bronfenbrenner in a post-war review of contemporary economics does refer to "The so-called Chicago School of economic policy, whose intellectual parent is Frank H. Knight, but whose best-known publicist is Henry C. Simons …"(1950:487). Naturally he does not make a distinction between old and new Chicago which would be similar to the anachronism of referring to the Great War as "World War I" in the 1920s.

[4] In his article, one of a series appearing in a precisely appropriate memorial to George Stigler (*The Journal of Political Economy*), Gary Becker aptly characterizes the duo. "Stigler and Friedman became Mr. Micro and Mr. Macro at Chicago during their two decades together." (1993:762) Stigler himself saw it somewhat more acerbically according to his wont. "Unlike some people who have really specialized one way or the other, I think he's [Friedman] covered both fields. I, on the contrary know very little macroeconomics, and I thank God for it every day, because it changes once a year, and that's an enormous strain to put on a person." (Stigler 1985:4)
In the 1960s and 1970s, when the Chicago counter-revolution made its greatest gains, the guiding lights, due to the force of their personalities, ideas and research were a particular gang of three (Friedman, Stigler and Director). Director, though not so obviously at the forefront in terms of

adamantine hub, had on economics and the profession that claims to advance that discipline. To do so, we need to go back in time and see the

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maintaining a public persona, exerted his influence informally on other members of the school. To imagine another group leading a similarly successful counter-revolution at Chicago would require a significant stretch of the imagination.

> I think he [George Stigler] had this belief, but yet he did have a great impact on the kind of work that people around him did. He was very influential in that way and he was very influential within the profession, particularly with respect to his insistence on testable propositions and gathering evidence to bear on those propositions. There is a lot more done on that than there used to be and largely because of George's evangelism on this point. (Conversation with Harold Demsetz, October 1997)

> There aren't too many people who can draw a circle around them, the way he [George Stigler] did. I think it's vanished from intellectual life. …I don't get the impression that there are many people like that in economics today. Maybe there never were. [laughs] … Friedman had a very huge influence on his students, but he was also a very difficult person to deal with, in a much different way than George. He was a much different type of person than George was. I don't know if Milton attracted as many people around him. He attracted his share, but not in the same way as George did. George was just a lot different … He was the smartest man I ever met, without doubt, the best working economist that there ever was. He was just the smartest, the very smartest man. (Conversation with Sherwin Rosen, October 1997)

Robert Van Horn, in a number of insightful articles (including Van Horn 2010) has shed light on Director's sphere of influence. Certainly, apart from Becker, no one else influenced George Stigler to a greater degree. It does seem ironic then that there is a need to emphasize and acknowledge the degree of diversity at Chicago when their core doctrine of rational choice focused on the commonality of all markets whether explicit or implicit. While a Robert Solow could object to Becker's extension of the calculus of markets into the more sociological fields of crime and marriage, at Chicago there was a general willingness to push price theory to its applicable limits.

> Gary [Becker] is certainly able to produce "explanations" of some observed facts about marriage as consequences of his assumptions. My problem is that almost each time I can think of equally plausible or more plausible explanations in terms of other motivations and institutions. (Robert Solow, letter to George Stigler, November 13, 1972)

However, this in no way implied that all markets were absolutely identical, but that the commonality linking them was more significant that the differences distinguishing them. Similarly, it should not be necessary to state that obviously the members of the economics department at Chicago were not clones of one another. There were, though, clearly a few key ideas that were held in common and dominated much of the work done there. The objective here is not to deny that diversity, but for the purposes of this chapter to hone in on some key common themes. Other authors are free to dissect all and any differences. However, the "us versus them" aspect of Chicago during those years was sufficiently explicit that someone like Robert Solow would be instantly typecast as an interloper when confronted by a host of true believers.

> There was a sixty-fifth birthday party for George that the University of Chicago put on, and my wife and I flew out there to be there and I remember that we were all put up in a University building, the Center for Continuing Education which had bedrooms. And we came in one evening and the next morning we walked into the dining room where all the other guests at this party were having breakfast and a hush fell over the whole dining room. And, finally a friend of mine, I think it was Si Rottenberg from the University of Massachusetts, came over and said, "We were all wondering 'what are you doing here?'" (Conversation with Robert Solow, November 1997)

way in which the strategic refusal to yield a theoretical inch to opposing forces,[5] an unbending adherence to the tenets of market fundamentalism and a rare flair for rhetoric and marketing could move an initially much ridiculed approach into the consensual mainstream of economics.[6]

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[5] Both Friedman and Stigler were known for their obstinacy and reluctance to admit error or change their mind. Their natural inclination seemed to be to defend their ground with whatever tools came to hand.

> And I think Milton quietly changed, he just quietly dropped that [100 % reserve ratio]. He doesn't particularly announce changes in positions, but instead, lets them just decay away. (Conversation with Paul Samuelson, October 1997)

> I never got a note from George saying "Well, this time around I've got to admit I was wrong. And your reading was right." Not at any time. And a lot of people would tell me that if they wrote to him complaining about something, he would answer something like "Well, if you're the kind of person who believes that, then you're just the kind of person who believes that." (Conversation with Paul Samuelson, October 1997)

In fact, Stigler was wont to portray changing one's mind in a negative fashion and obstinacy as a virtue.

> A second and related trait of scholars is that they seldom change their minds. We must remember that we are discussing scholars with good minds, and they would lose in a swap of minds. (Stigler 1988:210)

What he would not seemingly countenance is a shift from sound to unsound doctrine. This can be gathered not from reading Stigler literally, but understanding how he wrote and expressed himself. Throwaway remarks are often slyly tongue in check, reflecting a dry and very sardonic sense of humor. Statements that would seem innocuous if written by some other economists are revealed to contain a hidden barb when flowing from Stigler's pen. In the above quote, notice the veiled implication. Those scholars who do switch their positions must almost by definition lack good minds.

> He has to a considerable extent gotten away with murder. Because, I'd say, unlike Milton Friedman, you have to be really very aware when you read George Stigler of these preconceptions. You know, a lot of people that read George Stigler are quite surprised when you tell them how, of course, very pro markets he was. OK, they realise that he was at Chicago and that sort of thing. But he is wonderful in disguising himself, with his wonderful, his very funny, ironic, cynical stance. (Conversation with Mark Blaug, April 2014)

Finding instances where Stigler did change his mind is simple enough. Many can point to the subject of monopoly or his definite break from the ideas propagated by Frank Knight or Henry Simons. But his fundamental vision did not change. Rather, as his ideas and research matured he discarded theoretical elements that no longer fit with his more comprehensive and consistent view of the world.

> I think he [George Stigler] went to a more satisfactory position, absolutely. The earlier view, as you say, he picked up, that was the literature, he hadn't really thought it through. I mean, you know, he hadn't thought through everything at that point, and he hadn't really thought it out. As he thought through more and more, I think he came to a more satisfactory thesis on the issue. I think you're absolutely right, he did. (Conversation with Gary Becker, October 1997)

[6] To avoid any inevitable misunderstanding and the unfortunate tendency to shift the terms of debate, the analysis presented has no intention of evaluating the correctness of the various stances

Like missionaries abandoned in a hostile environment, these emissaries from Chicago converted many of the once hostile forces opposing their canonical beliefs.[7] A key to their ultimate success lay in creating stories that would eventually resonate with and bewitch economists, as well as

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adopted by those at Chicago. Questioning consensus beliefs, which was the Chicago stock-in-trade, can only be a positive virtue when it prevents any discipline from slipping too far into a dogmatic sleep. However, iconoclasm based on unwavering a priori knowledge that remains invulnerable to all and any evidence is never desirable, no matter the ideological base from which it emanates, whether left or right.

[7] Both Friedman and Stigler had an extraordinary talent for marketing their views. Certainly they didn't invent or even pioneer the idea of selling theories, either within the profession or to the public at large. Alfred Marshall had campaigned relentlessly against the ideas of Henry George and pushed for the professionalization of economics and the exclusion of those lacking the requisite credentials. However, Friedman and Stigler made the marketing of ideas co-equal to the formulation of those self-same theories. Despite having elevated the importance of selling their ideas, they need not have done so by making a conscious decision to appropriate equal weight to the packaging as well as the substance of their ideas. But they both realized that they would have to hone their already considerable marketing skills if they were going to successfully shift the profession in the direction they desired. Theories and analytical frameworks do not sell themselves. Stigler certainly believed that being ignored was a much worse fate than being attacked. (He quit his column in *Business Month* when readers failed to react to his deliberately provocative columns. Marketing can only do so much.)

> I am going to end my association with Business Month at that time. This decision represents no dissatisfaction with you or the magazine. Rather, it results from my lack of enjoyment of a process in which there is no reaction at all: for all I would know, Business Month is published in Cantonese and sold only in China. I should indeed think this absence of reaction would raise doubts of the value of the column to the magazine. (Letter from George Stigler to Gerald Rosen, executive editor of *Business Month*, October 17, 1988)

As a true believer, someone confirmed in his vision, he would be unlikely to have allowed his light to be hidden beneath a bushel basket of modesty. His remarkable ability to express his ideas and debate them against all comers helped to float his goals. Evangelists don't succeed by being soporific. Any idea, no matter how substantial, can remain largely ignored without the proper packaging.

> The tenacity with which people hold the ideas in which they have a proprietary interest is not due simply to vanity. A scholar is an evangelist seeking to convert his learned brethren to the new enlightenment he is preaching. (Stigler 1988:211)

This unwavering belief in the post-war Chicago vision ultimately paid off. When Keynesian orthodoxy was deemed inadequate, Chicago had a fully formed alternative ready to take its place. Moreover, its formulation was based on a microeconomic foundation that had managed to successfully dominate the post-war period. In fact, the disjunction between Keynesian macro theory and the standard price framework would provide a useful wedge which the Chicagoans could deftly exploit. By the 1970s, Stigler and others working along similar lines had effectively demolished each and every attempt to substitute an alternative microeconomics that might have proven more conducive to the Keynesian form of analysis.

possessing an almost intuitive understanding of the importance of effectively marketing one's wares.[8] Thus part of understanding the rise of the Chicago School lies in identifying Friedman, Stigler and their various allies as consummate storytellers within the profession.

> George Stigler, I remember when I was a young person, wired and said, 'Selling is very important in your research. So write better. Work on writing because that is important. You've got to sell what you are doing.' (Conversation with Gary Becker, October 1997)

Ultimately, success comes to those most able to create and market stories that reflect the dark heart of any given era. The landscape of the post-war period is littered with the carcasses of theories and approaches that seemed bound to succeed in their adolescent years, only to flounder and fail to achieve a critical degree of leverage among practicing economists. Administrative pricing, the Phillips curve trade-off, x-efficiency and numerous others at one time appeared to catch fire, generating interest and sucking in adherents in their wake. Why theories such as these never quite break some unseen threshold while others, seemingly discarded, later arise from their graves, has long been a matter of controversy. *The General Theory,* in its day, seemed to suck the air out of anything else published contemporaneously, as Schumpeter and Haberler discovered

[8] What would seem to be two unnecessary clarifications are required here due to curiously distorted readings I have stumbled across. As noted in a previous footnote, marketing was not invented by those at Chicago nor did it exist as a unique tool that only they employed. That they were more skilful at sizing up their audience and ultimately successful at influencing the profession seems fairly unarguable. I would suggest that their very skill forced others to raise their game, perhaps making the marketing aspect of the discipline more important than it had been previously. Nor should it be necessary to state that they would not have been successful if the work they were trying to sell contained no intrinsic value. A campaign extending over more than three decades does not eventually triumph by consistently trying to pawn off shoddy wares. But the work done by Stigler and Becker (1977) demonstrates the importance of appealing to existing preferences and skilfully selling one's offerings, whether in commodity markets or in the market for ideas. Theories do not sell themselves. The purpose of this chapter is not to award bouquets or toss brickbats at the work done in Chicago during this particular post-war period. Focusing on the marketing and ideological aspect of their work simply sidesteps those issues. That Chicago made influential and important contributions to the discipline goes without saying. Or I would hope so.

much to their disappointment.[9] Why this should have been the case has generated countless explanations without any conspicuous consensus being achieved.

Like any reasonable whodunit, then, this chapter explores the rise of the Chicago approach in the post-war era and discusses how ideas formulated during this counter-revolution have influenced the broader profession. Ultimately the very success of this hard-edged campaign eventually led to a fading, or at least mellowing, of this same unyielding Chicago approach, but not before leaving behind an ineradicable residue that remains a defining characteristic of the discipline today. The first step, then, is to sketch the context and clearly map the terrain in which this particular struggle for the benighted soul of economics occurred.

The Post-War Landscape in Economics

> It was the best of times, it was the worst of times, it was the age of wisdom, it was the age of foolishness. (Charles Dickens (1859) *A Tale of Two Cities*)

Unarguably, the two great events that shaped the course of the post-war period were the Great Depression and World War II. The lessons to be learned seemed obvious to most observers. Government had earned an intrinsic right to its role as an economic planner and source of economic initiatives. The triumph of the war economy in the USA, as well as the defeat of a seemingly intractable economic depression, had boosted the standing of direct government intervention and planning.[10]

[9] Haberler (1937) and Schumpeter (1939) both wrote books during this period which, like Keynes, attempted to place economic depressions in their appropriate analytic contexts. Both considered these books as representing definitive works, the ultimate final or conclusive word on the subject. Identically, both works shared the same fate of being swept away by a tide of excitement surrounding the *General Theory* (1936). As in all endeavors, timing is always crucial.

[10] Much the same had occurred after World War I. Business/government cooperation in the conduct of economic policy (symbolized by such figures as financier Bernard Baruch) had boosted confidence in the conviction that the anarchy of the marketplace could to some degree be stabilized and channelled by the combined efforts of government administrators and business leaders. (See Weinstein (1968) for a useful description of the progressive era from which this form of corporatism developed.)

This was the period when the Keynesian approach seemed unassailable, both amongst academics and policy makers.[11] The center of the economic universe, both academically and effectively, had shifted to the USA. American dominance in the world economy ran parallel to the clear-cut dominance of its academics within the economics profession. European refugees had sought a safe haven in the USA and had joined with native talent to sweep the field before them. Perhaps the new (quasi-engineering) look in economics was best represented by the Cowles Commission in Chicago, where some of the most promising young economists worked to provide mathematical rigor to the new Keynesian approach, while others pioneered applied statistical methods (econometrics), which sought to move the profession away from the anecdotal, the purely deductive and the pitfalls of simple assertion.

Overall, a can-do optimism seemed to dominate the younger members of the profession.[12] Work spearheaded by Paul Samuelson and Kenneth Arrow often highlighted the shortcomings of the marketplace, implying an almost automatic role for government to engineer solutions, in much the same way that government had pulled the economy out of the depths

[11] As ever, policy formation would inevitably lag behind waves of theoretical insight. The watermarked height of Keynesian influence came during the Kennedy administration when Harvard, MIT and allied departments conducted an ostensible lend-lease program with the White House in forming economic policy. This migration largely ignored the gathering success of the Chicago counter-revolutionaries, a movement commenced during the immediate post-war period and gaining increasing traction with the commencement of the 1950s. Notice, however, that economic debates were now intractably anchored to sites within the USA. The real action had shifted permanently across the Atlantic, abandoning the once dominant position enjoyed by Cambridge in the era of Marshall and Keynes.

[12] In a narrower, more self-interested sense, the Keynesian revolution provided a unique opportunity for young, ambitious members of the profession to leapfrog ahead in their academic careers.

> … it [*The General Theory*] neatly shelved the old and established scholars, like Pigou and Robertson, enabled the more enterprising middle-and lower-middle-aged like Hansen, Hicks, and Joan Robinson to jump on and drive the bandwagon, and permitted a whole generation of students (as Samuelson has recorded) to escape from the slow and soul-destroying process of acquiring wisdom by osmosis from their elders and the literature into an intellectual realm in which youthful iconoclasm could quickly earn its just reward (in its own eyes at least) by the demolition of the intellectual pretensions of its academic seniors and predecessors. Economics, delightfully, could be reconstructed from scratch on the basis of a little Keynesian understanding and a lofty contempt for the existing literature—and so it was. (Johnson and Johnson 1978:187–188)

of depression and won a terrible war. The spirit of A.C. Pigou seemed to be the prevailing genius of that age, with economic engineers destined to tinker with the nuts and bolts of the economy in a useful, if less than breathtaking, fashion as foreseen by John Maynard Keynes.[13]

The Conservative Counter-Attack

"Bah," said Scrooge. "Humbug!" (Charles Dickens (1843) *A Christmas Carol*)

It should come as no surprise that the conservative counter-revolution became centered on Chicago. From its inception, the school had been imbued with conservative yearnings, especially when framing economic policy. Founded and fertilized with Rockefeller money, the institution was initially derided as Standard Oil University. This image was hardly ameliorated by the faculty raids conducted under the aegis of its first President, William Harper, with much the same cutthroat rapacity displayed by the fabled robber barons as chronicled by such muckrakers as Ida Tarbell.[14] The initial economics department, led by John L. Laughlin, only buttressed this characterization of combative conservatism.[15] Laughlin seemed to live

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[13] Along with an optimistic (perhaps audacious) belief that economic problems of scarcity would be largely solved in a few generations, Keynes foresaw a future where economists would become useful, but mere, technicians. He perhaps imagined a corresponding macro version of Pigou's micro corrective policies, aligned with an inevitable amelioration of class conflicts reminiscent of Alfred Marshall.

> But, chiefly, do not let us overestimate the importance of the economic problem, or sacrifice to its supposed necessities other matters of greater and more permanent significance. It should be a matter for specialists-like dentistry. If economists could manage to get themselves thought of as humble, competent people, on a level with dentists, that would be splendid! (Keynes 1963:373)

[14] Keeping this reference within the Chicago boundaries, Tarbell (1904) won fame as the author of the muckraking volume *The History of the Standard Oil Company,* which purported to expose the machinations of John D. Rockefeller.

[15] The initial linkage of the University of Chicago with conservative business interests and the appointment of Laughlin to shape the Economics Department would, whether legitimately or not, identify these economists with a certain given position or viewpoint. Chicago in this era was never quite so unified.

> In its early years, the University of Chicago was nicknamed the "Standard Oil University." The first head of the economics department was J. Lawrence Laughlin, one of the most conservative economists in the country. Laughlin was a combination of neoclassical theorist

for controversy, alienating much of the professional establishment by not only scorning the recently initiated American Economic Association, but by having the gall to set up his own house organ, *The Journal of Political Economy* (1892) in competition with *The Quarterly Journal of Economics* (1886) produced by the hitherto unchallenged Brahmins of Harvard University. (Perhaps these early events planted some of the seeds of rivalry that would blaze in the post-war period between the Chicago upstarts and their more establishment counterparts at Harvard.[16]) It was Laughlin, then, who established a perhaps wrongly perceived tradition of Chicago as the home of ultra-conservatism and one firmly attached to ideologically tinged policy prescriptions.[17] However, as A.W. Coats (1963) points out in his thumbnail sketch of the origins of economics at Chicago, traditions may have little to do with reality. In fact, a certain dollop of the Chicago orthodoxy seems to have been

> and aggressive big business apologist—the type that seemed to confirm the suspicion of those who regarded the University of Chicago as a tool of business interests. (Djelic 2007:100)

[16] This contentious response to Chicago, perhaps reflecting the intense marketing that had become one of the hallmarks of that School, was particularly notable in the 1960s and 1970s when the Chicago marketeers fixed their sights on the newly acknowledged orthodoxy propounded at Harvard and MIT. Those at Chicago perceived the Harvard and MIT economists as being unduly dismissive of their work, reflecting an "us versus them" mentality that if anything spurred their crusade onward.

> It is utterly amazing how, in MIT for a period of years, particularly in Samuelson's classes, but also in Solow's classes, hardly a lecture went by without some sort of a quip aimed either at Milton, or at Chicago, or at the Midway or something like that, you know. It was something that had got under their skin. (Conversation with Arnold Harberger, October 1997)

[17] It would be more realistic to describe Laughlin as positioning Chicago as a refuge for the profession's outsiders, a safe haven for its refugees. Thorsten Veblen, an early member of the department, was certainly a classic example.

> So I think Chicago has been much more open to these kinds of approaches. Knight talked about it and Knight was a great mentor in the department. Before Knight, apparently Veblen when he was here was attracted to a wide number of different interests as well. So this place has been very tolerant. (Conversation with Sherwin Rosen, October 1997)

This renegade position would work to Chicago's advantage in the years leading up to and immediately after World War II. The relative tolerance exhibited during these years (as opposed, for instance, to the sharp Anti-Semitism displayed by the Harvard department) enabled Chicago to snare European refugees as well as somewhat gauche, but sharp-witted young men from the provinces (untouched by the major urban centers). Both Friedman (Jewish) and Stigler (provincial) fit this bill. Curiously, these two showed much less tolerance than Laughlin when making their own selections. For the issue of anti-Semitism in economics see Reder (2000).

delicately created by employing a sort of backwards extrapolation. The inter-war teachings and ideas of Chicago's classical liberals became under the nimble interpretation of guiding lights such as Friedman and Stigler, something of a palimpsest on which they could engrave their current verities and insights.[18]

However, one aspect of Chicago that remained consistent from its founding was an intrinsic injection of an ornery quality to any debate in which faculty members chose to engage. This cultivated image of contrariness, of pointing out 'what ain't so,' was only heightened by the leading lights of the department during the 1930s, especially the domineering figure of Frank Knight.[19]

> Milton Friedman: I think you're getting at something that is (a) the atmosphere at Chicago, and (b) intensified by Knight. That an academic is concerned not with being diplomatic, not with trying to avoid hurting people's feelings, but an academic is concerned with saying what's right. Telling the truth, or trying to get at it. And if you disagree with somebody you don't say, "Well, now there may be something in what you say."
>
> Rose Friedman: "You may be right."
>
> Milton Friedman: You say, "That's a bunch of nonsense."
>
> Aaron Director: Exactly. That's not surprising. (Conversation with Milton Friedman, Rose Friedman and Aaron Director, August 1997)

[18] Both of these luminaries had a distinct weakness for creating purpose-built traditions which they were able to fortuitously unearth whenever it suited their purposes. Friedman battled Don Patinkin for a number of years as he attempted to ground his quantity theory of money in an ersatz oral tradition, one that supposedly developed and thrived in the Chicago of the 1930s. (See Freedman (2006) for an investigation examining the roots of this creation and the reasons behind it.) Also the traditional kinked demand curve, a predictable part of any textbook examination of oligopoly in that post-war period, had much more to do with Stigler's (1947) deliberate transformation of Sweezy's (1937) work than it did with the original work itself. (Freedman (1995) provides a useful guide to Stigler's strategic campaigns.)

[19] To point out the obvious, contrariness or being a maverick is not necessarily either a positive or a particularly regretful characteristic. The term refers to a tendency to simply refuse to accept mainstream theories, at least not without first disparaging them and subjecting them to trial by ordeal. Knight was of course famous for being allergic to any received doctrine, as was his one-time protégé Aaron Director. The danger inherent in being such a self-styled iconoclast is that a commitment to truth telling can sometimes be a simple rationalization for an innate ornery personality. At its worse, this path can deteriorate into critical self-indulgence. Such an attitude may manage to provide comfort to its holder, whether rightly or wrongly, when a seriously contrarian disposition is confronted by the unnerving reality of holding a distinctly minority position, in some cases being almost a majority of one.

However, these pre-1950s departments were never particularly uniform. Veblen and Mitchell were notables during the Laughlin period, while Douglas and Schultz made key contributions in the 1930s. (Even the arch-conservative Aaron Director, the *éminence grise* of the Chicago School, originally came to the South Side as a PhD student after working for the Oregon State Federation of Labor. He later served as a research assistant to Paul Douglas with whom he published (1931) *The Problem of Unemployment*. (These events were prior to his subsequent and irretrievable conservative apotheosis.[20])

In the immediate post-war period, the Cowles Commission, with joint appointees like Don Patinkin and Jacob Marshak and departmental Keynesians like Lloyd Metzler, provided ballast to the department. This apparent transition led newly appointed member Milton Friedman to sharply lament the department's direction, a shift seemingly aggravated by Paul Samuelson's initial willingness to accept a proffered position. To the 'true believers' this uncongenial event appeared to be the final nail in the coffin of responsible scholarship. What would become the defining characteristic of the department seemed in danger of being still-born.[21]

> The Samuelson matter was again forced to a head—by Douglas—& thanks mainly to his efforts we lost badly. The dep't has voted to make Samuelson an offer. We don't yet know the end of the story. But whatever it is, I am very much afraid that it means we're lost. The Keynesians have the votes & mean to use them. Knight is bitter & says he will withdraw from active participation in the dept. Mints, Gregg, & I are very low about it. (Hammond and Hammond 2006:46, Letter from Milton Friedman to George Stigler, November 27, 1946)

[20] Whether there was anything resembling a conversion while crossing the Midway remains an unsettled question in any in-depth analysis of this rather retiring academic. See Van Horn (2010) who seems to suggest a more consistent thread to Director's character. Given this perspective, Knight's influence did not convert the young economist, but rather allowed his natural tendencies to blossom.

[21] The point that should be kept in mind here is that there was no pre-existing Chicago School to be defended or continued, but a School waiting to be founded. In the words of one of the stalwarts of the Chicago counter-revolution, "There was no Chicago School of Economics when the Mt. Pelerin Society first met at the end of World War II." (Stigler 1988:148)

This lament only echoed another bleak summation drawn by one of the key defenders of the classical liberal faith.

> The outlook at Chicago, if better than elsewhere, is not very promising. Our divisional dean has no apprehension of economic liberalism and a distinct hostility toward it and the same is true of most persons in the other social science departments. … In the Department [of Economics] we are becoming a small minority. (Henry Simons quoted in Coase 1993:245)

This Chicago-style attempt to incorporate diversity, to be open to recent developments within the profession, let alone the fear that the department would be swamped by Keynesian planners, receded significantly and something closer to homogeneity was unambiguously confirmed when George Stigler was lured back to Chicago through the largesse attached to his appointment as the Charles R. Walgreen Professor of American Institutions in 1958.[22] "He had an enormous grant at that time. And his salary was maybe $25,000 in 1958 dollars. That was what I think he was making per year… It was one of the biggest salaries in economics" (Conversation with Claire Friedland, October 1997). As the economic counter-revolution launched in Chicago appeared to pick up speed, the necessity of a concomitant dose of ideological soundness concurrently achieved a greater cachet. "Now, Aaron Director, for example, would never have written a good letter of recommendation for somebody who wasn't a staunch conservative but neither would Milton. And I remember for years after I left the University of Chicago, when they were contemplating influential appointments they would ask me about the person, 'Is he really sound?'" (Conversation with Paul Samuelson,

[22] Stigler was to generously use these Walgreen funds to build up a roster of key economists furthering his economic project, one that sought to change the mindset of the profession. Nik-Khah (2011) details the importance of these funds, as well as other corporate cash Stigler attracted, in creating his preferred team of economists. For instance, these funding sources enabled him to lure his top student, Sam Peltzman, back from UCLA or convince Gary Becker to leave Columbia.

> And then, after a lot of hesitation, he [George Stigler] and Milton Friedman worked on me a lot, he offered me extra research money if I stayed on. This was Walgreen money and it was a large sum. So he said I could have some research money and I decided to stay. (Conversation with Gary Becker, October 1997)

November 1997).[23] The shift was striking and noticeable even to one of the pillars of the department in the 1930s and the academic who had had a long-lasting influence on George Stigler.

> It was not until after I left Chicago in 1946 that I began to hear rumors about a 'Chicago School' which was engaged in *organized* battle for *laissez faire* and 'quantity theory of money' against 'imperfect competition' theorizing and 'Keynesianism.' I remained sceptical about this until I attended a conference sponsored by University of Chicago professors in 1951. The invited participants were a varied lot of academics, bureaucrats, businessmen, etc., but the program for discussion, the selection of chairmen, and everything about the participants were so patently rigidly structured, so loaded, that I got more amusement from the conference than from any other I ever attended. Even the source of the financing of the conference, as I found out later, was ideologically loaded. (Jacob Viner quoted in Patinkin 2003:112)

What had changed in this post-war environment to precipitate this concerted Chicago counter-revolution was the birth of a concerted effort by conservative forces struggling to shift the intellectual terms of debate. This reaction is best symbolized, if not epitomized, by the 1947 focused pilgrimage to the first Mont Pelerin meeting. This gathering coincided, not so coincidentally, with the acknowledged start of the Cold War. A former wartime ally, the Soviet Union, had transformed itself into a deadly enemy, one that threatened the basic freedom and liberty of Western nations. The initiative leading to this intellectually based conservative

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[23] Stigler's later attempts to persuade Robert Solow to switch from MIT would seem something of an aberration. However, Solow gained Stigler's abiding friendship by intellectually meeting him head to head and by being equally sharp-witted and well-read. These characteristics overrode matters more ideological in nature. However, Solow wasn't tempted.

> Oh, I don't think it would have much effect … I'm a very counter-suggestible person. It might have changed me methodologically a little, but I don't think I would have been happy because being involved intellectually in controversy with one's colleagues all the time is never a formula for relaxation. I also think probably I might have been more productive at the University of Chicago, more productive in terms of volume although I'm not so sure in terms of quality. It's the ethic here. It has always been a little more laid back, a little more relaxed. So, I don't think it would have turned me into a conservative, or a monetarist or any of those sorts of things. Or a, you know, a gung-ho free marketer. (Conversation with Robert Solow, November 1997)

counter-offensive came a mere year after Winston Churchill had identified and outlined a clear and present danger in his "Sinews of Peace" address (the "Iron Curtain Speech"), at Westminster College in Fulton, Missouri on March 5, 1946. "From Stettin in the Baltic to Trieste in the Adriatic an iron curtain has descended across the continent." The fear of collectivism, shared by many occupying the right side of the political divide, would to some extent be able to piggyback any of its future effectiveness on what would become a rising tide of anti-communism. Timing in all human endeavors remains essential, especially when assessing the potential success and the indisputable marketing prowess of even the most focused counter-revolution.

Both George Stigler and Milton Friedman attended that first Mont Pelerin meeting, which succeeded in catalyzing the direction of their subsequent research programs.[24] At this initial convocation of conservative minds, a widely accepted recognition of a perceived clear and present danger prevailed. Planned economies would henceforth not be casually dismissed as merely an alternative program gaining some semblance of intellectual acceptance. Instead they would be designated and recognized as constituting a menace, one that was unfortunately fast becoming the new norm among Western democracies. To those meeting at the formation of the Mont Pelerin Society, the largely unacknowledged slide into totalitarianism, a menace only recently defeated, and now once again a threat masquerading under the guise of communism, was ever present. That the inspiration and prime mover for this first gathering of conservative eagles was Friedrich Hayek hardly resembled anything surprising. *The Road to Serfdom* (1944) served as an unmistakable warning to those focused on a perceived array of eroding freedoms in the post-war period.[25] The ostensible goal of this historic meeting was to preserve and

[24] It would be naturally incorrect to look for a direct link between the initial Mont Pelerin proposals and subsequent research by Stigler and Friedman. That particular agenda did not constitute anything resembling a research program. However, the Mont Pelerin meeting helped sharpen their political and ideological outlook, which in turn would come to influence their subsequent research. Certainly their output after this first meeting became much more focused and directed than had been the case prior to this time.

[25] More prosaically, *The Road to Serfdom* either in its original version or in the *Reader's Digest* condensation became a best seller. The work brought Hayek to the notice of H.W. Luhnow of the Volker Fund. It was this fund which would bankroll many of Hayek's projects, including sponsor-

strengthen the virtues of classic liberalism (perhaps best exemplified by the work of John Stuart Mill). Those attending remained convinced that this venerable tradition was in danger of being carelessly discarded, given the engineering-like optimism that accompanied the planning aspect of the Keynesian revolution. The Mont Pelerin project, as well as the creation of a Law and Economics program in the Law Faculty at Chicago, attempted to provide the sort of counter-weight required to realize the deepest aspirations of Henry Simons. He had envisioned an institute that would grow to represent a redoubt of core liberal values, namely freedom, individual choice and liberty. Simons hoped to see a smoothly functioning organization that

> … should not be mainly concerned with formal economic theory nor should it engage substantially in empirical research. It should focus on central, practical problems of American economic policy and governmental structure. It should afford a center to which economic liberals everywhere may look for intellectual leadership or support. (Henry Simons quoted in Coase 1993:244)

The key figures in establishing both the Law and Economics program and its legitimacy as a field of economics were Friedrich Hayek and the ever-elusive Aaron Director. The two had found a congenial meeting of minds during Director's 1937–1938 secondment to the London School of Economics. Later, it would be Director's influence with and recommendation to the University of Chicago Press that would facilitate the publication of Hayek's seminal classic. The subsequent success of that book would generate the crucial outflow of Volker Fund money that would not only help underwrite the Mont Pelerin meeting, but also establish the Law and Economics program within the Law Faculty. This

ing the initial meeting of the Mont Pelerin Society to the extent of underwriting the travel undertaken by Knight, Director, Friedman and Stigler to attend that first gathering. (Other sponsors included the Foundation for Economic Education and especially the international bank Credit Suisse, which would become the major source of funding bankrolling that meeting.)

initially small green shoot would become the vehicle allowing Director to end his exile and return to a secure position at Chicago.[26]

> In the end it was agreed to appoint Director for five years as research associate with the rank of professor to conduct what was called in the memorandum sent to the Volker Fund, "a study of a suitable legal and institutional framework of an effective competitive system." However, before the final arrangements were made, Simons died, and Katz asked that the terms of the Volker grant be modified to allow Director to do some teaching. (Coase 1993:246)

Director never bothered to publish extensively (or anything much) during his long career, but his impact as a teacher was undeniable.[27] His influence was felt by such luminaries as Richard Posner, Robert Bork and Kenneth Dam. He was one of the few people who clearly helped mould the thinking of George Stigler during Stigler's last 33 years at Chicago.[28] Milton Friedman's brother-in-law, Aaron Director, played the

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[26] Caldwell (2011) very deftly sums up what he refers to as the Robert Van Horn–Phillip Mirowski story linking Simons, Director and Hayek. He finds this account, based on extensive documentation, convincing though he is much less accepting of the meat of their analysis.

> Van Horn and Mirowski are to be commended for providing this new and, in its broad outline, compelling account of the origins of the Chicago School. Given the facts of the case, as summarized previously, it is clear that both Henry Simons and Friedrich Hayek played important parts in the construction of the Chicago School: As VHM put it, Hayek might be viewed as "the prime external contractor" and Simons as "the prime architect." At the end of the day, however, I think that Simons played the most important part—to change metaphors, he was the leading man. (Caldwell 2011:304–305)

[27] Director was a classic backstage manager. He preferred to discuss and influence selected individuals face to face. The sort of marketing and the more scattershot approach that distinguished publishing never seemed to appeal.

> You would write—you would want to write just to first make sure that you yourself understood well, and secondly to show other people … Well, you want to do it even if you didn't want to explain it to anybody else. Even if you wanted to explain it to yourself … And maybe just a few students. (Conversation with Aaron Director, August 1997)

[28] The close relationship between Milton Friedman and George Stigler is largely taken for granted. However, fewer realize the larger influence that Aaron Director had on Stigler's thinking.

> Milton Friedman: Added to that, well a lot of George's attitude came from Aaron. I think you had a lot of influence on what he said.
>
> Aaron Director: I don't think so.

classic role of the *éminence grise* figure perfectly. From the meeting at Mont Pelerin he remained one of only three people capable of intellectually shaping George Stigler's views (along with Milton Friedman and later Gary Becker). "It would be inappropriate as well as time-consuming to name all the members of this community, but perhaps I will be forgiven for acknowledging my inexpressible debt to Aaron Socrates Director." (Stigler, Birthday Party Talk ms 1976:10)

> It was in the 1960s that I began the detailed study of public regulation. My interests were aroused, and my faith in the clichés of the subject destroyed, as so often with other subjects, by the discussions with my friend Aaron Director. This wonderful man is that rarest of scholars: a clear-headed, imaginative, erudite man who enjoys the task of constructing luminous and original theories but does not even write them down! (Stigler, George J. (1988?) "Biographical Notes", ms. p.3; George Stigler Papers Addenda, Box 27)

Director's essential role in shaping the path taken by the Chicago counter-revolution has been too often undervalued. To say that his was one of the pivotal hands steering that intellectual revolt is no exaggeration. However, his handiwork was often obscure to all but insiders, since a select band of brothers fronting the crusade remained far more visible.

> Both in and out of the classroom, Director was extremely effective as a teacher, and he had a profound influence on the view of some of his students and also on those of some of his colleagues at the University of Chicago both in law and economics. (Coase 1993:245–246)[29]

> Milton Friedman: Between you and me, you were more influential. But of course, you know, people get into patterns of what they say and it doesn't always correspond to what they do. (Conversation with Milton Friedman, Rose Friedman and Aaron Director, August 1997)

> By the last part of his life, whether in the last half or the last third, it was my impression that George was of the opinion that *laissez-faire* itself pretty much approximated to tolerably effective competition. And I think Aaron Director was the prime source of this view (Conversation with Paul Samuelson, November 1997).

[29] Others were more skeptical of Director's contributions.

> Aaron Director was extremely conservative. Why, I don't know. By the time I knew him he was already like that. And he was an iconoclast. But he didn't develop new data with respect to industrial organization. He didn't develop and articulate new theories. He just said that the conventional belief wasn't so. (Conversation with Paul Samuelson, November 1997)

To take root and even thrive, a counter-revolution such as Chicago attempted not only demanded determination, skill and a categorical compulsion to triumph no matter what the cost, but also required a fertile environment. Here the conservative attempt to redirect intellectual debate timed its initial sally astutely. The insistence on markets promoting economic liberty and individual freedom fortuitously struck the right note during the virulent anti-communist campaigns defining the McCarthy era.[30]

> I once did a little informal investigation of whether people who were economic libertarians and tended to favour low taxation and low regulation and *laissez-faire*, were also people interested in civil liberties and freedom of expression, and that sort of thing. So I would ask innocent questions. 'Now what do you think of this group?' Of course, I had a placebo question control group. 'What do you think of the fact that this professor at the University of New Hampshire, the one who invited Paul Sweezy in the McCarthy era to give a lecture, is losing his job because neither he nor Paul Sweezy will testify as to what was the content of the lecture?' And Milton [Friedman] said, 'Gee, it's a simple case. It's a free speech society. If a man will not do what he should, this professor should be fired. Society has a right to know.' I said, 'You don't understand. They've got the notes on the lecture, verbatim. It's not a question of information.' (Conversation with Paul Samuelson, October 1997)

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[30] Stigler and Friedman would fiercely defend academic freedom from the excesses of the left in the 1960s/1970s. More tolerance seemed to be extended during the McCarthy period when the threat to free expression on campus had to be taken more seriously. (The threat carried the imprimatur of governmental authority.) Simply put, an ample correspondence exists demonstrating their repugnance for the student movement of the 1960s. No such objections were raised during the McCarthy period, either in private or public. Paul Samuelson, who knew them both from the 1930s onward, notes conversations with Friedman that indicate he had no qualms about the crackdown in universities during the 1950s aimed at academics on the left.

> I once did a little informal investigation of whether people who were economic libertarians and tended to favour low taxation and low regulation and *laissez-faire*, were also people interested in civil liberties and freedom of expression, and that sort of thing. So I would ask innocent questions. 'Now what do you think of this group?' Of course, I had a placebo question control group. 'What do you think of the fact that this professor at the University of New Hampshire, the one who invited Paul Sweezy in the McCarthy era to give a lecture, is losing his job because neither he nor Paul Sweezy will testify as to what was the content of the lecture?' And Milton [Friedman] said, 'Gee, it's a simple case. It's a free speech society. If a man will not do what he should, this professor should be fired. Society has a right to know.' I said, 'You don't understand. They've got the notes on the lecture, verbatim. It's not a question of information.' (Conversation with Paul Samuelson, October 1997)

The ideology of free markets and *laissez-faire* perfectly complemented a rising wave of fear and suspicion. Price theory, the theoretical underpinnings of this market approach, largely eliminated the need for the sort of government intervention prescribed by Pigou or Keynes.[31] This Hobbesian solution eliminated any dangerous exercise of economic power. Paradoxically under these circumstances, by surrendering all economic power to the market, individual agents were able to exercise free choice, to be, in the later words of Milton and Rose Friedman, "free to choose."[32]

Among academic economists, this revived conservative backlash trickled down to seemingly innocent decisions. Textbook debates roiling individual departments or extending throughout universities seemed driven by a combination of ideology and theoretical standpoints having more to do with the direction of the profession than with whether these volumes served as effective teaching tools. Thus the new wave of post-war Keynesian textbooks met with resistance from the old guard, as well as from politicians and businessmen keen on sniffing out anti-free market (communist) influences.

> I [Carolyn Bell] occasionally met a student who asked if it was true that the book [Samuelson] was communistic and if she would be required to read that radical, Keynes. Like the parents who prompted these questions, many economics faculty condemned the new approach, sometimes in a destructive power struggle. (Bell 1988:147)

[31] Steven Medema (2011) constructs an interesting case for the existence of different approaches to price theory at Chicago, or perhaps more accurately, variations grounded in somewhat different concerns. All, however, emphasized the competitive nature of the marketplace. Competition when left to operate optimally must almost by definition undermine the significance of private economic power. Certainly, as the Chicago program developed, the importance of monopoly, for instance, was minimized (Stigler 1965). In the absence of private economic power, the need for government intervention would seem to fade away, or at least do so to a significant extent. Certainly no Chicago economist claimed that this insight remained unprecedented. Both Friedman and Stigler did identify their particular approach to economic analysis as being largely grounded, if not firmly rooted, in the efforts made by Alfred Marshall, as did for that matter the Cambridge economist, A.C. Pigou, some years earlier. Such statements of fidelity would seem to be meaningless since each would-be acolyte was able to take from their designated source of inspiration whatever they might find most convenient. Whether any of these three economists faithfully carried on Marshall's tradition is open to question.

[32] This question of the role market power plays is dealt with in Freedman (2005). Clearly, if the issue was presented in a more complex fashion and the choice displayed was a less salubrious one than between public and private power, repackaging that option as a battle to defend the individual and preserve liberty would have been quite difficult. Eliminating economic power by presenting a carefully structured view of market processes delivered a much more palatable result.

Battlegrounds: Chicago Versus Harvard

> "Now, what I want is, Facts … Facts alone are wanted in life."
> (Charles Dickens (1954) *Hard Times*, Mr. Gradgrind)

In the post-war period, Harvard became Chicago's 'other', the peddler of suspicious theories and too-good-to-be-true policies pandering to collectivist sentiment. Only the South Side renegades were willing to challenge these dangerous heretics who carelessly cast doubt on market efficiency and by doing so undermined the integrity of the economics profession. This may appear to be a melodramatic and exaggerated characterization of these two poles-apart departments, but in fact only a bit of an exaggeration when it comes to the way in which the relevant players viewed themselves at the time.

For leading lights like Friedman and Stigler, Harvard represented the privilege and self-assurance that they both studiously disdained. Some of this derived from the fact that had they absorbed a good dollop of this attitude at Chicago in the 1930s. Certainly Knight dismissed his one-time student Chamberlin, later to become a fixture for many years at Harvard.

> It started with Knight. Knight was actually a teacher of Ed Chamberlin at the State University of Iowa. That's not at Ames. … Knight always said, "All that's good in Chamberlin he got from me and there isn't anything good in him," you know, something like that. And, there's no reason why this should have been of any importance, but it really riled Knight that Chamberlin was a Catholic convert. "The man believes in the Immaculate Conception. What can you do with him?" Knight would say. So from the start, of course, they didn't like the notion that if you were analysing imperfect competition, you were analysing cases of market failure. They always played this down. Now, the early Stigler wasn't as strong on this as he was later on. But Friedman was from early on. (Conversation with Paul Samuelson, October 1997)

It would be incorrect, however, to claim that it was their Chicago experience alone that turned Stigler and Friedman into the crusaders they became, especially once reunited back on their home turf. Friedman, for instance, was awarded his degree at Columbia rather than Chicago and spent only two years as a graduate student there. But the fateful meeting of the two at Chicago for a year as graduate students and the impression made on them by Knight and Viner represented more of a synchronicity as personalities and inclinations clicked simultaneously across time and space.[33]

> But still, it could have been that he would have gone there. For example, if they hadn't given him [George Stigler] a big scholarship at Chicago, or something like that. But you can't rule out his going to Harvard. And I think it would have made a very big difference. His going to Harvard would have made a big difference to me too, because I would not have become a George's friend. (Conversation with Milton Friedman, August 1997)

In the post-war period, Harvard of course had the name and prestige that attached to one of the world's acknowledged great universities. But in economics, it had also gained even greater prominence by championing the Keynesian revolution in the USA, led by the unlikely figure of Alvin Hansen, who in a previous incarnation had shown few signs of harboring radical proclivities. This conversion later met with a terse Stiglerian dismissal.

> A second and related trait of scholars is that they seldom change their minds. We must remember that we are discussing scholars with good minds, and they would usually lose in a swap of minds. It was most unusual when Keynes' *General Theory* led Alvin Hansen, a hitherto conservative economist of mature years, to abandon his previous scientific position and become a vigorous exponent of Keynesian doctrine. (Stigler 1988: 210)

Perhaps only Stigler could transform being open-minded to the equivalent of a mid-life crisis complete with much younger girlfriend

[33] In fact, Stigler would later decisively break with Knight in terms of economic thinking as would Knight's close friend and protégé, Aaron Director.

Milton Friedman: Well, Frank Knight had a particular influence on the people who came close to him, including Aaron. Aaron was a disciple of Knight's as well, much more so, in a way. Would you say you were more or less so Aaron than George was?

Aaron Director: Maybe for a while, but not for long.

Milton Friedman: For a while I would say you were more so, I would think. Aaron and Knight once jointly owned a farm in Indiana. But Knight had a very peculiar, a very real influence on those who became his disciples, which George broke from in the main. (Conversation with Milton Friedman, Rose Friedman and Aaron Director, August 1997) and expensive sports car.[34] But their reaction to Keynesianism was again a combination of nature and nurture. Knight had scorned *The General Theory*, writing a withering review in the *Canadian Journal of Economics* and resisting an effort by the University of Chicago to award Keynes an honorary degree.[35] Moreover, neither would have found the policy implications to be noticeably congenial. "On the contrary, so far as policy was concerned, Keynes had nothing to offer those of us who had sat at the feet of Simons, Mints, Knight, and Viner" (Stigler 1988:153).

While Friedman battled Harvard on the macroeconomic front, raising important questions but pursuing his battles with a missionary zeal, Stigler, both before and after being reunited with Friedman at Chicago, doggedly defended and advanced price theory as the only sound basis

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[34] There might be a more innocent reading of that quote, but again that would only be possible by ignoring the personality and style of the author. As an anonymous referee pointed out, Hansen's previous scientific position is contrasted with his later embracing of Keynesian doctrine (science versus ideology). Such backhanders are quite common in his writing. For instance, Stigler had a tendency to refer to John Maynard Keynes as "Lord Keynes," and not due to some hidden reverence for the English nobility. When attacking the work of Berle and Means (1932), his prime target, Gardiner Means, is reduced to "Dr. Means" (Stigler 1988:5), "a most inventive economist" (Stigler 1988:53). In contrast, Adolph Berle is allowed to be a "brilliant law professor (Stigler 1988:53). Stigler would find neither legitimate justification nor excuse for Hansen to abandon sound scientific theory for what he regarded as a simply wrong doctrine. At least academics with "good minds" would never stoop so low. Hansen's backflip then can only be viewed, at least from Stigler's perspective, as an embarrassment, the equivalent of being forced to watch a friend's midlife crisis. Both Stigler and Friedman retained little, if any, respect for Keynes himself or his doctrines.

> re 4th edition of Theory of Price—I congratulate you on restraining yourself from including a picture of Keynes and even more on not even mentioning him in your index. (Letter from Milton Friedman to George Stigler, December 16, 1986)

[35] Knight's onslaught was so relentless that Keynes could perceive no virtue in replying. When presented with the opportunity by the editor of the *Canadian Journal* of replying to Knight's strongly worded review:

> Keynes declined, saying that "with Professor Knight two main conclusions, namely, that my book caused him intense irritation, and that he had great difficulty in understanding it, I am in agreement. So perhaps you will excuse me if I leave the article alone." (Patinkin 2003:384)

As indicated above, Knight displayed his continued fierce opposition to Keynes when opposing the granting of an honorary doctorate:

> After crediting Keynes with "a very unusual intelligence, in the sense of ingenuity and dialectical skill," he went on to complain: "I have come to consider such capacities, directed to false and subversive ends, as one of the most serious dangers in the whole project of education … I regard Mr. Keynes's [views] with respect to money and monetary theory in particular … as, figuratively speaking, passing the keys of the citadel out of the window to the Philistines hammering as the gates." (Bernstein 1999: 222)

for the discipline of Industrial Organization. In fact for Stigler, the field was no more than a reformulation of applied price theory.[36] Early on, Stigler had taken issue with approaches he labeled as deviating from the true faith in his review of *A Survey of Contemporary Economics* (1949), a compilation reflecting in many ways the Harvard view of microeconomics. Certainly he would go on to mock the "case study" and "structure, conduct, performance model" that were often the mainstays of work undertaken at Harvard.[37] Most of all, Stigler launched a stinging dismissal of Chamberlin's theory of monopolistic competition at a time when it was becoming increasingly accepted.[38]

> Years later when I was a professor at Columbia University, I attended a meeting of the American Economic Association in Washington D.C. and on the flight back to New York to my surprise I found myself sitting next to Edward

[36] For Stigler, any distinction between well-conducted applications of price theory and the field defined by the proper investigation of applied industrial organization was non-existent.

> Let us start this volume on a higher plane of candor than it will always maintain: there is no such subject as industrial organization. The courses taught under this heading have for their purpose the understanding of the structure and behaviour of the industries … of an economy. These courses deal with the size, structure, the effects of concentration on competition, the effects of competition upon prices, investment, innovation, and so on. But this is precisely the content of economic theory—price or resource allocation theory, now often given the infelicitous name of microeconomics. (Stigler 1968:1)

[37] Stigler dismissed the Harvard approach with a careless wave of his hand:

> There is also a less honourable reason for the separate field of industrial organization: much of its literature has been so nontheoretical, or even antitheoretical, that few economic theorists were attracted to it. (Stigler 1968:1)

Harold Demsetz, a close colleague of Stigler's, noted his disdain for such ramshackle economics:

> Stigler never undertook such a study, at least not in its traditional form. The traditional industry study involved in-depth empirical description linked only loosely to neoclassical price theory … Stigler did not treat the traditional industry study as an effective source of theory or even as having policy generalizing power, and this type of study never achieved great importance at Chicago. (Demsetz 1993:796)

[38] His attack came during a series of five lectures presented at the London School of Economics in 1948. "Monopolistic Competition in Retrospect," simply carpet bombs Chamberlin's thinking, motivated in some small part by Chamberlin's earlier critical review of Stigler's own textbook. As was his wont, Stigler regarded this effort as encapsulating a mission accomplished. Stigler's lectures (1949) are succinct, raising key issues, but motivated by clear objectives.

> I am writing mainly to swell your head—though God knows it must be big enough already. Hayek reports that your lectures were brilliant & successful." Indeed, he said yours were by all odds the most successful series of lectures they had ever had. (Letter from Milton Friedman to George Stigler, April 7, 1948 in Hammond and Hammond 2006:80)

> Chamberlin. He opened the conversation, "You and Professor Knight are the two most mistaken economists I know on the subject of monopolistic competition." Thank heaven it was a short trip. (Stigler 1988:58n)

Certainly Friedman and Stigler had an almost preternatural knack of asking important and pertinent questions about theories that had been embraced far too quickly and perhaps too uncritically. But the approach they used to criticize what they conceived to be bad theories, and which allowed them to substitute their own version for the theories they dismissed, depended far too often on misrepresenting the views of their opponents. This distortion seems not to be the result of any deliberate attempt to mislead, but rather a lack of generosity and forbearance compelled by the ideological stance driving their hard-sell tactics.[39] They viewed these opposing theories as a priori dangerous and requiring a 'no holds barred' approach aimed at incineration rather than evaluation. Their marketing predilections persuaded them not to hedge their claims or attempt to mitigate any potential faults infecting Harvard-friendly theories.

The aggressiveness that later served Milton Friedman so well, when once established at Chicago, severely hindered him at the start of his career and could only have added to his animus against the Harvard establishment. Friedman in fact managed to secure his initial toehold in Chicago only due to the misfortune of his close friend, George Stigler.[40]

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[39] I cannot emphasize sufficiently, especially for those readers who might jump automatically to a defensive stance or not bother to read with care, that such distortions were never deliberate. I have found no evidence for such claims and would in fact be surprised if any relevant evidence came to light. That such unfounded interpretations were always created in good faith made them, if anything, more dangerous. For both of these Chicago mainstays, there seemed an overwhelming desire to beat back faulty theoretical positions. Those interested in the foundations of such claims are referred to a number of my own writings (Freedman 1995, 1998a, 1998b, 2002, 2006). Although I am reluctant to self-cite,there is always the possibility of being accused of making yet another assertion without providing proper evidence.

[40] Though he always dismissed his rejection with a joke, George Stigler refused to return to the mother ship of Chicago until 1958 when tempted by an offer that in fact made him one of the highest-paid academics in the USA (an offer engineered by his old classmate Allen Wallis).

> In the spring of 1946 I received the offer of a professorship from the University of Chicago, and of course was delighted at the prospect. The offer was contingent upon approval by the central administration after a personal interview. I went to Chicago, met with the President, Ernest Colwell, because Chancellor Robert Hutchins was ill that day, and I was vetoed! I was too empirical, Colwell said, and no doubt that day I was. So the professorship was

> But what is a little bit surprising in the first place, Milton only got an offer of an associate professorship. And he accepted it. Which I think was too little and rather late. Now Milton had certain troubles, because of two things. Anti-Semitism, but also people were afraid of him, his corrosiveness and so forth. Gottfried Haberler wanted Milton Friedman to be appointed to Harvard and somebody like Ed Chamberlin, who was a very conservative person, was the department member most violently opposed, because the Chicago School hated both the theories of Imperfect and of Monopolistic Competition. (Conversation with Paul Samuelson, October 1997)

While Milton Friedman fought both the well-endowed Cowles Commission in Chicago and the Keynesianism creeping into his own economics department, his counterpart George Stigler established himself in exile at Columbia. Stigler was quickly disenchanted and dismissive of his colleagues there. It was probably only a combination of his wounded pride and the low level of compensation provided by Chicago that kept him biding his time in New York.

> As we have both feared, I have decided to decline the Chicago offer. I must say that I have not found this an easy decision, and I have no deep conviction that it has been a wise one.
>
> Financially—I almost said economically—there is a large sacrifice in leaving. Columbia raised me to $10,000 after you called, and with the NB the total is $14,000. Hence the move would cost almost $3,000 a year, if I make an allowance for moving costs. This seems a large cost.
>
> On the professional side, it may be that the balance is ambiguous. There is no one whose advice and company I value more than yours, but there are few other great attractions in the present Chicago economics department. I'm not the least bit inclined to boast of Columbia, which has a fine assortment of damn fools, but the N.B. crowd—if one may average a universe

offered to Milton Friedman, and President Colwell and I had launched the new Chicago School. We both deserve credit for that appointment, although for a long time I was not inclined to share it with Colwell. (Stigler 1988:40)

> ranging from Arthur to Mills—has a lot of sense and knowledge.[41] (Letter from George Stigler to Milton Friedman, June 1951, in Hammond and Hammond 2006:124)

The Take No Prisoners Approach

> "This ain't the shop for justice." (Charles Dickens (1838) *Oliver Twist*, The Artful Dodger)

As noted, the Chicago department (always something of a state of mind) reveled in its self-selected role of selecting plucky outsiders estranged from the profession's bedrock establishment. The city of Chicago was always itself a town of rough edges. "I have struck a city—a real city—and they call it Chicago... I urgently desire never to see it again. It is inhabited by savages." (Rudyard Kipling, 1891, http://www.corsinet.com/chicago/chicagoq.html) Those who can be seen as laying the foundations of the post-war Chicago approach, Director, Friedman and Stigler, were provincials, children of the Depression, schooled in the 'take no prisoners' approach by mentors like Knight and Viner. The Chicago School took pride in being the mythical 'one-armed economist' requested by Harry S Truman.[42]

> But that's part of the culture at the University here also. This is a *very* critical place. So, you need to place him in that culture. I don't know what he was like when he was younger. He [George Stigler] was a student here, so he grew up in and was molded by this culture, here at Chicago. Friedman, who was George's very close friend, was in this Department for years. He is a very critical fellow too. He also has very strong opinions and is not shy

[41] This is in a letter from George Stigler to Milton Friedman dated Tuesday June 1951. NB refers to the National Bureau of Economic Research which contained both Arthur F. Burns and Frederick C. Mills to whom Stigler refers.

[42] The quote apparently stems from Truman's frustration at the hedged advice always produced by his economic advisors. "I'm tired of economists who say, 'On the one hand … and then on the other hand.' Send me a one-armed economist" (attributed by Howell Raines, editorial page editor of *The New York Times*).

> when it comes to expressing them. (Conversation with Sherwin Rosen, October 1997)

The importance of what Stigler describes as "this pattern of aggressive academic salesmanship" (1988:215) lay behind his support for removing the History of Economic Thought as a requirement of graduate education, despite his own extensive work in this field.[43] The goal was inevitably one of triumphing over foes. Anything that would weaken this resolve must by its very nature be discarded. Perhaps the very idea of market competition had such intensive appeal to these founders of the Chicago School, not only as a theoretical tool, or as the underlying catalyst of individual liberty and choice, but also as an accurate reflection of their own personalities. Both were merciless debaters who were never known to back down an inch.[44]

> Milton was a firm adherent of the bombing of Hanoi. We would have these incredible arguments. Now, I had read quite a lot about Vietnam. I don't think Milton had read anything. I was much better informed. Nevertheless, we would start these arguments at 9 o'clock and by 2 o'clock in the morning I would say, "Milton, I just can't go on. I'm tired. I just can't take any more." And he would say, "Let me just give you one more argument." He was patiently prepared to spend eight or ten hours trying to persuade me of the error of my ways. He knew nothing at all about Vietnam, or Communism. This was outside his knowledge. … He was however always patient, always polite, never got short tempered like I do in an argument, never got nasty. He was a horrible person to argue with, just a nightmare. My idea of a nightmare is to stand on a stage and debate with him in front of the public. … He was just, an amazing,

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[43] See Freedman (2007) for Stigler's ambiguous relation to the History of Economic Thought and his entanglement with academic marketing.

[44] Stigler and Friedman, along with their younger colleague Gary Becker, were known for pushing what they saw as the truth to its furthest edge, not allowing for any equivocation.

> Bill Buckley: Yes, some public regulation can be necessary. Suppose the democratic legislature made prostitution legal. Surely then requiring prostitutes to pass a monthly test for venereal disease is a worthy idea.
>
> Milton Friedman: Not at all. If a woman on the street, professing to be disease free does infect a customer that will hurt her reputation. If, nevertheless she does infect you, then that is a *tort* that you can sue her for in court. (Samuelson 2011a: 864)

> amazing guy. …But, a madman, a madman; one of the few people I could strangle with my bare hands. I feel I could actually do it. (Conversation with Mark Blaug, April 1998)

The overweening emphasis placed on marketing reflects the essence of this competitive spirit. Within this context, winning becomes of paramount importance. Academic debate is thus transformed from an exchange of ideas to the almost gladiatorial contest that became the hallmark of George Stigler's Industrial Organization workshop.[45]

> Some of these traits of intellectual leaders are caught in the statement that they lack a sense of humor. I mean by this, not the inability to laugh at the right point when hearing a joke, but the ability to view oneself with detached candor. Ridicule is a common weapon of attack but amused self-examination is a form of disarmament; one so endowed cannot declaim his beliefs with massive certainty and view opposing opinions as error uncontaminated by truth. (Stigler 1988:213–14)

The rationale for ferocious marketing of this intense variety is that without such an approach your work might simply be ignored, lost in the paper storm of articles produced each year. Of course a persuasive argument is necessary, but marketing goes beyond that. Selling turns subtleties into black-and-white propositions. Clothes become whiter than white and good becomes great. The argument can be offered that there is no choice if an academic is to compete and survive, a sort of "market or perish" mentality. But this is clearly a fallacy of composition type of argument. Who gains when all sides arm themselves with marketing tools

[45] Only the very brave ventured to stand up before the baleful eye of George Stigler and those attending this particular workshop.

> He had this workshop. People had their knives out. I participated in some of them. I think people were using George's example. No prisoners were taken in other words. [laughs] And everybody just jumped in. It was just *chaos* those workshops. [laughter]
>
> *Was a paper actually ever given?*
>
> No. A paper was never given. It was just discussed. It was taken apart. And it was *breathtaking*. [laughs] It was totally breathtaking (Conversation with Sherwin Rosen, October 1997)

of mass destruction? Is there any discernible way in which the discipline advances as a whole?

> So I do think marketing is very important when undertaking an intellectual activity. It's probably more important *now* than it was before. The market is much bigger [laughs] for one thing. You have people making serious money out of marketing ideas and getting reputations from successfully doing that. But it's not only the money. It's the associated prestige and all kinds of other things as well. It seems to me that there's a lot more score keeping these days. Unlike in previous days, economists are now reckoned to be in a pretty interesting discipline. So to succeed, you've got to be able to sell your ideas somehow. How do you do that? I don't really know. Maybe you do attack other people. That's been done over the years, but it doesn't always work. It doesn't work if there's nothing behind your attack. (Conversation with Sherwin Rosen, October 1997)

Stigler and Friedman engaged in hard-nosed marketing because they could and they greatly enjoyed the sport. They were confrontational by nature, educated in an environment that prized that sort of strategic approach.

> So you want to push it everywhere and in every way, shape or form. That's the loyalty oath that you take to your profession. I don't think a lot of economists do it. They don't have that sort of loyalty. You've got to take your ideas as far as you can. You've got to push them until they fail. There are a lot of places where I think economics does fail. But you can't just say, "Well let the sociologist handle that." They're *worse!* Or they certainly ain't any better. There's a blood and guts attitude here in Chicago about economics. That's what makes the Chicago School. That's what I think the Chicago School is about. (Conversation with Sherwin Rosen, October 1997)[46]

[46] Sociologists as the butt of jokes have a long, honorable tradition at Chicago. Frank Knight engaged in a noted public debate with Talcott Parsons, one of the founders of sociology at Harvard in the 1930s. Knight ended a frustrating exchange with a final, damning remark. "Sociology is the science of talk, and there is only one law in sociology. Bad talk drives out good." (Knight quoted in Swedberg 1990:15)

This aspect of the Chicago approach is reminiscent of the political strategy encapsulated by the phrase "whatever it takes." Politicians rationalizing this belief manage to sanitize their approach by focusing on the necessity of being elected. But the suspicion remains that they conduct themselves in this manner not out of compulsion alone, but because they are at heart head-bangers. A self-selection process would then tend to weed out those who find such an approach distasteful. Clearly such reasoning is insufficient when accounting for Stigler's and Friedman's entire motivational drive. They were, as pointed out, also fierce but honest ideological warriors, who viewed America as facing a crisis, at a sort of moral as well as political crossroads. They fought hard since for them individual choice and freedom were at stake, faced as they believed by a virtual tsunami of governmental collectivism. But it is important not to dismiss the degree to which they simply loved playing the game in this manner, how the very sport of it enlivened their lives. However, it was this joy and the accompanying unrelieved ferocity that spread their academic style throughout the profession, if only adopted out of self-defense.

Part of this "take no prisoners" approach also embraced a reluctance, if not an almost phobic abhorrence, of admitting error. To do so was to display a weakness that could only encourage one's opponents. Neither Stigler nor Friedman were known for switching positions or admitting errors. Of course this doesn't make them unique. The generosity of spirit required to acknowledge defeat and publicly dismiss one's cherished theory is not a frequently observed phenomenon.

> But I think that he [Schumpeter] believed you go down in history books for what your ideas are. You don't admit a mistake: "let's go down with all flags flying." And there was something of that, I think, in George. (Conversation with Paul Samuelson, November 2010)

Stigler would rather spar with or deflect criticism than accept the possibility that what he saw as a dangerous alternative theory might contain some kernel at least of validity.

> He was a very strange person to sum up because his methods were not those of anyone else. I think he was quite unique. If you put a point to him,

> he liked to answer it with a joke. Then if you pressed him, he produced some fact or other. You pressed again, he'd give you his answer. But he was sort of an economist even in argument. He used the easiest way out. (Conversation with Ronald Coase October 1997)

Except with someone like Gardiner Means, who matched Stigler in the adamantine nature of his ideological convictions,[47] Stigler simply refused to take rebuttals seriously. If he deigned to acknowledge a returned volley at all, his response would be curt and more often dismissive of the proffered objections.

> The article[48] was accepted and he would surely have been asked since I directly referred it to him as an editor. I never got a note from George saying "Well, this time around I've got to admit I was wrong. And your reading was right." Not at any time. And a lot of people would tell me that if they wrote to him complaining about something, he would answer something like "Well, if you're the kind of person who believes that, then you're just the kind of person who believes that." (Conversation with Paul Samuelson, October 1997)[49]

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[47] In a sense, Gardiner Means was George Stigler's doppelganger. Both were almost implacable when pursuing the vindication of their a priori beliefs. Perhaps that is why each managed to so provoke the other. Both were skilled marketers, which proved a key to their career successes. They were at opposite ends of the political spectrum, though Stigler was far more skilled and clearly the better economist.

> Both of them, both of these men had taken strong positions on this … Maybe – I almost come close to saying that they had taken these positions before I was born. That's not true, but it certainly was before I was born as an economist. I could have predicted George Stigler's predictions before I signed up from what I'd learned in graduate school [University of Chicago Graduate Business School] of his writings. And I could have predicted, to a great extent, Gardiner Means' predictions and you could guess what I learned at graduate school about Gardiner Means. (Conversation with James Kindahl, October 1997)

[48] Samuelson is referring to his 1988 article on Ricardo that substantially refuted Stigler's 1958 consideration of Ricardo's labor theory of value.

[49] There may be a tendency to dismiss Samuelson's evaluations given that he was on the opposing end of so many economic and political debates with Friedman and Stigler. But he knew them for nearly 60 years and certainly respected them as economists no matter what their differences.

> For Stigler was like an older brother to me back on the Chicago Midway … George Stigler will be long remembered by all economists. To me he had a special significance. He was my first mentor back in the Chicago Midway of 1932–1935 … Stigler left our subject permanently altered and that is the only immortality a scholar can have, or want to have. (Samuelson 2011b: 949)

Neither Stigler nor his long-time ally, Milton Friedman, seemed to believe that any core responsibility attached to a successful marketing campaign. Any necessity of admitting error when a stance was shown to be demonstrably wrong was implicitly denied.[50] At the very least, such a possibility should necessarily be entertained and even taken seriously. Instead, both either continued in their attempt to wear down the opposition through constant repetition of their precise position,[51] or quietly sidestepped and forgot about any theory that proved to have only a tenuous claim on the reality provided by observed facts.

> Gary Becker, I think, cured him [Milton Friedman] of that [100 % reserve ratio]. Probably he said, "Look. You have barriers to money in the banking system and private banking under one disguise or another will inevitably arise. You will simply make the banking system ineffective with a kind of Gresham's Law arising." And I think Milton quietly changed, he just quietly dropped that. He doesn't particularly announce changes in positions, but instead, lets them just decay away. That idea actually became a prominent principle, "there's always a way out of bad regulations." (Conversation with Paul Samuelson, October 1997)

[50] As a stark example, the seminal paper by Stigler and Friedland (1962) that raised doubts about the effectiveness of government regulation turned out to contain a fundamental error due to a simple data entry mistake. Upon being apprised of this slip, Stigler refrained from making any revised results public.

> It was a decimal point error. That's why it was such a terrible error. Our coefficient was wrong by a factor of ten. But our standard error was off by a factor of ten as well. So our t-statistic was OK. But our remarks about the size of the effect were wrong. This was all entirely my fault because I'd never seen a computer before and I had made a mistake in configuring the data … But in any case, I told George about it. I asked him what to do about it. This was some twenty years later. George's answer was that there was no point in making a big fuss about this mistake because it was twenty years ago and nobody cared anymore. (Conversation with Claire Friedland, October 1997)

[51] Stigler makes this clear in his autobiography.

> Another aspect of this salesmanship is the heavy use of repetition, perhaps the most powerful of arguments. I once served on a committee with John D. Black, the famous Harvard agricultural economist, and a master of the technique. He would state a position and I would offer one or two conclusive (to me) objections to it. His response would ignore my points and merely restate his position. I might then present one or two lesser objections to his position, chiefly to avoid boredom, only to founder again on his invincible obstinacy. (1988:211)

It also meant pushing the core ideas of rationality, consumer sovereignty and individual choice as far as possible, in order to develop a consistent and comprehensive approach to the core theory of human behavior. Chicago, with Friedman, Stigler and especially Becker, pioneered the approach that would leave other disciplines accusing economics of harboring imperialistic designs.[52]

> I was George's student, in some sense I'm viewed as his protégé, something like that. Personally, Gary and George were much closer than George and I. Much closer and in many ways, I think, they were much more on the same wavelength, especially in this area, "how far you could push rational choice," that kind of stuff. George was absolutely enthusiastic about everything Gary was doing, with the family, with marriage, with this, with that, with everything. But the outside world apparently wasn't. And apropos to the outside world, the graduate students at the end of the year would put on a party where they would perform a play: *The Economics of Mud,* by Gary Becker. *The economics of "fill in the gap,"* by Gary Becker, you know what I mean. It was a standing joke. It has become a standing joke. But George loved that kind of thing. As I do. I mean, I agree with it. I think it is a very powerful part of the message of economics that there's growth in its thinking, and standards and a reason to take economics seriously. But in terms of actually working in that area, the "Tastes" paper, I told both of them I thought it was absurd. (Conversation with Sam Peltzman, October 1997)[53]

[52] Becker and Stigler clearly took the lead in championing the rational choice model. Friedman admired and supported Becker, though not taking as strong an approach in his own work. However, aspects of employing this strong form of rational choice underlie Friedman's policy positions on switching to a volunteer army, legalizing drugs or implementing a negative income approach to relieve poverty. Remember also that Becker wrote his dissertation on discrimination under Friedman. See Medema (2011) for an examination of the possible differences between these three pillars of the Chicago School.

[53] "Tastes" refers to the much-cited paper by Becker and Stigler (1977) "De Gustibus, Non Est Disputandum."

Lasting Influences on the Economics Trade

> As she frequently remarked when she made any such mistake, it would be all the same a hundred years hence. (Charles Dickens (1839) *Nicholas Nickleby*, Mrs Squeers)

Without too much exaggeration, an astute historian of fashion could assure women that if they could only demonstrate sufficient patience to hold on to their wardrobe long enough, those very same clothes might find themselves once again at the heart of a current, fashionable trend. It is therefore a truism that styles recycle and though their various returns do not represent exact recapitulations of the past, their antecedents are quite easily discerned. In a somewhat parallel fashion, practitioners of economics might be hard put to specify any particular theory, however dubious, that has been completely ruled out of court, and banished as completely unacceptable, once and for all. Only allow enough time to flow under the appropriate academic bridge and resurrection, at least in some form, is virtually assured. Younger members of the discipline, especially those lacking an adequate historical background, may be distracted by a new layer of glitter which is essential to any successful attempts at rebirthing, but what reappears each time is actually only some new variation of a previously discredited theory. So while chemists are not waiting for a repackaged phlogiston theory to once again gain serious supporters, a parallel statement cannot be proffered with equal conviction for the economics fraternity. This recycling bent, then, is one of the reasons why marketing can assume a far more pivotal role in a discipline like economics when compared to some of the hard sciences.

> One of the interesting things about economics is that the subject has been so static over the years. That is, it's not that you don't know much more, but your way of looking at things, your vision doesn't change. One of the things I want to do, if I have time, is to take one passage of the text books in chemistry or biology or physics, say after the War and compare them to what's happened today. You'd see an immense change.
>
> *There would be almost no relationship.*

> That's right. Now take an economics textbook. It's more or less the same. Exposition improves, techniques improve. There is a lot more illustrative material that didn't exist before, but anyway that's my view. So the empirical work doesn't seem to change a vision, or hasn't in economics, but I don't know. It's very tricky, this whole business of how ideas emerge and subjects change, and so on. (Conversation with Ronald Coase, October 1997)

For instance, Keynesian economics, at least at one time, seemed to excoriate any version of Say's Law to such a degree that most serious economists would have blushed had that topic been broached in polite academic circles. Yet, to a certain extent, the embrace of rational expectations in the early 1980s can be crudely characterized as transforming Say's Law by setting it to an insistent disco beat and mounting its proselytizers on platform shoes. Given the still largely non-experimental nature of economics, such recycling of ideas must be seen as inevitable. In fact, many of the same set of incentives and drives that led a new generation of economists to push the Keynesian boat out as quickly and as far as possible were also at work during the rise of rational expectations.

Since economics is a discipline where much depends on the interpretation of past events, the role of ideology tends to come much more to the fore than in other more experimental disciplines.

> The interesting thing is he [George Stigler] was a great enthusiast for quantitative methods. … So, it doesn't seem altogether consistent. But he certainly was. On the other hand, he knew what the answer was going to be. He just regarded it, as I say, as a way of persuading other people. (Conversation with Ronald Coase, October 1997)

While in physical sciences such ideologically tainted battles seem to be staged between practitioners and external agents (think of ecclesiastical battles against the heliocentric theory or the struggle to dismiss evolution), in economics, the competing camps more often reflect internal schisms within the discipline. These theoretic cycles of economic fashion, then, remain tinged by a priori preconceptions. The underlying intellectual core remains the same. As a result, the ideological positions associated with them also refuse to budge. The Chicago approach solidified such partisan positions, while simultaneously claiming the achievements of neutral, fact-based science. In many ways the contentiousness that flowed from such an aggressive strategy has not washed away with the passing of many of its key players.

Periodically, some commentators or self-nominated experts have definitively proclaimed the end of ideology.[54] But if anything, we now live in an increasingly more ideologically divided world where the linkage between rent seeking and belief systems is separated with great difficulty. To be painfully honest, we cannot escape the ideological currents in which we swim. In a surprisingly succinct metaphor, Hegel once posited:

> As for the individual, everyone is a son of his time; so philosophy also is its time apprehended in thoughts. It is just as foolish to fancy that any philosophy can transcend its present world, as that an individual could leap out of his time or jump over Rhodes. (Hegel 2001:18)

While this constraint must be necessarily true, we can continue to guard against the worst excesses of a priori reasoning and at least reject this part of the Chicago legacy. Self-awareness, and a simple recognition of past history, will more easily allow us to realize when we are simply gift wrapping old theories or if we are attempting to tailor evidence to fit our preconceived notions.

> I could disagree 180 degrees with his [Milton Friedman's] policy conclusion and yet concur in his diagnosis or the empirical observations and inferred probabilities. Yet such is the imperfection of the human scientist, an anthropologist studying us academic guinea pigs will record the sad fact that our hearts do often contaminate our minds and eyes. The conservative will forecast high inflation danger on the basis of the same data that leads the do-gooder to warn against recession. (Conscious of this unconscious

[54] This thesis was perhaps most recently and dramatically stated by Francis Fukuyama (1992) in the aftermath of the Soviet collapse. His inevitable triumph of the liberal democratic state has a more than passing resemblance to the end of history idea articulated so succinctly in the work of G.W.F. Hegel. History, undeterred by these man-made conduits, seems to be determined to proceed according to its own inclinations, rather than fulfilling the hopes nurtured by flocks of analysts.

source of bias, as the subsequent discussion will elaborate on, I make a special effort toward self-criticism and eclecticism—with what success, the record must testify to.) (Samuelson 2011c: 888–889)

The Advantages of Simple Story Telling

"We are so very 'umble." (Charles Dickens (1850) *David Copperfield*, Uriah Heep)

Economists are essentially storytellers as Robert Lucas has pointed out.[55] The stories have changed over the years, as well as the way we tell them. Classical economists, certainly since Ricardo, have told fables of distribution, with each weaving tales that identified the key resource dictating distributive outcomes, whether Ricardo's land or Marx's capital. The neo-classical revolution brought markets and exchange to the center of all economic analysis and we have been telling such similar stories ever since. Chicago created and refined the ultimate market story. These series of ever more intricate fables have charmed the very heart of the profession, since we have all been thoroughly trained to believe in the essential magic of markets. That is where our analysis begins in the absence of some very strong extenuating circumstances. As Becker and Stigler realized, academic markets are no different than the more commonly observed ones. The poets of economics must appeal to the given preferences of their audience and convince their listeners that the tales they tell provide greater service flows at lower user costs than the alternatives on offer. Like any other economic agent in the seminal work by Becker and Stigler (1977), economists are expected to weigh alternative theories using a basic cost/benefit analysis. Clearly the profession was receptive to the message that the core of these Chicago economists created over the years. They provided themes so clear and appealing that even today, when the simple faith provided by these stories may no longer hold theo-

[55] Lucas (1988) does this by employing a very pointed and entertaining manner in a surprisingly lively speech. "I'm not sure whether you will take this as a confession or a boast, but we are basically story-tellers, creators of make-believe economic systems." (Lucas 1988:1)

retically, the policy statements honed during that period still remain and contest the political stage, if not the pages of leading journals.

However, an unanswered question is whether economics prospers when economists act more like knights errant[56] than sober sifters of fact. "If that's what the evidence is, that is what it is and we have to, we *have* to, you know at the end of the day, if we are convinced that that is what the evidence is, that is the truth. Not our pride. Our pride is not the truth." (Conversation with Sam Peltzman, October 1997) The problem with this commendable Chicago insistence on quantification and empirical work is that facts refuse to speak for themselves.[57] The danger here is that statistical work becomes just another rhetorical tool to push a preconceived conclusion. "Does it contribute to the progress of the profession to push approaches to extreme, to push a position until it collapses? As far as you can! That's what you're bound to do. That's your obligation as a citizen of a profession." (Conversation with Sherwin Rosen, October 1997)

Quite clearly, the better storyteller need not be the bearer of truth or enlightenment. In this case the considered opinion of Gary Becker, "Moreover, bad ideas may be put persuasively. And they may gain the necessary threshold;" (Conversation with Gary Becker, October 1997)

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[56] Demsetz takes note of Stigler's abiding defence of neoclassical theory: "Evidence of Stigler's attachment to neoclassical price theory is also given by that part of his work mainly critical of the work of others. Price rigidity, administered price inflation, the theory of monopolistic competition, and X-efficiency were prominent targets, and each of them denied the efficacy of the neoclassical analytical framework." (1993:800) Friedland also identifies the surreptitious Quixote motivating Stigler in his campaigns: "Much of his work centered around saving the damsel in distress, neoclassicism, from her attackers" (1993:780).

[57] That is perhaps a fundamental problem lying at the heart of Stigler's "brave new world" presidential address at the AEA meeting in 1964 (Stigler 1965). Evidence is vital and the move toward using statistical methods proved to be essential in improving the profession. But the very nature of the available data creates an irresistible trap for those with strong a priori convictions.

> Leaving George Stigler aside, this is a problem for all economists, as to what the alternative to the controlled experiment is as a way of deciding between models, between different theories when the results are so similar. And we like to say that the econometric estimate is the alternative. And to a certain extent, it is, but I think that life shows that when you are using casual data, and by casual data I mean data that were not collected with this issue precisely in mind, when you are using casual data, it is very difficult to refute, fundamentally to refute a thesis. (Conversation with Robert Solow, November 1997)

may be closer to the mark than that of Edwin Canaan, "However lucky Error may be for a time, Truth keeps the bank and wins in the long run." (Canaan quoted in Stigler 1988:174)

Of course we are all reluctant to surrender any of our cherished theories. They are our creation, our children. But even more dangerous than our vanity is the bedrock of beliefs that may implicitly shape what we produce. Stigler rejected the role of ideology in economics as well as in human action.

> I don't know how important ideology is, but think it is unimportant. You don't know how important it is, but think it is important. My position is better because I try—feebly and so often unsuccessfully—to use a trusted theory of human behavior to explain social phenomena. Your position is worse because you try—with marvelous ease—to explain the mysteries by a deus ex machina. (Letter from George Stigler to Milton Friedman, March 29, 1984)

But this seems a potential form of self-deception. As Knight insisted (though perhaps at times finding it difficult to follow his own dictum), we needs must be modest. Our stories must not be so much epic and heroic, but rather simple folk tales that mute rather than emphasize sharp edges.

Despite Stigler's suggestion,[58] economists should not preach or crusade, not at least insofar as they are economists.[59] Unfortunately, this idealized

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[58] Stigler takes up this issue in, among other places, "The Economist as Preacher" (1982). As Stigler insists, such academics have performed an insufficient amount of preaching. This assertion may be true as far as the explicit version of an economist's output is measured, but implicitly we may be faced with a different story.

[59] Economists of course are humans (at least to a certain degree) with their own sets of beliefs and accordingly are entitled to act passionately in urging causes close to their hearts. But when they do so, they are not acting as economists, no more than a doctor is when urging nuclear disarmament. In this, Stigler (1988) had the correct view in his deprecating characterization of the economist as an expert witness, though he himself proved less than immune in resisting the perfumed temptations of performing that role.

goal cannot be accomplished without some measurable deal of difficulty. Without the greatest care we are all influenced by personal vanity, ambition and ideology. Although we cannot entirely avoid these traps, we can try to be aware of the seductive nature of such problems and attempt to minimize them. In fact, we can be willing to change our minds when evidence points in a contrary direction. In this respect, Keynes had the better of the argument. ("When the facts change, I change my mind. What do you do, sir?"[60]) What evidence, for instance, would make George Stigler change his mind about markets? "He would come across empirical work which was contradictory to other empirical work. Somehow it always seemed to him that the empirical work which favored his side was done better than the empirical work which didn't." (Conversation with James Kindahl, November 1997) If anything, the Chicago approach appears to start by assuming market efficiency and then proceeds to work its way backwards

> Oh, George was a puzzle solver. George was definitely that. As far as George was concerned, I would think that the system building had already been completed by Adam Smith and there was not a hell of a lot of room for him or for anybody else to do that. He was interested, I would say primarily, in a particular sort of puzzle and it's a typical Chicago puzzle. And I don't mean that in any bad way, it's the sort of puzzle that the Chicago School's presuppositions require. Show me an apparent anomaly, something that does not seem to be explicable using the Smithian apparatus and the Marshallian apparatus, and I will show you that it can be explained that way. That was exactly the sort of thing that George went looking for. And

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> I conclude—and perhaps I am alone in concluding—that when the economist goes to Washington, he deserves no more credence, and no less, than any other political appointment, and it is mildly deceptive to address him as Doctor or Professor. (Stigler 1988:136)

[60] Keynes' reply to a criticism during the Great Depression of having changed his position on monetary policy, as quoted in *Lost Prophets: An Insider's History of the Modern Economists* (1994) by Alfred L. Malabre, p. 220. Keynes was also perhaps closer to the mark in choosing generosity over confrontation.

> This means, on the one hand, that an economic writer requires from his reader much goodwill and intelligence and a large measure of co-operation; and, on the other hand, that there are a thousand futile, yet verbally legitimate, objections which an objector can raise … (Keynes 1973:469–470)

> that's not a bad thing. I'd have to say that it can actually be very good. (Conversation with Robert Solow, November 1997)

This type of a priori Chicago approach, which seemed to dominate the output of the department led by Stigler, Friedman, Becker and others was rescued to a great degree by the cleverness and incisiveness of those who practiced the art. The strong point was their ability to ask the right question, upsetting the prevailing dogmas and eliciting a useful questioning of assumptions. The negatives were the formation of a counter-dogma. Economists under an ideological sway approached these theories privileged to comprehend a priori how the story must necessarily end. What kept such analysis interesting, then, lay not with the expected conclusions, but rather in discovering how these Chicago people were able to chart a course through which they could successfully reach their desired objective. Chicago output also displayed a strong proclivity for those strategies capable of putting forward the unexpected explanation, displaying cleverness by stressing the unintended consequences attached to any market intervention.

> Now, what you have to understand with somebody like Allen Wallis, and so to a degree those people who were in his circle, is that Allen Wallis had the sharpest priors—I'm using the language of Bayesian probability—of anybody I ever knew. Almost no new data could change his view for this reason. On the other hand, if he thought of somebody as a dangerous, or an incompetent thinker, but Jimmy Savage assured him that the man was very smart and had good judgment, that carried more weight with Allen Wallis than a two-year study of the person's vitae and an audit of his writings. There's an in-group of the good guys and the much larger out-group… But it was interesting that in this particular group, this also was the case with Allen Wallis, for example, they liked the complicated explanation better than the simple one. It was kind of like syncopation, always the after-beat. So there was an awful lot of nonsense going on. (Conversation with Paul Samuelson, November 1997)

If then obstinacy, an obsessive reliance on sharp marketing and a "take no prisoners" approach are a few unwelcome bits of the Chicago legacy still clinging to the profession, what are the positive aspects we are still able to welcome? Most important would be a certain fearlessness in questioning received dogma and in being willing to rebel against accepted traditions. The other might be an insistence on quantifiable evidence and the need to carefully test any proffered hypothesis. And perhaps most important of all is the insistence that the endeavors of a working economist have a clear connection to the world around us.

> You don't have much of that kind of theory building that has no empirical bottom line. We're, especially the business school faculty, pretty much agreed that that is not the way to go.
>
> *It is clear that mathematical theory, mathematical models were in that category as well.*
>
> Yes. Yes, not just game theory, but any enterprise which starts to become an end in itself… You understand what I mean. It's when all that is important is whether you got the thing right. That's not important. What's important is that you say something about the world. (Conversation with Sam Peltzman, October 1997)

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Love among the Ruins: Understanding *The Romantic Economist* by Richard Bronk (2009)

> "No," replied Elinor; "her opinions are all romantic." … "A few years, however, will settle her opinions on the reasonable basis of common sense and observation; and then they may be more easy to define and to justify than they now are, by anybody but herself." (Austen 1933a [1811]:33)

Titles can be eye-catching, inflaming the imagination and making a book stand out from its blander brethren. Unfortunately, most economic volumes have usually been notable for their ingrained and irremediable dullness. Only cookie cutter-type monotony seems to grace their grey and indistinguishable covers. The results resemble what might be spat out by a cut-rate random design program. The titles, and their forgettable covers, seem intended to repel any potential customer, to defy any but the most self-motivated from picking up the volume.

As a result, a reader discovers only joy in the unlikely juxtaposition of two terms which seldom find themselves frequenting the same

C. Freedman, *In Search of the Two-Handed Economist*,
DOI 10.1057/978-1-137-58974-3_4

neighbourhood.[1] Romanticism is permeated by the sweat of passion, while economics conjures a vision of eschewing all else save a razor-sharp rationality. Bringing them together creates the type of unsettling whimsy produced by imagining Sherlock Holmes coming out of the closet and embracing Doctor Watson.

To convey the seeming elegance and even deliciousness (though marinated with the slightest whiff of absurdity) of the title, imagine trying to pitch a film project wrapped with that forbidding label. Perhaps *The Amorous Accountant* would give it a run for its money, but both would be stigmatized by the strong scent of certain failure. Any studio chief would instantly conjure up screaming headlines in that bible of show biz (*Variety*) reading "Film fails to add up" or "Feature makes no cents." The author, however, is choosing to be deliberately provocative in attempting to bring together a nineteenth-century literary and philosophic movement with the science of self-interest and market prices. Bronk's aim, which he clearly confesses, is to pinpoint the dangerous murmur lurking within the ultra-rational heart of economics. Namely, he contends that the discipline has long been self-handicapped by the limited metaphor driving its analytic engine. Bronk attempts not only to diagnose the illness, but to prescribe a restorative homeopathic remedy as well. Bluntly put, he sees a distinct advantage in successfully levering economics off its self-imposed Procrustean bed composed of a single, stunted metaphor or paradigm.

[1] Unusually, some considerable thought has been invested in devising a cover that is both appropriate and provocative, overlaying a reproduction of a painting by Romantic artist Caspar David Friedrich with a photo of the City of London skyline by Martin Twomey. Sadly, the effort doesn't really bear fruit and slips rather too easily into the "tried, but failed" basket. Covers should signal or reflect the material waiting to be unlocked inside. But doing this requires a delicate visual balance to be achieved, which is at the same time arresting even to a casual browser. In the Bronk book, you can certainly tick off the essential contrast between romanticism and calculation. Nor could you choose a better representation of the German Romantic Movement as part of the cover art. Perhaps part of the reason I find the end result a bit tired is my inability to fully appreciate the art produced by Caspar David Friedrich. But I suspect what mostly makes the cover something that the eye simply slides over is a combination of the greying and fuzzing down of the original works, as well as the fact that the marriage of the images doesn't really work. This hardly bodes well for the central theme of the volume, which would seem to be somewhat akin to a shotgun wedding.

The Magic of Metaphor

> If a physician mishandles a number of patients, there is the danger that they will lose their lives. If a teacher interprets a poem to his students in an impossible manner, "nothing further happens." But perhaps it is good if we speak more cautiously here. By ignoring the question concerning the thing and by insufficiently interpreting a poem, it appears as though nothing further happens. One day, perhaps after fifty or one hundred years, nevertheless, something has happened. (Heidegger 1967:53–54)

Almost from the inception of the discipline in the nineteenth century, economists have silently (and not so silently) revelled in an invariant sense of intellectual superiority as the self-appointed scientists of the social domain.[2] Economists felt compelled to dispel flights of fancy with the hard glare of deductive logic. Like Plato so many

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[2] The early split between the Romantic Movement and the rising class of economic calculators in part stems from the irritating certainty with which many of its proponents issued their conclusions and formulated their laws.

> Indeed, some of those who felt that the free working of the market was the only long-term solution to mass poverty criticized the possessing classes for their feeble response to mob violence. The Rev. Thomas Malthus denounced "the extreme ignorance and folly of many of the higher classes … particularly the clergy." He said that by denouncing "middlemen" as the cause of shortages they actually encouraged riots—"half the gentlemen and clergy of the country," he wrote, were thus liable to be prosecuted "for sedition." Both the country gentry and the urban middle class—whose social conscience became an important phenomenon from the 1790s onwards—took elaborate steps to interfere in the workings of the market, much to the fury of experts like Malthus. (Johnson 1991:163)

Perhaps Edmund Burke, whom Bronk somehow ensnares within his Romantic net, best sums up the disdain for the cold-blooded calculations of those early economists.

> The age of chivalry is gone.—That of sophisters, economists, and calculators, has succeeded; and the glory of Europe is extinguished for ever. (Burke 1790:113)

The unattractive dogmatism that inflicted some of these early economists (and many of them since) has been pointed out by their brethren who tended to take a more flexible approach to the discipline.

> Their [Ricardian economists'] agreement with one another made them confident, the want of a strong opposition made them dogmatic; the necessity of making themselves intelligible to the multitude made them suppress even such conditioning and qualifying clauses as they had in their own minds. Therefore, although their doctrines contained a vast deal that was true, and new, and very important, yet the wording of these doctrines was often so narrow and inelastic that, when applied under conditions of time and place different from those in which they had their origin, their faults became obvious and provoked reaction. (Marshall 1923: 759–760)

centuries before, economists have dismissed poets, and others of their ilk, as unsuitable contributors unlikely to prove worthwhile partners in any serious conversation.[3] For Bronk, the intolerance and narrow-mindedness of rejecting alternative approaches and starting points out of hand, displays a self-imposed poverty limiting the parameters of economic thought. If one is willing to distort the author's more complex objectives through extreme simplification (and who could easily resist such a temptation), Bronk's book explores the convolutions of economic methodology. He wants to determine and illustrate how economics as a discipline can best be explored and understood. His answer (again leaning for comfort on the extended crutch of ruthless simplification) is that any set of tools and methods that work together well should be employed in a measure proportionate to the problem at hand. This approach implies a quite broad rather than narrow road to economic enlightenment. From this perspective, the discipline remains such a complex subject that everything, including the proverbial kitchen sink, needs to be tossed at the varied array of problems facing the earnest economist.[4] The real skill and finesse comes in the attempt to balance all the necessary elements of analysis. This poses a nearly impossible challenge since the correct recipe for success may change with each and every case, sometimes only negligibly, but at

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[3] Plato pushes this point much more explicitly, deliberately castigating poets for their inability to provide rational explanations or to enlarge to any degree reasonable understanding. Perhaps this is an early statement of Snow's (1959) *Two Cultures* which provides one of the platforms from which Bronk launches his critical analysis of mainstream economics. Certainly, characterizing economists as possessing literary skills became a back-handed compliment within the profession.

> For a poet is indeed a thing ethereally light, winged, and sacred, nor can he compose anything worth calling poetry until he becomes inspired and as it were mad; or whilst any reason remains in him. For whilst a man retains any portion of the thing called reason he is utterly incompetent to produce poetry or to vaticinate. (Plato 1821)

[4] A perennial issue in economics continues to be the usefulness of the mechanical metaphor given the complexity of any economic system. It may be a convenient simplification or equally an unfortunate distortion.

> The Walrasian model is admirably suited to the study of the incentive function of prices, the performance of which is a necessary but not sufficient condition for the coordination of a decentralized system. But from the standpoint of dynamic theory, it remains what cyberneticians term a clockwork model—a strictly deterministic, Newtonian mechanism in which prices perform the functions of levers and pulleys. (Leijonhufvud 1968:394–395)

other times more significantly. The degree of judgment required could necessarily balloon rather exponentially.

The reader is then faced with a book that is not just a critical analysis of the shortcomings of modern economics, but rather a method for mending some of those flaws by re-imagining the central metaphor that powers the rest of the apparatus. Moreover, to pacify a good deal of the predictable resistance to any such change, the author strategically suggests that much of the current tools and concepts defining the prevailing orthodoxy can (and moreover should) be retained, although not applied quite so broadly as is the common practice within the profession. Theories should be tailored to, or at least selected for, the specific theoretical requirements in much the same way that fish may be fried or grilled to meet given objectives with neither method of cooking being in and of itself a superior mode. Perhaps more controversially, Bronk foresees a new approach that would combine elements of different theoretical modes equivalent to combining cooking techniques to yield a more succulent result. He is therefore not proposing a more general theory with the current approach reduced to a special case. Rather we are asked to grapple with a fortified and expanded version that incorporates seemingly contending methods.

The core issue in evaluating Bronk's work must therefore lie with the metaphor and associated methodology he chooses as appropriate for unlocking the secrets of actual economies. Part of the task of promoting such an approach will lie not only in its explanation but in a convincing application to suggestive economic issues. Unless the author finesses the difficult hurdles required, the judgment on such efforts must rely on the ancient Scottish option of "not proven."

Romanticism: Method or Malarkey?

> Because, said Scrooge, a little thing affects them. A slight disorder of the stomach makes them cheats. You may be an indigestible bit of beef, a blot of mustard, a crumb of cheese, a fragment of an underdone potato. There's more of gravy than of grave about you, whatever you are! (Dickens 1875:27)

We all prejudge and are slaves to our ideologies, if not mere victims of our own whims and prejudices. So I must confess to being put off initially by a volume combining two aspects which manage to inevitably annoy, namely romanticism and methodology. Romantics seem singularly categorized by a perpetual yearning for the unobtainable, something wished for but basically non-existent. Such spiritual groundings seem to be the case whether the individual is looking back to an idealized history or forward to a future incapable of eventuating. The result creates an individual who somehow can't resist being perpetually dissatisfied. The yearning seems directed toward either the uncorrupted world of the noble savage or the future freedom bestowed by some utopian society.[5]

Methodology retains a somewhat disreputable edge for me ever since reading *Rules for the Direction of the Mind* and *Discourse on Method* (Descartes 1988) as an undergraduate. The author betrayed a desire to be ineffectively prescriptive rather than descriptive, creating a sort of early intellectual self-help volume. Ever since, methodology has remained mostly an exercise in self-improvement, describing how the investigator assumes individuals should think and analyse, rather than the way that they actually do. Instead of accepting the way individuals operate as a given, people's mental procedures are assumed to be easily programmable. Economics is perhaps notorious in this regard with the most famous consideration of methodology originating from a desire to close down any future questioning in that area.[6] The result, however, was the con-

[5] In one case the reader is asked to believe that humans are born innocent and initially good, while the other forgets that the literal meaning of utopia is "no place." The eternal dissatisfaction clinging to Romantic thinkers is best exemplified by Rousseau (1953) in his *Confessions*. Rousseau is what Bronk (2009:247) chooses to categorize as belonging to "the cult of sensibility," a forerunner of the nineteenth-century Romantic Movement. (The Earl of Shaftesbury is also in this group, the man often credited with the questionable concept of the noble savage.) In his *Confessions*, Rousseau admits to being unable to perform with a beautiful courtesan on finding that her right breast failed to be symmetrical with her left.

[6] On the 50th anniversary of its publication, the author, Milton Friedman (1953), speaking at the annual American Economic Association meeting via a phone hook-up, categorically insisted that he had said all that needed to be effectively stated and could see no reason to alter his original analysis. However, placed in the context in which it was written, Friedman and his close friend George Stigler were intent on pushing back the challenge of judging theories based, at least in part, on the soundness of the assumptions chosen. The aim was to shut down further methodological discussion rather than to encourage further debate.

struction of a methodology that was neither descriptive of the work of economists nor even potentially operative according to Friedman's own colleagues.

> When I was a graduate student we were taught a paradigm of how you do research. I've got to tell you, it's all wrong. It's not the way we operate. We don't sit up here and develop hypotheses and go out and test them. That's just not what we do. George taught me that. Milton taught me that. They're wrong! And I understand that. I'm older enough now to figure out that's not the way we do work. (Conversation with Sam Peltzman, October 1997)
>
> If all economists followed Friedman's principles in choosing theories, no economist could be found who believed in a theory until it had been tested, which would have the paradoxical result that no tests would be carried out. This is what I meant when I said that acceptance of Friedman's methodology would result in the paralysis of scientific activity. Work could certainly continue but no new theories would emerge. (Coase 1994a:24)

Bronk clearly feels, however, that economics employs too narrow a perspective. He pushes for a discipline that displays more openness and a willingness to experiment with different systematic approaches. This desire, at least on a surface level, seems unexceptional since narrow, pre-emptive attitudes combined with defensive reactions to original ideas seldom provide the paving stones of a productive path to intellectual progress. The underlying idea is to shift decisively away from the mindset that seeks something like a unified general theory, acting as an abstract Holy Grail

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The view that the worth of a theory is to be judged solely by the extent and accuracy of its predictions seems to me wrong. Of course, any theory has implications. It tells us that if something happens, something else will follow, and it is true that most of us would not value the theory if we did not think these implications corresponded to happenings in the real economic system. But a theory is not like an airline or bus timetable. We are not interested simply in the accuracy of its predictions. A theory also serves as a base for thinking. It helps us to understand what is going on by enabling us to organise our thoughts. Faced with a choice between a theory which predicts well but gives us little insight into how the system works and one which gives us this insight but predicts badly, I would choose the latter, and I am inclined to think that most economists would do the same. (Coase 1994a: 16–17) for economic investigation.[7] In Bronk's view, there are occasions when the prevailing mainstream theory can be usefully transformed to produce a more complex and interdependent approach encompassing and perhaps fulfilling the skeletal and abstract limitations reflected by the economist's usual bag of tools.

> To use a specialist language—and so structure our vision according to a different conceptual and metaphorical framework—may be very useful in giving us a different perspective on the same situation. But to avoid completely the "language really used by men", as much economic analysis does, runs the risk of missing what, from the actors' point of view, are the key elements of the situation or problem being analysed. Economists should therefore follow Wordsworth's example and avoid an over-reliance on specialist economic terminology and the dehumanised language of mathematics and algebra, except in situations where their use yields important analytical gains in relation to a particular problem. In other words, the choice of language should always be driven by the nature of the problem being analysed; and, crucially, the true nature of the problem can often be best assessed in the first instance in the multifaceted language of everyday usage. (Bronk 2009: 17)

What Bronk makes clear by insisting that economics has for too long clung to the wrong metaphor, that Romanticism rather than Mechanics provides a more powerful tool, is his unflagging belief that the Mill solution to the problem is inadequate. In other words, though sympathetic, he rejects the alternative method favored by many classical liberal economists to perform the nitty-gritty tasks of applied economics.[8] John Stuart

[7] This drive is reflected to some degree in the post-war Chicago School and particularly the work of George Stigler who spent his career expanding the reach of rational choice theory and the related theory of perfect competition.

> It's getting more and more, more and more part of him as he got older actually, this whole view. He insists it's rational. He would tell you, "There is some rational explanation for it. It's just that you haven't looked completely into it and found it."(Conversation with Sam Peltzman October 1997)

[8] To forestall any potential confusion, David Ricardo and his influential work of this period has to be eliminated from this category of classical liberal economists. More than anyone else at this formative stage he exemplifies the peculiar vice of jumping from abstract and broadly generalized theory to specific policy recommendations.

Mill certainly appreciated the limitation of the theory he helped formulate (and which would dominate economics for decades) but for him it remained the best starting point for analysis.[9]

> Political economists generally and English political economists above others, have been accustomed to lay almost exclusive stress upon the first of these agencies, to exaggerate the effect of competition, and to take into little account the other and conflicting principle (custom). They are apt to express themselves as if they thought that competition actually does, in all cases, whatever it can be shown to be the tendency of competition to do. This is partly intelligible, if we consider that only through the principle of competition has political economy any pretence to the character of a science. (Mill, 1967: 242)

Mill firmly believed that careful judgment took over when analysing any specific case. In practise this meant that the mechanical metaphor in favor didn't yield an automatic solution, but that in fact different economists could reach opposing policy recommendations based on their evaluation of concrete situations.[10]

> No one who attempts to lay down propositions for the guidance of mankind however perfect his scientific acquirements, can dispense with a practical knowledge of the actual modes in which the affairs of the world are carried on, and extensive personal experience of the actual ideas, feelings, and intellectual and moral tendencies of his own country and his own age. (Mill quoted in Bronk 2009:54)

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[9] This approach left one free to argue over theory and to put forward alternatives; however each and every one would be left with the problem of how to make any conclusion operational. The leap from theory to policy would remain illegitimate.

> More generally, it can be said that establishing a theory is somewhat like passing a curve through a certain number of fixed points. An infinite number of curves can satisfy this condition (Pareto 1971: 31–32).

[10] Many of the policy tenets ascribed to classical liberal economists seem to be based on observation, history and experience rather than the conclusion of any theoretical deductions. This didn't mean that such a stance left them free from dogmatic stances or ideological imperatives, but it did help curb the worst of these excesses.

> People understand their business and their own interests better, and care for them more, than government does or can be expected to do. (Mill 1967:947)

Perversely, even some of those modern economists who would later sin against these liberal precepts still recognized the methodology that provided such economists as Mill with their resilience and perspicacity.

> The discrepancies between pronouncements and practice are notorious in the field of methodology; can it not be so also in the theory of value? In writing their treatises, may not the classical economists have employed an apparatus which is different and in modern eyes inferior to that which they employed to analyse concrete problems? (Stigler 1949: 25–26)

Bronk chooses to soundly reject this method as being an insufficient palliative. The problem for him lies in the theoretical approach adopted by economists which no retrofitting of facts and conditions can adequately rectify. Mill is dismissed as failing to comprehend the inherent fault at the heart of his method. Thus for Bronk, a more inclusive approach based on an entirely different central metaphor must characterize economic analysis.

> Mill believed that the assumptions made by economists (constituting the "general laws" of economic behaviour) can be used to predict at least a significant part of the truth in the real world; and he believed that the effects of other relevant causal factors ("disturbing causes") could be predicted on the basis of laws from other relevant social sciences, and then "added to, or subtracted from," these economic partial truths to produce the whole truth. In short, he saw the different causal factors operating in societies (and captured by different social sciences) as like the different forces in a Newtonian force field that, when combined, behave in an entirely predictable and systematic manner. John Ruskin realised the error of this in *Unto this Last*—his late-Romantic diatribe against Mill's political economy. Ruskin noted that in human social affairs, disturbing causes "operate, not mathematically, but chemically": that is they alter the essence of what is going on. (Bronk 2009:55)

The use of Ruskin, Coleridge and Carlyle as exemplars of Romantic methodology betrays an avoidable error of judgment.[11] This potential difficulty is amplified when using their critical attacks on economics as a basis for useful analysis. Relying on Ruskin to dismiss Mill's approach is especially dangerous since Bronk's analysis seems at best grounded in the early Mill, one still in thrall to his dominating father, James Mill. To reduce Mill's analysis to a simple summation of Newtonian forces seems to badly misunderstand his approach and to underestimate the role judgment played in his work.

> The rigid outlines of the doctrines of Ricardo and the earlier Mill were partly caused by the temporary return of Europe between 1790 and 1820, to a reign of violence: and John Stuart Mill was so much under their influence when young, that he laid down canons as to the province of the economist which seemed to exclude the use of all faculties except the intellectual (Marshall 1923:764)

The problem is that Bronk appears to misunderstand what Mill is assaying when analysing the investigatory method appropriate to economics. Mill here is looking at what he would term the science of economics, which produces what truths can be known about the subject. It is possible to argue for an alternative method. However it is incorrect to confuse this issue with the application or art of economics. What Bronk fails to do is to explain why the two-step process is unsatisfactory. It would seem that adopting his alternative Romantic metaphor would still leave us with the need to exercise art and judgment in formulating policy. Questioning Mill's theoretical approach is certainly a useful exercise, despite the evasiveness of Ruskin's criticism. The doubts expressed seem to a certain degree sensible.

[11] Carlyle remains a classic Romantic appealing to a past that never was. The fact that Mill's maid accidentally burned the only copy of Carlyle's first volume of his *The French Revolution: A History* could have hardly endeared Mill to Carlyle. However, his dyspeptic personality does not, of course, negate the validity of any of his criticism of the economic discipline nor that contributed by his Romantic cohort.

> It was very good of God to let Carlyle and Mrs Carlyle marry one another and so make only two people miserable instead of four. (letter between Samuel Butler and E. M. A. Savage, November 21, 1884)

But what it precisely means and how a practical implementation would operate seems as elusive as anything offered by Mill.

> This constitutes the only uncertainty of Political Economy; and not of it alone, but of the moral sciences in general. When the disturbing causes are known, the allowance necessary to be made for them detracts in no way from scientific precision, nor constitutes any deviation from the *a priori* method. The disturbing causes are not handed over to be dealt with by mere conjecture. Like *friction* in mechanics, to which they have been often compared, they may at first have been considered merely as a non-assignable deduction to be made by guess from the result given by the general principles of science; but in time many of them are brought within the pale of the abstract science itself, and their effect is found to admit of as accurate an estimation as those more striking effects which they modify. The disturbing causes have their laws, as the causes which are thereby disturbed have theirs; and from the laws of the disturbing causes, the nature and amount of the disturbance may be predicted a priori, like the operation of the more general laws which they are said to modify or disturb, but with which they might more properly be said to be concurrent. The effect of the special causes is then to be added to, or subtracted from, the effect of the general ones. (Mill 1844:v.59)

Wandering Lonely as a Cloud: The Romantic Metaphor Undressed

> This dull product of a scoffer's pen. (Wordworth 1814: *The Excursion:*Bk.ii, line 484)

The Romantic period was in some sense a reaction to the eighteenth-century Age of Enlightenment, which spawned Adam Smith and his close friend David Hume. It gave birth to poetry, literature, art and philosophy. Bronk chooses to cast his net as widely as possible in capturing the collection of thinkers that defines his proposed methodology, embodied by his creation of the newly born Romantic Economist. While remaining open to the charms of this nineteenth-century movement, it is still wise to remember the innovations and achievements defining the

Age of Enlightenment. Individual self-interest, previously condemned as avarice, gained a new and powerful role in curbing and controlling the destructive passions infecting mankind. Bettering oneself through commercial activities became a method of unintentional control, supplanting avenues of persuasion or force which had proven ineffective in the past.[12]

Bronk concludes that economics took a wrong turning in hitching its progress to the Mr. Spock-like rationality of the Enlightenment and by failing to absorb the lessons provided by Romantic thinkers. To some extent Bronk attempts to rehabilitate the trio composed of Coleridge, Carlyle and Ruskin whom Levy and Peart (2001) had previously exiled to economic Siberia. These two authors surgically portrayed these essayists as reactionaries attacking the progressive ideas pursued and supported by nineteenth-century economists. Whether Bronk airbrushes away or simply dismisses their less salubrious ideas is a niggling but not directly relevant question. But perhaps unintentionally, there is a tendency to glean from these authors, ideas which seem to buttress the conception of Romantic Economics and to slide over the more dubious thoughts which they championed.[13]

Richard Bronk, however, has a much more ambitious agenda than the simple desire to restore Romantic ideas and methods to their rightful place. Instead, he pursues a broader quest, perhaps one that manages to be even more quixotic than that conducted by Cervantes' famous Don. Tilting at a few stray windmills doesn't measure up to the attempt to integrate, deeply and seamlessly, two seemingly irreconcilable approaches, rationality and romanticism. In doing so Bronk takes aim at mainstream economics, but in a fashion that sometimes muddles or at least blunts his

[12] Centuries later this approach was deftly summarized by the idea that "It is better that a man should tyrannize over his bank balance than over his fellow-citizens and whilst the former is sometimes denounced as being but a means to the latter, sometimes at least it is an alternative" (Keynes 1964:374). Albert Hirschman (1997) devoted an entire book to exploring the revolutionary approaches that sprang forth from that Enlightenment idea of self-interest. This intuition has a much older heritage. The concept of unintended positive consequences, created through myopic self-interest, finds something of a reverberation in the work of the ancient Greek writer Galen (1916), who saw an anatomical concordance arising out of the individual operations and objectives of the bodies' organs.

[13] In particular, Levy and Peart (2001) see nineteenth-century British economists as supplying an essentially progressive voice in that era. Carlyle's (1849) attack on Mill and on economics as being the "dismal science" stemmed from his determined refusal to accept human equality, mounting instead a defense of slavery.

objective. At times Bronk focuses on producing unfavorable contrasts by comparing nineteenth-century economic approaches to the thoughts and investigations of their Romantic contemporaries. But, the sharp wedge of his attack seems mainly to centre on the discipline's post-war developments. A formalized, mechanical paradigm is often contrasted with a more imaginative and complex Romantic alternative. Unfortunately the relevant context and critical connections are not always clarified, leaving the target of his critical attacks insufficiently identified.

Bronk trips through the Romantic era cherry-picking those aspects he finds amenable to construct an idiosyncratic metaphor for a rejuvenated economics. Herder (rather than Goethe) becomes something of a benchmark, with the elastic idea of *organicism* (the use of organic metaphors) forming a mainstay of Bronk's constructed analysis.[14] Resurrecting biological metaphors inevitably creates a forced march to Marshall, who supported such an approach but resorted to mechanical metaphors for their ease of operation.[15] Like Mill, Marshall felt that the judgment and skill required in applying theories could compensate for the shortcomings of theoretical constructs.

> Many Romantic writers advocated *organicism* – the use of organic metaphors from biology to understand the nature of social interaction and the workings of the mind. Perhaps the most important of these was the German

[14] The English Romanticism that Bronk explores had definite roots in the major German thinkers and artists of the late eighteenth and early nineteenth century. (Notice it is a painting by Caspar David Friedrich that partially composes the cover art.) Bronk however chooses to rest much of his case on Herder and Schlegel despite Goethe and Schiller being more towering figures of that era. For that matter, though classified as a German Idealist, Hegel's exclusion from this group, which includes Nietzsche but bars Marx, is far from self-evident. (As something of an aside, Bronk attaches a Hegelian label to the well-worn structure of thesis/antithesis/synthesis. Unfortunately, this bit of analysis can be laid more precisely at the door of Fichte, even though commonly attributed to Hegel.) On examination, what these Romantic club members seen to have in common appears less striking than what differentiates each one from the other. Accusing Bronk of contriving associations through convenience would be going too far. Such charges would dismally fail to survive any careful scrutiny. Bronk is too scrupulous to fall into any such obvious errors. Yet, the particulars that tie together his chosen Romantic groupings can still leave a reader feeling more than a bit uneasy.

[15] Volume I of Marshall's *Principals* (1920) clearly focuses on static equilibrium analysis using a mechanical analogy. Whether the unwritten Volume II on dynamics would have employed something resembling economic biology can only be a matter of supposition since the volume was never written. Nevertheless, Marshall firmly believed that complexities of biology offered a more conducive path than mechanics. "The Mecca of the economist is economic biology rather than economic dynamics." (Marshall quoted in Brinley 1991:1) For further analysis of Marshall's objectives and methods see Hart (2012).

> philosopher J. G. Herder. In stark contrast to the use of mechanical metaphors by most Enlightenment philosophers and scientists, he set out a consistently organic vision of human societies, the human mind and even the physical universe. In each case, Herder used the organic metaphor to signal that the whole is more than the simple sum of the parts—its nature and development being a function of the mutual complementarities and complex self-reinforcing interdependence of the parts. (Bronk 2009:88)

Bronk delivers an ample helping of general discussion, baked in somewhat abstract terms, about the Romantic metaphor and the potential insights its application might provide. It is clearly messier than the more mechanical, ball-bearing-hitting-ball-bearing structure that has in the past enamoured the economics profession. As expected, reason has to be balanced by sentiment and imagination, mixed together in precisely the right proportion. Logic should not be simply linear but interwoven in a complex and nearly unfathomable fashion.[16] On the surface these are unquestionably admirable aims. The challenge is how to make them operable.

> There are four broad sets of Romantic *themes* highlighted in this book, each of which is common to many, but not all, "Romantic" writers. These are

[16] Based on these criteria, Marx would seem like a natural candidate, or at least as capable of providing an accurate template, for the role of Romantic Economist that Bronk draws as a scholarly exemplar. If we accept the Althusserian interpretation of Marx, where over-determination rules out the essentialism disliked by Bronk, then rejecting Marx most likely reflects a failure to understand precisely what dialectical materialism attempts to accomplish. Anything resembling a mechanical metaphor seems to predetermine Bronk's judgment. There are a multitude of reasons for finding Marx wanting, but misinterpretation lacks any grounds of legitimacy. Marx may have dismissed imagination and sentiment, not because they fail to make a contribution, but because they could reduce the clarity that Marx sought in his constructed framework.

> Nevertheless, Marx—like the classical economists—thought that organicism has no role to play in understanding the capitalist system itself. Indeed, his invective against capitalism is derived almost entirely from seeing it through the prism of the machine metaphor favoured by classical economists. So for example, in *Das Kapital,* he wrote that capitalist methods "mutilate the labourer into a fragment of a man, degrade him to the level of an appendage of a machine"; workers, he thought, are alienated from their true nature by the inhuman functioning of the capitalist machine, which turns them into mere commodities. There was, of course, some truth in Marx's diagnosis of the wretched state of the labourer at the time of the early Industrial Revolution. His failure, however, to see that the mechanical metaphor may not be the best template for understanding or realising the potential of the capitalist system contributed significantly, I believe, to the ultimate failure of his doctrine. (Bronk 2009: 146–147)

> the importance of organic rather than mechanical metaphors, especially when applied to society or the mind; value pluralism and the absence of any single scale of value; the need for a fuller psychology of human motivation than is allowed for by some versions of rationalism—one that recognises, in particular, the role of imagination and sentiment as well as reason; and finally, the key roles played by language, perspective, metaphor and imaginative intuition in mediating our perception and understanding of the world we live in. One hypothesis examined here is that these four themes are interdependent and mutually reinforcing in the sense that each one of them is more obviously valid if the others are also recognised as true. A second related hypothesis is that these themes together entail or suggest a number of practical Romantic *lessons* for how we should live out, and understand our lives. (Bronk 2009:87)

Some confusion naturally arises as we flit back from the Romantic period and the antagonism these thinkers harbored toward economic thinking and then forward to modern-day economics. However, the heart of the book lies in presenting an alternative to the dominant modern economic paradigm (or what is presented by Bronk as representative). It might then be more effective to limit the overly extended discussion of nineteenth-century economics. Particularly since the essential hostility, whether from more progressive representatives like Byron and Shelly, or from those with a decidedly reactionary bent, namely Wordsworth, Coleridge and Carlyle, is not sufficiently clarified despite being examined at great length. Romantic writers and thinkers were not averse to rational thought or to science.[17] Remember that the beginning of the nineteenth century marks the formation of modernism, plus the start of the industrial revolution. Coleridge, for instance,

[17] In the early nineteenth century, the line between poetic imagination and scientific hypothesis was not sharply drawn. In the excitement generated by the construction of a new, modern world, inspiration was welcome no matter what the source. Poets were not to be lightly dismissed.

> A more likely explanation, in Coleridge's view, was that "all *power* & vital attributes" depended on "modes of arrangement." Davy, thus prompted, put it in more "scientific" terms: "forms of natural bodies may depend upon different arrangements of the same particles of matter." This foreshadowing of atomic theory is a striking demonstration of the importance of imagination in forming scientific hypothesis: Coleridge and Shelley could see possibilities in nature with the intuition of poets and so open the eyes of the experimental scientists. The early nineteenth century was a great age of science precisely because it was also a great age of poetry, (Johnson 1991: 555–556)

was keenly interested in scientific controversies and discoveries. What all these members of the Romantic movement uniformly shared, clearly seen in the poetry of William Blake and forming the subject matter of the novels of Charles Dickens, was an abhorrence of the changes wrought by the industrial revolution. In their eyes, economists seemed either blind to the poverty and misery produced by this process or Candide-like in their faith in the progress and rising wealth that this new system would produce.[18] Certainly insisting that iron-clad laws of motion propelled an economy reflected a belief that the sting of serious unintended consequences would counteract any well-intentioned interference in the unfolding of these principles.[19]

Bronk's nineteenth-century wanderings are extensive. Clearly he has utilized a wide range of Romantic thinkers in distilling their insights into an essential few. (Though I remain puzzled as to the wisdom of evaluating Burke and Shelley as even distant soulmates.) This tramp through the Romantic century and his extensive look at the economists of that era, both those of whom he approves and others he finds lacking, encourages a good deal of thought but in some respects remains an elaborate sideshow. The real issue seems unalterably to reside in the provision of an alternative (and superior) paradigm to the current (as opposed to the nineteenth-century) structure of economic analysis. Bronk rightly feels

[18] Bronk's preference for poets rather than novelists is curious. Dickens' sentimentality and yearning for a bygone and kinder world dominates his crusading writing. His works are full of the downtrodden poor as well as grasping and single-minded capitalists to an extent worthy of any Marxian diatribe.

[19] It is wise to keep in mind that during the first few decades of the nineteenth century, the conclusions of the new science of economics were propounded with certainty. Its laws seemed more unalterable than those of the physical sciences.

> Economics was not so much a subject to be studied as a theory which its advocates believed was unarguable. The only problem, in their view, was how to teach it to the working classes, to stop them from burning hayricks and smashing machines when times were hard. Its "iron laws" were as true and immutable as Isaac Newton's. As the *Quarterly Review*, by no means extreme in such matters, put it in October 1825: "It would be a real blessing if the working classes could be made acquainted with some of the fundamental principles of Political Economy, such as the laws of population … the circumstances which regulate the market of corn, or the market of labour. They would then perceive that inequality does not originate in the encroachments of the rich or the enactments of the powerful, but has been necessarily coeval with society itself in all its stages; they would learn that the recompense of labour is governed by definite principles—and we are grateful for any measures which may tend to diffuse such knowledge." (Johnson 1991: 862–863)

compelled to explore the limitations of the ideas dominating economics in order to clear the way for a more viable substitute (or a paradigm) incorporating current analysis, but expanding this method in a way that fills out the gaps and lacunae. It is here that one of the key weaknesses of the book appears. Though Bronk has clearly immersed himself in much of the critical literature and alternative approaches surrounding the discipline, a careful reading of his argument leaves the reader wondering about his grasp of mainstream economics. He seems to look at the surface theory without fully understanding the dominant metaphor or back story that drives it. Nor is there any recognition of the extensive literature that takes up many of the questions he raises despite largely defending the mainstream approach. Bronk is careful to maintain an appearance of balance in his evaluation and always pulls back from the brink of overstatement, but the echo of tone-deafness remains when he trots out a standard list of criticism.

Making matters worse is a seeming confusion at times between the construction of theories and making those same theories operable. The problem stems from a misunderstanding and rejection of Mill's staged approach to economics. Concerns that would seem more effectively dealt with at the application stage become shifted into the more abstract theoretical level. Such a procedure is more than a little unsettling. Perhaps my weakness is that I am liable to become counter-suggestive when a position is oversold by overlooking or misinterpreting a contrary view. Although I can't find myself disagreeing with many of Bronk's conclusions, the road down which he chooses to journey remains questionable.

Weasel Words: Clarifying by Obfuscation

> For more than twenty years I have been telling my students that one of the widespread uses of "Statics" and "Dynamics" was to distinguish a writer's own work from that of his opponents against whom he tried to argue. Typically, "Statics" was what those benighted opponents have been writing; "Dynamics" was one's own, vastly superior theory. (Machlup 1959:100)

In Bronk's vast flurry of citations and references, what stands out is a paucity of mainstream economists explaining their approach.[20] Dogs that fail to bark should always capture the attention of any thoughtful reader.[21] There are many introspective economists who would qualify for membership of this group, all of whom have described the boundaries, faults and implicit back story of their particular paradigm. Though Richard Bronk scrupulously attempts to deal a fair hand, his need to conceptualize and make operational his Romantic vision inevitably tips the scales, no matter how frequently his disclaimers appear. Consequently, proponents of mainstream economics remain wraith-like and bloodless, reduced to being a poor relation to applied mathematicians. It seems hardly likely that an author as erudite as Bronk could remain unacquainted with such work given the breadth of his excavations into many of the byways of economic methodology. Therefore the non-appearance of this wide-ranging literature and the over-simplistic presentation of core aspects of mainstream economics remains a mystery. For brevity, I'll focus on just a few of these issues, but they are striking since to some degree they cast an unwanted shadow over the good intentions of the author. The shortcomings of the current paradigm are repeatedly trotted out without allowing the opposition to mount an adequate defense.

Laying the foundations for a new approach does involve more than a bit of marketing. But in pointing out the problems of the reigning method, due care must be taken to present it in its fullest and most sympathetic evaluation. Erecting straw men and igniting them may be rhetorically effective but ultimately leaves one open to the same sort of tactics. It is true, then, that the rational choice model represents a core assumption of standard economic theory, but it is crucial to understand the basis for its

[20] Bronk is much more conscientious when marshalling critical voices that point out limitations and inconsistencies in modern mainstream economics, leaning on Colander (2001), Dasgupta (2002), Maki (2002), Mirowski (1989) and Ormorod (1994), among others, to support his position.

[21] More exactly, though the dog does not clearly bark, close attention to the matter will yield a few stifled whimpers, which are as much of interest for the fact that they are stifled as for being made at all.

"Is there any point to which you would wish to draw my attention?"

"To the curious incident of the dog in the night-time."

"The dog did nothing in the night-time."

"That was the curious incident," remarked Sherlock Holmes. (Doyle 1961: 347) widespread use and the objectives it is able to accomplish. Bronk seems at times to forget the purpose of making critical assumptions. An older and in some ways more accurate characterization of microeconomics is "price theory." This is certainly how it was understood and taught at Chicago for decades. Models of rational choice provide a reasonable basis for understanding how prices coordinate market activity. Markets themselves are simply one among a number of institutions that facilitate exchange and under certain conditions accomplish this objective efficiently.[22] Rational choice theory provides a starting point for understanding how individual choices responding to price signals can achieve a coherent and coordinated economic outcome. Prices, and more particularly changes in price, do not explain everything an observer might notice about the exchange process, but most economists think that this approach can shed much light on economic coordination.

If, then, economic outcomes reflect the choices individuals make when responding to price signals, how those choices are made becomes crucial. The objective is to develop and investigate a choice model that would facilitate the optimal operation of a price-coordinated market. Constructing the rational economic man, a human calculating machine motivated solely by narrow self-interest, is a potentially useful behavioral model, since it would seem to provide a predictable and usually reliable basis for choice. There are certainly other bases for choosing. Mill of course insisted on the importance of custom. People certainly act conventionally, seemingly without thought or at other times dominated by the throes of irrational passion. Nor are individuals necessarily good judges of their own self-interest. Mistakes and regrets are all too common in everyday life. Yet, building a system of individual choice resting on some alternative assumption has proven elusive.[23] Conclusions based on the limiting assumptions of rational choice can and should always be

[22] Without delving into formal definitions, what is intuitively meant by economic efficiency (the back story) is that resources are used to produce the greatest possible output and that the output produced best meets the demands of consumers. Note that the nature of these demands or their formation is not questioned.

[23] The emphasis here is on systems based on individual choice rather than Marxian alternatives reliant on class struggle. For Marx, the individual is not the building block of analysis.

modified in specific cases, but self-interested choice should certainly not be dismissed automatically and Bronk does manage to avoid that trap.

However, in pointing out the limitations, at least the impression is sometimes conveyed that he is taking the theory too literally.[24] Rational economic action is built upon three core assumptions: actions have a purpose, the consequences of actions are known, and individuals choose actions which leave them better off. If in fact these requirements reflected any strict interpretation of reality, individuals would consistently maximize utility (be as well off as possible) and choice itself would be something of an illusion. People would be free to choose, but would have only one feasible choice.

The mistake comes in confusing this potentially useful heuristic model with an exact portrait of human behavior. The intuition supporting this methodological avenue is that people really do want to improve their situation. Even if mistaken, the belief remains that the choice taken is the alternative that will leave them better off. This means that decisions made will yield a desired flow of services attached to some specific user costs. To make this estimation we do, as Shackle (1979) insists, use our imagination. We imagine those future flows of services. In some cases, our imagination is based on the solid ground of past experience. We can think productively about trading one flow of services for another in a pretty exact manner. Ranking alternatives under such circumstances is not too perplexing a task. At other times, we face critical problems in imagining those future flows. This causes us much hesitation in committing to a choice, since we tend to shift from one imagined version of future flows to another. Blurred trade-offs inevitably translate into difficulties when constructing those preference rankings. Yet undeniably, choices do get made. These intrinsic problems invalidate the model only to the extent that there is an alternative starting point which can yield more desirable results. A preferred approach would then have to be superior to one that

[24] Literal readings often serve only to impede communication and understanding. Strangely enough Biblical fundamentalism, based on the literal and inerrant word of God, which needs to be believed in its smallest detail, grew its deepest roots during the last half of the nineteenth century, part of the very same Romantic Period admired by Bronk. Such readings seem more fascinated by details than the core intuition conveyed. In this case, examining rational choice theory too literally distracts rather than illuminates.

started with rational choice theory, but adjusted any such conclusions to the specific problem analysed. Certainly a more generalizable model would be welcome, but only if it could be made operational with any degree of ease.

> Where incommensurable values are involved (for example, autonomy, health and wealth) there is by definition no one uniquely rational trade-off ratio between them; and, in these circumstances, it seems off to privilege consistency and transitivity of preferences (particularly over time) as a hallmark of rationality. Likewise, where uncertainty, novelty and creative choice abound, it is again unclear why we should privilege the ability to rank preferences in order as central to rationality. (Bronk 2009: 238)

Certainly preferences do change over time and there is a process through which such desired hierarchies are formed. But when we actually make a choice our preferences are given. It would seem that the importance of how such preferences evolve over time depends on what is being examined and what difference such a process makes. In other words, how much does a deviation from rational choice affect the ability of prices to coordinate economic activity? Models are tools to achieve specific objectives. The issue is not whether rational choice theory achieves many things, but to what degree it accomplishes the objective for which it has been constructed. To answer a number of the questions posed by price theory, there is simply no need to delve into the formation of preferences or to deal with many of the issues posed by Bronk, no matter how intrinsically interesting they may be. It is then up to Bronk to demonstrate what exactly a better grasp of psychology or sociology would bring, not at an applied level but rather at some theoretical starting point. Time and place are always vital when dealing with specific problems, but this may be more an issue of application than of theoretical approaches.

> It is just as foolish to fancy that any philosophy can transcend its present world, as that an individual could leap out of his time or jump over Rhodes. If a theory transgresses its time, and builds up a world as it ought to be, it has an existence merely in the unstable element of opinion, which gives room to every wandering fancy. (Hegel 2001:19)

Bronk, then, correctly points out the focus that rational choice models place on independent decision making as their theoretical pivot. Decisions are seemingly made on the equivalent of isolated squash courts where individuals are fed an endless series of prices. Largely ignored is the sense in which actual decisions are made in nation-specific communities where preferences and choices are intrinsically interdependent.[25] The issue which seems momentarily to get lost in his extended discussion is whether such deviations from independent choice radically change insights into price coordination. The reader is left to puzzle out why this reality is better encompassed by a reconstituted theory rather than by tinkering at the application level.

Strangely, Bronk fails to use the best fictional example of the rational economic man, one devised specifically during the Romantic period as an indictment of economic analysis and nineteenth-century economists. Ebenezer Scrooge is a human calculator, consistently focused on costs and his own narrow self-interest. He is without any seeming imagination or humor, lacking also any viable connection to others. Scrooge remains something of the apotheosis of the independent decision maker, studying trade-offs and computing best outcomes. He is an excellent man of business. One who keeps to the terms of all contracts to which he submits, expecting others to do the same. Cold-blooded and without passion, he is no one's friend yet in terms of market activities he is a good citizen

[25] At least national differences seem more a question of application rather than necessitating alternative theoretical structures. Certainly development, history and customs mean that incentives operate in different ways depending on time and location. But basic trade-offs and sacrifices that advance individual well-being seem operable in diverse countries. The poor of Kenya have been observed to skip meals to pay for mobile phone service which in the longer run allows them to improve their position. Hence Bronk's extended discussion on varieties of capitalism seems once more to fail to recognize the difference between theory and application. In this particular case, Bronk doesn't consider an alternative analysis that recognizes the ways in which different countries may utilize an alternative combination of governance structures, which rely more heavily on implicit rather than explicit contracts. It is not clear that the alternative examples can't be comprehended by the core principles of contract theory.

> Different institutional frameworks allow for different forms and degrees of coordination. In particular, the German national system of capitalism (and its close relatives in Sweden and Holland) provides firms with *non-market* institutional resources for coordination (such as business associations and works councils); whereas the US and UK systems force firms to rely much more on *market* coordination. (through competition, price signals and legal contracts) (Bronk 2009: 159)

steadily advancing economic activity. A world full of such solitary, unfeeling individuals would yield a bleak, but highly civil, society.[26]

> And Scrooge's name was good upon 'change for anything he chose to put his hand to. (Dickens 1875:1)
>
> Oh! But he was a tight-fisted hand at the grindstone, Scrooge! A squeezing, wrenching, grasping, scaping, clutching, covetous, old sinner! Hard and sharp as flint, from which no steel had ever struck out generous fire; secret and self-contained, and solitary as an oyster. The cold within him froze his old features, nipped his pointed nose, shrivelled his cheek, stiffened his gait, made his eyes red, his thin lips blue! And spoke out shrewdly in his grating voice. (Dickens 1875:2)

Bronk then goes a step further by suggesting that models of independent rational calculators, as well as those stressing interdependent embedded members of complex societies (the economic versus sociological approach), are both equally and unnecessarily limited.[27] Bronk further rejects the possibility that either one, at least intuitively, can effectively comprehend the other. He instead offers an alternative vision that moves beyond these carefully constricted models.

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[26] Literature of the Romantic Period, which Bronk chooses to ignore, is literally chock-a-block with narrowly self-interested characters. Some of these are not quite as dry and unemotional as Scrooge, but perhaps even more memorable than the more self-sacrificing heroes occupying other works. Thackeray's (1847–1848) Becky Sharp ("Sharp by name, Sharp by nature") and Balzac's (1846) Cousin Bette (*enrichissez-vous*) are just two of many figures that fascinate and even enrage the reader. Even old Scrooge is more interesting before his redemption.

[27] These two modes of being would seem to incorporate the two worlds in which decision makers simultaneously operate.

> Nineteenth-century German sociology, drawing on Marx and Hegel and Sir Henry Maine, established a distinction between two ideal types of human society, the *Gemeinschaft* and the *Gesellschaft*. No pair of English nouns adequately conveys the intended contrast, but we may translate *Gemeinschaft* as "community" and *Gesellschaft* as "association." They are distinguished by the different relations between people characteristic of each. In an association men connect with one another only when each expects private advantage from the connection. Links between people are impersonal and contractual. The perfect capitalist market embodies this idea. … A man is merely a means to another man under capitalism, but in the *Gemeinschaft* (and, to all appearances, under feudalism) his position is respected by his fellows. It restricts the uses they are prepared to make of him, and the uses he is prepared to make of them. (Cohen 1978:331–332)

> In contrast to the relative certainties and predictability of the world according to *homo sociologicus* (socially structured and constrained) and *homo economicus* (tending to the rational optimisation of given preferences and factors), the world inhabited by *homo romanticus* is apt to be confusing and uncertain: the social constitution of our preferences, and of the roles we play and constraints we face, is only partial and always in flux; there is no way of predicting the future yet to be created by the free and creative choices we make; there is no single rational answer as to what trade-offs we should make between our own conflicting and incommensurable values; and there is no guarantee that we (or other agents) are motivated by rational calculations (even where these are possible) rather than by sentiments and emotions. It is in this very indeterminate state of uncertainty, however, that the seeds of human freedom and greatness lie. (Bronk 2009: 252)

But very few economic agents spend much time playing at being Byronic heroes. Elevating entrepreneurs to these heights may be stretching the metaphor further than it can reach. Some of the basic flaws in his analysis appear here. Clearly economic agents can't literally optimize, but only try their best to improve their lots. Often choices are not carefully calculated. But individuals seldom make such dramatic decisions. Most are trivial and many occur automatically. Bronk also fails to consider whether the difficulty posed by uncertainty and the use of imagination is best dealt with at the theoretical level or when focusing on the specifics of application. If it is to be a theoretical construct, the problem of how to make such vaguely specified characterizations into an operable model is left as unexplored and unincorporated territory. Here one would assume that Bronk would consider those mainstream theorists who have at least recognized and to some degree attempted to respond to these difficulties. Becker (1962) points out that the objective of analysis is to explain economics at the market level rather than predict the actions of each individual agent. What we observe at a market level, though consistent with individual agents acting rationally, does not really necessitate such behavior in each and every single operative. Individual consumers may act irrationally, yet the observed market demand curve would still indicate an underlying rationality.[28]

[28] Statistically we would be painfully naïve and uninformed to muddle the average age of a population with that of each member of the community. The more interesting question is whether different distributions with the same average represent different potential effects.

> Little attention has been paid to the distinction between group or market and individual responses. The distinction is unnecessary in traditional theories of rational behaviour because a market's response is usually simply the macro-version of an individual's response. A group of irrational units would, however, respond more smoothly and rationally than a single unit would and undue concentration at the individual level can easily lead to an overestimate of the degree of irrationality at the market level. (Becker 1962: 13)

Theories of the firm run into problems similar to the consumption side of the market when filtered through Bronk's pre-set lenses. This result can be easily anticipated and soon verified by the reader, since mainstream economics presents one model as the near mirror image of the other.[29] In each case, the resulting construct represents the underlying assumption required for optimal operation of a market economy. The sensitivity of that outcome to variances in these maximization assumptions is a key issue with which Bronk never satisfactorily engages. But just as every consumer is not presumed to actually maximize his or her utility in each and every choice, firms as consistent profit maximizers exist more as a useful shorthand than anything else. The intuition or implicit back story imagines firms as essentially profit seekers. Those that consistently gain more profits than competitors are those that succeed over time. Alchian (1950) explores this simple Darwinian type of story. He tries, like Becker (1962), to create a more intuitive context within which profit-maximizing firms can gain greater credibility. Alchian (1950) tries to create this unconventional space by incorporating uncertainty within an evolutionary context. Placed in the proper light, even bland neoclassical offerings can appeal to the Romantic imagination.

> The existence of uncertainty and incomplete information is the foundation of the suggested type of analysis; the importance of the concept of a class

[29] Simply understood, firms, like consumers, are one-dimensional, input/output constructs. Consumers and firms transform flows of services into optimal levels of either utility or profit. This degree of abstraction serves to foster a greater theoretical generalization.

> In order to study first the most essential features of exchange relations, it will be necessary to simplify the situation as far as possible by a process of "heroic" abstraction. (Knight 1971:76)

> of "chance" decisions rests upon it; it permits of various conflicting objectives; it motivates and rationalizes a type of adaptive imitative behaviour: yet it does not destroy the basis of prediction, explanation, or diagnosis. It does not base its aggregate description on individual optimal action; yet it is capable of incorporating such activity where justified. (Alchian 1950: 221)

As Bronk undoubtedly comprehends, simplistic approaches to the theory of the firm have often faced criticism from both within and without.[30] Certainly Clapham (1953) focused on the problems of abstraction and economies of scale, an intractable issue which Sraffa (1953) further analysed. But again, this depends on the objectives of the model and how such models are applied. Decreasing returns to scale (and diminishing returns to a fixed factor) play pivotal roles in standard economic theory. Yet part of the unresolved debate depends, for instance, on the frequency with which increasing economies of scale occur and whether theoretical inconsistencies automatically eliminate economic models. Certainly Edith Penrose (1959) forcefully made the case that managerial coordination intransigencies limited the size of firms in terms of cost efficiency. More recent empirical work seems to argue that in certain sectors economies of scale have been achieved.[31] Firms continue to merge and grow in size despite the fact that many mergers destroy shareholder value. Clearly other reasons (empire building, reduced competition or reduced risk)

[30] Bronk focuses on Marshall who was cautious of using mechanical models as more than heuristic devices.

> Marshall understood well enough that eventually, of course, even the giants of the forest "lose vitality": competition is a continual organic process of growth and decay marked by vastly different and unpredictable rates of success. (Bronk 2009: 123)

[31] The u-shaped cost curve is easily ridiculed, but again this depends largely on looking at the back story, requiring a willingness to look beyond the textbook grasp of the issue. Such curves might be better conceptualized as having a flat (constant returns to scale) bottom, but entailing costs that eventually do increase, especially as the relevant market expands.

> If average costs fall as a firm of a given size grows bigger, this suggests economies of scale exist for firms of that size. Results vary by industry. American dairy farms, for example, have been getting bigger but a recent paper shows there are still economies of scale to exploit, especially at those many farms with fewer than 200 cattle. By contrast, rail-industry studies show dwindling economies of scale over time as companies have grown. Overall, estimated cost functions suggest the limits of scale may have been reached for some very large firms. (Free exchange 2012:72).

rather than cost concerns are driving such strategies. But such issues point more toward the specific adjustments required when applying theoretical conclusions rather than to deficiencies in the models themselves. Better models might lead to better applications, but Bronk allows this problem to largely slide beyond the acknowledgement of his radar.

Even if we are willing to nonchalantly avoid Machlup's (1967)[32] interesting survey and defense of marginal models, following decades of postwar debate, there seems less reason for largely overlooking Frank Knight's contribution. His ability to identify and categorize issues, allows an insightful examination of the nature and usefulness of such maximizing theories embedded in perfect competition, while also focusing on the key problems raised by uncertainty.[33] The point is not that a new paradigm or approach to economics wouldn't be welcome, but that this requires a real intuitive grasp of prior debates and being scrupulous not to cherry-pick the terms defining the relevant issues.

> In many connections the uncertainty which is associated with change, is more important for economic theory than the change itself. (Knight 1969:76)

[32] Curiously, Machlup arrives at a conclusion that to some degree lies within Bronk's comfort zone, even though it is devoid of Romantic figures or Romantic imagination. Like Bronk, Machlup allows the neoclassical model free rein within certain defined perimeters. Issues then arise in determining appropriate boundary conditions.

> I conclude that the choice of the theory has to depend on the problem we have to solve. Three conditions seem to be decisive in assigning the type of approach to the type of problem. The simple marginal formula based on profit maximization is suitable where (1) *large groups* of firms are involved and nothing has to be predicted about particular firms, (2) the effects of a *specified change* in conditions upon prices, inputs, and outputs are to be explained or predicted rather than the values of these magnitudes before or after the change and nothing has to be said about the "total situation" or general developments, and (3) only *qualitative answers*, that is, answers about directions of change, are sought rather than precise numerical results. (Machlup 1967: 33)

[33] Though model building must involve simplifying and limiting assumptions, to be pleased with too little is a misleading enticement. Taken to its logical conclusion, a frictionless economic world is highly contradictory. Thus Knight is less concerned with the abstract surface of a theory than the intuitive meat that can render it potentially useful.

> The difficulty is, of course, avoided if "friction" be so broadly defined that "perfect mobility" means the absence of all resistance to the human will. But in a world where a breath could transform a brick factory building into a railway yard of an ocean greyhound, there would be no need for economic activity or economic science. (Knight 1971:34n)

Failing to fully grasp the underlying intuition of some well-known economic models leads Bronk to unintentionally misuse such work to promote his own agenda. This foible seems more a case of clumsiness than an intentional distortion. Yet such cases often deteriorate into missed opportunities. Some of his targeted economists come closer to practicing the sort of imaginative approach to which Bronk subscribes than seemingly he realizes.[34] Coase (1937) attempted to use marginalism to explain specific problems, including the existence of firms. But taking his work in its broader scope, Coase (1994b) insisted on examining institutional detail and delving into specifics when fruitful. He in fact derided the idea of "blackboard economics" which conveniently confused abstract theory with its applied counterpart.[35] Bronk's depiction exiles Coase to the narrow confines of his early 1937 paper. Coase never abandoned the core principle represented by transaction costs, but he hardly spent the rest of his career hemmed in by those boundaries.

> Coase's theoretical model suggests, for example, that the firm will do those things (and only those things) that can be done more efficiently within the employer–employee relationship than by trading at arm's length through the market. The insights generated by such a classical approach are real enough, but they leave out much that is special about the firm. (Bronk 2009:143)

However, the multitude of issues arising from standard conceptions of consumption and production gain their significance only in light of Bronk's primary target, the mechanical equilibrium model that has

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[34] The temptation to turn well-known economists and their work into convenient, but distorted, replicas can be overwhelming. Strategic use of such tactics can eventually undercut the practitioner's own credibility.

[35] Coase's intentions are hard to miss given that his Nobel Prize speech provided him with the opportunity to underline what he viewed as the core of economics.

> What is studied is a system which lives in the minds of economists but not on earth. I have called the result "blackboard economics." The firm and the market appear by name but they lack any substance. The firm in mainstream economic theory has often been described as a "black box." And so it is. This is very extraordinary given that most resources in a modern economic system are employed within firms, with how these resources are used dependent on administrative decisions and not directly on the operations of a market. (Coase 1994b: 5–6)

long dominated economic thinking. Bronk's contention is that such a simplistic metaphor has severely constricted the discipline's grasp. This consequence underlies Bronk's decision to champion Romanticism. The roots of this antagonism to mechanical visions can be traced back to the eighteenth-century Age of Enlightenment. Economies were presumed to be ordered by laws quite similar to those operating in the physical world, both being a reflection of God's wisdom.

> God, the clockwinder, had not only ordained the laws of the universe, he had created all its structures as well: one creation followed by the complete evolution of all preordained structures to the apparent end of time. (Gould 1977:22)

There is much to criticize here, though some of the appeal of equilibrium analysis has been its tractability and the absence of comparable tools for alternative methods. Computer simulations have perhaps made this less of a problem more recently.[36] Still, Bronk doesn't quite seem to have a full grasp of this multifaceted issue. The starting point of such analysis is that for a given set of market conditions, there exists a price that would clear that specific market (everyone willing to buy and sell at that price could do so). This is merely an existence proof. Extending that logic, if one of the underlying factors determining market conditions changed, some new price would necessarily equate supply with demand once again. Stripped to its barest level that is all the theory yields. The big step is to equate a perceived price change with a shift from one equilibrium to another. Besides the formal mathematics saturating such models, there is a back story based on economic agents trying to better themselves. At equilibrium, no agent is able to improve his or her position. With changed conditions, those same agents alter their actions to take advantage of, or at least to adjust to, the new prevailing circumstances. This desire for improvement leads to all potential opportunities being exploited until once again a new equilibrium is reached. This clearly is a story that

[36] It can be argued that certainly in the past, most economists had been trained to operate in an equilibrium framework and would find it difficult, if not impossible, to discard such a useful tool.

does not have to be accepted. Moving toward a new equilibrium position may in fact result in that predicted destination shifting, an action that creates an ever-moving target. It might be more useful simply to talk about the path price takes.[37] And since change is constant, the very idea behind equilibrium approaches can be, at best, no more than a useful heuristic. Much of the confusion is generated by the way such equilibrium and disequilibrium or statics and dynamics have been employed by the profession.[38] Bronk's discussion might have been more to the point had he at least acknowledged some of the vast literature on the subject, especially that written in defense of this mechanical approach.

> The idea of equilibrium is employed in this scheme as a mental tool, a methodological device; it aids in establishing to our satisfaction a causal nexus between different events or changes. Events or changes can be imagined as well as observed; a causal connection between them can only be imagined, and the idea of equilibrium has the function, as we shall presently see, of making this connection plausible. (Machlup 1958:3)

Often Bronk seems to simply lose his focus. Sometimes he is intent on sketching out the weakness of using this limited mechanical (instead of biological) metaphor by honing in on nineteenth-century classical economics,

[37] Schumpeter points out the importance of path dependency in discussing the work of Sismondi.

> Sismondi's great merit is that he used, systematically and explicitly, a schema of periods, that is, that he was the first to practice the particular method of dynamics that is called period analysis. Moreover, he saw clearly the difference this makes and in particular the disturbances, discrepancies, and hitches that result from the fact that economic life is bound to sequences of which every unit is determined by the past and in turn determines the future. (Schumpeter quoted in Machlup 1959: 101)

[38] A perennial confusion, one which Bronk often either ignores or muddles, is the use of heuristic models versus the way in which such models are applied.

> Frequently equilibrium will refer to a set of variables that fails to include some which are both relevant and significant for practical problems; such an equilibrium is sometimes denounced as a worthless or dangerous tool of analysis. To be sure, if an economist were to base policy recommendations on an analysis confined to such oversimplified equilibrium models he would be guilty of a gross lack of judgment. Yet such models may have considerable heuristic value. This value is not impaired if an important factor is left out, provided the omission is not inadvertent. Indeed, the importance of any factor can be demonstrated only by leaving it out of account and then showing the difference it makes when it is reinstated as one of the variables in the equilibrium system (Machlup 1958:7).

while at other points we seem to find ourselves suddenly immersed in the formalism that increasingly defined economics in the post-war period. But in targeting the dominant paradigm of modern economics, he doesn't consistently avoid setting up figures which, while not entirely untrue, seem at times to be no more than textbook cut-outs. For instance, Bronk seems to identify economic thinking about statics and dynamics with the more narrow scope favored by someone like George Stigler.

> Statics: Theory "which explains the equilibrium position in the particular problem on the assumption that the data of that problem do not change." Dynamics: Study "of the path by which a set of economic quantities … reach equilibrium within a static framework." (Stigler quoted in Machlup 1959:99)

Contrast this with the ideas on the same subject formulated by George Stigler's teacher, Frank Knight.

> Statics: "Reasoning" about "economic change" in the only possible method: varying one thing while holding others constant/ Dynamics: Cannot exist as "economic science" … though perhaps as "evolutionary or historical economics." (Knight quoted in Machlup 1959: 95)

The difficulty of using such a mechanical model has long been recognized, but for want of a better alternative it has become something of a default choice. For some economists faced with these limitations the challenge became one of exploring how elastic these seeming constraints might be. Could equilibrium analysis be used in a somewhat more poetic fashion? Keynes certainly stretched these limits in his *General Theory,* where the idea of equilibrium is often used loosely at best. Such values form boundaries rather than precise destinations. Keynes simply followed and extended the methods used by his teacher, Alfred Marshall, who avoided anything definitive when at all possible.

> Marshall argued that "mechanical analogies" have a large place in economics textbooks only because they are easier to handle mathematically and conceptually than biological or organic analogies. He thought that the term "equilibrium" "suggests something of statical analogy," and that the

> "fragmentary statical hypotheses" used in equilibrium analysis should be seen as no more than "temporary auxiliaries to the dynamical—or rather biological—conceptions." (Bronk 2009: 70)

In comparing mechanical (equilibrium) versus romantic metaphors, Bronk unfortunately also manages to fumble an exemplary opportunity to examine the deleterious role teleology plays in mainstream economic analysis. Perhaps this is due to the fact that teleology plays a crucial role in the prevailing mechanical, as well as in Bronk's alternative metaphor. Whether it is nature performing as a reflection of God or God himself, Romantic thought is just as imbued with the idea of purpose as is the Enlightenment's clockwork universe run according to immutable laws of motion. For classical economists such as Malthus, Ricardo, James Mill, McCulloch or Senior this idealization unarguably meant progression to the stationary state. Romantic thinkers leaned heavily toward a more optimistic destination. But for both these groups, motion had an intrinsic meaning, making it ultimately comprehensible.

> … ambitiously attempting to analyse the growth and development of entire economies over relatively long periods of time—decades or even centuries … The basic theme … was the development of the economy from a progressive state into a stationary state … with no net investment, subsistence wages, and low or even zero profits. (Baumol quoted in Machlup 1958: 100)

Ditching the Romantic for a simple evolutionary (biological) metaphor would promote the suggestion that economists should instead focus on the paths that markets and economies take instead of implicitly accepting a determinate destination. Evolution in its strictest sense is intent on explaining the mechanism of development without regard to a conceivable end point or implied purpose. The mechanism is amenable to theological concerns without being dependent on them. Thus animals don't possess eyes in order to see but rather have sight via the development of eyes.

But the validity of Bronk's proposals ultimately rests not so much on his analysis of the present status quo but rather on what his alternative offers. We need to wait for 288 pages of detailed discussion and often interesting excavations of the past before the preliminaries are in some sense complete. Only then have we been properly prepared and readied to comprehend how the Romantic Economist would operate both in theory and in practise. The remaining issue lies in whether the patient reader is provided with a basis for comparing the two paradigms.

Paradigms Lost: Paths Versus Outcomes

> "No," replied Elinor; "her opinions are all romantic. … A few years, however, will settle her opinions on the reasonable basis of common sense and observation; and then they may be more easy to define and to justify than they now are, by anybody but herself."
>
> "This will probably be the case," he replied; "and yet there is something so amiable in the prejudices of a young mind, that one is sorry to see them give way to the reception of more general opinions."
>
> "I cannot agree with you there," said Elinor. "There are inconveniences attending such feelings as Marianne's, which all her charms of enthusiasm and ignorance of the world cannot atone for. Her systems have all the unfortunate tendency of setting propriety at nought; and a better acquaintance with the world is what I look forward to as her greatest possible advantage." (Austen 1933b:33)

Historically, the Enlightenment arose in part in the hope that rationality would curb the excess of the passions, a notion that harks back to the analogy made between the self-controlled individual and the efficiently functioning State in Plato's *Republic* (1968). Romanticism can be seen as something of a reaction to an excess of rationality, a counterbalance.[39] But Romanticism itself was distinctly capable of veering awkwardly into sentimentality or self-indulgence. When Bronk finally reveals his hand in his concluding chapter, does he in fact strike an appropriate balance?

[39] Adam Smith, a product of the Scottish Enlightenment, never promulgated the vision enshrining the rational economic man on the analytic throne of the discipline. Thus *The Moral Sentiments* (1976) is the natural complement to *The Wealth of Nations* (1976). Without societal values and the need to be regarded by others, market economies based solely on narrow self-interest would hardly triumph. For Smith, a certain sense of responsibility and duty oiled the wheels of commerce and economic life. "We resolve never to be guilty of the like, nor ever, upon any account to render ourselves in this manner the objects of universal disapprobation" (Smith 1976b:224). Smith was not imagining a world of Ebenezer Scrooges, one where narrow calculation would render even biological reproduction uncertain.

Here perhaps is the most disappointing part of Bronk's project. Sympathy with his goals and objectives doesn't automatically transfer into enthusiasm for paths or means. The Romantic metaphor and the packaging in which it is marketed tend to be off-putting, translating into an unfortunate resistance to a proposed paradigm which seems to lack practical application. While subscribing to the idea that any review must judge the work on the goals it sets, on the author's own terms the book fails to effectively accomplish its self-imposed objectives. Since the author's core criticism is that the economics profession leans too heavily on one metaphor, it is then quite legitimate to question the alternative metaphor the author evokes to supplement the existing one.

> It is for these reasons that the Romantic Economist advocates a twin-track approach: first, complementing standard economics with models that work better when dealing with certain types of problem; and secondly, bolstering the effectiveness of both standard and more Romantic models by defining carefully the limits of their applicability. It is only by defining the extent of their remit that we can get the most out of the systematic models or fragments at our disposal. (Bronk 2009: 296)

This quandary isn't resolved, or desired analysis facilitated, by insisting upon the need to invest in holistic theories. Essentially Bronk fails to develop the nitty-gritty rationale for and necessary steps in choosing different models. (It doesn't help to say that an economist should choose the best or most appropriate one.) Should one then manage to propose some reasonable basis, the quest then takes a step back and the search for a reasonable grounding for deciding those criteria, and then the basis for choosing that basis, continues until we are looking at turtles all the way down.[40] After considering Bronk's detailed and scholarly argument,

[40] Stephen Hawking begins *A Brief History Of Time* with an anecdote.

> A well-known scientist (some say it was Bertrand Russell) once gave a public lecture on astronomy. He described how the earth orbits around the sun and how the sun, in turn, orbits around the centre of a vast collection of stars called our galaxy. At the end of the lecture, a little old lady at the back of the room got up and said: "What you have told us is rubbish. The world is really a flat plate supported on the back of a giant tortoise." The scientist gave a superior smile before replying, "What is the tortoise standing on?" "You're very clever, young man, very clever," said the old lady. "But it's turtles all the way down." http://www.the-funneled-web.com/Hawking.htm

I yearn for some practical enlightenment. My inability to see how such an approach could be put into practise leaves me (and perhaps other readers) badly frustrated. I would not descend to Dr. Johnson's sardonic dismissal of uncongenial work, because Richard Bronk's efforts are intrinsically valuable.[41] However, *The Romantic Economist* cries out for something like a case study to clarify what should be done and how this approach differs and delivers something different from standard economic analysis. Granted, seizing a convincing method for verifying or dismissing contending results is always difficult. Bronk reasonably points out the problems facing statistical analysis as a test of applicability or usefulness but fails to articulate a more obvious alternative.[42]

Even when Bronk is clearly correct, his arguments fail to either fully convince or convert. Entrepreneurs, Schumpeter's loci of creative destruction, clearly fall outside even the most elastic limits of a mechanically based model. Casson's (1982: 337) diagrammatic attempt to trap this concept within the boundaries of a simple supply and demand equation becomes an excellent demonstration of the limitations of applying such

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[41] The statement often attributed to Dr. Johnson in his role as editor is, "Your manuscript is both good and original. But the part that is good is not original, and the part that is original is not good." Unfortunately, this piquant quote remains one of a number of things Dr. Johnson should have said, but seemingly failed to say.

[42] It is true that economists tend to discover the statistical support for a theory that they expected to find. To a certain extent this has reduced statistical work to yet another form of rhetorical marketing, perhaps not even the most convincing alternative.

> I remarked earlier on the tendency of economists to get the result their theory tells them to expect. In a talk I gave at the University of Virginia in the early 1960s … I said that if you torture the data enough, nature will always confess, a saying which, in a somewhat altered form, has taken its place in the statistical literature. Kuhn puts the point more elegantly and makes the process sound like a deduction: "Nature undoubtedly responds to the theoretical predispositions with which she is approached by the measuring scientist." (Coase 1994b:24)

However, the alternative to statistical analysis is far from apparent while correspondingly, methods and techniques have improved. In the 2012 US presidential election, statistical analysis proved a far more accurate and precise predictive measure than the more romantic intuition and gut feelings of noted pundits.

> When it comes to assessing the chances of some complicated combination of events, gut feelings are pretty much useless. Pundits are no better at forecasting election outcomes than they would be at predicting the final path of a hurricane. Smart pundits should consider either abandoning this activity, or consulting with the geeks before rendering their guess. (Thaler 2012:2)

an approach. Certainly entrepreneurs have been noted for their power to harness their imagination and to recognize opportunities in subsequently productive ways. Yet, Bronk fails to provide any real hints, let alone useful instructions, for modelling this economic contribution using his Romantic framework. Again, as in the case of rational decision makers, the standard aim is not to model how each individual entrepreneur may act but rather the collective impact of their action in a market system. Assuming that it is in the self-interest of individuals to take advantage of perceived opportunities is at least a start to addressing that issue, despite its obvious limitations. Entrepreneurs aren't magicians able to create opportunities out of thin air. They recognize possibilities that others leave untouched, but without the right conditions even the brightest of potential entrepreneurs are scotched.[43] Some economic historians have insisted that only with the establishment of patents (restructuring and creating new property rights) in England, acting as something of an imposed *deus ex machina*, could individual initiative succeed in spurring the subsequent industrial revolution (North 1981; Mokyr 1999).[44] A possible problem of Romanticism is that it embraces a heroic analysis of economic history which is just as flawed as Marx's historical materialism or Tolstoy's historical tides. Bronk is right to insist that such models tell only part of the story. Pointing out that the search for a general, one-size-fits-all approach may be harnessing research to the wrong cart does serve a useful purpose. So he is perfectly right in suggesting that the type of model employed should depend on our analytical objective. However, if his new paradigm is to prove useful, readers need to see it in operation. Otherwise even those most sympathetic to Bronk's aims are left wondering whether the user manual has been misplaced.

[43] Even the great Ghenghis Khan might have never left Mongolia had not a particularly wet and warm period created grazing that produced the horses powering his Golden Horde (*The Economist* 2012:74).

[44] More recent research has raised questions about the value of these same patents. (See Boldrin and Levine (2013) and Moser (2013) for the case against patents.) The point here is that such issues must always be questions, resolved at least to some degree by examining the historical record as carefully and objectively as possible. We should not be gulled by the bedtime stories which soothe us into a dogmatic sleep.

These flaws in Bronk's approach end up, much to my own surprise, leaving me more sympathetic to the broad applicability of rational choice theory, which was hardly the author's intention. When subjected to the extravagant romantic sensibilities of a Sydney Carton, I find myself yearning instead for the hard-bitten rationality of an Ebenezer Scrooge. He lacks the slightest shred of imagination, he's cranky, bad-tempered and no doubt even worse company, but he's dependable, a man of his word and is an essential cog keeping the boring wheels of commerce turning.[45] He merely wants to be let alone to perform his duties and wouldn't think of bothering others with his sensibilities.

Bronk, then, has delivered a rare treat in many ways. It is a volume that will repay careful reading. He raises issues that need to be raised and forces the reader to reconsider their own assumptions and beliefs. However ultimately, the Romantic Economist is not the answer to these problems, or even a vital contribution. The metaphor doesn't seem to repay the time it takes Bronk to develop the approach. If we are to look to the field of literature for assistance, perhaps Jane Austen provides some better rules of thumb for economists than a collection of Romantic poets or thinkers. Austen's heroes are either sensible to begin with or learn the importance of sense to balance a measured share of sensibility. An Austen approach to economics would provide a few guidelines which could only improve the practise of economics no matter what the paradigm employed.

- Curb ideological inclinations by becoming aware of them
- Be open to other approaches to economic modelling
- Judgment is needed when applying theory to a specific case – there is as much art as science in economics.

[45] Despite his failure to provide entertaining company, Scrooge's detachment from the imaginative and emotional sphere is not entirely without some merits. Better a cold fish than a whining romantic.

> Scrooge had as little of what is called fancy about him as any man in the City of London. (Dickens 1875:12)

> "It's not my business," Scrooge returned. "It's enough for a man to understand his own business, and not interfere with other people's. Mine occupies me constantly." (Dickens 1875: 9)

Austen's heroes are ever admirable and ultimately sensible. They find and marry a man they can love and admire as an equal. But though happy, they are also as practicable as any rational choice maker. When asked by her beloved sister to explain when and how she first started to love Mr. Darcy, Elizabeth Bennet responds only partly facetiously:

> "It has been coming on so gradually, that I hardly know when it began. But I believe I must date it from my first seeing his beautiful grounds at Pemberley." (Austen 1933b:456)

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The Chicago School of Anti-Monopolistic Competition: Stigler's Scorched Earth Campaign Against Chamberlin*

He has to a considerable extent gotten away with murder. Because, I'd say, unlike Milton Friedman, you have to be really very aware when you read George Stigler of these preconceptions. You know, a lot of people that read George Stigler are quite surprised when you tell them how, of course, very pro markets he was. OK, they realise that he was at Chicago and that sort of thing. But he is wonderful in disguising himself, with his wonderful, his very funny, ironic, cynical stance. (Conversation with Mark Blaug, April 1998)

* I'd like to thank all those individuals who patiently put up with my questions concerning the life and times of George Stigler. These include Milton Friedman, Aaron Director, Rose Friedman, Harold Demsetz, Arnold Harberger, Armen Alchian, Sam Peltzman, Lester Telser, Ronald Coase, Gary Becker, Sherwin Rosen, Steven Stigler, James Kindahl, Paul Samuelson, Robert Solow, John Kenneth Galbraith and Paul Sweezy. Too many of these voices are no longer with us. Special thanks to Claire Friedland whose wit and good humor continued to be a joy throughout. She is too modest to acknowledge this, but George Stigler was fortunate to have her for his loyal research associate for more than three decades.

C. Freedman, *In Search of the Two-Handed Economist*,
DOI 10.1057/978-1-137-58974-3_5

By Way of a Prologue

George Stigler did not think much of case studies, the 1950s Harvard approach to industrial organization. "Each new PhD gravely decided in some mysterious fashion whether the industry chosen for his doctoral dissertation was or was not acting in a socially desirable way" (Stigler 1988a:162). So perhaps he would have been amused or annoyed that his campaign to destroy the idea of monopolistic competition serves as an illustration of how marketing and ideological demands intertwine to shape methodological exposition. While Milton Friedman and his work might seem to provide a more obvious candidate for such a case study, because he was much more the public face of the Chicago counter-revolution, it is easy to overlook the subtlety of his marketing which, at least for a number of professional readers, served to underplay the ideological component helping to shape and drive his work. If Chicago is to serve as an ideal encapsulation of the interplay of ideology and marketing in economics, then the logical imperative is to focus on someone who distinctly shaped that post-war Chicago School.

> He identified with the University of Chicago. He cared about it, not just about his own progress. If something was good for the Economics Department, he approved. If the Department was in danger of losing somebody whom he felt would be a loss, he cared and he worked on it. He really had this funny identification, you see. What do I care if the Department moves ahead? I only care if Claire Friedland moves ahead. But George really cared. And he cared about the profession in the same way. (Conversation with Claire Friedland, October 1997)

As mentioned in previous chapters, both his allies and antagonists recognized his unique way with an argument, the way he could package his ideas so that they would achieve maximum effect. "That was another distinctive aspect of George. George was a *terrific* writer. He had real *style.*" (Conversation with Sherwin Rosen, October 1997) He was a witty debater and someone who could quickly eviscerate an opponent with a single line, as when he effectively sank Galbraith's argument of countervailing power by titling his critical remarks "An Economist Plays with

Blocs" (Stigler 1954). He simply had the knack of marshalling an array of rhetorical devices and ploys to distract and entice his readers and/or listeners.

> I think he was one of the most difficult people to explain because I mean, there is no one like him. I've described how in an argument he jumps around. He puts in a bit of theory, a bit of statistics, a reference to the earlier economists. It's like no one else's form of argument that you can recall. (Conversation with Ronald Coase, October 1997)

Accordingly, the way George Stigler defended price theory (his first and most intense economic infatuation) against contending frameworks displays his use of marketing as a competitive device. The way he sought to bury Chamberlin and his alternative ideas raises questions that are essential when considering the effect that ideology has on economic thinking. Does it in fact matter that Stigler sought to destroy, rather than attempting to evaluate a theory he found both distasteful and dangerous? Crucial here is his ability to influence both his own department and the profession at large. Truth doesn't shine forth capable of recognition simply by those who squint in that direction. At the heart of any continuing conversation is persuasion. That Stigler harnessed his own abundant powers of persuasion to achieve predetermined ideological objectives is what needs continuing evaluation and judgment. Perhaps those with the greatest powers are those most needful of taking care.[1] "So, he's made a

[1] Comparing George Stigler to a Shakespeare Sonnet (Number 94) may seem like a reach, yet the use and abuse of powers of persuasion does bring to mind the following:

They that have power to hurt and will do none,
That do not do the thing they most do show,
Who, moving others, are themselves as stone,
Unmoved, cold, and to temptation slow:
They rightly do inherit heaven's graces
And husband nature's riches from expense;
They are the lords and owners of their faces,
Others but stewards of their excellence.
The summer's flower is to the summer sweet
Though to itself it only live and die,
But if that flower with base infection meet,
The basest weed outbraves his dignity:
For sweetest things turn sourest by their deeds;
Lilies that fester smell far worse than weeds.

lot of difference, at the university at least, to the economics department. He was a real presence." (Conversation with Ronald Coase, October 1997)

> There aren't too many people who can draw a circle around them, the way he did. I think it's vanished from intellectual life. I think this university had several people like that. In Physics, there was a guy that everyone just came here to be around. I don't get the impression that there are many people like that in economics today. Maybe there never were. [laughs] I think Marshall was, from what I can gather. He had this huge influence on his students. Friedman had a very huge influence on his students, but he was also a very difficult person to deal with, in a much different way than George. He was a much different type of person than George was. I don't know if Milton attracted as many people around him. He attracted his share, but not in the same way as George did. George was just a lot different. (Conversation with Sherwin Rosen, October 1997)

The Chicago School of Anti-Monopolistic Competition[2]

Years later when I was a professor at Columbia University, I attended a meeting of the American Economic Association in Washington D.C. and on the flight back to New York to my surprise I found myself sitting next to Edward

[2] The title derives from the label with which Chamberlin stigmatized his opponents. This was not, although Stigler claims it to be the case, the first written example of the label 'The Chicago School' to denote a particular approach to economics. As Stigler himself explains:

> Edward H. Chamberlin had written a chapter on the Chicago School in his *Toward a More General Theory of Value* in 1957, the earliest such explicit essay I have found. He found the school to be distinguished "by the zeal with which the theory of monopolistic competition has been attacked." And called it the Chicago School of Anti-Monopolistic Competition. What was a minor recreational activity for us was the raison d'être to him! (Stigler 1988:150)

But as Chamberlin points out in that same quoted article, Stigler's erstwhile classmate Martin Bronfenbrenner had used the same terminology earlier. (Ironically the school is characterized by Bronfenbrenner (1950:487) as being concerned with income redistribution via progressive taxation policies. Stigler would soon come to entirely reject this Henry Simons-inspired objective.)

> The so-called Chicago School of economic policy, whose intellectual parent is Frank H. Knight, but whose best-known publicist is Henry C. Simons, author of *Economic policy for a Free Society*, believes these optimum conditions would in fact be realized quickly and painlessly in a free economy despite the complications raised by economic progress

> Chamberlin. He opened the conversation, "You and Professor Knight are the two most mistaken economists I know on the subject of monopolistic competition." Thank heaven it was a short trip. (Stigler 1988: 58n)

I. Stigler's Counter-Reformation

> Bliss was it in that dawn to be alive
> But to be young was very heaven!
> (Wordsworth (1809) "The French Revolution, as it Appeared to Enthusiasts")

For all his lifelong striving after consistency, George Stigler's public and private personae fundamentally clashed. Not a trace of ambiguity existed in the public image he so carefully cultivated over the many decades of his academic life.[3] To his younger colleagues and associates

> and the possible multiplicity or instability of equilibrium conditions, if three conditions were satisfied. (Bronfenbrenner 1950:487)

One wonders whether Stigler is being at least a bit deliberately disingenuous here in imputing an absence of a 'Chicago School' formulation within the profession prior to Chamberlin's own specification. Suspicions regarding his intentions arise given his mastery over and finesse in using a wide range of rhetorical devices. Certainly Bronfenbrenner appears to be using terminology in common coinage at the time that he writes, though notice the classical liberal focus on policy as the definitive identity marker, rather than theory. To claim that Stigler was distinguishing two separate Chicago Schools (pre- and post-war) has limited credibility, since in 1957 such a distinction would have clearly been an anachronism. The post-war school was in its infancy with key players such as George Stigler still missing. Specifying two distinguishable schools at this moment would suffer from the same logical failing as labelling the recent conflict "World War I" in 1925. Moreover, while it is clear that Chamberlin was a classic hedgehog, more than self-obsessed with his one major idea, Stigler's response to the theory should not be reduced to the status of some mild diversion, a simple game of noughts and crosses worth only passing attention. It is after all Chamberlin alone who became the target of a separate lecture during the series presented at the London School of Economics with Joan Robinson reduced to a bit player.

> Of Mrs. Robinson's work I need say little. It is amply clear, on a re-reading at this distant date, that her message was in no sense revolutionary, although at times her language was rebellious. … Her volume marks no break with the tradition of neo-classical economics; indeed it contains, I think, too uncritical an acceptance of the substantive content of orthodoxy. … Professor Chamberlin was a true revolutionary. (Stigler 1949a: 12–13)

[3] He died in harness. The idea of George Stigler imitating his erstwhile teacher, Lloyd Mints, and shifting to a life of cabinet making (a skill Stigler happened to possess) for his last three decades remains inconceivable. To an alarming degree, what George Stigler was, was what he did. (Though he also managed to remain an unswerving family man.) Perhaps this is not remarkable amongst the very best academics. (Samuelson continued to occupy an office at MIT in his nineties.) Stigler could no more operate without the constant intellectual stimulation of ideas, than an opium smoker could remain at ease deprived of his pipe.

alike he managed to maintain the unyielding image of a stern Protestant father. Stigler persistently projected a conservative, unquestioned authority whose definitive views brooked no argument.

> But how much of that was show, and how much was actual belief… There was a lot of show in that. I mean there is no doubt. He had this Protestant father image and he constantly supported that image. One way to do it was just to wave your hands. At times he just waved his hands. (Conversation with Sam Peltzman, October 1997)
>
> Well, he was very intimidating in his critical approach. Your biggest fear was that he would make a joke at your expense. So one was always somewhat on guard. (Conversation with Sherwin Rosen, October 1997)[4]

Shades of grey simply did not exist in his carefully constructed intellectual spectrum. This preferred colour palette was a trait perhaps inherent in his own personality, but certainly nurtured and allowed to flourish at Chicago where the Marquis of Queensberry persistently failed to put in even a token performance.[5] He was a street fighter, living in a world where battles were fought in dire earnest with no quarter provided.[6] Influence

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> George was still a young man intellectually when he died at 81. I think he would have changed his views still further, unlike a lot of people who reach that age. They stop thinking. George always said to me, the reason he didn't retire, and stayed around the University of Chicago was he didn't want to become ossified. He wanted to be exposed to new ideas and to changes. (Conversation with Gary Becker, October 1997)

[4] It is interesting that three of Stigler's colleagues with vaguely similar backgrounds (East Coast/Jewish) all reacted in a similar way to his public persona.

> But his *public* persona was this very strong Protestant father figure and you just don't cross that father figure. (Conversation with Sam Peltzman, October 1997)

Claire Friedland and Sherwin Rosen expressed similar sentiments (conversations, October 1997).

[5] Keep in mind that Stigler's supervisor and idol while a graduate student at Chicago was Frank Knight, certainly no shrinking violet. Knight may provide an extreme example, but Chicago from its inception cultivated a 'no holds barred' academic approach.

> It's a sort of a "Marines" approach to Economics. Stigler was certainly one of the leaders of the Chicago School. I think that's what distinguished the Chicago approach. We take what we do very seriously. And we take it as far as you can. You reinterpret the world. Becker has made a tremendous career for himself out of doing that with great success. That's very much in the spirit of what's been going on here. I think, to some extent, George was a product of that environment when he was a student here. (Conversation with Sherwin Rosen, October 1997)

[6] The Industrial Organization workshop he founded at Chicago could best be described as a bull pit where the weak were customarily gored.

was exerted entirely by pursuing a 'purist' agenda. He moulded himself into the type of economist who was incapable of taking a step backward.

> At the other extreme is the purist, who wishes to implement his economic reforms directly and not later than Friday. His reforms will be stated in stark and preferably outrageous, terms, to dramatize their differences from the present situation. His policies are right, and alternative policies are wrong, as can be shown mathematically. The "practical" difficulties in a radical change of policy are to him a euphemism for cowardice and mental confusion. (Stigler 1963b:23–24)

Though surprisingly, for someone who took his economics so seriously, Stigler never became unduly impressed with himself. A lot of the "brash young man from the provinces" failed to wash away.[7] Nor for that matter, despite all his international experience and subsequent sophistication did the sharpness of his very American viewpoint really fade to any noticeable degree.[8] However, that tough, unrelenting surface may have disguised an inherent shyness, an individual who suffered from being uncomfortable

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> He had this workshop. People had their knives out. I participated in some of them. I think people were using George's example. No prisoners were taken in other words. [laughs] And everybody just jumped in. It was just *chaos* those workshops. (Conversation with Sherwin Rosen, October 1997)

[7] Growing up in Renton, Washington, the only son of immigrant parents, cultivated in Stigler a sense of being an outsider, a perspective that seemed impossible to eradicate despite a career full of accomplishments, awards and recognition. Perhaps that was one of the reasons that the University of Chicago proved capable of providing him with a congenial home from the days of his graduate studies. He seemed uncomfortable elsewhere. His stay at Columbia resembled more of a willed exile than a desired destination. From its inception, the economics department at Chicago strove to develop a critical, non-conformist view that seemed destined to clash with the ideas honored at the more establishment outposts of the East Coast.

[8] It is perhaps a common American fault to view the world through a very provincial lens. Stigler's studies and focus remained ineradicably fixed on US data, problems and issues. In his defense, however, one might add that this is not a startling trait among American economists.

> Another story about George, I've always found it to be a problem, which is how incredibly American he was. I used to be shepherding these Latinos through and here they would come to some question in his Price Theory examination. "Explain something, something about the Dred Scott Decision" … But anyway you might want to just bear in the back of your mind, as you read through George Stigler's work, how much did he cast his eyes beyond the borders? How heavily was economic thinking influenced by the things that happened beyond the borders? My hunch is relatively little. He was very much more American-oriented than the average economist. (Conversation with Arnold Harberger, October 1997)

in unfamiliar environments, keeping people at a distance with a gruff joke at their expense.

> Aaron Director: I think George was shy.
> Rose Friedman: I think basically Aaron's correct though I'm not sure that I would call it "shy." I think George was very sensitive.
> Aaron Director: Oh, I think that. I agree. (Conversation with Rose Friedman and Aaron Director, August 1997)

However, experience should teach us that such two-dimensional figures rarely exist, no matter how carefully or plausibly the façade is constructed and maintained. George Stigler was no exception, but rather an exceedingly complex individual, more of a conundrum and an unsolvable puzzle than anything else.[9] If then we are to understand his work, we need to focus not only on his academic output, but on his motives as well.[10] It is not sufficient merely to set out his accomplishments. Any deeper understanding requires an extensive exploration of his intentions and objectives. Without properly exploring the relevant context of his work, articles are comprehended only at a relatively superficial level. This is especially so in the case of George Stigler. Perhaps a strong hint of this motivation is provided by his continued interest in the economics profes-

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[9] The Japanese make a distinction between *honne* (inside face) and *tatamae* (outside face). In other words, you need to be careful when drawing conclusions based on superficial appearances. There is a tendency to simply see an article as unrelated to an author's other work. In Stigler's case, this would be a major mistake. He had a clear, connected vision and his work tended to fit together. Of course human emotions and desires muddle what anyone produces or writes. And in a certain sense, the side of Stigler that was fiercely private makes many of his objectives less than transparent.

> So, the question really is, how does the one George transfuse into the other, or how do these two Georges live side-by-side in the same head. That is a puzzle that I've always had. (Conversation with Arnold Harberger, October 1997)

[10] Attempting to investigate Stigler's motives flies directly in the face of a key tenet of the Chicago approach to economics which he not only subscribed to, but also helped fashion. In this analysis, what is fundamental, the only element that in fact counts, is what can be measured. Given this methodology, economists are limited to observing what people do rather than what they say.

> So, I think the emphasis on behavior, about what people actually *do* is the only thing you've got. Are people irrational? Probably, sometimes. But I don't know if that's going to concern me when I think about what happens when OPEC raised the price of oil. You can ask whether people really have feelings about the Arabs. You can worry about what these are. But, I don't see where that's going to help you out. (Conversation with Sherwin Rosen, October 1997)

sion itself. He was alert to what incentives effectively moved its members and caused the profession to change, and how new theories and modes of analysis developed.

> He identified very much with the profession. He cared whether the profession moved ahead. So much of his work dealt with issues concerning the profession. If you look at my catalogue of his papers, you'll see what I mean. There are a lot of categories under the heading 'Professionalism.' (Conversation with Claire Friedland, October 1997)[11]

His continued interest in the mechanics of the discipline itself was not limited to the more standard History of Thought aspects of the subject. He also took something of a sociological stance, striving to understand what drove economists to perform. Although he above all others acknowledged the self-interested motivations and the rent-seeking behavior lurking behind human action, Stigler characterized the profession as dominated by a search for knowledge and understanding.[12] He never quite squared his intense belief that human action was reducible to narrow self-interest with this more transcendent version of the objective professional removed from all distraction and temptation. His sometimes strained attempts to balance these conflicting motivations infected his thoughts with a persistent whiff of cognitive dissonance swirling around them. True economists, those honestly following their chosen calling,

[11] For a few of these, see Stigler (1959, 1960, 1965, 1969, 1975). A working title of a review of Galbraith's *New Industrial State* (1967) displays Stigler's patent dismissive style of framing (and defaming) in his description of the author as "Galbraith—Puritan of the Left" (Box 23, Galbraith Folder—Stigler Archives, University of Chicago Library).

[12] Stigler recognized that most economists are "more impatient to do good and probably … not sanguine about our ability to engage usefully in full time scientific work" (Stigler 1976:354). Though acknowledging the egotistical pleasure of pursuing the applause from one's own peers, he was far from optimistic about the probable outcome flowing from such ambitions.

> Nor, perhaps, are we wholly satisfied with what Samuelson calls our own applause—indeed we cannot be confident that this applause is unaffected by our policy positions. I concede to Samuelson, nevertheless that to a scientist educated hands make more melodious applause than ignorant hands, but too often the educated hands seem to be sat upon by educated asses. (Stigler 1976:354)

were almost by definition incapable of being solely motivated by narrow, self-interested gains.[13]

> We wish to be scientists, with sound logic in our theories, reliable procedures in our empirical applications of those theories, and objective and fair-minded statements of the limitations of our knowledge. (Stigler 1976a:353)

Ostensibly for Stigler, this scientific imperative became far more powerful for him than any possible policy implications his work might contain or that might be implied and pursued by any other economist.[14] The

[13] The obvious problem with wanting to define an economist as a scientist (rather than a commonplace engineer) caused Stigler to indulge in a bit of sleight of hand. The scientific economist would somehow discover the capability of pulling himself (or given the gender divide, occasionally herself) out of the self-interested, rent-seeking milieu in which economic agents operated. By some version of Gatsby-like re-creation, the economist, as scientist, would move perceptibly closer to the realm usually afforded to angels. This would not only free him of self-interest but of influences due to ideology or the specifics of time and place.

> The dominant influence upon the working range of economic theorists is the set of internal values and pressures of the discipline. The subjects for study are posed by the unfolding course of scientific developments. (Stigler 1960:40)

> I gradually realized, I don't know when I realized, that he was one of the most fascinating examples … of how economists act, even though they say that economics ought to be value free (and it is sometimes value free). I find it extremely difficult to resist reading a lot of economic theory, politically. (Conversation with Mark Blaug, April 1998)

[14] This belief epitomized his well-known difference with his close friend and colleague Milton Friedman. Shaping public views remained an important goal for the irrepressible Friedman. Stigler insisted that preaching when performed appropriately reached no further than the boundaries defined by the economics profession. Whether he really craved public anonymity, like Friedman's brother-in-law, Aaron Director, is a much more subtle issue to determine.

> On one other personal occasion I remember something related to this question coming up. I was at lunch with Milton Friedman and George Stigler at the Quadrangle Club in Chicago and I was then a very young man. And somehow the younger you are, the more evangelistic you are. So I would debate and argue with people about policy issues and as I recall Milton asked me if I would be interested in going on a tour of some campuses, I think in the Southern United States, to talk on these policy issues. Milton said, "What have you got to lose by doing this?" And George said at the table to me, "Only your anonymity." So, on that occasion, I think he was hinting that maybe I ought to stick to my scientific work. (Conversation with Harold Demsetz, September 1997)

This took the more extreme form of arguing that there is no direct role for the economist in the formation of public policy. His belief in the efficiency of the political marketplace (which followed logically from employing consumer sovereignty as one's starting point) and the parallel assumed rationality of individual voters allowed no other conclusion to be reached.

advancement of knowledge and its usefulness as a platform for future analysis was indisputably paramount in forming his conception of the academic economist. He remained adamant that ideology or outside influences did not, and could not possibly, affect professional work.[15] This went beyond simply a belief in the integrity of its practitioners.

> One evidence of professional integrity of the economist is the fact that it is *not* possible to enlist good economists to defend protectionist programs or minimum wage laws. The groups who seek such legislation accordingly must seek elsewhere for their spokesmen and theorists—and judging by their success, the *ersatz* economists do their work well. (Stigler 1976a:349)[16]

> How to reconcile consumer sovereignty, or voter sovereignty, with his previous notions of inefficient government? Can we say this is illegitimate if the public wants it? Is that consistent with our extreme position on consumer sovereignty which is that no matter what horrible things the public wants, as free-market economists we can never question it. That's certainly one of the basic principles of neoclassical economics. Consumer sovereignty is both the end of the story and the beginning. And we don't argue with the consumer, no matter how self-destructive these demands are or how inappropriate. Anyway, if you want consumers to be free to choose in the marketplace, how can we argue with them in the political arena where, in a sense, they are acting as consumers too? Well, in his last years he was writing frequently about this. (Conversation with Claire Friedland, October 1997)

[15] As previously indicated, for George Stigler, progress in economic thought was almost entirely internally driven rather than a reflection of outside events. In the same sense, ideological considerations could have little impact on economic research or the consensus reached by the profession. This approach would inevitably cause Stigler to be blind to his own ideological biases. Readers should remember that he was born into an age where American men were not given to Hamlet-like introspection. They acted rather than reflected. In Stigler we find a seemingly non-tragic Lear rather than an internally racked Macbeth. "Yet he hath but slenderly known himself," (Shakespeare 1958:45). If carefully examined, the issue of scholarly motivation is actually far from clear cut. Certainly the 1930s were driven by a number of internal debates (including those centred on increasing returns) but it would take a daring leap of faith to claim that the work done did not reflect the unavoidable environment created by the Great Depression. Logically, as well as practically, it is impossible to completely escape one's time and place.

> Beginning with the 1930s, there was a period of very active work on economic theory, macro and micro, in both areas. What became prestigious was work in a kind of economic theory, namely pure and largely mathematical oriented. And it did not really have any considerable history. Now that period of change and development, that excitement, has disappeared. We are now in, what I would say is, a relatively flat period of additions to the structure. (Conversation with Milton Friedman, August 1997)

[16] This nearly tongue-in-cheek remark tends to beg the question more than a sliver. By definition, any economist who would defend such measures is not a good economist. Therefore, not a single good economist would support such policies. Stigler may be correct here, but would know all too well that you need to define criteria first before placing someone in the category of "good econo-

In terms of integrity, no economist, or certainly very few, is known to have directly shifted positions according to the whims of his or her current paymaster. Vested interests, then, need not inherently corrupt practicing economists. However, the very nature of the profession, one that encompasses a sufficiently diverse and divergent group, would allow any vested interest to shop for and find an appropriately matched view. Nor would it appear exceptional for an economist to fiercely market his or her honestly held position in order to gain such advantageous recognition.

> It is possible, and in fact usually the case, that an intellectual can please his customers without recourse to professing beliefs he does not actually hold, or other dishonourable practices. Each economist has a variety of views and let us assume for a moment that they come directly from heaven or hell. Unless one of us is singularly narrow in his inventory of views, some of the views appeal to some people and some views to others, and the audiences to which they appeal vary widely in size. It would be astonishing if we did not cultivate those views which had the largest audiences. (Stigler 1976a:349)[17]

mist". Only then can such a statement be endowed with any redeeming value apart from the residual enjoyment gained from reading a well-fashioned dismissive and typically acerbic remark.

[17] Stigler here is specifically focused on policy alone rather than theoretical matters. In the realm of pure theory (where economics is equated to science from Stigler's viewpoint) self-interest doesn't enter (or perhaps does so much more subtly).

> Such scientific information is value-free in the strictest sense: no matter what one seeks, he will achieve it more efficiently the better his knowledge of the relationship between action and consequences. (Stigler 1976a:350)

Notice how Stigler here tries to resolve the tension between the necessities of a self-interested academic and the objectivity he wants to attribute to the scientist. The pure researcher gains satisfaction and a sense of accomplishment (of doing good) from the process itself without widespread recognition.

> I have already argued that within narrow limits these goals are compatible: the society wants and will benefit from increments of the objective knowledge of economic life. A few men actually adhere only to this type of work, eschewing all pronouncements on matters of current policy. … These near-saints of scholarship are wholly unknown to the public, and not always well-known within the profession. Frank Knight was an approximate illustration of this rare type. (Stigler 1976a: 353–354)

It is not clear that Stigler ever actually resolved this issue to his own personal satisfaction. He persistently claimed to be unmoved by the applause of the masses, yet at times seemed resentful of those who did gain public acclaim. So it is not clear that the acknowledgement of his peers was ultimately sufficient.

It would seem reasonable to agree with Stigler that economists, for the most part, do not change their views to suit special interests. Economists are not especially venal, but they are human, as even George Stigler was forced periodically to admit. They have egos that need stroking and they nurture a priori beliefs even when unacknowledged or unrecognized.

> But on the other hand, the motives that drive them and me are not completely clear, either. When we strive to solve a scientific problem, is ambition for our own professional status completely overshadowed by our love of knowledge? I wonder. When we write an article to demonstrate the fallacies of someone else's work, is our hatred for error never mixed with a tiny bit of glee at the display of our own cleverness? I wonder. (Stigler 1963:92)

Thus it is difficult to differentiate Stigler's clear disapproval of public notoriety from his own possible yearning after a similar recognition, a sort of simultaneous evangelical abhorrence and attraction to sin.[18]

> I remember an amusing conversation I had with him at one time. Milton Friedman was making his *Free to Choose* television program. George Stigler had advised him not to do it. 'Don't do it. It's only cheap publicity and really a television program can't express ideas properly.' And he said it again, when it was very successful. I had the feeling that George Stigler was slightly jealous. (Conversation with Mark Blaug, April, 1998)

Stigler was persistent in constructing a more subtle image than his close friend and colleague, Milton Friedman, especially by eschewing a controversial public personality. However, despite a convincing attempt to hew to the course of the dispassionate researcher, the gleam of his

[18] Perhaps that is what partly formed a bond between George Stigler and John Kenneth Galbraith. Stigler couldn't resist twitting Galbraith as an attention-seeking populist in articles and reviews (1954, 1967, 1977). In reply, Galbraith, no rank amateur when it came to the caustic dismissal, fired back and possibly hit his target dead on.

> I am reluctant to reply to Professor Stigler for I could seem to be urging the claims of my book against those of a very great classic. And I could conceivably be missing the deeper cause of Professor Stigler's sorrow which may be not that so many read Galbraith and so few read Smith but that hardly anyone reads Stigler at all. (Galbraith quoted in Hession 1972: 89)

scientific white coat did not entirely eliminate the strong set of a priori beliefs which inevitably affected his work. As pointed out previously:

> He has to a considerable extent, gotten away with murder. Because, I'd say, unlike Milton Friedman, you have to be really very aware when you read George Stigler of these preconceptions. You know, a lot of people that read George Stigler are quite surprised when you tell them how, of course, very pro markets he was. OK, they realise that he was at Chicago and that sort of thing. But he is wonderful in disguising himself, with his wonderful, his very funny, ironic, cynical stance. (Conversation with Mark Blaug, April 1998)

Certainly there is enough empirical evidence of ideological bias within Stigler's beloved profession to make it difficult to dismiss such claims out of hand, as he did with such great ease. Too often, merely seeing the name of an author is sufficient to accurately forecast the results and conclusions categorically displayed in any article or other relevant output.[19]

> Well, this is a popular Friedman view too. And it's wrong. I say that flatly. But it's interesting that just recently—I have somewhere a National Bureau Yellow Jacket manuscript of a research study by Victor Fuchs from Stanford University, Jim Poterba from this university and Allan Krueger of the Woodrow Wilson School at Princeton. They did an extensive sampling of economists in two areas of economics. … And what they did was they gave a whole set of questions on what each person's factual opinion was on that question. What do you think is the elasticity of supply of labor under this condition? And so he had all these factual differences in the group. But they also asked questions about their value

[19] Predictability, operating at such a high level of degree, inevitably raises the question of ideology, beliefs held so firmly (whether consciously or not) that all evidence seemingly confirms these unassailable truths.

> It seems to me that when you get to his [Stigler's] later work, say with Becker, you know what the conclusion is going to be before you start the argument. In a sense, you're assembling arguments to support a conclusion. I mean, that may be unkind and untrue but it's an impression. And, it's even more so in the work of Richard Posner. Have you read any of that? It seems to me that the plot is always the same, and the characters stay fixed. It's the same thing, to some extent, one might say about Becker. But his work is so very good. And you learn so much from studying it, that that element in it is not a problem. (Conversation with Ronald Coase, October 1997)

> judgments. … Then they tried to see how you explained the differences in policy recommendations. And their finding is the opposite of Milton Friedman's. … Now what they found was the difference in their policy recommendations were—I'm using your language, not their language—ideologically premised values. They were not fact-driven. (Conversation with Paul Samuelson, October 1997)

Although Stigler's arguments were consistently forceful and regularly persuasive, as far as this insistence on a value-free scientific output goes, the weight of evidence tends to point in the opposite direction.[20] No better example can be provided than his own conduct, particularly when engaged in a no-holds-barred attack on an opposing position.[21] To comprehend his method for choosing a particular target and why he sought to demolish, rather than engage opponents, more evidence is needed than that provided by his own words and description. More was at stake

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[20] Recent psychological research indicates that people tend to be significantly influenced by the strong views of others. Thus, unequivocally worded comments reacting to news articles or opinion pieces can steer subsequent debate in a chosen direction. Certainly preconceptions formed by reading reviews or listening to friends can shade the way in which films are viewed or restaurants experienced. Stigler would often employ dismissive, humorous or even downright cruel comments in an attempt to steer debate.

> John R. Commons wrote on the legal foundations of capitalism in a book that I believe is impossible to read. Clarence Ayers started a school in Texas that never got beyond the state lines. (Stigler speaking in Kitch 1983:170)

Almost like a magician who spurns discussing the tools of his trade, Stigler rejected any analysis of economic rhetoric as essentially empty and unproductive. Although he believed fervently in the importance of marketing ideas, the sort of enterprise delineated by McCloskey was in his eyes a sideshow without any redeeming value.

> To date, the only clear consequence of the study of rhetoric for economics appears to be conferences and volumes such as these. (Stigler 1989:840)

[21] Stigler openly admitted that such a "no quarter given" strategy was simply a basic requirement for gaining acceptance or at least recognition of one's argument. However this insistence on taking a hard-nosed approach fails to account for his almost instinctual strategy of attempting to immolate key positions which countered or opposed his deeply held views.

> The tenacity with which people hold the ideas in which they have a proprietary interest is not due simply to vanity. A scholar is an evangelist seeking to convert his learned brethren to the new enlightenment he is preaching. New ideas encounter formidable obstacles, the foremost being indifference, but also the new ideas will often conflict with old ideas or clash with apparently contradictory experience. A new idea proposed in a halfhearted and casual way is almost certainly consigned to oblivion. A scholar who cannot convince himself that what he proposes is certainly true and possibly important is asking a good deal of others to generate enthusiasm for the idea. (Stigler 1988: 211)

during these ensuing battles than simply an innocent quest that sought to establish the goals of truth and clarity alone. Consequently, more is needed than what his carefully published public persona can reveal. To understand the ferocity behind his attacks, the reader requires the context, history and objectives behind each of his particular conflicts, as well as any common thread linking them together.

> Now, you can see the way George Stigler had a wonderful nose for attacks on neo-classical economics, let's call it, which were dangerously capable of undermining the very foundations of orthodox economics. It's no accident that he went for Gardiner Means' *Administrative Prices,* Harvey Leibenstein's *X-Efficiency*, Galbraith's *Affluent Society* and his other things, the *Kinked Oligopoly Curve* of Sweezy. These are all ideas that are very, very dangerous. They are subversive even of orthodoxy. (Conversation with Mark Blaug, April 1997)

To bring such evidence to light entails stubborn and intensive subterranean excavation. A simple description or recapitulation of the battles themselves is unlikely to suffice. What is needed is a careful reconstruction of a particular era and the players engaged during that period. There is always a lead-up that preceeds any of Stigler's key muggings. With a few notable exceptions, once he had driven that wooden stake through the black heart of a heretical theory, so that it could do no more than moulder harmlessly forgotten, Stigler seemingly did not return to the fray. However, more careful research tends to show that each one of these theoretical challenges would continue to niggle in his brain for many decades to come. In cases like the kinked demand curve, Stigler (1978) continued to attack despite a lack of any viable opponent remaining.[22]

[22] Paul Sweezy never responded to Stigler's attacks, perhaps the most irritating reaction possible. However Stigler's reconceptualization or version of the kinked demand curve certainly determined the standard textbook depiction (see Freedman 1995). Stigler's continuing annoyance, however, most probably arose from the model's zombie-like persistence even after undergoing what he deemed to be an effective immolation.

> I haven't read it. I don't think I ever did. I don't think I was aware of it actually. I didn't pay much attention to Stigler in those days. I was probably in one of my ultra-left moods, or something like that. (Conversation with Paul Sweezy, November 1997)

Once he got hold of an idea, the possibility of shifting his views or convincing him to let go often appeared to be slim.

> The article was accepted and he would surely have been asked, since I directly referred it to him as an editor. I never got a note from George saying "Well, this time around I've got to admit I was wrong. And your reading was right." Not at any time. And a lot of people would tell me that if they wrote to him complaining about something, he would answer something like "Well, if you're the kind of person who believes that, then you're just the kind of person who believes that." (Conversation with Paul Samuelson, October 1997)

My argument is that when roused, Stigler initiated what were essentially barren debates. Rather than encouraging any fruitful exchange of ideas, Stigler chose to see conflicts in terms of the adversarial role attorneys perform when engaged in a courtroom battle.[23] Placed within that environment the objection becomes one of triumph rather than extending understanding or knowledge. Such an objective does not necessarily preclude enlightenment, but only places it at a distinctly lower order of priority. Combatants enter the realm of winner-takes-all contest, a zero-sum game, rather than the mutually beneficial exchange characterizing a positive-sum game, or for that matter a simple market contract. In doing this, Stigler is by no means unique, but he was perhaps one of the

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[23] Viewed from a certain standpoint, the struggle engaged in by George Stigler is not unlike the Christian Church in its relentless crusades to eliminate heretics and other idolaters.

> The philosopher, who considered the system of polytheism as a composition of human fraud and error, could disguise a smile of contempt under the mask of devotion, without apprehending that either the mockery or the compliance would expose him to the resentment of any invisible, or, as he conceived them, imaginary powers. But the established religions of Paganism were seen by the primitive Christians in a much more odious and formidable light. It was the universal sentiment both of the church and of heretics, that the daemons were the authors, the patrons, and the objects of idolatry. Those rebellious spirits, who had been degraded from the rank of angels, and cast down into the infernal pit, were still permitted to roam upon earth, to torment the bodies and to seduce the minds of sinful men. The daemons soon discovered and abused the natural propensity of the human heart towards devotion and, artfully withdrawing the adoration of mankind from their Creator, they usurped the place and honours of the Supreme Deity. By the success of their malicious contrivances, they at once gratified their own vanity and revenge, and obtained the only comfort of which they were yet susceptible, the hope of involving the human species in the participation of their guilt and misery. (Gibbon 2009:389)

more accomplished practitioners of this dark art. Unfortunately, Stigler's attacks more often than not shifted debate onto unproductive and even somewhat misleading terrain.[24] This outcome should produce little, if any, surprise given that his aim was simply to curtail, and if possible end, any further debate or discussion. This strategy is perhaps a hallmark of ideological attacks motivated by something that exceeds mere doubt or disagreement.

The best way to explore and demonstrate the mode and method of Stigler's critical attacks and the subsequent impact such strategic behavior had on the economics profession, is to look carefully at a distinct case in point. In Edward Chamberlin, George Stigler discovered a willing and very odd bed-mate. Odd, because unlike other sparring partners, they were both exceedingly conservative economists. Each of them had studied under Frank Knight, with Chamberlin completing his dissertation under the aegis of Knight's mentor Allyn Young at Harvard.[25] No profound policy differences divided the two when George Stigler took the stage at the London School of Economics in 1948 and proceeded to mercilessly savage Chamberlin's life's work, his theory of monopolistic competition. What then incited Stigler's wrath? Keep firmly in mind that even at this early stage in his career he had unquestionably decided that neoclassical price theory was the only sound vehicle that could advance economics as a form of scientific thought. Transgressing this faith, Chamberlin's approach violated three cardinal principles that formed the backbone of Stigler's position.

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[24] In his more seminal articles, George Stigler had an almost prescient talent for asking the right questions even though simultaneously maintaining a persistently bad habit of knowing the answers to those questions in advance. In his attacks, the issue isn't whether the objects of his wrath were flawed (more often than not, they were) but whether his methods were productive or not.

[25] Both turned their dissertations under Young into seminal works. Knight's work became *Risk, Uncertainty and Profit* (1921) while Chamberlin converted his into the first edition of *Monopolistic Competition* (1933). Chamberlin was a student of Knight when both were at the University of Iowa. Such an incestuous mingling of relationships is bound to make pinpointing the exact origin of specific ideas difficult, especially since Young, though very highly regarded, published sparsely, preferring to share his ideas generously with students and colleagues. Unfortunately, populating an academic environment with such idiosyncratic players almost inevitably yields a veritable hothouse of malicious rumor-mongering, envy and disdain.

> And in fact it was always rumored that what was good in *Risk, Uncertainty and Profit* came from Allyn Young, who never published much. The rumor about Chamberlin's economics was the same. … I think this is not true in either case. (Conversation with Paul Samuelson, November 1997)

> Three distinguishing characteristics of neoclassical price theory are important here. First, the theory exudes confidence that rational behaviour succeeds in realizing mutually beneficial exchange opportunities. Second, it counts the individual—whether consumer, labourer, or business owner—as unimportant, despite its reliance on self-interested individual behaviour: it uses aggregations of the behaviour of individuals to construct its equilibria, and in doing so it deprives the individual of any force in the economic system. Third, it relies on Marshall's two-bladed scissors, supply and demand, to construct these aggregations of the behaviour of individuals. (Demsetz 1993:795)

To understand fully what at first glance would seem to be a largely unprovoked or out-of-proportion eruption (or perhaps fulmination) against Chamberlin, we will need to understand and reconstruct the key elements that characterized Stigler's reaction to monopolistic competition.

- Stigler's Chicago education, the influence of Frank Knight and his eventual theoretical break with his teacher
- Stigler's response to Chamberlin's review of his textbook
- Stigler's conception of economics as a science and his evaluation of the contribution of Chamberlin's theory
- Underlying ideology—price theory and economic liberalism

II. The Young Lochinvar: *Bildungsroman* or Coming of Age at Chicago

> It seems that a great many thoughtful people in the world are like Pontius Pilate in that they ask the question of our title, but "do not wait for an answer." But a considerable number differ from him in the interesting respect that instead of asking others the question they volunteer to give the answer themselves, to others, and to the world, without waiting to be asked. This leads to the writing of books of varying character and size, which one suspects are more interesting on the average to their authors than they are to any considerable number of readers. And to many of those who do read them this may be a comforting thought, since it means that books on methodology probably do not do much damage. The chief reser-

vation would be that they are most likely to be read and taken seriously by the young. (Knight 1940:1)

Chicago economists in the 1930s appeared determined to let two major revolutions in economic thinking wash right over them. Many of the faculty largely dismissed the demolition of Marshallian price theory and the invention of macroeconomic analysis, in particular under the tutelage of Keynes.

> This swift adoption of the Keynesian system came about, I believe, because its analysis in terms of the determinants of effective demand seemed to get to the essence of what was going on in the economic system and was easier to understand (at least in its broad outlines) than alternative theories … The speedy adoption of these new approaches was in large part due to the very unsatisfactory state of the existing price theory. That this was so had been demonstrated beyond doubt by the controversies in the *Economic Journal* in the 1920s and perhaps above all by Piero Sraffa's 1926 article. We were therefore looking for ways to solve the dilemmas these discussions revealed. (Coase 1994:21–22)

"Sufficient unto the day" was a fundamentalist belief in the precise application of partial equilibrium comparative statics.[26] Though this might not have been strictly true of every single faculty member teaching in the economics department at that time, it would accurately comprehend those who most influenced the young Stigler's thinking, particu-

[26] Though self-defined as staunch defenders of Marshall's heritage, their preciseness in the application of the perfect competition model missed the very essence of Marshall's approach. His method grounded in practical reality was a deliberate fudge, an attempt to stretch static techniques to cover dynamic analysis. Such an approach provided guideposts for economic thinking rather than any precise mechanism. In this regard at least, Marshall maintained the core principles of the earlier classical liberal economists (Mill being the clearest example). The earlier Chicago approach, as exemplified by such staff members as Viner, Simons or Knight, tended to follow Marshall in spirit rather than opt for a more simple-minded, literal or fundamentalist reading. However, Friedman and Stigler seemed determined to equate Marshall with a strict employment of partial equilibrium analysis. The more elusive or slippery elements would have clashed with their dogged insistence on following a self-identified, scientific approach. This denoted a possible confusion, allowing precision to become muddled with insight.

> The Statical theory of equilibrium is only an introduction to economic studies; and it is barely even an introduction to the study of the progress and development of industries which show a tendency to increasing return. (Marshall quoted in Loasby 1989:69)

larly Knight and Viner, with Simons a more indirect force. (However any attempted leap to equating the work or viewpoints of Knight and Viner should be strictly avoided.) The two fellow graduate students who became his most congenial classmates and friends, Milton Friedman and Allen Wallis, were equally under Knight's influence at the time.[27] This friendship could only deepen the conservative leanings already nurtured during his Chicago maturity.[28] Though methods of application would later separate them,[29] he remained faithful to Knight's insistence that economic theories, by definition, had to deal with universals.[30] What those

[27] The three would have only existed as an inseparable trio (along with soon-to-be wives) during the academic year 1934–1935. The more administratively minded Wallis would bring them together once more during the last few years of the war at the Statistical Research Group (Columbia University). Finally it was Wallis once again, acting as the Dean of Chicago's Graduate Business School, who persuaded Stigler to return to Chicago as Walgreen Professor (jointly appointed to the Economics Department and Business School).

> Twenty-five years for the tale to unfold
> Yo-ho-ho and again there are three
> Walgreen was good and Kimpton was bold
> Yo-ho-ho and again there are three. (Friedman 1993:771)

[28] From his own writings, and from those who knew him, it is clear that Stigler was greatly influenced by a very limited number of people (few other than Friedman, Director or Becker could sway his thoughts). Unquestionably all of the most influential of them held (or were poised to hold) fundamentally conservative political views.

> Aaron Director, for example, would never have written a good letter of recommendation for somebody who wasn't a staunch conservative, but neither would Milton. (Conversation with Paul Samuelson, October 1997)

[29] The reverence in which Knight was held by graduate students in the 1930s approached an almost cult-like level.

> At that stage he was, as many people were at the University of Chicago, quite besotted with Frank Knight. George's thesis topic was Carl Menger, the father of the mathematician Karl Menger with a 'K.' I remember a sentence he said. He said, "Carl Menger is very good, but everything good that is in him is already, (I can't say already) in Frank Knight." Frank Knight's influence on the student body was profound and not, I say in retrospect, a hundred per cent positively constructive. … Knight had a very strong influence on George Stigler and all the graduate students. (Conversation with Paul Samuelson October 1997)

Ultimately Stigler's objectives ran at cross-purposes to Knight's skeptical view of economics. He turned increasingly toward the price theory learned under the tutelage of Jacob Viner.

> He changed in his view about Knight, the assessment of Knight's work. I think that his opinion of Knight went way down. He still thought Knight's work was important, but nowhere nearly as important as he did when he was an undergraduate and a young person. And contrariwise, his assessment of Viner went up and that's the same for Henry Simons. If you look at parts of the *Program for Laissez Faire*, it doesn't look that interesting at this stage, so he went down on that. (Conversation with Gary Becker, October 1997)

[30] Logically, an economist, according to Stigler, must assume that underlying economic relationships persisted across time. This view is behind conceptualizing or speaking even metaphorically

universals should be remained a core issue throughout George Stigler's long and fruitful career.[31]

> … all concepts sufficiently general and sufficiently precise to be useful in scientific analysis must be abstract: that, if a science is to deal with a large class of phenomena, clearly it cannot work with concepts that are faithfully descriptive of even one phenomenon, for then they will be grotesquely undescriptive of others. (Stigler 1957:17)

Stigler though departed from at least some of his natural allies in rejecting ideology as playing any more than a trivial role in economic

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about economic laws. Once committed to such laws, we are then strolling within the domain of the natural environment rather than in a world where a combination of art, judgment and pragmatism form the basis for progress.

> If the problems of economic life changed frequently and radically and lacked a large measure of continuity in their essential nature, there could not be a science of economics. An essential element of a science is the cumulative growth of knowledge, and that cumulative character could not arise if each generation of economists faced fundamentally new problems calling for entirely new methods of analysis. (Stigler 1983:533)

Contrast this position with the sharply distinguishable approach favored by Keynes that imagined economics as an approach to analysis, rather than a machine for mechanically churning out results.

> It seems to me that economics is a branch of logic, a way of thinking; and that you do not repel sufficiently firmly attempts a la Schultz to turn it into a pseudo-natural-science. One can make some quite worthwhile progress merely by using your axioms and maxims. (Keynes 1973: 296–297)

In the 1930s at least, many in the profession still followed Marshall in taking a cautious approach to theory, wary of imposing a physics-like agenda on the discipline. In his presidential speech to the American Economic Association, a future colleague of Stigler's (John Maurice Clark) could sum up the past and present by reminding his audience of crucial limitations inherent in theoretical approaches. (Contrasting Clark's speech with Stigler's, almost thirty years later, reveals to a considerable degree the transformation of the profession's methods and approaches.)

> … while a picture of perfect equilibrium deals in its way with forces which are at work in the actual world, the form in which it presents these forces will almost inevitably need to be modified when we move on to the task of studying them as they actually operate. (Clark 1936:4)

[31] It is easy to see Stigler as jumping from one idea to the next without stopping to root out crucial details. Some colleagues tended to see him more as type of hybrid fox/hedgehog in knowing many big things. But there is an underlying consistency and universal basis in all his post-war work. His vision of market efficiency made price theory the overall glue holding all his work together. He attempted to push the logic of perfect competition to its explanatory limits. While someone like Chamberlin was an obvious, if even obsessive, hedgehog, Stigler might best be pictured as a hedgehog disguised as a fox.

outcomes.[32] Given consistency as a categorical touchstone of his career, Stigler could choose no other viable alternative.

> An ideology … is a commanding set of beliefs, beliefs that are probably not grounded upon self-interest or are related to the interests of the holders in so subtle and obscure a manner as to make it more useful to treat the beliefs as data. (Stigler 1988b: xiii)

Providing ideology with any shred of credibility would at least partially dislodge the anchor of all his analysis, basically that human action was ruled by rational self-interest.[33] Only the most complex of manoeuvres allowed him to elevate the economist to a position where narrow self-interest assisted rather than interfered with the search to uncover the workings of an economy. Bringing ideology back into the mix would only succeed in upsetting this fragile balance. It is questionable whether such a position would

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[32] Surprisingly, this was one of the rare moments when Stigler did not read closely from the same Book of Common Prayer as his intellectual comrade, Milton Friedman. For Stigler, any resort to such a vaguely founded principle could only undermine the scientific progress of the discipline, positing a causative value that lacked any real explanatory power.

> I don't know how important ideology is, but think it is unimportant. You don't know how important it is, but think it is important. My position is better because I try—feebly and so often unsuccessfully—to use a trusted theory of human behavior to explain social phenomena. Your position is worse because you try—with marvellous ease—to explain the mysteries by a deus ex machina. (Letter from George Stigler to Milton Friedman, 29 March 1984)

[33] Self-interest couldn't simply remain a useful assumption to pose, or a reasonable characterization of a dependable human motivation. The assumption itself had to be testable, universal and consistent. Actions which then might appear to have alternative explanations must undeniably reveal themselves, upon examination, to be driven by narrow self-interest.

> Man is eternally a utility maximizer, in his home, in his office—be it public or private—in his church, in his scientific work, in short, everywhere … I believe it is a feasible and orthodox scientific problem to ascertain a set of widely and anciently accepted precepts of ethical personal behavior and to test their correspondence with utility-maximizing behavior for the preponderance of individuals. My confidence that the test would yield this result will be disputed by many people of distinction, and that argues all the more for making the test. (Stigler 1982: 35–37)

Given Stigler's unassailable belief and unshakable conviction, he could logically find no place in which ideological preferences were free to lurk.

> The simplest way to test the role of ideology as a nonutility maximizing goal is to ascertain whether the supporters of such an ideology incur costs in supporting it. If on average and over substantial periods of time we find (say) that the proponents of "small is beautiful" earn less than comparable talents devoted to urging the National Association of Manufacturers to new glories, I will accept the evidence. But first let us see it. (Stigler 1982:35)

ultimately withstand any careful scrutiny. Given Stigler's own assumptions about human psychology, sanitizing conclusions against a deep-seated set of beliefs seems a Houdini-like sleight of hand, rather than anything resembling an assured method. Like any other academic economist, Stigler does not come to his analysis with anything resembling a blank slate. Self-consciously scrubbing out obvious influences and accrued pre-judgments would inevitably be difficult within any context, but nearly impossible if such influences are categorically denied. Empirical testing provides no Messiah-like salvation in this case, since it is not capable of delivering any assured route of escape. Facts fail to speak for themselves, but rather need to be articulated through nuanced interpretation. The nature of such epistemological excavations cannot under such circumstances be strictly neutral or objective. Consequently, when marketing his own theories, Stigler succeeds in multiplying the difficulties attached to this possibly irreconcilable problem, given his instinctively adversarial approach. His competitive and almost obsessively combative nature led him to effectively destroy contending theories in a Pacman-like fashion. Individual self-interest and ego cannot fail to play a key role in such gladiatorial battles.

> For lawyers to write tendentious briefs in an adversarial environment present few problems; no one expects, or demands, the truth from only one side. But for academics to twist facts, no matter how brilliantly, to fit the preconceived interest of their clients is disturbing. (Weinstein 1992:75)[34]

Monopolistic competition represented a challenge if not a veritable gauntlet to Stigler's core beliefs. As he came to view the matter, the theory violated two cardinal aspects of economics as a discipline that allowed no forbearance to be offered. Whatever Chamberlin's intentions, Stigler's

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[34] It should be noted that Stigler did appear as an expert witness for clients in legal disputes and was well aware of the danger of shading analysis to win the battle. However, reference here is made to the more frequent case where Stigler is his own client fighting for a set of deeply held values and beliefs. By turning discussion into an adversarial process, the very nature of one's analysis and investigative procedure must correspondingly change.

> As Viner recounted his participation to me many years later, he began testifying as an objective academic student of price theory. After sharp cross-examination by the attorneys for the other side, however, he found himself becoming more and more defensive of the government's position and more and more critical of the steel companies. That adversarial process had turned him into an adversary. (Stigler 1988a: 130)

adamantine evaluation of the theory convicted it as unredeemable on these two key counts. It was badly flawed (or even unrepairable) and thus wasted the valuable time of the profession. Equally, this alternative approach, by knocking out the underpinnings of perfect competition and substituting market power instead, provided an unfortunate excuse or justification for government intervention. The inevitable result would be to encourage bad policy (or possibly collectivist action) and the reinforcing of distorted, and thus poor, economic incentives. Attempts to do well by employing such badly flawed means could only end in tears. These errors lacked the grace of being merely venial sins, but instead proved to be deadly sins of the very deepest hue. This danger provided Stigler's periodic crusades against selected theories with a certain moral tone that seemed to motivate his subsequent desire to decimate these heretical theories, instead of seriously discussing the challenges they posed. Although entering the fray with no intention of misrepresenting what were deemed threats to traditional price theory, he was nonetheless determined to rescue his profession from its own folly. In doing so it became somewhat inevitable that he would also end up misleading himself about the objective and motivation of his own intentions.[35]

[35] He only attacked alternative theories that he viewed as threats. Reaching this category required two characteristics. The theory had to be gaining popular support within the profession (or at least holding its own across a widespread swathe of economists) and it had to be basically irreconcilable with price theory, namely by offering a viable alternative to perfect competition as the starting point for economic analysis. Thus despite an acknowledged (and growing) contempt for Clark's workable competition, Stigler refrained from any concerted professional attack. Though it is true that he did make a dismissive remark in his Nobel Prize speech (Stigler 1983:536), as well as in the classroom.

> Few, if any, areas of economics have as much confusion, circular reasoning, definitional traps, and fervent nonsense as industrial organization. It was the perfect place for Stigler to conduct a Demolition Derby. Nor was he hesitant about the task. Theories like "monopolistic competition" and "countervailing power," which were treated reverently at Harvard (where they originated), were eviscerated by Stigler. Another concept—one I had wrestled with in vain at Harvard—was "workable competition," which he didn't deal with at all in the course. When I asked Stigler after class one day what it meant, he replied, "It means such competition as pleases the economist who is talking." (Sowell 1993:787)

Taking on Clark's theory would have failed to meet both fundamental criteria for action. (It never achieved the appeal of monopolistic competition.) Plus, he might have wanted to avoid an unnecessary and unproductive feud with a Columbia colleague, keeping in mind the Knight/Douglas hissy fit at Chicago in the 1930s.

> He cared about the profession in the same way. What he thought about monopolistic competition, *aside* from its being inconsistent with neoclassical economics, was that it wasn't productive. (Conversation with Claire Friedland, October 1997)

This is a particularly honest, if only partial estimate of what drove George Stigler. He really did think that some very good minds in the profession were wasting their time on a particularly non-productive area of analysis. Such theories could only serve to divert the attention of the profession and to befuddle the minds of newly entering members. The responsibility, if not obligation, of every professional economist was, accordingly, to steer discussion relentlessly back onto the right track.[36] The requisite urgency of this aim might at times translate into adopting whatever tactics were deemed necessary to accomplish the betterment of the discipline. This does provide at least a partial explanation for Stigler's devastating attacks when faced with dangerous examples of heretical price theory. Narrow self-interest became more than a touch entangled with implicitly collectivist aims, though the collectivism (improving professional standards) was of the private and ostensibly voluntary category.[37]

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[36] This created more damage than if the underlying purpose was merely a matter of occupying the time of mediocre graduate students in much the way that Stigler sought to dismiss Mason's case-study approach to industrial organization. Harrod similarly dismissed Tinbergen's econometric analysis as being of only trifling importance. "Otherwise the ordinary competent researcher finds nothing to do but write a history of the Milk Marketing Board, or to indulge in the mathematical but rather fruitless refinements of the green publication of the L.S.E." (Harrod, letter to Keynes 1973: 298–299)

[37] At least to some degree this collectivist or group responsibility would appear to contradict Stigler's increasing insistence on narrow self-interest as the sole driving motivation and all-encompassing basis for human action. Though, at an earlier and more unformed stage of his career, he would have found at least some room for a Knightian hierarchy of altruism. These exceptions would seem to depend on their proximity to a web of close personal relationships that might underlie the actual exchange. In fact, Stigler's continuing extension of his own binding ties to department, university and professional activities stood as a distinct affront to his unyielding insistence on ascribing human actions solely to narrow self-interest. All choices could be rationalized, at least retrospectively, in this manner. However when examined more carefully, such altruistic actions remained recalcitrant in lying outside that narrow sphere of operations.

> I raised this issue with him in a slightly different guise. When George was skeptical as I said about the altruism issue I said, "Look George, look how generous you are to your children. Are you doing that out of self-interested motives? Who are you kidding? You're not doing it out of that." He looked at me and he didn't answer and he knew he wasn't doing that out of self-interested motives. He was actually very generous with his family, as he was with people in general. So, I think George, when pressed hard like I did there, would admit there is a motive beyond simply selfish self-interest. (Conversation with Gary Becker, October 1997)

Certainly his concern for his beloved profession was genuine and always proved to be a motivating force. Whether attacking Chamberlin, Sweezy or Leibenstein, his attachment to the advancement of economics would unarguably form one of the necessary ingredients stoking the ferocity of his attacks. Yet despite Stigler's intensive Chicago experience and training, which engrained his unshakeable affection for economics, such an explanation fails to reveal either the basis for choosing his targets or the unforgiving adversarial nature of his debating tactics, including the vehemence with which he decimated selected opponents. His need to defend the Chicago style of price analytics was not simply a matter of professional pride. Instead, there were a number of intertwined forces that fortuitously joined together as Stigler was preparing his LSE lectures, some of which can certainly be traced back to his graduate days at Chicago. Others probably reached back into his childhood years outside Seattle. (These more psychological issues will be deliberately left to moulder rather than be disinterred. Poking around Stigler's imagined subconscious would provide an amusing diversion, but would admittedly be performed by a rank amateur, guided only by suppositions and possibly muddled intuitions.)

Here it is important to remember that when he delivered his lectures in 1948, he had in fact little empirical evidence to justify the definitiveness of his beliefs.[38] He consistently trusted what might be termed his economic intuition or innate sense of how an economy worked.[39]

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[38] Stigler attributed this propensity to his training at Chicago and particularly the influence of Frank Knight. His claim seems justified, though he did take to this decidedly aggressive approach like a politician to graft. In 1948 Stigler had begun his long struggle to escape from the shadow of Knight and perhaps unbeknownst to him, from the kingdom of Marshall as well, but the combative stance and belligerence of his mentor remained.

> Chicago had a strong influence upon us. One lesson that I learned, or possibly overlearned, was that of scepticism toward received beliefs and authoritative reputations. Knight in particular was prepared to dispute the Ten Commandments. I suspect that we heard the word "nonsense" too often. I certainly came away believing that the popular acceptance of an idea was little support for its validity. On the other hand, the school failed to immerse some of us in what, in retrospect, was a major irreversible wave of the future—the systematic use of statistical data to estimate economic relationships and to test economic theories. (Stigler 1988a:26–27)

[39] He did in fact have an uncanny ability to home in on key issues and questions of an economic nature. Whether his perceived solutions ultimately held water is an issue beyond the focus of this analysis, but his ability to raise and tackle essential issues is difficult to deny.

> Stigler's achievements in many of the fields in which he wrote were well ahead of their time and so required from him a great deal of ingenuity in arriving at solutions to the problems he himself posed. He seemed to possess an uncanny ability to recognize a problem and an even greater ability to suggest possible solutions. … Much of Stigler's reputation was based on his abilities in this respect, and on the persuasiveness of his analysis. The problems of the discipline tended to become those which had been recognized by Stigler. (McCann and Perlman 1993:997)

As a graduate student he had come to trust implicitly in the ideas and perhaps intuitions of his classmate, Milton Friedman. Only after the first fateful meeting at Mont Pelerin did he add Aaron Director to this select group and the much younger Gary Becker was to arrive more than a decade later. All of these figures shared approximately the same approach to classical liberalism, all believing they were, to some degree, faithful disciples of Marshall.[40] Discussions within this core group could only reinforce Stigler's own closely held intuitions when formulating and tackling economic issues. In some sense it is not unreasonable to claim that George Stigler knew something was true because he needed it to be so.

> His judgments were very good. As I say, he could get the right conclusion from inadequate data. And, I don't know how you do that … I mean, how you explain that sort of thing. He had an intuition that was always very good. And I think it particularly helped when he was dealing with the history of economic thought. For then you have to think, there is so much you don't know. You've got to fill in the background. But he filled in the background. (Conversation with Ronald Coase, October 1997)

[40] Trust of course provides an efficient short-hand for judging issues. Or in the case of the inner Chicago circle, Milton Friedman deciding whether someone was "one of us," to use a phrase he often employed. The group forming around George Stigler, Milton Friedman and Allen Wallis synchronized to that particular tune and when convenient even deliberately ignored empirical evidence which they found inconvenient. Such behavior unilaterally contradicted the empirically tested evidence that they persistently championed.

> Now what you have to understand with somebody like Allen Wallis, and so to a degree those people who were in his circle, is that Allen Wallis had the sharpest priors—I'm using the language of Bayesian probability—of anybody I have ever known. Almost no new data could change his view for this reason. On the other hand, if he thought of somebody as a dangerous, or an incompetent thinker, but Jimmy Savage assured him that the man was very smart and had good judgement that carried more weight with Allen Wallis than a two-year study of the person's vitae and an audit of his writings. There's an in-group of the good guys and the much larger out-group. This showed itself in things that aren't even political. (Conversation with Paul Samuelson, November 1997)

The task then became to figure out a path which would lead to that preordained and quite necessary conclusion.

> He was interested, I would say primarily, in a particular sort of puzzle and it's a typical Chicago puzzle. And I don't mean that in any bad way, it's the sort of puzzle that the Chicago School's presuppositions require. Show me an apparent anomaly, something that does not seem to be explicable using the Smithian apparatus and the Marshallian apparatus and I will show you that it can be explained that way. That was exactly the sort of thing that George went looking for. (Conversation with Robert Solow, November 1997)

Whether consciously or not, when we adopt such a path our work becomes shaded by our own ideological prisms. Often the influence is so subtle as to lie unrecognized by many casual investigators. Such seems to be the case with George Stigler, especially for those who only know a handful of his most cited papers. However this ideological coloring can be discerned throughout his carefully selected, bare-knuckled brawls with designated opponents. A later, but highly representative, embodiment of Stigler's own ideological flavoring appears when examining his attitude to game theory. His stance was in many ways highly reminiscent of a much earlier, but equally effective, surgical evisceration of monopolistic competition. Ostensibly in both cases he questioned, purely from a neutral perspective, whether either approach was operative, only hinting that they might be essentially inoperative theoretical constructs.[41] His underlying

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[41] George Stigler had turned much of his attention away from the strict confines of industrial organization when a resurrected game theory became the leading analytical tool in the very field that Stigler had done so much to shape and define. (His work on regulation and political markets is not so much a departure as a continuation of his earlier work in industrial organization.) As always, he was painfully blunt in his estimation of employing this particular technique. However, speaking in all fairness, at the time of his death in 1991, a lot of the empirical results that he required had failed to develop. But even granting these circumstances, his time line still seems peremptory, if not arbitrary at best. Typically, he made his unadorned opinion of the game-theoretic approach known to his colleagues in tones that were hardly equivocal.

> It's OK to try game theory. But to stick around for twenty years and come up with a result that anything is possible and then to say that this is economics. This is almost the way George would be talking if he was sitting here. "Having you and your six friends argue about a lemma, that's progress!" He wouldn't be indignant. He would be laughing. He would be dismissive. Saying, "You're dopes. You're dopes." What should you do with them George? "Exile them to Samoa." Dismissed with a wave of the hand. (Conversation with Sam Peltzman, October 1997)

adamantine antipathy, however, more likely lay in the potential these two theories seemed to embody. Both, if embraced, would undermine to a fundamental degree what Melvin Reder (1982) refers to as Chicago's insistence on a tight prior equilibrium.

> The most recent development in industrial organization is the emergence of game theory, which has dominated the writings of the younger economists in the major eastern schools and Stanford. This literature is closely related in spirit to Chamberlinian economics: It is much more rigorous (as well it should be, fifty years later) but has not shown equal gains in empirical motivation or empirical applicability. (Stigler 1988a: 168)

Stigler displayed a notable degree of intolerance for what might be termed pure theory. As signalled by his consistent criticisms, the problem posed by such an approach is to some degree a reflection of his own impatience for a theory to flower.[42] He seemed willing to blow the whistle on approaches prematurely. Game theory, under his self-prescribed terms, could be dismissed as theoretically consistent but barren. Being allotted to one of his many non-productive bins of economic reasoning transformed the offending theory into a simple drain on the profession's resources. In Stigler's judgment, the path such theories generated led to neither predictions nor generalizations. Game theory thus shared with monopolistic competition the unhappy result of only claiming that results must depend on a number of different factors specific to each case. Given the created distractions and deadweight drain of such unproductive sideshows, any honorable economist would be obliged to do no less than to employ any means at hand to place the profession back on the right track.

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[42] Colleagues and friends testified to an almost compulsive need to achieve his goals, perhaps even reaching to the level of being a workaholic. He was impatient with himself and with others.

> George was a very kind of protestant-ethic guy. He was hard working. He had a conscience. If he didn't finish what he set out to finish by a certain day, he was upset. "Oh, I haven't done what I intended to do." He was always setting deadlines with me. "Let's try to finish this by Friday." Well, if economics had waited 200 years for this, why are we trying to finish it by Friday? (Conversation with Claire Friedland, October 1997)

> On all these counts, game theory could not and did not appeal to Stigler, whose own work was guided by his desire to extend the reach of neoclassical theory. He believed that the past work of economists had created a useful set of tools by which to solve economic problems. He could not abandon these tools for an untested set that elevated negotiations between individuals to the level of a central paradigm of thought. He refused to play the game. (Demsetz 1993:799–800)

While Stigler was honestly concerned with the fact that further work on monopolistic competition was a misuse of valuable time, his belief that such analysis did not carry the profession forward seems inextricably bound to the fact that it presented a clear alternative starting point to the perfect competition approach that defined neoclassical, but especially Chicago-style, price theory. Like some latter-day Don Quixote, George Stigler saw neoclassical theory as his irreproachable Dulcinea. Perhaps like the conjured beauty of that maiden, price theory created a faith born out of the necessity of Stigler's hopes and objectives. Claire Friedland succinctly points out the degree to which he would impetuously hurl himself into the fiery heart of a battle whenever heretical forces posed a threat to this particular damsel in distress.[43] It is noticeable that Stigler was loath to

[43] In a statement she may have come to regret later, Claire Friedland claimed that "Much of his work centered around saving the damsel in distress, neoclassicism, from her attackers" (Friedland 1993: 780). The claim, whether subsequently disowned or not, is painfully accurate, but too easily open to misinterpretation. It transforms Stigler into some sort of latter-day Quixote figure, where the allusion carries with it some distinctly negative baggage. Such a response encapsulates an understanding which remains the direct opposite of what is intended. Some readers may in fact perceive such an analogy to be largely demeaning, if not trivializing, of the issues with which Stigler battled. Reaching this particular conclusion would, however, indicate a much greater familiarity with the Broadway musical version than with Cervantes' classic novel. Don Quixote's tilting at windmills has connotations which signal a more serious, underlying importance to what may appear to be only buffoonish behavior. Quixote was in no sense some figure of fun that is best dismissed lightly. He grappled with that badly defined boundary separating necessary from delusionary belief. In a parallel effort, the problems Stigler faced and attacked were at the forefront of professional importance. These were the giants with the temerity to masquerade as harmless windmills. Often only Stigler's own perspicacity allowed him to sprint ahead of the academic pack and recognize these unexplored dangers. He possessed a rare knack for sensing what issues were indeed essential to clarifying and deepening the discipline. Both Stigler and Quixote demonstrate the undeniable importance of belief as forming the underlying foundation of action. Moreover, what appeared to be a metaphoric "tilting at windmills" at the start of the Chicago counter-revolution would, with marked persistence, allow a transformation which would turn those comical mills into countless vanquished giants.

waste his formidable firepower on any but the most infectious and deadliest of perceived dangers. In this regard, Stigler was a virtual weathervane of orthodoxy, able to detect any serious heresy within the established canons defining the Church of the Economics profession.

Stigler regarded the natural merits of individual choice, provided only by unhindered working of the market mechanism, as an unwavering article of faith. Consequently, such a tenet could not possibly be defined in any sense as a means to some other end. A basic objective of this type must remain a good in itself if not the highest achievable goal possible. In the post-war period, George Stigler came to regard this approach as the basis for moral as well as economic judgments. Ethical personal behavior depended on the freedom of individuals to make unconstrained utility-maximizing decisions. Individual responsibility regulated market exchange while also constituting the core of any workable moral code. Consequently for Stigler, this perception could not be reduced to the level of a heuristic assumption, but rather must inevitably exist as a potentially testable hypothesis.

> … I believe that it is a feasible and even an orthodox scientific problem to ascertain a set of widely and anciently accepted precepts of ethical personal behavior, and to test their concordance with utility-maximizing behavior for the preponderance of individuals. In fact Rawls's proposal of a method of constructing an inductive ethical system, which I briefly described earlier, is exactly the procedure that would show that the ethical system was based on utility-maximizing behaviour. My confidence that the test would yield this result will be disputed by many people of distinction, and that argues all the more for making the test. (Stigler 1982:36–37)

In many ways, Stigler did definitively break with Knight and any strictly Marshallian notion of classical liberal economics. But in a more fundamental sense he believed he was keeping faith with the core of classical liberalism, which rested on the fundamental importance of the indi-

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Heavens knows whether there be a Dulcinea in the world or not, and whether she be a notional creature or not. These are mysteries not to be so narrowly inquired into. Neither have I engendered, or begot that lady. I do indeed make of her the object of my contemplations, and as I ought, look on her as a lady endowed with all those qualifications that may raise the character of a person to universal fame. (de Cervantes 1993:553) vidual. Liberal thought, in his understanding, emphasized the underlying dominance of choice as the most secure basis for freedom and liberty.[44] To ensure that these core values would remain inviolate required a willingness to bear sacrifices, even of a repellent nature, if the periodic waves of collectivism and socialism were to be kept at bay.

> I dare say, I think that would have been Milton Friedman and George Stigler's attitude to McCarthy. One would have wished he didn't do it so loudly, he didn't do it so vulgarly, but they would have said that he was essentially right. In the same way that a lot of people said you know, you have to put up with McCarthy to keep communism in check. Stigler would have regarded McCarthyism as not being a threat. (Conversation with Mark Blaug, April 1998)

III. Defining the Battlefield

> Those, who are strongly wedded to what I shall call 'the classical theory', will fluctuate, I expect, between a belief that I am quite wrong and a belief that I am saying nothing new. (Keynes 1964:viii)

Stigler's attack was aimed directly at the work of Edward Chamberlin and not that of Joan Robinson. Superficially this might come as something of a surprise given that politically, even in 1948, the two were poles apart. Nor did Stigler subsequently demonstrate any particular admiration for either the woman or her work.

> Joan Robinson is here for a visit, I had my first view of her today. You're a fool to leave Bobby and go to Corfu to see her. (Letter from George Stigler to Robert Solow, April 20, 1961)

However, Stigler was never one to waste his ammunition on secondary targets. He took aim at Sweezy's (1939) kinked demand curve, quickly

[44] Not for Stigler the sardonic jest of Anatole France in his reflection on the mirage of economic choice.
The law, in its majestic equality, forbids the rich as well as the poor to sleep under bridges, to beg in the streets, and to steal bread. (France 2009: 51)

dismissing Hall and Hitch (1939) as something of a minor distraction.[45] In his LSE lecture, Stigler warms up for an unconstrained attack on Chamberlin by reducing Robinson (1933) to a mere cameo appearance within a larger frame containing matters of much more vital import.

> Of Mrs. Robinson's work I need say little. It is amply clear, on a re-reading at this distant date, that her message was in no sense revolutionary, although at times her language was rebellious. … Her volume marks no break with the tradition of neo-classical economics; indeed it contains, I think, too uncritical an acceptance of the substantive content of orthodoxy. (Stigler 1949a: 12–13)[46]

It is difficult to grasp exactly what was at stake when Stigler honed in on Chamberlin without understanding to some degree the internal debate that had flared up among economists in the 1920s. Starting from Marshall's theory of the firm, various young economists came to question the proper basis and starting point of economic analysis.[47] A key contributor and influential figure in the debate was Chamberlin's (and Knight's) PhD supervisor Allyn Young (1928), who attempted to tackle the problem of increasing returns to scale.

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[45] Freedman (1995) discusses the way in which Stigler managed to reframe the manner in which the profession discussed the kinked demand curve. Notice Stigler's (1947) use of the dismissive framing device deployed in the title, "The Kinky Oligopoly Demand Curve and Rigid Prices." As Rosewell Perkins remarked when introducing George Stigler as the dinner speaker for the 65th Annual Meeting of The American Law Institute, "That [the title] sounds to me not only humorous but perhaps a bit salacious" (Perkins 1988:60).

[46] This evaluation accords with Milton Friedman's recollection of how he and his fellow graduate student, George Stigler, reacted when the two volumes came out in 1933.

> My recollection is not worth much, but for what it's worth, it's that the Robinsonian emphasis on the individual firm economics, the analysis of marginal revenue and marginal cost, fitted in very well with what we were otherwise thinking. There were no problems about that. (Conversation with Milton Friedman, August 1997)

Such an estimate is oddly consistent with Robinson's latter evaluation of her early work.

> In the introduction to the first volume of her *Collected Economic Papers* Joan Robinson declared that when she "worked out *The Economics of Imperfect Competition* on assumptions" she "took the wrong turning;" the correct path would have entailed "abandoning the static analysis and trying to come to terms with Marshall's theory of development." (Loasby, 1989b:71)

[47] An intriguing discussion of this debate is covered by George Shackle (1967).

The once-familiar claim that "it's all in Marshall" perhaps indicates the origin of the festering theoretical issue that burst upon the profession in the 1920s. Marshall's attempt to use static, partial equilibrium models to explain time-dependent changes and development required a good dose of creativity and hand waving that seemed to elude his successor, Alfred Pigou, as well as many other economists that followed.[48] (Keynes would also run into a similar problem in overburdening his static tools. Like Marshall, he would be misinterpreted by his more literal-minded brethren and disciples.)

> Marshall acknowledged with great candour that he was flummoxed by the problem of dynamic analysis. After discussing the problem of equilibrium with falling supply price he writes; "But such notions must be taken broadly. The attempt to make them precise over-reaches our strength." … There are other indications that Marshall habitually thought of a movement to the right along a supply curve (output increasing) as a movement forward through time. This accounts for the extraordinary importance that he attached to what now seems a mere *curiosum*—economies of large-scale industry in competitive conditions. The reason is that he somehow boiled

[48] Given Marshall's almost pathological need to qualify and fudge, it is hardly surprising that in the desire to make the analytics more rigorous, much of the subtlety was lost. The problem remains that Marshall used his models more as a story-telling device. For economists who feel the urge to pose as mature scientists, the idea of telling tales around a campfire would be deemed beneath their professional dignity.

> …as Samuelson rightly observed, what is in Marshall cannot be revealed by reading Marshall alone. What one sees is very largely a reflection of one's own viewpoint. (Loasby 1989a:47)

Certainly Marshall's method predictably would only end up frustrating a mathematical economist of Samuelson's mould for whom Marshall's approach could only create a confused and muddled analysis.

> How did a wise Second Wrangler get himself into this quagmire? His gratuitous attempt to treat increasing returns (to scale) processes by his competitive SS-DD cross was his fatal idiocy. He knew better. Anglo-Saxon economics lost 10 years [1922–1932] working out of his unnecessary swamp. (Letter from Paul Samuelson to George Stigler, February 27, 1991)
>
> I recently reread all of Marshall … I did not begin with a high opinion of Marshall (he was overpraised by my teachers and I rebelled). But I ended with a diminished opinion. He shows no development and this from a highly unsatisfactory 1880 state. He never got partial equilibrium right: not only did he fail ever to work out the demand functions for independently-additive utilities; he never shows that he knew how to do so. He simply stopped working at his mathematical economics. (Letter from Paul Samuelson to George Stigler, April 11, 1990)

> the effect of technical progress going on through time into the movement down his supply curve. (Robinson 1974:41)

The upheaval of the 1920s that spilled over into the 1930s was not so much a questioning of Marshall but of his designated successor Pigou, who in attempting to formalize the received wisdom and make it more rigorous (or scientific) in line with his own proclivities seemed to blur the line between formal theory and policy conclusions without sufficiently shoring up the logic of that theory. Unlike Stigler, who persistently downplayed the effects of historical environment on theoretical developments, Shackle traces some of the background impulse behind the questioning of formal price theory to the unsettling impact of the Great War on a younger generation of economists. That war led to a noticeable crumbling of received structures and authority.[49]

> But the second of those decades brought to an end the Pax Britannica and the tranquil generation-and-a-half which had favoured and fostered a belief in a self-regulating, inherently and naturally self-optimizing, stable and coherent economic system. When men had got back their breath after the war and turned to apply their conceptual tools to repair the ruins of European organization, their failure (which a few year of endeavour forced them to acknowledge) to bring back the old order of things made them begin to ask for new tools. A new generation of students, which went seriously to college only in 1919 or after, had graduated and begun to think,

[49] Even theoretical developments that seem stubbornly internal to a discipline cannot be completely divorced from the time and place in which the debate occurs. Economists do not develop or continue to live in the equivalent of a hermetically sealed cacoon. Their thinking must to some degree be shaped by their surroundings. Even the marginal revolution of the late nineteenth century, which appears to be part of an internal questioning of value theory, cannot be isolated and kept apart from the contending scientific classicism of Marx and its revolutionary content. Marshall, the great ameliorist, was to some degree guided by his need to present the workings of the marketplace in a more favorable light, allowing the march of history to yield increased fruits of labor shared equitably by all.

> Whatever happens, every individual is a child of his time; so philosophy too is its own time apprehended in thoughts. It is just as absurd to fancy that a philosophy can transcend its contemporary world as it is to fancy that an individual can overleap his own age, jump over Rhodes. If his theory really goes beyond the world as it is and builds an ideal one as it ought to be, that world exists indeed, but only in his opinions, an unsubstantial element where anything you please may, in fancy, be built. (Hegel 1967:11)

> impelled by new questions and freed in some degree from old pre-conceptions. (Shackle 1967:5)[50]

Pigou's narrower vision and derived policy prescriptions inadvertently created controversy and invited debate. His formulations would inevitably bring into question the refined model of perfect competition, its use and role in economic analysis. Thus Pigou was battered on two fronts, namely that the logic of the theory was inherently flawed or conversely that the theory, though perfectly usable, was being misappropriated and misapprehended.[51] On one hand this led to Sraffa's (1926) explosive criticism based on the internal logic of the approach, while on the other Knight, maintaining the value of the theory for scientific inquiry, rejected Pigou's policy conclusions. The only prescriptions that could flow directly from theoretical constructs should accordingly be restricted to the hypothetical variety.

> The contention is that individual profit-seeking leads to an excessive investment of resources in industries of increasing cost (decreasing returns), part of which would yield more product if transferred by social action in some form to industries of constant or decreasing cost. The fallacy to be exposed is a misinterpretation of the relation between social cost and *entrepreneur's* cost. (Knight (1953:161)

For Schumpeter, like Knight, a theory of perfect competition represented a sound and even necessary basis for a scientific approach to eco-

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[50] The profession itself immediately after World War II seemed intent on understanding why and how their profession had tacked so noticeably away from its more traditional theoretical headings. This became a repeated subject for discussion at the annual American Economic Association meetings.

> This can only mean that some very general and considerable change was going on for some time prior to 1929 in the region of the basic preconceptions upon which all economic thinking rests, or even in the region of the social attitudes and apprehensions of which such postulates are rationalizations. That is, it must have transpired somewhere before the level of abstraction on which for the most part economic analysis now takes place. (Ayres 1946:114–115)

[51] Pigou served as a negative inspiration for a number of economists. Coase, as a case in point, seemed driven to redirect economics away from the dangerous chasms that Pigou's work engineered.

nomics.[52] Given this perspective, the problem lies not with the mechanics of exchange operating in a theoretical world of perfect competition, but in adapting those insights to the way in which markets actually operate. The conditions required for markets to function optimally should not, then, be confused with desirable or pursued objectives.

> It cannot be repeated too often that the case of perfect competition owes the fundamental importance which it always had and still has in economic theory to certain properties characteristic of it and neither to any tendency in the facts to conform to it nor any "desirability" of the state of things it depicts. … the theory of perfect competition still remains a useful and almost indispensable background with which to compare, and therefore by which to understand, any other situation, however far removed it may be from it. (Schumpeter 1934:249)

Knight's problem with Pigou, then, is that his focus is simply misguided if not muddled. Such a wrongheaded approach is bound to generate confusion, rather than clarity, within the profession itself.

> Under certain theoretical conditions, more or less consciously and definitely assumed in general by economic theorists, the system would be ideal. The correct form of the problem of general criticism … is, therefore, that of bringing these lurking assumptions above the threshold into the realm of the explicit and of contrasting them with the facts of life—conditions under which competitive dealings are actually carried on. (Knight 1953: 178)

The initial controversy, treating the internal contradictions that evolved out of standard Marshallian theory, commenced with a series of discussions, highlighted in *The Economic Journal*, between Clapham (1922) Pigou (1922) and Robertson (1924). These critical dilemmas essentially culminated in Sraffa's (1926) seminal paper appearing in the same journal.

[52] Knight recognizes and satirizes the idea that the model of perfect competition reflects perceptible market operations. In fact, the sort of costless, frictionless world depicted employing textbook-like logic was at best absurd.

> The difficulty is, of course, avoided if "friction" be so broadly defined that "perfection mobility" means the absence of all resistance to the human will. But in a world where a breath could transform a brick factory building into a railway yard or an ocean greyhound there would be no need for economic activity or economic science. (Knight 1921: 34)

His article disrupted the fundamental assumptions buttressing an independent and operative supply and demand curve. Removing this requisite condition underpinning partial equilibrium analysis left Marshall's Economic Bible in tatters, or so the critics claimed. With his demolition work complete, Sraffa's reconstructed resolution of this seemingly intractable issue necessitated a progressive path forward that lay steeped in the precepts of market power, namely, the systematic vision usually associated with standard models of monopoly.[53]

> It is necessary, therefore, to abandon the path of free competition and turn in the opposite direction, namely, towards monopoly. Here we find a well-defined theory in which variations of cost connected with changes in the dimensions of the individual undertaking play an important part … Everyday experience shows that a very large number of undertakings—and the majority of those which produce manufactured consumers' goods—work under conditions of individual diminishing costs. Almost any producer of such goods, if he could rely upon the market in which he sells his products being prepared to take any quantity of them from him at the current price, without any trouble on his part except that of producing them, would extend his business enormously. (Sraffa 1953:187)

The roadmap detailing the direction which such a critical examination of equilibrium analytics could pursue is best summed up by one of Allyn

[53] John Hicks would reach a diametrically different response to the more drastic alternative of acquiescing to a terminal demolition of partial equilibrium analysis. In essence, he appeared willing to allow the reach of economics to be much more circumspect, in order to preserve the basic analytic apparatus provided by perfect competition.

> It has to be recognised that a general abandonment of the assumption of perfect competition, a universal adoption of the assumption of monopoly, must have very destructive consequences for economic theory. Under monopoly the stability conditions become indeterminate; and the basis on which economic laws can be constructed is therefore shorn away. … It is, I believe, only possible to save anything from this wreck—and it must be remembered that the threatened wreckage is that of the greater part of general equilibrium theory—if we can assume that the markets confronting most of the firms with which we shall be dealing do not differ very greatly from perfectly competitive markets. … At least, this get-away seems well worth trying. We must be aware, however, that we are taking a dangerous step, and probably limiting to a serious extent the problems with which our subsequent analysis will be fitted to deal. Personally, however, I doubt if most of the problems we shall have to exclude for this reason are capable of much useful analysis by the methods of economic theory. (Hicks quoted in Loasby 197:873)

Young's (1928) infrequent publications.[54] In this widely acknowledged, but sometimes misinterpreted article, Young evaluates the impact increasing returns has had in shaping economic theory. His ensuing verdict fails to shine a particularly favorable light on the past efforts of the profession. The need to change course and head down a more fruitful path becomes a paramount prescription.[55]

> The apparatus which economists have built up for the analysis of supply and demand in their relations to prices does not seem to be particularly helpful for the purposes of an inquiry into those broader aspects of increasing returns. In fact, as I have already suggested, reliance upon it may divert attention to incidental or partial aspects of a process which ought to be seen as a whole. If, nevertheless, one insists upon seeing just how far one can get into the problem by using the formulas of supply and demand, the simplest way, I suppose, is to begin by inquiring into the operations of reciprocal demand when the commodities exchanged are produced competitively under condition of increasing returns and when the demand for each commodity is elastic. (Young 1928: 534)

Young easily sets the stage for his student Edward Chamberlin, at that time diligently working on his dissertation. It is clear that both Young's paper and Chamberlin's work had been influenced by conversations between the two. Both seemed to be travelling down similar paths.[56] Reading Young's

[54] The work is actually a presidential address Young gave before Section F (Economic Science and Statistics) of the British Association for the Advancement of Science. Unlike most economists, his career was cut short at the early age of 52. (Perhaps not young for poets or mathematicians, but practically cut down in his youth for an economist.) Nonetheless he influenced a number of noted economists including Frank H. Knight, Edward Chamberlin, Nicholas Kaldor and Lauchlin Currie.

[55] Young has often been praised as influential and his 1928 paper seen as a forerunner of endogenous growth theory. However, not all economists have been equally impressed.

> I do not wonder that you have found Young on Increasing Returns somewhat enigmatic. That has always been my reaction. When I have asked those who profess to understand its main substance to explain that to me, from half a dozen volunteers I get six different answers. The article is like a Rorschach blot: looking at it, you fabricate your own profundities. (Letter from Paul Samuelson to James Buchanan, April 12 1990)

[56] Unsurprisingly this has led to accusations that Chamberlin essentially simply repackaged Young's ideas.

From Earl J. Hamilton
University of Chicago

analysis foreshadows key elements of *Monopolistic Competition*, a work that would manage to achieve almost instant influence some five years later.

> … to conceive of all economic processes in terms of tendencies towards an equilibrium might even maintain that increasing returns, so far as they depend upon the economies of indirect methods of production and the size of the market, are offset and negated by their costs, and under such simplified conditions as I have dealt with the realising of increasing returns would be spread through time in such a way as to secure an equilibrium of costs and advantages. This would amount to saying that no real economic progress could come through the operation of forces engendered *within* the economic system—a conclusion repugnant to common sense. … I shall merely observe, first, that the appropriate conception is that of a *moving* equilibrium, and second, that the costs which (under increasing returns) grow less rapidly than the product are not the "costs" which figure in an "equilibrium of costs and advantage." (Young 1928:535)

Added to this non-equilibrium focus (a moving equilibrium may imply that movement toward such a locus results in an ever-shifting notion of equilibrium) is the increased importance of demand given that firms could act and determine their own path rather than simply responding to market mandates. The need to pursue customers rather than a sole focus on costs was to characterize Chamberlin's subsequent work.

> It is dangerous to assign to any single factor the leading role in that continuing economic revolution which has taken the modern world so far

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February 14, 1973
Dear Professor Blitch:
… It is a pity that I could not foresee your request for information concerning Allyn Young before I began packing to leave Duke, for I took extensive lecture notes under him in two major courses and preserved them until 1944. One thing that my notes taken in 1924–1926 conclusively showed was that every worthwhile idea in E. H. Chamberlin's subsequent work on imperfect competition had been clearly expounded by Allyn Young in class long before Chamberlin put pen to paper. Curiously, Young credited Cournot for most of what he said! He was the epitome of modesty (quoted in Sandilands 2009:22).
Later, 7 April 1999, Samuelson wrote that "My words about Knight and Chamberlin were to correct an impression that each of these were mere dummies who spoke with the voice of the master ventriloquist. Young himself spoke *against* such exaggerated rumors—which was not to deny that he did them lots of good" (Sandilands 2009:34).

> away from the world of a few hundred years ago. But is there any other factor which has a better claim to that role than the persisting search for markets? No other hypothesis so well unites economic history and economic theory. (Young 1928: 536)

Essentially this concerted questioning, leading to a perilous loss of faith in the accepted version of prices and markets, let the issue of private economic power seep back into legitimate economic debate. This meant the focus was no longer primarily on how the price system coordinated markets to allow for a division of labor and specialization. The acknowledgement that the exercise of market power might be the starting point for analysis was joined, almost opportunistically, with the economic collapse that defined the Great Depression. No longer could an almost blind faith in markets and their operational imperatives automatically retain adherents. In particular, the idea that a market economy yielded not only an efficient distribution of resources and rewards, but also a fair one, slowly sank under the weight of its own assumptions.

> Thus society in its business aspect is presented as a coherent system where each participant, given his endowment of capacities and material resources, gets the best for himself that is compatible with a similar best for each other participant. This conception, which answered all questions that could be asked in its own terms, depended on a notion whose extreme, abstract unreality could be justified only by its enormous theoretical efficacy. Perfect competition was indispensable to that triumphant theory of value which prevailed from the 1890s to the Great Depression of the 1930s. It solved a central problem: on what natural principle can the national product be supposed to be shared out amongst the factors of production? Calling in aid a mathematician of the eighteenth century, it was able by means of Euler's theorem to show that the whole product of an industry will just and only just suffice to pay all factors at the rate of their marginal products. Thus it clamped together the whole theory of "value and distribution". In that theory, moreover, unemployment appears impossible. (Shackle 1967: v)

The issue of economic power, its responsibility for the economic collapse of the 1930s and the need to regulate and control this perceived threat, became a recurrent theme of Roosevelt's New Deal. Business had demonstrated that it could not be trusted and the Pecora Commission excoriated the banking community for its collusive and self-dealing practises.[57] Markets, then, could not be blithely defined as self-regulating, but rather required constant oversight by government agencies. Such a charged atmosphere elevated seemingly ground-breaking work, such as Chamberlin's, but also brought to the fore the exploratory work of Gardiner Means, a figure who was to evolve almost into a satanic figure and a continuing *bête noire* for George Stigler.[58] Means championed government intervention by providing early statistical evidence of price administration. He claimed that his data-based analysis clearly demonstrated that rising pools of economic power translated into rigid prices, which negated their ability to act beneficially as a market barometer. For Means, non-adjusting prices implied a dysfunctional economy incapable

[57] Ferdinand Pecora was the flamboyant chief counsel of what was officially designated as an inquiry into the Wall Street Crash by the United States Senate Committee on Banking and Currency. Pecora seemed to take unbounded joy in humiliating banking chiefs (for instance the head of National City Bank). What became something of a staged show trial of the banking sector delivered a substantial measure of popular appeal, given the circumstances of that period (early 1933).

[58] Gardiner Means (1896–1988) was a Harvard-trained economist lifted out of obscurity by Roosevelt's New Deal. His insistence on greater competition to combat market power makes him something of a romantic, at least when viewed from a more modern perspective. (Though operating from a diametrically opposed political perspective, like Henry Simons, he insisted on the necessity of small competitive firms as the best defense against market malfunctions.) Curiously, the power of his arguments and his rising fame depended on his statistical work in a period when the employment of such tools remained a novelty. Stigler (1965a) in his AEA presidential address would champion a new empirically based economic science not unlike that produced by Means, but one that employed correct analysis in stark contrast (in his opinion) to the misuse of statistics by the likes of Means. In some sense, he was Stigler's doppelganger (though it must be left to individual taste or perspective to determine which one was Jekyll and which was Hyde). Both were ideologically charged, reluctant to admit errors and skilled marketers of their views. The two conducted an unresolved and unresolvable conflict that spanned decades. Their core argument, focused on the existence of administrative prices, reflected irreconcilably opposed beliefs in the existence of private economic power. What was at stake was ideologically fuelled by their policy concerns. Sadly for these two avowedly empirical economists, no conceivable evidence seemed capable of changing their minds.

> Both of them, both of these men had taken strong positions on this … I [James Kindahl] almost come close to saying, that they had taken these positions before I was born. That's not true, but it certainly was before I was born as an economist. I could have predicted George Stigler's predictions before I signed up from what I'd learned in graduate school of his writings. And I could have predicted, to a great extent, Gardiner Means' predictions and you could guess what I learned at graduate school about Gardiner Means. (Conversation with James Kindahl, October 1997)

of responding to periodic crises and instituted to benefit those wielding control in the marketplace. Even at Chicago, a remaining bastion of classical liberal economy, Henry Simons in defense of the market mechanism laid out a program heavily dependent on ruthless enforcement of anti-trust provisions to ensure competition and remove any lingering vestige of economic power.[59]

> Eliminate all forms of monopolistic market power, to include the breakup of large oligopolistic corporations and application of anti-trust laws to labor unions. A Federal incorporation law could be used to limit corporation size and where technology required giant firms for reasons of low cost production the Federal government should own and operate them… Promote economic stability by reform of the monetary system and establishment of stable rules for monetary policy… Reform the tax system and promote equity through income tax… Abolish all tariffs… Limit waste

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[59] Though Stigler as a student was an enthusiastic backer of Simons' program, by the time he came to write his intellectual autobiography he looked back on that enthusiasm more than a bit ruefully. However, he did realize that it had to be evaluated according to the tenor of the time. Perhaps this tacit re-evaluation underscores a reality not explicitly recognized by Stigler. The classification of classical liberalism, the view identified with that position, clearly can change over time, at least if it is to be stretched to include Stigler and his colleagues.

> Henry Simons had preached a form of laissez-faire in his famous 1934 pamphlet *A Positive Program for Laissez Faire*, but what a form! He proposed nationalization of basic industries such as telephones and railroads because regulation had worked poorly. (I am reminded of the king who was asked to award a prize to the better of two minstrels. After hearing the first, he said, "Give the prize to the second." Simons urged an extremely egalitarian policy in the taxation of income and detailed regulation of business practices such as advertising. Much of the program was almost as harmonious with socialism as with private-enterprise capitalism. (Stigler 1988a:148–49)

It is interesting to note how far from Simons, George Stigler (as well as Milton Friedman and Aaron Director) would travel. That markets needed careful government oversight to maintain a competitive level would soon become anathema to Stigler, though a firm belief in the need to attack concentrated economic power would remain a hallmark of his stated position throughout the 1950s (see Stigler 1952).

> He comes from a background in which a tough anti-trust policy is absolutely consistent with liberal economic policy and it's absolutely consistent with powerful market forces operating and so on and so forth. You have to remember the time. He was a student of Henry Simons who was the incarnation of that view. Markets would be great if we didn't have advertising. Simons is on record saying stuff like that. Markets would be great if we had atomistic competition. Just like the textbooks tell us. Right. So that's his background and I'm sure he believed that. He believed that. There was no contradiction in his belief system. (Conversation with Sam Peltzman, October 1997)

> by restricting advertising and other wasteful merchandising practices. (Simons 1934:40)

Thus George Stigler came of age during years of continuing economic depression when even those politicians like Roosevelt, striving to keep capitalism alive, would turn to dramatic interventionist programs. The National Recovery Act exemplified such a pragmatic and fundamentally experimental strategy. This clearly corporatist policy was launched in the hope that regularizing economic planning would prove to be an effective counter to private consolidation and control.[60]

> Actually, Wagner [Senator–New York] argued, the antitrust laws had never prevented the growth of monopoly. They had been used chiefly against labor and small business; and the proposed bill, by providing a method of rationalizing small business, would remove the existing incentive for further mergers and consolidations. The real intention was not to abolish competition, but to purify and strengthen it. (Hawley 1966:29)

George Stigler in the 1930s was thus surrounded by an almost feverish level of debate with economists of all stripes offering policy programs to resolve the prolonged and seemingly intractable economic slump.

> The economists at other universities were on average less critical toward the New Deal programs that Roosevelt launched. They did not share so quickly or fully the Chicago economists' outrage at the honeycomb of restrictions imposed by the National Recovery Administration. Economists elsewhere were not on average so incensed as we were by the farm programs. (Stigler 1988a:139)

This cacophony of debate, with alternative policies proffered and rejected, subsequently raised for him, perhaps far more than for other

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[60] The NRA (National Recovery Administration) tackled the seeming irreconcilable objectives of individual choice and monopoly power by interposing government as the protector and champion of economic freedom. Effective intervention would serve to rein in monopoly power. However, Stigler's eventual approach would be to deny any pervasiveness of monopoly power and instead label government as the chief aggrandizer and abuser of power. Thus private monopoly was reclassified as a strategic Trojan Horse enabling the unwanted intrusion of the visible government hand.

academics, the exact role that an honorable academic should perform and was capable of playing. George Stigler never quite worked his way through this very fundamental problem.[61] His life and career appeared dominated by ideas, yet Stigler continued to firmly insist that narrow self-interest provided a necessary as well as sufficient explanation for all human action. If anything, as he largely escaped the at times suffocating influence of Frank Knight and pushed for even greater universality and consistency in economic theory, this insistence seemed to intensify. Of course, at the very heart of his ideas system lay those concepts developed in his formative years. In one way or another (despite continu-

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[61] Self-doubt was never part of Stigler's public persona. "A second and related trait of scholars is that they seldom change their minds" (Stigler 1988a:210). Complete confidence was part and parcel of his extended marketing campaign to guide the profession away from the bogs and swamps of false persuasion and inchoate theory. Showing signs of wavering or hedging would be to undermine the compelling rhythms which imbued the economist's story with a patina of truth. In a similar sense, providing a discernible whiff of doubt would be as much out of character as having Don Quixote cavalierly dismiss the romance of chivalry as containing no more than a collection of bedtime stories. Yet there are moments of introspection when even a Don Quixote can exculpate his behavior by falling back on the necessity for holding to consistent and public beliefs, no matter what whiffs of lingering doubt might remain. Stigler's confidence in markets, in their efficient performance, never wavered. There might be private acknowledgements of intractable problems. Yet in his published work or public presentations such difficulties or potential stumbling blocks were not accorded a bare whisper.

> But I don't think that George had misgivings. I think that his presentation was what he felt. He had confidence in his theory and he felt that his data supported it. The defects, they are always mentioned somewhere, not in strong terms, but they are always there. Maybe they're in a footnote or an appendix, but the defects in the data, as far as we were aware of them, are there. We always tried, of course, to find a way to overcome them. But I think it was really a personality thing that saw him always presenting everything with great confidence. He was always a kind of rhetorician, although, he did give that bad review of the McCloskey book on rhetoric. (Conversation with Claire Friedland, October 1997)

This same inherent confidence thus allowed him to maintain without exception that vested self-interest motivated regulation and public policy.

> Like when he was confronted with some fact about regulation, he would say "Ah, you're going to find some Congressman was bought off. [laughter] You are actually going to find that. That's what you're going to find. Are you sure that you didn't find that this Congressman wasn't on the take?" (Conversation with Sam Peltzman, October 1997)

Yet he lived in a world (the academic sphere) ruled by ideas, one in which he seemed to believe that those ideas, rather than self-interest, were the dominant and decisive factor. Though not entirely unaware of this paradox, he often seemed drawn to the idea that the observer (economist) could somehow be removed from the realm in which the observation occurred (the world of self-interest). This portrait of the economist as the neutral scientist paints a picture of an academic capable of being swayed by evidence alone.

ing to dismiss the effect of historical influences (1960)), those young economists coming of age during the Great Depression remained forever marked by those events, as did their work (though the impact would have very different effects on this varied cast of characters). As previously noted, depression in the USA seemed to revive a perennial focus on the issue of economic power, in particular the problem of monopoly.[62]

According to a growing number of economists at that time, the US economy was growing ever more concentrated, less characterized by competition and its mediating system of flexible prices. Consequently, subsequent calls for a radical reorganization of the existing business system were a predictable, rather than surprising, development. As might be expected, such demands solicited considerable public support.[63] If we focus momentarily on the future that followed, this type of policy prescription perfectly foreshadows work that would be done by Galbraith, Means, Sweezy and Berle/Means. It was also exactly the type of thought that would elicit the full force of Stigler's shotgun attacks. All of these heterodox economists shared roots that could be traced back to the type of monopoly problems formulated in the 1930s. In those later years, Stigler's own work in industrial organization would testify to the essential nonexistence of such problems. In Stigler's detailed constructs (ostensibly

[62] In contrast to the thrust of the microeconomic revolution as represented by Chamberlin, macroeconomics, as reconstructed and pioneered by Keynes, deliberately ignored the problem of monopoly, dismissing it as an unfruitful distraction, given the nature of his objective. Debates that centered on the existence and degree of economic power, or on alternative theories of the firm, would shunt analysis and any subsequent debate onto a less productive spur line. His focal point was not the structure of markets or the degree of competition, but rather the inherent nature of any market based system when viewed in the aggregate. Especially in the USA, raising questions about monopoly would have fatally shifted the debate onto terrain that came to be stubbornly inhabited by Gardiner Means. Unlike England or the Continent, the USA had a long tradition in which such market power played a dominant role in any discussion of economic policy or reform. (Perhaps this reflects a long-held American sympathy for the underdog, a tendency to support David rather than Goliath.) The anti-trust provisions and progressive tradition of the late nineteenth and early twentieth centuries lack any real parallel with economic concerns elsewhere. In contrast, Keynes sets out to show that even competitive markets, which by definition were efficiently self-regulating, inherently faced demand-constrained conditions. Thus the core issue driving the economic collapse of the 1930s lay in the limitations and incentives of the aggregated economic agents rather than in the reflection of any specific market structure.

[63] The then political balance of the majority Democratic Party, heavily dependent on its 'Solid South' wing, ensured that the more radical aspects of Roosevelt's New Deal would never see the proverbial light of day.

tested against empirical reality) prices were flexible (Stigler and Kindahl 1970) and industry was not particularly concentrated (Stigler 1949b).

> Evidence of Stigler's attachment to neoclassical price theory is also given by that part of his work mainly critical of the work of others. Price rigidity, administered price inflation, the theory of monopolistic competition, and X-efficiency were prominent targets, and each of them denied the efficacy of the neoclassical analytical framework. (Demsetz 1993:800)

As pointed out, the young Stigler was still much under the influence of Henry Simons, who took the lead publicly during the 1930s in presenting the classical liberal position, at least as defined in the USA. This saw a highly limited, but definitive role for government in promoting competition through the use of anti-trust enforcement. What ensued during that period of the 1930s was a struggle for the soul of the New Deal with battle lines drawn between the economic planners, anti-trusters and the business rationalizers. This intellectual conflict was a legitimate struggle without a predetermined solution, as Roosevelt himself was an arch-pragmatist willing to explore any avenue that might lead to rapid results. To varying degrees, all three options were tried, sometimes in contradictory fashion and mostly without any conclusive results.[64]

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[64] See Hawley (1966) for a useful account of the warring factions vying for dominance during the New Deal. Speaking of Roosevelt's early economic policy, Hawley points out:

> Within a single piece of legislation, the authors of the measure had made room for the aspirations and programs of a variety of economic and political groups. But in a phraseology that could be used to implement any of several policies, they had laid the basis for confusion and controversy. (Hawley 1966:20–21)

The seeming irreconcilable problem presented by markets remained the irrepressible conflict between individual choice and monopoly power.

> The trend of modern industrialism has been determined in all countries by two conflicting tendencies: The one toward liberation of the individual from ties and codes inherited from the middle ages and the mercantilist era; the other toward integration on a more or less monopolistic basis. (Stolper quoted in Hawley 1966:4)

Stigler would easily slice this presumably Gordian knot by downgrading and even denying the pervasiveness of monopoly power. He instead developed a clear tendency to focus on government as the prime source of monopoly power and economic abuse. Such a strategic framework becomes clear, at least initially, in the five lectures Stigler delivered in 1948 at the London School of Economics. These are the very same lectures in which Stigler's virulent attack on Chamberlin's monopolistic competition occurs.

> Neither the planning approach nor antitrust action nor any of the compromises in between ever contributed much to economic recovery, although they did lead to increased governmental activities of each sort. (Hawley 1966:15)

To a degree, economists would continue to fight the battles of the New Deal long after that era was politically extinct.[65] The full weight of this influence becomes clear on introspection. George Stigler's career in many ways reflects a desire to undo the dangerous thinking and influence of the New Deal. Stigler consciously tried to expunge it from the collective memory of the economics profession, while his friend and colleague, Milton Friedman, explicitly took the fight into a more overtly public arena.

> Aaron Director: No, no, but I don't really think you can make something out of so very little. You can believe that your role is really—has very little to do with wanting to change the world. Yet, you know, if somebody wants to re-examine the price index; and they ask you to … I don't think that's really a problem.
> Milton Friedman: There's no problem. It's true, that George did want to change things.
> Aaron Director: But he preferred to study them, not to change them.
> Milton Friedman: He preferred to say that he preferred to study them.
> Aaron Director: He preferred to study them. I should quit the argument.
> Milton Friedman: It was partly a long-running difference between him and me.
> Aaron Director: You're right.

[65] Stigler would almost certainly deny that he was personally battling to change history. Having written extensively on the economics profession, Stigler favored ideas of internal development. Economics largely responded to the concerns and problems posed by other economists, rather than to outside events. Stigler would have preferred to perceive his critical efforts as responding to a wrong turning by the economics profession. Though I suspect he would even make the stronger claim that he was responding to problems of an intellectual nature when constructing his major work. This means that Stigler would strenuously deny that his work, critical or otherwise, reflected the surrounding issues created by his environment. Of course, holding this position to such a rigid degree created a persona that clashed with a younger version of George Stigler. This young man was a disciple of Frank Knight, who in some of his earlier works was fond of using the word *Weltanschauung* and not always in an ironic fashion.

> Milton Friedman: And he liked to stress, "I just want to understand the world and Milton wants to change it."
> Aaron Director: That's right. And predominantly I think that is correct.
> Rose Friedman: You would have to have them both psycho-analysed. (Conversation with Milton Friedman, Rose Friedman and Aaron Director, August 1997)

Chamberlin is a bit of an odd man out in a theoretical debate that at times reached theological levels. As recounted above, the post-war policy struggle reflected many of the battle lines distinguishable from the New Deal and to various degrees calcifying as a result of the war effort. The planners, or social engineers, made most of the running during this period, based on their wartime performance and the public's collective memory of the Great Depression.[66] Those supporting market solutions to economic problems opposed the planners' almost inbred optimism with a corresponding pessimism that could rise to nearly biblical proportions. In this view, the public was selling its freedom (its birthright) for the proverbial bowl of pottage. The economic freedom delivered by these collective decisions was only made possible by relinquishing individual freedom of choice, which meant forgoing the prized possession of personal liberty. These divisions and battles only accelerated as the war ended, leaving the collective planners to triumph.

> But a comprehensively planned state implies eventual rule by a self-perpetuating group of specialists. For in planning what industries are to expand we almost inevitably plan what groups hold power. And how long will political freedom survive severe restriction of the right to transfer beyond the reach of an arbitrary bureau chief? The little business that counts is the little business that has a chance to be big business. It is time

[66] Compare Stigler's war-time experience to that of Galbraith. Though each, at least for a time, worked for the same price-setting agency both drew diametrically opposite lessons from that period. In some ways, the reactions were predictable based on their prior positions and backgrounds. There were no "road to Damascus" moments of transcendence. Rather, this bureaucratic experience nurtured ideological slants that would later blossom during the Cold War. However, working in these administrative salt mines did not immediately set Stigler off down the specific path that would define the remainder of his academic career. This apotheosis would better be served by his Mont Pelerin ascent, including his initial bonding with Aaron Director at that meeting.

> that we liberals considered how much our redistributive income tax and other policies are serving to foster monopoly, to stratify society, and to hamper the independent access to the top on which technical progress—and even more important things—depends. (Wright 1946: 142–143)

Chamberlin seemed more driven by a professional debate arising out of the theoretical discussions of the 1920s, than in a metaphysical balancing of collective rights versus individual freedom.[67] What becomes strikingly apparent is his corrosive need to substitute his theoretical framework for that of perfect competition. At times his egotism, ambition and thin-skinned frustration at those who resisted boiled over.

> Monopoly, then, and its cousins, are descriptive of reality, but not a part of "economic theory." It is now clear why "theory" is to Professor Knight virtually a finished subject—any new development (and monopolistic competition is only one example) which is not a part of the theory of a perfectly competitive market simply is not a part of the subject! (Chamberlin 1946:142)[68]

Chamberlin's policy compulsions seem less apparent, not so easily discernible. He does not appear to equate a generalized recognition of economic power as creating anything resembling an incipient problem within the market mechanism. Although his work does seem to actively motivate subsequent discussions along this line. However, a good deal of this effect is built upon the inevitable misreading of that work and the period in which the theory was introduced. The concurrent 1930s depression provided his work with an undeserved and certainly unin-

[67] Shackle (1967) provides an insightful analysis of the debates informing economics in the 1920s and 1930s. To a significant degree, the discussion was geared to effectively remove what seemed at the time to be the pernicious influence of Marshall (or at least Marshall as reconstituted by Pigou). The disillusionment with markets, intensified by the economic collapse of the 1930s, would make the gradualism championed by Marshall unappealing. While the move by the younger generation of economists, writing in the newly established *Review of Economic Studies,* toward precision and rigor, would largely leave Marshall's story-telling techniques abandoned in a slough of incomprehension. Marshall's own self-purported progeny would fail to grasp his punch line.

[68] Exclamation points in academic discussions usually denote exasperation rather than any attempt to carefully listen to and evaluate another's position.

tended glaze. Some of these potential limitations of the theory would at least partially be recognized during the post-war period.

> There is a strong undercurrent of dissatisfaction with all received monopoly and imperfect competition doctrine. Its assumption of short-term profit maximization on marginal principles seems unrealistic, its results are not always identical with those observed in the market place, it leaves a number of important cases "indeterminate" without any solutions whatever, and it fails to provide administrators with any clear criterion for deciding what is a monopoly or monopolistic practice and what is not. (Bronfenbrenner 1950: 491)

Curiously Chamberlin, who seemed to be specifically addressing the second half of Sraffa's 1926 paper (focused on marketing), had nearly completed his original dissertation at the time of Sraffa's publication. His work concentrated on the demand side of the problem as opposed to the niceties of cost curves. Unlike Robinson, he was not intent on proving the inefficiency of the market system. Robinson, concerning herself with disproving Pigou while undermining the marginal productivity theory of wages, resolved Sraffa's conundrum by simply commanding her demand curves to slope downward.

> Romney Robinson says that Chamberlin was writing about a situation in which there was diverse demand and an important point here is that Chamberlin started with differentiated demand, while Joan Robinson starts with the problems with economies of scale and there is no theory whatever in Joan Robinson's book about why the demand curve should fall. It has to fall because this is the only way you can get equilibrium so you do not argue about it. Well, of course, having made them fall, that is the way you get an equilibrium, and then you discover that the equilibrium is inefficient. Then you say, well, the inefficiency must be caused because consumers do not understand their preferences properly or something. The preferences have no particular validity and therefore any reorganisation of industry which violates these demand curves is perfectly justifiable. But of course, Chamberlin said that these preferences do have validity and any adjustment generates welfare losses; at least, there is no presumption that the change will be an improvement. (Loasby interviewed by Foss 1997:4)

Robinson's analysis remained essentially and unalterably conventional despite her rebellious language. Unlike Chamberlin, her work provided the promise of upheaval without in fact delivering any revolutionary results or implications. Neither Stigler nor the other Chicago stalwarts could discern any compelling reason to take issue with her work. They kept their powder dry for Chamberlin. (Stigler would follow a career-long regimen of reserving his deadly scorn for those theories that seriously threatened his own market approach.) Thus it was the politically conservative Chamberlin's work, rather than the much more radical Robinson's efforts that would set off alarms and draw prolonged, unfriendly fire.[69]

George Stigler's Long Struggle of Escape[70]

We few, we happy few, we band of brothers
(Shakespeare, *Henry V* (1599) act 4, scene 3, line 35)

[69] This reality causes no end of confusion since the obvious target was simply dismissed. If anything, Chamberlin was an extremely conservative economist who imagined that he provided a more realistic and convincing defense of market efficiency. Robinson, in the second edition of her work, admitted that she did have a policy agenda buried in her bag of tools. Her work attacked market income distribution by showing that labor failed to receive its marginal product. By Pigou's definition this outcome encompassed the idea of exploitation. The clearly radical Robinson attempted to justify income redistribution, while the conservative Chamberlin used the same approach to justify the market status quo. In fact he buttressed market exchange by providing what he believed to be a more realistic theoretical structure. Despite this surface smokescreen, Chicago had no difficulty in identifying Chamberlin's approach as posing the real danger. The Chicago economists did have an uncanny ability to sniff out heresy and potential threats to orthodox price theory. The absence of any intrinsic equilibrium in Chamberlin's work lies embedded in the very nature of his constructed markets.

> In Chamberlin's story, firms have problems of finding out where their customers are and it is actually what is called a process story: firms are trying out different product combinations, testing them. Chamberlin's selling costs are part of the problem of finding out where the customers are and letting customers know what one now has to offer. Nowadays, one can see that Chamberlain's monopolistic competition theory is a link between Marshall's theory of external organisation and Coase's theory of transactions costs. However, that of course is not the way it happened; it is a rational reconstruction. (Loasby interviewed by Foss 1997:4–5)

[70] This heading, quite naturally, refers to Keynes' famous remark in the preface to the *General Theory*.

> The composition of this book has been for the author a long struggle of escape, and so must the reading of it be for most readers if the author's assault upon them is to be successful,—a struggle of escape from habitual modes of thought and expression. The ideas which are here expressed so laboriously are extremely simple and should be obvious. The difficulty lies, not

Frank Knight had a profound influence on his students at Chicago and George Stigler, a graduate student from 1933 to 1936, was hardly

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in the new ideas, but in escaping from the old ones, which ramify, for those brought up as most of us have been, into every corner of our minds. (Keynes 1973:xxiii)

Whether Keynes escaped the dominating figure of Marshall is an unanswered question. However, it is certainly arguable that he still clung to many of Marshall's exposition methods, whatever their perceived weaknesses. Keynes continued to try to describe dynamic changes using an essentially static framework as a story-telling device. However, in many ways, George Stigler was more successful in breaking away from his own initial infatuations with Frank Knight and Henry Simons.

> George was a pretty self-confident person, also. At that stage he was, as many people were at the University of Chicago, quite besotted with Frank Knight. George's thesis topic was Carl Menger, the father of the mathematician Karl Menger with a 'K'. I remember a sentence he said. He said, 'Carl Menger is very good, but everything good that is in him is already, (I can't say already) in Frank Knight.' Frank Knight's influence on the student body was profound and not, I say in retrospect, a hundred per cent positively constructive. (Conversation with Paul Samuelson, October 1997)

In the post-war period, as Stigler developed his own academic identity, he came to throw off much of what Knight represented, including his theoretical standpoints and approaches to economics. (He largely also detached himself from the influence of Knight's student Henry Simons.) Where Knight conceived of economics as a science in terms of a highly limited arena, Stigler saw before him unlimited vistas to be explored and conquered.

> He still thought Knight's work was important, but nowhere nearly as important as he did when he was an undergraduate and a young person. And contrariwise, his assessment of Viner went up and that's the same for Henry Simons. If you look at parts of the *Program for Laissez Faire*, it doesn't look that interesting at this stage, so he went down on that. He changed a lot of his evaluations of that type. But the big changes, I think, were his assessment of government, his application of economics to politics, things like that. And of course he became more empirical. (Conversation with Gary Becker, October 1997)

He did retain Knight's pointed skepticism, which was the trademark of Chicago in the 1930s. But Stigler's brand was one-sided. He would turn it on opposing theories with ease, but proved almost inherently incapable of turning that disbelieving stare inward. Unlike Knight, he saw himself as the point man in an academic crusade where self-examination or doubt signalled a potential weakness.

> But the public persona was unequivocal. And it was this public persona which was very much responsible in some sense for his image. He was a real leader. Have you talked to Harold Demsetz or folks like that? Harold would tell you that. You just dreaded the warning look … that dirty look from George. That is the way he led. (Conversation with Sam Peltzman, October 1997)

Stigler did quite naturally retain a respect for Knight, the sort of sustained academic link between one-time supervisor and aspiring apprentice.

> But what struck me about the lunch was that, in some ways, whenever I met George Stigler I regressed to infantilism. It's a function of your fear of your PhD supervisor. "Got to be on my best behavior. Hope he approves of what I'm doing. Hope he feels that I'm doing okay." He was exactly the same with Frank Knight. He truly treated Frank Knight as if he was his father. I'd never seen him do this to any other person, this total deference. (Conversation with Mark Blaug, April 1998)

Though Knight did acknowledge the continuing assistance that his former pupil, George Stigler, provided him in his later years, Knight's evaluation of Stigler remains equivocal. There are claims, at least, by one of Knight's former students that Knight did not rate Stigler that highly.

an exception. "The teacher who greatly influenced me and most of my fellow students was Frank Knight" (Stigler 1988a:16).[71] He was the very definition of cantankerous and imbued in his students not only an abiding fascination with economics (something he was to lose in his later years) but an unshakeable skepticism that caused them to scour everything with an inevitable glaze of suspicion.[72] "He was more hesitant about how far you could go on everything, quantitative or not" (conversation with Milton Friedman, August 1997). He also harbored a life-long obsession with religion or more succinctly a repulsion for all things religious. Knight embodied the very essence of a crotchety academic who delighted in skirmishing with anyone of whom he disapproved and that was a large and fairly select contingent.[73]

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[71] The sort of besotted reaction common to a number of his students was hardly due to commanding personal charms or the enthrallment provided by a great story teller.

> Who could forget Frank Knight, a little dumpy figure in a workman's cap when he first gave a guest lecture to us University of Chicago sophomores? His squeaky voice emitted a mixture of Will Rogers' profundities and Ludwig Wittgenstein one-liners. Anyone so un-understandable you knew had to be a deep thinker. (Samuelson 2011a:925)

Yet the force of his influence, though never completely removed, seemed to inevitably fade with time.

> Indeed Frank Knight was the irresistible Pied Piper. For five years—from the time I was sixteen until I was twenty-one—I was bewitched by Knight. The cream of the graduate school—a Stigler, Friedman, Wallis, Homer Jones, or Hart—downplayed the Vinerian sagacity and erudition. (Samuelson 2011b:590)

[72] Perhaps this came from his philosophical turn of mind. Much of his most interesting work could be almost categorized as economic philosophy rather than economic theory. (He never did, nor seemed particularly impressed, by strictly empirical work.) For Knight, a career in economics was a choice somewhat akin to the second voyage described by Plato's Socrates in the *Phaedo*. He arrived at Cornell with the ambition of achieving a doctorate in philosophy, only to be subsequently urged to shift his objectives. He found a home in economics where he located a comfortable roost under the care of Allyn Young.

> … I learned that Knight began at the Cornell Graduate School in philosophy. But he talked too much. When the exasperated chairman gave Knight the choice of talking less or leaving the field of philosophy, legend has it that he levitated down into economics. Luckily he did not have to drop one notch further for it was Knight who used to say: Sociology is the science of talk; and there is only one law in sociology—Bad talk drives out good! (Samuelson 2011a:925)

[73] Most people have filters which allow them to consider the impact of what they are thinking of saying before it escapes the environs of their mouth. But much like his star pupil, George Stigler, Knight would write and make verbal comments that could be unnecessarily wounding or possibly devastating to the target. Both seemed to be perennially surprised when the inevitable offence was taken. What they saw as honesty easily, though quite unintentionally, often lapsed into the unwanted territory of self-indulgence.

> Frank Knight was conservative. His prime characteristic was that he was a flaming atheist and he just couldn't leave the subject alone. He was an iconoclast, but he was also very critical of simple conservatism. His views were complicated … Knight notably combined an extreme skepticism with a deep repugnance for all religion. (Conversation with Paul Samuelson, November 1997)

This bedrock of skepticism, of forever believing that something wasn't true and only surrendering when bludgeoned with facts, or not even then, was something that Knight transmitted to his most sincere admirers. Given what seems to be a natural caustic wit, George Stigler not only closely adopted this stance, at least toward opposing theories, but maintained it with a white-hot intensity throughout his long academic career. Stigler himself would eulogize Knight by characterizing him in this fashion.

> Knight was a man of formidable character as well as intelligence. He was fiercely independent and insisted upon a critical and searching examination of all matters intellectual. I suspect that he approached even the multiplication table with initial scepticism. One episode of his youth has been reported by one of his brothers. Frank Knight was the oldest child of a large family of deeply religious parents who lived on a farm in southern Illinois. When Frank was about 14, on one Sunday the children were all asked to sign pledges at church that they would attend church the remainder of their lives. On returning home, Frank assembled the children behind the barn and asked them to destroy the pledges. "Burn these things because pledges and promises made under duress are not binding." (Stigler 1991:1).

Being a father of a large brood, I am an expert on children's quarrels: it is absolutely pointless to try to determine who was first at fault. Let me only say that Jacob Viner, never known for his diplomacy or sweetness, was something of a saint in getting along with Knight. (Only in his eighth decade did Viner permit himself to say privately: "I always felt we should have treated Frank as if he were on the verge of a nervous breakdown in the 1930s. His financial problems and concerns about the disintegrating world economy and society were an important part of the picture.") (Samuelson 2011b:591)

As one of Knight's few PhD students,[74] the influence exerted by Knight inescapably shines most clearly in Stigler's completed dissertation, as it does in much of his early articles and the poses he tended to strike.[75] Reading some of these early pieces, their similarity to the type of statements and the criticisms that Knight would feel compelled to make if placed in the same position is unmistakable. Stigler's withering attack on the new welfare economics of the 1930s in which its proponents claimed "that many policies can be shown (to other economists?) to be good or bad without entering a dangerous quagmire of value judgements" (Stigler 1943:355) is met with Knightian scorn. Like his teacher, Stigler adopts a recognizable pose of skeptical liberalism, which refused to allow a theoretical potential to measure or to dictate either policies or objectives. As Stigler found his own feet and

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[74] James Buchanan arrived at Chicago in the decade following George Stigler and soon came under Frank Knight's spell, just as Stigler had before him. Very few graduate students managed to complete a dissertation with the ever irascible Knight. ("Knight had very few PhD students. I think there were only three or so." (Conversation with Milton Friedman August 1997).) Only the confident and the tough survived. But both Stigler and Buchanan went on to achieve Nobel prizes. For a very select few, Knight provided just the sort of tutelage required.

> Buchanan entered the University of Chicago's graduate economics program as a "libertarian socialist." After six weeks of taking Frank Knight's course in price theory, recalls Buchanan, he had been converted into a zealous free marketer. (Econlib 2002)

Curiously, in some ways Stigler, consciously or unconsciously, modelled himself, when acting as a dissertation advisor, on his own mentor. (Youthful habits die hard.) Again, only the most self-confident and self-motivated students could thrive under his tuition. His evaluation and treatment of Thomas Sowell could politely be labelled caustic.

> You know students can build things up in their heads. What he [Stigler] was, he was aloof. He was non-directive in an era where you had very powerful people like Milton Friedman and Al Harberger who were the opposite. In the case of Friedman, this was the era of the money demand function. If you were a graduate student in money, you did a demand for money study, you did it in a way that Friedman pioneered and you came up the results. (Conversation with Sam Peltzman, November 1997)

[75] If we roughly divide Stigler's academic output into pre- and post-war periods, one noticeable difference is that much of his earlier work can be largely assigned to those historical dustbins labelled history of economic thought. He would later neither refer to nor build on any of that more youthful output, exactly those papers where Knight's influence is most evident.

> On the other hand, if you read his first book, *History of Production.* He has a lot of highly critical things to say about Marshall there. But then he says highly critical things throughout that book. I once spoke to him about that book and said something like that to him and he indicated he hadn't read it for a long time and didn't propose to read it in future. That was something he had done in the past. I think he questioned whether parts of it were right. (Conversation with Ronald Coase, November 1997)

danced increasingly away from Knight, some of the positions on display in 1943 would be completely turned around. However, the young George Stigler was still to a large degree in thrall to Frank Knight. Attacking the logic of the New Welfare Economics, Stigler contends that "it would outrage our moral sensibilities to pay voluntary tribute to thieves. The maximization of real national income is not the only end of a society. Is it even the first?" (Stigler 1943:356) "Compare the bizarre approach of the new welfare economics: it would be possible to recompense manufacturers for not degrading the quality of their products" (Stigler 1943:357). The older George Stigler would not have uttered a similar value judgement. One of the implications of Coase's contractual viewpoint (and Stigler's reconceptualization) is that paying such voluntary tribute might prove to be a more efficient alternative than regulatory fines or criminal proceedings.

We can easily anticipate Knight sharing his pupil's scorn for the arrogance of these young economists who so blithely proposed to regulate policy by means of their theoretical constructs. For Knight, this would demonstrate the absurdity of trying to overreach the bounds of the discipline's capabilities. But by the time that Stigler joined with his colleague Gary Becker in writing "De Gustibus Non Est Disputandum" (1977), imagination would have to stretch to its farthest reaches to associate Stigler with his earlier, Knight-inspired pronouncements.[76]

> The familiar admonition not to argue over differences in tastes leads not only to dull conversations but also to bad sociology. It is one thing to recognize that we cannot *prove*, by the usual tests of adequacy of proof, the superiority of honesty over deceit or the desirability of a more equal income distribution. [Yes, Stigler here is clearly establishing the desirability of having a more equal income distribution.] But it is quite another thing to conclude that therefore ends of good policy are beyond the realm of scientific discussion. (Stigler 1943: 357)

[76] Emmett (2006) presents a convincing detailing of this break by imagining just what Knight's response to the Becker and Stigler (1977) analysis might be. It would be equally interesting to have the young George Stigler argue with his older counterpart.

Certainly the young George Stigler is still finding his feet and would be expected to fall back on the ideas of those he respected, on their more familiar thoughts. Thus when faced with writing a review of Lerner's *Economics of Control*, he, like his close friend Milton Friedman, would channel the voice of Frank Knight and throw up reasonable but predictable arguments against this bit of heterodoxy. They are still largely classical liberals in the Knightian mould, but during this post-war period are definitely gaining direction and confidence in their work. It is this new focus and its associated objectives that would move them both persistently away from thinking within a strictly Knightian perspective, as they took on what would become a distinctive, and irreducibly, ideological tone. Abandoning Knight necessitated an almost simultaneous loss of those key elements determining the methodology that distinguished the liberalism of classical economists. This shift implied forgoing those liberal precepts that advised economists to always remember the limitations of economic analysis and never to jump from theory to policy.[77] Thus, when reviewing Abba Lerner's (1944) breakthrough effort, Stigler's criticism is predictable and still heavily overlaid with the distinct influence of Knight.

> Once an economist enters the terrain of policy he must be detailed and comprehensive in his analyses and proposals: it is essentially irresponsible to jump from a textbook on theory to Capitol Hill. There is more than a

[77] Perhaps this break in methodology partially explains the very different trajectories of their careers. Knight became increasingly disillusioned, questioning the ability of economic analysis to provide even a modicum of useful insight. Stigler, however, seemed to conduct a love affair with the discipline that only gathered heat as he aged. He dreamed of a theory that would conquer all previously self-imposed boundaries, metaphorically one theory to rule them all. His support for Becker's social science projects, then, should come as no surprise. Neither Stigler's quest for a universal economics nor his championing of quantification would be likely to win Knight's applause.

> If economic theory is interpreted as a critique of the competitive system of organization, its first and most general problem is that of determining whether the fundamental tendencies of free contractual relations under competitive control lead to the maximum production of value as measured in price terms. The problems of the validity of the price measure of "real value," and of the distribution of the value produced, are larger but subsequent problems, and belong to ethics as much as to economics; while the detailed comparison of the theoretical tendencies of perfect competition with the facts of any actual competitive society lie in the field of applied economics rather than that of theory …Professor Pigou's logic in regard to the roads is, as logic, quite unexceptionable. Its weakness is one frequently met with in economic theorizing, namely that the assumptions diverge in essential respects from the facts of real economic situations. (Knight 1953: 161, 163)

> lawyer between classroom arithmetic and legislation, and more than a "manager" between legislation and the achievement of the ends listed in its preamble. A non-political treatise on economic welfare is a Hamlet with only one gravedigger. (Stigler 1945:115)

The classical tradition in economics, which chose to take a more skeptical view of markets as well as government, meant that income distributions could not become sanctified simply by being the end result of some market process. Neither Mill nor Knight assumed that economic theory had much to say directly about distributional matters. Stigler, whose focus on distributional matters predates his dissertation and even his arrival at Chicago, would ultimately find himself at odds with this classical approach, one that acknowledged the ineradicable ambiguity attached to distributional issues.[78]

> It is not so with the Distribution of Wealth. That is a matter of human institution, solely. The things once there, mankind, individually or collec-

[78] "George had had a year of graduate work before he came to Chicago" (Conversation with Milton Friedman August 1997). Actually Stigler completed an MBA at Northwestern (1931–1932). In the following academic year (1932–1933) he returned to the University of Washington before turning up, ready to commit to serious graduate work at Chicago. It was while at Northwestern that Stigler first considered a future as an academic. His lifelong romance with History of Thought seems to have begun there. During his year at Northwestern, he submitted two lengthy surveys to one Professor Deibler, both focused on the historical development of economic ideas. The first looked at "Theories of Value from Adam Smith to Stanley Jevons." However, it is his second effort that signals the start of what would be an abiding interest by Stigler in matters distributional. The second, running to some 92 pages in length, is simply titled "A Theory of Distribution." Stigler entered Chicago, then, not only with previous graduate experience, but with an interest that would quickly mature into a history of thought dissertation under the formidable and cantankerous Frank Knight. The same work would evolve into his first published book (1941). Ironically, Stigler would be instrumental, at least according to some accounts, in removing History of Thought from the graduate requirements at Chicago in 1973.

> Now, I don't know if he was a *leader,* but he was certainly in *favor* of it. I don't know how that issue first came up. I think it came up because most young Economists had lost interest in it and it was just a big tax on everyone's energy. No-one was working on it. It wasn't really much of a research field. I think it's had a slight come-back but it's still pretty small. Even now, you see very few PhDs with that particular interest. There's also been a general *laissez-faire* attitude around here about graduate degree requirements. So I think that's how it came up for discussion. In the same way, there was an Economic History requirement which I don't think we have anymore either. That was also dropped. So I don't think he was in favor of putting artificial [laughs] restrictions on a degree. That was probably his motivation. But I don't know. I don't think *any* of us were too happy about that move. (Conversation with Sherwin Rosen, October 1997)

> tively, can do with them as they like. They can place them at the disposal of whomsoever they please, and on whatever terms. Further, in the social state, in every state except total solitude, any disposal whatever of them can only take place by the general consent of society. Even what a person has produced by his individual toil, unaided by any one, he cannot keep unless it is by the will of society that he should … The distribution of wealth, therefore, depends on the laws and customs of society. (Mill quoted in Stigler 1988c:7)[79]
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> And the task of separating the portion of product or capacity to produce which is due to conscientious effort from that which goes back to inherited advantage or pure luck is about as impossible—and the evil results of making a false separation perhaps about as great—in one case as in the other … The only conclusion as to social policy which we shall insert here is the insistence that "society" must get rid of the idea that because income is "earned" it is "deserved" and not otherwise. (Knight 1921: 128–129)[80]

Stigler's impression of monopolistic competition would then have been heavily influenced by Frank Knight, even though by the time he came to deliver his dagger thrust to the heart of the theory during a series of invited LSE lectures (1948), his agenda was clearly evolving and changing. Still, Stigler's initial brush with Chamberlin's breakthrough 1933 opus came during his first year as a graduate student at Chicago. Those were the years in which Knight's influence held sway. According to his compatriot and fellow student Allan Wallis, the theory was not discussed at all in class. Instead, a select group of students (including Stigler, Friedman and Wallis) met together independently to try and decode this revolutionary new approach. The appropriate year would need to be

[79] Stigler quite expectedly objects to Mill's logic since it largely substantiates the legitimacy of government intervention. If market distribution lacks an unarguable foundation, then redistribution is at least a distinct and possible alternative. Stigler's rejoinder to Mill in this case lacks focus. Mill is not saying that society's ultimate decision can be automatically justified or that it is even preferable, but is offering an observation rather than a defense or justification.

> Conversely, no society could survive if it set the compensation of workers in proportion to the reciprocal of their marginal productivity, so the most productive workers earned the least. (Stigler 1988c:7)

[80] This coincides with Patinkin's memory of Knight's classroom stance.

> Knight viewed most of labor income as returns to "human capital" and stressed the role of the family (both genetically and socially) in endowing its children with this capital. (Patinkin 1973: 794)

during the 1934–1935 academic period, which saw all three of them attending Chicago together.

> We also studied Edward Chamberlin's then—new book on *Monopolistic Competition*. We graduate students considered that this work would revolutionize economics. The faculty, however, gave it no attention: literally none. So George and a group who resided at International House, among whom I remember especially Albert Hart and Kenneth Boulding, organized a seminar including me, that met every Saturday morning and worked through—or debated—Chamberlin's book line by line and diagram by diagram. The judgment of history, I believe, would side more nearly with the appraisal of the book by the faculty than with that of the graduate students. But it is a significant work and the exercise had more lasting value than verifying Pareto's mathematics. (Wallis 1993:776)[81]

Again, assuming that memory is accurate, graduate education did not continue to ignore Chamberlin's work. Dom Patinkin, a great admirer of Knight, remembers that by 1942 at least, when he was making his way through Knight's famous Econ 301 course, the work was covered in a thorough and conscientious manner by Knight himself. Neither Frank Knight's personal antipathy to Chamberlin, nor his skeptical stance toward this theory caused these ideas to be ostracized and banished from the curriculum.[82]

> … Knight discussed (and referred us to the relevant readings on) the theory of monopolistic competition … indeed, to an extent greater than one might infer from some of the things that have been written on the attitude of Chicago school to this theory. It is, however, true that Knight was critical of Chamberlin's concepts of imperfect competition, and denied that the possibility that an "…enterprise can have diminishing (i.e., negatively

[81] Accepting the accuracy of memory here, such a close reading would certainly prime Stigler for his subsequent attack at the London School of Economics.

[82] These two memories are not necessarily contradictory. Knight certainly never shied from disembowelling theories he rejected. Ignoring such approaches would seem to be atypical of the man. However, 1934 was almost immediately after the publication of both Chamberlin's and Robinson's work. It is conceivable that any formal study of Chamberlin was deferred for a few years while the work gained traction. But certainly, Knight's basic antagonism to Chamberlin's break with tradition was bound to influence, at least to some degree, Stigler's developing attitude to the work.

> sloped) demand curve but still no monopoly profits … corresponded to fact." (Patinkin 1973:794)[83]

The exact substance that so irritated Knight about Chamberlin is difficult to pin down. Certainly there seemed to be some bad blood between the two, perhaps stemming from the period when Knight was on the faculty of the University of Iowa and Chamberlin was an undergraduate.

> Knight always said, "all that's good in Chamberlin he got from me and there isn't anything good in him." You know, something like that. And, there's no reason why this should have been of any importance, but it really riled Knight that Chamberlin was a Catholic convert. "The man believes in the Immaculate Conception. What can you do with him?" Knight would say. So from the start, of course, they didn't like the notion that if you were analysing imperfect competition, you were analysing cases of market failure. (Conversation with Paul Samuelson, November 1997)

Such explanations are at best problematic. Chamberlin stoutly objected to being labelled with the "imperfect" terminology, a categorization which more nearly reflects the objectives and ambitions of Joan Robinson. He presented his vision as an alternative starting point for economic analysis, one more closely aligned with observed reality. Markets in the absence of any interference did not tend toward a structure resembling perfect competition but rather emphatically drifted away from that direction.[84] The markets which Chamberlin illustrates neither require nor invite outside

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[83] Certainly this must be a misreading by Knight of Chamberlin's intentions. The zero-profit equilibrium referred to is not meant to reflect observation, but is instead employed more as a heuristic story-telling device.

[84] For Robinson, her label of imperfect competition conveys the theory's deviation from the ideal state of perfect competition, which Robinson allows to stay in place as something of an aspirational model. This construct represented a characterization that Chamberlin was incapable of swallowing.

> … a much greater factor than this in the wider use of "imperfect competition" is that it involves no more than an explicit recognition that actual competition is imperfect, which anyone would always have admitted anyway. The term is purely negative. Competition and monopoly go their ways without the least overlapping, and interference with one's categories of thought is held at a minimum. Thus "imperfect competition" has undoubtedly contributed and will contribute a great deal to perpetuating competition and monopoly as mutually exclusive categories. (Chamberlin 1937:572–573)

intervention of any kind. Ironically, in a world best described by monopolistic competition, only the heavy hand of meticulous government regulation could shove an economy in a direction resembling the atomistic structure of perfect competition. Product differentiation and large-scale enterprises would seem instead to be the natural result of competition. If anything, Knight in an early paper (1921) seemed to be veering toward Chamberlin's position, barring a noticeable wobble that prevented him from arriving at that exact destination.

> One of the most serious oversights in the discussion of decreasing cost is the neglect of the mixture of competition and monopoly which is a general characteristic of the type of business supposed to exhibit this type of cost function. Just as part of the traffic of a railroad is competitive and part monopolistic, nearly every manufacturing and mercantile business has a monopoly on *some feature* of its product; its good or service is differentiated from others in some manner and to some degree … The correct approach to the explanation of price in the case of partial monopoly would seem to be to apply the theory of monopoly, not that of competition. Instead of attempting to allow for a degree of monopoly in the supply, which there is no easy way of doing, it is vastly simpler to allow for partial competition as a phenomenon of *substitution*, on the demand side. No difficulty whatever is involved in assuming control of the supply (of commodity defined in the narrowest sense) and allowing for competition by substitution of more or less similar goods in drawing the demand curve. And this is the more

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Chamberlin's key point is that competition in freely competitive markets must inevitably involve firms that operate with some mixture of monopoly, while engaging in competition with their rivals. The most *laissez-faire* of markets would never trend to the picture presented by perfect competition.

> In economic theory the identification has been with "perfect" or with "pure" competition. Yet it must be obvious that the outcome of free enterprise is most often not pure competition but monopolistic competition. Commodities do not standardize themselves, and their natural heterogeneity is vastly increased by business men under "free enterprise," in their efforts to distinguish their commodity from others and to manipulate the demand for it through advertising. In other words, an essential part of free enterprise is the attempt of every business man to build up his own monopoly, extending it wherever possible and defending it against the attempts of others to extend theirs. There is no tendency for these monopolies to be competed out of the picture: on the contrary, they are as much a part of it as is the competition which restrains them. (Chamberlin 1937:576–577)

realistic view as it represents the way in which the producer would naturally envisage the situation. (Knight 1935: 213)[85]

A real bone of contention between Knight and Chamberlin, personal differences aside, would continue to be the relevance of perfect competition as a productive model. For Knight it formed the only viable, though highly limited, basis for theoretical analysis. For Chamberlin it remained a device for generating confusion within the profession. The justification for maintaining such a spurious story seemed largely unsustainable from his perspective. These distinctly different standpoints created what was essentially an unresolvable clash in their respective visions. Stigler would pick up Knight's defense of the atomistic approach, but ultimately for reasons differing sharply from his old teacher. Not only would all such attempts at such heresy be repelled, but the theory itself would be extended to cover ever-widening applications, territory which Knight himself had always scrupulously avoided as being beyond the bounds of economic theory.

Upon examination, a lack of communication eventually developed into the hallmark of exchanges between Knight and his former student Edward

[85] Stigler preferred instead to transform this into an either/or process. Whether one treated a market as competitive, or as a monopoly, simply depended on the issue in question. Unfortunately, he fails to depict the basis for choosing one framework over the other. By leaving his conclusion open-ended, Stigler fuels the cynical suspicion that the choice depended largely on the desired result.

> Should monopoly or competition be used to analyse the New York housing market? The answer is: both. If we are interested in the effects of rent ceilings and inflation, the theory of competition provides informative predictions. If we are interested in why one location rents for more than another, the theory of monopoly may be an informative guide. Different theories, each with its particular assumptions, can be applied to the same phenomena to answer different questions. (Stigler 1949a:23)

This approach seems too heavily redolent of the Ptolemaic quest (in astronomy) to save appearances by concocting a convenient theory congruent with a perceived reality. Achieving the nominated quest requires only a clever generation of sufficient epicycles to explain the movement of any heavenly body. Whatever appears to be an anomaly can be deftly theorized out of existence. (Notice the affinity of this approach, with the methodology insisted upon and propagated by Friedman (1953) after a series of discussions with his close friend George Stigler.) Certainly Stigler would have been familiar with the possible direction Knight had signalled in his 1921 paper. Since the same article appears in *The Ethics of Competition* (1935), a collection of Knight's papers edited by Friedman, Wallis, Jones and Stigler, he could have hardly been unaware of the ideas Knight expressed there. His clear preference however, was to salvage the structure provided by perfect competition and to extend its application in an inexorable fashion.

Chamberlin. The extent of any subsequent influence on George Stigler's formulations is muddled at best. Knight's analysis and objectives can be clearly differentiated from those of Stigler. In less than three years after an address by Knight at the annual December meetings of the American Economic Association (1945), Stigler appeared before the notables comprising his audience at the London School of Economics (1948). Knight's presentation was met by an immediate and somewhat snarky and surprisingly thin-skinned response from a seriously short-tempered Chamberlin. Had Chamberlin been in turn present for Stigler's British performance, the subsequent attempt to contain his fury might have resulted in apoplexy, rather than in anything resembling a cogent reply. Putting aside Chamberlin's spluttering reactions for the moment, Knight's (1946) thoughts on theory ("Immutable Law in Economics: Its Reality and Limitations") clarify the nature of Stigler's incipient break with his teacher. The discernible difference becomes evident with a comparison of Knight's thoughts on economics to Stigler's (1949a) lecture on monopolistic competition.

> All general laws are necessarily "abstract" and empirically more or less inaccurate. The axioms and propositions of geometry are in all degrees "untrue" of actual figures; and addition and other operations of arithmetic are empirically invalid, since we never deal with perfectly homogeneous magnitudes: counting must ignore differences, and measurement is never exact. (Knight 1946:94)

For Knight, human subjectivity must inherently limit the applicability, especially the direct applicability, with which theory could hope to comprehend observed events.

> In view of unconscious purposiveness, we cannot consider here the question whether economic law is universal in the sense that all human behavior has the economic quality or aspect, or whether this is true of all deliberate action, or precisely what is the universality of economic law. (Knight 1946:95)

What perfect competition offers, as a salve to any aspiring economist's ambition, is an idealized market, a sort of benchmark against which to compare and measure observations. The theory lacks the presumption of representing anything paralleling a descriptive reality. Or in another sense, its allure lies in a serviceable ability to propose conditions underlying optimal market efficiency.

> The "perfect" market, or theory at its highest level of generality, is conventionally described as perfectly or purely "competitive." But use of this word is one of our worst misfortunes of terminology. There is no presumption of psychological competition, emulation, or rivalry, and this is rather contrary to the definition of economic behavior. Market relations are impersonal, between persons and goods; and persuasion or "bargaining" is also excluded. … The meaning of "competition" is that they are numerous and act individually;' "atomistic" is a better word for the idea. (Knight 1946:102)

This must imply that given pure theory we are back to Clapham's (1922) empty boxes. We have units of production and consumption as the operative counterparts to buyers and sellers. Such decision-making atoms are devoid of any internal working parts or complexities. Perfect competition remains content with defining these key players only by their assigned objective goals, which they strive to maximize.

> Under the idealized conditions of pure theory, the internal organization of enterprises is equivalent to exchange between individuals in a perfect market, and may be ignored. The ideal enterprise might be more or less approximated through some form of (democratic) producers' cooperation; in the typical real case, initiative and direction and responsibility for the result (risk-taking or uncertainty-bearing) are more or less concentrated in the hands of an "entrepreneur." (Knight 1946:103)

For Knight, the need to abstract in order to generalize must move theory away from any accurate vision of economic activity. In his opinion the resulting gulf is a necessary trademark encountered when exploring purely theoretical territory.

> Economic laws describe what men try to do rather than what they actually do: and even the former statement must be largely qualified because of the limited reality of given ends—to be discussed later. The ideal or limiting

> case of pure economic theory is impossible, self-contradictory; if behaviour is perfectly economic it is not economic, but a purely mechanical response … As pointed out above, if we picture economic activity purged of its problem-solving character, hence of error and uncertainty, either by reduction to an unvarying routine or through giving the subjects omniscient foresight, it will be an unconscious process and not economic, or only so in the sense in which we apply the term to plant life. (Knight 1946:105–106)

Knight allows for the sort of 'moving equilibrium' (1946: 106) also mentioned some eighteen years earlier by Allyn Young. He readily agrees (as who would not) that monopolistic competition provides a more realistic view of the economy. The question is whether it constitutes a theoretical advancement as well.

> On the map of real productive organization a typical situation is "monopolistic competition," a combination of a small degree of monopoly with oligopoly in what may be called either an "industry" or "a group of industries"; they may show any distribution of profit and loss among the units. (Knight 1946: 104)

In his presentation, Knight is attempting to formulate his own understanding, or the version of classical economic methodology to which he subscribes. For immutable law (that which must be true) we can read theory. These are propositions that remain unquestioned simply given their assumptions and internal logic, much as propositions in Euclidean geometry are in this sense indisputable. For Knight, such laws form the starting point of analysis. But his exploratory investigation is equally devoted to questions of reality and the limitations inherent when applying such immutable laws. These constraints turn out to be quite severe, again consistent with his classically tinged approach. There is naturally room to argue, since Knight doesn't spend much time investigating why irreducible precepts such as these form the basis of economic analysis. Such reasons have to be inferred. This approach to economics need not be portrayed as canonical and irrefutable. Room for a possibly more productive alternative is not arbitrarily rejected. Knight certainly does not seem to be aiming any serious criticism, scathing or otherwise, at those who choose a different path. He may have such objections tucked in his ammo bag, but he resolutely keeps his powder dry at this particular session. In contrast, Chamberlin in his invited reply comes across as particularly cross and ill-tempered, transforming Knight's thoughts into a personal disquisition focused on the sustainable relevance of Professor Knight.

> But I must state bluntly what seems appallingly evident to me, that in awareness of the theoretical problems of this field, Professor Knight has not, in his own words, reached "the point where real discussion should begin." Witness his dismissal of them as mainly due to imperfect knowledge! And the reason is not difficult to find. He is interested in "economic theory," and these matters are not a part of the subject. This will seem strange to many, but apparently there is nothing to be done about it. To Professor Knight, that is just the way "theory" is. (Chamberlin 1946:142)

Unbidden, and seemingly unprovoked, Chamberlin had thrown down the gauntlet to Chicago, or at least a select part of it.[86] Notice Chamberlin's emphatic mention of information, which does feature in Knight's presentation, as well as his classic (1921) volume (*Risk, Uncertainty and Profit*). In this seminal work (or at least a work that should have been seminal) Knight depicts the role information plays as a determining factor forming the market mechanism. In a somewhat more domesticated version, information will also form a keystone for Stigler's evolving alternative approach to both Knight and Chamberlin. But this gets ahead of the story. At this point in the drama, George Stigler is still warming up for

[86] Reference is sometimes made to a Chicago School that thrived during the interwar years and another embodiment that defined the post-war years with clear links to the first. However, talking about the first Chicago School is probably misleading, since it conveys the impression of more uniformity than was ever actually present. By contrast, comparing this nominated first conclave with the Chicago department that dominated the 1960s and even 1970s (built around the Friedman/Stigler axis) makes the difference evident. To avoid unnecessary confusion it may be best to simply limit the label "Chicago School" to the stalwarts of the post-war years.

> The University of Chicago economics department, like Gaul, was divided into parts. Knight and Viner were the theorist patriarchs and rivals. Paul Douglas was the more-than-token liberal. Henry Schultz represented the wave of the future in econometrics and mathematical economics. Henry Simons, critic of the regulated state and advocate of redistributive income taxation, was in Knight's camp. Although Aaron Director began in the Douglas workshop, his heart was with Knight. (Samuelson 2011b: 590)

his ultimate match with Chamberlin. First however, like any trained prize fighter, Stigler will have to prepare for his post-war showdown by appearing in a few more minor tussles (prelims) at the lower end of the fight card. Only then can Stigler be confident of overwhelming Chamberlin with his unleashed firepower (the equivalent of an Ali-like rope-a-dope strategy).

IV. Skirmishes

> War never breaks out wholly unexpectedly, nor can it be spread instantaneously. Each side can therefore gauge the other to a large extent by what he is and does, instead of judging him by what he, strictly speaking, ought to be or do. (Clausewitz quoted in Paret 1976:358)

The young George Stigler's initial recorded reaction to monopolistic or imperfect competition provides some subtle (and not so subtle) hints of future, more ferocious campaigns. The issues surrounding these alternative frameworks provided the leading focal point for micro-economic debates in the 1930s. So it is hardly surprising that a young, ambitious academic, freshly arrived at his first posting—Iowa State College (now University)—should take a stab at such a hot topic.[87] It was certainly

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[87] From his very start as an academic, and continuing throughout his career, Stigler deliberately eschewed macroeconomic issues. There are only a few (maybe four) of his papers that can be construed as falling into that classification and these were largely the result of outside requests rather than motivated by his own intrinsic interests.

> If you look at my catalogue of the Stigler papers you will see that the number of papers on macro is four or five, practically nothing. In fact there may be only one or two if you only count the ones that George sort of spontaneously decided to work on. The two or three papers in there are usually because somebody asked him to come to a conference on interest rates, you know, something like that, or international trade. I threw together macro and international because there was practically nothing on the topic. I mean, he was *really* a micro economist. (Conversation with Claire Friedland, October 1997)

A more pointed view makes it clear that, quite admirably, George Stigler avoided making pronouncements in areas he considered to be outside his expertise.

> I imagine when all's said and done—I don't remember George writing particularly on the real bills doctrine or the quantity theory—George would say "inflation is everywhere a monetary phenomenon." This is like taking a personality loan from people whom he admires, who believe that kind of thing. More than most, I think, George kept out of things that he felt he wasn't entitled to an opinion on. Most economists would say "How do you

a sensible way to gain notice.[88] Unsurprisingly then, the subject forms the basis for one of his earliest publications (the second listed). The importance of the article (Stigler 1937) is not so much its immediate impact on the profession, but rather as a warm up, something of an unforeseen trial balloon, for his 1948 lecture, which would focus his matured firepower on Edward Chamberlin. This 1937 paper was less focused, analysing a broader and less specific area of imperfect structures, as a deviation from perfect competition. A few character aspects of this budding academic strike the reader forcefully (though perhaps with retrospective knowledge) when examining this now largely forgotten work carefully. The obvious trait is the extent to which he is still under the influence of Knight and Simons.[89] By 1948, his more targeted effort would contain only the faintest of residues linking his thought with that of his Chicago

> spell ‘gold’?” And then they’ll tell you what we should be doing about gold or anything else that you can imagine. (Conversation with Paul Samuelson, October 1997)

In an interview conducted by *The Region* (May 1989), the official publication of the Federal Bank of Minneapolis, http://www.minneapolisfed.org/pubs/region/89-05/int895.cfm, George Stigler clearly confirms Paul Samuelson’s claim.

> Well, I’m a monetarist in the sense of believing that the control over some money supply is important (which measure of money and over what periods, for example, are decisive questions in the control over the rate of growth of the price level). I think that the rate of growth of money is a critical variable in controlling inflation and that, for example, the massive troubles we are having with the savings and loan industry are, in part, the product of the fanatical inflation we had at the end of the ’70s and the beginning of the ’80s. Those alone are indications of the kind of costs that are imposed upon a society. It wouldn’t be too bad maybe if you went completely crazy, like the South American countries, and let inflation go on and everybody indexes on some more stable currency, and so forth and so on. But we aren’t going to do that. We’re going to put all kinds of strange regulations in: we won’t let this go up, and we’ll let this price go up. They cause immense distortions in an age of inflation. That’s one of the great problems plaguing the Israeli economy.

[88] George Stigler took up his post at Iowa State in the fall of 1936. His paper on imperfect competition was presented at the Mid-West Economic Association meeting on April 16, 1937. The paper was then published, with slight modifications, in August 1937. The article (Stigler 1937) is more important for what it reveals about the young Stigler’s thinking than for its contribution to the ongoing debate at that time.

[89] These influences show up regularly throughout the paper and are alien to the work of the more mature Stigler. Certainly the Marshallian idea that an economist must consider a broader public when writing would have been dismissed later on as irrelevant by any self-respecting academic.

> As a corollary, the newer literature of imperfect competition is so complex that it is impossible for the legislator or the layman to glean even its major conclusions. (Stigler 1937:708)

The older Stigler would tend to dismiss or simply ignore non-economic incentives and motivation as distractions or reducible to self-interest. But Knight here is still a dominant factor in Stigler’s analysis and echoes of his objections inevitably surface.

graduate experience. However, the undeniable confidence expressed by his tone, more than the actual content of this early article, is particularly noteworthy.[90] The absolute certainty that he presented to the economics profession would form the hallmark of a lifelong campaign to effectively market his approaches and theories in an unmistakably implacable manner.[91] His self-assurance often papers over problems and inconsistencies

> For much economic behaviour is indubitably affected by non-economic factors. Love of home and neighbourhood, racial prejudice, personality, and similar factors creep into every broad economic problem. (Stigler 1937: 713)

[90] Stigler recognized the importance of selling one's theory. An effective campaign left no room for self-doubts. If they existed, they would need to be publicly suppressed and never acknowledged. There was no room for equivocation when staging a theoretical counter-revolution in the economics profession. This approach reflected an almost military type of strategy. Never provide an opening for opponents; always capture and defend the high ground in any battle. This unwillingness to take even a step backwards was also a seemingly ingrained component of his personality.

> The article was accepted and he would surely have been asked since I directly referred it to him as an editor. I never got a note from George saying 'Well, this time around I've got to admit I was wrong. And your reading was right.' Not at any time. And a lot of people would tell me that if they wrote to him complaining about something, he would answer something like 'Well, if you're the kind of person who believes that, then you're just the kind of person who believes that' (Conversation with Paul Samuelson, October 4 1997).

[91] The Chicago program acted as an immersion process analogous to completing a Marine boot camp. Either you toughened up and learned to perform under pressure, met any adversity, or you washed out. Both Knight and especially Viner performed superbly as Marine drill sergeants.

> When one victim alibied, "I am beyond my depth," Viner is supposed to have said, "Sir, you drown in shallow water." If a graduate student was refused admittance to 301, the basic course in theory, he had no choice but to drop out or to transfer to the slums of political science or sociology. (Years later when I discussed with Jack Viner the legend of his ferocity, he said that the department had given him the function of screening the candidates for higher degrees. It was not work for which he was ill-equipped.) (Samuelson 2011b:597).

The degree to which aggressiveness became a mark of Chicago is easily observed. Greater difficulty lies in determining the extent to which this was a function of self-selection or the result of surviving the pressure-cooker approach that seemingly became a distinctive characteristic of its graduate program. Other departments might cull at the entry level or later at the dissertation stage. Chicago developed a system of using its courses to accomplish any required weeding. Nonetheless, both George Stigler and his close friend Milton Friedman were exemplars of brash confidence that could be infuriating.

> In a famous Cook County debate on wage-price controls, Solow declared, "What Sydney Smith said to Thomas Babington Macaulay, I say to you, Milton": I wish, Babington, I was as sure of anything as you are of everything." (Samuelson 2011c:617)

in his argument unless it is examined scrupulously when trying to parse out the grammar of his logic.

> But in other situations he said that presenting your theories in a convincing way was very important. And I think that it was really a part of his personality that when he presented some theory he did the best job he could. And I haven't really answered your question because it is such a difficult one. But, I think that he really was essentially confident. I'm talking about intellectually confident. He was pretty sure that his theory was in good shape. If his data didn't quite support it, that was because there are inevitable problems in all data. However, he was sure that he had used pretty good data and that we had done a pretty good job in supporting, or at least not contradicting, the theory. But when he presented his results, he didn't use words like 'pretty good'. That's clear. He made it the best case he could, and that's the kind of person he was. He would never be intellectually dishonest, but when he came to writing up his results, I was always somewhat astounded that he made it sound so good. And yet he never, ever left anything out. (Conversation with Claire Friedland, November 1997)

His attention to the details of marketing his position thus starts early on in his career. The goal ultimately is to influence a debate by shifting its grounds, in effect reconceptualizing the issues and transporting them to more congenial territory. Success allows a hardened campaigner to then guide, to a certain degree, the direction of any further debate. Following this strategy often required Stigler to employ tactics more familiar in legal briefs, where every potential precedent and decision obliquely relevant to a case is dredged up on the off-chance that something will somehow stick. Thus, as in many of his subsequent critical papers, Stigler delivers a

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Certainly with Friedman there was little in the way of equivocation or any tendency to compromise his ideas.

> He [Friedman] was always patient, always polite, never got short tempered like I do in an argument, never got nasty. But he was a horrible person to argue with, just a nightmare. My idea of a nightmare is to stand on a stage and debate with him in front of the public. I watched him debating at Cambridge with Joan Robinson on flexible exchange rates. Unbelievable! I mean, Joan Robinson was one of the world's most aggressive, hostile debaters. He wiped her analytically, he wiped her rhetorically, he had the entire audience eating out of his hand after an hour, an hour and a half. An amazing, amazing guy, but a madman, a madman. One of the few people I could strangle with my bare hands. I feel I could actually do it. (Conversation with Mark Blaug, April 1998)

number of jabs from several different directions in the hope that landing some combination of these will at least grant him a technical knockout.

If anything, the article's title misleads, leaving the potential reader unprepared for the argument to follow. It is difficult to conceive how a theory can be effectively generalized by categorizing it as no more than an appendage to what Stigler clearly identifies as the universal approach that must underlie any serious attempt at economic science. The paper is in essence a defense of perfect competition. In other words, Stigler enrols himself unreservedly in the ranks of the army whose banners have been clearly unrolled by Generals Knight and Simons. Mobilizing an effective defense, however, often requires a strategic attack. The initial step in undermining a heretical challenge to orthodox beliefs is to treat alternatives to perfect competition as nothing more than a generic amalgam carelessly labelled imperfect competition. The term is firmly anchored to the standard approach by identifying it as simply a deviation from what is essentially still the core theory of economics. In which case, what is on offer is presented not as an alternative starting point for analysis, such as that offered by Chamberlin, but rather a theory that presupposes perfect competition.[92] Arriving at the end of Stigler's guided journey, the reader is left awaiting a promised generalization which has emphatically failed to materialize.

A careful examination of the article through the lens of a skeptical perspective shows Stigler apparently tipping his hand throughout his analysis. He yokes this new theory to the old by stating categorically "All students of imperfect competition have accepted, either explicitly or by their practice, the received theory of competition as the point of departure, and this is of course both desirable and inevitable" (Stigler 1937:710). Such

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[92] Some of Chamberlin's frustration at having his approach and that of Joan Robinson muddled together becomes at least somewhat understandable if the intentions of both are clearly understood. This differentiation has in part often been lost, with Chamberlin's insistence seeming to be merely a reflection of a rather testy personality. The contrasting names, if considered, do denote the very different objectives and purposes behind these two approaches.

> The theory of value was apparently in a state of crisis, and Mrs. Robinson's book appeared as the culmination of a painful and difficult struggle to resolve it, in which many of the keenest economic minds had publicly engaged. The urgency and difficulty of that struggle made impossible the perception of Chamberlin's work in any other way than as a solution to the crisis. Its entirely different origins, being neither a common concern nor well-publicised, aroused no interest whatsoever. (Loasby 1971:878)

a sweeping statement directly contradicts what Chamberlin is trying to accomplish, if we take him at his word—and even more so if we even-handedly evaluate his theory. Such a charge reduces Chamberlin to someone who is either unaware (self-delusional) or deliberately evasive. The type of all-encompassing generality Stigler commits is difficult, if not impossible, to refute because it is always possible to argue that a theory reflects a previous one even if such analysis is constructed as a deliberate alternative. Stigler here is strategically placing opponents on the back foot by putting them on the defensive.

Stigler essentially accuses such theorists of jumping the gun: “The new theory, in other words, has become something of a distracting fad. It seems often to be an escape from the very hard thinking necessary to secure a satisfactory and useful theory of perfect competition.” (Stigler 1937:708) Notice that this claim depends strongly on his assertion that a theory of imperfect competition is only possible once a clear and precise version of perfect competition is developed.[93] In other words, you cannot hope to describe departures from a system until you have properly described the system itself. But any aspiring economist who follows this path is hopelessly tying him or herself to the theory they are hoping to dislodge.

> … unfortunately a great deal more must be said about the theory of value before we enter more complicated branches of economic reasoning. We ought not to repeat the error of mathematics, which constructed a tremendous superstructure in a period of over 2000 years, and only in the last century found it necessary to retrace its steps, in order to secure rigor. (Stigler 1937:708)

Such analysis directs any self-respecting economist back to square one, thus shifting attention away from viewing the theoretical turmoil in price theory as arising from the fundamentally flawed vision of perfect com-

[93] Notice the use of the label ‘fad’ as a way of branding imperfect competition as essentially frivolous, something that will fail to make any lasting contribution to the discipline. The idea that some economists do think perfect competition to be fatally flawed as a theory enters only as a footnote. “Doubtless some part is also played by the belief that perfect competition theory is valueless” (Stigler 1937:708). The strategy is to cover all bases, but at the same time to try to demote or possibly hide in plain sight unwelcome facts or thoughts.

petition itself. Instead we are chivvied into a cosier environment where the major point of contention is whether what can be seen as deviations from this core theory of economics may simply be mistaken. If successful, such a strategy transfers the grounds of debate to more conducive territory. The accepted supposition dominating these discursive constraints insists that once the key elements of perfect competition are more precisely developed such apparent imperfections that momentarily roil academic waters may simply dissipate in the light of superior understanding. Though Stigler dates the controversy from Sraffa's 1926 paper, the year itself is somewhat arbitrary. What is essential is that though he uses Sraffa's publication to kick off his own article, he ignores Sraffa's contention that monopoly theory offers a more productive and coherent starting point then perfect competition can ever generate. (Indeed Sraffa has merely a walk-on part in Stigler's prologue, where he is briefly paraded before the assembled audience, but is subsequently unceremoniously hustled off stage.) In doing so he skims over Sraffa's contention that the standard short-run supply and demand apparatus formulation lacks any defensible foundations. To assume independently derived functions is to command the short-run marginal cost curve to increase out of theoretical necessity. In standard analysis diminishing returns to a fixed factor assures the inevitability of this convenient property. But for Sraffa, this useful but implausible leap contradicts common business sense.

> Almost any producer of such goods, if he could rely upon the market in which he sells his products being prepared to take any quantity of them from him at the current price, without any trouble on his part except that of producing them, would extend his business enormously. … Business men, who regard themselves as being subject to competitive conditions, would consider absurd the assertion that the limit to their production is to be found in the internal conditions of production in their firm, which do not permit of the production of a greater quantity without an increase in cost. (Sraffa 1953:189)

The key to understanding Stigler's objective, to grasping his paper's hidden mechanics, however, is the introduction of what will prove a recurring theme throughout his career and eventually become the basis for his Nobel recognition.[94] The theory that has its inception as a model of imperfect competition will, under the watchful guidance of George Stigler, be transformed over the intervening decades into a newly hatched economics of imperfect information. Understanding the market for information using the standard tools of analysis becomes the logical way forward. The crucial assumption of this approach insists that as information improves, any residual imperfections must in turn vanish. For Stigler it is perfect knowledge that supports the theory of competitive prices. This leap, however, bootstraps the required level of information to one resembling an almost godlike omniscience rather than the more humble modicum necessary to render an optimal decision.

Stigler's assertions, then, are often superficially plausible but confusing when examined more intensively.[95] Perfect information, according to his strictures, must automatically imply competitive, not monopoly, prices. This assertion is based on the proposition that monopoly prices, though undoubtedly of benefit to any single firm, cannot be universally generalized. If all firms set prices at monopoly levels, then all would suffer. Given the assumed level of complete information, this realization would scotch any inclinations to post monopoly prices (Stigler 1937:711). Such a conclusion would hold true if we assume representative firms and consumers. But in fact, monopolistic competition hypothesizes heterogeneous buyers and sellers. Elasticities of demand for the output of different firms would have to vary under reasonable assumptions. In that case, monop-

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[94] The reference here is to Stigler (1961).

[95] Stigler's embrace of the economic rational man seems defensible if he is saying it has proven to be a useful heuristic, though even here some level of desired precision would be absent.

> It is elementary to all scientists that certain methodological assumptions, which everyone admits are contrary to fact, are indispensable to theoretical reasoning. No one begrudges the physicist the right to ignore friction, and the mathematician is permitted perfect circles no one will ever see. The case for the economic man is just as strong, and had he not been imported into economics, today there would be no science worthy of the name. (Stigler 1937:713)

Unfortunately for Stigler's argument, the physicist can look to the heavens to observe frictionless movement and the mathematician works in a field where the real objects are mental ones, namely mathematical objects. There may be observed physical objects that resemble circles but the material world is outside the interest of the mathematician. Stigler wants to imply that any discrepancy from this ideal decision maker has at its root an informational cause. However, that understanding would seem to diverge from alternative psychological constructs which could form equally useful heuristics for any economic analysis. These cannot be dismissed *ex cathedra.*

oly profits would do so as well. Under such conditions, it is conceivable that some firms would profit by setting monopoly prices. This outcome would prevail even though welfare in general might drop. A measurable drop, however, could only be asserted if consumers displayed no marked preference for variety. But Stigler seems unable to resist presenting sweeping challenges to his reader. Problems of indivisibilities are exiled to the further regions of Siberia.

> If indivisible resources are typical or important, then long-run decreasing average costs are typical or important, and competition is impossible. The presumption to be drawn from theoretical analysis, however, is that indivisibilities are exceptional and unimportant. Professor Knight has stated the case well: "No fallacy is more pernicious with reference to intelligent economic policy than the popular illusion that large-scale business is in general more economical than small-scale." A priori the case for indivisible factors is very weak, and urgently needed empirical studies will probably make it much weaker. (Stigler 1937: 714)[96]

Essentially Stigler's punchline in his generalization of the theory of imperfect competition is that more attention needs to be paid to refining the theory of perfect competition. If there was any doubt before, in his conclusion Stigler clearly unveils the hand he's been playing. The connective link between the right theory and the benefits of a liberal economic society is one that will continue to underline much of his life's work.

> The first is methodological, and has already been given. Perfect competition theory is an invaluable weapon with which to attack more complicated economic phenomena. The second reply is that it is now established beyond reasonable doubt that most of the theory of competition would be applicable in the type of totalitarian state which is being so uncritically

[96] Such a statement begs the question of why large firms exist, unless that is also to be sheeted home to imperfect information. Though with a bit of imagination many apparent indivisibilities can be theoretically dissolved, in practice, such niceties may be often observed more in the breach. Moreover, if we step away from homogenous output, firms with decreasing costs can continue to compete with one another given perceived differences in that output. Apples, for instance, at the same farmer's market don't all sell for the same price. Such empirical observations do not necessitate any required lack of information. The result merely underlines the modest assumption that buyers have different preferences.

> urged in the current reform literature. And finally, of course, there are those who still adhere to the belief that a liberal economic society is the object of all good social policy. To them it is still important to compare economic policy under perfect and imperfect competition, to understand the causes of imperfect competition, and to formulate remedies to restore a competitive regime (Stigler 1937:717)[97]

Stigler's early paper on imperfect competition is perhaps best categorized as the work of a still young academic seeking to find his direction and support his abiding interests. This is not to deny, however, that even this tentative product is still quite a good indicator of what his future path might reveal. But in the pre-war years, and through the war itself, Stigler is largely experimenting. Parts of his early work seem driven by the shadow of Knight and can be categorized as continuing an investigation into the same issues that formed the core of his dissertation. Other work, especially during the war years and immediately after, is largely empirical (1946, 1947) in nature or even mathematical (1945).

> George actually had, although he wasn't very deeply trained in mathematics, obviously he had an original mind. For example, I can think of two things that he did, that a person who was illiterate in mathematics would not have been expected to do. His "Least-Cost Diet" article, I should think came out about 1939, or some date like that, it came out in the *Journal of Agricultural Economics*, done when he was at Minnesota. It is a clear formulation of a particular version of a linear programming problem. George did not know there was a prior history of this kind of problem and he did not work out the Dantzig simplex method for solving it. But he was able to get

[97] Clearly the influence of Simons shows through. Gone in future years will be the ever-present conjecture that formulated remedies are required to restore a competitive regime. The flavor of this assertion reflects Simons' fascination with an aggressive employment of anti-trust policy to maintain competitive markets.

> He [Stigler] begins to understand that there must be good reasons why Eastman Kodak dominates the film industry. Obviously there must be market forces involved. Why wasn't capital flowing into an industry with high basic returns? He is asking himself the kinds of questions that just didn't occur to Simons. A guy like Simons would just say, "Well, they're too big. Break them up! The text books tell you, the more firms, the better." And that's it. Simple. End of story. Advertising screws up people's minds, tax advertising and it'll be fine. (Conversation with Sam Peltzman, October 1997)

lower bounds and upper bounds. (Conversation with Paul Samuelson, October 1997)

Despite this acknowledged aversion to alternative approaches, the 1948 attack on Chamberlin's work was set off rather serendipitously with a gratuitously nasty review of George Stigler's textbook on price theory (1946). Whether such an attack would have inevitably irrupted somewhere down the line is of course debatable. But Stigler had already warmed up his tools of attack (1946) and would continue to set his sights on any alternative theory that had gained some traction within the profession and posed a clear threat to traditional price theory.[98]

> Now, you can see the way George Stigler had a wonderful nose for attacks on neo-classical economics, let's call it, which were dangerously capable of undermining the very foundations of orthodox economics. It's no accident that he went for Gardiner Means' *Administrative Prices,* Harvey Leibenstein's *X-Efficiency*, Galbraith's *Affluent Society* and his other things, the *Kinked Oligopoly Curve* of Sweezy. These are all ideas that are very, very dangerous. They are subversive even of orthodoxy. It's those ideas which roused his critical fire. (Conversation with Mark Blaug, April 1998)

In the 1940s, leading journals reviewed books, which were still taken to be a serious enterprise, and even extended this tolerance by casting a critical eye over the production of textbooks.[99] George Stigler's initial

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[98] Stigler is clearly attempting to define for the profession the precise identity of price theory by both writing his textbook and struggling to clear the Aegean Stables of the prolonged accumulation of faulty frameworks. That his preferred approach achieved orthodox acceptance reflects his multitude of skills. Though some of his former students, such as Mark Perlman, who suffered Stigler as a member of his dissertation committee at Columbia University, were left largely unimpressed by these abilities.

> I think that I agree with your points on our Boy George, but I wish you were not so intense on the topic. Stigler's attacks are now mainline economics; and I think that the way to counter the mainline is simply to laugh at it. "Surely one cannot take Professor Stigler, the Nobel Laureate, really seriously—he is clever, even amusing, but to those of us to whom the good Lord has never revealed The Light, Stigler's approach is on the scale of sophistication (1–10) debatably 6.9 although on days when I feel stimulated by callow youth, I've even been known to give him a 7.1. My trouble, which invariably I later regret, is that I am always overgenerous to glibness." (e-mail from Mark Perlman, October 16 1995)

[99] The idea of producing a considered review of a textbook these days would indicate a very idiosyncratic expenditure of limited academic time. It is doubtful that anyone, even a professor who may

foray into this field (1942), *The Theory of Competitive Price*, had been limited to the case of perfect competition (which these days seem to provide the climax for the first half of any micro textbook or course). *The Theory of Price*, appearing in 1946, left his work on perfect competition untouched and added the now expected chapters on monopoly and other market imperfections. The textbook was reviewed widely, but for the story unrolling here, the key response appeared in the June number of the *American Economic Review* (1948). Edward Chamberlin would on the surface seem a logical and certainly reasonable choice for this task. He was a well-established micro-economist, a professor at Harvard and of course, one of the authors who had propelled the theory controversies of the 1930s with his work on monopolistic competition. Intellectually, he more than met all and any reasonable requirements, but temperamentally he was an unfortunate selection. Edward Chamberlin was then entirely engaged, if not immersed, in a continuing battle to demonstrate the difference between what he attempted in *Theory of Monopolistic Competition* and Joan Robinson's efforts in *The Economics of Imperfect Competition*, both of which appeared in 1933. It didn't help that Robinson had abandoned the field and showed little interest in her earlier work.

> … Joan Robinson, along with other economists, had switched her principal attention to Keynes's theory of unemployment, and although she commented occasionally on imperfect competition, she never attempted any significant further development or revision of the theory. (Loasby 1989b:84)

In a more than slightly paranoid fashion, Chamberlin divided his academic environment into camps populated by those economists who supported his views and strategies and by those who proved

prescribe a given text for a course, even reads the volume from cover to cover. In an attempt to meet all possible demands of instructors, textbooks have ballooned until some exceed a thousand large-sized pages composed of text, brilliant colors, photos and some random graphs. Stigler's 1946 effort ran to some 340 rather petite pages and cost the princely sum of $3.75. Today the internet provides an outlet for both the trivial and profound, but an economist would still see textbook reviewing as the equivalent of expecting an English professor to spend time in thoughtful consideration of the latest Harlequin romance. Instead, solicited blurbs from the author's academic colleagues, allies and friends find their way on to the marketing material.

incapable of seeing the light or just stubbornly refused to jettison shop-worn theories. Trusted allies were those capable of distinguishing his approach from that of Joan Robinson. He seemed to view all of economics from the limiting prism of his own work. Everything said and done ultimately became about him. Since Stigler's new textbook added work on monopoly and market imperfections, the value of this additional material, in Chamberlin's estimation, would clearly depend on whether he saw Stigler as a faithful member of his intellectual army or as an enemy figure. Chamberlin's pose was predictable even before he opened Stigler's text. Stigler was one of Knight's few students. Moreover, he had already produced an article, heavily laden with Knightian overtones, which had unambiguously disparaged imperfect information.[100] Stigler's text then managed to aggravate an already perilous position by largely ignoring monopolistic competition (except for an odd sideswipe). Predictably, Chamberlin reacted by conducting a search and destroy mission that was most notable for its unnecessary snarkiness and self-obsession. Chamberlin's thin skin compounded by other less than admirable characteristics unfortunately gushes forth in his detailed, five-page review.

> One reason I [Paul Samuelson] chose to go to Harvard for graduate work in 1935 was my admiration for Edward Chamberlin's *Theory of Monopolistic Competition* (1933). Taking his so-called advanced course in the fall of 1935 deflated my esteem for him. What completed the operation was his comment on the margin of the term paper I wrote for his course. It was a very good paper for the time—my fellow student Robert Triffin assured me of that. But what really browned me off was the fact that Chamberlin's only remark on my paper was a pencilled in 'Good' next to a paragraph in which, while criticizing some implicit theorizing in passages from Joan's

[100] Stigler in his 1937 paper essentially convicts Chamberlin of self-contradiction. He claims that in essence Chamberlin failed to understand the consequences of his own assumptions. Chamberlin was unlikely to forget such pointed criticism.

> Chamberlin, for instance, postulates the existence of perfect knowledge on the part of the consumer, when he treats with differentiation of commodities. Yet it should be almost obvious that in an economy where consumers possessed perfect knowledge, branded articles would be of interest only to historians. (Stigler 1937:710)

> *Economics of Imperfect Competition* (1933), I referred to her cheaply as Madame Robinson. (Samuelson 2011c:665)

Though Stigler could not have expected fair play given Chamberlin's well-deserved reputation, he did have every reason to be furious. Chamberlin essentially accused him in that fateful review of not knowing what he was talking about. In Chamberlin's eyes, Stigler's new text was archaic, a volume that would not bring a dutiful student up to date with the latest developments occupying the profession's attention. He did not explicitly dismiss the book as not worth opening, but that is the sense conveyed by his review.[101]

> All this is not very different from what could be found in the textbooks twenty-five years ago. I can only conclude that the student who takes these chapters of Professor Stigler's as a guide will be off to a bad start in understanding what writers on "imperfect" and monopolistic competition have been talking about in recent years. (Chamberlin 1947:418)

Exercising even the vaguest of rational expectations, Stigler must have anticipated Chamberlin's probable reaction. He had, after all, insisted on

[101] Chamberlin found Stigler's understanding of monopolistic competition wanting in the extreme and spent much of his five-page review convicting Stigler of those trespasses. But those attacks alone did not mollify Chamberlin's wrath. He also felt compelled to nit-pick about the organizational structure of the textbook, condemning it as unnecessarily clumsy, repetitive and ill-conceived. Not satisfied with a blanket condemnation, Chamberlin was willing to stoop to a certain degree of coy cattiness.

> These latter pages are at the beginning of the Costs chapter, where some of the worst effects of Professor Stigler's random methods of assembly and (shall we say?) carelessness are strikingly illustrated. (Chamberlin 1947:415)

Chamberlin even twits Stigler about the use of the German term *Weltanschauung*, reducing it to a matter of intuition or perhaps even gut feeling. There is an undertone here that Stigler is being pretentious or in some way showing off. The sarcastic tinge to his tone can only represent a simple attempt by Chamberlin to steer the way in which readers perceive Stigler and thus his textbook.

> They are given in the opening chapter of the new material … offered specifically as "a detailed statement of the author's *Weltanschauung* which underlies the selection of content in subsequent chapters." (Chamberlin 1947:417)

The unabridged version found in Stigler's textbook seems a bit more innocuous.

> This material can be viewed as either a detached statement of the author's *Weltanschauung* which underlies the selection of content in subsequent chapters or a survey of the major factors underlying the important departures from perfect competition. (Stigler quoted in Kamerschen and Sridhar 2009:186)

waving the very reddest of clichéd red flags before an academic with the thinnest of all thin skins and one who had clearly demonstrated an obsessive concern for his theoretical position. To label an academic's lifetime's work as a minor deviation from orthodox theory (a not-so-subtle code word for trivial) is bound to elicit critical buckets of scorn on one's own work if only as a simple form of retribution.

> It can be argued that the recent literature of economics has materially exaggerated the importance of such departures from competition. (Stigler quoted in Kamerschen and Sridhar 2009:186)

Stigler had basically incorporated in his textbook all the core objections and analysis from his 1937 paper. He certainly had not altered his position, and if anything, had become more convinced of his particular perspective. In his unarguable vision, monopolistic competition could basically be dismissed as a mere minor deviation from perfect competition, motivated by such information deficiencies as consumer ignorance. According to Stigler, such ignorance alone generated product differentiation.

> The reason is not far to seek, for the author himself is quite explicit in his introductory paragraphs to the new chapters that he is now taking up what is to him, by contrast with "Perfect" competition, the unsystematic part of economic theory. It cannot be overstressed that, although "imperfect" (monopolistic) competition has in fact come to be widely accepted as either more general than pure competition, and hence designed to replace it *in part*, it is none of this to Stigler. It merely refers to the "literally infinite number of possible deviations from perfect competition" … a hodge-podge without any system of its own (until we have more "factual knowledge") and from which selection can only be made by intuition. (Chamberlin 1947:416)

Stigler then is determined to steer discussion away from analysing monopolistic competition as an alternative approach to a more innocuous exercise limited to cataloguing the effects and deviations caused by imperfect competition.[102] Despite Stigler's insistence, Chamberlin does have a valid argument which Stigler simply left untouched. A reasonable assumption that consumers have heterogeneous preferences yields differentiated products no matter what the state of information or ignorance might be. Selling exactly the same loaf of bread to all buyers in this case would stray from the touchstone of economic rationality. In neither his 1937 paper nor his textbook does Stigler convincingly explain why variability in output (and hence individual, negatively sloped demand curves) depends solely on insufficient information.

> Far from being the explanation of those "departures from competition" which economists have discussed in recent times, consumer ignorance has played virtually no part at all in explaining them. Important as it may be, and especially in connection with the particular subject of advertising, it is a fact that by far the greater part of the literature on "imperfect" and monopolistic competition has been written without reference to it, *and would remain intact under the assumption of perfect consumer knowledge*. Product differentiation is explained, not by consumer ignorance, but… by the conscious production of variety in response to the demand for it, a demand arising from the diversity of tastes and needs. (Chamberlin 1947:417–418)

As negative as the review proved to be, the controversial spark did not in the end consist in Chamberlin finding fault with an approach that can be argued to be too one-sided, but of him in essence labelling Stigler an unapologetic ideologue. This charge was largely unnecessary unless the review was meant more as a therapeutic act allowing Chamberlin to vent his anger than as an actual evaluation of the textbook. Stigler may have been an ideologue, but labelling him as one would only serve to infuriate his sensibilities. This perspective transforms Chamberlin's review into a gauntlet thrown down, an insult demanding a rejoinder. Given Stigler's caustic personality, there was no chance that a challenge once presented

[102] In this case Stigler is directly trying to influence a new set of graduate students, but indirectly those who teach such classes by having them acquiesce to his own understanding of economics. Certainly Stigler's text focuses on perfect competition as the single theory underlying all economic analysis. This stance would only be extended over the years as he attempted to transform it into a universal theory.

would not be accepted. George Stigler throughout his career remained an academic who thrived on and particularly enjoyed the rough and tumble of debate.[103] It was almost inconceivable that he would let such remarks pass unanswered. The only question was when he would choose to respond. In such cases, an immediate response is often only of negligible value; far better to choose one's time and place.

> "Monopoly (or better, … imperfect competition)": is like friction in physics, and hence to be dispensed with in the "first approximation." Although it was in protest against all this that the theories both of "imperfect" and of monopolistic competition developed, they have led him only to reaffirm—and repeatedly—this old position … In addition to this "faithful unto death" attitude towards perfect competition, and (I should like to believe) as a possible explanation of it, there is confusion and misconception in rare degree as to what "imperfect" and monopolistic competition theories are all about. (Chamberlin 1947: 416–417)

What remained something of a mystery at this point was what drove Stigler's clear animus against Chamberlin's theory. There naturally was some strong connection between his negative response to the work and his unshakeable defense of perfect competition. At least two factors were clearly at work here, although they might have lurked somewhat below the surface rather than bobbing along as obvious candidates. One clear issue was his dislike of what he saw as unproductive theory, conjectures that led essentially nowhere. He firmly believed that it was every economist's duty to try to steer the profession into the correct path of thought. For whatever reason, and despite his dismissal of Smith's *Moral*

[103] Stigler was a professional stirrer. It was important to him that what he said and wrote got a reaction, whether positive or negative. He most appreciated those who could give as good as they got.

> In the same way that, at Columbia, I was the only student that George Stigler ever had. This is an important thing to say. Basically the reason was that he mellowed as he got older. But even when he was older, he was extremely bitchy. And he slaughtered his students. Right at the beginning of our conversation together, I realized (I don't know whether I grasped this consciously) that you always had to give back as good as he gave. He'd say, "Oooh, I think, you'd better watch out for this chapter, you know you didn't take this into account."I'd say, "Yes I did." I think I argued with him. And the moment I did that, our relationship was brilliant. No problem dealing with those students who stood up for themselves. But if a student was crushable, he crushed him. Couldn't resist doing it. And he was a bully, you know. You had to stand up to him. (Conversation with Mark Blaug, April 1998)

Sentiments, he felt a distinct obligation to improve economics by improving its rigor and level of debate.

> Very often George, when he wanted to decide how he felt about something, would ask himself not whether this stands up as a theory, as an intellectual exercise, but whether it had some applicability that moved the profession on in a useful way. It had to be *useful*. And I don't mean useful in terms of government programs. I mean useful in terms of being productive. Productive of useful predictions, of certain facts, which when you examine the data are confirmed. But it was also a question of whether the profession built on it. He identified very much with the profession. He cared whether the profession moved ahead. (Conversation with Claire Friedland, October 1997)

However Stigler was not a simple thinker or individual. He came from a generation that by nature did not disclose motives and certainly did not reveal emotions. We are left with taking a leaf from Stigler's own bible and trying to discern from his own actions what his other motives were likely to be.[104] He felt an almost compulsive need to parallel the achievements of someone like James Clerk Maxwell who had managed to construct a model, a set of equations, such that previous work completed in optics, magnetism and electricity could be unified into a single theory.[105] Stigler saw the theory of perfect competition as the best way of

[104] This implies that even a deathbed confession by Stigler would have to be dismissed as largely irrelevant. For Stigler, it was what you did and not what you said that was relevant. In which case, discovery of telegrams from John D. Rockefeller threatening and planning a bout of predatory pricing would serve only as an amusing historical note. He could thus dismiss a case reported by J.B. Clark as either anecdotal hearsay or worse.

> Candor forces me to state my belief that the distinguished Columbia professor invented this dialogue, but even if he had a recording of it, it is no evidence for an economist. Modern scholarship, I may observe, has raised strong doubts about the frequency of use of predatory competition, and has by no means resolved the theory of its operations. (Stigler 1982c:47)

Unfortunately the evidence Stigler tended to cite was often purely logical. In other words theory said that it could in no way happen and therefore it became impossible no matter what the appearances might be. Certainly Stigler insisted on empirical evidence, as he should, but we are left wondering exactly what evidence would convince him that there was anything resembling an inherent flaw in market operations or that the theory of perfect competition should be jettisoned.

[105] This aspiration to transform economics into a discipline and a profession that reflected scientific principles was not only a product of Stigler's mature years. Such a mindset appeared even as he busily attempted to establish his reputation. He saw in the theory of perfect competition a sturdy

putting economics on a sound scientific basis. By doing so, he would be following in the footsteps of such distinguished theorists as Maxwell. In and of itself this was an intensely ambitious goal and constituted a clear break from his teacher Frank Knight. Whether economics could in fact be moulded to this vision was an unanswered challenge.

> The main explanation for the power of an abstract theory is that it has not specified a lot of factual content. If I specify factual content, if I get descriptive in my assumptions, there is a great danger that while I can tell a very good story, it doesn't tell me anything about the world. A striking example is the theory of monopolistic competition of the Chamberlinian variety which now is no longer used. It paid attention to things such as the fact that every seller differs from every other seller in location, or the charm of his personality, or the fact that his brand of toothpaste is advertised differently than another brand, and so forth. All those things were realistic, but they were part of a theory that didn't tell us anything that was seriously interesting, different, or more insightful that the abstract theory of perfect competition. (Stigler 1988d:2)[106]

The theory of perfect competition also provided another dimension that Stigler failed to explicitly discuss. It solves the problem of private market power. In a Hobbesian resolution of this perennial issue, individuals surrender economic power to the market. Shorn of all individual power, each economic agent is set free, each is free to choose, having only to respond to the discipline of the marketplace. This maneuver neatly removes one

vehicle which could be pushed further to yield a wide-ranging, general theory. Moreover, it was a theory consistent with what he perceived to be core liberal principles, those which buttressed individual choice and freedom.

> …indeed we have Stigler's Law: The gorgeousness of a theory varies with the range of phenomena it embraces and inversely with the number of its constants.
>
> The great scientific discoveries have been syntheses of diverse phenomena that previously had defied generalization:
>
> Newton: joined astronomy, mechanics, optics, hydraulics
>
> Einstein: joined Newton and curvature of light
>
> Marshall: joined short and long run theories of price. (Letter from George Stigler to Milton Friedman, December 10, 1947 in Hammond and Hammond 2006:75)

[106] Notice the insistence that monopolistic competition is an unnecessarily complex theory that yields no more than perfect competition. This is a refrain that will be used throughout his long and fruitful career.

of the prime justifications for market intervention by government forces.[107] Such action is said to redress the imbalance caused by private power. Remove the power and there is nothing left to redress.[108] There is, however, one additional step to take. Under perfect competition output is perfectly exhausted according to the marginal product of each factor of production. This produces a type of productivity ethics where each is rewarded according to their contribution. Stigler in later years would claim that such a market outcome would gain widespread public support rather than being strictly subscribed to only by academic economists.

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[107] The perfect competition model also neatly removes problems arising from perceived market inefficiencies. In a sense, markets are self-healing in that they inherently contain incentives that encourage economic agents to fix and improve any existing circumstance. A simplified explanation would conclude that everything observed is efficient, or as it should be, since if it were not, there would then exist a potential gain from correcting that status quo. This is sometimes known as the "no fifty dollar bill left on the sidewalk" hypothesis.

> Okay, I imagine that he [George Stigler] got this from Milton Friedman. This happened around 1952, at the Paris Colloquium or Conference on Risk, put on by The Econometric Society. Milton Friedman gave a paper which said in effect, life is a constant procession of events that impinge on us with a considerable amount of uncertainty out there. (This is my broad gloss on what he said.) At every stage on the road there are forks in the road, and we are making choices. And, in effect, we end up in the beds that we have made for ourselves. This would have grown, in Milton's mind, out of the Friedman/Savage article of 1948 on gambling. You postulate an epicycle in the form of a convex stretch, a non-concave stretch of the utility function so that the people falling in that become inveterate gamblers. And so the inequality is the result of their own *ex ante* decisions. Now, it's undoubtedly true that if everybody started out exactly alike in genetic composition and environment—for this they would have to be clones, identical clones—and if for some reason, even though they are clones, they differ in their risk aversion, then, what you will find is that those with the greatest risk tolerance will end up bi-polarly at the extremes more than the people with less risk tolerance. And so what is, is right. (Conversation with Paul Samuelson, October 1997)

[108] Perfect competition provides the comforting result for economists that markets ensure all will be well. This outcome, theoretical alone or reflecting observed results, remains something of a religious equivalent of being rocked in the bosom of Abraham. Such a description is a deliberate exaggeration, but not entirely misleading. The key to elevating the self-correcting nature of markets to an almost ineffable level requires an unshakeable belief in the self-correcting nature of those markets. This faith arises from recognizing perfect competition as a comprehensive theoretical tool with widespread and remarkable explanatory properties.

> George was focused on the way the market marches in to *eliminate* the externalities, to work *around* them to make them a market problem instead of a non-market problem. I think I've quoted him in my memoir as saying something like, "externalities are what the market has not *yet* eliminated." That's not an exact quote but in my memoirs I do have the exact quote. You see he saw the market as *the* force. He was looking at the other side of the market, at how the market may provide an appropriate solution. (Conversation with Claire Friedland, October 1997)

> If you declare to a layman that a certain individual is paid his marginal product, after explaining perhaps more clearly than I have what a marginal product is, and then add, "Isn't that simply outrageous?," I predict that this layman will be amazed by your comment. (Stigler 1982d: 17)

The problem of income distribution arises sporadically throughout Stigler's work, but seemed to form one of the drawing cards that originally pulled him into economics. This fixation predates Chicago, given his work at Northwestern, but subsequently such a concern became explicit when undertaking his dissertation under the guidance of Frank Knight. But for marginal productivity ethics to hold, we need to assume the type of unique equilibrium that guides perfect competition. Once we allow multiple equilibria or dare to conceive of a non-equilibrium theory, any resulting income distribution lacks the justification of either efficiency or ethics.[109] Instead we are back with Frank Knight's objection in his *Ethics of Competition* (1923) to the supposition that competition yields just outcomes. By accepting a somewhat arbitrary feature attached to market distributions, we open the door for government action to once again redress an inherent imbalance.

> The paramount defect of the competitive system is that it distributes income largely on the basis of inheritance and luck (with some minor influence of effort). The inequality of income increases cumulatively under competition. (Knight quoted in Stigler 1982d:18)

If random good fortune provides the basis for future success, while inheritance tips the scales, then a blind faith in markets is ultimately not sustainable. Stigler had no real choice here. He could keep faith with the skepticism that characterized Knight or he could nail his colors to the mast and devote his future to defending, in the trenches if need be, the vision supported by perfect competition. Given the growing direction of

[109] One glaring issue with monopolistic competition as devised by Chamberlin is that it lacked a unique equilibrium, whether we concede the contention of multiple equilibria posited by Demsetz (1959) or push pass that contention to claim the operational side of the theory as essentially non-equilibrium.

his work, turning his back on the more nuanced approach of his teacher was hardly a question for debate.

> When I first read this essay [*Ethics of Competition*] a vast number of years ago, as a student writing his dissertation under Professor Knight's supervision, you should not be surprised to hear that I thought his was a conclusive refutation of "productivity ethics." When I reread it a year or so ago, I was shocked by the argumentation. Knight made a series of the most sweeping and confident empirical judgements … for which he could not have even a cupful of supporting evidence. Moreover, why was it even relevant … that real-world markets are not perfectly competitive in his special sense: one can define a perfect standard to judge imperfect performance, and assuredly real-world performance under any form of economic organization will be less than perfect by any general criterion. Knight kept referring to the objections to competitive results under any "acceptable ethical system" but never told us what such a system contained in the way of ethical content. His own specific judgements do not seem compelling, as when he asserted that "no one contends that a bottle of old wine is ethically worth as much as a barrel of flour." Dear Professor Knight, please forgive your renegade student, but I do so contend, if it was a splendid year for claret … The fact that more than skill and effort go into remuneration … [that]bearded women get good circus jobs simply by not shaving—is not enough to dismiss productivity ethics. (Stigler 1982d: 18–19)[110]

[110] The long quote reflects a type of argument Stigler was not loath to provide. Such a response sounds decisive. He also skilfully tries to dispel any negative reaction by resorting to jokes at the end. But if considered carefully, the quote doesn't really sustain itself to any serious degree. Knight did not provide empirical evidence (of which there was little, if any, then available) but instead appealed to common experience. Stigler, though, failed to provide any counter-evidence, nor did he show himself willing to acknowledge that evidence might exist which would support Knight's contentions. Moreover, Knight in his article was not examining the characteristic of a perfect standard. Instead he remained focused on whether an actual competitive market system would yield a morally acceptable result. What might happen in a perfect world then becomes as irrelevant as an estimation of what heavenly markets might provide. Knight perhaps assumed that there was no need to explain what ethical systems of conduct might contain. Stigler condemned Knight's omission but also declined to enter those philosophic realms. Lastly, Stigler indulgently allowed himself to be witty at Knight's expense. But if I take Stigler's joking remark seriously for the moment, I am at a loss to understand what such a statement might mean. Certainly, a good claret is no more ethically desirable than an average one. But in criticizing his former teacher Stigler ironically seems to be co-opting the very techniques for which he took Knight to task.

As George Stigler's thinking developed after leaving the classroom confines of Chicago he experienced a moment, or probably an extended transition, that was roughly equivalent, at least for an economist, to a nearly religious transcendence. He had to leave his old idols, Knight and Simons, behind to find the one true universal system. Certainly we are not talking about a Damascene social or political conversion. Politically, Stigler seems to have been born a conservative. Certainly such innate leanings were only reinforced by his stay as a graduate student in Chicago.[111]

> It was the 1930s when he was a student here in Chicago that had the real impact. This was when George and Milton, I *think* under the influence of Knight and Viner, started to think strongly about the individual being the important building block of economic theory. The Cold War, I don't think, was really that important. They'd already reached their position by then. They'd gone through their formative years and they never changed from that Knightian point of view. (Conversation with Claire Friedland, November 1997)

Being reunited with Friedman and Wallis during World War II at the Strategic Research Group (Columbia University) could only have reinforced this turn of thought. But what might be termed, with only a smidgen of exaggeration, a life-defining event came with George Stigler's first trip to Europe.[112] Arriving at Mont Pelerin in April of 1947, he was not unlike a poor boy from the country entering a *madrassa* for the first time.

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[111] Stigler clearly believed that a rigorous course in price theory inevitably produced politically conservative individuals. He had seen this for himself at Chicago.

> But I learned very fast, as everybody who comes to Chicago does. I mean, nobody comes here a socialist and leaves here a socialist unless he has been living in a very private world. … However, by the time they had spent a year at Chicago, they had changed. (Conversation with Claire Friedland, November 1997)

He even went so far as to deliberately provoke a Harvard audience (the opposition personified) by delivering a speech (1959) which claimed that economics produced politically conservative practitioners.

[112] Undoubtedly the post-war atmosphere, given the gathering Cold War forces and the turmoil within the economics profession, succeeded in sharpening the academic direction of Stigler's research. He was gaining his spurs as a lethal defender of marginalism as providing an irrevocable basis of price theory (1946c, 1947a). Mont Pelerin did not represent an implantation of new or alien thought, but a nurturing of seeds that had been planted and subsequently had rooted successfully for more than a decade.

Clearly such a boy would not be selected for such an honor in the absence of already discernible qualities. In a similar fashion, the journey was to become something of a reaffirmation of faith for the rising academic. Existing tendencies would be sharpened and refined both by the training received and the company kept. Stigler became one of the chosen partly due to the link between Director and Friedrich von Hayek, who was responsible for organizing this inaugural meeting. The debt owed to Director meant that not only would Frank Knight be asked to attend as a leading liberal figure, but they in turn would be able to extend the invitation to their two younger associates.[113] Milton Friedman was Aaron Director's brother-in-law while George Stigler was one of Frank Knight's few dissertation students. Consequently, the point is not that attending this pivotal meeting transformed Stigler into a liberal/conservative, but that it served to focus his career.[114] It is not coincidental that Stigler describes this trip in his 1988 biography as representing his conservative apprenticeship.[115] The purpose of the meeting was to recapture the intellectual high ground from what the gathering saw as the ominous forces

[113] Director indirectly assisted that financing since he was instrumental in getting the University of Chicago Press to publish Hayek's (1944) opus. It was this volume which would bring Hayek to the attention of politically conservative donors, specifically Harold W. Luhnow, who through the Volker fund would support the Mont Pelerin Society as well Von Mises at the NYU Business School, Hayek at the Committee on Social Thought and Director at the Chicago Law School.

> What happened was that Hayek … met a person called Luhnow, who was then responsible for a lot of money in the Volker Fund. He persuaded Luhnow to give a certain sum of money to establish a center that would promote private enterprise. It was earlier decided that Chicago was the only place that was likely to accept such a project, and it was also decided that the law school was the only part of the University of Chicago that would accept such a project. (Director quoted in Kitch 1983:180–181)

[114] The resulting Mont Pelerin Society would be identified with the idea of liberalism in its traditional European form. It is not accidental that Hayek initially proposed that the organization be named the Acton/Toqueville society after those two notable conservative thinkers. The use of the term liberal to connote left-leaning politics derives from the post-war period in the USA and seems largely limited to that specific country.

[115] Stigler shares a trait seemingly common to many conservative thinkers of claiming that they were somewhat to the left of the political spectrum in their halcyon youth. Perhaps introducing a small element of that Road to Damascus conversion adds a certain piquancy to their story. But although it may not be uncommon for people to become more conservative with age, surely some, if not many, of these conservatives started out being fairly conservative with no notable dalliance whatsoever with leftish political positions.

> Before I went to the University of Chicago I suppose I had vaguely liberal political inclinations, but no strong convictions. (Stigler 1988a:138)

of collectivism. The dangers were viewed as imminent. As leading liberals strategically positioned throughout the world, they had little choice other than to accept the responsibility for rolling back this malignant tide.[116]

> [They] believ[ed] that what is essentially an ideological movement must be met by intellectual argument and the reassertion of valid ideals … [because] The central values of civilization are in danger.[117] Over large stretches of the earth's surface the essential conditions of human dignity and freedom have already disappeared. In others they are under constant menace from the development of current tendencies of policy. The position of the individual and the voluntary group are progressively undermined by extensions of arbitrary power. (Statement of Aims, April 10, 1947 at Mont Pelerin Society 2003:1)

For the young George Stigler, being once again immersed among self-professed liberals could only prove to be both inspirational and comforting. He would come to realize that he had other colleagues and comrades travelling down the same road as the one he had chosen. However, the long-lasting impact of that meeting would not come from the arguments or poses struck during that fateful meeting. The crucial consequence would come from a bond formed between George Stigler and Aaron Director during those ten days in Switzerland. Though widely recognized within the Chicago circle as having played a central role in shaping the Chicago program and intellectual development, those on the outside have tended to ignore this man glimpsed at best as haunting the background, deliberately avoiding any public limelight. With reputation within the profession stemming largely from publications, Director's reluctance to appear in print

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[116] It is interesting that by the early 1970s, Milton Friedman thought the society had served its purpose and should disband. Clearly the majority of members thought otherwise. Both Friedman (1970–1972) and Stigler (1976–1978) would serve as president during that decade.

> I follow Darwin in accepting survivability as the test of an institution, so at least *part* of modern society wished a repeated gathering of old-fashioned liberals. I confess that none of the later meetings I attended equalled for me the interest of the first session. (Stigler 1988a: 145)

[117] Notice how the opposition is derided as being blinded by ideological concerns, while the self-selected liberals pursue their goals and foil the ideologues by the adept use of sweet reason alone. Ideologues of all stripes seem remarkably skilled at sniffing out the intransigent set of beliefs held by their unyielding opponents, while limiting themselves to using only their own intellectual resources.

most likely generated something of a benign neglect in matters touching on his career. Yet George Stigler would send all his work to him and Director's opinions would be among the few (along with those of Friedman and Becker) that could have a substantial impact on his thinking.[118] If anything, Director's highly critical and conservative voice would come to outweigh that of his brother-in-law, Milton Friedman.[119]

> I really didn't get to know him until the time of the first Mont Pelerin meeting, which was thirty-four years ago, and I've considered myself to be a pupil of his ever since. With some embarrassment, I think of the number of times in which I've confidently advanced a view that I've known was true all the time and after half an hour of gentle and friendly Socratic discussion I have discovered I was standing on air. That's not an experience I've been

[118] The impact of Friedman, Stigler and Director in shaping the Chicago program cannot be underestimated. They were in some sense the high acolytes of price theory, refining its underlying theology through constant discussion, though probably minimal fasting or self-abasement.

> Now, here is George in the midst of Chicago, where all his contacts are such sharp people that if George made a mistake he'd know about it right away. Besides, he sent everything he wrote to Milton Friedman and Aaron Director. I'm exaggerating, but he sure sent a lot of it to Milton and Aaron. (Conversation with Claire Friedland, November 1997)

[119] Aaron Director was a classic one-sided skeptic finding faults with arguments for government regulation, but less skeptical regarding market operations. He is rightfully credited with founding law and economics as a legitimate and rigorous field in economics. His influence while at the University of Chicago Law School reached almost legendary status.

> I might comment as a student in the economics department during this period [early to mid-fifties]. I and Lester [Telser] and others got to know Aaron, and some of us attended the antitrust course he taught with Edward Levi. The economists there began to feel inferior. The law school students could talk so well, in complete sentences … But I felt at the time that what I got out of there was the feeling that business practices shouldn't be explained away as irrational. They have a purpose. What they are is often difficult to assess, but there is a way to understand them if one is perceptive enough. It is an influence that has stayed with me and I know with a number of other students. (Becker quoted in Kitch 1981: 185)

However others have been not quite as enthusiastic about Director's influence.

> I believe this is evident in that short breezy autobiography that he [George Stigler] was persuaded to write by the Sloan Foundation Committee on Scientific Autobiography. He was reluctant of course to do it, but he then did it in a remarkably short time once he made up his mind. The key character in this change was probably Aaron Director. Which is surprising as Aaron Director was a scratch tenure appointment at the University of Chicago. He published almost nothing and never took his PhD degree … Aaron Director was extremely conservative. Why, I don't know. By the time I knew him he was already like that. And he was an iconoclast. But he didn't develop new data with respect to industrial organisation. He didn't develop and articulate new theories. He just said that the conventional belief wasn't so. (Conversation with Paul Samuelson, November 1997)

> alone in having. In that form of personal discourse his influence has been magnitudes larger than almost anyone else I've met. (Stigler quoted in Kitch 1981:185)

Stigler returned from the rarefied and inspiring atmosphere of the Swiss Alps only to be confronted by Chamberlin's searing and overly critical review of his textbook. Despite what must have been a natural sense of outrage at being academically manhandled, resorting to an immediate bout of retributive character assassination seemed unlikely to be productive. Nor did the satisfaction of delivering a physical blow seem within the realm of acceptable possibilities. He passed up a natural impulse to gain a quick sense of satisfaction by turning down the offer extended by the *American Economic Review* of publishing a short reply. Stigler had a longer game that he wished to play. He wanted time to marshal his thoughts and the opportunity of exercising his right to a much more extended reply. He naturally turned to his closest friend, Milton Friedman, for advice, while only hinting at his obvious annoyance.

I'd like your advice on the following matter:

1. Chamberlin sent me a proof of the review of my book in the AER and in reply I sent the enclosed letter.
2. Homan referred to the review (and regretted the fact he hadn't given me opportunity to reply), and I sent a copy of the letter for his own interest.
3. He now writes that I should either quote the central matter or write a short note, for the benefit of readers of our books.

I am not inclined to do this because

(1) of a general feeling against replies to reviews, and
(2) the inappropriateness of a short note in dealing with this matter (and the disinterest in a long one). All I gain by a reply is creation of doubts in the minds of those economists (numerous, alas) who think Chamberlin is a great man. What say? (Letter from George Stigler to Milton Friedman, August 1947 in Hammond and Hammond 2006:61)

As indicated in his letter to Friedman, Stigler decided to content himself by sending a polite, if pointed, letter to Chamberlin, taking him to task for shortcomings displayed in the contested review.[120] Whether or not Chamberlin responded, and what that response might have been, is unknown. However, given his thin skin, Stigler's remarks, even though largely private, must have stung.

> I am not at all angry at your review. It seems to me to make some good points, and in any event it is a sincere expression of an important viewpoint … I am distressed that my failure to accept the theory of monopolistic competition is a crime, per se. This may be so, but requires proof. I criticize your distinction between production and selling cost; you are silent. I disagree with your abandonment of the industry concept and explain why; you are silent … In any event, it is not a sin to reject your orientation; in this I have very illustrious companions. I am prepared to argue (1) that your theory is indeterminate, and (2) that is it not useful (often) in realistic analysis. I do not recall a single consistent application of it to a real problem, and this is the ultimate failure of a theory. (Letter from George Stigler to Edward Chamberlin, August 1947 in Hammond and Hammond 2006:62–63)[121]

Stigler's insistence that without consumer ignorance Chamberlin's theory collapses seems dubious unless one assumes identical consumers harboring homogeneous preferences. Heterogeneous preferences would imply that demand for different cigarette brands could vary even if they were sold at the same price and lacked any auxiliary advertisement. Stigler possibly previewed in this private note that his subsequent presentation of Chamberlin's theory would fail to reflect Chamberlin's

[120] If Stigler's subsequent correspondence with Friedman is read carefully, it is possible to catch a whiff of regret at the tone adopted in his letter to Chamberlin. Not because of any possible wounds he inflicted, but rather that he revealed his own sensitivity to Chamberlin's venomous attack. To the John Wayne generation, a man never deliberately allowed a chink in his armour to be placed on display.

[121] There are some noted ironies to be found in his note to Chamberlin. Stigler complained in tones tinged with bitterness about the way Chamberlin had presented his text. In essence Stigler lodged his complaint and then turned around some months later to use similar tactics against Chamberlin, reminiscent of the way an abused child will often morph into an abusive parent.

> But I am disappointed that certain charges were made on a misreading or at least a very unsympathetic reading, of the text. (Letter from George Stigler to Edward Chamberlin quoted in Hammond and Hammond 2006:62)

intentions or try to neutrally understand the basis for his model. Like a terrier, Stigler seized on a perceived weakness and refused to back down a step. He also based one of his many dismissals of Chamberlin on a pure assertion, stated with confidence as always, but lacking any empirical evidence. Curiously, for someone who would later push for a quantitative revolution, Stigler could never resist working backwards. He would start confidently from an unarguable position of confidence and then locate data that would support his a priori assertion.[122]

> The one point of substantive disagreement is on consumer ignorance, which I hold necessary to monopolistic competition. You concede this only for advertising. But you must either (1) show that there are many cases of many firms producing poor technological substitutes—a class I think is unimportant—or (2) admit my position. (Letter from George Stigler to Edward Chamberlin, August 1947 in Hammond and Hammond 2006:62)

To a certain degree, Chamberlin is forewarned of the direction subsequent attacks would take. The battle itself would be resumed in March of the following year. Stigler's growing reputation as a stalwart defender of price theory and as Knight's protégé had toward the end of 1946 earned him a tentative invitation from Friedrich Hayek to present four lectures at the London School of Economics, contingent on funding. If anything, the subsequent Mont Pelerin meetings in April 1947 would have encouraged such an invitation. Unsurprisingly, the lectures were extended to five with March 1948 being decided upon as the fateful date. In the lead-up, however, George Stigler consulted by mail with his trusted and like-minded friend Milton Friedman. He decided not to pursue publication of his letter to Chamberlin but certainly took on board Friedman's feedback on his draft lectures, including the suggestion that he might

[122] A clear example came in his long-running battle with Gardiner Means. Just as Means had no doubt that prices were administered and thus rigid, Stigler knew in his heart that prices were flexible, reflecting market shifts in demand and supply. He started his major investigation into pricing regimes (1970) without the slight doubt as to its outcome.

> You tell me. He was absolutely convinced that prices were flexible from Day 1. That was clearly his a priori intention. It was more than an intention. It was his belief, in the true believer sense. He really believed that. (Conversation with Jim Kindahl, October 1997)

follow a line of attack similar to the one appearing in a short review article Friedman had written on Robert Triffen's (1940) *Monopolistic Competition and General Equilibrium Theory*.[123]

> It's too late to do anything about the letter, and frankly I still think it shouldn't appear: two items would seem to be piling it on; and the tone of the letter seems a little petulant to me at this distance. And when in doubt don't print it. Anyway it isn't important. (Letter from George Stigler to Milton Friedman, December 1947 in Hammond and Hammond 2006: 72)

In the Triffen review, Friedman stated a key idea which would be further elaborated in Stigler's subsequent LSE lecture. Triffen eliminated the idea of a Marshallian industry. For Friedman this was the logical outcome of providing each firm with its own differentiated demand curve. Once established, there was no method for aggregating each and every distinct demand curve into a single industry curve. For Friedman, this foolishly surrendered the usefulness of industry analysis (which he seemed to identify with market analysis).[124]

> The most important problems in the real world relate to "industries," though admittedly the exact content of an "industry" depends on the problem under investigation. The fact that we state a problem in terms of a

[123] The book was essentially a revision of the dissertation Triffen had written under Chamberlin while at Harvard University. Rejecting the idea of analysing Marshallian-type industries, or the workability of adapting Marshallian partial equilibrium, Triffen explored the possibility of examining individual and distinct firms in a general equilibrium framework. Triffen chose not to take this approach further, shifting his focus throughout the rest of his career to monetary issues. In 1960 before a Congressional committee he pointed out the inherent instability of the Bretton Woods system that was then providing the underpinning of the international monetary system.

[124] It's not entirely clear that a focus on producers is the same as a focus on consumers. Friedman's insistence in his short review seems to be that a demand curve should arise from adding up the demand for each individual firm's output in a given industry. This of course can't be achieved if each output is distinct. Yet if we focus on the consumer side of the equation might there be a demand for ice-cream based on individual consumer preferences as well as sub-demands for different flavors or types? This is a somewhat different question from looking at the discernibly distinct demand for vanilla ice-cream sold by different firms. These alternative vanillas might be differentiated by flavor, packaging and advertising. The problem is that neither Friedman nor Stigler bothered to be precise when they chose to talk about demand. Whether the muddle was deliberate, or accidental, remains to be seen.

> particular "industry" is likely to mean that the differences among the products of the members of the industry are less important, *so far as the specific problem is concerned*, than the similarities. (Friedman 1941:390)

If we accept Friedman's idea that the most important economic problems in the real world relate to industries, and that monopolistic competition must surrender industry analysis, then we can only conclude with Friedman that the theory offers nothing of value (a position that Stigler would explore at greater length in his own analysis).

> The reviewer deduces that monopolistic competition adds little to our box of tools other than a refinement of Marshall's monopoly analysis … Triffen's positive analysis strengthens the reviewer's belief that the Marshallian tools have been thrown away too lightheartedly, and that the absence of industry tools in the kit of monopolistic competition theory severely limits its usefulness … Dr. Triffen's real contribution—and to the reviewer an exceedingly important one—is his demonstration that the concept of an industry has no place in monopolistic competition theory. (Friedman 1941: 390–391)

For Friedman, Triffen had voluntarily cut his own throat, as well as the throats of those who used some variation of the theory, by demonstrating the necessary exclusion of industry analysis. If this approach provides the key to economic understanding and insight, then monopolistic competition would be left with little to offer. This insistent link between applicability and industry analysis would also be insisted upon by George Stigler, forming one of his key points of attack. By this strategy they hoped to rescue the Marshallian supply and demand apparatus. Both Friedman and Stigler insisted that they were loyally carrying on Marshall's traditional approach despite the fact that the essence of this framework had been so severely disputed and consequently fallen out of favor. Claims of this sort—of being loyal and true believers— call into question the extent to which either of these self-proclaimed adherents really understood Marshall's project.[125]

[125] There is little doubt that having learned economics from reading (and rereading) Marshall, both Friedman and Stigler saw themselves as operating within that tradition. In his 1941 review,

> If any single action led to the overthrow of the Marshallian system, it was Pigou's establishment of the equilibrium firm as the central instrument of analysis; for it was now a simple step (though not one taken by Pigou) to define an industry in equilibrium as a collection of equilibrium firms … and this exaltation of the equilibrium firm was the work, not of a critic, but of a 'loyal but faithless' Marshallian. The phrase is D.H. Robertson's … but it has to be admitted that Robertson was also faithless. In his criticism of Pigou's analysis of the welfare gains supposedly attainable by subsidizing industries which were subject to increasing returns, he had insisted that these returns were the results of time and progress of organization yet in the symposium which followed the publication of Sraffa's (1926) article, he accepted Pigou's definition of perfect competition as a correct (if uncomfortable) interpretation of Marshall's competition … Mrs. Robinson would therefore have been entitled to claim that, if she took the wrong turning, the diversion signs were erected by Marshallians. (Loasby 1989b:76)

What is most singular in the available but limited hints of Stigler's preparation for London was the seminal discussion the two conducted on an essential methodological issue. A key defense of monopolistic competition is that it rested on firmer foundations since it employed more realistic assumptions than alternative approaches such as perfect competition. Ultimately the aim of their methodological discussions was to undercut the validity of realism in assumptions as a basis for evaluating

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Friedman points out that Marshall was "concerned with the kind of competition that prevails in the real world" (Friedman 1941:390). But less certain is either's grasp of how Marshall employed his theoretical apparatus to analyse that real competition.

> Sure he was against it [monopolistic competition], but Jesus, nobody at Harvard was. Or for that matter in England, where the conventional kind of Marshallian price theory went to hell. I mean Marshall invented a lot of that stuff. [laughs] Yet it was gone by World War 2. You couldn't learn it there. You had to come to the US to learn about it. That was shocking.
>
> *Stigler certainly saw himself in the Marshallian tradition.*
>
> Yeah. Friedman too. All those Micro-Economists, and I guess I'm a student of theirs, feel that way. I think younger people don't feel themselves so attached to that tradition. They are more concerned with the strategic aspects of theory. They're attracted to political economy, game theory and so on. They've become fairly divorced from that Marshallian tradition. (Conversation with Sherwin Rosen, October 1997)
>
> I think, the emphasis in Chicago—this is a very difficult question because I was about to say, it was really the fact that Chicago followed what I've always called a Marshallian approach as opposed to a Walrasian approach—where the individual and the enterprise, where self-interest is dominant. (Conversation with Milton Friedman, August 1997)

competing theories and by doing so sideline the importance of any further such arguments. In his letter analysing this proposed methodological approach, Friedman urges Stigler to push the logic of his argument further. In this suggested framework, the only thing that ultimately matters is the implication of those assumptions and its relation with observed reality. For Friedman, what then becomes crucial are the predictions a theory could make and how those implications in particular corresponded with measured reality.

> … I have been led to go farther than I had before in distinguishing between description and analysis and in discarding comparisons between assumptions in reality as a test of the validity of a hypothesis. I should like to offer the general proposition that every important scientific hypothesis almost inevitably must use assumptions that are descriptively erroneous. It is of the very nature of a really important scientific generalization that it provides a simpler rationalization of a mass of facts than was available before …In a way, the better the hypothesis the greater the extent to which it simplifies, the more sharply will its assumptions depart from reality. (Letter from Milton Friedman to George Stigler, November 19, 1947 in Hammond and Hammond 2006:65)

The initial problem with this approach is at least two-fold. Economic analysis should provide a greater degree of insight and understanding into how the world of markets actually works. A simple concordance between the implications of a theory and observed results could be chalked up as a necessary requirement in any hypothesis, but surely not as a sufficient one. Such an approach raises the issue of whether assumptions are selected in order to reach a pre-determined conclusion rather than as a device with which to explore observations. In the case of the supply and demand apparatus, is it useful to assume complete producer passivity, or that a firm's output is limited by rising marginal costs? Certainly the supply and demand apparatus produces useful predictions, but that simply reflects the indisputable fact that any change in price can be mimicked by an appropriate combination of shifts in supply and demand curves.

Notice the difference between seeing which assumption produces useful implications and claiming that this is the only essential characteristic to consider when crafting a set of assumptions. The core assumptions producing the Ptolemaic and the Copernican (or for that matter the option provided by Tycho Brahe) planetary systems all rested on realistic, though very different, assumptions. Simplifications are inevitable, but these are not the same as deliberate distortions. We can only imagine a world of perfect competition by constructing a dystopian world where onerous government oversight and regimentation yields markets composed of small, identical firms. Market forces themselves move inevitably away from such an imposed artificial structure. The reality of assumptions clearly is not the sole or perhaps even the most important of all criteria for evaluating a theory. Dismissing such a consideration altogether is, unfortunately, quite another matter.

As Stigler sailed for England, the entire set of lectures he was carefully crafting was intended to take a broader, and even fundamental, view of economics. The goal was to clear away misconceived ideas that were hindering the progress of the discipline. His was a John the Baptist role, preparing the profession for a better, namely liberal, version of economics. He would act as a good shepherd, painstakingly guiding his flock firmly away from the unforeseen edge of the theoretical chasm toward which the discipline was unheedingly heading. Monopolistic competition was simply one of the more pernicious elements that had to be swept away if reason was to prevail.

V. In for the Kill[126]

> War is an act of force, and there is no logical limit to the application of that force. (Clausewitz quoted in Paret 1976:358)

[126] The purpose of this section is not to redeem or justify the work of Edward Chamberlin. Others may happily enter that fray. I am interested instead in mapping out the way in which George Stigler dealt with a work he clearly found lacking.

> I think that was mainly a question of what he thought was a poorly devised model, from which it was very difficult to draw empirical implications that you could test. The model purported to be something different from neo-classical economics. It was treated as being different from neoclassical economics, but it wasn't coherent.(Conversation with Harold Demsetz, October 1997)

George Stigler arrived in London in March 1948 fully prepared to overcome any resistance or skepticism opposing his logic. Key points had been tested during discussions with his close friend Milton Friedman, who shared with Stigler the sort of razor-sharp mind that could winkle out any potential pitfalls hidden within an argument. But, also quite fortunately, at the London School of Economics he faced a potentially conducive environment with the man responsible for his invitation, Friedrich Hayek, undoubtable sympathetic to his stance. Moreover, LSE had during these post-war years successfully resisted succumbing to the new Keynesian approach that was relentlessly gaining a dominating hold over many of the other leading economics departments. Given his talent for constructing an argument, and the receptive audience he faced, anything less than a success would have been astonishing.

> That hostility to collectivist restrictions on personal freedom, as well as the liking for a competitive order, were somewhat stronger in the University of Chicago's economics department than at most other places (except, and especially, the London School of Economics). (Stigler 1988:139)

Stigler, as always, refrained from pulling his punches when crafting his five lectures. Whether he was ever capable of pulling his punches is another issue. His ability to make a joke at another's expense or to exercise his acerbic tongue on the unwary would become legendary. As expected, Stigler laid into his perceived opponents, including Chamberlin, with all and any weapon that came to hand. As he would prove repeatedly

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Many of the points made in this section are certainly consistent with those made by Jan Keppler (1998). I prefer to see my efforts here as complementary to, rather than as a substitute for, his own insightful work. Keppler accurately picks up a technique that served Stigler well when launching one of his scorched-earth attacks directed at what he deemed to be a dangerous piece of economic heresy. Reading his opponents' work with an intent to destroy usually involved setting up a strawman version of the offending doctrine and then demolishing every last trace of it. He would either ferret out the weakest point of the targeted theory or unconsciously manufacture one if necessary. Once established, he would leverage this vulnerability by characterizing that flaw as representing the revealed inconsistency of the theory in general. However, Stigler often cloaked his narrower objectives by donning an air of ostensible objectivity.

> This text ["Monopolistic Competition in Retrospect"] is interesting not only for historians of economic thought, but also for those researchers interested in rhetoric, as Stigler consistently sustains the attempt to deny monopolistic competition theory any legitimacy as an economic paradigm, while keeping the tone of an even-handed evaluation. (Keppler 1998:267)

throughout his long career, opposing theories and their progenitors were to be shown no mercy.

> … it wasn't just that he criticized Gardiner Means, or that he criticized John Kenneth Galbraith, but he was sarcastic. He was funny, but he was sarcastic. He devastated them. They didn't take it too well. George was just telling them what he thought. He didn't want to hurt anybody's feelings. He wanted to show that there was something wrong with their thinking. But he didn't soften his remarks in order not to hurt their feelings or in order to allow them to save face. He really went after them and did his best to demolish their arguments and, that was George. And you don't make a lot of friends that way. (Conversation with Claire Friedland, October 1997)

The lectures themselves were more than just an attempt to strike back at Chamberlin or the theory of monopolistic competition. Although there was no overt fanfare at the time, these lectures signified, along with a few other signposts, the launch of a counter-revolution against the newly adapted Keynesian orthodoxy and other heretical imposters. This claim might seem to edge into the unwanted realm of overstatement. Certainly nothing equivalent to Luther's posting of his 95 theses on that church door at Wittenberg occurred, but buoyed up by his Mont Pelerin experience, his London appearance marks the start of a dedicated crusade that was to see the Friedman/Stigler approach make the transition from the fringes of orthodoxy to its very center. Neither Friedman nor Stigler seemed to have any doubts of the correctness of their path despite its initial minority status. "One man and the truth is a majority" (Milton Friedman quoted in Samuelson 2011:617).

> I dissent from this talk of a consensus of economists. What is true, I think, is that a proposal should be bandied around for a while in professional circles, because logical errors and empirical oversights are likely to be caught. The consensus notion is dubious because the people who fill the journals,—and they're the ones we tend to judge the profession by—(1) are inherently more different-minded, (2) have vested interests in ideas, (3)

> include some bastards. (Letter from George Stigler to Milton Friedman, October 1947 in Hammond and Hammond 2006:64)[127]

Stigler's arguments seem to consist of colorful bits and pieces carefully woven together like an accumulation of Penelope's many nights of weaving while she patiently waited for the return of Odysseus.[128] The careful scheme of their construction leaves these lectures with something of an impregnable aura. Yet careful examination can reveal the essential, sometimes hidden, threads binding the whole together. A good measured yank may cause the entire fabric of his argument to begin to unravel and even collapse back onto itself. However, the confidence with which these lectures were delivered and his unique style of argumentation would undoubtedly leave Stigler's audience convinced, even if not yet totally committed to his stated position.

> I am writing mainly to swell your head—though God knows it must be big enough already. Hayek reports that your lectures were "brilliant" & successful. Indeed, he said yours were by all odds the most successful series of lectures they had ever had. I didn't realize the state of English Economics had sunk so low—though, come to think of it, Hayek was including pre-war experience, so I guess I'll just have to take it to mean that the English are still smart enough to agree with the rest of us. (Letter from Milton

[127] Others have distinct opinions as to whether Stigler should be included in the genus *bastardium maximus*. What is more interesting is that although he easily sniffed out the vested interests in ideas that economists seem to accumulate throughout their careers, there is little if any innate recognition that he also might fall into this self-same category. Throughout his career he tended to flounder when required to apply his universal standard of narrow self-interest to economists and especially to himself. It was as if the simple recognition that narrow self-interest pervaded human actions provided a golden ladder which allowed economists to escape the very same restricted boundaries that they themselves set.

[128] George Stigler throughout his career retained a wonderful knack for knowing just how to reach and convince an audience, whether face to face or through his writing. He, and his close friend Milton Friedman, essentially had an intuitive sense of how best to market their ideas. Stigler employed a unique pattern of sticking together striking colorful pieces of content drawn from his own wealth of resources.

> I think he was one of the most difficult people to explain because there is no one like him. I've described how in an argument he jumps around. He puts in a bit of theory, a bit of statistics, a reference to the earlier economists. It's like no one else's form of argument that you can recall. (Conversation with Ronald Coase, November 1997)

Friedman to George Stigler, April 7, 1948 in Hammond and Hammond 2006: 80)

The five lectures are all connected, with logic, links and a distinct sense of direction. Together they form a coherent piece of thought. Like any good showman seeking to influence a crowd, Stigler included a perfect warm-up piece in his initial lecture, which alerted his audience to the liberal principles he was seeking to promote and defend, perhaps buoyed up by the meeting at Mont Pelerin. The liberal ethic, of course, had been deep-rooted at Chicago with such stalwarts as Knight, Viner, Simons, Mints and Director certainly wrapped deeply within that fold. The style of liberalism that Stigler and Friedman would eventually develop and embrace did not replicate these older stances. Both certainly broke with Knight's ideas and approaches.

> We were all enormously influenced, I assume,—surely I was—by the teachings of Knight and Viner and Simons and Mints. They did not sing a single song—Knight was incapable of singing in chorus with any man. These men all shared two related traits, however. The first was a fierce intellectual independence: the idea of winning an argument at Chicago by citing a high authority, or prevalent opinion, was simply inconceivable. (Stigler 1976b:4)

Stigler takes his aim at post-war collectivism by examining what he considers to be an unhealthy recent obsession with income redistribution. The issue of income equality, according to his reading of the past, never garnered any serious attention from the classical liberal economists. This was due to a belief in self-improvement, the power of the individual to take responsibility for his own actions and outcomes. Even though Stigler might be unwilling to share Marshall's or Mill's idealism of producing better men, he at least wanted to avoid encouraging an onslaught of inferior men by fostering incapacitating policies that featured income redistribution.[129] In his analysis, rationalizing the right to more equal

[129] Stigler may have pulled back a touch from his original draft, which supported the Marshallian idea of making better men. In a letter, his close friend Milton Friedman indicates the Orwellian tone of this objective.

shares ultimately displayed indulgence rather than kindness. Blunting the need to take on individual responsibility would only produce a society that was distinctly repugnant to George Stigler.[130]

> But we are persuaded that an economic system will not help us to move in the right direction unless it grants both opportunity and responsibility to the individual: the very uncertainty of our ultimate ethical goals dictates a wide area of individual self-determination. We are not able to supply a blueprint of the ideal life, but we are persuaded that even if it were known it would be ideal only for the person who individually and knowingly and voluntarily accepted it. It is not necessary, however to know what it best; it is enough to know what is better. (Stigler 1949c: 8)

Having fully warmed up, Stigler was now ready to unleash a barrage of criticism aimed at Chamberlin's vitals, or at least what Stigler perceived to be the soft underbelly of his argument. Stigler in his criticism was engaging in what might be termed an attempt to eviscerate a theory under the guise of objective analysis. When the objective is to engage in a targeted search and destroy mission, it is often advantageous to present that theory in a somewhat simplified rendition. At least initially, a too nuanced presentation might tend to muddle any critique, making it difficult for an audience to comprehend. The dangerous temptation is to pare down a theory, not only for clarification purposes, but also to further one's own agenda, to hone its marketing versatility and edge.

> Re your solution: "the improvement of the individual" is about as ambiguous a touchstone as "equality." I don't know how to define either. You cite Marshall. In him "the improvement of man" equals the remaking of other peoples into the image of the Englishman, which is warning enough that this slogan has danger of leading to the narrowest kind of presumptuous provincialism. (Letter from Milton Friedman to George Stigler, February 7, 1948 in Hammond and Hammond, 2006:78)

[130] Collectivism arrived through the agency of government intervention which inevitably equated to some form of income redistribution, whether through regulation, taxes or through some other less obvious form. All such interventions weakened individual responsibility. Failure to provide rewards proportional to individual production distorted incentives. More important to Stigler than the idea of protection via some form of social safety net was individual freedom reflected through economic choice. Stigler at his 65th birthday celebration in fact quotes his close friend and ally, Milton Friedman, to this effect. Notice how by doing so he reconfirms his unshakeable belief in his ideas.

> True, the number of citizens who regard compulsory old age insurance as a deprivation of freedom may be few, but the believer in freedom has never counted noses. (Stigler 1976b:1)

Carefully scrutinized, we glimpse a version of the theory constructed as being vulnerable to specific attacks. Equipped with such a providentially tailored Achilles' heel, it comes as no surprise when critical arrows easily find their mark. Skilfully executed, what seemed to be a towering theory appears now to collapse from its own inherent weaknesses and contradictions, rather than from the critic's cleverly applied prosthetic device. An audience, or subsequent readers, are lulled into believing that the analyst did no more than pinpoint the obvious. If executed with sufficient force and finesse, future debate may focus on this hothouse theoretical version rather than the sturdier and more complex original. With the passage of time, too many academics gain a stake in keeping the lame version afoot. Their arguments and counter-arguments have been honed to meet the specific requirements of the ersatz theory. When largely successful, the simplification can supplant and even erase the original. In this case, a tradition, translated into oral folklore, becomes firmly implanted in the daily discourse of the discipline and nearly impossible to uproot.

Stigler knew that to win a verbal sparring contest you had to control the terms of debate in much the same way that securing the commanding heights was essential during a military battle. In this sense Stigler plotted his campaign against Chamberlin in a fashion that might have won a degree of grudging admiration from Clausewitz. Taking a leaf out of the latter's military text, Stigler seemed to believe that verbal jousting was somehow just a legitimate extension of academic discussion and analysis. To gain traction, he frames the theory of monopolistic competition in the most unflattering light possible, one that is bound to distort Chamberlin's theory rather than present a true reflection of it. What Stigler understood was the importance of marketing when attempting to steer specific discussions and ultimately in determining the direction of the economics profession. Part of Stigler's ability to triumph over targeted economic heretics, or at least transform the discussion focused on those theories, was his knack of knowing what the profession wanted to hear and delivering that message in a very comprehensible manner. In his later work with Gary Becker (1977) he knew how to appeal to the profession's preferences, convincing other economists that his work provided a validation of what they would prefer to hear concerning market efficiency. In the challenge presented by monopolistic competition, he was successfully saving appearances, providing evidence that the mainstream approach of perfect competition continued to be valid and dominant in explaining market behavior. The sunk costs invested by the profession in that theory would not then need to be jettisoned.

> George Stigler, I remember when I was a young person, wired and said "Selling is very important in your research. So write better. Work on writing because that is important. You've got to sell what you are doing." I think he's exactly right. You've got to sell what you are doing. It may be that in the long run good ideas do surface but they surface faster, if written in a persuasive fashion. (Conversation with Gary Becker, October 1997)

Adopting this method, Stigler, performing before his London audience, employed a scattershot strategy, with several rounds fired at Robert Triffen (1940), Chamberlin's student, as well. The attack rests its strategy on four widely spaced tent poles. (Notice that when savaging opponents, Stigler often applied the legal tactic of battling on as many fronts as conceivable, a sort of early version of "shock and awe".) In Stigler's reduction of the theory, monopolistic competition, in whatever package it might be presented, suffered from being:

- Inherently inapplicable
- Theoretically inconsistent
- Incapable of providing any additional insights or different results than perfect competition
- Deficient in showing the methodological advantage of pursuing more realistic assumptions

> It was a methodological position, which, only when Milton Friedman published his famous (methodology of positive economics) article in 1954 did I then realise he was using the type of argument that sounded exactly like the kind of things Stigler would drop in his articles. It was a kind of (what shall I call it) a poor man's Popperism. I mean it is basically Karl Popper's falsification with a tremendous emphasis on prediction, etc. And I later realised, discovered this because I asked him, that he and Milton Friedman talked about all these things. Milton however just ran away with it. George Stigler always slightly resented the fact that the entire world learned all this

> stuff from Milton Friedman, when in fact, if you look at the order of precedence, George Stigler was slightly ahead in this sort of attitude to the testing of hypotheses. (Conversation with Mark Blaug, April 1998)

Stigler's work must be read with great care since he was a master when it came to constructing a storyline and embellishing it in ways that best reached his audience. Appearing at the LSE, he immediately tipped off his implicit intent of ostensibly examining the nature and contributions of monopolistic competition by the carefully fashioned title he chose to employ. "Monopolistic Competition in Retrospect" in a precise, yet at the same time not enunciated manner, conveys a sense of looking back on the past, or in this case looking back on a once popular theory which failed to provide any guidance that might further economic knowledge or understanding. In effect, George Stigler adopts the pose of a medical examiner conducting an autopsy on a lifeless corpse. Post mortems might explain the reason for failure without yielding any positive contributions to the living. At best, the title warns you away from dangerous directions of research and investigation. (In this case, anything that drifts too far from systems of unique equilibrium.) One might suspect at the very start that while disassembling the theory in a seemingly detached manner, the result of this particular autopsy will leave behind an empty shell devoid of any practical applicability.

> The title, "Monopolistic competition in retrospect" itself demonstrates the rhetorical character of the argument against monopolistic competition theory. The formulation "in retrospect" implies that the subject under consideration constitutes an historically closed episode on which a final verdict can be formulated, rather than a paradigm of research that has to be evaluated on equal grounds with alternatives. The title reveals also a second interesting rhetorical figure: the phenomenon studied is substituted for the conceptual framework, i.e., "monopolistic competition" for "monopolistic competition theory." This implies that the empirical phenomenon of monopolistic competition itself can be considered a closed chapter. This would not only make the approach itself obsolete, but it would also free any alternative approaches from the need to come to grips with the problems and questions posed by the empirical phenomenon of monopolistic competition. (Keppler 1998:268)

Stigler, when seeking to shred what he perceived as dangerous heresy, acts not unlike a cagey pugilist or hired thug. Right off the bat he starts jabbing away, seeking to soften up his opponent before raining down his more serious blows. He therefore feels obliged to undercut the validity of the theory by raising doubts about the era in which it was conceived. By damning the period you damn all its creations. In fact, the 1930s are dismissed as an anomaly of feverish ideas, which now, in less troubled times, are revealed as being bereft of thought and lacking any practical significance. While doing so, Stigler also manages to twit Chamberlin's obsessive behavior in a casual, but nonetheless, damning manner. From the start, the target is unmistakably outlined.

> Before the Great Depression, that chasm between darkness and light, economists had generally looked upon the economy as a mixture of industries that approximated conditions of perfect competition and industries that were "monopolies." The competitive industries, it was believed, were satisfactorily analysed by the theory of competition, and although the "monopolies" were diverse in structure and power, they could be informatively analysed by a discriminating use of the theory of monopoly … Then came the works of Mrs. Robinson and Professor Chamberlin, who criticized this viewpoint and demanded a new orientation of our thought. Because of the high quality of their volumes, and because it was the "thirties," they were enthusiastically received.[131] Then too, their messages seemed to reinforce

[131] Notice that Stigler has quickly trivialized an attempt to reconstruct economic theory by defining it as a typical 1930s fad, not much different than the zoot suit or dance marathons. Stigler never misses an opportunity to deliver a "zinger" or as Rose Friedman put it (conversation, August 1997), "a smart remark." Thus being previously twitted by Chamberlin for his use of the semi-pretentious term *Weltanschauung*, Stigler feels little compunction in referring to 'Professor Chamberlin's *Weltanschauung*' (1949a:13) when dismembering Chamberlin's work. He then quickly turns around to give his designated opponent a double tap by equating Chamberlin's theory with the despised Institutionalists. This ragtag group, in Stigler's opinion, could also be dismissed as safely buried, with the ground sufficiently salted to protect against any viable resurrection.

> This picture of economic life was not fundamentally new, but Professor Chamberlin's reaction was. Customarily the picture had led to some sort of "institutional" economics, that strange mixture of magnificent methodological pronouncements and skinny, *ad hoc* analyses. Chamberlin, however, persevered to construct an analytical system of recognizable type to deal with the picture: the co-ordinates of his diagrams would be price and quantity, not Church and State. (Stigler 1949a:14)

> one another, but this was a confusion that was quickly detected by, and almost only by, Professor Chamberlin. (Stigler 1949a:12)[132]

In constructing his plan of battle, Stigler trod carefully when presenting Chamberlin's theory, particularly in the aspects he chose to emphasize. Preliminary or opening remarks were meant to reframe the appropriate grounds on which discussion could proceed while simultaneously stripping the theory of some of its dignity. The Chamberlinian group bore much of the brunt of Stigler's attack. It comes then as no surprise that Stigler wasted little time in attempting to poke such a sufficiency of holes in this concept that it ended up resembling a badly gnawed piece of Swiss cheese. The heavier theoretical artillery would shortly follow. Stigler's strategy was based on the argument that if he could demonstrate that a group equilibrium was an empty construct, then Chamberlin was providing nothing of substance, nor anything that was new. Part of the objective in attacking Chamberlin on this point was that the concept itself had no consensual definition. This would allow Stigler to craft his own strategically constructed and inherently impaired version.

The key lies in ignoring or being simply oblivious to the full range of meaning with which Chamberlin imbues this term. In an intuitive sense (and one Chamberlin employs), if firms are competing (are interdependent rivals of one another) they must be selling goods or services recog-

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George Stigler could never forget or even pragmatically disregard economic flavors that at some period must have stuck badly in his craw. Some 35 years later, he couldn't resist giving what he conceived as the institutionalist approach a targeted boot in its most vulnerable aspect.

> I would say the institutional school failed in America for a very simple reason. It had nothing in it except a stance of hostility to the standard theoretical tradition. There was no positive agenda of research, there was no set of problems or new methods they wanted to invoke. (Stigler quoted in Kitch, 1983:170)

[132] With his nose ever alert and capable of detecting the slightest hint of heresy, Stigler had no difficulty in parsing the clear difference between these contending theories. Thus before his audience could even settle in, Joan Robinson is summarily dismissed. She is sent packing, though not without relegating both Robinson's and Chamberlin's efforts to some distant, and now best forgotten past, rather than recognizing both as theories still very much alive at the time. Quite typical is Stigler's implied insistence that he would never waste time on anything other than the most dangerous threats facing what he saw as the canonical approach to price theory.

> Of Mrs. Robinson's work I need say little. It is amply clear, on a re-reading at this distant date, that her message was in no sense revolutionary, although at times her language was rebellious. (Stigler 1949a:12)

nizable as substitutes. But Stigler was incorrect in reducing this to a set of cross-elasticities which change over time for given people. The construction of this angle on the issue seems to exist only for the purpose of being presented and subsequently destroyed. When we talk about the housing market, for instance, we automatically exclude non-housing commodities and services although these certainly compete for income and are to some degree substitutes for one another. Restaurant owners don't consider themselves to be in competition with Manhattan landlords. Moreover, speaking about markets in the very broadest sense is an effective strategy for rendering them absurd. Consider carefully the general market for labor. No such thing precisely exists. There are definitely a number of specific labor markets. But competition between these markets may not exist to any great extent. Analysts understand this bedrock reality and feel no compulsion to point out the obvious. Clumsily aggregating such markets may prove misleading since different compositions can lead to the same broad statistical profiles, yet these distinctive assemblages will often fail to respond in the same way to variable shocks and changes.

Stigler's further attempt to claim that the Chamberlin group consists of only one firm thus doesn't carry any weight, since a firm is clearly incapable of competing with itself in any substantial sense.[133] Firms within the same market compete with one another for customers. This clear intuition underpins any idea of an applicable group. Just as a single firm is only comprehensible in relation to its competitors, we cannot equally extend the idea of a group to comprehend all firms. The relevant criteria do not rest on some simple idea of substitution, despite Stigler's best effort to reduce it to that level. Chamberlin does point this out in his 1937 *Quarterly Journal of Economics* article which Stigler quotes at one point in his lecture, but only with the object of dismissing and destroying it. For Stigler it appears that initial mis-statements are mortal sins, while any subsequent corrections or changes from the original text demonstrate a fatal weakness, indicating unwanted ambiguity or indecision. Stigler

[133] The only concrete way in which a firm competes with itself is in persuading consumers to update their products. This has long been a core strategy of car companies, but is also a feature of other consumer durables including software programs.

seems in a subliminal way to be insisting that theorists "Get it right the first time since any subsequent changes can and will be used against you."

As we follow the path taken in his lecture, the adversarial nature of his argument becomes increasingly evident. Stigler strategically is not seeking to understand what Chamberlin might be trying to say, but rather is only looking for ways to undercut, mince and destroy Chamberlin's theory. This tactic remains the hallmark of a zero-sum game where, as in the courtroom, one side's gain comes at the expense of the opposition. He is fond of quoting quite selectively from the editions of Chamberlin's book, but largely ignores other work which might have usefully clarified points of confusion. He should, for instance, look at the replies, explanations and alterations that appeared in the leading journals of the time. Clearly Stigler had at least read Chamberlin's 1937 *QJE* article, but proceeded to blissfully ignore all the clarifications that appear there, referring to them only in an offhand and slighting fashion, relegating this entire article to an unproductive footnote (1949a:20). After carefully reading Stigler's lecture, the question that should inevitably arise is whether we have gained a useful insight into Chamberlin's work, or have only witnessed a sledgehammer applied skilfully to a despised alternative.

Thus the three initial implications Stigler explicitly desired to draw from Chamberlin's theory were not the result of suspending judgment, but were rather deliberately engineered to provide a later and rather damning assessment of Chamberlin's approach. The first seems based on a key misunderstanding of what Chamberlin was seeking to accomplish. You are welcome to argue that we lack an a priori method for distinguishing where one group ends and another begins. But this seems more a matter of application, and the intricacies of the particular problem under examination, than an inevitable fatal flaw in the theory itself. For that matter, the way in which Stigler unswervingly insists that the group be based on cross-elasticities seems a formal attempt to make the group concept largely inoperative. The second implication, that "products be heterogeneous from a technological viewpoint" (Stigler 1949a:15) was not inaccurate so much as generalized more than was necessary for the theory at hand. However, Stigler's third implication simply failed to ring true to any critical degree. The large or even dominant role that firms outside the competitive group might play was no more or less true than the role such firms played in standard monopoly, or perfectly competitive, theory.

At this preliminary stage of his argument (where he lays out its theoretical implications), Stigler is revealed to have been only toying with Chamberlin and his misbegotten theory of monopolistic competition. Stigler's strategic attack had now reached the point at which it had become appropriate for him to drive a square wooden stake so firmly down into the theory's black heretical heart that the possibility of a future return would be negated for all time. Heresy once again would be rendered harmless. From his perspective, the profession had to be protected from the false enticements of a theory that on its glittery surface seemed the very height of rational and reasonable thinking.[134] However, Chamberlin's approach managed to manufacture an ersatz appeal by casting its net over a wider surface than previous (and more legitimate) contenders, accounting for everyday observations in a more realistic manner.

> "Monopolistic competition" is a challenge to the traditional viewpoint of economics that competition and monopoly are alternatives and that individual prices are to be explained in terms of either the one or the other. By contrast, it is held that most economic situations are composites of both competition and monopoly, and that, wherever this is the case, a false view is given by neglecting either one of the two forces and regarding the situation as made up entirely of the other … It contains, not a technique, but a way of looking at the economic system; and changing one's economic *Weltanschauung* is something very different from looking into the economics of the individual firm or adding new tools to one's kit. (Chamberlin 1937: 570)

As pointed out, Stigler relished using the fog of rhetoric to sell his arguments when possible. His points always seem convincing when bundled together and framed with carefully chosen disparaging remarks that erode the logic of opposing positions. Yet these carefully crafted barbs fail to contain the same degree of faultless logic when they are isolated and

[134] There is a strange parallel with the romantic young Holden Caulfield (Salinger 1951), who yearned to protect the innocence of children by catching them before they tumbled into the iniquities of their adult years. Doubtless, George Stigler himself would have found such a parallel patently absurd. (I am deliberately employing a more polite term here.)

carefully examined individually. The initial impenetrability of that fog often succeeded in working its magic and subsequently shaping the terms of debate surrounding controversial areas. If left to settle, the fog could prove stubbornly difficult to disperse. The objective was to shift a discussion so that it flowed around Stigler's reshaping of a theory, largely disregarding the intention of the original theorist. This held to some degree equally with those theories he was keen to eviscerate as well as those he wished to champion.[135]

> My recollection is not worth much, but for what it's worth is that the Robinsonian emphasis on the individual firm economics, the analysis of marginal revenue and marginal cost, fitted in very well with what we were otherwise thinking. There were no problems about that. And that was clearly part of the agenda. But the Chamberlinian attempt to make it into a theory of the general equilibrium was not. The attempt, as it were, to discuss about closer or less close substitutes in different markets, that kind of thing, trying to talk of an industry of imperfect competitors was not. Now maybe it's only that I'm really going back to George's later discussion, but I think from the very beginning that we got on very much less well with that general approach and those preconceptions. That received less support. (Conversation with Milton Friedman, August 1997)

In the case of monopolistic competition, Stigler seized on the claim that the theory as presented was inherently inoperative. In other words, the logical inconsistencies at the core of Chamberlin's approach made it practically inapplicable. The apparent implications flowing from Chamberlin's revolutionary ideas could not essentially materialize. Appraised in this light, the theory was rendered still-born, hardly worth the eulogy that

[135] A good example is that surrounding the Coase theorem which was basically George Stigler's creation. You might say it was based on a story by Ronald Coase, but as Coase himself made clear at the famous dinner held at Aaron Director's house, he thought that only Arnold Harberger had actually grasped the point he was trying to make (conversation with Ronald Coase, October 1997).

> But Stigler and Friedman jumped on to Ronald Coase and felt that the Coase doctrines about transaction costs and property rights—just get the property rights right then *laissez-faire* could be relied upon—was the lifeline that they sought. Now, all that I know about this part of the story is what's called the Coase Theorem. And that's a coinage of Stigler's. I don't think Coase knew what his theorem was. There's great argumentation as to whether there is a theorem. (Conversation with Paul Samuelson, October 1997)

Stigler had prepared. The focal point in this attack was what Stigler identifies as the Chamberlinian group. Stigler from start to finish left this construction without a shred of dignity or intelligibility. By insisting that the theory stood or fell on the analytical validity of such a construct, he could successfully shift the terms of debate and dismember the theory. Stigler adopted a basically Gertrude Stein approach to the issue by claiming that after carefully examining the group structure, it turned out that "there is no there there" (Stein 1937:289). In other words, once you crease the surface plausibility of what Chamberlin asserted, the concept is revealed to lack any consistent logic, rendering it essentially inapplicable. Stigler could then step aside to let the theory collapse, weighed down by its own absurd conceits.

> The importance of the group concept for the theory of monopolistic competition must be emphasized … The group is no mere expedient to get the analysis started, it is the vehicle of Chamberlin's theory of interdependence of products. (Stigler 1949a: 15–160)

Stigler simply, for his own strategic reasons, made more of an issue of Chamberlinian groups than was honestly warranted. This was especially true if considered in light of Chamberlin's own recognition of the problem in his 1937 *QJE* article. Stigler insisted on taking the idea of the group precisely and literally. This misses much of the point. Chamberlin was using this device as a heuristic organizing principle. But this is no more than what the theory of perfect competition does. Chamberlin essentially wanted to discard the view of firms as passive and independent of one another. To do so he allowed the simultaneous elements of competition and monopoly to work together. Accomplishing this, while maintaining his Marshallian roots, meant that Chamberlin required a method that would allow economists to examine any given subset of interdependent firms, depending on the problem at hand. The group construct served this purpose. How well it could achieve this end was largely an empirical issue. As Chamberlin admits in that 1937 paper, talking about substitution rates (those pesky cross-elasticities of demand) was likely to be unsatisfactory and far too arbitrary. It was an approach that faced the same bundle of problems that Stigler (inspired to a certain degree by Friedman's book review (1941)) would gleefully enunciate some eleven years later. Many of the objections that Stigler raised throughout his lecture could in fact be erased in the process of applying practical sense when doing some applied empirical work (assuming the investigator hadn't been raised by wolves or lived most of his life outside human society).

But this is exactly where Stigler's rigid categorical vision of economics clashes with that of Chamberlin. Stigler is determined to save appearances by fending off any attack on the Marshallian supply and demand apparatus. By doing so, he falls away from Marshall himself and places himself in that rather amorphous territory of being a loyal but faithless Marshallian.[136] The employment of the group is meant to be a heuristic to allow for easier analysis. In this Chamberlin is much closer to the spirit of Marshall than is Stigler. As previously mentioned, models for Marshall are closer to a hat rack from which can be dangled many an interesting story. Marshall uses them as storytelling devices, rather than as machines employed to mechanically grind out duly rigorous results. Playing somewhat fast and loose with equilibrium analysis equates to a permanent red card in Stigler's view. Theories that violate or stray outside this boundary earn from this merciless referee an automatic disqualification.

> I'm not speaking with any great confidence here, but I think his mind always worked in an equilibrium framework. It didn't deviate to any great degree from this. For example, in "The Economics of Information," he wants to find out what is the equilibrium quantity of information that's going to be collected. Even in something like delivered price systems where he's looking at something like the Pittsburgh cross pricing system and the cross hauling system, he wants to know what the equilibrium amount of a shipment in each direction is going to be. I don't recall him ever adopting a problem to investigate in which you couldn't pose some kind of equilibrium arrangement that he could then seek the answers to. Whether that was what motivated his opposition to some of the theories we spoke of, I don't know. But it is clear in the case of Chamberlin, that he was very much

[136] It is highly questionable whether Marshall would classify himself as a Marshallian. The gulf between a formulator and self-proclaimed acolytes is reminiscent of the pointed remark attributed to Marx: "*Ce qu'il y a de certain c'est que moi, je ne suis pas Marxiste*" ("what is certain is that I myself am not a Marxist") (quoted by Engels in a letter to Eduard Bernstein, Marx and Engels 1985: 388).

> concerned with what he considered as logical gaps. One could interpret these as not letting one get to an equilibrium result in the analysis by using the tool kit that was set forth by Chamberlin. (Conversation with Harold Demsetz, October 1997)

If his objective was to present a balanced analysis, Stigler should have readily compared the practical difficulties of defining the group or market in Chamberlin's conception with those found in his more favored alternative of perfect competition. The emphasis here should be on specific, concrete difficulties, since that is the type used by Stigler to sandbag Chamberlin's approach. He conjures up a New York City accommodation market in which any effort to group the varied offerings is portrayed as simply absurd.

> Again we must pause: the uniformity assumption is only temporary, we are promised, but even a temporary assumption should be meaningful. How can different products have uniform costs and demands? The quantity axes of the various product diagrams are simply not the same: one measures three-room apartments, another four-room houses, and perhaps still another, restaurant meals an excellent substitute for a kitchen). We cannot translate one into another by the ratio of their prices, for we are constructing the apparatus to explain prices. We do not wish to say that two physically similar apartments are "really" the same. They are not the same if their prices differ, and perhaps even if they do not differ—this is the fundamental picture. And we do wish to say that restaurant meals plus a bedroom may form a better substitute for a Manhattan apartment than does a Brooklyn apartment—this is also part of the picture. (Stigler 1949a: 16)

Presented in this very stylized, unfavorable light, Chamberlin's concept could be reduced quite easily to no more than childish nonsense. Locating an apparent deadly inconsistency means that monopolistic competition will necessarily yield no testable hypothesis or any verifiable predictions. This conclusive estimate could not be otherwise since the very data on which empirical analysis must rest seems virtually impossible to aggregate. Each individual accommodation must represent a separate group. Each distinctive instance thus becomes an unviable market surrounded by other numerous close or distant substitutes. Monopoly theory triumphs with Chamberlin's alternative a clearly non-functional option.

> The effect of diversity of demand and cost conditions is even more devastating: there may be monopoly profits throughout the group at equilibrium—and then again, there may not. Indeed, although Professor Chamberlin does not state the possibility, it is not even clear that equilibrium is attainable: under these vague conditions prices may continue to change, and new firms may continue to enter and old firms continue to leave the "group." The indeterminacy is especially likely if we recognize variety through time—the consumers' liking for novelty, which Professor Chamberlin should surely add to his picture. (Stigler 1949a: 18)

This fusillade of attacks seemed to strike home. To be honest, monopolistic competition appeared in part as a theory still under construction. However, this shortcoming, of whatever extent, should hardly have been surprising given the lack of maturity and still experimental nature of these ideas. The associated models and methods could hardly have been expected to match those of such mainstream theories as perfect competition, which itself was still in a development phase. A theory not quite ready for prime time should not be summarily rejected based on the tentative quality of its reasoning. Leaping to equate flawed with fatally flawed doesn't signal a road that might lead to greater understanding.

Still, even given the theory's state of development, some of Stigler's thrusts fail to have the ring of sound judgment, with others resembling tactical courtroom disparagements. The problem with groups and substitutes as formulated was deliberately expressed so as to appear in its most unflattering light. Perhaps retroactively defining, or at least dispassionately examining, these as markets might clarify the problem. To speak of a housing market in New York doesn't seem to require a leap of imagination. When the price of the median or mean house or apartment unit for a particular urban region is listed, the result is not universal bafflement. Heterogeneous, individual demand curves could potentially be constructed and aggregated without the necessity of either homogeneous products or identical consumers. Within any larger market there would inevitably be submarkets which can be further classified until focus remained on the individual home or the output of an individual firm. But the problems don't appear to be insurmountable. If we consider a particular service, for example, hotel accommodation in Tokyo, the range is extremely wide depending on the room or rooms themselves, location and other associated services and facilities. Prices range from inexpensive to hardly affordable. Yet it doesn't strain the conceptual imagination to understand what one means by analysing the Tokyo hotel market, even if each hotel cannot seriously be considered as a close substitute for another.[137] Hotel operators within a particular market know who their competitors are and recognize them as such. On the supply side, it is equally valid to think of what hotel accommodation is available at different prices. Using the simple idea of opportunity cost must inevitably yield some sort of supply schedule.[138]

Chamberlin had tried to explain why this approach could be approximately true or potentially useful. Stigler obsessively focuses on the differences between each and every apartment in New York in order to destroy the logic behind the group concept. His chief weapons seem, at least at times, to be dismissive language and ridicule. Perfect competition deftly avoids the problems Stigler poses by constructing ideal markets instead. But the key characteristics defining the issue posed appear to be deliberately muddled by Stigler.

Apartments can consist of approximately the same living space even though layout and rooms might vary. So we could, merely as an example, start by thinking of similar apartments with locational differences, and with those apartments being uniformly distributed. They may be distinguished by perceived difference (architects who designed them or distinctions in their outward façade), whether these perceived difference are typically rational or not, as Chamberlin was anxious to point out. Stigler seemed to be going out of his way to make this an insurmountable issue by exaggerating the differences, no matter how superficial, that inevitably crop up, rather than examining how the market actually operates. The

[137] The more recent advent of such services as Airbnb further complicates the task. But still, those wishing for short-term accommodation seem to have little difficulty in concretely conceptualizing the appropriate market and identifying those who are competing for their selection.

[138] Quality and the level of services provided at a given establishment is often offered, at least roughly, by a designated star system.

idea of the group as a discernible market cannot be dismissed as devoid of meaning, though whether it leads to any useful form of analysis is another issue entirely. The additional factor that groups or markets may depend on the particular focus of the investigator does not categorically form a legitimate basis for denying the concepts of any potential value.

Stigler proceeded to torture, if not thrash, Chamberlin's text by lifting lines out of context and brandishing them for all to see as evidence of the theory's fallibility. (This practise is not entirely removed from the former English tradition of spiking the heads of traitors on London Bridge. Appropriate, given the location of the lectures.) Stigler insisted that by "the uniformity assumption, Chamberlin had implicitly defined the group as a collection of physically homogeneous products. The identity of costs and demands is otherwise meaningless, and so also is the demand curve he proceeds to draw for a firm on the assumption that 'competitors' prices are always identical' (p. 90). We simply cannot attach meaning to the statement that physically diverse things have the same price." (Stigler 1949a:16) Stigler's statement here fails to make watertight logic.

What Chamberlin is actually trying to explain in this snippet removed from its context is how a single firm can face two demand curves. One is determined by a competitor that matches any price change and the other where the competitor fails to respond. This approach should not pose a serious problem. Soft-drink manufacturers can instigate a price war where they unswervingly decide to match any and all opposing prices. Airlines sell very similar services and may also choose to match prices. Stigler simply asserts that this framework fails to offer any useful analysis. But such a conclusion can only rightly be based on empirical findings. Stigler, however, is arguing that the theory is not even worth the briefest of attention because it is fundamentally illogical. Thus Stigler seems to be veering into the territory of descriptive reality despite having vanquished economic analysis from inhabiting what he has defined as a barren landscape. Quite frankly, Stigler here seems to be vehemently protesting that by definition, namely Stigler's craftily supplied definition, Chamberlin's statements are a contradiction in terms, making his analysis inherently inapplicable.

Stigler continued this eviscerating attack in his own particularly relentless fashion. Again, his strategy is to leave the theory bereft of any shreds of redeeming value. “We simply cannot attach meaning to the statement that physically diverse things have the same price. This physical homogeneity possibly destroys, at least temporarily, Chamberlin’s monopolistic competition (except for spatially distributed firms), for he has also assumed that buyers have perfect knowledge (p. 73), in order further to simplify the analysis. With perfect knowledge and homogeneous products, must not the demand curve confronting each firm be infinitely elastic?” (Stigler 1949a:16–17) Again we have to carefully trace out the way in which Stigler is trying to corner and condemn Chamberlin. He had previously twisted a statement by Chamberlin, “We therefore proceed under the heroic assumption that both demand and cost curves for all the ‘products’ are uniform throughout the group (p. 82)” (Stigler 1949a:16) into the equivalent of assuming homogeneous products. From homogeneity it is a small step to take a comment about perfect knowledge out of context, stir lightly and produce infinitely elastic demand curves previously disguised by the monopolistic competition trademark.

The actual full quote by Chamberlin about knowledge refers to the problem of advertising. “For the present, then, advertising as a competitive activity is put to one side, and attention confined to the two variables of price and ‘product.’ This may be done by proceeding explicitly on the assumption of given wants and perfect knowledge concerning the means available for satisfying them.” (Chamberlin 1933:72–73) Notice the assumptions are there not to vaguely simplify the analysis, as Stigler (1949a:16) brashly claims, but to delay the complicating discussion on advertising or selling costs. If consumers have given wants and perfect knowledge concerning the means available for satisfying them, obviously there is no need for advertising. This simply means that consumers are aware of whatever differences they find to be significant. It doesn’t automatically mean that products can’t be differentiated by those self-same consumers. Chamberlin takes this initial step because in many ways, selling costs will turn out to be crucial for his analysis and should logically be introduced only after the preliminary apparatus is in place. This is what he intends to convey by implicitly asking permission to make heroic assumptions. Stigler in fact misleads by not stating the driving force behind this potentially useful simplification. But this merely helps to demonstrate that he is not intent on explication, but rather on revealing the theory to be empty by having it collapse into the familiar form of perfect competition.[139] Stigler here appears intent on performing the rhetorical equivalent of prestidigitation, a persuasive form that eschews careful logical analysis.

Stigler wants to claim that Chamberlin's heroic assumptions erase the diversity or differentiation of products that lies at the heart of this mixture of market power and competition. It is this removal that would ensure that the theory was essentially vacuous and therefore of little value. However, a more extended quote than Stigler is willing to provide at least makes Chamberlin's aims clearer. Remember that Chamberlin was struggling to construct a Marshall-like scaffolding from which to hang his more complex story. Like Marshall (and Keynes in a macroeconomic context) he was trying to fit a tale of dynamic change and complex interactions into a simple static framework. This challenge may be doomed from the start, but it is only fair to try to understand the attempt, rather than peremptorily dismissing it out of hand.

> Meanwhile, it may be remarked that diversity of "product" is not entirely eliminated under our assumption. It is required only that consumers' preferences be evenly distributed among the different varieties, and that differences between them be not such as to give rise to difference in cost. This might be approximately true where very similar products were differentiated by trade-marks. It is also approximately realized in the fairly even

[139] Stigler takes great delight in tormenting his chosen victim with tiny barbed comments. Chamberlin made it clear in his 1938 article (569n) that he had previously used the number 100 merely for illustrative purposes. In his original volume (1933:49), Chamberlin tried to distinguish interaction between firms when there are only a few players and when there are many. With a few players, any change in price by a single seller would have a major impact on the others in that market. Conversely, with numerous sellers on hand, a similar price change would have only a negligible impact on the rest of the firms. This is the assumption that Chamberlin at least wanted to make. He used the number 100 as shorthand to refer to a large number. Stigler takes up the identical number merely as another means to nail its author. This is not to say that Chamberlin's assumption yields a useful apparatus. But once again, Stigler is not entitled, based on his own constructed standards, to simply dismiss, ridicule or laugh off these assumptions by making a priori assertions or by appealing to his own economic vision. Stigler is trying too hard, seemingly driven by a desire not simply to show the problems facing this theory, but rather to eradicate its presence and even its memory.

> So we have 100 products of various sorts (blinking the inconsistency) or of one sort, but with negatively sloping demand curves (dropping the assumption of perfect knowledge). Our vision tells us that we are unlikely to find symmetry, continuity, or any sort of smoothness in the relationships among these products. (Stigler 1949a:17)

geographical distribution of small retail establishments in the outlying districts of a city. (Chamberlin 1933:82–83)

Maintaining his unrelenting attack, Stigler persisted with his carefully structured demolition of the theory's logic, but was persistently nagged by the difficulty of keeping a residual smirk out of his lecturing tone. "But the uniformity assumption is only temporary, we recall" (Stigler 1949a:17). What Chamberlin was attempting by his constructions should be obvious to a more unbiased reader. He was trying to lay out the mechanics of his framework, namely how firms interact within an industry or market and what could be said about individual firm equilibrium relative to industry prices and output. It is true that by doing so he is necessarily abstracting away from the crucial element of his theory (demand reactions) in order to generalize. This problem is not posed in Joan Robinson's world since her demand curves slope down when faced with the necessity of dealing with production under increasing returns to scale. The developed framework may not turn out to provide much insight. But ridiculing Chamberlin for making the attempt to abstract and generalize, the exact procedure that Stigler is wont to extol, seems a curious and rather disjointed response. No doubt Stigler did think that the theory lacked plausibility and usefulness. But the issue is not whether the construction is poorly done. The point is whether trying to dismember a theory through the use of ridicule is likely to advance academic discourse, no matter how amusing the result might be.[140]

[140] There is no doubt that Stigler could be and was often amusing. He was without question one of the wittiest economists on record. Not that historically he faced much competition. The economics profession has never been categorized as a discipline largely composed of distinctively fun-loving members who are given to making sly rejoinders. Alfred Marshall never left his colleagues rolling in the aisles. Whether he could even make Mary smile is open to question. But by any imaginable benchmark, George Stigler had a rare cutting wit. He enjoyed battles against or conversations with those who could come close to matching him blow for blow. His close friendship with Robert Solow, the economics version of *The Odd Couple*, seemed based on two people with equally quick minds who were both willing to take as good as they gave. However, his lack of a filtering device, no matter what the social interaction, meant that he was often witty at another's expense.

> I didn't bear the brunt of it because he liked me, so people said. But he gave it to people. He had such a quick wit about him. And very few people had his wit. He could always come up with something. Often it was at people's expense. As they got to know him, they knew he didn't mean anything by it and it was just that he had this wit about him. But people who

Once again, Stigler served up a damning claim that Chamberlin's pet theory was empty of content. Any focus on such a one-dimensional conceptualization must by definition be a cardinal waste of valuable time. In one sense we can understand this specific lecture as a retelling of "The Emperor's New Clothes" with Stigler assuming the curious role of the innocent young child. His strategy is to apply a relentless logic which is capable of demonstrating that if Chamberlin's assumptions were taken to their logical conclusion, monopolistic competition, in effect, is bereft of the ability of adding anything of value to the standard approach. Stigler didactically states, "We have utterly abandoned the picture with which our analytical technique was designed to deal: there is no variety and there is only one possible type of inter-relationship between products. We probably have a Marshallian industry." (Stigler 1949a:17) The accuracy of his statement here is more than a little dubious since Stigler in a neat twist evaluated this preliminary heuristic device as though it were the end and ultimate objective of Chamberlin's story. This assessment is merely a bit of legerdemain on Stigler's part. He deliberately transforms a plot device into a conclusion.[141] The legitimacy of unilaterally claiming that an analytical technique has been abandoned, despite a clear-cut deficiency of evidence, is difficult to justify. Discovering a total lack of variety by misinterpreting an author's intention is overstating the situation, to put it mildly.

Stigler tried quite skilfully to show that Chamberlin's demand curves, under the terms of his own theory, are not negatively sloped. In effect, he was trying to perform a type of brain surgery on the theory. Devoid of differentiation, shorn of any market power, monopolistic competition is bereft of its essential foundation, a lifeless, moribund hulk of analysis.

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didn't know him, they could be very hurt by it. (Conversation with Gary Becker, October 1997)

[141] In the Stiglerian version of Cinderella, the story would end with poor Cinderella rushing back from the ball and condemned to spend the rest of her life as a household drudge to her stepmother and stepsisters. The moral Stigler would draw was that individuals need to take responsibility for their own life and not waste their time hoping to be rescued by fairy godmothers or charming princes. (These two *deus ex machina*s in this retelling represent the dead hand of government or collectivist intervention that provides only spurious external assistance, a disruption that ultimately distorts the realm of individual choice and freedom. In Stigler's stories, only tax collectors and government bureaucrats arrive, rather than cheerful fairy godmothers or princes of any variety.)

Stigler varied his attack with a the claim that "Possibly of more importance is the finding that even under these extreme conditions our new variable, 'product,' cannot be 'measured along an axis' (p. 79)—that is, cannot be measured" (Stigler 1949a: 17). A habit of selective quoting is behavior that any economist should learn to shun. Chamberlin's exact quote and meaning is otherwise. "Another peculiarity is that 'product' variations are in their essence qualitative, rather than quantitative: they cannot, therefore, be measured along an axis and displayed in a single diagram. Resort must be had instead, to the somewhat clumsy expedient of imagining a series of diagrams, one for each variety of 'product'." (Chamberlin 1933: 79)

Chamberlin then proceeded to analyse price and quantity using an example of two different products diagrammatically. Stigler's subsequent use of a footnote further quoting Chamberlin fails to provide substantive clarification, since he allows himself to skip to page 97 of that lengthy volume where Chamberlin is trying to describe the difficulty in defining what an equilibrium state might be where products are themselves variable. Here Chamberlin is attempting to supply a definitional tethering for his theory. At equilibrium there is no incentive to change the existing status quo. Chamberlin does not pose this as a solution. Instead the claim is only that when we look at product variation as a variable, the product will cease to vary when it can no longer benefit its producer to do so. This is hardly worthy of Stigler's observation since the notion is practically definitional. Certainly Chamberlin's system lacks any guarantee of forces pushing the system back into an equilibrium state. "Indeed, although Professor Chamberlin does not state the possibility, it is not even clear that equilibrium is attainable: under these vague conditions price may continue to change, and new firms may continue to enter and old firms continue to leave the 'group'" (Stigler 1949a:18). But such criticism could be levied against perfect competition as well. Potential equilibriums may theoretically exist without being attainable. Achievement is then accomplished only by imposing restrictive constraints on the relevant markets. Stigler saw fit to sneer at the indeterminacy of Chamberlin's version of equilibrium. Obviously, quite a lot of case-specific information (very possibly unattainable) would be required prior to describing such a precise state. But this problem would seem to be the case with any interdependent firms of the type Chamberlin describes. However, simply poking a few holes in a theory does not necessarily unravel it. Stigler at most demonstrated that monopolistic competition, as it was then conceived, was not yet applicable, rather than being by its own construction intrinsically inapplicable.

Equally it does not seem sound to use the standards of perfect competition to reject another theory based on an incompatible approach. Equilibrium in mainstream theory is represented as a pricing point with gravitational attraction. Absent any shocks, changes or intervention, market forces drive prices to a market clearing level where neither buyers nor sellers see any advantage to altering their current strategy.[142] Stigler has no problem employing these simplifications. Although with assurance equal and opposite to that of George Stigler, the claim might be lodged that it is inconceivable to see how a system would ever get out of an equilibrium position, given identical and complete knowledge. Adjustments would need to be accomplished instantaneously. But systems underpinned by unique equilibriums have a distinct advantage over multiple or non-equilibrium models. Even a supply and demand model built upon the shaky foundations of perfect competition realizes a unique equilibrium, one that is distinguished by each factor of production receiving the value of its marginal product as its reward. Given this distributional fairness and efficiency, at least in Stigler's view, the justification for government intervention remains slim, if not non-existent. Non-equilibrium analysis disturbs the balance of such a salubrious world. Equally, what might then appear to be a tendency for market systems to operate at some sub-capacity level could be used to explain under- and unemployment, providing yet another rationale for government intervention. These implications

[142] If anything, comparative statics, strictly defined, is even more modest. The extent of its claims is that under a given set of circumstances there is an equilibrium set of prices that would clear all markets. (In the case of partial equilibrium, only one market would be under examination at a time. The conditions underlying all other markets would remain constant.) If that environment changed, then the equilibrium set of prices would necessarily shift as well. Consequently, an analyst would be able to compare one set of equilibrium prices with another. However, in this limited scenario, movement from one equilibrium to the next is outside the scope of the constructed framework.

alone would impel Stigler to see the warts of one theory and the beauty marks of the other.[143]

> [In] the earlier writings on the theory of monopolistic competition, when it first came out, it's clear that an important aspect was the excess capacity theorem that produced an equilibrium solution to the left of the minimum point on the average cost curve. Excess capacity is in some sense consistent with unemployment and the Depression in the '30s and gave a possible explanation for this. These policy conclusions did not flow out of Chamberlin's mind, but it flowed out of the literature that followed on from Chamberlin. (Conversation with Harold Demsetz, October 1997)

Given his target market (academic economists) and their learned proclivities, Chamberlin's use of normal profits, with its associated tangency condition, is meant as a starting point to assuage an audience of economists weaned on such concepts. But given a theory that has as its central story a picture of individual sellers competing for buyers through a variety of enticements, there is no reason why each differentiated seller must end up just covering its opportunity costs.[144] Even with other firms entering the broader market, profits can persist and vary between participants. Far from demanding a leap in one's analytical imagination, such occurrences are far too common to appear remarkable.

> It is the notion that monopolistic competition is concerned only with situations where the demand and cost curves are tangent, hence where there

[143] Chamberlin himself was not advocating anything resembling an activist political agenda. Stigler's concern was not with Chamberlin's intentions, but rather the implicit implications his theory held. In a sense, the potential problem lay in placing this tool of mass theoretical destruction (insofar as perfect competition was concerned) in the hands of academic bomb throwers and assorted collectivists.

[144] What may be overlooked is that while Robinson's (1932) work was focused on the production side, a response to Sraffa's (1926) issue of increasing returns to scale, Chamberlin was largely concerned with the demand side of the equation.

> The matter might be expressed in another way by saying that "increasing returns" are neither *necessary* nor *sufficient* for monopolistic competition. They are not necessary because it is possible for the demand curve to lie above the cost curve in such a way that marginal revenue and marginal cost intersect above and to the right of the point of minimum average cost. They are not sufficient because a horizontal demand curve makes equilibrium within the "increasing returns" phase of the cost curve impossible. (Chamberlin 1937: 561n.3)

> are no monopoly profits, whereas any situation where there are such profits is to be classed as a monopoly. A moment's reflection will show that this is an artificial distinction … It may perhaps be accounted for by the over-prominence given to this solution in my own statement of the theory. All that need be done here is to call attention to passages where it is made clear that the solution of tangency flows from certain heroic assumptions which are later dropped, and is to be regarded as of only limited direct applicability, being mainly an expositional device, which represents an intermediate stage in the development of the theory. (Chamberlin 1937: 561)

Saying that a theory is inherently inoperable is naturally a much bolder statement than merely pointing out that it has yet to be made applicable in anything resembling a productive manner. The theory itself was still in its comparative infancy in 1948 so that it would perhaps be a mistake to take the young child for a potentially more mature adult version.[145] The rapid support for the theory in the 1930s had not been predicated on any empirical validation (nor had the enthusiasm for Keynesian analysis been data based). Of course the same could be acknowledged in regard to perfect competition. Measurement and quantitative testing was hardly par for the course at that time. It is a little harsh to penalize one theory for sins shared widely.

> The fact that supporters of the theory of monopolistic competition had not made empirical tests comparing the predictions of the alternative theories of competition (and, I may add, do not appear to have made such tests in the years since Stigler wrote) lends support to the view that Friedman's methodology is not positive but a normative theory. Certainly this is the way Stigler used it. Stigler was not saying that supporters of the theory of

[145] Stigler's patience with a theory seemed unreasonably limited. Gestation can take time. An immediate "put up or shut up" might do in courtroom struggles where gaining the upper hand rather than achieving insight or even justice prevails. But it becomes counter-productive when nurtured into a ruling academic passion.

> If you look at the larger "what do we know as a result of this approach?", of course these approaches have a certain amount of application, there are a few things we've learned about the firm, but I'm talking relative to the time spent. I mean there are serious folks now, his students, who understand that there is a problem and are trying to fix it. But you know what George would have said. "Twenty years! … Or whatever it is … Thirty years! They've been working on this stuff. That's getting to the end of the short run." Quote, unquote, he would say that. (Conversation with Sam Peltzman, October 1997)

> monopolistic competition made such tests but did them badly and so came to the wrong conclusion. He was saying that they did not make them at all. (Coase 1994:23)

But it remains unclear why the problem of applicability is eased in the case of perfectly competitive markets. Certainly the theoretical issues are largely removed. Given identical firms and consumers, aggregating poses no problem. However, doing any serious empirical work that tests markets for bread, cigarettes or nails is another matter. Only quite differentiated markets are observed. Yet the problems which Stigler in his Torquemada mode found insurmountable never seemed to give him pause in his own empirical work. Certainly in his efforts with Jim Kindahl (1970), which entailed improving on the Bureau of Labor Statistics price indices, he was faced with hundreds if not thousands of different types of nails sold by a number of different manufacturers. Yet, this did not thwart his attempt to improve available price data in broad categories. The point is that in actual applied work, perfect competition seems to face much the same problems as Chamberlin's work did, yet Stigler fails to explain the practical advantages of one approach relative to the other.

But Stigler, in his final, formidable blow, uses a bit of logical jujitsu to turn Chamberlin's claim to reality on its head so that an advantage is turned inside out into a leading liability.[146] He does this by essentially attempting to close off the methodological debate then current in the profession. His strategy at first glance looks unexceptional. "The purpose of the study of economics is to permit us to make predictions about the behaviour of economic phenomena under specified conditions. The sole test of the usefulness of an economic theory is the concordance between its predictions and the observable course of events." (Stigler 1949a:23) Few would argue that our search for theories should lead us to those that yield inconsistent and even wrong predictions and conclusions. But to go no further seems self-defeating. If all that was required were useful predictions, then Ptolemaic astronomy would meet the extent of

[146] Prior to his methodological uppercut, Stigler dismisses the advantages of Chamberlin's reality as merely illusory.

> Chamberlin's picture of reality has finally led, when consistently followed, to the familiar reaction: *ad hoc* empiricism. (Stigler 1949a:22)

this qualification and Newtonian physics would satisfy such scientific standards. Consequently, it is difficult to feign enthusiasm when Stigler categorically asserts that "One can but show that a theory is unrealistic in essentials by demonstrating that its predictions are wrong" (Stigler 1949a:23). Certainly other factors can be driving such a discord. Nor will concordance between the two lead to any judgment on the reality of any underlying assumptions.

This is a strange charge to level against Chamberlin since he does not actually stake his case on the strength of descriptive reality. Chamberlin (1933:9) makes much the same argument against perfect competition that Stigler raises to destroy the concept of the group in monopolistic competition. Namely, formulating a way to apply the homogenous, abstract markets of perfect competition to the reality of market diversity is not without its own daunting complexities. The test for Stigler is to formulate hypotheses and to then apply quantitative methods to test their validity. But Chamberlin finds no evidence that perfect competition conforms more closely to observed data than does his own attempt at explication. In effect, they share the same acre of quicksand between them. Stigler's case might be stronger if he could demonstrate that his preferred approach tackles the corkscrew issues he raises rather than merely condemning an alternative approach for these perceived failures. In fact, Stigler's complaints remarkably parallel those of Chamberlin.

> Consider, for instance, the competitive organized market for a homogeneous product. Consider, for instance, the competitive analysis as applied to the automobile industry. How is one to conceive of demand and supply curves for "automobiles in general" when, owing to variations in quality, design, and type, the prices of individual units range from several hundred to many thousands of dollars? How define the number of units which would be taken from or put upon the market at any particular price? How fit into the analysis a wide variety of costs based mostly upon a correspondingly wide variety of product? These difficulties are great; perhaps they are not insurmountable … Competitive theory does not fit because competition throughout the group is only partial and is highly uneven. The competition between sport roadsters and ten-ton trucks must be virtually zero; and there is probably more justification for drawing up a joint demand schedule for Fords and house rooms than for Fords and Locomobiles …

> The theory of pure competition, in explaining the adjustment of economic forces in such an industry, is a complete misfit. (Chamberlin 1933: 9–10)

Behind this surprising claim is something much more fundamental for George Stigler. In the same way that Alfred Marshall wanted to professionalize economics, transforming it from the moral science of political economy, Stigler saw himself as steering the discipline onto a course which would nudge it, or if needs be shove the profession, into being more like the physical sciences. To accomplish this transition, economic theory had to expand its reach by focusing on similarities rather than emphasizing differences. Aiming for general, comprehensive and consistent theories required very abstract and stripped-down assumptions.

> Often a theory is criticized or rejected because its assumptions are "unrealistic." Granting for a moment that this charge has meaning, it burdens theory with an additional function, that of description. This is a most unreasonable burden to place upon a theory: the role of description is to particularize, while the role of theory is to generalize—to disregard an infinite number of differences and capture the important common element in different phenomena. (Stigler 1949a:23)

He accordingly called his textbook *The Theory of Price* for good reason. Progress in economic theory could come only through applying a method and a model to ever-widening spheres of observation. Therefore what was similar about observations had to clearly dominate the differences. George Stigler was able to touch a chord of the Gertrude Stein hidden within him by emphasizing that a market is a market is a market. Thus whatever implied an exchange of some sort, with associated price-like signals, fell easily into the domain of the one true theory, the one theory that was formulated to rule them all.[147]

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[147] This imperative partly explains the meeting of minds between Gary Becker and George Stigler. Unlike Knight, who saw the useful boundaries of economics steadily constricting as he grew older, his two students preferred to push those boundaries, to turn the theory of markets into something resembling the theory of human activity.

> Clearly, he was anxious to do that. That's partly … are you going to talk to Becker? I was George's student, in some sense I'm viewed as his protégé, something like that. Personally, Gary and George were much closer than George and I. Much closer and in many ways, I think, they were much more on the same wavelength, especially in this area, how far you

> Well George did not think that differences were so important for economic analysis. You wanted to understand prices, demand and supply? You could use the same kind of model no matter where you applied it and you didn't have to have a really special model for it. I think that was why he was not a fan of the 1930s and '40s industry studies. They thought every industry was unique. I think that was one of the consequences of the Chamberlin monopolistic competition model and he didn't see any useful generalization coming out of that. He was always interested in generalizations. And he was not interested in explaining the particular as much as he was in the generalization that you could deduce. (Conversation with Harold Demsetz, October 1997)

What turns Stigler's efforts away from a reasonable attempt to evaluate a problematic theory is his focusing solely on Chamberlin's book instead of supplementing his analysis with the various attempts by the author to clarify his position and respond to various criticisms, no matter how testy that author might have become. The exchange with Kaldor (1938) might have broadened Chamberlin's take on his theory. Certainly his 1937 paper should not be casually ignored. Unfortunately, Stigler's only footnoted mention of this very relevant paper is to take the last sentence of one of Chamberlin's own footnotes as evidence that the group must be discarded or be based on some other qualification which Stigler regards as belonging to a retro-Marshallian understanding.

> It is not meant by this argument to discard completely the concept of an "industry." In many connections, it is obviously useful to delimit a portion of the economic system and study it in some degree of isolation from the rest. And if this can be done, it is not wholly without meaning to speak of the relative ease with which this particular field may be entered. One emerges from any attempt to classify industries, however, with a feeling that it is all exceedingly arbitrary. It seems much easier and more defensible

could push rational choice, that kind of stuff. George was absolutely enthusiastic about everything Gary was doing, with the family, with marriage, with this, with that, with everything. But the outside world apparently wasn't. And apropos to the outside world, the graduate students at the end of the year would put on a party where they would perform a play: *The Economics of Mud* by Gary Becker. *The economics of "fill in the gap*," by Gary Becker, you know what I mean. It was a standing joke. It has become a standing joke. But George loved that kind of thing. (Conversation with Sam Peltzman, October 1997)

> to set up classifications based upon technological criteria than upon the possibility of market substitution. (Chamberlin 1937:568)

Perhaps the critical blow to monopolistic competition is Stigler's dismissal of it on the grounds that it offers to analysts no more than the theory of perfect competition already generates with a noticeably less complex model.[148] According to Stigler (and Friedman as well), when asked to display their theory's advantages, proponents of this approach must inevitably come up empty-handed. It is this claim, more than anything else, which ultimately allowed Stigler to shred the theory by savagely wielding Occam's razor.[149] Yielding no more than existing approaches, while generating much more fuss and bother, certainly eliminates monopolistic competition from serious consideration. However, Stigler provided no real demonstration to concretely support this critical claim. The impression left from sensing the flow of his argument is that this conclusion is too self-evident to require any serious substantiation. Stigler here once again ignores Chamberlin's attempts to explain why entry does not necessarily translate into markets where competing products are close (even near perfect) substitutes for one another. Chamberlin's contentions may be dismissed, but at least casual observation would yield similar conclusions, or perhaps a tad more support for these heterogeneous markets, rather than ones even roughly resembling the end result of perfect competition.

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[148] Ironically, Stigler confidently predicts that demand curves for individual firms will tend toward the highly elastic. He then turns around and scolds supporters of monopolistic competition for not basing their claims on empirical evidence. The same responsibility would seem to fall on the doubters as well.

> I personally think that the predictions of this standard model of monopolistic competition differ only in unimportant respects from those of the theory of competition because the underlying conditions will usually be accompanied by very high demand elasticities for the individual firms. But this is a question of fact, and it must be resolved by empirical test of the implication of the two theories (a task the supporters of the theory of monopolistic competition have not yet undertaken). (Stigler 1949a:24)

[149] Occam's razor can become a simple and strategic way to discard unwanted alternatives. Claim that a new theory either offers nothing new or that it is completely wrong (in Stigler's case the preferred strategy is to indict a theory on both counts) and into the ashcan it conveniently slips. The tactical appeal this had for Stigler should be obvious.

> Since I am splattering my name about, I'll add Stigler's Razor: In dealing with economic theory, always use the most advance branch of mathematics you can apply. (Letter from George Stigler to Milton Friedman, December 10, 1947 in Hammond and Hammond 2006:75)

> Do larger numbers make the demand curves approach more nearly to the horizontal position characteristic of pure competition?—that is the question. Clearly there is no general presumption that they do. … Moreover, the concept of "in-between products" is not always easy to apply outside of geographical problems. … Are menthol cigarettes "in between" other brands? It seems clear that large or small numbers indicate nothing *necessarily* as to the degree of substitutability between the products concerned. This is perhaps most clearly evident from the fundamental proposition that the number of producers in any field depends first of all upon how broadly the field is defined. (Chamberlin 1937: 563)

Upon examination, this assertion fails completely to be self-sustaining. The different starting points do seem to yield quite different results and contain within themselves quite distinctive implications. Perhaps a somewhat more concrete example would clarify the issue. If we think of Hotelling's (1929) famous beach strip and substitute ice-cream for hot dogs, important distinctions arise as we let more ice-cream sellers pitch their stands on the beach.[150] We can conveniently start with a vision of perfectly competitive markets, taking note of the sort of simplifying assumptions Stigler would likely make and/or of which he might approve. First let all sellers compete by offering identical vanilla ice-cream for sale. Again, to simplify, let them all buy from the same dairy so that there is no possible way of distinguishing the vanilla sold at one stand from that of another. Let all sellers be identical and offer dishes of vanilla ice-cream in the same blue plastic bowls and accompanying clear plastic spoons. This identical product is priced by the gram (or ounce if the beach is in the USA). Since we also assume that buyers and sellers each have available to them at no cost all the information needed to make optimal decisions, we can conclude that each individual on the beach has a specific amount of ice-cream he or she is willing to buy at any given price. We can then aggregate all these individual demand schedules yield-

[150] Ice-cream eliminates any of the contortions needed to avoid divisibility problems in output. It is just more comforting to imagine scoops of various sizes then selling fractions of a hot dog or imagining ten extremely friendly, but different, customers sharing a single dog. Besides, ice-cream is just more beach-like and summery. The sellers can either have fixed stands or roam up and down the strip carrying freezer bags on their shoulders.

ing a result which should represent the demand for vanilla ice-cream on that particular beach.

Shifting to the supply side, we assume that each seller labors under the burden of an increasing marginal cost curve by asserting that he or she operates within bounded conditions that reflect a decreasing return to a fixed factor constraint.[151] This assumption allows us to aggregate all the identical marginal cost curves into a single supply of vanilla ice-cream for that beach. The price that equates demand with supply indicates the market clearing and equilibrium level of the market. With selling price dictated by the market, each seller responds by selling bowls of ice-cream only up to the point that the constant price gained from the sale of more ice-cream just equals the incremental cost of preparing it for sale. This determines how much ice-cream each identical vendor will sell. Notice that the marginal cost curve turns upwards mostly because economists like Stigler command it to do so. For simplification he assumes rising incremental costs by conveniently asserting that the marginal output of each seller will decrease as more bowls of ice-cream are put up for sale. Such an assumption, however convenient it may seem to be, also appears counter-intuitive and perhaps baseless. The actual assumption Stigler would be making is that vendors would stop selling ice-cream even though it was possible to continue to sell at the given market price. This conclusion is more than convenient; it is needed if we are to have independent supply and demand schedules for vanilla ice-cream. Unfortunately, convenience and necessity ring false when grafted together in this manner. In any case, the end result is an optimal number of sellers offering the identical product for sale, spaced along the beach so as to minimize the potential walking distance for each potential buyer. In other words, the distribution of these stands along the strip of shore depends on how the identical buyers are scattered throughout that same strip. (Consumer distribution would naturally be part of the known information equally available to all sellers.)

If we shift to the monopolistic competition approach, the same assumptions no longer hold. To maintain the contrast at a simple level,

[151] Of course each stand has to cover variable operating costs. This limits the relevant portion of the marginal cost curve.

again imagine different sellers of vanilla ice-cream. But now it is possible to introduce the simple idea that not all vanillas are perceived as equal. Taste and quality will vary according to the recipe used and the dairy churning out the ice-cream.[152] It is also perfectly permissible to let the individual sellers differ, meaning that they could have different personalities and physical appearance which will all affect the complete experience of buying ice-cream. We can further posit different styles of bowls and spoons and basic marketing techniques to woo the perspective buyers, even given a complete knowledge of the market and the customers scattered over the elongated beach. Or if we want to become even more complex, we could imagine each seller offering different combinations of exotic varieties of vanilla but perhaps specializing in just one type. This focus on marketing doesn't require informational problems. It is consistent with Chamberlin's reasonable assumption of potential customers each with his or her distinctive set of preferences.

In any case, we could either aggregate all the beach-goers' demand for vanilla ice-cream, ignoring the different varieties, or aggregation could proceed according to subspecies of vanilla. On the other side of the equation, we could potentially aggregate according to what each seller has to offer, including the whole marketing package as well. The sales at each stand would depend on the additional, or marginal, revenue accruing from each bowl of vanilla sold and the marginal cost of producing that extra bowl. But notice that unlike George Stigler we do not need to command marginal costs to rise. Instead we can claim that each stand's sales depend, in part, on the demand for its ice-cream combined with the constant variable costs of producing the item ready for sale.

The test here is whether the beach would look the same under each set of assumptions. One big difference is choice, even given the absence of an information problem and limiting ourselves solely to one flavor of ice-cream. Moreover, instead of every stand selling exactly the same amount and earning normal profits, our alternative conceptual beach produces outlets with a variety of profits. The choices of what to sell at each stand

[152] Once there are discernible differences, even in the same flavor, there is no reason to suppose that they all need to be purchased from the same manufacturer. Different companies may aim to occupy different market niches. Some will aim for a richer product, others the low-fat end of the spectrum.

are inter-dependent and it is clear to see the relationship between a theory of monopolistic competition and game theory. What we view each day at the beach depends on the choices made by all the various players. Given these circumstances, the unique market equilibrium honored and adored by George Stigler needs to be dropped. There remains instead a multiple equilibrium situation and even a potential non-equilibrium approach if we give the theory a tactful shove. Despite Stigler's insistence on consumer sovereignty, the perfectly competitive beach resembles a relic out of an old Soviet Union planning book, with identical buyers and sellers all selling and buying the exact same type of vanilla ice-cream as endorsed by the five-year plan. Each day would look much like the last, though shifts in demand would reduce or augment the number of sellers. In contrast, Chamberlin's beach is stocked with sellers eager to seduce each consumer's fancy. To say then that monopolistic competition offers no additional value, that perfect competition yields much the same result, more closely resembles a flight of fancy rather than any hard-headed analysis. To do so is to assume that new sellers fill in gaps between those already established on the beach. That step may be plausible, but it is hard to envision this process leading to a state where each stand became a close substitute for another. The step-by-step process, which would yield an approximation of the perfectly elastic demand curve, remains elusive. As Chamberlin claims, entry just isn't sufficient to guarantee Stigler's confident assurance.[153]

> Chamberlin, by contrast, started not with costs but with demand, which reflected differences in consumers' preferences and between competitive

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[153] Stigler either couldn't see or simply dismissed Chamberlin's idea of focusing on, and indeed starting with, the demand side of the equation as the basis for building the groundwork of an analytical method.

> The concepts of monopolistic competition harmonise readily with the emphasis in marketing theory on the profit opportunities in offering distinctive consumer satisfactions, as evaluated by the consumer. To the marketer, as once to the economist, the customer buys, not a product, but the expectation of benefits: the variety of ways to consumer satisfaction, and the importance of consumer satisfaction, both submerged by the tendency of imperfect competition theory to regard every means by which a firm may aim to give added value as a wasteful device for bamboozling the ignorant customer, are fundamental assumptions of monopolistic competition. In the current management jargon, imperfect competition is producer-oriented, monopolistic competition is consumer-oriented. (Loasby 1971:876)

> products. His assumption of uniform demand and cost curves is much more obviously a temporary expedient to aid exposition than it is in Mrs. Robinson's analysis, and the tangency equilibrium is not presented as a general solution. Because products and consumers do differ, even in equilibrium "some (or all) of the demand curves may lie at various distances to the right of the point of tangency, leaving monopoly profits scattered throughout the group—and throughout the price system." [1] We should therefore be much less surprised than even the sympathetic D. H. Robertson to find Chamberlin speaking of his equilibrium—monopoly profits and all—as a sort of ideal. [2] If economies of scale are frustrated in this paradigm, it is largely because the alternative is to frustrate the need for variety which flows from "differences in tastes, desires, incomes, and locations of buyers, and differences in the uses which they wish to make of commodities." [3] But perhaps Chamberlin did not take his falling cost curves too seriously. After all, they were a methodological necessity, given a falling demand curve, for equilibrium with normal profits, just as falling demand curves were a methodological necessity, given falling costs, for Mrs. Robinson. (Loasby 1971:876)

Stigler, after leaving the theoretical earth thoroughly scorched and salting it for insurance reasons, does throw a few crumbs Chamberlin's way.[154] This gesture perhaps served to create an illusion of fairness, camouflaging to some degree the deeper objectives of Stigler's attack. A scientific pose can serve as a classic strategic manoeuvre, if successful in shifting the terms of debate, by reframing the argument. The theory under attack finds itself more vulnerable, since its opponents have managed to seize the high ground and can now more easily steer the direction of any ensu-

[154] Perhaps labelling Stigler's admissions "crumbs" is being overly generous. They equate to a back-handed compliment, which shrinks monopolistic competition down to a trifling addition to standard price theory. In effect, it is now a theory which is too trivial to consider or discuss. The overriding attitude seems to be one of "tough luck, but no cigar."

> The general contribution of the theory of monopolistic competition, on the other hand, seems to me indisputable: it has led to reorientation and refinement of our thinking on monopoly. We are now more careful to pay attention to the logical niceties of definitions of industries and commodities. We are now more careful to apply monopoly theory where it is appropriate … These and other improvements may seem disappointing to the hopeful proposers of a proud new theory, but they should not be. This is the way sciences grow. One of the prominent lessons of the history of human thought is that new ideas do not lead to the abandonment of the previous heritage; the new ideas are swallowed up by the existing corpus, which is thereafter a little different. And sometimes a little better. (Stigler 1949a:24)

ing debate. Leaving the theory with some shred of achievement is all part of this strategy of reframing. The idea is to have any future discussion of monopolistic competition focussed largely on aspects that view this approach as a rather innocuous extension of standard price theory.[155] If we recollect the resulting textbook version of Chamberlin's approach, we are able to judge just how successful Stigler was in his reframing.

Stigler in his lecture takes elements of Chamberlin's work and identifies them as composing the essence of that tentative theory. This stitched-together tapestry would be plausible and convincing except to the most careful of readers since his claims would have a certain resonance with the average audience or readership. Once he convinces his audience that monopolistic competition rests entirely on the assumptions he skilfully displays, the theory itself becomes much easier to dismantle. His characterization is chosen specifically to achieve his predetermined objective. Stigler doesn't believe that he is distorting Chamberlin's intentions. But his a priori beliefs and implied objectives led him to consistently transform into a more dismissible shape the alternative theories that he opposed. His desire to annihilate such theories, rather than to examine them, induced Stigler to search for, and seize upon, what he construed as obvious shortcomings.

There is of course no way to verify the extent to which Stigler was aware of his underlying motivations when crafting his skilful evisceration of monopolistic competition. Very likely he honestly believed that his intention was only to point out flaws in Chamberlin's analysis, rather than disabling what he saw as a ticking bomb within the profession.[156] (Whether

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[155] Employing much the same strategy, George Stigler's close friend, Milton Friedman (2003) [1956], attempted to dismiss Keynes to the position of a minor quantity of money theorist.

[156] Stigler's concern with distributive issues remains unvoiced in this attack, although it appears explicitly in his first LSE lecture where he (1949c) plights his troth to the cause of liberalism. However, even if one accepts the simplified schematic that allows for an equilibrium solution within the realm of monopolistic competition, factors of production must fail to receive the value of their marginal product, but rather only their marginal revenue product. Thus the rhetorical case equating market distribution with equity and efficiency frays discernibly.

> The leading proposition that a sloping demand curve for the individual firm reduces the remuneration of a factor below the value of its marginal product has now received some measure of general acceptance. In view of the fact that it is so readily demonstrable and that it has not to my knowledge, been contested by anyone, it seems fair to say that its acceptance is general among those who have turned their attention to the problems of monopolistic and imperfect competition in recent years. (Chamberlin 1950: 154)

he garnered any personal satisfaction from what might be considered as retaliation, a settling of scores with Chamberlin, is an interesting subject that must remain an area that is not up for general discussion.) But keeping in accord with Chicago's own preferred approach, we are limited to observing actions, since we are inherently incapable of perceiving thoughts.

> From a scientific point of view, it seems fallacious to treat motives as data and use them for causal explanation. They seem to be, if not unreal, at best uncertain for causal explanation. They seem to be, if not unreal, at best uncertain inference from behaviour, which alone is directly observed: hence they form a distinct problem to be dealt with after behavior is explained. The "Occam's razor" principle is cited as telling us to ignore mental states, observe and measure responses to situations, and find such regularities as we can … . (Knight 1946:108)

VI. The Aftermath: Mopping up Operations

> Lastly, even the ultimate outcome of a war is not always to be regarded as final. (Clausewitz quoted in Paret 1976:358)

Stigler's evisceration of monopolistic competition, along with Friedman's (2003) publication of his essay on the quantity theory of money in 1956, represents some of the first shots fired, marking the incipient Chicago counter-revolution. They sought to turn the momentum that had gathered around the two great breakthroughs of economic theory, hot-housed in the turbulent 1930s, but drawing the allegiance of ever-growing numbers of academics in the post-war period. In place of Keynesianism or monopolistic competition they offered the comforting certainties associated with old-time religion.[157] Friedman drew back into the purported oral tradition of Chicago to resurrect the quantity theory of money, while Stigler revived a much expanded version of Knight's perfect

[157] Stigler didn't restrict himself solely to monopolistic competition when it came to pushing back a growing flood of heresies. He reacted severely against the anti-marginalism reflected in Richard Lester's work (Stigler 1946c) as well as Hitch and Hall's mark-ups or Paul Sweezy's kinked demand curve (Stigler 1947a).

competition.[158] There was something almost biblical about their insurrection. The flock had strayed from the path of economic righteousness, misled by false prophets offering the tawdry idols of glittering new theories. Their goal was to turn the profession away from these illusions and back to the fundamentals that had nurtured the profession over many past centuries. But superficially at least, and unlike his friend Milton Friedman, George Stigler often seemed to lose interest after a declared victory.

> He was a guy with a lot of ideas and yet impatient. George had an impatient personality. Even in playing tennis, if he didn't get the point in two shots you could tell he really tried for a hard shot, he didn't want to just sit and hit it back and forth. He was a good tennis player, particularly when he was younger, but he had a real impatience. And that was true intellectually too. He wouldn't sit with one problem for five years as some people might do. And so in his papers, he has an idea and he tests it. So there are a lot of good ideas in his work but he doesn't sit and stay with a problem for a long time. I think that's true. In that I think that he's substantially different from Milton, you know his work on money or his work on consumption functions were longer ventures, he took longer periods of time. He had more patience. George was not the most patient researcher. (Conversation with Gary Becker, October 1997)

Stigler, for instance, seldom returned to a field of battle. Having tilted at a particular windmill, he was wont to move on.[159] This fixed habit was certainly consistent with his apparent behavior. Though this is not to say

[158] Friedman's insistence on tying his reconceptualization of the quantity theory of money to some Chicago oral tradition essentially germinated a debate that dragged on for decades. Those incapable of stilling their interest might look at Freedman (2008) for a lengthy analysis of this seemingly endless debate.

[159] Gardiner Means provides a distinct deviation from this pattern. Means to some degree served as Stigler's doppelganger. Both had fixed views from which they proved incapable of deviating.

> Both of them, both of these men had taken strong positions on this … Maybe – I almost come close to saying, that they had taken these positions before I was born. That's not true, but it certainly was before I was born as an economist. I could have predicted George Stigler's predictions before I signed up from what I'd learned in graduate school of his writings. And I could have predicted, to a great extent, Gardiner Means' predictions and you could guess what I learned at graduate school about Gardiner Means. (Conversation with Jim Kindahl, October 1997)

that unresolved issues, no matter how confident his public face, did not continue to gnaw and demand something of a resolution in his mind. Despite all his bravura, for instance, questions surrounding the political marketplace, what he termed alternatively "the paradox of legitimacy" or "the problem of legitimacy" was still a work in progress up to the time of his death.[160] In the case of monopolistic competition, aside from a few barbs, his only further formal contribution would come directly when he perceived he had no other option but to respond to Archibald (Stigler 1963c). Despite appearances, however, issues surrounding increasing returns and informational shortcomings, which for Stigler were closely connected to monopolistic competition, still lay unresolved in the years following his 1948 lectures. However, on the surface he seemed to have scrupulously washed his hands of a subject that he had essentially labelled as defunct in the title of his lecture.

> The article was accepted and he would surely have been asked since I directly referred it to him as an editor. I never got a note from George saying "Well, this time around I've got to admit I was wrong. And your reading was right." Not at any time. And a lot of people would tell me that if they wrote to him complaining about something, he would answer something like "Well, if you're the kind of person who believes that, then you're just the kind of person who believes that." (Conversation with Paul Samuelson, October 1997)

Neither man could seemingly let the issue of flexible versus administered prices alone. Each was willing to reply and counter-reply long after extensive discussion of the issue had dampened any remaining interest.

> Borts [editor of the *AER*] unilaterally declared that enough is enough, though I don't think he said it that way. I'd forgotten who had the last word in this controversy, whether it was Means or Stigler. As everyone knows it was Stigler, I remember now. Anyway, Borts declared that this is enough. That was it. George wrote to me. He sent me that letter from Borts with a little note that said (I was already here at the University of Massachusetts by that time). The note said something to the effect, "Let us hope for more attacks on our work. How else can a professor advertise?" (Conversation with Jim Kindahl, October 1997)

[160] This issue essentially forms the basis of his final published article (1992), a post-mortem voice pointing out the key questions and issues even after he had been properly interred.

> He was not an Austrian and he was not a libertarian. But what exactly would he allow as a proper realm for government? Then he got into this—I remembered his name for it after talking to you—"paradox of legitimacy" he called it, or sometimes he called it the "*problem* of legitimacy." (Conversation with Claire Friedland, November 1997)

> He was a very strange person to sum up because his methods were not those of anyone else. I think he was quite unique. If you put a point to him, he liked to answer it with a joke. Then if you pressed him he produced some fact or other. You pressed again, he'd give you his answer. But he was sort of an economist even in argument. He used the easiest way. (Conversation with Ronald Coase, October 1997)

On the surface, quiet prevailed. There were no post mortems. George Stigler, clinical economist, had performed his autopsy on monopolistic competition and had declared the theory to be dead on arrival. To be more precise, Chamberlin's baby was diagnosed as being still-born. But it is always a mistake to think that once Stigler had his teeth sunk into a problem, he would ever relinquish his grip. It was true that at least to his own satisfaction he had demonstrated that someone who appeared to be a giant in the field was actually a dwarf.[161] He did, however, act as a bit of a grim executioner when the next edition of his maligned textbook saw light (1952). Gone was the methodological first chapter that resembled something not unlike crib notes taken in Knight's classroom.[162] Missing in action was the slightest whisper of monopolistic competition, with Chamberlin himself reduced to the merest of cameo roles.

> In the 1946 book, though, he does devote a section to monopolistic competition… The phrase "monopolistic competition" is not included in the index. Subsequent editions of the text do not talk about monopolistic

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[161] Perhaps in this case, George Stigler demonstrates that sometimes it is not folly to tilt at windmills, since in doing so you can sometimes bring down giants. Thus the practical sense of the Sancho Panzas of the world must occasionally be ignored.

> "Pray, look better, sir," quoth Sancho; "those things yonder are no giants, but wind-mills, and the arms you fancy, are their sails, which, being whirled about by the wind, make the mill go." (Cervantes 1993: 43)

[162] In this 1952 edition, he takes great pains to emphasize that theories must be tested by evidence, not just logic, thus incorporating the methodology championed in his LSE lecture. In other words he introduces a methodology that eliminates methodological concerns.

> That Stigler was not much of a fan of quantitative jugglery or economic jargon is evident from his apologetic introduction to the chapter on basic concepts, where he says, "In economics … there is a language to be learned. And as elsewhere, the language is probably carried to excess: it is possible to have one's ideas snubbed merely because they violate the grammar of the profession" … In fact, in this book the chapter on methodology is dropped. In its stead, "A few mathematical notes are placed at the end, where they are easier to ignore". (Kamerschen and Sridhar 2009:187)

> competition at all, even in passing. Edward H. Chamberlin … who coined the phrase to describe competition for a product with several good substitutes, is cited six times in the 1946 textbook. This drops to one citation in the 1952 book. And if you studied out of the 1966 textbook, you would not even know that Edward Chamberlin existed. (Kamerschen and Sridhar 2009:195)

Stigler never rested, though. If he ran across a problem, such as increasing returns to scale or product differentiation, that Chamberlin had wrestled with, simply destroying a difficult opponent was never sufficient. He had to provide a positive alternative. His aspiration for economics was to offer a comprehensive and consistent theory that would also leave his ideological leanings unruffled (whether or not he was consciously aware of that self-imposed constraint). Working in tandem with Milton Friedman, methodological concerns had been erased.[163] To resolve the remaining difficulties he would fall back on the idea first posed in his 1937 article, the problem of information. As it turned out, it was not to be the aspect of information that Knight posed, defined as the puzzle of uncertainty, but rather the issue of information as a search objective. In doing so he would transform the question into a problem of structuring a market which was much like any other market (and start on the path that led eventually to his Nobel Prize).

> The problem is that economics seeks to do more than just predict. The objective is to assist in understanding the phenomena observed. If assumptions cease to matter then prediction is always possible simply by tacking on a sufficient number of epicycles to one's theory. Shifting supply and demand curves will eventually predict a price change no matter how weak the underlying assumptions of the model might be. It's partly a licence for self-indulgence. You don't have to have a correspondence between a theory and the facts, or a close correspondence. In fact, the theory is all the better

[163] Methodological quibbles had vanished to Stigler's own satisfaction, but even those who were his natural allies were not necessarily convinced.

> Testable predictions are not all that matters. And realism in our assumptions is needed if our theories are ever to help us understand why the system works in the way it does. Realism in assumptions forces us to analyse the world that exists, not some imaginary world that does not. (Coase 1994:18)

> if it doesn't fit the facts, closely. And I think that there are some profound errors in that form of positivism, but it is there for a purpose. It serves a purpose. Do you think the cigarette industry with only four big producers in it is not competitive? Well, if one raises its price, another one will and so forth. That's the same paradigm of comparative statics that would happen under competition. So under the doctrine of "as if," we can use the competitive theory. And as I said, the early Stigler didn't quite believe that, but the late, greater Stigler sort of believed that the facts had changed or had only now been properly interpreted. You could see this in the role that information played for Stigler. It also extenuates what had seemed like market failure because it is all very well to have one price but that's under the naïve assumption that you could have ideal information. And, actually they're working out their own version of a theory of imperfect competition. It just isn't the Joan Robinson or the Ed Chamberlin version. (Conversation with Paul Samuelson, October 1997)

Stigler was determined to resolve the conundrum posed by monopolistic competition (or imperfect competition) by grounding any solution in the standard price-regulated framework of supply and demand. He did so with a wonderful sleight of hand by domesticating information within those desired boundaries. Information can usefully be seen as being no different than any other market with just a few minor adjustments. The crucial insight into transforming information into a commodity is to limit it to that type of economic factor defined by any generic search. The data already exists. It need only be located to be used.

> But in some things that I've written, I have felt compelled to say the following: that the central problem with monopolistic competition is imperfect information. But, the apparatus that Chamberlin devises is not at all fit for the study of imperfect information. It doesn't look at: the cost of information, the demand for information, the supply of information at all. It just uses the notion of imperfect information to draw a negatively sloped demand curve on a graph. George certainly got away from this treatment of information, although he doesn't really mention Chamberlin or anything like his theory. What he comes out with is his very influential paper "The Economics of Information" sometime in the early 1960s, I think. He frontally attacks the problem of information. He asks "What's going to

determine how much information people collect?" And he looks at the underlying determinants of this question. He doesn't use his work to draw any kind of normative conclusion at all, but he uses it to draw conclusions about what we might expect about the dispersion of prices in this market versus the dispersion of prices in some other market. So his attack on imperfect information is 180° different from Chamberlin's attack. It is much more designed to facilitate gaining an understanding of the phenomenon, and the underlying determinants of, the degree of imperfection of information. But that certainly is a clear difference in favor of George Stigler's approach and a very different attack on the problem. I think, it's a better attack because it is not mainly motivated by an attempt to reach any normative or policy conclusions. Although, it can be used to do that after you finish that type of attack. This attack, this piece by George Stigler, is a clear, simple extension of neo-classic economic theory. He looks at the supply and demand for information. (Conversation with Harold Demsetz, October 1997)

Stigler very cleverly turns what appears to be a market flaw, the lack of free and sufficient information, into a mere failure by less perceptive observers to comprehend the wisdom of competitive systems.[164] The inevitable persistence of price dispersions signals the vital role of information in the continuing efficient operation of markets. The challenge posed to economists is to decipher and explicate the underlying efficiency in such arrangements. This is what Stigler (1961) deftly attempts to do.

[164] Stigler started off any analysis with the assumption of market efficiency (or perhaps simply in control of that innate knowledge). Not that Chamberlin, unlike others of that period, was seriously questioning issues pertaining to efficiency. But George Stigler focused his quite powerful intellect on demonstrating that what might appear to naïve analysts to be a market flaw or failure could be rightly understood as an efficient solution to an underlying problem. If anything, as he grew older he came to conclude more and more that price-adjusted models could be extended to analyse more and more phenomena, even those lying outside the traditional boundaries of economics.

Oh, George was a puzzle solver. George was definitely that. As far as George was concerned, I would think that the system building had already been completed by Adam Smith and there was not a hell of a lot of room for him or for anybody else to do that. He was interested, I would say primarily, in a particular sort of puzzle and it's a typical Chicago puzzle. And I don't mean that in any bad way, it's the sort of puzzle that the Chicago School's presuppositions require. Show me an apparent anomaly, something that does not seem to be explicable using the Smithian apparatus and the Marshallian apparatus and I will show you that it can be explained that way. That was exactly the sort of thing that George went looking for. And that's not a bad thing. I'd have to say that it can actually be very good. (Conversation with Robert Solow, October 1997)

Recent work (Levy and Makowsky 2010) demonstrates that at least under a given set of restrictive conditions, a version of a Stiglerian competitive market can survive over time. Such findings provides support, though hardly conclusive evidence, that Stigler's alternative approach is viable. Though whether such a model deviates too far from Stigler's preferred vision is much more difficult to evaluate.

> We have constructed a model in which price dispersion and temporarily excludable knowledge allow firms to survive from one short run to the next in a world of increasing returns to scale. The market remains competitive in the long run, distinguished by a restless disequilibria, where firms are always approaching, but never settling into uniform, marginal cost pricing, save for a distant, monopolistic future likely only reachable if market entry is held at bay. (Levy and Makowsky 2010:416)

It is important to also understand the limitations of what Stigler proposed. His search theory explored the optimal amount of search required to obtain pricing information. The approach deliberately restricts itself to finding information that already exists. Speaking in terms of the technology of the day, this is basically a "yellow pages" search to find the optimal seller of a known product. The question then is not whether such information can be found but the level of effort that would best be expended in such a situation. What this treatment deliberately avoids is those aspects of information that Knight stressed when he used the term uncertainty to cover uninsurable risk. In this context, there is of course information that is to some degree unobtainable.[165]

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[165] Milton Friedman broke with Knight over this issue of uncertainty. For him, the concept of subjective probability permanently laid to rest any idea of uninsurable risk. It is difficult to imagine George Stigler breaking with his close friend on this point. In some sense the approach assumes that no conceivable risk is uninsurable. A market almost by definition must exist, no matter what the configuration of such risk might be. This is similar to the assumption that in any given market both buyers and sellers will simultaneously exist since it will always be mutually beneficial to so do.

> See I'm a great admirer of Knight. But I think his distinction between risk and uncertainty is untenable. In the aspect of, I believe that it uses a false theory of probability. I believe that the only theory of probability that can hold water is personal probability, the kind of thing that Jimmy Savage help develop. If you take that approach, you can't distinguish uncertainty from risk. There's no break point. But also, you see, it means that Knight implicitly was working on a definition of probability as a relative frequency. And that misleads people into thinking that there are objective probabilities that you can know. Therefore it leads to

> Well, not everything. No, I wouldn't think he would say he had captured everything about information. I think he thought that he captured a lot about how people search for greater information. I did think that he thought he captured a lot of that aspect. There are other aspects of the information problem that he doesn't get at. The bargaining interaction between people when they each have information about themselves the other doesn't have. I would think, I'm pretty sure he would agree with that. He had recognized his framework doesn't get at some of those issues. But I think he felt that it was a good framework for getting at the issue of how people search for additional information, when they have opportunities to search for it. These may be complicated searches, they may just simply be that you call up a store and find out what their price is. There may be more complicated things involved. But this is a search process. There are certain optimal problems to it, and therefore you can extend the usual optimization analysis of economists to include also aspects of the acquisition of information and knowledge. And I think, yes, that he felt that he had captured a good chunk of that aspect of the problem. (Conversation with Gary Becker, October 1997)

The resolution that his theory of information provided deftly underpinned his work on oligopoly theory which followed three years later (1964).[166] While Chamberlin wanted to see a continuum defined by the

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> a distinction between risk and uncertainty in terms of costs. Knight assumes you know some probabilities and that there's no way you can know others. In a personal probability sense, nobody really knows any probability. There are no objective probabilities. If I can experiment with your willingness to bet, I can determine your probabilities. There's going to be a war next year. Knight would say that's uncertainty. But in principle, if I can experiment with you, I can find out at what odds you are willing to take a bet that there will be a war next year. And thus I can extract your subjective probability of there being a war and in that sense there's no distinction between risk and uncertainty. At any moment of time, you will in principle have subjective probabilities of any strategic event. And I think George was influenced by that approach to probability as well. (Conversation with Milton Friedman, August 1997)

[166] The link between the two papers is made sufficiently clear by Stigler in his autobiography. Though hardly emphasized in a work that is a somewhat breezy account of his intellectual life, the idea that oligopolies could be treated much as any other competitive market can be reasonably deduced.

> The article I wrote on this problem, "A Theory of Oligopoly," dealt with the detection of cheating and concluded that often cheating could not be reliably detected. That makes conspiracies harder to form and weaker if they are formed. This line of argument was an extension of my work on the economics of information. (Stigler 1988a:103)

degree to which each market exhibited the dual qualities of competition and monopoly, Stigler was keen to carve out the middle, leaving only the extremes.[167] His "Theory of Oligopoly" effectively demonstrates that this middle ground substantially collapses into what can be treated either as a competitive market, or should the inevitable attempt at collusion hold, the result would mirror the case of monopoly. Demonstrating the irrelevance of Chamberlin's alternative by offering something of a reinforced restatement of competitive price theory signals Stigler's attempt to bury the issue forever. The vehicle he constructs specifically to accomplish this objective is to present positive proof that the concerns raised by Chamberlin and others present an illusory, rather than a conclusive, problem for mainstream economics.

> George never in my memory attacked the interdependence problem in the sense that it might be attacked by game theory. He attacked it in a quite different way—by assuming that these guys will work it out by trying to collude. So, he's not going to get into the details of the specific interdependence that results. They'll work it out because it is in their joint interest to work it out. They'll try to collude and then the only question that remains is, "How successful will their collusion be?" If you look at his famous paper

[167] His belief in the importance of monopoly (and the need for anti-trust law to counter-balance it) steadily weakened in the years following his LSE lectures. In fact the final lecture in that 1948 series raised questions about the prevalence of monopoly pricing (Stigler 1949b).

> Aaron Director takes economics seriously. In each of the various practices he has analysed (tie-in sales, patents, resale price maintenance, etc.) he has sought the profit-seeking reason that led businessmen to adopt the practice. Sometimes the reason was the exercise of monopoly power, but other times an important efficiency was achieved by the industrial practice. Monopoly receded from its near-monopoly position in explaining business behaviour. The researches of Sam Peltzman, Harold Demsetz, Lester Telser, and others reinforced the decline in the role assigned to monopoly. (Stigler 1988a: 103)

Even what would seem to be the unassailable position enjoyed by natural monopolies crumbled under the relentless onslaught of competitive price theory. By equating competition within a market as though it was effectively mimicked by competition for the market, Harold Demsetz manages to decisively banish such structural difficulties from receiving any serious consideration (Demsetz 1968).

> Harold Demsetz made an important contribution to this argument by analysing the so-called natural monopolies: public utilities such as water companies, which are given exclusive rights to supply a city. He pointed out that here the force of competition would be felt at the stage of granting the franchise; There would be many bidders for the right to supply the water and their competition could be directed to the consumers' benefit. (Stigler 1988a: 164)

> on "The Theory of Oligopoly,"[168] he sets up a world in which he tries to trace through what will affect the probability that successful cheating under collusive arrangements will take place. Or, as he puts it, the way in principle it will affect the probability that the colluders in the issue will detect the cheaters who are trying to cheat on the collusive arrangement. He sweeps away the interdependence problem by saying, "Hell, if it's in their interest to solve it, they'll solve it. I don't have to worry about how they solve it." He approached the problem implicitly with the following kind of postulate in his mind, which would be one that I would have in my mind too. If there are mutual gains to exchange available, they will be realized. We don't have to worry about the details of how it is realized, unless that's the object of the analysis. Well, the object of his analysis was not that, but it is maybe an object of the analysis of game theory. (Conversation with Harold Demsetz, November 1997)

Convincing Stigler to revisit the territory covered in his 1948 lecture was clearly not a task easily accomplished.[169] Having sufficiently steered the profession away from the potholes presented by a confused and convoluted economic theory, he saw nothing to gain by going another round with Chamberlin or one of his acolytes.[170] Only a very wily bit of entrap-

[168] Stigler, George (1964) "The Theory of Oligopoly", *Journal of Political Economy.*

[169] We are attempting to draw a rather fine line here. Although seldom returning to a debate following a competent demolition of a chosen opponent, Stigler could never resist getting in a few gratuitous kicks at, or even a body blow, to an old opponent whether living or dead. In his autobiography, Stigler slyly depicts Chamberlin as an obsessive, one-trick pony, someone who simply lived off his early fame.

> Chamberlin devoted his entire life to the support, defense and modest elaboration of his book (which was based upon his doctor's thesis). Legend has it that when he taught economic theory, he made a determined effort to cover the broad field of price theory, but always ended up concentrating upon monopolistic competition. (Stigler 1988:96)

[170] Chamberlin's 1957 volume was to some degree a detailed response to the previously levelled criticisms. Stigler turned a very deaf ear to these efforts. However, his article that same year on perfect competition (Stigler 1957) serves almost as a companion piece, with Stigler trying to strengthen the case for his favored approach by examining its historical roots.

> The analytical appeal of a definition of competition does not depend upon its economy of assumptions, although gratuitously wide assumptions are objectionable. We wish the definition to specify with tolerable clarity—with such clarity as the state of the science affords—a model which can be used by practitioners in a great variety of theoretical researches, so that the foundations of the science need not be debated in every extension or application of theory. We wish the definition to capture the essential general content of important markets, so the predictions drawn from the theory will have wide empirical reliability. And we wish a concept with normative properties that will allow us to judge the efficiency of

ment would do. George Archibald succeeded by posing a challenge that Stigler, even when adopting his most glacial pose, could not easily ignore. Archibald found an area tender enough for Stigler to be unable to adopt any honorable position other than to issue some sort of response. He essentially accused Stigler of ignoring his own professed methodology during his disembowelment of Chamberlin's theory. Being accused of inconsistency, of contradicting one's own position, was just the right dart needed to locate an insufficiently protected and highly sensitive target.

> We should expect to find the Chicago critics endeavouring to discover what predictions monopolistic competition yields, comparing the predictions with those of perfect competition and monopoly, and finally addressing themselves to such empirical testing as seemed necessary. But we do not find this at all. Rather we find that much of their argument has the a priori character that we would associate with a very different methodological school. (Archibald 1961: 3)

In a strategic sense, Archibald is generously performing the heavy lifting for Stigler and the Chicago School. Keeping the Friedman/Stigler methodology firmly in place, Archibald ignores the realism of any assumption made but instead tries to work out exactly what predictions

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> policies. That the concept of perfect competition has served these varied needs as well as it has is providential. (Stigler 1986:280)

For Stigler, the theory had passed the almost Darwinian test of time, namely it had survived. Unfortunately, this simply testifies to the fact that perfect competition has fit the prevailing preferences of economists over time. Such a 'providential' match is axiomatically neither a guarantee of the theory's truthfulness nor of its accuracy.

> But a man that intelligent, given the nature of economic conservation, would almost always find what he's looking for. Is that bad? That's I think a really tough thing about economics. I don't think he was aware of the problem day to day. But if I said that to him, or it might not only just be I, if Paul or someone said that to him, he would agree. And then I think he would have said, "It's the better part of wisdom when you come to these really narrow decisions and the data speaks ambiguously, to accept the long-standing, the long view we've come to accept as knowledge and it's unwise, on the basis of that kind of evidence, to say I should throw over something that has stood us in good stead since 1776." (Conversation with Robert Solow, October 1997)
>
> My fundamental thesis, in fact, is that hardly any important improvement in general economic theory can fail to affect the concept of competition. But it has proved to be a tough and resilient concept, and it will stay with us in recognizable form for a long time to come. (Stigler 1986:284)

the monopolistic competition approach yields. These alone become the proper vehicles for testing. In this case, Stigler is playing fast and loose by appearing to reject a theory based on its predictions without knowing, or certainly stating, what those predictions might be. The proper approach would be to give Chamberlin the benefit of the doubt and instead assiduously apply oneself to ferreting out those implications.

> I justify the attempt on the grounds that we should see what testable predictions we can get from so famous a theory when we make the best of it, i.e., suspend judgement over difficult assumptions. This justification should commend itself to Chicago methodologists. (Archibald 1961:5)

Archibald is asking Stigler for no more than fair play. But to do this is to misapprehend Stigler's task in broadly dismissing, rather than refuting, Chamberlin's stance. Conducting a virtual search and destroy mission reinforced the sort of one-sided skepticism that became something of a hallmark of the post-war Chicago School. Theories which sought alternative approaches to economics were met not so much with skepticism as with scorn. Preferred frameworks, like perfect competition, were cleverly defended rather than examined. It is small wonder that Archibald could not refrain from noting the "tenacity with which Chicago economists cling to perfect competition" (Archibald 1961:4). Fair play would not have induced Stigler to claim that perfect competition should be used to analyse a given market under certain conditions, but that the monopoly model would best serve those purposes in a different context (Stigler 1949a:13–14). This pronouncement is delivered embedded within the confident assurance of providing an obvious and unimpeachable solution to the problem implicitly raised by Chamberlin. But Archibald points out that Stigler provided no method for knowing which to choose at any given moment. Supposedly, each method would have to be tried in turn to see which provided the better response. This strategy, however, seems a rather sad and inadequate way to test a hypothesis. Since either of the two may sometimes be correct, Stigler is essentially pleading mitigating circumstances for dismissing whatever difficulties might arise.

> It appears then, that the Chicago School have muddled two distinct questions. They have asked the practical, engineering question: what do we know? They have neglected to ask the enquiring, scientific question: what don't we know? And they have somehow inferred that, because the answer to the first was "something", the second could be safely neglected, and that we could accept complacently a hypothesis which we knew could not produce satisfactory answers to all the questions we wanted to ask. (Archibald 1961:50)

Archibald's argument is clear: Stigler cannot have things both ways. To arbitrarily complain about the assumptions on which Chamberlin constructs his group is basically out of bounds given the Chicago methodology. Arguing that heterogeneous output voids analysis is simply another a priori assertion that, according to their own self-imposed rules, is insufficient to disqualify the employment of the group as a strategic tactic. The implications of any testable hypothesis require testing before any judgment can be determined. Heterogeneity as a disqualification, if taken to an extreme, would seem to doom much of economic analysis. A single firm given this criticism could not be considered to be a unity since no two units of output are necessarily identical. A firm would seem to be composed of many firms each tied to a given unit of output.

> This view seems so obvious, and so much in keeping with the methodology of the Chicago School (according to which we are not to worry too much about the realism of the assumptions of a theory, but to develop and test its predictions), that it is difficult to understand what all the fuss is about. (Archibald 1961:5)

After performing a careful examination, Archibald's judgment tends toward the Scottish (not proven) alternative rather than to the strict dichotomy provided by the guilty/not guilty verdict. In a comparative static framework he finds the required information unavailable to formulate a testable hypothesis. If we remember that Marshall employed his framework more as scaffolding upon which to hang his stories, then Chamberlin's adventure at, either deliberately or not, flirting with a non-equilibrium approach is unlikely to snuggle easily, or be comfortable existing, within the world defined by comparative statics. This may ultimately rule out the usefulness of monopolistic competition, but in 1961 the jury was still out on that particular question. Archibald, trying to stay above the fray, maintains a reasonable degree of objectivity by deliberately confining himself to the rules laid down by Stigler and his Chicago cohort.[171]

> Perhaps the most serious criticism of Chamberlin's critics is that they have concentrated upon a priori discussion of his assumptions, instead of on discovering what facts were needed to give the theory content, and endeavouring to obtain them so they might test it. Discussion of the empirical material required is beyond the scope of this paper, but the results … are a little discouraging: we need facts about the demand functions of individual firms (with respect to quality and advertising as well as price) which are likely to be very difficult to get. Hence it may be that, with the observational techniques at our disposal, the theory is not likely, at present, to be very fruitful. As for the theory of the group, it appears that, to proceed further, we require some knowledge of demand relationships within groups. This suggests both theoretical and empirical investigation. (Archibald 1961:21)

In a better, or at least different, world full of modest economists open to opposing ideas and willing to see the flaws in their own conceptions, Stigler would have been grateful for Archibald's efforts. Where Stigler had merely dismissed, asserted or assumed, Archibald had done the more painstaking task of following up on Stigler's suggestion and seeing whether Chamberlin's work did yield any testable hypotheses. But in 1961 that world did not exist (nor does it today). Archibald mistakes Stigler's purpose in taking Chamberlin to task. His 1948 lecture did not seek to examine, but rather to bury, Chamberlin, his theory and any sundry artefacts connected with it. To say the least, Archibald's contribution was not welcomed. In fact it aroused a short and tart reply despite

[171] To avoid any confusion, at the time of his LSE lectures, George Stigler was a member of the Columbia University faculty. He was not to shift to Chicago and be reunited with his comrade-in-arms, Milton Friedman, until 1958. However, spiritually it can be argued that Stigler, in fact, never left the Midway environment.

Stigler's obvious reluctance to return explicitly to this particular argument. If anyone could ever be characterized as taking, or demonstrating, a degree of umbrage at an unwanted response to his or her work, Stigler could be justly described as taking umbrage in the extreme. His response followed a predictable pattern. (Here perhaps is our only genuinely testable hypothesis within this whole thicket of suppositions and assertions.) George Stigler saw this as an attack. Opponents were not to be reasoned with but smashed, scorned and dismissed. In Stigler's reply we return to the courtroom where the prosecuting attorney prepares to rebut the earnest public defender.

> G.C. Archibald's attack on Chamberlin's theory of monopolistic competition proceeds to its goal by way of a detour through Chicago. The detour appears to be dictated by a curious desire to repel the "Chicago" attacks (my own, by the way, was written at Columbia and launched in London) before presenting his own. (Stigler 1963c: 63)

Stigler's response can only be categorized as snarky, barely extending beyond a page in length. Certainly Archibald intends to evaluate Chamberlin's position as fairly as possible, but there is nothing malicious in pointing out contradictions in an earlier attack and finding it wanting. Stigler and Friedman were the ones staking out a specific and controversial methodological framework. A fair conclusion to draw is that by doing so, either the draftsmen should feel obliged to follow the implications and guidelines provided by their own creation or they should disown or publicly transform this methodological position. It certainly makes no difference who his employer was when he gave his lecture or where he presented those ideas. Stigler cannot deny the reality of a Chicago School or that in 1948 he was attuned to the ideas that would form the backbone of the School. Given his well-known allegiance, pointing out his location seems a rather unworthy, carping point to make.

Adopting a lawyerly, dismissive mode to deal with Archibald's objections is at best disappointing. His methodological ripostes are not reassuring. They seem to be reduced to an admission that methodological discussions are fruitless and his use of them was purely strategic.

> Chamberlin's theory is not an ideal platform for debating methodology. My views on methodology have undergone some change, but I am duly impressed that these views have a tendency to change much more than my judgment of the quality of specific economic studies. I propose the hypothesis that methodological controversy has never had a marginal product (of scientific progress) above zero. Archibald will surely agree that it would not do for me to provide confirmation of my own predictions. (Stigler 1963:63)

Stigler here employs a tactic prominently displayed in his 1948 lecture. Back then he discussed the nature of predictions without formulating them. Here he talks about a change in his methodological position without specifying what this position might now be or whether it would make any difference whatsoever. Archibald refuses to nibble at the bait. Instead he is far too polite in his response.

> Stigler declares that the marginal product of methodological controversy is zero. I submit that, if this is to be taken literally, it is a shocking piece of obscurantism, and an indefensible attempt to close discussion. As Friedman said, "More than other scientists, social scientists need to be self-conscious about their methodology."… But I doubt if it is really meant to be taken literally. In the context, its meaning is, I suspect, the harmless one that Stigler is not impressed by my methodological remarks about which, certainly, no more need be said. (Archibald 1963:69)

George Stigler, by disposition, tended throughout his life to say and take positions that would stir his designated opponents and by doing so conjure up a blistering response. This was an often successful rhetorical (or even marketing) technique that allowed him to steer the profession by determining the direction any significant debate should assume within a conclave of respectable economists. Extreme statements were likely to incite inflammatory comments on the terms set down by the argued point. When performed with a sufficiently deft hand, the subsequent back and forth might move away from the initial idea forming the original objection toward the points that Stigler or his allies wished to make. In essence it is a strategy for hijacking opposing arguments. Stigler when responding to any criticism often seemed to take the position that the best defense one can offer is an incisive offense against any perceived enemy redoubt. Charged with being careless in his own use of logic, Archibald deflects that particular poisoned lance by aiming it back at its originator.

> Stigler goes on to chide me for being unduly cavalier about logical defects in theory. I cannot think, though, that in taking the position that the justification of a concept of a group is ultimately an empirical matter, I am in any more *logical* trouble than Stigler, who is prepared on occasion to treat the New York housing market as a competitive industry (group). As Chamberlin remarked, "People who live in ad hoc houses should be more indulgent." (Archibald 1963: 69)

Economists would hardly be surprised that this interchange had no discernible effect. In fairness, Archibald did in fact present the more reasoned and balanced approach. At the very least, he did not appear so obviously anxious to bury monopolistic competition or to praise it. If he had one, he was very careful not to tip any prior agenda he might support. Stigler in contrast sought to eradicate an offending theory in whatever manner might appear to be effective and efficient. Thus Archibald may have had the better of the argument while at the same time leaving Stigler virtually unscathed.

> I think when he first went for Chamberlin, in that ''49 lecture; it was very difficult to understand what bugged him about Chamberlin. But now we see exactly what it was. I knew Chris Archibald very well, spent a lot of time in his company. We discussed the famous debate Archibald had with Chicago, the article by Archibald and the replies from Stigler and Friedman. Stigler and Friedman managed to win that argument. They shouldn't have won it, but they managed to win it. They got the better of Archibald rhetorically, even though Archibald really had the much stronger argument. It had to do with whether the qualitative calculus gives you enough information to predict prices. When you look at the last part of these documents where you discuss Euler's theorem, you come to the conclusion that the only sustainable assumption (about costs and cost curves in economics) is constant costs and constant returns to scale. If you have increasing returns to scale, well that's very subversive. Marshall realized the problem of returns to scale. It screws everything up. Increasing returns to scale requires a

dynamic theory of why firms get into trouble when they get bigger and bigger and then get more inefficient. Economics has tended to concentrate on constant costs. Now this is very ironic. Because Stigler's thesis was written in 1938 and then published in, I think, 1947 [1941]. Sraffa in the 1920s had already investigated production functions. But Stigler was just too fatally attracted by constant returns and constant costs functions. The same thing holds with Marxism, naturally. I'm really fascinated by this. I think it's funny how the orthodox theory of the firm and Marx's value theory, subjective value theory and the labor theory of value, come together. The conclusions of both really have to assume constant returns to scale. The production function becomes a tangent automatically in the case of constant costs. Constant returns to scale in the production function, if there is anything else, you get into analytical difficulties. What I want to emphasize is that there is a strange marriage of opposites. (Conversation with Mark Blaug, April 1997)

VII. Spoils of the Victor

Indeed it can be said that they [the Chicago approaches] conquered the field [of industrial organization]; by 1980 there remained scarcely a trace of the two Harvard traditions of Chamberlin and Mason in the current work of economists. (Stigler 1988a:166)
Or as S.J. Perelman (a well-known toreador) once said: "Before Nature made you she threw away the mold." (Letter from Robert Solow to George Stigler, October 20, 1959)

As Mark Twain was reputed to have claimed, "Reports of my death have been greatly exaggerated."[172] Stigler's claim of annihilation seems a

[172] Here once again we are faced with a slight discrepancy between the oral tradition and the actual printed word. Twain replied in an edition of *The New York Journal* 2 June 1897 that "The report of my death was an exaggeration." There is always a temptation to edit comments into a more pleasing or useful form. Nonetheless, despite George Stigler's attempt to inter monopolistic theory from his 1948 lecture through to his 1988 autobiography, he was perhaps too eager in writing its obituary.

Because in fact in his autobiography George Stigler refers to monopolistic competition as dead and buried.

A bit premature.

Never pronounce a death sentence on anything.

No, no that's right. (Conversation with Harold Demsetz, October 1997) bit peculiar (or perhaps a brief moment of indulging in wishful thinking). His dismissal appears to be targeting an exact facsimile of Chamberlin's final version of his theory at the time of his death in 1967. In fact Stigler to a certain extent later undercuts his own recognized glee by linking Chamberlin's theory to the initial resurgence of game theory in the 1980s.

> The most recent development in industrial organization is the emergence of game theory, which has dominated the writings of the younger economists in the major eastern schools and Stanford. This literature is closely related in spirit to Chamberlinian economics. It is much more rigorous (as well it should be, fifty years later) but has not shown equal gains in empirical motivation or empirical applicability. (Stigler 1988a:168)

There is a hint that he expects game theory to be as ultimately sterile as monopolistic competition and by tying the two together is bestowing the kiss of death on this enterprise. This attempt at prophecy would be in line with his strategy back in 1948.

> A lot of the game theory work that's come in later, not all but a lot of it is, at the end of the day: "under this structure that will happen, under that structure this will happen." I can remember as a journal editor, you would look at an article like that and you would say, "He's telling me that what is, is possible, but I knew that. What did I learn? I learned nothing." And he was hostile to this kind of ingrown fascination with only the logic of the argument. That's what he was reacting against. There's nothing coming out of this. If they could have come out with anything, if, at the end of the day there was a clear empirical implication, widely accepted, or at least widely discussed and tested … Fine, but there wasn't. Not only that, but the people who were doing it didn't even care. That's really what got him. These people didn't even care about it. They cared about communicating with each other. I mean, it's a view I have too. We will become irrelevant. (Conversation with Sam Peltzman November 1997)

The market for ideas, including those with an economic flavor, can only guarantee that those ideas best conforming to the demands of the profession will continue to prevail. But it is not the responsibility of the market to test validity. Thus errors can continue to be propagated since economics continues to lack anything closely resembling an irrefutable testing structure that would eliminate faulty ideas once and for all.

> Well, yeah. Now that is what you would be saying. No question about it. Because what meets market demand may have an element of so-called truth in it, in the sense that what people think better organizes the world for them, about how do we know what shapes events and so on, would presumably be one of the factors (and we hope an important factor) in determining legitimacy. But it wouldn't be the only factor. We would need other factors. And where these other factors dominate you'll get other results. That's why you get fads and fashions in ideas and so on that don't have lasting value. They will still be meeting the marketplace of ideas and I would say, I think that is right (Conversation with Gary Becker, October 1997)

In other words a fiercely competitive market favors those ideas which fit best with the given preferences of academics. This precept both has the ring of validity and also adapts the guidance provided by Becker and Stigler (1976). Or to be more exact, those ideas which can be made most palatable to their audience in terms of flows of benefits will ultimately triumph. In this way marketing skills come to the fore. Stigler sensed early on the importance of honing this skill. Unfortunately, pursuing this path to its logical conclusion pushes the profession into something akin to a fallacy of composition error. By raising the bar on marketing, Stigler forced equally ambitious economists to raise the level of their game as well. But a marketing race can leave tolerance for opposing ideas and a willingness to publicly discuss them behind.[173]

[173] Stigler was an inherent stirrer. His personality seemed to form a sympathetic match with the characteristic of the natural marketer. To be simply ignored was for him the worst sort of ignominy. Better to be reviled than anonymous. In a much less dramatic form Stigler's stance is not unlike Milton's Satan who found it "better to reign in Hell than to serve in Heaven" (Milton 1667: line 263). To what degree this desire was motivated by the persistent insecurity that some of his colleagues noticed is a question best avoided as placing us awkwardly in unwanted psychological waters.

> It is an achievement when others think that one's arguments are important enough to be denounced and demolished. Even to be demolished is better for one's self-esteem and reputation than to be ignored: It requires some ability to excite and especially to outrage one's fellow professionals (Stigler 1982:213).
>
> He liked upsetting people. I told you he wrote that column for *Business Month*. After a year went by, nobody had criticized it. They didn't get any letters to the editor. And you

The confusion perhaps comes from a failure to distinguish Darwinism from the social Darwinism of Herbert Spencer. "Tell her [Bobby Solow] also that I predict that eventually every nineteenth century figure will experience a revival, and Herbert Spencer's turn is soon." (Letter from George Stigler to Robert Solow, November 6, 1959) Fierce competition certainly doesn't ensure the survival of the fittest, but the survival of the idea that best fits. The loss from this muddle is the mutual advantage that comes from being willing honestly to exchange ideas and being as skeptical of one's own position as of others'. (We can note here the gains that have accrued to the profession by opting for communal intellectual property rather than the private or collective categories.) A skeptical attitude is perhaps a requisite characteristic of any honest academic. The willingness to accept received wisdom is no real virtue. Though to go to the extent eventually favored by Knight, flirts with an unhealthy degree of nihilism.

> This despite the fact that the more you really know about Frank Knight, the more you realize that actually socially and politically he must have made George Stigler very uncomfortable. You couldn't categorize Frank Knight as left-wing/right-wing. He was more complicated. But, he had huge doubts about the whole of economics, orthodox economics. He changed very slowly. By the 1940s and 50s he no longer believed that economics really had anything to say about the important problems of life. And that must have annoyed Stigler, irritated him, because you know, he really did believe economics had answers to everything. (Conversation with Mark Blaug, April 1998)

Stigler's unshakable skepticism surrounding government intervention produced a healthy respect for the damage that government failure could bring. This conclusion in retrospect should have come as no surprise since government is a human creation and humans are inevita-

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know, he had said so many outrageous things: that insider trading is really okay, that sort of thing. He said things meant to upset people. Well, he gave it up. He wasn't having any fun. He wanted people to criticize his ideas and then he wanted to come back with his rejoinders. You know, he wanted to have a little controversy. (Conversation with Claire Friedland, November 1997) bly given to error rather than perfection. But it seems at times that his burnishing of the marketplace and of the role that markets play makes them appear other than what they are, namely yet another reflection of human frailty.[174] You can argue over which institution is more fallible, but both are given to error, which should require economists to be wary of either.[175]

> Milton Friedman: It is true that George would tend to emphasize, more than I would, the extent to which you were just spitting in the wind if you try to advise governmental officials to do something which has not hereto-

[174] An unwavering belief in the efficiency of contracting and an inherent faith in the lack of any significant economic power within the marketplace seems the basis for such an absence of skepticism. This apparent assumption that all economic agents contract from positions of equal bargaining power seems to drive much of Armen Alchian's work. An inventive economist who remained an unyielding believer in the efficiency of markets, he has been described by Paul Samuelson as, "Armen Alchian, who is more Catholic than the Pope, never went to University of Chicago but is a real Chicagoan" (conversation with Paul Samuelson, November 1997). This assumption of equal bargaining power between contractual agents is easily noticeable in his classic work with Harold Demsetz (1972). In essence, this approach can be summed up by saying "what is, is efficient"—applying the survivor principle to all economic outcomes. The implicit associated lemma is that what is efficient also turns out to be fair. This surprising conclusion pops up in the 1972 paper as well as in Stigler's Tanner Lectures (1982a). If anything, Alchian was even more of a true believer than Stigler, occasionally pointing out his minor transgressions in forgetting that a market is a market and none should be privileged.

> I also admit to no privileged position for the university … Like a shoe producer, a butcher or an advertising agency, the university has a product to sell. I care not if some of these universities propose to suppress certain ideas of their employees (who can go elsewhere) nor if the university has an ax to grind. I care not so long as the university is not a monolithic monopoly of the state—as it threatens to be. The access to the market for education, truth, falsity, shoes, wax, and what you will is what gives truth a chance to survive. Nor is it necessary—not even desirable possibly?—that the truth be the goal of the agents that get it revealed. Your position strikes me as perilously close to advice to a paternalistic agency. On the same grounds I see no reason to expect newspapers to have a responsibility to be honest. Let them lie as they will. I rely on access of others to expose the lie and to cater to the public's desire for truth—or to whatever extent the public and individuals wish it. (Letter from Armen Alchian to George Stigler, March 25, 1967)

[175] Evolution and competition doesn't necessarily yield any sort of perfection in the biological sphere as well, since the process must use the materials at hand with the selection process being time and place specific. Certainly individual acts of creation by some all-powerful incarnate spirit might be preferred, at least in theory. People suffering from chronic back problems might well agree with such a hypothesis.

fore been in their self-interest to do. It may be so most of the time, but where that comes from, I don't know.

Aaron Director: Where what comes from? I'm just asking him to say it over again.

Milton Friedman: Where George's going so far in the direction of not letting anything other than self-interest influence governmental behavior comes from. (Conversation with Milton Friedman and Aaron Director, August 1997)[176]

Unfortunately, competition encourages putting forward one's own ideas in the strongest light possible while increasing the temptation to strengthen that position by belittling and trying to eviscerate opposing arguments.

Rose Friedman: I don't think George had much tolerance for stupidity.

Milton Friedman: I don't think you're getting at anything that is really specifically George Stigler. I think you are getting at something that is (a) the atmosphere at Chicago, and (b) intensified by Knight. That an academic is concerned not with being diplomatic, not with trying to avoid hurting people's feelings, but an academic is concerned with saying what's right. Telling the truth, or trying to get at it. And if you disagree with somebody you don't say "well, now there may be something in what you say …"

Rose Friedman: "You may be right …"

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[176] The need for his approaches to be consistent and universal perhaps acted as a compelling factor forcing what seemed at times to be an extreme black-and-white view of the world, one without nuance. This aspect of his pronouncements seems more closely to image a marketing necessity than perhaps what Stigler would consider to be the case, under more reflective conditions.

Like when he was confronted with some fact about regulation, he would say "Ah, you're going to find some Congressman was bought off. [laughter] You are actually going to find that. That's what you're going to find. Are you sure that you didn't find that this Congressman wasn't on the take?" You know, that kind of writing, that kind of a very strong view … I know he would often criticize people for trying to explain every observation and not accepting the fact that there was a certain degree of error. But when in the middle of a lunch time conversation, if you confronted him with something like that, with some anomaly, he could have just said, "Look, that's the error. Let's just go on and talk about the substance." But he would in fact say, "No. You're going to find that some interest was in there on the political front. You'll find it if you look hard enough." You wouldn't find it. (Conversation with Sam Peltzman, October 1997)

Milton Friedman: You say "that's a bunch of nonsense."[177] (Conversation with Milton and Rose Friedman, August 1997)

In effect, the profession itself has only been restrained not by the Smith of *The Wealth of Nations* but rather by the Smith that Stigler found to be something of an anomaly, namely the one found in *The Moral Sentiments,* where narrow self-interest is constrained by concern and sympathy for how we appear to others. Smith describes the feeling of not living up to our own standards, of letting others down.[178]

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[177] Sherwin Rosen, who had seen the profession evolve over his years as an economist, had to confess that marketing played an increasingly significant role in the field. Certainly you couldn't succeed by marketing any random concepts; it had to somehow connect with deep-seated preferences of your target audience. But it remains a question as to whether it is now possible to achieve anything resembling a reputation within the discipline without some ability to adeptly spin one's ideas.

> Marketing is part of that business. There's no doubt that some people are able to market their work better than others. Or perhaps, it somehow catches on for reasons that are hard to say. I *hope* it catches on when it has something to it. I don't think marketing is all there is to acceptance. If you happen to have a product that is no good you might market it well but it's going to fail. So I do think marketing is very important when undertaking an intellectual activity. It's probably more important *now* than it was before. The market is much bigger [laughs] for one thing. You have people making serious money out of marketing ideas and getting reputations from successfully doing that. But it's not only the money. It's the associated prestige and all kinds of other things as well. It seems to me that there's a lot more score keeping these days. Unlike in previous days, Economists are now reckoned to be in a pretty interesting discipline. So to succeed, you've got to be able to sell your ideas somehow. How do you do that? I don't really know. Maybe you do attack other people. That's been done over the years, but it doesn't always work. It doesn't work if there's nothing behind your attack. (Conversation with Sherwin Rosen, October 1997)

[178] Given Stigler's personality, stance and intellectual standpoint it is hardly surprising that Smith's *Moral Sentiments* continued to puzzle him. Building a grand abstract and generalized system using self-interest as a cornerstone left the detail, psychology and the methods of application in this work at odds with the direction in which he saw Smith's *Wealth of Nations* moving. He failed to see how Smith constructed these works so that they formed pieces of the same puzzle explaining humans as economic agents.

> I don't think he thought that much of the *Moral Sentiments*. He thought *Wealth of Nations* was the greatest book ever written in economics. There was no question about Smith as the greatest economist. I can't recall exactly what he said, I mean about Smith, except he would sometime speak about the literature on the so-called Smithian problem. How could somebody who wrote the *Wealth of Nations* write a *Theory of Moral Sentiments*? He felt that markets were best understood, George as well as Smith, in terms of self-interested behaviour. And *Moral Sentiments* doesn't deal much with that issue. So he thought in terms of understanding markets, economists deal with markets. What was in the *Moral Sentiments* wasn't much relevant. You know it would be George's view basically. I don't know if he'd say it wasn't a good book. He didn't think it was as good a book as Ronald Coase thought it was, or I thought it was. That I know. I wouldn't say he thought it was a bad book, but in terms

> We resolve never to be guilty of the like, nor ever, upon any account, to render ourselves in this manner the objects of universal disapprobation. (Smith 1966: 224)

Keynes had hopes that economists would one day belong to a modest, but useful profession, one perhaps resembling that of dentists. Now Stigler might be right in that Keynes saw himself more as a brain surgeon than as a mere dentist.[179] However in the relevant article, Keynes is looking to the future and perhaps a bit of brain surgery in the short run might prove expedient.

> But, chiefly, do not let us overestimate the importance of the economic problem, or sacrifice to its supposed necessities other matters of greater and more permanent significance. It should be a matter for specialists—like dentistry. If economists could manage to get themselves thought of as humble, competent people, on a level with dentists, that would be splendid! (Keynes 1963:373)

George Stigler demonstrated that by nurturing a talent for marketing it is possible to shape future discussion within the profession by shifting the terms of debate. Like a good general, by controlling the high ground he could have greater leverage in directing any subsequent argument or theoretical analysis. This shifting often meant that issues would be addressed that were more conducive to his own position and which existed on more congenial terms. These objectives remained quite achievable. However, he instead insisted on clinging seriously to some quite unrealistic dreams of entirely eliminating despised hypotheses. Doing so could prove to be downright foolish. Stigler would have displayed more

> of understanding markets, what you got out of the *Wealth of Nations* was a hundred times more useful than what you got out of the *Moral Sentiments.* (Conversation with Gary Becker November 1997)

[179] In his Adam Smith lecture given to the National Association of Business Economists (October 1987) George Stigler estimated that Keynes' own ambitions were not quite so benign. However Keynes did see the dentistry track as only lying in the future, with any luck arriving in the time of his generation's grandchildren. But clearly, the necessity for Keynes in the present was to refashion economics by helping the discipline escape from its pernicious past.

> He [Stigler] concluded that Keynes sought to be, not a dentist, but "a brain surgeon who operated on ideologies." (Leeson 1997:6)

sense by trying to defuse dangerous alternatives rather than attempting to obliterate their living memory. Thus it is probably naïve to attribute the death of any theory to one particular individual or group. This holds true despite the wish by Stigler that such theories had been euthanized at birth. Tactics are shaped by circumstances and what is deemed to be possible. Creating confusion and muddle concerning the intentions or meaning of an author, or groups of theorists, is unfortunately far from a daunting or an unachievable objective. Recent research has confirmed an intuition that many academics may hold. Namely, once a widely spread opinion gains credence, it is hard to subsequently dislodge it from the oral history of the discipline. Our interpretations, what we thought we read, or even what we think we know, can be skilfully shaped.

> The Web, it should be said, is still a marvellous place for public debate. But when it comes to reading and understanding news stories online … the medium can have a surprisingly potent effect on the message. Comments from some readers, our research shows, can significantly distort what other readers think was reported in the first place. (Brossard and Scheufele 2013:1)

Despite a necessary realization of the numerous problems to be surmounted by attempting to communicate an idea, Stigler chose to cut theories and their presenters off at the knees whenever possible.[180] Partly, it is possible to be reductionist and claim he did this because he could. But his competitive nature and the objectives for which he battled, especially when he saw himself as one of a small bank of counter-revolutionaries battling the monolith of received economic wisdom, made such generosity and tolerance difficult for him to embrace. Tolerance doesn't impose

[180] Deirdre McCloskey recalls Stigler reducing a presenter to tears at his Industrial Organization workshop (personal communication, June 2003). But perhaps Claire Friedland, who was George Stigler's constant and faithful research assistant during his many decades at Chicago, summed up his rare ability to be simultaneously amusing and devastating when taking aim at another academic. It is doubtful that his targets were able to properly distinguish whether the objective was personal annihilation or simply aimed at questionable theory.

> As for George's caustic wit, he never let go one of his barbs for the sake of mere one-upmanship. They were always aimed at the target's ideas, not the target himself: even when a workshop speaker asked whether he should deliver his paper standing or seated and George responded, "With a paper like this, under the table would not be inappropriate." (Friedland 1993:781)

a prohibition on tough questioning, or a mandatory imitation of John Stuart Mill, but it does require that an effort be made to fully understand another's idea as well as a willingness to respect thought which runs counter to one's own inclinations.

> This means, on the one hand, that an economic writer requires from his reader much goodwill and intelligence and a large measure of co-operation; and, on the other hand, that there are a thousand futile, yet verbally legitimate, objections which an objector can raise … .(Keynes 1973: 469–470)

But the fact that marketing still holds sway within the profession perhaps reflects a wrong turning taken in those post-war years. Going back to an older tradition of modesty, in our results and policy prescriptions, might initiate a reign of mutual respect and tolerance. Unfortunately, how that might be achieved continues to elude not only me, but seemingly the rest of the profession. It is a pity then that more attention was not paid to John Maurice Clark's presidential address at the 1946 meetings of the American Economic Association. Implementing these simple desires might have proven difficult but, with a smidgeon of good fortune, they would not have proven an insurmountable barrier.

> Can he treat every theorem as a tool of analysis and not an end-product; an approach to reality rather than the terminus of the journey; a partial and provisional truth, always on trial? And can he rise superior to his own intellectual vested interests in these theorems … which he will always have: … Let us have not cleavages but fruitful differences, and let us build as many bridges as we can. (Clark 1947:10–11)

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De Gustibus Non Est Disputandum: George Stigler through Gary Becker's Eyes

I waited in an anteroom on an unseasonably hot October morning while Gary Becker's secretary typed away. Like all the offices housing the Chicago economics department, the room is shabbily comfortable. Professor Becker walks in and is reminded of a staff seminar later that day. As the secretary recites the title of the scheduled paper, Gary Becker's eyes involuntarily roll. I can easily relate to that. It is the "Life's too short to listen to a yet another incredibly narrow paper on arcane techniques without any clear economic applications." Many of us reach that age, only the timing varies from one individual to the next.

Gary Becker is perfectly still in his chair behind the desk and the prospect of carrying on any extended conversation with him is more than a bit daunting. Yet, he proves to be interested in considering questions and thinking through any implications that arise. What can't be conveyed by simply reading the edited transcript of the conversation are the pauses. I received the distinct impression that any issue I may have aired was examined thoroughly, flipped over a few times and carefully weighed before a response was forthcoming.

C. Freedman, *In Search of the Two-Handed Economist*,
DOI 10.1057/978-1-137-58974-3_6

This would turn out to be my most exhausting session as I needed to focus constantly on keeping on top of, or at least not being submerged by, the extended conversation. In a sense I was like a young child able to remain in the room only as long as my good behavior was assured. As long as our talk continued to spark his interest we were able to keep the discussion going. But throughout, Gary Becker remained perfectly amiable without putting any limitations on where the conversation might flow. The promise initially made was that I would finish off in about an hour. It is actually some 80 minutes before we do complete, which explains Professor Becker's gentle reminder as we near the end.

The Conversation with Gary Becker

If I could start off by asking you a bit about how you first came to Chicago. Why did you in fact come?

Me, well, I came as a graduate student …

As a graduate student?

Yes, it goes back a long time. I knew about two things. I knew about Milton Friedman, and I knew that the Cowles Commission[1] was here. They were here in those days. I was interested in the mathematical aspects of economics. And, I had my choice between Harvard and Chicago, that's where I was thinking of going. I'm not sure I came for very good reasons.

[1] The Cowles Commission was established in 1932 by Alfred Cowles, president of an investment counseling firm who also underwrote the founding of the journal *Econometrica.* Its initial home in Colorado Springs proved too removed from academic activities. The Commission moved to Chicago in 1939 and remained there until 1955. Strong ties developed between the Commission and the University of Chicago, Department of Economics. Jacob Marshak was not only the Director of the Commission (before giving way to Tjalling Koopmans) but was also a member of the Department. Chicago academics holding dual appointments in these early years included Joel Dean, H. Gregg Lewis, Jacob Mosak and Oscar Lange. They were later joined by others, such as Don Patinkin. However, relationships between the Cowles Commission and other members of the Department of Economics (like Milton Friedman) were not so cordial. George Stigler, writing to his friend Milton Friedman (December 1947), describes Colin Clark as being someone who "would be marvelous in infuriating the Cowles boys, although probably not your equal" (Hammond, J. Daniel and Claire H. Hammond (2006) *Making Chicago Price Theory.* London: Routledge, p.73). During this Chicago sojourn, Milton Friedman mounted opposition that nearly reached the level of a guerrilla campaign against the Commission. In 1955 the Cowles Commission relocated to Yale University. For those interested in its sojourn in Chicago details are available in Hildreth, Clifford (1986) *The Cowles Commission in Chicago: 1939–1955*. New York: Springer Verlag.

I came because of the Cowles Commission and I came out because I was on the East Coast and I wanted to go out to the Midwest. You might say I came for accidental reasons. I'd known Friedman. He'd come to Princeton once, but gave a paper that didn't impress me that much. It was probably my own limitations at the time I think, but the fact was it didn't impress me that much. I didn't talk to him at all when he was there, so I can't say I came for Milton Friedman or the great Chicago traditions. I would say I was interested in the Midwest and the Cowles Commission. These were probably the reasons I came.[2]

Do you ever speculate what would have happened if you had gone to Harvard? How things would have turned out?

Well, I don't because my close friend at Princeton also applied to Chicago and Harvard, got into both like I did. Both of us got good fellowships. He chose Harvard. He became pretty well known. Otto Eckstein[3] is his name. I don't know if you are familiar with that name.

Yes.

And so, we both speculated about how different we would have been if we had changed universities. No question we would have been different. Although we both, I think, made the right choice. His interests were very policy-oriented, interventionist. I was, you might say, more pure theory, if you want to use that word. It wasn't quite the case, but close to that. I think we both made the right choice. I always feel I was extremely lucky that I made the choice of coming to Chicago. I cannot believe I would have been nearly as successful as I have been if I'd gone to Harvard. I mean, I had such a great collection of teachers and students, but teachers in particular. The atmosphere here, to me, was a wonderful atmosphere.

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[2] Gary Becker arrived as a graduate student at Chicago in the fall of 1951. After three years of graduate school he was appointed assistant professor in the Department. He accepted a position at Columbia in 1957.

[3] In some ways, Otto Eckstein (1927–1984) was the antithesis of his friend Gary Becker, being the very model of those post-war economists who moved so easily between academia, government and the corporate sector. Among a number of government posts, he served as a member of the Council of Economic Advisors (1964) alongside Walter Heller and Gardiner Ackely. In the late 1960s he co-founded Data Resources Inc. to provide econometric forecasting services to corporate clients. When he sold to McGraw-Hill in 1979 his shares were worth $20 million. From 1975 he served as Paul M. Warburg Professor at Harvard University. He was a registered Democrat and moved in those circles. Gary Becker, during his lifetime or even after, would never be mistaken for a registered Democrat nor was he ever inclined to move in such realms.

So, you never know. I won't do any counter-factual history, but I know I feel very lucky I went to Chicago.

Now, when did you actually come into contact with George Stigler, because when you were here in Chicago in the early fifties, George Stigler, of course, wasn't here?

No, he wasn't here. My contacts with George Stigler, well there's an interesting history in that regard. Oh, and I did send him a copy, when I completed my thesis. Of course, I'd read Stigler's stuff when I was at Princeton as an undergraduate. He came down and gave what I did think was an interesting paper. I can't say I fully understood it. That's his paper on the division of labor being limited by the extent of the market.[4] He gave that at Princeton. I attended that lecture. I found it very interesting, and I found him to be very witty. And I read it, as well as a number of other things when I was an undergraduate. I read his *Theory of Competitive Price* at the end of my freshman year.[5] That was a predecessor of his *Theory of Price*. I read that, and a lot of other things by Stigler. I knew his name very well. I knew he was a close friend of Milton Friedman's who I got to be close with at Chicago. So when I did my thesis on discrimination, I sent him a copy of it.[6] He sent me back some very favorable but very useful comments.

So then when I was going on the job market, I wanted to come back out to the East Coast and I thought Columbia would be a good place for me because Stigler was there and then I could be with the National Bureau, too, like he was. So, the Bureau offered me a position. I accepted Columbia. Unfortunately, George Stigler was away that year. He was at the Centre for Advanced Study. It was '57. He and Friedman were both

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[4] The paper appeared in 1951 as "The Division of Labor Is Limited by the Extent of the Market", *Journal of Political Economy* 59(2): 185–93.

[5] *The Theory of Competitive Price* (1942) New York: Macmillan extended Stigler's exploration of economics only through perfect competition. He later expanded this base to include other market possibilities in *The Theory of Price* (1946) New York: Macmillan. This version went through four editions with the fourth (1987) perhaps encouraged by the afterglow of Stigler's Nobel Prize. Becker would have been a freshman at Princeton in 1948. Why he read *The Theory of Competitive Price* instead of *The Theory of Price* must remain a mystery, though of only trivial import.

[6] Becker completed his dissertation under Milton Friedman with a pioneering study focused on the economics of discrimination. It was soon afterwards published by the University of Chicago Press. Gary S. Becker (1957, 1971, 2nd ed.) *The Economics of Discrimination*. Chicago, University of Chicago Press.

out there. And I heard rumors that he wasn't coming back. I wrote him and remember saying to him "I hope you're coming back. You know, I would plan to stay on here if you are coming back." And so on. Well, he left. And that was a big blow to me, his leaving. Because he was the one main person I was interested in at Columbia. Some time later on he said to me, I'm sure joking, he said if he had known I was going to do as well as I did he might have thought of staying. But, I doubt it. His leaving was a big blow to me. But obviously, there was nothing I could do about it. I mean, I wasn't privy to what was going on at Columbia. They handled it very badly. I did think they in fact had a good shot at keeping him, but it was terribly handled, as Columbia handled a lot of things and, he decided to go. But it wasn't an open-and-shut decision on his part.

So, when did you actually start to have more contact with him?

Well, you know, in the interim before I came back to Chicago. That was between '57 and '69. I was at Columbia. I did see him on various occasions and corresponded with him a little. Didn't know him very well, but you know, because I was, had been, a student of Friedman's and so on we got to be, not friendly, but to know each other. But my real contact with him began in the 60s when I got to know him. Then I was thinking of coming to Chicago as a Visitor for a year. … I got to get this history right. Yeah, for a visit in '69. I already knew him, and I told him that I was coming. Then, I had to cancel because my first wife was sick. And I had to cancel the appointment. Then at the last minute she was getting better, but I already cancelled the appointment. I wrote to Stigler, this was in September. I said to him, I could come if there is anything open. And he said "Come. I'll guarantee we'll cover it… cover your income if the Department can't do it." So it means that I knew him already well enough for that to happen. I can't remember all the details of how I knew him so well. But I came at that time. I came as a Visitor. I got to know him quite well during that year. Attended the Industrial Organization workshop regularly, which was very much an eye-opener for me. And then, after a lot of hesitation, he and Milton Friedman worked on me a lot, he offered me extra research money if I stayed on. This was Walgreen[7] money and

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[7] Stigler was the first to hold the Charles R. Walgreen Professorship of American Institutions. In 1958 it had an attached salary of $25,000, unheard of in those days. The chair also came equipped

it was a large sum. So he said I could have some research money and I decided to stay. After this, basically, he became my best friend. I got to see him very, very frequently, at the IO workshop. We wrote a couple of papers together. We saw each other a lot socially. I played tennis with him and with Milton Friedman. So I had a lot of close contact with him over that time and definitely he became an even closer friend of mine than Friedman was. He was a close friend of mine, even though there was a fairly big age gap between us. We saw each other a lot.

Can I ask you now a few questions about your work together?

Yes.

C*an I start with the joint paper, the **Non Gustibus**[8] paper? First, though to make sure we're communicating let me just see if I've got the right idea. Thinking through the argument, it's sort of saying, "OK, this idea that we're just buying goods and services isn't really the right way to think about it. Actually, we've got these preferences. We have got sort of a production function that'll provide us with utility to satisfy these preferences. So if, for instance we buy an insurance policy we're not buying an insurance policy. Maybe our preference is for security and we see utility flows of this sort coming from insurance. In this world, advertising is simply another input to persuade us that this product will give us the right flows. Is that sort of a rough …?"*

Yes, I mean roughly what we were doing, we were trying to go behind what people with demands for goods want, to discover something more

with ample research funds which enabled Stigler to provide inducements for rising young lights in the profession to come to Chicago.

[8] The paper referred to is "De Gustibus Non Est Disputandum", *American Economic Review*. 67(1): 76–90. The original referee report of what would become a seminal paper was mixed.

> The referee and I have read your paper, "De Gustibus Non Est Disputandum", subtitled "There's no arguing over differences in the household production functions." The referee and I like the paper, but its unfulfilled claims and self-congratulatory tone offend his sense of modesty and its size offends my sense of economy. The most interesting and original part of the paper is the section on advertising. The other sections are weaker either because they do not appear to generate any conceptually refutable hypotheses (addiction, habits, and fads) or as in the case of time preference and search theory, they are so far removed from the original premise (behaviour which had been rationalized by changes in taste). (Letter from the AER editor to Gary S. Becker and George J. Stigler, April 26, 1976.) (Letter located in George Stigler Papers, Special Collections, University of Chicago Library, Box 20/De Gustibus)

basic. I'd written a paper in '65, "A Theory of the Allocation of Time"[9] trying to do that, but in this paper we were saying, "Suppose we take that framework and apply it to some challenging and difficult issues, addiction and advertising and so on. Does it shed light on these problems?" That's what we tried to do, take really hard problems and see if we could make some progress with them. Actually the origin of the paper was that Stigler came to me one day and said, "Look at this quote by Alfred Marshall." Alfred Marshall had said something like "With respect to classical music it's not true that the more you hear the lower the marginal utility. The more you hear, the higher the marginal utility." How do we explain that? I thought about it. I came back with some suggestions about putting it in this production framework. We started talking about a lot of other examples and working it out. We worked on it and eventually worked it out. That's exactly what we tried to do, but in the context of difficult questions that we felt hadn't been satisfactorily answered.

And, it does take a different approach to advertising. I'd say the standard approach in the 50s and 60s is that somehow advertising changed preferences, which, of course, eliminates consumer sovereignty to some extent.

Right, I think it does, yeah.

This approach, though, restores consumer sovereignty.

That's what we tried among other things to do. We thought that's what it could accomplish, as well as help explain some parts of advertising. We got some different implications in areas like advertising, in competitive cases as well as monopoly and things of that type. I think that's true. It did. It doesn't mean necessarily everything is for the best with advertising, but at least you can discuss it in a framework that is similar to the usual framework and talk about consumer sovereignty and get an efficient outcome, or not. With the ordinary approach you really had to do handstands to achieve anything. It's very hard to do this, if you assume there are shifting preferences. I agree.

Now, can we then conclude that George Stigler and you would say that we can apply this same framework if we think about the marketplace for ideas? Is it similarly applicable?

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[9] Gary Becker refers to his 1965 paper, "A Theory of the Allocation of Time", *The Economic Journal* 75 (299): 493–517.

I would think so, in general. No reason why not. There is a marketplace. We didn't do that application, although Stigler's written on that subject.[10] But, in principle, surely it would be applicable. Yeah.

OK. Let me just throw this out to you. If you think through this approach, this means then, if you've got, say, a theory, a model and evidence etc., *you go into the marketplace, say, among the academic profession. There you have to convince people that this approach delivers whatever their preferences are. That it meets their preferences.*

Mmm.

So, in a sense you can't say this is getting somewhere nearer to the truth, whatever that may be, or to reality. This is simply meeting the market demand better than some other theory which we are trying to displace.

Well, yeah. Now that is what you would be saying. No question about it. Because what meets market demand may have an element of so-called truth in it, in the sense that what people think better organizes the world for them, about how do we know what shapes events and so on, would presumably be one of the factors (and we hope an important factor) in determining legitimacy. But it wouldn't be the only factor. We would need other factors. And where these other factors dominate you'll get other results. That's why you get fads and fashions in ideas and so on that don't have lasting value. They will still be meeting the marketplace of ideas and I would say, I think that is right.

So then, the job as an academic is simply to persuade. Part of the input is not only the model you construct, but part of it is the persuasion, the marketing etc.

Part of it is the persuasion. There's no question. George Stigler, I remember when I was a young person, wired and said "Selling is very important in your research. So write better. Work on writing because that is important. You've got to sell what you are doing." I think he's exactly right. You've got to sell what you are doing. It may be that in the long run good ideas do surface but they surface faster, if written in a persuasive fashion. Moreover, bad ideas may be put persuasively. And they may gain the necessary threshold. However, taking that same analogy in competition

[10] This appears in the 1963 collection of essays, *The Intellectual and the Marketplace, and Other Essays*. New York: Free Press of Glencoe.

among ideas, there is a presumption, although not a certainty, that in the longer run, the good ideas are going to compete out the bad ideas. But that may take a long time and may not even always operate. There's nothing necessary about that. Nothing guaranteed about that.

Absolutely, just the hope …

Yep. Nothing is guaranteed. But it is a framework for understanding. It is a positive rather than a normative framework. It's a framework for understanding how ideas get accepted or rejected and so on.

OK. So George Stigler would accept that?

I think he would, yeah.

Good. That clarifies things. Now, how far then would you, would George Stigler want to push this?

With ideas or in general?

In general. With ideas also, but in general as well.

As a way of looking and choice and so on? The way we did in "Non Gustibus"? Well, I think pretty far. I mean it's hard to say how far. We pushed it pretty far in that paper already and we'd be willing to push it even further. We did push it separately in our own thinking about subjects into other areas. I don't think he had any qualms about pushing it pretty far. In that I don't think we ever were in serious disagreement. He may have questioned pushing certain ways of doing it … certain particular things or particular innovative ways. For example, when I was working on altruism, initially he was very skeptical about whether that was important even in the case of the family.[11] Although, I *think* he eventually came round on that view, there, once again, you just put something in people's preference function. But there the question was more technical. Do you need that to understand family behavior? That was a relevant question. He and I may have had some differences beyond that. I may be more inclined to believe that altruism was more important than he was, although I don't think he would deny some family feeling. His own behavior to his

[11] Among other work, Gary Becker may be referring to such seminal works as his 1974 paper, "A theory of social interactions", *Journal of Political* Economy 82(5): 1064–93 as well as such later works as a 1986 publication with Robert J. Barro, "Altruism and the Economic Theory of Fertility", *Population and Development Review* 12 (Supplement): 69–76, or a later article in 1988, "Altruism in the Family and Selfishness in the Market Place", *The Journal of Political Economy* 96(4): 675–700.

children and his wife indicated that there was some altruism. So in particular applications he may have had a view where he'd insist that this point of view while possible was not useful. But on the whole I think he wanted to push it very far. He did push it very far. Take his work on the study of so-called sociology of science. I don't think he hesitated to say that people resisted ideas when they had a lot of capital built up in older ideas. He says that, I'm sure, someplace. He believed that, I know and I'm sure he said it.

Okay, because he does make a very big break from the teaching of Frank Knight on that account.

Yeah. You know his view of Frank Knight. When he was a graduate student early in his career he thought Frank Knight was the greatest, as he told me on more than one occasion. As he got more mature and he read Knight's stuff, his opinion of Knight went down. I know that comes across in many of his writings. I'm reluctant to say that because he was very close, one of the few students who ever wrote a dissertation with Frank Knight. He always said that. But he came to feel that Knight was flawed … and that was my opinion, independent of Stigler. When I was a student I thought Knight was great, but my opinion of Knight went down over time. The reason why mine went down, I knew was the same as George's. Knight makes a lot of strong assertions, but when you ask "why, why?" a lot of his things don't hold up so well. I think Stigler began to get disappointed. Now, he believed *Risk, Uncertainty and Profit* was a great book. He continued to believe that. It is a great book, but in general, his opinion of Knight went down. Over time it went down and probably his opinion of Viner[12] went up. These were his two major teachers at Chicago. But I know he said that about Knight on several occasions.

Certainly in his own work, the ideal of any sort of uncertainty, randomness or those sort of factors don't seem to play any role.

No. In the information stuff he writes, he brings in uncertainty, but he wasn't a major worker with uncertainty. I think the way he worked was

[12] Jacob Viner (1892–1970) along with Frank Knight were both key figures in the first Chicago School of economics which flourished during the interwar period. He remained at Chicago from 1919 to 1946 before leaving for a position at Princeton. He was most noted for his approach to trade theory though also possessing an almost encyclopaedic knowledge of the History of Economic Thought. Through his graduate course on price theory he ultimately influenced the thinking of such distinguished economists as George Stigler, Milton Friedman, James Buchanan, Don Patinkin and many others.

more similar to Jacob Viner than to Frank Knight. The sort of economics he did was more like Viner's custom theories and so on, which were empirically based. Knight would not look at any data. In fact, Frank could hardly be convinced by any data. Like inequality. Knight always thought inequality was growing in the United States while all the evidence up until 1970 said it was falling. And Stigler and Friedman and others would point this out to Knight, and George told me this, Knight would say "yes, yes" but next time he'd say the same thing. So, I guess, he differed with Knight in this regard but that was not unusual. He began to differ with Knight in a lot of respects.

Also, on the idea of quantification.

Yes, I'm saying he becomes more like Viner. Look at Viner, early on in Viner's own dissertation on international trade, he is already testing the Canadian and American data. George became a big empirical testing guy. As with Friedman, Viner was the greater influence. Not Knight.

It is interesting that in almost all, at least many, many of his papers there seems to be empirical tests involved. Often, even though the empirical test itself may not be the most convincing part of the article. I'm thinking, say of the '71 Regulation paper.[13] *The argument is compelling. It certainly catches your attention. The empirical evidence however is sort of dubious.*

Right.

If he presented a testable hypothesis why did he feel he had to actually do the empirical work?

Well, you know a lot of his papers are pure theory. Particularly in some of his earlier days, but as he developed, he came more and more to believe that you had to try and explain behavior. I don't think he felt it was strictly necessary for everybody to do their own testing. But the Chicago tradition began to be that if you were a theorist, you had to do some empirical work. George certainly felt that. And that was what was conveyed to students. We had very few purely theoretical dissertations here. People had some theory and they went out and tested it. And a good theorist was an applied theorist who did some testing. Maybe as you say, it was not the most detailed or most convincing empirical work. He was not an

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[13] The article appeared as "The Theory of Economic Regulation", *Bell Journal of Economics and Management Science* 291): 3–21.

econometrician. He did not work with a problem for years. He had an intellectual impatience about him, George. But he felt strongly that much of the time, to see if the theory was working, he couldn't rely on other people just showing or testing it. You had to show that it explained what was going on. Not in a conclusive way always. It's impossible to do that, but at least he brought some evidence to bear on it. I think he felt that strongly and that's why you find in his later work, beginning in the '60s, in much of this work there is some empirical testing. Not in all of it, but his theory of information stuff has some data he put together and in most of his papers he had that.

It seems that in a lot of his work he doesn't want to continually follow on and do all the detailed work …

He didn't want to …

He's more of a catalyst then?

He was a guy with a lot of ideas and yet impatient. George had an impatient personality. Even in playing tennis, if he didn't get the point in two shots you could tell he really tried for a hard shot, he didn't want to just sit and hit it back and forth. He was a good tennis player, particularly when he was younger, but he had a real impatience. And that was true intellectually too. He wouldn't sit with one problem for five years as some people might do. And so in his papers, he has an idea and he tests it. So there are a lot of good ideas in his work, but he doesn't sit and stay with a problem for a long time. I think that's true. In that I think that he's substantially different from Milton, you know his work on money or his work on consumption functions were longer ventures, he took longer periods of time. He had more patience. George was not the most patient researcher.

No.

And that was his characteristic. As a person too.

Did he have any reaction to the stuff that started to come out in the 1980s by people like Tversky and Kahneman?[14]

[14] Daniel Kahneman and Amos Tversky (now deceased) are two psychologists who are largely credited with helping to formulate behavioral economics as a legitimate alternative to the sort of rational decision maker at the center of standard economics. A seminal collaboration of the two appeared in a 1979 article, "Prospect theory: An analysis of decisions under risk", *Econometrica* 47(2): 313–327. Kahneman was awarded a Nobel Prize in 2002. He highly influenced the work of Richard Thaler who later went on to become one of the leading lights of behavioral economics.

He wasn't particularly sympathetic to that stuff. He wasn't of course heavily involved with it. He kept up with some of the experimental literature. I ran a seminar on rational choice and social sciences with Jim Coleman, a sociologist, which I still run. We had Kahneman, we had a number of other experimenters… and George came to almost every one of our meetings. So he knew that stuff. And he was not one of the people who were sympathetic to it. He felt that, as a number of economists did, that they were experimental situations and not market situations and so they are not really capturing people's behavior. On the other hand, as an editor of the *JPE* he published some of that stuff. He felt, well, he himself wasn't personally sympathetic, but it was a worthwhile strand of research and so on. But he was not a big advocate of that experimental work.

He also seemed to have no particular time for any survey type of work. Where you…

Survey data. Well, he would use survey data. He wasn't opposed to surveys. You know, but a survey is a big empirical project. It wasn't the sort of thing he did, as you said before.[15] He didn't do that type of work. You know, he wasn't trained in doing that. He could have learned it. He wasn't opposed to it on intellectual grounds. He thought surveys could be useful. He knew a lot of the work going on in labor economics, for example. He was familiar with that survey data. No, I don't think there was any intellectual opposition; he just wasn't experienced in dealing with that data. And in the IO field, survey data wasn't so important. Although he would use the census data that was obtained through surveys, census of manufactures and all that data that is obtained through the surveys. Individual household data, no he didn't tend to use that. Not as a matter of principle but given what he was working on it just didn't seem to fit. But if it did, he would use it.

He wouldn't though have much faith in going around and asking opinions of people?

Depends on the question. If you asked them what their earnings were he could accept that. He used that data. If you asked them why they did something, no. You know like in the minimum wage disputes

[15] However his 1970 book with James Kindahl, *The Behavior of Industrial Prices* (New York: Columbia University Press) is based on survey work of that type.

and so on. He didn't believe you could find out by asking people questions what their motives were. No. Factual things you could get, but not their motives.

I'm curious, I've always been curious and since I have you here maybe you could satisfy my curiosity. I was always intrigued by an article you wrote. (It's a short article, which I think is also applicable to the point you just made.) You wrote a short piece in the JPE about restaurant prices.[16]

Oh, yeah.

And, I have to say I was drawn to it because in another, earlier life, I put in five years working in restaurants, doing all sorts of things. In the article, you take the problem presented and you come up with an interesting pricing model that then is more generally applicable. But it's not actually consistent with my experience. You know, I had an alternative explanation. But what struck me then, is a question, I'm sure you've been asked, which is "Why didn't you just ask the manager of the restaurant?"

Yeah. Well, because I had the same view as George, on that issue. That I don't think you can talk with restaurant managers, in fact, about such things. You know they are not trained, they know in a certain deep sense, but they are not trained to articulate why things are happening. But any restaurant owner does in fact recognize it's good if you can get customers to come in and you can lose your audience pretty easily. You know there's unstable demand. Even when you're in the door, you can go out. That, they're all aware of, and so, in that sense they would say, "sure this is going on." In terms of pricing and so on, that would be a hard thing for them to articulate. I think that would be a hard thing to get by asking them. So I would have the same view, that yes, I use surveys in labor economics a lot. Surveys may give you suggestions about behavior but you can't really take that as the same type of evidence. That's my view in that little note. It actually was written, published, just about the time George was close to his death. I think I got it published in 1991 in the *JPE* in the later issues of that year. And he died December 1st, 1991. So it was around that

[16] The article referred to appeared in 1991 as Gary Becker accurately notes as "A Note on Restaurant Pricing and Other Examples of Social Influences on Price", *The Journal of Political Economy* 99(5): 1109–1116. In miniature, this is a gem that perfectly demonstrates the Chicago approach. It also provides a quick guide to how Becker analyses a given problem. It's an article well worth tracking down.

time. I'm trying to think if I knew what his reaction to that note was, but I can't really remember.

Now, many, many people have told me that he had a great deal of loyalty to his friends, but also to institutions like his department, school, and the profession. That he was very generous to his friends, even supporting students anonymously at times. Did that square with his professional belief in a sort of very self-interested mode of action? Or is this …

That's a good question. He was very generous. No question. He had a very tough exterior and he could be sarcastic and biting with people. George, if it was a choice between a good joke and making somebody feel bad, he'd take the good joke every time. That's how he was. He wasn't deliberately mean to some people, he didn't dislike them a lot, but he had such a terrific wit about him. You had to get used to it. It took me some time to get used to it. It took my wife a while to get used to it. But she began to love him too. Now did it square? I think, not in the following sense. And I raised this issue with him in a slightly different guise. When George was skeptical as I said about the altruism issue I said, "Look George, look how generous you are to your children. Are you doing that out of self-interested motives? Who are you kidding? You're not doing it out of that." And he looked at me and he didn't answer and he knew he wasn't doing that out of self-interested motives. He was actually very generous with his family, as he was with people in general. So, I think George, when pressed hard like I did there, would admit there is a motive beyond simply selfish self-interest. It could include his concern about others and generosity and so on. He didn't think we should bring that up too often, probably because he thought if we keep bringing that up, it's going to be a crutch that we're going to rely on just to use when the going gets tough. I think that's what he would answer. Yes, it's there. Sometimes it's crucial. Maybe especially in the family, but let's not rely on that for all the explanations. A lot of them have a more narrow self-interest, selfish interpretation. And it would be too easy if we opened it up to too many motives. That would be my view. I don't know if he ever put it to me in so many words, but that's how I would interpret it. That would be his view on these issues. But, yes, you don't want to push it too strongly 'cos he was an extremely generous person, despite his very ferocious and intimidating exterior that he had. And it was. I don't know if you ever met him?

No.

All right. But he was. I didn't bear the brunt of it because he liked me, so people said. But he gave it to people. He had such a quick wit about him. And very few people had his wit. He could always come up with something. Often it was at people's expense. As they got to know him, they knew he didn't mean anything by it and it was just that he had this wit about him. But people who didn't know him, they could be very hurt by it. Despite this, he was a tremendously generous person. He was very loyal. I always felt he was an extremely loyal person and so did other of his friends. And he was overly generous in giving credit to other people, including myself. I've seen him do it in the things he's said. He was overly generous about giving credit. He was a very generous person. And I think that is absolutely true. He was.

OK. Now, if I could look at another paper you wrote together, I think it's the '74 paper on Crime and Enforcement.[17] *One thing that is interesting is that some of the work in that is very reminiscent of work that came later on, things like efficiency wage, bond posting and all that sort of work.*

I agree.

And it is in there, to me reading it now, it is there quite clearly. Did anyone pick up on it?

Some people are aware. But some people … yeah we had all the bonding stuff. We had I think a better discussion of efficiency wage because we look at how you get rid of the rents. That people talk about. Some people who know that literature know that. Other people are ignorant. So it's a mixture. But there are people who are aware of that. People who are, I suppose not only here but elsewhere. We did have a lot of the principal and agent stuff in this particular context. We didn't use the language of principal/agent—we didn't use those words, but it's there when we talk about malfeasance. So I think, I think that paper's been pretty well celebrated. Maybe some people are ignorant of the fact that we had a lot of these ideas. But a lot of the profession is aware of it, particularly people

[17] We are discussing their first collaboration which appeared in 1974 as "Law Enforcement, Malfeasance, and Compensation of Enforcers", *Journal of Legal Studies* 3(1): 1–18. In the discussion it may sound as though I am trying to flatter. My admiration for the paper is quite sincere. It is a very interesting article whether or not you are ultimately convinced by it. Of their two collaborations, I would be willing to argue that this is the superior one.

working in crime and some of those related areas, even some of the malfeasance literature.

But, the two of you didn't take it forward, or did you?

No. We didn't do much else. I'm trying to think. Certainly not jointly. We did start a joint paper on trying to understand Congressmen but we never finished that together. In fact, he went out and did some work of his own on that.[18] No, we didn't do anything further on that jointly. I didn't certainly do anything separately. And I'm trying to think if George did. I don't think he did, but we were aware that we had the bonding stuff, principal and agent stuff, and so on. We knew we had that. Also sometimes we felt we didn't get credit, but often we did. So, you know, the profession is very poor in general in giving the right credit, so I didn't feel we were particularly discriminated against in that. We didn't use the principal/agent language. If we'd called it principal/agent I think maybe people would have seen. But, a lot of people have written articles indicating, you know, that they understood that we had it in there.

But neither one of you, for whatever reason, wanted to really push it further?

No, we didn't go any further. We should have I think. I always felt we should have written another paper. And, you know, pushed it further. We both got involved in other stuff really. That's what it amounts to, and we didn't do it. But I tend to think that was a mistake and we should have written a second paper on that. Put it in more specific language etc. Unfortunately we didn't do it.

What also strikes me is that, as opposed to a lot of the other papers of that time, although there is an obvious testable hypothesis, there is no actual empirical work.

No, there's not a stitch of data in there. We refer to what we think are observations, but no quantitative material. No. We didn't do any data gathering on that. Maybe we should have but, this was written initially for this volume celebrating Milton Friedman's birthday party.

I see.

It was published in *The Journal of Legal Studies*, but it was written even earlier than that, for his 65th birthday. And we were under pressure. I remem-

[18] See the article that came out two years later in 1976 as "The Sizes of Legislatures", *The Journal of Legal Studies* 5(1): 17–34.

ber when we were working on it at our summer homes … I was up at Cape Cod … We were under pressure to deliver it, deliver it at that conference. And on other occasions we also delivered it at the Mount Pelerin meeting when Enoch Powell (do you remember him?) was our discussant. His comments were completely orthogonal to anything that we said. He was a very smart guy, but his comments had nothing to do with our paper. But we gave it there and then I think Richard Epstein, no Richard Posner was the editor of *The Journal of Legal Studies* and he said we might like to publish it there. We decided to publish it with him. We could have published it in the *JPE* and anywhere else. I've always thought that was a really good paper to tell you the truth. Had we made more out of it, it would have had more influence. Because there were a lot of good ideas in the paper, but, you know, people who are really in the field know the paper and know that we had all this efficiency wage thing better, but a lot of people are ignorant and don't know it. I think that's true. I think it was good and regret that we didn't follow it up.

OK. What is also interesting to me, when I read that article is, there is a clear policy message, to me. I mean, it is in fact prescriptive to a certain extent.

It is. It does have a prescriptive component to it. I'd say that there are two sides of that paper. One is about how you should pay government people, and the other is private enforcement versus public enforcement. Both have been picked up. In fact I'm just publishing a little column in *Business Week* that will come out in a couple of weeks, where I talk about corruption and mention a paper by Stigler. I said Stigler and I wrote a paper in which we advocate higher salaries, so this is a prescription. No question about it. It wasn't just descriptive; it was more prescriptive than anything else. Again, not something maybe that he had done so much of, or I had done so much of, but that is what we were doing here. Now it's in the news. It did have an explanatory aspect. One person who picked up on some of our work was a labor economist by the name Lazear[19] who

[19] Edward Lazear is often considered to have pioneered the study of personnel economics. He was the founding editor of the *Journal of Labour Economics*. Lazear served at Chicago as Professor of Urban and Labor Economics between 1985 and 1992, before moving to the Graduate School of Business at Stanford University. In 2006 he was appointed to The Council of Economic Advisors. One of his most widely cited papers written with fellow Chicago habitué Sherwin Rosen appeared in 1981 as "Rank Order Tournaments as Optimum Labor Contracts", *Journal of Political Economy* 89 (5): 841–64. Gary Becker refers to his paper on mandatory retirement which appeared several years after the Stigler/Becker collaboration (Lazear, Edward (1979) "Why Is There Mandatory Retirement?" *Journal of Political Economy* 87(6): 1261–1284).

spoke about mandatory retirement. That's really based on our work. Now we didn't go into it because we didn't see the implication of it, but if you are paying people more than they're worth, you don't want to let them just keep working. That's the idea of it. You want to force them to retire. So you get the idea of mandatory retirement, based on our argument, which he recognizes in his paper. So, it had positive aspects to it but we didn't go into those, very little.

What is particularly interesting is that it seems as he progressed and specially toward the end of his career, I'm thinking for instance of one of the last things he published, 'Law or Economics'[20]*…*

Oh yeah

At least at the beginning of that paper, he's talking about, well if there's this sugar subsidy and even though we all know it's created so much waste etc., what's important is the test of time and if it is still there it must somehow optimally redistribute income. This is what the public wants, so economists really don't have a policy role to play. Now that position, which is sort of an extreme passive position, seems to be at odds with the 1974 paper which is clearly saying "this is what should be done."

Yeah. It is true and there is some tension between the two in social areas. One of them is saying what you see politically is not by accident, it is the result of a configuration of political forces, and so just exhorting the public to pay government officials more won't get it done. I think that's true. I think there is a difference of opinion. The only way one can reconcile the two, and in a way I think George recognized this, perhaps even explicitly, certainly implicitly, was that in the long run some of the ideas about how to improve things may make a difference if it is picked up in the political arena by groups that want to use it for their own purpose. So, I think an idea may eventually have an impact. I think there is some place in his work where he talks about the long-run impact of ideas on public policy. I think he believed that and I certainly believe that, but it's true there's one that's saying let's do this and the other is saying "well, what economists recommend is not much use because it's politics that is determining what gets done." I think he became more and more convinced of that view. I would say this was true from the time he started

[20] This article appeared only after his death in 1992 as "Law or Economics?" *Journal of Law and Economics* 35 (October): 455–68.

systematically studying politics around the late 60s or early 70s, I'm not sure when, up until the end when he was moving more and more in that direction. So he would have a lot of arguments with Milton Friedman. Milton Friedman was the advocate of "do this, do that." He would say, "well, what's the point?" I mean it's there for a reason. Not because the public is ignorant. You're just telling them what is right won't matter. I think he had some influence on Milton. Milton still advocated, but he tried to do it in a more sophisticated way, recognizing that you've got to maybe change the structure of the politics in order to get it done. I would say George had an influence on Milton's views … but there were plenty of differences that I heard between them on this issue.

Seems clear. Now, on some other issues, he clearly did pioneering work on the economics of information. His very famous '60s papers.[21]

Right.

He takes a standard market approach. It's a standard demand/supply structure. Now, did he think that he had captured all you need to say about information in that particular framework, because it's sort of, to me, a yellow pages framework? You know, you're looking for something and then you figure out how to minimize the cost of finding it. Did he think that was really sufficient?

Well, not everything. No, I wouldn't think he would say he had captured everything about information. I think he thought that he captured a lot about how people search for greater information. I did think that he thought he captured a lot of that aspect. There are other aspects of the information problem that he doesn't get at. The bargaining interaction between people when they each have information about themselves the other doesn't have. I would think, I'm pretty sure he would agree with that. He had recognized his framework doesn't get at some of those issues. But I think he felt that it was a good framework for getting at the issue of how people search for additional information, when they have opportunities to search for it, whatever they may be. They may be complicated searches, they may just simply be that you call up a store and find out

[21] George Stigler was awarded a Nobel Prize in 1982 partially in recognition of his essential work on the economics of information. Two key papers are the one that he produced in 1961, "The Economics of Information", *Journal of Political Economy* 69(2): 213–25, followed by another in 1962, "Information in the Labor Market", *Journal of Political Economy* 70(5, pt.2): 94–105. The former paper is by far the more influential of the two.

what their price is. There may be more complicated things involved. But this is a search process. There are certain optimal problems to it, and therefore you can extend the usual optimization analysis of economists to include also aspects of the acquisition of information and knowledge. And I think, yes, that he felt that he had captured a good chunk of that aspect of the problem.

But just that aspect.

Just that aspect. I wouldn't feel that he thought he had solved the information problem.

Now would he think that you could continue using the same apparatus to encompass the rest?

Well, you know, he would think you could go far with it, but you know, George was … you know he was open to new ideas. You know, the proof of the pudding he would say was in the eating. If somebody came out with a good way of looking at it that was different, then OK. He didn't see it yet. He thought this could be extended in a lot of different directions. But I think he would leave that open for what other researchers might contribute. As long as it was in some kind of an optimizing, maximizing framework he would look on it with potential favor, if it looked interesting. I don't think he felt it had to be this one framework. But it had to be something where people, rational economic agents were involved. That's what I think he felt strongly about.

OK. So, rational optimizing …

I think it was those crucial aspects of human behavior that had to be in the model. That's why he wasn't so sympathetic to this Tversky/Kahneman stuff.

Now, why, for instance, was he not at all favorable to things like game theory?

Well, they had rational maximizing so that wasn't the reason. It was because, I think, he felt it didn't have much empirical content. That would be his view. I think he was sympathetic to Sam Peltzman's paper. That was published in the *Journal of Political Economy*[22] on the evaluation of game

[22] Becker refers here to a very critical review article on the Handbook of Industrial Organisation which appeared in February 1991 as "The Handbook of Industrial Organization", *The Journal of Political Economy* 99(1): 201–217. George Stigler could have easily read a submitted copy in 1990.

theory, a very critical paper. I think he was sympathetic to that article. I think he felt that game theory didn't have much content, that it hadn't contributed much to Industrial Organization. That would be his ground. Not the assumption of rational maximizing … that he liked. He didn't think that the element of strategy and interaction was that important in the industrial organization area. But he had no objection in principle to game theory. He had an objection to what it led to in terms of whether it provided many insights into the world. At that point he became an empiricist. You had to have a theory that threw light on behavior. That would be the gain. Just spinning theories for their own sake wasn't much use. You didn't have to test it yourself, but he had to be convinced it had insights. If he wasn't convinced, he wasn't interested in it.

It had to have some very applied …

Had to have empirical implications so that you could see what it was implying. Is this throwing insight on a problem or not? So game theory I don't think had reached that stage. I don't know if he would have said that it would never reach that stage, I never heard him say anything like that, but I just don't know. I know he thought it hadn't reached that stage.

And could you say the same thing about the sort of theory of the firm that Williamson[23] *creates with his transaction costs analysis as well?*

He was skeptical about a lot of the transaction cost stuff. He thought a lot of it was tautology that it hadn't gone very far. I don't know about Williamson in particular, but in general I think that would be his view. There were just too many tautologies in that area. Williamson would have to try to do some empirical work to see if it could become a useful organizing device. Much of the work so far, he felt hadn't yet been tested. But in this case he wouldn't say it could never be. Thought it might amount to something, but it hadn't yet.

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[23] The reference here is to Oliver Williamson (1932–) who advanced a particular use of the idea of transaction costs in his work in Industrial Organization. He was awarded a Nobel Prize for these advancements in 2009. Williamson championed the importance of a type of behavior he labelled opportunism, or self-interest with guile. Asymmetric information allowed the individual with information to use that advantage to his or her self-interest. Stigler wasn't impressed. In a letter (March 17, 1981) to Douglas North commenting on his work, *Theory of Institutional Change* George Stigler noted: "There is evidence of a similar if lesser flirtation with opportunism, an even more faddish notion."

Because in his own work itself he's never that concerned with the individual firm or even the individual industry, it's always a generalization or across industry …

It is not a case study, not a case study. He wanted to develop more general principles. He may have applied the data to particular industries and he did. He did study the steel industry and other industries. But it had to be in regard to a principle, to a more general principle. He wasn't interested in simply understanding the steel industry. I think that's right. If he could understand what the principle was … and that it might be applicable to many other industries, then he'd be happy. But not going and doing a case study. He always made fun of case studies. The Harvard method, see how the industry works, you go in and do a case study. That was Edward Mason.[24] He made fun of Mason. Yeah. I agree with that. He always wanted a more general principle.

OK. Now, I remember reading something that he said about Adam Smith that he wanted to, in a sense, dismiss the ***Moral Sentiments*** *as not applicable to economics. Now, why was that? Because there is an alternative view that the two are very much interrelated.*

Well, I know he thought, I mean I don't think he thought that much of the *Moral Sentiments*. He thought *Wealth of Nations* was the greatest book ever written in economics. There was no question about Smith as the greatest economist. I can't recall exactly what he said, I mean about Smith, except he would sometime speak about the literature on the so-called Smithian problem. How could somebody who wrote the *Wealth of Nations* write a *Theory of Moral Sentiments?* He felt that markets were best understood, George as well as Smith, in terms of self-interested

[24] Edward Mason (1899–1992) spent his career at Harvard, being awarded a PhD in 1925 under the guidance of Frank Taussig and teaching there for 46 years until his retirement in 1969. Along with Joe Bain in the 1930s and 1940s he developed the Structure, Conduct, Performance approach to Industrial Organization which rested heavily on the case-study method. This form of analysis became the standard for research in this field only to be supplanted by the Stigler-inspired approach that transformed Industrial Organization into a form of applied price theory. Mason's approach was the very antithesis of what for George Stigler was the essence of economic theory. Stigler's response was to largely dismiss such efforts. In his 1988 autobiography (*Memoirs of an Unregulated Economist*, New York: Basic Books), Stigler essentially skewers Mason's method.

> Edward Mason's group at Harvard was the leading producer of Ph.D.'s in industrial organization. Each new Ph.D. gravely decided in some mysterious fashion whether the industry chosen for his doctoral dissertation was or was not acting in a socially desirable way (162).

behavior. And *Moral Sentiments* doesn't deal much with that issue. So he thought in terms of understanding markets, economists deal with markets. What was in the *Moral Sentiments* wasn't much relevant. You know it would be George's view basically. I don't know if he'd say it wasn't a good book. He didn't think it was as good a book as Ronald Coase thought it was, or I thought it was. That I know. I wouldn't say he thought it was a bad book, but in terms of course of understanding markets, what you got out of the *Wealth of Nations* was a hundred times more useful than what you got out of the *Moral Sentiments*.

OK. Now the interdependency of people as agents, also didn't seem to interest him that much?

Well, not in terms of direct linkages. I mean of course there were firms that combined people. And markets that combined people but the links between people, I don't think he ever did any particular work on it. I mean he worked on the division of labor and where people link because they specialize. They have to be combined. That would be the closest that he went to any of that. He wasn't interested particularly in those problems. I'm not sure which ones you have in mind but in generalizing, you're right. Those were not the sort of things that interested him. No.

What about his reaction to what is generally called the Austrian approach?

He wasn't sympathetic to that.

I suspected, you know …

Ah, no … why?

Yes.

That's a tougher question and I'm trying to think. I know he wasn't sympathetic to them, because I've heard him make remarks about some of them. He thought some of Hayek's stuff was good, particularly his work on knowledge and ideas. Other than that, I don't know if he thought so much of most of Hayek's philosophical stuff to tell you the truth. But he liked, respected Hayek. I can't remember any explicit conversations, but I think George felt these modern Austrians, what were they adding to what we knew about economics? I think he felt they weren't adding much to what we knew. Hayek's paper on knowledge was good. Schumpeter's theory of economic development he probably thought was good. I know he thought highly of Schumpeter. And he thought his business cycles were a very interesting book. *Capital Socialism and Democracy*, he thought it was interesting. The modern Austrians, the so-called new Austrians, I don't think he thought they were adding much. I don't know exactly why, but I think that was his view.

You think it had anything to do with the empirical basis of their work?

Well, he was of course hostile to their hostility to empirical work. He thought that was totally wrong. You couldn't have an empirical science without having some empirical base. Not that everyone had to do empirical work, but it had to have a strong empirical base. He thought ignoring that was nonsense. He just thought that was totally wrong. On a theoretical level, on entrepreneurship, he didn't say that was wrong he just felt they hadn't gone very far. I think that would be more his perception. Yeah, hostility to concepts of equilibrium, he would think would be wrong, misplaced. I don't think he had a lot of sympathy for the modern Austrians. He liked Hayek, he liked Schumpeter and the early guys like Menger and Böhm-Bawerk. He knew their work well and he had a lot of respect for them. When I say he was hostile, I meant the modern Austrians, the so-called new Austrians. I don't think he had much sympathy to what they were doing.

To people like Kirzner and Rothbard?

Rothbard surely not. Kirzner, I think he thought he had … said some interesting things on entrepreneurship but didn't carry them very far. I would say he had a higher opinion of him than of Rothbard. Right.

The role of institutions and path dependency?

I don't think he did anything on path dependency. Again, he would think that institutions were important but he would say that people who talk about it a lot, like North and others have not said anything other than a bunch of empty statements. You know institutions were important, he was no fool on that, he knew they were important. The question was whether people had been saying anything interesting about their evolution. Not yet. Maybe someday we would. So he didn't say institutions weren't important and didn't affect agents, but the so-called New Institutionalism, he didn't think it really got beyond the point of being pretty empty.

OK, so he didn't think that there wasn't potential work to be done …

No. It hadn't been done. No, he would never say given his view, that institutions weren't important but that people hadn't done it yet.

No.

And he didn't think much of any of the work yet on institutions …

And the idea of path dependency?

I never heard him really talk about that. I don't know, I don't know his views on that. Would he have been sympathetic? If I had to speculate, it's not the sort of thing he would have been too sympathetic to, although he would have recognized that maybe you get Silicon Valleys because of some luck of where you get started from. I think George would have recognized that. I wouldn't have thought he would think that is a major problem in economics, but he would have recognized it might have a role sometimes. But, I'm just conjecturing. I've never heard him comment on it.

OK. Did you two disagree?

Oh, yeah. We disagreed. We agreed a lot more than we disagreed let me say, but I mentioned one example. On the altruism issue, he was not sympathetic to that. I think he came round a little bit, but not so much. The same way with some of the stuff I did on marriage.[25] When he was editor of the *JPE,* I sent it in. He got a very negative review from a famous economist, Bob Solow. He said "take account of Solow's comments and we'll publish it." So, he may not have been so sympathetic to me there. It was interesting. He had that ability as an editor to rise above his own opinions. I think eventually he became more positive on it. But we didn't certainly start off together, for a while there was disagreement. And on other issues we had our differences. Malthus, I thought the Malthusian model was a great contribution. George thought it was no good because it was wrong. And so, we had a strong disagreement about Malthus, whether Malthus was a great man or not. I thought Malthus was a great man, George didn't think so. No question about that. This was an explicit difference and we never resolved that. I think he was great and he didn't think so, so that was a clear area of disagreement. And there were many others. I mean disagreements arose all the time. We would be taking different positions, although our agreements were much stronger than our disagreements, but these are just a few that I can recollect offhand. We disagreed often on individual people, assessing the quality of peo-

[25] Gary Becker refers to an article that appeared in two parts over the year 1973–1974, "A Theory of Marriage: Part 1", *Journal of Political Economy* 81(4): 813–46 and "A Theory of Marriage: Part 2", 82(2): S11–S26.

ple, students and young faculty members. We once had a bet on one of my former students, whether he'd ever amount to anything. Well, I was not clear who won. He started to amount to something, he was doing very well. I wrote with him, Nigel Tomes.[26] Then he quit economics and became a minister or something, a Christian fundamentalist sect. So George won in a sense but, you know, he said Tomes would never finish. He did finish, he did very well. So we disagreed a lot on students and on our young faculty. There were a number of such votes in the department. I'm not even going to say now that I voted one way and he voted the other way. We were very much on side. But George had good judgement. I'd say more of the time he turned out to be right than I did. He had good judgement, but there were certainly disagreements. And, yeah we had a lot of disagreements. Our fundamental agreements were on points of rationality and economics. Economics was important in understanding the world and you could use economic-type reasoning in political areas, as he did, or in sociology. It provided a broad way of interpreting a lot of the behavior. I think George came around to that view. The younger Stigler was not of that persuasion. I think as he got older he grew more in that direction. So, I would say for the most part we were in agreement, but in the day-to-day things, and a lot of the evaluating of particular articles it was quite frequently that we were in disagreement. But, you know, it is hard to be friendly, intellectually friendly with somebody if you disagree on fundamentals. We may be friends as individuals, but not intellectually. We were basically agreed upon fundamentals, as he did with Milton Friedman, even though they began to differ more and more as they got older. And that's how we felt with George. He and I were possibly closer to each other than he and Milton eventually were in terms of views. But of course there were a lot of issues and there was disagreement assessing articles and ideas and so on.

[26] Nigel Tomes wrote a 1978 dissertation, "A model of child endowments, and the quality and quantity of children." He co-authored such papers with Gary Becker as "Child Endowments and the Quantity and Quality of Children", *The Journal of Political Economy* 84 (4, Part 2: Essays in Labor Economics in Honor of H. Gregg Lewis (Aug. 1976)): S143–S162 as well as "Human Capital and the Rise and Fall of Families", *Journal of Labor Economics* 4 (3, Part 2: The Family and the Distribution of Economic Rewards (Jul., 1986)): S1–S39.

I know that he was very impressed and very excited about the Coase 1960 article.[27]

No question.

Yet, he never seemed particularly interested in the 1937 article of transaction costs.

Well, I don't know if that's true. I'm trying to think. I think he thought it was a major article. I can't remember anything explicitly he said about it. But didn't he include it in …

He included it in the collection with Boulding.[28]

Yeah. That's when I first came across it in fact. So I thought that was an important article. But I can't remember anything.

He doesn't ever seemed to have mentioned it or …

But Coase came out with the later stuff, so the earlier stuff looks like a special case in some sense of the same type of reasoning. That's how I always interpret it. No, I think he thought that was an important article. He may have felt it didn't get very far with that understanding of what firms are and what they did. That is, it's a framework but it doesn't get you very far. That, I think, was what he probably thought. I think he thought it was an important insight. He was very high on Coase. He thought he was really a first-class economist, not only the Coase theorem, but in general Coase was very good. He was very high on Coase. I think he thought that the early "Theory of the Firm" article was an important article. But I can't remember any discussions of that.

I've just never seen it.

I don't think he paid it much attention, but I think he did think it was important.

That's just it. The attention he paid to the 1960 article was tremendous. There's not so much …

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[27] The article is of course the much-cited work, "The problem of social cost", *Journal of Law and Economics* 3 (October): 1–44. It is the article from which George Stigler discovered (or invented) The Coase Theorem.

[28] The article, "The Nature of the Firm", by Ronald Coase appeared in Boulding, Kenneth E. and George J. Stigler (eds) (1953) *Readings in Price Theory*. London: George Allen and Unwin Ltd., pp. 331–351. Including the work by Coase was actually Stigler's direct decision according to Ronald Coase (conversation, Oct. 1997).

Well, the 1960 article had a more revolutionary impact on how people thought about externalities and law so that was a bigger impact. He thought the earlier article was a very good article, I'm sure of that.

OK, then that's good.

Now we're coming on …

We're almost there. I'm not going to take much longer. We talked about disagreements, did George Stigler ever make major changes in his point of view?

In the course of his career, he made major changes in his point of view. I'll give you a few examples. He once wrote an article about monopoly, that you should break up companies. He eventually abandoned that theory.[29] And the reason he abandoned the theory was very interesting. In those days he thought, you could tell the government what to do and the government would do it; but his later view was, no. He adopted the view that there are politics and politics is worse than monopoly, or can be worse than monopoly. So he changed, drastically changed his point of view on that. And he began to take this view that, well, you are better off having a rotten something that doesn't work perfectly in the market than having the government get involved. That would make it a lot worse. I mentioned his work on politics. That would be one major area of where he changed his point of view.

I mentioned earlier his view that you should go beyond markets to politics and sociology. The young Stigler would not have had that point of view. He was narrowly focussed. He got a much broader view of things as he went on and the power of economic theory changes for him. So, these are two big areas where he changed his point of view. He became less of a preacher and more of a positive economist, I would say.

And there were other areas. I'm thinking big changes. I'm sure there were a lot of little changes as well, but I can't think of any offhand. But these are the ones that stand out in my mind as some of the big changes in his view of the world. He became more empirical as he went on. He got attracted by this methodological view. He had to test theories. In the early Stigler you don't see much of that. You have the theory of price; you have his

[29] The most likely piece referred to is a magazine article which has since its initial publication been reproduced a number of times. It shows that at this time, George Stigler was still clearly under the influence of Henry Simons. Stigler, George J. (1952). "The Case against Big Business", *Fortune* 45 (May): 123 ff.

thesis which is a discussion of marginal productivity theory. Then he goes to the National Bureau and starts writing some purely empirical papers, very little theory in any of them.[30] Some of them were pretty good papers, maybe even *very* good papers, I think. But still they were very different. I think he shifted to the importance of doing testing. And later on he began to act as if you had to do the testing yourself, every theory that you had. A little bit of datum begins to appear in most of his papers, though not in all. So, on that I would say there was a significant change in his point of view. I think the broad outlines of what he thought economics was, markets and so on, I don't think that changed. That remained the same. He changed his view about Knight, I think I mentioned that. He changed in his view about Knight, the assessment of Knight's work. I think that his opinion of Knight went way down. He still thought Knight's work was important, but nowhere nearly as important as he did when he was an undergraduate and a young person. And contrariwise, his assessment of Viner went up and that's the same for Henry Simons. If you look at parts of the *Program for Laissez-Faire*, it doesn't look that interesting at this stage, so he went down on that. He changed a lot of his evaluations of that type. But the big changes, I think, were his assessment of government, his application of economics to politics, things like that. And of course he became more empirical. I think those were the big changes.

Can I ask you a final question: following up on what you just said, would it be at all accurate to say that his views in this sense became more consistent? Because for example, the early monopoly view was a sort of conventional view he got from Henry Simons etc. A lot of his other views didn't really square with that core vision he had of markets, of rational optimizing agents, and he was moving more and more to a complete view that was consistent within itself.

I think he went to a more satisfactory position, absolutely. The earlier view, as you say, he picked up, that was the literature, he hadn't really thought it through. I mean, you know, he hadn't thought through everything at that point, and he hadn't really thought it out. As he thought through more and more, I think he came to a more satisfactory thesis on the issue. I think you're absolutely right, he did. And George was

[30] A good example of this would be his 1946 book, *Domestic Servants in the United States, 1900–1940*. New York: NBER.

still a young man intellectually when he died at 81. I think he would have changed his views still further, unlike a lot of people who reach that age. They stop thinking. George always said to me, the reason he didn't retire, and stayed around the University of Chicago was he didn't want to become ossified. He wanted to be exposed to new ideas and to changes. And he did. He kept changing them. And he would have changed them forever if he had stayed on with us. He was in pretty good physical shape actually, too. But mentally he was still in very good shape. He was still a very interesting guy to talk to when he retired. He attributed it in part to staying in a tough intellectual environment and he always made that point to me on a number of occasions. But his views did become more consistent. I agree with you on that. Other people may not think so, but I think definitely that was true. He began to re-think some positions he had just inherited. Inherited you know, from his teachers and so on, or from the literature and he put it in a more consistent framework.

Thank you.

Marching to a Different Drummer: Sam Peltzman Reflects on George Stigler

It is one of those hot, sultry October days in Chicago when I make my way to the University's Graduate School of Business to talk to Sam Peltzman about the life and times of George Stigler. To be more precise, my aim is to discuss how Stigler approached economics and dealt with economic research. Sam Peltzman, of course, had been one of Stigler's PhD students and was certainly acknowledged (by Stigler) to be one of the best of that select group. To some degree, he was considered by many other academics to be Stigler's one true protégé. Their mutual respect becomes obvious not simply by examining past comments, but by tracing out their subsequent dealings with each other. Once established at UCLA, Peltzman's old teacher made strenuous efforts to lure him back, going as far as to guarantee research funds from his own Center.[1] Stigler clearly appreciated him not only for his obvious ability, but also for his willingness to go head to head with anyone when a question concerning economics arose. George Stigler tended to be most comfortable with people who could give as good as they got, in other words, people not

[1] The Center for the Study of the Economy and the State was renamed the George J. Stigler Center for the Study of the Economy and the State soon after his death in 1991.

C. Freedman, *In Search of the Two-Handed Economist*,
DOI 10.1057/978-1-137-58974-3_7

dissimilar to him, at least in terms of this character trait.[2] And it is quite true that Sam Peltzman doesn't suffer from any signs of acute shyness when expressing his clearly held views. While doing archival research, I found a letter to George Stigler from a consultancy discussing the suitability of various members of the Chicago Department of Economics or the Business School as consultants for the private sector. The judgment on Sam Peltzman was distinctly negative. The letter noted that Peltzman was only in the same office with a client for five minutes before managing to insult him. This response apparently didn't signal either a propensity for tact or a future usefulness as a consultant.[3] One suspects this didn't faze Sam Peltzman in the least.

In contrast, I find his no-nonsense approach refreshing. It is soon clear that I will receive his unvarnished thoughts.[4] Naturally, he will consider carefully any question or thought I throw at him, but will not be shy about rejecting them or even sharply correcting me when he deems it appropriate. But, as long as the conversation remains interesting to him, he proves happy to continue. We in fact end up having an extensive talk, lasting something close to an hour and 45 minutes. Though at one point, he does call me sharply to order when I return to a discussion that he had considered sufficiently settled. The clear, if implicit warning is: don't bother wasting his time.

Talking to Peltzman proves useful. This is not only because of his history as George Stigler's prize pupil, but because of a thoughtful article he had written for the *Journal of Political Economy* on Stigler's work in the field of governmental regulation.[5] This, of course, was one of the distinct

[2] This same idea is much later made explicit by another former student, Mark Blaug. Moreover, a conversation a few weeks later with Robert Solow also conveys the strong impression that this ability not to be intimidated, but to verbally spar in a playful fashion, is one of the bonds that cemented a long friendship between Stigler and Solow.

[3] I exaggerate, but not by that much. The exact quote in a letter discussing the usefulness of members of Stigler's Center acting as contractors for Lexecon is: "… we once tried to hire Peltzman on a project, but he insulted the client and it fell through" (Letter to George J. Stigler from Richard A. Posner, February 13, 1979). Compass Lexecon, as it is known today, is one of the world's largest economic and financial consulting firms, founded in Chicago in 1977.

[4] As Lucky Ned Pepper says to Mattie Ross in *True Grit* (2010), "You do not varnish your opinions."

[5] The paper referred to can be found in Peltzman, Sam (1993), "George Stigler's Contribution to the Analysis of Regulation", *The Journal of Political Economy* 101(August): 1253–1289.

fields in which Stigler made his mark, as did his student, Sam Peltzman, years later.[6] Similar recognition would extend to the more general field of Industrial Organization. Although Stigler himself was wont to dismiss the field as simply applied price theory, he was perhaps more influential than anyone else in establishing this area of study in its modern incarnation. One suspects however, that were he alive today he would be unhappy with some of the directions in which the field has wandered, particularly as regards game theory.

Later, after receiving a transcript of this talk, Sam Peltzman stays very much in character, giving his permission to publish the conversation verbatim without any amendment or deletion of his comments. In keeping with his mentor, Peltzman's outspoken approach is simply to let the chips fall where they may.

The Interview

I'm very concerned with the particular role that George Stigler played in opposing alternative theories to standard price theory. I've been looking at the debates of the 40s, 50s and 60s. I'm most interested in the way he operated, the way he debated, the context in which he pursued his objectives. So I've been trying to talk to people who knew him and worked with him on a variety of these issues in order to get a better idea, to understand more exactly what he was trying to do and what motivated him.

Well, go ahead then.

OK. Can I just start off by asking you a little bit of the background, how you got to know him and work with him, that type of information?

I was a graduate student here in the early 60s. I kind of knew that my interest would be in Industrial Organization and he was the leading figure in Industrial Organization. So that's how I got to know him. In addition to that (I don't know how much your interviewing has told you already) he didn't have many students.

[6] Making his mark refers to the impact Stigler had on the profession in influencing subsequent work in this area. His Nobel Prize singles out his work in regulation as one of his distinct contributions to the field of economics.

Yes.

So I was, like, the only one.

You were actually …

In that cohort.

So, you were actually one of his students?

Yes. I eventually ended up writing a dissertation under him.[7] I got to know him in that context. I guess partly because there weren't that many students who worked with him, and partly because I acquired some professional success. There was kind of this view that I was his boy in some sense. Viewed by the rest of the world to some degree and viewed by him to some degree. I came back here as a post-doctoral Fellow in '68 and I'm sure that was his feeling. I came back as a visiting faculty member in '73 and I'm sure that was his view. He had an enormous influence within the business school in that era. And then when I was a regular faculty member from '74 on, till his death, we were colleagues in Industrial Organization in the business school. So that's the connection.

From the viewpoint of a student, I know that people have said that he was very tough and very critical and that that kept students away. How did you find him?

That's part of it, though personally I didn't see that side of him. You know students can build things up in their heads. What he was, he was aloof. He was non-directive in an era where you had very powerful people like Milton Friedman and Al Harberger who were the opposite. In the case of Friedman, this was the era of the money demand function. If you were a graduate student in money, you did a demand for money study, you did it in a way that Friedman pioneered and you came up with the results.

I know what you mean.

There was kind of a pattern to it. Some very good students were turned out. Greg Lewis in labor economics, very closely supervised, in painstaking detail, the research of his students. As I said, Al Harberger did the same thing at that time. It was investment and durables. You did

[7] The dissertation became: Peltzman, Sam (1965) *Entry into Commercial Banking*. March: University of Chicago Library. A shorter version appeared as: Peltzman, Sam (1965) "Entry into Commercial Banking", *Journal of Law & Economics* 8(1):11–50.

cars and you did refrigerators and you did houses and all that kind of stuff. George was not like that at all. I mean, you would go to George and say, "What should I be working on?" He would say ten words and six months later you would come back and you would discuss with him what you had done. His input into my research was in laying out what he thought was an interesting area to look at, namely the effects of regulation. At that time he had just finished his paper with Clare Friedland on the effects of electrical regulation and the conclusion of that article was that regulation had no measurable effect on electricity prices.[8] And he thought that was a powerful generalization. So he said to me, "Why don't you go look at the banking industry or the wood industry?" One of his suggestions, I remember, was you could look at the way government manages the woods that it owns… "I claim it is no different than the way Weyerhouser manages timber. Or, once you look for a difference between the way government-owned firms and privately owned firms operate, maybe you'll find the same result I found for the electric utility regulation." That was it. The rest of it was entirely up to you. I think that gave him a certain remoteness. This put off graduate students who had a ready alternative available to them of close supervision, a definite relationship with a supervisor. That was part of it. Then you know, George, I'm sure people have told you, George first of all didn't suffer fools gladly. He was always curt. He never screamed at you. He never was that way, but he was very curt. Secondly, and this is related, he very much maintained an image of this strong, Protestant father. That was his heavy vibe. His colleagues would tell you this. You know, he could get his point across with two words, and the way he was turning his head. Like so. And it's kind of human, for a young person to find, that that's a heavy burden to bear sometimes. The fact is, very few tried to do it. I was perfectly happy to do it because I'm that type. In my career, I've always been someone who likes to do my own thing. So George was an appropriate choice. I was more or less committed to doing something in government regulation, in the industrial organization area. But, I think the total input I got from

[8] The work referred to here, and later in the interview is: Stigler, George J. and Claire Friedland (1962). "What Can Regulators Regulate? The Case of Electricity", *The Journal of Law and Economics* 5(October): 1–16.

Stigler himself was in defining the area. "Why not look at some effect of government?" That was it mostly, and the rest of the time, I was pretty much on my own. As a program put forth for a very, very sure-of-where-you-want-to-go type of student, that was fine.

What about his input when you showed him drafts of your work?

I can remember that I actually ended up doing a dissertation, which was, "Did the regulation of entry into banking have effects?" I had already spent about a year chasing blind alleys as graduate students will do, and which is generally the case. I finally settled on this banking issue. I became convinced that it did have effects. And he didn't like that result. At that time in his life (I mean he later changed), but at that time in his life, the results should have been that regulation had no effect. That was the key challenge. Now this was done in the early 60s and if you looked at the time series pattern of entry into banking, it went to hell of course in the Depression. It practically went to nothing. And it stayed down there. So, you had all these restrictions on entry being imposed during the Depression. His story was of course that it didn't matter because we had an over-supply of banks. It's economic forces that are important: "If you don't believe me, prove me wrong!" So I proved it to him. I didn't set out to prove him wrong. You know, it was one of those nice things where if I'd gotten one result that would have been OK and if I'd gotten the opposite result it would have been OK too. So I didn't have any stake in this, but the result that I got clearly showed that regulation had some importance.

So, I gave him the first draft and he looked at it. He said "Well, that's nice for your preliminary results" [laughter]. You know he had this way with a quip. That was it. That was it.

Really?

Yes. The message I got from him was that he couldn't find anything himself that was wrong with it so it was OK. I really got (the rest of my dissertation committee was Milton Friedman and Les Telser) more feedback in terms of the detail of what I was doing from those guys than I did from George. That was it. I don't remember any other feedback I got from him other than "Gee, I don't like this result, but I can't find anything wrong with it." So there was not a lot of interaction with him in that way.

He had this kind of, I want to stress, a *public* persona. I mean Claire Friedland who you are going to talk with tomorrow, knew him much better than I did during his career.[9] But his *public* persona was this very strong Protestant father figure. And you just don't cross that father figure. Behind it there was something else. You could see a side of him at times which was very shy. This was kind of a shell that he used to defend himself. You could see that aspect of him. But the public persona was unequivocal. And it was this public persona which was very much responsible in some sense for his image. He was a real leader.

Mmm.

Have you talked to Harold Demsetz or folks like that?

I talked to him, yes.

Harold would tell you that. You just dreaded the warning look … that dirty look from George. That is the way he led.

I talked to Armen Alchian.

Armen. Yeah.

He said that he could only remember once when they were playing golf that he ever got the last word in.

Yes. Yes [laughter].

And he treasured that moment.

Yes. Well, I didn't play golf. I didn't really socialize with George all that much. We weren't close in that sense. But there was this intellectual leadership and you could see that part of it was just the force of his personality. Part of it was that.

He seemed to be very persuasive. Very much leading by the strength of his intellect and character. That's the picture I'm getting. Well, if we can focus a little bit more on the work.

Okay, that's probably better.

The theory of regulation paper. The '71 paper.[10]

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[9] Claire Friedland was George Stigler's research assistant for more than thirty years and credited as his co-author in a number of seminal published papers. Claire Friedland has insisted that Stigler was giving her more credit than was her due. But Claire Friedland is unusual among economists in being decidedly modest about her accomplishments.

[10] The regulation paper refers to Stigler's much-cited article that helped to change the way economists saw the relation between government and the economy. Stigler, George J. (1971),"The Theory of Economic Regulation", *Bell Journal of Economics and Management Science* 2 (Spring): 3–21. It is quite possible to see a direct relationship between this paper and the earlier 1962 paper on regulation. If in fact regulation doesn't improve outcomes, the issue then becomes one of discovering why regulations exist in the first place. What in fact is the driving force behind such legislation? In this 1971 paper, Stigler offers an explanation using a simple supply and demand for regulation apparatus that is based on the assumption that government is not some unified representative individual with an undifferentiated set of aims, but rather a collection of agents who have their own agendas and objectives.

Right.

To begin with, there are certain things that are interesting to me in that paper. You have his argument, which is a very strong argument, and then there is this empirical work.

Yes.

Now, to me it always seems that the empirical work really added nothing to the paper.

Yes.

I mean, what is it doing there?

Yeah. Well this is not atypical. I mean if you look at the Stigler and Friedland piece, the empirical work is not that strong.[11] It may be OK for its time, but it's not outstanding. It's not outstanding. But George had, whatever he did personally, he had a very deep conviction that we do too much economics without empirical implications.[12] It's not economics without implications. It's very important that there be implications. What I take all that stuff as trying to do is to say "Look, here's my story. But look again, there's a bottom line that can be appended to this story." He took his empirical work probably more seriously than you or I might. I'm sure he did. But I think that what he's trying to do is by setting an example, saying "Look, what you should do is go out and do this sort of work. Go ahead, you do it better. But this is what you've got to do. This is the kind of thing you've got to do. You've got to systematically show me that regulation has an effect on electric prices. You've got to systematically show me what's behind entry control in" (whatever he was doing, trucking I think or occupational licensing). Others should do it better. That was the implicit message. I mean it was a powerful message. If you look at surveys of the literature, if you look at the *Handbook of Industrial Organization*s, the chapter on the effects of regulation, right?[13]

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[11] The reference is to the 1962 paper previously mentioned.

[12] The interested reader might want to refer to Stigler, George J. (1965) "The Economist and the State", *American Economic Review* 55(March): 1–18. This is the printed version of the presidential address that Stigler gave at the December 1964 American Economic Association meetings. It is at some points almost a visionary piece and certainly a call to arms for the profession to do more empirical work rather than depending entirely on theory and assertion.

[13] Peltzman refers here to Noll, Roger G. (1989). "Economic Perspectives on the Politics of Regulation", in Schmalensee, Richard and Robert Willig (eds), *The Handbook of Industrial Organization, Volume II*. Amsterdam: Elsevier Science B.V., pp. 1253–1289.

It's written by somebody who is a non-Chicago person. It's very clear there. Stigler and Friedland's paper in 1962 is the watershed paper. After that there are 100 studies. His example leads to them. And it's the same with the regulation paper. It set up a paradigm, which very powerfully influenced subsequent research. Everybody afterwards is saying "Well let's look at the political economy of that. How do you measure it? How do you measure it? Let's measure it right." So, his stature gave him, I think, the freedom to do a sloppy job on the empirical side, certainly not a finished kind of job. And it broadcast a message to the rest of us working in the field. I think that you have to take seriously the idea. But I think I would agree with a lot of what you said. And I've said so. I've said so in my *JPE* paper.[14] Very clearly, I could see it right from the beginning.

So, it wouldn't be enough for instance in the regulation paper, to say "OK, here's my argument. These are obviously his testable hypotheses. Now it's up to you, now you go out and do it." In effect, Stigler could make that statement, without actually having to do it himself. Because when I (especially in the occupational licensing section) see what he does, I'm thinking, well I could come up with a much better explanation than the one he offers. You could interpret the results in several different ways. I mean, you've got no conclusive results. You know it's sort of a let-down almost. You've got this powerful argument. Here's the climax. And you get this sort of squib. I just wonder if he had said, "Well, it's clearly a testable hypothesis." (As you know this was his basis, namely that you have to set up a theory so that you can examine the available evidence.) "Okay, I can stop there."

Right. Well, very few people cite the evidence when they cite the paper. It's a canonical article to this day. I mean, I did another piece which showed at least up until the late 80s, the citation counts for the paper weren't going down.[15] So it just keeps going on. It's a canonical cite for the

[14] The paper referred to can be found in Peltzman, Sam (1993), "George Stigler's Contribution to the Analysis of Regulation", *The Journal of Political Economy* 101(August): 1253–1289. The article is one of a number of tributes in this memorial edition of the *JPE*, where about half of that number is devoted to an appreciation of George Stigler and his work. Under the guidance of his friend and close colleague, Gary Becker, these articles more than adequately repay any close reading. The Peltzman article and a few others are of particular interest.

[15] The article referred to is: Peltzman, Sam (1989), "The Economic Theory of Regulation after a Decade of Deregulation", *Brookings Papers on Economic Activity, Microeconomics.*

political and economic theory of regulation. It's not cited for the evidence. No one will cite it in an article on occupational licensing, as the seminal article on occupational licensing. So you raise a point. Would it have been enough? Who knows? Who knows? I know personally, personally writing a paper with very powerful empirical implications was enough for him. When he was a journal editor, having that was enough for him. But I think he liked the idea of actually taking the next step and showing that it had some empirical content. Or at least taking the next step and showing you how you ought to do it. You could also go off and do occupational licensing: "You go do it better than me, but this is the way you should look at the phenomenon."

Yes.

So, I don't know. Whoever really knows? You can't play that history back. I suspect though, that it's the combination of the very simple storyline, plus when you read it you can see that there's a way of doing it. So you can give it to a graduate student, and the graduate student will say, "Oh, yes." You know if you think of graduate students, they don't know very much about how you do empirical research. They don't know that. So just telling them that there are empirical implications often is not enough. Giving them a kind of a demonstration of how you can do empirical tests of this type is an important additional step. I know when we give field exams in Industrial Organization, sometimes around here anyway, you say "And how would you test empirically your theory that you've just developed?" You get nonsense. You get absolute nonsense. It's important, if only for pedagogical reasons, to say, "Here are the implications and here is the evidence. Bad as it is, here is the evidence."

Actually, picking up on that, on the earlier piece, as you mentioned in your article, there was an error. And the results weren't actually as dramatic as they appeared to be.[16]

Right. In fact they were the other way. You could draw the opposite inference.

[16] This again refers to the article: Peltzman, Sam (1993), "George Stigler's Contribution to the Analysis of Regulation", *The Journal of Political Economy* 101(August): 1253–1289. The paper under discussion is: Stigler, George J. and Claire Friedland (1962). "What Can Regulators Regulate? The Case of Electricity", *The Journal of Law and Economics* 5(October): 1–16.

Exactly. And I'm wondering, had that piece come out without the mistake, with the actual result, would it have had the same sort of impact?

There the answer would be no. The impact of that was in the result.

So there was a bit of serendipity in the…

In some sense, a serendipitous mistake, yeah.

Because, that seemed to have brought it to people's attention.

Right. Again, you can't play the world back.

No.

There were two things, two things coming out of that. One was the methodology. (This was the early 60s, it's old hat by now.) There is a systematic way of testing a hypothesis that should be applied to this area. That would have had an impact. It is clear it would have had an impact. But then, there was of course the result of applying it, which was shocking. People then believed that it was shocking.

Yes.

"You mean the public utility commissions doesn't have any effect on this monopoly?" That turns out to be, I wouldn't say wrong, but it's certainly not as powerful (when you correct the mistake) a result as the one that he was portraying. And it's clear that the advertising value of that result was, in combination with the methodological innovations, what made this a very powerful paper.

I wonder if he had a tendency to push his results more than they were actually warranted.

You really should ask Claire that because she did them. She actually did the empirical work.

But I mean the way the way he wrote it up. He always seems to be quite confident and very positive. Sometimes, I mean to me it seems…

Did he overstate?

That's what I'm asking.

Yeah. Again, I'm on record. One of the things that motivated my own work in this area is that I'm reading this stuff and I'm saying, "There's something wrong. Why the hell do you have regulations that never have an effect?" You could tell a story about why you might have it in the beginning, but why would you have this whole area of regulation when there is no effect? Why would you have seen this? So this is the reason for my dissertation. Later on, when he's doing the regulation piece, one of the things that motivated me and others was (you can go back to Posner's article in the very same issue of *The Rand Journal*) his very strong result.[17] What is regulation? It is the government selling its services to the highest bidder. That's it. That's it. And who is the highest bidder? It's always the concentrated interests. "Have a look around and you see that there is something fishy if you try to apply it to this one particular case. And hey, the same thing happens in that particular case, and again in that particular case." This was an overstated result.

Yes.

Right. But it got people riled up and they jumped in. It's not the result that we have today. But we have a whole literature as a result. So he was very much this kind of a person who would say, "I'll state this result as strongly as I possibly can, even if it's not completely justified by the evidence." I mean I think at some level he said to himself, "I'll make the strongest case I can and then if that stirs people up …" In many ways he was using a bully pulpit that he had acquired from his stature in the profession.[18] He was able to do that.

Because people would take notice of his work?

Right. I think that that's realistic. Milton Friedman has a little bit of that with his money stuff. It's the way we do business.

The marketing?

Yeah. I think that there's that aspect. When I was a graduate student we were taught a paradigm of how you do research. I've got to tell you it's all wrong. It's not the way we operate. We don't sit up here and develop hypotheses and go out and test them. That's just not what we do. George taught me that. Milton taught me that. They're wrong! And I understand that. I'm older enough now to figure out that's not the way we do work. There's a lot of salesmanship, there's a lot of taking positions, defending them. Right. The facts will win out. I'm not saying that we're not in that sense correct. The facts do win out. But the process by

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[17] The article referred to is: Posner, Richard A. (1971). "Taxation by Regulation", *Bell Journal of Economics and Management Science* 2 (Spring):22–50. As the title hints, the paper attempts to explain the reason why some regulations lead to internal cross-subsidizations.

[18] Those familiar with the presidency of Theodore Roosevelt will recognize this particular reference to a "bully pulpit."

which that happens is not the clean one of scientific method rigorously applied all the time.

No.

I've come to understand that, and George had a big part in that process. He staked out a position. People attacked it. The empirical work goes back and forth and after a while it does sort itself out.

Yes. Because as you say, he came over to a position of a more complex understanding of…

Yes, he did. He did himself. I mean he did believe in the power of economics to have empirical implications, but you had to look at the empirical evidence. And it was very, very important to him to look at the evidence. Yet, you almost have to think that he is doing this, doing his own empirical work in what you say is not a very convincing way. He is doing it exactly to stimulate others. That was a big part of his contribution. To steer the profession. Look, you can argue about why we have regulations, what their effects are "Models are going to be a dime a dozen. You've got to give me a reason to take it seriously. Here, I'm giving you a reason. You don't like it, do better. But do better on these terms."

But you needed both, right? You needed a strong logical argument backed with empirical evidence.

Yes. But I think without the evidence people are not going to take it seriously. Stigler wouldn't take it seriously. That's what I got. I didn't get the scientific method. But I got a combination of: one, "You should have a view of the world"; but two, "You have to rigorously submit it to the evidence."

Yes.

That is what you should do.

You need both.

Yes.

Or equally, you can't simply infer from pure empirical evidence.

That's right.

Now this is the curious thing about Stigler as far as I am concerned, because his position on policy, his view of the role of the economist as far as policy recommendations goes is not seemingly in line with that. When I read his late work, and especially one of the last things he publishes, 'Law or Economics', *he comes to a very strong position.*[19] *If I can remember, it goes something like this, "Look at something like the sugar subsidy. If it remained all this time, it must in fact be somehow an optimal way to redistribute income or else it would have vanished by now. In fact, that means that there is very little in the way of a role for economists to play. They really don't have anything to say." What was your take on that position?*

Well, he very much believed that the role of economists in formulating or moving policy was overstated. More than I do. It's not something I agree with him. He would always take this very strong position. We were part of what Marx would call the superstructure. Bought by one side or another and we really didn't have an independent role to play in the evaluation of policy. Yet he had this belief that the world should be a certain way. It's clear. You know, he was a believer in markets. He didn't like the sugar subsidy for sure. I don't know how you really square it ultimately with his position that this is the optimal way to redistribute. I guess he didn't like redistribution. He feels this down in his gut. That's all he can tell you. He couldn't say, "I don't like the sugar subsidy because although I really like the redistribution that occurs, this is an inefficient way of doing that redistribution." He was beyond that. That is what we teach in Econ I. He is beyond, clearly beyond that. He wasn't going to take that position. So the only thing that's left is, on some other principle I don't like the redistribution, personally. Right? But there was a conflict there. There was a clear conflict there that a lot of us economically doing regulation kind of feel.

Because of course in many ways his whole profession is about persuasion.

[19] This article was published posthumously, representing the equivalent of a voice from the grave. In his later years, Stigler struggled with what he came to name "the paradox of legitimacy." If in fact there is a political marketplace allowing voters to express their preferences, then how can you label something like the sugar subsidy which has stood the test of time illegitimate? If choice was necessarily grounded in consumer sovereignty, then that must hold sway in any market whether of an economic or political persuasion. This is one of the issues that Stigler takes up in the article. Stigler, George J. (1992). "Law or Economics?", *Journal of Law and Economics* 35 (October):455–68. This position was earlier presented in a 1987 speech Stigler gave to the National Association for Business Economists upon being awarded its Adam Smith Award. The speech later appeared as: Stigler, George J. (1988) "The Effect of Government on Economic Efficiency", *Business Economics* 23(January): 7–13. This position quickly attracted a scathing reply from William Niskanen who seemed shocked by the heresy of such a previously faithful follower of Adam Smith. He calls Stigler dangerously mistaken. See Niskanen, William A. (1989) "A reply to George Stigler: evaluating government policy", *Business Economics* 24 (January):1–7.

Yes.

…persuading people to his point. But then, on a public basis he, I suppose to be totally consistent, he is forced to say, "No, we have to remain passive because anything else is a basic waste of time."

Right. But he wasn't passive. I mean you know that.

But that's exactly it!

He was quite prepared to advocate the end of sugar subsidies.

Yes.

But there are just things like that which can't be explained. "Consistency is the hobgoblin of small minds," somebody once said.[20] He was not consistent. That's clear. And this inconsistency has led to a lot of misunderstanding. There are people outside of Chicago who read him this way. With Becker it is even more powerful. It's all part of Becker's stuff about optimality and redistribution. Outsiders kind of read both of them as, "This is kind of the senescence of the Chicago School. They have become toadies for big government, apologists for big government." And I could see why. It is a really subtle kind of distinction we are making here between the two. But look, if you're going to regulate, conditional on wanting to redistribute income, I can't tell you that this is wrong. So, if I don't like it, if I tell you it's wrong, it has to be because I don't like the resulting redistribution.

Yes.

Or you can argue on global grounds. If you have a lot of this in society, it may have global effects on efficiency. You and I can fight over a cookie, but if everybody's doing it, it is spending a lot of society's resources, that kind of argument. But that's a more subtle basis on which to oppose these things then economists usually do by saying it's inefficient.

[20] Ralph Waldo Emerson is the "somebody" that Sam Peltzman mentions. The exact quote, one of those for which Emerson is best known is: "A foolish consistency is the hobgoblin of little minds, adored by little statesmen and philosophers and divines" (Emerson, Ralph Waldo (1841[1908]) "Self-Reliance", *Essays: First Series*. East Aurora, NY: The Roycrofters). It could be argued that George Stigler greatly admired consistency. However, the real question was whether he was given to indulging in a foolish consistency. For Stigler, this would imply refusing to change his mind no matter what the evidence might be. From the examples Sam Peltzman offers, it is doubtful that he would designate George Stigler as ever suffering from such a foolish consistency.

Yes. Then there are his views on anti-trust. I remember reading in his autobiography that he says, his mind changed. If you read what he is saying in the 50s and what he is saying later on, this seems clear. But I wonder, because, let me run this by you. It seems to me in the 40s, early 50s he's adopting a conventional view of the profession at the time.

Yes.

This switch later in his career, I think, makes him much more consistent in his basic view of price theory and how markets work.

Yes.

And this new view on anti-trust policy would be one which if presented to him, would be very attractive.

Yes.

So, it's more of a shuffling off a set of beliefs that really didn't fit with his central objectives. Does that seem accurate?

No. I don't think so, that's going against the evidence. He comes from a background in which a tough anti-trust policy is absolutely consistent with liberal economic policy and it's absolutely consistent with powerful market forces operating and so on and so forth. You have to remember the time. He was a student of Henry Simons who was the incarnation of that view.[21] Markets would be great if we didn't have advertising. Simons is on record saying stuff like that. Markets would be great if we had atomistic competition. Just like the textbooks tell us. Right. So that's his back ground and I'm sure he believed that. He believed that. There was no contradiction in his belief system. What's happening is that over time the literature, the empirical literature in Industrial Organization, which

[21] Henry Calvert Simons (1899–1946) was a staff member of the Economics Department at Chicago during the time that George Stigler was a graduate student there in the 30s. Simons became best known for his book, *A Positive Program for Laissez Faire* (1934), which proposed more competitive markets as a solution for overcoming the Great Depression. The staff of the economics department at the time regarded Simons as something of a protégé of Frank Knight. One consequence of that relationship was that Simons, in the 30s, would become the catalyst, a principle cause of bitterness, initiating an intense feud that subsequently flared between Frank Knight and Paul Douglas. George Stigler describes the unfolding of this feud in his autobiography,Stigler, George J. (1988). *Memoirs of an Unregulated Economist*. New York: Basic Books. Simons after the war inaugurated something of a tradition by becoming the first economics professor to serve as a faculty member of the University of Chicago Law School. Unfortunately, he held the position for only a very short period before committing suicide. However his successor, Aaron Director, would have a greater impact in this position, becoming instrumental in the creation of a law and economics tradition at Chicago.

is trying to test whether mergers affect price, whether they raise prices or lower prices, that literature takes a long time to develop. That literature continues through the structure-conduct-performance stuff. It runs into the 80s. You have important stuff in the mid-80s, I would say. In fact it hasn't died down yet. We are still teaching it to our classes. But that literature goes through a transformation. Now, along with this, I think he is maturing as a person. He's getting away from Simons. He begins to understand that there must be good reasons why Eastman Kodak dominates the film industry. Obviously there must be market forces involved. Why wasn't capital flowing into an industry with high basic returns? He is asking himself the kinds of questions that just didn't occur to Simons. A guy like Simons would just say, "Well, they're too big. Break them up! The text books tell you, the more firms, the better." And that's it. Simple. End of story. Advertising screws up people's minds, tax advertising and it'll be fine. So, he is beginning to understand that market forces are deeper than simple textbook stories. That's one of the lessons that I learned from him. By the early 60s when I was a student, I understood that the foolish kind of economist is the one who reads a textbook and takes it literally as applying to the world. What we are trying to do with a model is to simplify the world so that we can learn more about what is actually going on. He came to that understanding. Not right out of graduate school. He came to that understanding gradually. The evidence is beginning to be a little bit murky.

You have guys trying, actually showing that there is evidence that Eastman Kodak is dominating its market because it is better. You're seeing high profit margins as the result of low costs, rather than high prices. He is simply becoming aware of all this. It is only very, very gradually that he gets there. This didn't happen all at once. In the late 60s he was still sticking to this traditional idea because he believed that you really ought to have an anti-trust policy. Of course, his belief is heavily qualified by that time. There was something called the Neal Commission. I don't know if you are familiar with this. It was a commission appointed by Nixon when he first took over. Phil Neal, who was a law professor here, was the head of it. George was on it. They wrote a report about what should be our policy towards concentration. Basically, what it said was "Look, anti-trust has often done a lot of harm." (If I remember correctly, I may have it all wrong at this point, but my memory of it is something like this.)[22] "Anti-trust has gotten off on a lot of side issues like conglomerate mergers, vertical stops and vertical restraints. That's all wrong. What anti-trust should focus on is price fixing and concentration. That is, it should focus only on those traditional areas." Concentration, however, was still part of the package. It hadn't gone away. Even in the late 60s, he was still ten years away from where he finally came out on that issue, which was very sceptical. But always remember that you can read, I don't know where, whether it is in his autobiography or wherever it is, but to the very end the bigger, the deeper contradiction in George was that to the very end he loved the Sherman Act. He loved it. He said, "This is one regulation I like."

Did he?

Yes "quote, unquote." Look and you'll find it. It's written. It's published.[23]

Without any change even to his…

[22] Here Sam Peltzman is jumbling up events a bit which is easy enough to do when they occurred some three decades before. Phil C. Neal in 1967 was a faculty member [and is still an emeritus professor] of the University of Chicago Law School when Lyndon Johnson asked him to head the White House Task Force on Antitrust Policy. [Neal, Phil C. etal. (1968–1969). "Report of the White House Task force on Antitrust Policy", *Antitrust Law and Economics Review* 2 (Winter).] The Neal Report embraced the then dominant structure-conduct-performance approach to anti-trust issues. Not surprisingly, it delivered the type of report that the Johnson administration was likely to expect. The report emphasized the dangers of the then dominant conglomerate mergers and advised more interventionist policy. The Nixon administration came to power with different inclinations and in its first year (1969) asked Professor George Stigler to head a new commission examining the same anti-trust issues. [Stigler, George J. et.al. (1969) "Report of the Task Force on Productivity and Competition", *Antitrust Law and Economics Review*. 2 (Spring).] Stigler reflected the changing attitudes that were becoming clear within the economics profession. The structure-conduct-performance paradigm was under attack, spearheaded by the faculty at the University of Chicago that had pioneered the study of law and economics. Unsurprisingly, the Stigler report saw little, if any threat, coming from conglomerate mergers and thus left the Justice Department with little to do in this regard. The debate became somewhat generational with the younger generation of the profession tending to support the Stigler report.

[23] Exactly where this is published is not clear. In his autobiography and in his *Ely Lecture* [Stigler, George J. (1982), "The Economists and the Problem of Monopoly", *American Economic Review Papers and Proceedings,* 72 (May): 1–11], Stigler certainly makes it abundantly clear that economists came to love the Sherman Law because of the income it bestowed on the profession. But where he may have claimed to himself love the law remained something of a mystery. However, after engaging in some casual detective work, Peltzman's memory appears to hold true. The quote is in a speech George Stigler delivered at DePauw University [Stigler, George J. (1965),"The Formation of Economic Policy", *An Address by George J. Stigler*. April 21: 1–20].

No, no, no. He said "the Sherman." What he meant by the Sherman Act involved a war against price fixing, and combinations in restraint of trade.

So, he maintained that?

He maintained till the very end, that it was possible by combinations to restrain trade and that we should have a law against it.

OK. But not….

He has an article which says it hadn't had that effect.[24] He really was sceptical about the way the policy was being implemented. He was totally inconsistent with his own world-view of why you got the law in the first place, which was some vested-interest muscle. There was a political economy story that he refused to take seriously. Others have, by the way. Others have taken it seriously, Bob Tollison and people like that. They've written stuff on why anti-trust can be viewed within the context of Stigler's own theory of regulation. But he said, "If there is one kind of regulation I like the sound of it's the Sherman Act."

OK. But only on the ideal of price fixing.

Price fixing and combinations.

But not in the case of concentration.

No, not all of it, or not all the perversions of it, of course. What someone should have told him is, "Well, George, you had a lot to say about why anti-trust in fact works out the way it does. You're teaching people that they shouldn't look on regulation in terms of what the ideal is, but what the practise is."

Yes.

> This study is concerned with the effects of the anti-trust policies of the United States. To be quite candid, one reason I chose it was because I am very fond of our anti-trust policy in its broad outlines, although not in all of its details. I wondered whether being fond of a policy would help me to find good effects of it! (Stigler 1965:12).

Perhaps Peltzman remembered the speech since in it Stigler lauds Peltzman's work.

> One of our very finest dissertations, recently completed, was a study of entry into commercial banking given the restrictive practices of the Federal Deposit Insurance Corporation. Professor Sam Peltzman, its author, concludes that since the introduction of the Federal Deposit Insurance Corporation in 1934, with its very high standards for admission of new banks, the entry into commercial banking in the United States has dropped by perhaps 60 % (Stigler 1965:16).

[24] Here Peltzman seems to be referring to Stigler, George J. (1966) "The Economic Effects of the Antitrust Laws", *Journal of Law and Economics* 9(October): 225–258.

Right? Again, it is an area where he is not consistent.

Was he consistent? He was still around when there was this initial deregulation of trucking, of airlines, of those industries. Did he fall back on a change of private interest explanation?

As to why? Why you had all the deregulation?

Yes.

I never really discussed it. It is an interesting question. I never discussed it with him. I don't know what he believed. I know what I believed, you know, coming from a world-view which he influenced a little bit. You know, we never really talked about it a lot, about how it fit in with his story about why we had to have regulation in the first place.

Because with his story, you would have to explain why there was a shift in the particular combination of special interests.

Right. To do it, that's the way he would have to do it. That's what you'd have to fall back on.

You can't say, well, economists have finally been effective or have come up with some other explanation.

As near as I can tell you, when I discussed it with him, he never objected to my story.

Which is?

Which is better articulated than I'm going to do it now, in a piece I did for the Brookings papers which is called … I forget the name of it now.[25] In any case, it was basically that Brookings has a conference every year in this type of applied micro-economics with economists discussing their papers. The guy who was running the show at Brookings called me up and basically asked me your question. How can it be, it's the late 80s, we've had basically ten years of this deregulation stuff going on, how can you reconcile what's happened with these theories of interest groups buying regulation? I wrote a piece. He wanted the whole story. It's articulated there in that article. My take, which as I say I think he [Stigler] accepted, was that the important point of the economic theory of regulation is the redistribution part of it. At the end of the day, if you spread rents around,

[25] The elusive name is: Peltzman, Sam; Michael E. Levine and Roger G. Noll (1989). "The Economic Theory of Regulation After a Decade of Deregulation", *Brookings Papers on Economic Activity, Microeconomics* 1989:1–58.

when the rents disappear, there's no point in having the regulation. That's the point. In part the dissipation of rents is endogenous to the regulatory process. You take the simple story of the airlines in America. I don't know about the rest of the world, but in America, after deregulation you simply had a price war. Under regulation, costs are bound to rise if you divert competition from price to non-price factors. Once that process is finished there's no point to the regulation any more. If in fact, once the dissipation of rents is complete, then you just have a substitution of non-price for price competition. Of course the producers aren't getting anything.

No.

The consumers are getting a non-optimal bundle. What's the point of the regulation? That's the way I explained it. I go through cases and I show where that view of the endogenous kind of dissipation of rents works, where it is satisfactory and where it is not satisfactory. But that's the one I articulated. George would go around the country making speeches about this sort of thing. He would ask me for example, "What did 'Y' do?" because he didn't know very much about how regulation actually worked. He would ask me, "Well what is the effect of regulation on the railroads? What do people who worry about this think about it? Or the airlines? Or the trucking industry?" And I would tell him this kind of a story. He would put it into his speeches when he went around the country. We came to some agreement on it. I mean he accepted, very quickly, that this view that regulation is just bought and sold by special interests at some auction (some sort of government auction where the prize is set and special interests win it) wasn't complete. He accepted that this was not a completely satisfactory view of the regulatory process. He accepted that. Once you do that, then deregulation does become more, at least more viable, than if you just have the very simple story which says, "We won, we got our prize. Now all of a sudden, it's being taken away from us?" I mean, can that be accurate? It becomes completely a power game. In which case, there must be some aspect of the underlying politics that has changed. My story was, "No, the economic background has changed. In very profound ways the economic background has changed, which makes it pointless to have the regulation." I happen to think that that's very important today, a very powerful force. My students will tell you that in the mid-70s, before any of this began, when I talked about banking, for example, I said, "There is no way this industry can remain regulated. There is no way." Even airlines, this is true even in airlines where the first response to the dissipation of the rents was to try to make them operate better. I said, "Look, there is something very funny going on here." This is the mid-70s. I can remember very clearly telling my class that there is something very funny going on. This is something never seen before. A regulatory agency is saying that what we need is more efficiency. I said, "This is a signal that either one of two things can happen. Either the regulators are going to be replaced by other regulators who get off this efficiency kick, or regulation is dying." Because a view that what they are trying to do is to use exactly what in textbook welfare economics are efficient price signals, efficient resource allocation, can't be correct. That's the view that I came to believe was the wrong view of what regulation is trying to do. Regulation only survives if it has rents to spread around. In the past it used price and entry in the traditional mode. Today it's more complicated. But in the airline case, it had price and entry as its delegated mechanisms. So it's got to create inefficient prices if it's going to do its job. If it's focusing on efficiency, something's wrong. So I knew it then. I had this view already back in the 70s that regulation can put itself out of business.

Seems it could also be a technological change. I think of the communications field, where the change in technology made the regulation superfluous. There were no more rents to be had.

Yes. Yes. Although I like to think a lot of that is endogenous.

What? The technology is spurred by…

Yes, it has to come from somewhere.

Another thing, what you said about how he'd often ask you for the details…

Yes. I wouldn't say often but he would when he …

I mean, in a lot of his work it seems that he had this broad picture or vision, but the details, the actual grubbing into how each industry or firm, or this or that worked … He wasn't really interested in that. That was for somebody else to do. Again, he wasn't against doing it, but he was pretty uninterested in, for instance, the working of a firm or the organization of the firm, the interior transaction costs or hierarchies. He doesn't seem to have any interest.

Yes, it's methodological. It just wasn't the right way to go.

Now getting back to these government officials and politicians, did he think it was purely narrow self-interest that was driving these…

As I said, he changed his views on this thing over time and I don't know what in the end he really thought as to narrow or broad. He kind of accepted all of my stuff and Becker's stuff that it was a more complex coalition that was driving this, and never really defended vigorously the very simple view that he articulated in the early 70s.

Well, would he entirely rule out it playing any part, not perhaps a big part, but any part? That there is this ideal of public service? That there are people who want to perform…

George never distinguished between performing a little role and no role at all. Okay. He never distinguished between the two. I mean that was it, that was his empirical outlook. There just wasn't any. There wasn't. If it made a difference between 90 % and 100 % then just forget about it. That was the way he viewed it. He's not going to deny that there could be a sense of public service. Of course there may be some people who go into public service for that reason. But it's not going to have much of an effect.

OK.

But it couldn't be very important; it couldn't have an important effect. What was important was the work he was doing. He once said the essence of economics, the very essence of economics is when somebody knows the difference between 1.01 to the one-hundredth power which is a big number and 1.01 to the 0.000001 power which is a small number, and makes a small difference. Knowing the difference between big and small is the essence of the science of economics. It's a surprising kind of statement, but he believed that. That's what he believed. So he would often take, and it got him into trouble, he often would take these very strong positions which he often convinced himself were actually true. Like when he was confronted with some fact about regulation, he would say "Ah, you're going to find some Congressman was bought off. [laughter] You are actually going to find that. That's what you're going to find. Are you sure that you didn't find that this Congressman wasn't on the take?" You know, that kind of writing, that kind of a very strong view.

Yes.

But it all can clearly be explained, you know. I don't think it was clear in his mind what the distinction was, between stating a powerful position that covers a lot of the cases, and what happens in a particular event. When confronted with a particular application he literally almost believed it. He wouldn't back off and say "Well, look, that's part of the remaining 10 or 20 percent, which is doing better than explaining just fifty percent – tossing coins, that's just part of the noise." Methodologically he understood that, but in his gut I don't think he really understood that.

But of course if you explain 80 % of the cases, I mean, what more do you want?

What more do you want? Exactly. Yet he would often criticize, I know he would often criticize people for trying to explain every observation and not accepting the fact that there was a certain degree of error. But when in the middle of a lunchtime conversation, if you confronted him with something like that, with some anomaly, he could have just said, "Look, that's the error. Let's just go on and talk about the substance." But he would in fact say, "No. You're going to find that some interest was in there on the political front. You'll find it if you look hard enough." You wouldn't find it.

It seems to me a reaction against any ambiguity.

I mean, there is that. There is that element in George, that's for sure. That's for sure.

I mean, people have different views, but to a certain degree, the world is ambiguous. You live with it. You try…

He wouldn't have been as influential a figure if he took that seriously.

Part of his marketing.

Yes.

But, I mean, what you're saying is you think it even extends to everyday conversation.

But how much of that was show, and how much was actual belief… There was a lot of show in that. I mean there is no doubt. He had this Protestant father image and he constantly supported that image. One way to do it was just to wave your hands. At times he just waved his hands.

That's very interesting. Because, it seems that in so much of his work, especially his later work, he is always trying to push that envelope, to stake out a position as extreme as he can. Certainly his work with Gary Becker on rationality, is always pushing, always pushing it to extremes.

Yes.

Now, was that do you think part of that approach? Part of that marketing approach?

It was part. But as I said, he really believed in the rational mind. You'd show him some example of an irrational behavior … there's a lot of this sort of work going on now it just so happens … and he would show you that it can't be true.[26]

Almost by definition …

Almost. Almost. It's getting more and more, more and more part of him as he got older actually, this whole view. He insists it's rational. He would tell you, "There is some rational explanation for it. It's just that you haven't looked completely into it and found it." On the other hand, he asked me, for example, I remember this very clearly. He asked me to review a revision of his textbook.[27] And again there is part of the textbook where he discussed this kind of issue, about "are people doing things in their own interest." This was during his phase of "everything has to have an empirical implication," and testing this was most important. He says well, look, we have to put this on the line. We have to put this world-view on the line. And it means that we are going to find some cases where it fails. And he gave as an example of a case where it failed, Christmas Clubs. Do you remember what Christmas Clubs?

I remember what Christmas Clubs were.

He said in his first draft "The fact that you have this, and that they never paid interest, and that you could have done exactly the same thing, put in a dollar a week or whatever it was, and you could have gotten interest, demonstrates irrational behaviour."

Except you could have withdrawn the money from the regular account …

I don't know if you couldn't have done it with a Christmas Club.

You were locked in.

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[26] Peltzman is most likely referring to work in behavioral economics which was gaining some steam in 1997. Among others, Daniel Kahneman, Amos Tversky and Richard Thaler had produced some initial work on the subject. At Chicago, a joint workshop with members from the economics and psychology departments was involved in discussing the possible merits and shortcomings of this approach.

[27] The reference here is to the 1987, 4th edition of George Stigler's textbook, the aptly named *Theory of Price* (New York: Macmillan). There is at least some foundation for assuming that this new edition was spurred by his 1982 Nobel Prize for Economics. The previous edition had appeared some two decades previously.

Maybe he didn't even know that. What mattered to him was that you had the higher return for exactly the same action. I said, "George, you should take that out. Because it is wrong. It's wrong. We don't know, economists don't know what is in the level of a utility function." We don't know whether it fits. It could be ten different things. Now I didn't even want to speculate about what they could be. The service of being constantly reminded, that's worth something. I told him, "George, the only thing that we economists right now can say about something like this is, 'If the gap in returns grows, less people will do Christmas Clubs'." And that's all we could really say. Because we don't know what's in people's minds, in their utility functions. In fact, it's the point that I try and take very seriously. You see a lot of this stuff about so-called irrational behavior. A lot of it is about levels of behavior. And you can't say much about that. What we can say is very mundane. The price goes up, whatever it is, people will buy less of it. And I said this to him and he took it out. He was convinced. You won't see a reference to the Christmas Clubs, or maybe if you do there is a footnote that says, "but of course, if they do less of it when the yield differential grows, then that is consistent with economics," or something like that. So I forget why I raised that, but it's an example of how he did believe that there was evidence with which the rational model could be tested. He became less and less sure of that as he got older.

Because many economists would say, well rationality is extremely useful for explaining a lot of what we observe. It isn't necessary; it isn't important one way or the other, whether it explains 100 % of all cases, all the time. It is useful. In an essay which I remember, George Stigler claimed that in fact it was a testable hypothesis.[28] *And it could actually be proved. Which is again pushing it, it seems, further than you really need to.*

Clearly, he was anxious to do that. That's partly … are you going to talk to Becker? I was George's student, in some sense I'm viewed as his

[28] This claim appears in the set of Tanner lectures he presented in 1981 [Stigler, George J. (1982) "The Ethics of Competition: The Friendly Economists" *The Economist as Preacher*. Chicago: University of Chicago Press, pp. 14–26.] The relevant description appears on pages 25–26. In this part of his lecture, he describes a test that would serve to verify his claim that "Much of the time, most of the time in fact, the self-interest theory (as I interpreted it on Smithian lines) will win." (p. 26)

protégé, something like that. Personally, Gary and George were much closer than George and I. Much closer and in many ways, I think, they were much more on the same wavelength, especially in this area, how far you could push rational choice, that kind of stuff. George was absolutely enthusiastic about everything Gary was doing, with the family, with marriage, with this, with that, with everything. But the outside world apparently wasn't. And apropos to the outside world, the graduate students at the end of the year would put on a party where they would perform a play: *The Economics of Mud* by Gary Becker. *The economics of "fill in the gap,"* by Gary Becker, you know what I mean. It was a standing joke. It has become a standing joke. But George loved that kind of thing. As I do. I mean, I agree with it. I think it is a very powerful part of the message of economics that there's growth in its thinking, and standards and a reason to take economics seriously. But in terms of actually working in that area, the "Tastes" paper, I told both of them I thought it was absurd.[29] Gary and George were much closer in those terms than George and I was.

In pushing that…

Yes, that particular envelope.

I know that in his work he was either very hostile or ignored anything that was basically non-equilibrium type of work. For instance, game theory…

Oh, yes. Very much more hostile than I am.

Basically you have bargaining with potentially many solutions. You've got this case, you've got that case and you've got some other case as well. It seemed that this was very antithetical to what his vision was.

I'm not sure that non-equilibrium is the right word, but multiple outcomes is.

Yes.

Absolutely! Absolutely, that is exactly what economics should not do. Economics is here to make the world more practical, and if at the end of the day all you can say is that what exists is possible… I could

[29] Sam Peltzman refers to one of only two papers Stigler and Becker co-authored: Stigler, George J. and Gary S. Becker (1977). "De Gustibus Non Est Disputandum", *The American Economic Review* 67(March): 76–90.

almost be putting the words George would be saying if he was sitting here: “It’s worthless.” He was extremely hostile to this debate. I may be hostile, but not as much as he was, as others here were as well. Ronald Coase was also hostile to that kind of stuff.

Part of that was exactly at the level of “where’s the beef?” It’s not telling us something he didn’t know before. It is exactly wrong to think of economics as a formalization of logic, which is the big contribution of game theory. What it does is it takes what we thought were simple problems in industrial organization like, “Should you build a big factory to deter entry?” If it takes what looks like simple problems and says, “If you think about anticipating your rival’s action and vice versa, having your rival anticipating your action, there is a logical structure that you have to impose on that problem.” That simply hadn’t been done before. I personally think that that’s a contribution of game theory, which may make it useful. The fact that it makes you think more rigorously about these things than you otherwise would. But I don’t think there’s much beyond that. He didn’t even think the first one was important. What he thought was important was to find a way to test whether big factories deter entry and that you should forget about individuals, as long as there is some simple, straightforward model, even if it doesn’t tie up all the loose ends. Even if you can show that under some set of assumed circumstances it would work but in others it wouldn’t … At the end of the day it was the empirical implications that counted with him. It’s the same with me. But I just think it is important to get your head clear about it, about what you have to assume to get this result. He thought that this was really taking economics off in precisely the wrong direction, one that would ultimately lead to its irrelevance.

He must have been particularly perturbed at the way game theory captured, to a large degree, industrial organization.

Oh, yes.

Because of course, if you look in the journals …

He was completely hostile.

Because it seems that he saw interdependency, which is what this is likely dealing with, as not a crucial issue.

No. Not true. Again, not true. One of his great …

Well, the oligopoly article.

The oligopoly article's all about interdependency.[30]

Yes. That's the solution.

Yes.

Yes, very much.

Yes.

But he saw that solution as within the framework. Within his framework…

Yes. Yeah. No, no, no, no. He's not viewing (look, let's make this clear) he's not viewing game theory as a competitor to his theory at all.

No.

His theory of oligopoly led to a lot of game theory models of how you solve that particular problem. What he would say is, "Look, my theory of oligopoly at least has an implication that the Herfindahl index should be related to the departure of price from marginal cost.[31] It may be wrong, but it has that implication." A lot of the game theory work that's come in later, not all but a lot of it is, at the end of the day, "under this structure that will happen, under that structure this will happen." I can remember as a journal editor, you would look at an article like that and you would say, "He's telling me that what is, is possible, but I knew that. What did I learn? I learned nothing." And he was hostile to this kind of ingrown fascination with only the logic of the argument.

Because that sort of navel-gazing…

Yes. That's what he was reacting against. There's nothing coming out of this.

So, if they could have…

[30] We are both referring to his seminal paper on oligopoly: Stigler, George J. (1964) "A Theory of Oligopoly", *Journal of Political Economy* 72(February):44–61. I might argue that the article attempted to do away with any need for a theory of oligopoly. However it is interesting to look at an article Sam Peltzman wrote more than a decade after this conversation.

Stigler's article was a landmark in the theory of industrial organization and in the practice of antitrust. For industrial organization economists it focused attention on the sorry state of oligopoly theory and, using information theory, proposed a theory that could explain the deviations of oligopoly pricing from competitive pricing. For antitrust practitioners the article came to have an important impact on the application of antitrust law, especially in the merger area. Indeed, it is not an overstatement to say that Stigler's theory of oligopoly remains a central pillar in merger policy in most, if not all, anti-trust regimes around the world (Carlton, Dennis W. and Sam Peltzman (2010) "Introduction to Stigler's Oligopoly", *Competition Policy International* 6(2):237–251.)

[31] The Herfindahl index is a commonly used concentration index in economics.

If they could have come out with anything, if, at the end of the day there was a clear empirical implication, widely accepted, or at least widely discussed and tested …

OK

Fine, but there wasn't. Not only that, but the people who were doing it didn't even care. That's really what got him. These people didn't even care about it. They cared about communicating with each other. I mean, it's a view I have too. We will become irrelevant if we become a bunch of people who are arguing about the logic of each other's models.

So, in essence, it has failed to come up with any application…

No. I wouldn't say "any." I wouldn't say "any." But, the power of the minds being trained in doing this relative to the output is sobering. That ratio is so overwhelming. It really wasn't productive. It wasn't leading us to a wider, comparative advantage kind of argument. Sure, you can get something out of it. Occasionally something will come out of it, but we are wasting our time, given the opportunity costs of the time that could be spent on real problems. It is a negative net effect.

OK. Now would that extend to, say, the Williamson type of transaction costs?

A little bit. A little bit.

Was it that there was nothing testable coming out of it? Or…

He wasn't hostile to that sort of thing per se. It just had a very narrow pathway. It's still true today. And that is a sore point. Williamson himself thinks it has a lot of applications; that would be his view. But he's looking at the world in a particular way. If you look at the larger "what do we know as a result of this approach," of course these approaches have a certain amount of application, there are a few things we've learned about the firm, but I'm talking relative to the time spent. I mean there are serious folks now, his students, who understand that there is a problem and are trying to fix it. But you know what George would have said. "Twenty years! … Or whatever it is … Thirty years! They've been working on this stuff. That's getting to the end of the short run." Quote, unquote, he would say that.

OK.

That was his reaction to Marx. I would always say "You know, Marx is a great economist, George. He had this powerful … he had this Chicago world-view that economics can change the world, that it's important, that it touches everything, literature and all this kind of stuff. I learned that from Marx, George."[32] And he would say "It's a hundred years since he predicted the downfall of capitalism and a hundred years is about ninety years after the end of the short run, that's the problem."

That's very interesting. Because that is very consistent with his, "This is how we make progress" approach.

Yes.

And "This is how we do not make progress."

Yes, that's a very strong feeling. It's OK to try game theory. But to stick around for twenty years and come up with a result that anything is possible and then to say that this is economics. This is almost the way George would be talking if he was sitting here. "Having you and your six friends argue about a lemma, that's progress!" He wouldn't be indignant. He would be laughing. He would be dismissive. Saying, "You're dopes. You're dopes." What should you do with them, George? "Exile them to Samoa." Dismissed with a wave of the hand.

Yeah. So it was very much of a "put up …

Put up, or shut up, and in real time. Don't tell me it's going to happen in the next generation.

If you're just patient enough.

Yes. I've been around too long to hear that story. That's a cop-out.

Basically, we're distinguishing about long-run productive work and fads.

Yes.

Things that come in and things that go out and then come back again. OK. So that's again one thing that is consistent. There is that very clear-cut view.

Yes, it's in all his work. And it's had a lasting impact around here.

Mmm.

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[32] One of his more pointed remarks about Marx appears in: Stigler, George J. (1976) Do Economists Matter?" *Southern Economic Journal* 42(January): 347–54. "My friend Robert Fogel has told me that I am a Marxist, perhaps partly because of the role I assign to intellectuals as spokesmen of important groups within the society. My reply is that if I were a Marxist, Karl would not be a very good one. His acid-filled pen was indeed thrown often at the apologists of capitalism and earlier systems. The unrighteous indignation aside, I accept the view that Smith was the honourable spokesman for the agricultural and worker classes of England, as well as the premier economist of all time. But Marx with unbelievable vanity or myopia, excused himself and his doctrines from this description, whereas on my view he was obviously the spokesman for rising industrial proletariat, as well as a premier sociologist." (p. 351)

You don't have much of that kind of theory building that has no empirical bottom line.

No.

We're, especially the business school faculty, pretty much agreed that that is not the way to go.

It is clear that mathematical theory, mathematical models were in that category as well.

Yes. Yes, not just game theory, but any enterprise which starts to become an end in itself… You understand what I mean. It's when all that is important is whether you got the thing right. That's not important. What's important is that you say something about the world.

So, I mean in that sense, he's taking a very applied approach. Economics, as many other economists have said, is an applied science. And if you are not doing an applied science, you're not saying anything, right?

Well, I don't say you're not saying anything. He's not telling applied scientists how to do the application. He's not saying this. But, yes, very much so. This is the most powerful message of Chicago economics. It's not a policy implication, it's not methodological, in the narrow sense about how you do it, it's that economics can say something about the way the world works. Economics is to be taken very seriously. Why, because it can tell you something real about the world.

So, if George Stigler confronts evidence that is convincing enough, he would be willing to shift.

Well, he was a complex person. And we've discussed that and we are going over old ground. He was a complex person. His instinct would be, say, the evidence needs to be re-examined very carefully and so on, and so forth. But, yes. I think he would be. "If that's what the evidence is, that is what it is and we have to, we *have* to, you know at the end of the day, if we are convinced that that is what the evidence is, that is the truth. Not our pride. Our pride is not the truth."But he was complex… Who was it, Mel Reder who wrote a piece in which he said "the distinguishing characteristic of Chicago economics is 'the tight prior equilibrium'."[33] That was wrong.

[33] This is an interesting attempt to analyse, quite critically, the Chicago approach from someone on the inside. See, Reder, Melvin (1982) "Chicago Economics: Permanence and Change", *Journal of Economic Literature* 20 (March) 1–38.

I told Mel it was wrong, but it is easy to see how somebody could come to that conclusion.

Yes.

There was a "tight prior" but George believed in it based on the evidence.

OK. So the initial reaction then might be to say we need to re-examine the evidence at a certain level.

Yes, that's the joke about my dissertation. He said "I'm glad to see your preliminary results." But, he accepted them. And he changed. He changed from that position that regulation couldn't have any effects. And his '71 paper wouldn't have been written if regulation never had any effects. So, he moved. He changed his position on anti-trusts because of the evidence. He had this Simonsian framework that he gave up, largely because the evidence wasn't consistent with it.

OK, that's excellent.

Great.

Thank you.

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C. Freedman, *In Search of the Two-Handed Economist*,
DOI 10.1057/978-1-137-58974-3

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