EDITED BY
ELENA ARONOVA & SIMONE TURCHETTI

SCIENCE STUDIES DURING THE COLD WAR AND BEYOND

Paradigms Defected

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Elena Aronova • Simone Turchetti
Editors

Science Studies during the Cold War and Beyond

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In memory of John V. Pickstone who encouraged us to pursue this research project.

Preface

This book was inspired by an international conference "Politics and Context of Science Studies during the Cold War and Beyond," held at the Alfried Krupp Wissenschaftskolleg in Greifswald on March 22–24, 2012. The workshop's organizer, Elena Aronova, was generously offered this opportunity as a 2011–2012 fellow-in-residence at the Wissenschaftskolleg. Her work on the history of the studies of science in Cold War America and the Soviet Union motivated the aim of the workshop: to investigate a variety of national "science studies" projects across the East–West and Global North–South divides during the Cold War, as a counterbalance to the extant historiography of science studies that focuses almost exclusively on the discussions in the West (the USA and Western Europe). The response to the call for papers confirmed what was obvious at the time: that almost everyone in the field was *interested* in the field's origins, contexts, and politics, but it was hard to find anyone who actually researched the subject the way historians of science research the history of *other* fields in the Cold War.

The workshop thus became an incentive for many participants to dig into the archives, to survey the literature in a systematic fashion, and, in some cases, to reflect on their own past. Indeed, several contributors have been directly or indirectly involved in the episodes they historicize in their chapters. The initial versions of the papers presented at the workshop generated the discussion that continued further. The institutional support of the Alfried Krupp Wissenschaftskolleg in Greifswald and the Max Planck Institute for the History of Science in Berlin allowed this project to evolve. With Simone Turchetti as a co-editor, we took the next step, making additional selection of contributions to strengthen the book's coherence around three parts: "Science Studies in the 'West,'" "Studies of Science Behind the 'Curtain,'" and "National Agendas of the Studies of Science Beyond the 'Two Blocs.'"

Elena Aronova would like to thank all the guests of the initial workshop and the keynote speaker, Naomi Oreskes, for their generous and stimulating interventions, as well as for their enthusiastic response and support without which this project would never have materialized. A special word of gratitude is due to the Wissenschaftskolleg staff that contributed to making the initial workshop an important inspiration to pursue the project further. Simone Turchetti would like to thank Jonathan Harwood, Gianni Battimelli, Paul Forman, John Krige, Michael Worboys, Roy MacLeod, John Christie, and Pietro Corsi for exchanging their views during the completion of his chapter. Turchetti's research was funded by the European Research Council. Michal Kokowski would like to express his gratitude to the editors of the volume, Elena Aronova and Simone Turchetti, for their helpful comments, criticism, and editorial work. Kokowski also acknowledges Aronova for arousing his interest in the subject through her publications. Lu Gao would like to acknowledge Zuoyue Wang who introduced her to Aronova and encouraged to pursue her research. Hans-Joachim Dahms thanks George Reisch for discussions in Greifswald, in Berlin, and on the internet. Thanks also for permission to cite from his unpublished articles! Dahms is also indebted to Aronova and Turchetti for discussions on his article. Ian James Kidd acknowledges Donald Gillies and Paul Hoyningen-Huene for helpful discussions, and the editors for their kind invitation to contribute to this volume and their helpful comments on earlier drafts. Kidd's research was funded by an Addison Wheeler Fellowship. Sommer's research was funded by the European Research Council and the Czech Science Foundation (GAČR). Vasen acknowledges the University of Quilmes and CONICET for institutional and financial support and Adriana Feld and Judith Naidorf for enriching discussions on the paper.

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CHAPTER 1

Introduction: Science Studies in East and West—Incommensurable Paradigms?

Elena Aronova and Simone Turchetti

No less than three generations of scholars have been introduced to science studies through the work of the American science historian Thomas Samuel Kuhn. His work, especially *The Structure of Scientific Revolution* (1962)—the book that fomented the field of science studies—continues to be a key reference, especially for university teachers. Few, however, would be prepared to lecture on the theories of *vědecko-technickárevoluce* (scientific-technical revolution) of the Czech communist scholar Radovan Richta. Today his name, in contrast to Kuhn's, means something only to a few *connoisseurs*, and his *Civilizacenarozcestí* (Civilization at the Crossroads) is more likely to be found in second-hand markets selling Cold War antiques than on a syllabus of a class introducing students to

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the field of science studies. The perils of Richta's production matter not only to the Cold War nostalgic. They show the remarkable gap in our knowledge of the origins and development of science studies in the Cold War period. Despite the fact that science studies scholars have urged to embrace symmetry in portraying key episodes in the history of science, they suddenly become linear and unidirectional when asked to discuss the history of their own discipline.[1]

As a form of reflexivity and expertise on science's present, past, and future, "science studies" and the neighboring fields of history and philosophy of science may appear to an observer as merely arcane lines of enquiry about the nature of scientific knowledge and its methodology. It is telling, however, that comparatively recent debates on the seemingly abstract issue of whether scientific theories are socially constructed, which were dubbed the "science wars," were accompanied by the kind of bitterness and deep politicization that suggests a larger significance of the studies of science beyond the relatively small community of students of science.[2] One indication of larger political ramifications of the field is the fact that in different forms, "science studies" surfaced during the Cold War in a variety of political regimes and nation-states: from Western liberal democracies to communist China, and from Soviet bloc regimes to new nations of the so-called "Third world." In the decades following the end of World War 2 (WW2), marked by the shock of Hiroshima and Nagasaki, scientists and politicians alike emphasized that science, which made the atomic bomb possible, had transformed the world. The centrality of scientific and technological prowess for the Cold War itself, and the high currency the technocratic social engineering arguments had acquired as a result, moved the study of science, now refashioned as *science of science*, from an academic periphery to a prominence it had never enjoyed before.

As several scholars have pointed out, the Cold War was as much about knowledge as about *knowledge about knowledge*—who had it and who hadn't, but also how to put knowledge by those who had it to use as a critical component of soft diplomacy.[3] The intellectuals' quest for the new ways to analyze science's growth and predict its development may well not have been directly enlisted in the service of the state. Yet, the repercussions of the new form of expertise were clear to technocratic managers who sought to cultivate new experts on science for various national and geopolitical needs. The proliferation of new terms—науковедение (*naukovedenie*) in the Soviet Union, *naukoznawstwo* in Poland, 自然辩证

法 (*dialectics of nature*) in China, and *science studies* in English-speaking countries—manifested the emergence of a new disciplinary identity.

What role did these meta-studies of science play in the Cold War? How did different national varieties of "science studies" meet the political demands of the Cold War? How did the studies of science, as intellectual projects, participate in defining the relationships between science, society, and the state? Was the conceptual core of the new field affected, and how? And how did Cold War politics shape, or fail to shape, the field of science studies as we now know it?

This collection brings together historians and philosophers of science, Europeanists, and science studies scholars from many different national traditions to address these and other questions. The chapters in this volume do not offer a synthetic narrative, but they do present a reader with the subject's wide parameters, considering various ways in which the studies of science as a new field of expertise broadly conceived intertwined with Cold War politics, in both familiar and less familiar "battlefields".

Recent years have seen the resurgence of interest in Cold War and its scientific spaces.[4] In the words of Naomi Oreskes, despite a long-sustained attention of historians to the subject, the scholarship had suffered from a "miasma problem": few historians who examined the cultural and political contexts of a given science made stronger claims about how exactly that context affected the *content* of scientific knowledge.[5] The recent new wave of scholarship on natural and social sciences in America during the Cold War had moved past "miasmas," elucidating the ways in which the conceptual foundations, frames of analyses, and the tools of such new sciences as cognitive sciences, area studies, and Soviet studies, as well as an array of well-established disciplines, were interwoven with a particular set of cultural norms, imperatives, and rationalities that underpinned American culture during the Cold War.[6] While these new studies have demonstrated how various situations were *created* by the Cold War, some scholars have called into question the usefulness of "Cold War" as an analytical category.[7] Questioning the novelty and the perceived ruptures in, for instance, American human and social sciences during the Cold War, these scholars highlighted the important continuities whose erasure in itself contributed into constructing the notion of "Cold War."

Indeed, even the term "Cold War," far from being a neutral periodization, embedded contestation and produced deep animosity.[8] The phrase, which was coined by George Orwell in 1945 to describe the undeclared state of war between the USA and the USSR made "cold" by the atomic bomb, was used in the first postwar decade by American analysts as a synonym of Joseph Stalin's confrontational politics.[9] Cold War, as the argument behind the phrase went, was imposed on the rest of the world by the Soviet tyrannical leader, who waged the war against the West (meaning mostly the USA and Britain). Not surprisingly, throughout the Cold War the term was used mostly in North America and Western Europe (albeit more in West Germany and Switzerland than in France or Austria), but not within the Soviet bloc. The Soviet Union was never a true competitor to the *other* superpower and was striving to keep the precarious legitimacy of its post-WW2 territorial gains, rather than to seek a confrontation with its more powerful and secure Western former wartime allies.[10] Scholars in the Soviet Union and East European countries described the tensions in other terms until the "Cold War" itself was over.

Yet, the Cold War is, for better or worse, an impossible issue to avoid, however we may wish to get rid of it. It has been, and remains, the frame of reference for much of what has been written about the role of science in the political and cultural diplomacy in the second half of the twentieth century. With only few exceptions, however, the historical reflection on Cold War science has been largely concerned with the North American, and, to a smaller extent, Western European side of the conflict. It is important therefore to de-link our understanding of the Cold War and its scientific spaces from the trajectory that remains focused on American–Western–European narratives.

The histories of various national intellectual projects under the umbrella of the studies of science that we summarily render "science studies," on the other hand, present a vantage point from which to pluralize some of the visions that were constitutive to the construction of "Cold War" as a juxtaposition of the liberal democracies in the "West" and communism in the "East." Lorraine Daston has pointed out that until very recently the history of science has been largely a "European self-portraiture."[11] Meta-reflection on science's past, present, and future was a brainchild of the Enlightenment.[12] As part of the Enlightenment project, historiography of science linked science to liberal notions of freedom, progress, and individual creativity, on the one hand, and with modernity, "civilization," "Europe," and the "West," on the other.[13] The institutionalization of history of science in the Anglophone West in the post-WW2 years consolidated this view. The new historiography of science has centered on "the Scientific Revolution"—the organizing master narrative of the new discipline—that posited the theoretical transformations of the sixteenth and seventeenth centuries as a pivotal moment in the history of modern science, and modernity itself.[14] As Herbert Butterfield, the doyen of the history of science in Britain in the first postwar decade, has put it, "The scientific revolution we must regard […] as a creative product of the West—depending on a complicated set of conditions which existed only in Western Europe."[15]

Many scholars have pointed out that the view of science as a uniquely Western product was encouraged by the political climate and, indeed, the notion of "the Cold War." This view posited "the East," which had not produced modern science, as "underdeveloped" vis-à-vis "the West" and, in the worst-case scenario, threatening to spread out coercion and totalitarianism.[16] With the Scientific Revolution conceptualized as a key defining moment of new intellectual history of science, the contribution of Islamic scholars during the Middle Ages, as well as the developments in modern Russia, China, and Japan, fell out of focus and was marginalized. As Geert Somsen pointed out in a recent essay, the narrative of "the Scientific Revolution" entailed conceptual, chronological, as well as *geographical* priorities.[17]

The consolidation of a Eurocentric conception of science in the first post-WW2 decades marked a decisive reversal of the earlier, interwar agenda of the field. As Anna Mayer has argued, the notions of science as global endeavor and of history of science as an international enterprise were crucial for the unprecedented growth and expansion of history of science as a discipline in the first half of the twentieth century in Europe and North America.[18] In the 1920s and 1930s, the kinds of academic forums, networks, professional associations, and specialized journals for the history of science that conventionally characterize academic professions have been established in the USA, Britain, and the Soviet Union.[19] Such institutional innovations as the journal *Isis* (founded in 1913 by a Belgian chemist-turned-historian George Sarton, who almost single-handedly launched the discipline of history of science in the USA), the History of Science Society (established in 1924), the Académie Internationale d'Histoire des Sciences (founded in 1927), and, in the same year, the Institute for the History of Science and Technology in the Soviet Union headed by Nikolai Bukharin, all emphasized that history of science must be pursued collaboratively by international practitioners.[20] In 1931, the surprise appearance of a Soviet delegation at the Second International Congress of the History of Science and Technology in London made a lasting impression on Western science historians, from John Desmond Bernal to Sarton's student Robert Merton, catalyzing the spread of Marxist history of science in Anglo-American literature.[21] Marxist approaches, which rooted scientific discoveries in socioeconomic forces and dialectical materialism, appealed to British left-wing scientists. In his influential book *The Social Function of Science* (1939), Bernal sought to provide the basis for the new scientific discipline, "the science of science," grounding it in Marxist historical materialism, Engels's dialectics of nature, and socialist planning.[22] However diverse the approaches to history of science were in these three settings, stretching from Marxism to classical liberalism, they embedded the idea that science is global and international, a means to unify the world at a time of social crises and political upheavals in Europe between the two world wars.

With the onset of the Cold War, history of science in the Anglophone West became dominated by historians with decidedly anti-Marxist views, ranging from a Russian-born, Paris-based Alexandre Koyré—who in the 1950s and 1960s became a leading figure in American history of science—to historians like Rupert Hall, who occupied the first chair for history of science at Cambridge.[23] These pioneers of the history of science who contributed to the professionalization of the teaching of the history of science in the 1950s in American and British universities endorsed a view of science as a particular—Western—cultural product rather than a universal means to solve practical problems. The orientation of the field that resulted from this view, as Somsen summarized, was "a conception of the field as completely depoliticized, […] reconceptualized from a global endeavor to a European production."[24] While the notion of science as European-qua-Western endeavor is thoroughly refuted in today's historiography, it still has an enduring widespread presence, adopted, for instance, in standard "Western Civilization" courses in American universities.[25]

Another imbalance in extant historiographies is the lack of contextualization of postwar history of science and science studies within their own political context. Until fairly recently, historiographic accounts mentioned the Cold War mainly as a setting: the dramatic reorientation of the history of science and the emergence of a new field, "science studies," associated with the proliferation of the sociology of knowledge and the extension of cultural anthropology, critical theory, feminist and postcolonial frameworks into the studies of science, occurred against the background of the political and social turbulences of the 1960s. That these two developments were related was clear in retrospect but the nature of this relationship remained largely unspecified.[26] That has begun to change. Some scholars have recently suggested that Cold War anti-communism was a shaping factor of the dramatic reorientations in the research agenda of the studies of science in the 1960s. For instance, George Reisch has argued that Cold War political culture, with its brainwashing and mid-control fascinations, informed Kuhn's notions of scientific theories as "incommensurable" mind-controlling paradigms, shaping his account of science and the nature of scientific knowledge.[27]

The Cold War context was neither monolithic nor deterministic. While Reisch contextualized the dogmatic and even totalitarian features of Kuhn's paradigms, other scholars pointed out that the identification of "science" with "democracy" was a staple of Cold War rhetoric and the locus of theoretical deliberations that shaped the intellectual agenda of "science studies."[28] Elsewhere, one of us has written about the ways in which the Congress for Cultural Freedom, an influential organization of intellectuals covertly funded by the CIA, promoted "science studies" in the 1950s and 1960s as part of the organization's broader agenda to offer a "post-Marxian basis for liberalism."[29] Michael Polanyi's programmatic article, "The Republic of Science" (1962) explicitly rejected Bernal's emphasis on the planning of science and stressed the importance to let scientists free to autonomously make their own choices when pursuing novel research.[30] Polanyi's analysis of science, which connected scientific mindset with the democratic one, and free marketplace of ideas with market-based liberal democracies, was a crucial component of the Congress for Cultural Freedom's agenda.

The present volume extends and complements the existing historical accounts by looking at the parallel developments elsewhere if not everywhere in the world. To the more familiar sites of the cultural Cold War in the studies of science—such as the marginalization of Marxist history of science[31]—the chapters in this volume add a much-needed comparative dimension by considering, for instance, the ways in which the intellectual ambitions of such an iconoclastic philosopher of science as Paul Feyerabend were affected by Cold War politics. It also elucidates the ways in which the supposed de-politicization of the field was a political act in itself in different national and political settings. Moving beyond the Western European and American world—to Eastern Europe, China, and, in less detail, Latin America—the book presents the accounts of various "science studies" projects embedded in local national as well as international contexts. For instance, "science studies" scholars in China embraced Engels's dialectics of nature in order to further strengthen their national scientific establishment and to put the study of science at the service of the communist state. In countries such as Poland and Czechoslovakia the proliferation of "science of science," including Richta's forgotten work, was prompted by the ambition to further advance these countries' scientific programs and to modernize Marxism in the wake of Stalin's death. Uprisings and tensions with the Soviet Union catered for appraisals and revisions of existing work, which further demonstrates that the political dimension was an inescapable reference for the students of science during the Cold War.

Taken together, the chapters highlight two primary roles of science studies in the Cold War. First, science studies played a *political role* in cultural Cold War in sustaining, or destabilizing, political ideologies in different political and national contexts. Second, it was an instrument of science *policies* in the early Cold War and as such science studies were promoted as the underpinning for the national policies framed with regard to both the global geopolitical chess game and local national priorities. As this book demonstrates, however, the wider we cast our net, extending our histories beyond the more researched developments in the Anglophone West, the more complex and ambivalent both the "science studies" and "the Cold War" become outside these more familiar spaces. These national stories may appear incommensurable with what we know as science studies today, but that these stories are crucial to consider in order to avoid bracketing the accounts of "Cold War" within the intellectual iron curtain, that is, by focusing on only one—winning—side of the debates.

The Volume's Outline

The book is divided into three parts. The first part examines the developments in the USA, UK, and Western Europe, showing how the work of such iconoclastic science studies scholars as Thomas Kuhn, Paul Feyerabend, and Paul Forman, among others, was informed by the anxieties of the Cold War era. As *George Reisch* argues in the second chapter, Kuhn's account on science was shaped by the demands of the early Cold War. Kuhn's analysis of scientific theories as incommensurable and mind-controlling paradigms resonated with the views of leading US administrators arguing for the existence of antithetical and incompatible political systems. Reisch examines two areas of concern of American Cold War foundations and American universities—specifically Kuhn's Harvard—to understand the puzzling inner workings of the communist mind, and to educate American students about the nature and power of communist ideology. As Reisch argues, Kuhn's revolutionary views were inspired by his historical interests, yet it was equally shaped by the surrounding discourse about the power of ideology to divide humanity into incompatible communities and by the urgent need to understand the peculiar semantic mechanisms at work in the "Soviet mind."

Ian Kidd focuses on another iconoclastic philosopher of science, Paul Feyerabend, whose intellectual and life trajectory was also deeply influenced by the Cold War. Kidd's analysis helps us to think about the relationship between the Cold War and science studies outside the frame of a bipolar logic and shows that science studies, both as an intellectual *and* political project, were invigorated by the political opposition to bipolarism that emerged in conjunction with the campus protests of the late 1960s. Feyerabend sought to unite political orientation and intellectual production through the critique of modern science and believed that this criticism could propel more democratic ways to manage it.

Continuing the theme of protest movements and political dissent of the late 1960s and 1970s, *Simone Turchetti* examines the role of radical science movement in transforming the studies of science and technology in later Cold War years. Focusing on the retrenchment that occurred *after* the protest movement ended, Turchetti shows that intellectual production lost its significance as political and social critique while it continued as a theoretical debate concerning the epistemology of science itself, spreading out provocative claims about non-neutrality of scientific knowledge. Although by the mid-1970s radical views on science had been marginalized, former activists and academics continued to cultivate relations internationally, contaminating each other's work with innovative ideas. Tracing the transnational flow of ideas between Italy, Britain, and the USA, Turchetti details the ways in which some of the most influential and provocative ideas such as Paul Forman's "distortionist hypothesis" manifest a legacy with the production of the radical science movement.

In the last chapter of this part *Hans-Joachim Dahms* tempers an analytical dependence on the supposedly all-encompassing role of Cold War politics, discussing the merits and shortcomings of reading Kuhn's *Structure* as a document of the Cold War era. By examining the ways in which Kuhn was influenced by and relied upon the ideas and works of Ludwik Fleck's and Vienna circle philosophers in the interwar period, Dahms shows how the cultural Cold War relied on relations and resources that existed before the Cold War started, which were mobilized and constantly reinterpreted throughout its duration.

The second part of this volume explores the simultaneous reconfigurations of studies of science behind the "curtain." In the Soviet Union the promotion of "science of science" was part and parcel of the vision of the socialist modernity that was showcased and exported throughout the Eastern Europe.[32] Yet, as chapters in this part show, this broad framework of reference had a range of local and regional variations and alternatives, which became especially tangible in the aftermath of Stalin's death in 1953. Khrushchev's February 1956 "Secret Speech" at a closed session of the Soviet Union's 20th Party Congress marked the turning point in the geopolitical alliances throughout Eastern Europe. The new Soviet leader's denunciation of Stalin caused bureaucratic confusion at the top and revolts from below that threatened the precarious legitimacy of Soviet bloc regimes. Two countries immediately affected were Poland and Hungary. The second and third chapters in this part document the ways in which the Hungarian Revolution of 1956 and the "Polish October" uprising the same year transformed the cultural debate on science in these two Soviet satellites.

Gabor Pallo shows how in the wake of the Hungarian Revolution of 1956, brutally crushed by Soviet troops, Hungarian historians adopted a particular strategy to cope with hostile cultural audiences by centering on "factology." Pallo argues that due to the narrowing of the cultural debate in post-1956 Hungary, the historians' "blindness" to philosophy helped them to write and publish work that leaned toward descriptive and "pragmatic" approaches to science and its history in order to cast aside more dangerous, politically sensitive interpretations. While in the aftermath of the Hungarian Revolution the cultural debate on science became crippled, in post-1956 Poland a critical discussion of the distortions of Stalinist power system became a backdrop for the revival of the prewar Polish "science of science" of the fame of Ludwig Fleck and Stefan Amsterdamsky. Yet, as *Michał Kokowski* documents in his chapter, this revival was cast as the "reinvention of tradition": one of the rhetorical strategies used by Polish party intellectuals to reconfigure science studies in the 1950s and 1960s was precisely to downplay the existence of this earlier tradition, in order to carve space instead for the figureheads of socialism and show that they were the real field pioneers. Thus, while in the West Fleck's work was "re-discovered" in the wake of Kuhn, in the East he was deliberately obliterated to legitimize a reinvented tradition.

In comparison to neighboring Poland and Hungary, Czechoslovakia was a haven of political stability. It is hardly a coincidence that Czech philosophers and social scientists advanced a modernized version of Soviet Marxian doctrine—the theory of scientific-technological revolution—which was eventually adopted as an official rhetoric of Soviet political leaders. The theory of scientific-technological revolution placed science, rather than class struggle, at the center of socialist vision of social change, and it offered a vision of socialist modernity that could be exported to the emerging Third World countries while prompting empirical and theoretical studies of science under this broad umbrella.[33] In his chapter *Vítězslav Sommer* explains the origins of scientific-technological revolution theory by following the trajectory of its main theorist and Czech science studies pioneer and philosopher Radovan Richta. A follower and interpreter of Joseph Stalin's theory of "active superstructure," Richta rose through the ranks of the Czechoslovak academy by offering innovative reflection about science and socialism. He eventually became a key reference for the Czech reform movement that culminated with the Prague Spring in 1968. As in the West, the year 1968 marked a turning point in the intellectual production of Eastern European scholars of science. In the cultural retrenchment that followed the collapse of Prague Spring, crushed by Soviet troops, Richta renounced the reformist cause and more radical propositions and focused instead on empirical research examining the functioning of science under socialism and capitalism and emphasized the importance of science for the functioning of modern societies.

The third part of this volume examines the politics of science studies beyond the bipolar political framework of Cold War East–West divide: in neutral Sweden, which integrated elements of both systems; in postcolonial nations of Latin America, which developed alliances growing out of their weak status on the transnational geopolitical arena; and in socialist China, which fostered an alternative model of socialism. As *Aant Elzinga* argues in his chapter, Sweden's politics of neutrality in the Cold War conflict and the adoption of a distinctive economic system, liberal corporativism, as a "third way" between state-controlled socialism *and* market-led capitalism, stimulated the cultural and scientific exchange with both the West and the East, which was translated into a technocratic vision of a "neutral" state's growth and development based on the advancement of science and technology. The result was a complicated mix of micro-politics of academic disciplines focused on "science of science" that informed similarly industrialized countries in the East and the West. The year 1968 turned out to be a litmus test for Swedish intellectuals' technocratic "neutrality." Elzinga shows that the protest movement in the West split

Swedish science studies scholars into two camps. While some continued to adhere to the fields' technocratic vision, others searched for new analytical categories to launch a passionate critique of that vision. The former would invest in the quantitative analysis of scientific research to avoid contentious questions about the political dimensions of science, whereas the latter emphasized these dimensions making it a focus of innovative research. The coexistence of these two traditions is a distinctive trait that typifies Sweden to this day.

Federico Vasen continues this focus on the national agendas of science studies, examining debates about "national science" in Argentina, and more broadly in Latin America in the 1960s and 1970s, in the context where issues about freedom and planning of science were coupled with issues of development. Latin American countries played a key role in American international politics of global Cold War. The introduction of reforms and scientific and technological advancement was intertwined with the search for consensus on the US model of development. Thus, analyzing approaches to the study of science from a Latin American angle casts new light on the heavily politicized debate regarding the role of science in weak, "underdeveloped" societies. Vasen examines the arguments of a group of Latin American science policies scholars who linked development and scientific progress. Ostracized by local academic administrators, some of them used this mild critique of science to argue for the need to adjust scientific change to social and nation-specific needs. Their argument that it would be wrong to assume that there is *one* science only leading to *one* type of development provides an interesting, and yet unexplored, contribution to debates on the foundations of science.

In the last chapter *Lu Gao* examines a context in which the frame of reference for studies of science was developed in isolation from the debates on science in both the West and the East. In Mao's China science studies emerged under the umbrella of the "dialectics of nature," an unfinished 1883 work by Friedrich Engels that extended Marxian dialectical materialism to science and nature. First published in the Soviet Union in 1925 and imported into China in the aftermath of the 1949 Communist Revolution, it was taken up by Mao and Chinese scholars and developed as a backup for Mao's radical policies.[34] In China, Gao argues, the equivalent to Western "science studies" was the "dialectics of nature"—an institutionalized discipline and an instrument of science policy in China, which promoted meta-discussions of science through the lens of Engels's laws of dialectic and considered science as an agent of cultural revolution.

By examining the historical developments that led to the formation of science studies on different sides of the global Cold War the book, as a whole, presents a picture of strikingly symmetrical developments that can be interpreted with similar categories and yet revealing interesting differences. While the individual chapters in this book are firmly embedded in national contexts, they, at the same time, shed light on various modes of circulation of ideas within and across political frontiers, revealing important exchanges between key national players that effectively communicated ideas beyond borders, and at times across the "Iron Curtain." It is both these interconnected national developments and cross-national dialogues that we consider in the reminder of this introduction.

The East and the West: Interconnections and Dialogues

While the case studies presented in this volume show how the studies of science participated in geopolitical confrontation between the "free" West and the "communist" East, the individual contributions problematize this monolithic view. First, the case studies show that there was a significant flow of knowledge across the "Curtain" and that this stream put into communication distant scholarly communities. Even when walls were erected and blockades interrupted trade and communication, East European science studies scholars kept viewing British scholar John Desmond Bernal as a guiding light in furthering their research (Sommer), while those in the West scrutinized, perhaps more critically but nonetheless with interest, the works of Soviet and East European authors (Elzinga). Kuhn's paradigms too, as the title of the book suggests, "defected" to the "East," producing novel interpretative frameworks enmeshing analyses on political and scientific revolutions in unexpected ways.[35]

Science studies in the East and the West differed considerably, constituting independent paths of development of the new field in their respective milieu. Yet these independent developments—often conceived in isolation from each other—looked at times strikingly similar, almost suggesting that while political ideologies were setting two distant worlds apart, the cultural developments in these worlds mirrored each other. On both sides of the "Curtain" the urgency of mobilizing scientists for Cold War purposes was decisive in spreading a positive, almost naively optimistic view of the ability of science and technology to address the problems of humankind (as shown by Reisch and Sommer). In the East as well as in the West, science studies scholars were expected to offer analyses of how to design new policies so as to fully exploit the potential of science and technology. In turn, this led to a flourishing of statistical work aimed at strategizing how to win the scientific race between the two blocs, how to recover from the missile gap, and ultimately how to let science and technology fulfill its promise (see papers by Kokowski and Elzinga). And in the so-called undeveloped countries belonging to both spheres of influence, science and technology came to represent a key feature in the construction and further advancement of nation-states, while, paradoxically, helping the superpowers to further assert their influence abroad. Cases as diverse as Argentina and China illustrate this contradiction (as shown by Vasen and Gao in the volume).

The pressures and tensions deriving from developing scholarly work in the Cold War climate were decisive in warping specific research trajectories. While in the West (at times hidden) military patronage and/or cultural-political agenda gave visibility to some scholars and groups while casting a shadow on others, in the East the central role played by the communist parties as (hegemonic) cultural agents produced similar effects.

In the late 1960s, both in the East and the West the arms race's "white heat" clashed with the realization of science and technology's shortcomings. Disillusionment with science intensified due to the environmental debates and concerns associated with industrialization and progress, the rallying of scientists at the service of warfare, or just the fact that science and technology had promised more but delivered less. Criticism surfaced on both sides of the political divide and, at times, found science studies scholars very receptive and eager to study the underlying problems. Their ambition was thus no longer to offer analyses of how science could do better, but rather what had gone wrong with it and what kind of reforms were needed. The overtones typifying these debates clearly signaled political commitment and engagement. Rather than cast aside the "science of science" from the Cold War political *milieu*, they brought it right in the middle of it (see chapters by Kidd and Vasen). Yet, scholars putting forward the more provocative theses and propositions were marginalized. Those in the East felt compelled to distance themselves from their original ideas, offering reading more attuned with the prevailing party line (as discussed by Sommer). Those in the West who had their heyday in the late 1960s protests spread out from university campuses later found it increasingly difficult to find a space in debates now dominated by "purified" views advocating the abandonment of the political rhetoric and aiming instead at more refined and sophisticated methods (as shown by Turchetti).

While the "Iron Curtain" was both a metaphorical and a very real divide preventing scholars from traveling and communicating with each other, it was also rather porous. Works of Western scholars continued to be translated in the East, and vice versa. This state of affairs created at times paradoxical situations. Polish philosopher Stefan Amsterdamski became extremely popular in the West while he was marginalized in his homeland. In socialist Hungary historians of science were working in an apparently self-imposed "isolation," while traveling abroad and meeting with émigrés as Michael Polanyi and Arthur Koestler. Scholars from both the East and the West traveled to neutral Sweden where they found an atmosphere that encouraged dialogue and transfer of ideas. It was, of course, "armed neutrality," but neutrality nonetheless.

Shaping the Cold War as a transnational, global conflict informed the trajectory of science studies as a discipline. Recent scholarship underscored the importance of scientific collaboration and competition across national borders in the reconfiguration of the conflict between superpowers. John Krige pointed out that co-production of scientific knowledge and political power was a distinctive factor of scientific and political relations between the USA and Western Europe. A number of other works demonstrated the critical role of international collaboration during the Cold War in fields ranging from nuclear physics to geosciences and genetics.[36] Chapters in the present book demonstrate that the circulation of *ideas* across political frontiers was an important factor in shaping science studies, especially in so far as this transfer of knowledge strengthened the ties between allied nations (between Soviet Russia and Communist China, for instance) or promoted international expansion of the discussion of state-funded science models (in non-aligned countries such as Sweden and in the Third World countries such as Argentina).

Before leaving the reader to the remaining chapters, we want to stress that we have sought to open up a space for debate and scholarship on the political stakes of science studies across national contexts. With this aim in mind, the book as a whole presents a mix of different approaches and methodologies representing the views of different generations of scholars coming from different disciplinary, linguistic, and cultural backgrounds. Some chapters are written by scholars reflecting on their own past, which they had an opportunity to witness or be personally involved in. Some chapters are written by scholars of the next generation, reflecting a heightened self-consciousness about the politics that science studies, as an intellectual project and a community of scholars, embedded during the Cold War, and beyond it. The resulting collection, we hope, will prove equally productive of debate and scholarship on the history of both science studies and Cold War.

Notes

1. On symmetry see especially David Bloor, *Knowledge and Social Imagery* (London and Boston: Routledge and K. Paul, 1976).
2. For instance, Andrew Jewett has noted that recent debates in academia "express something more than a substantive debate over epistemological issues, and something deeper than a dispute over academic status [of science studies]." (See Andrew Jewett, "Science and the Promise of Democracy in America," *Daedalus* 132/4 (2003): 64–70).
   On the "science wars" see Ian Hacking, *The Social Construction of What?* (Cambridge, MA: Harvard University Press, 1999).
3. See, for instance, Michael D. Gordin, *Red Cloud at Dawn*: *Truman*, *Stalin*, *and the End of the Atomic Monopoly* (New York: Farrar, Straus and Giroux, 2009) and Naomi Oreskes, "Science in the Origins of the Cold War," in N. Oreskes and J. Krige, eds. *Science and Technology in the GlobalCold War* (Cambridge, MA: The MIT Press, 2014), 11–29, on 13.
4. Important recent works include Paul Erickson, Judy Klein, Lorraine Daston, Rebecca Lemov, Thomas Sturm, and Michael Gordin, *How Reason Almost Lost its Mind*: *The Strange Career of Cold War Rationality* (Chicago: University of Chicago Press, 2013); Jamie Cohen-Cole, *The Open Mind*: *Cold War Politics and the Sciences of Human Nature* (Chicago: University of Chicago Press, 2014); Joel Isaac, *Working Knowledge*: *Making the Human Sciences from Parsons to Kuhn* (Cambridge, MA: Harvard University Press, 2012); Mark Solovey and Hamilton Cravens, eds.,*Cold War Social Science*: *Knowledge Production*, *Liberal Democracy*, *and Human Nature* (New York: Palgrave Macmillan, 2012); Simone Turchetti and Peder Roberts, eds.,*The Surveillance Imperative*: *Geosciences during the Cold War and Beyond* (New York: Palgrave Macmillan, 2014).
5. Oreskes, "Science in the Origins of the Cold War," 11–29.
6. On the origins of the cognitive sciences see Erickson et al., *How Reason Almost Lost its Mind*: *The Strange Career of Cold War Rationality*; Cohen-Cole, *The Open Mind*: *Cold War Politics and the Sciences of Human Nature*; David C. Engerman, *Know Your Enemy*: *The Rise and Fall of America's Soviet Experts* (New York; Oxford: Oxford University Press, 2010).
7. See Isaac, *Working Knowledge*. An important recent analysis of conceptual problems with the notion of Cold War is also in: Joel Isaac and Duncan Bell, eds. *Uncertain Empire*: *American History and the Idea* of the *Cold War* (New York: Oxford University Press, 2012). For the discussion of the concept of "Cold War social science" see Mark Solovey, "Cold War Social Science," in Solovey and Cravens, eds, *Cold War Social Science*, 1–22.

8. For the discussion of the contested and polemical nature of the historical periodization see Isaac and Bell, eds. *Uncertain Empire*.
9. George Orwell, "You and Atomic Bomb," *Tribune*, 19 October 1945. See also the commentary by Odd Arne Westad in "The Cold War and the International History of the Twentieth Century," in M. P. Leffler and O. A. Westad, eds.,*The Cambridge History oftheCold War*, Vol. 1 (Cambridge: Cambridge University Press, 2010), 1–19, and Annette Vowinckel, Markus Payk, and Thomas Lindenberger, "European Cold War Culture(s)?" in A. Vowinckel, M. Payk, and T. Lindenberger, eds. *Cold War Cultures*: *Perspectives on Eastern and Western European Societies* (New York and Oxford: Berghahn Books, 2012), 1–20. For an alternative genealogy of the term see Anders Stephanson, "Cold War Degree Zero," in Isaac and Bell, eds.,*Uncertain Empire*, 19–49.
10. For detailed discussion of the ideological role of presenting the Soviet Union as a threat see John A. Thompson, "The Geopolitical Vision: The Myth of an Outmatched USA," in Isaac and Bell, eds. *Uncertain Empire*, 91–114. See also Muriel Blaive, "Utopian Visions: The 'Cold War' and its political aesthetics," *ZeihistorischeForschungen/Studies in Contemporary History* 5 (2008): 313–322.
11. Lorraine Daston, "The History of Science as European Self-Portraiture," *European Review* 14/4(2006): 523–536.
12. See John R.R. Christie, "The Development of the Historiography of Science," in R. C. Olby, G. N. Cantor, J. R. R. Christie, and M. J. S. Hodge, eds., *Companion to the History of Modern Science* (London and New York: Routledge, 1990), 5–22 and Peter Dear, "What is the History of Science the History Of? Early Modern Roots of the Ideology of Modern Science," *Isis* 96 (2005): 390–406.
13. See Geert J. Somsen, "A History of Universalism: Conceptions of the Internationality of Science from the Enlightenment to the Cold War," *Minerva* 46 (2008): 361–379.
14. For critical discussion of the invention of the notion of Scientific Revolution and its function as the major focus of the history of science see Andrew Cunningham and Perry Williams, "De-centering the 'Big Picture': 'The Origins of Modern Science' and the Modern Origins of Science," *British Journal for the History of Science* 26/4 (1993): 407–432. For the recent reappraisal see David Wootton, *The Invention of Science*: *A New History of The Scientific Revolution* (London: Allen Lane, 2015), 15–51.
15. Herbert Butterfield, *The Origins of Modern Science* (New York: Macmillan, 1949), 139.
16. See discussion in Somsen, "A History of Universalism," 361–379.
17. Ibidem, 372. See also Anna-K. Mayer, "*S*etting up a Discipline II: British History of Science and the 'End of Ideology'," *Studies in History and Philosophy of Science* 35 (2004): 41–72.

18. Anna-K. Mayer, *Roots of the History of Science in Britain, 1916–1950*. PhD Dissertation, Cambridge: University of Cambridge, 2003.
19. For USA see Michael Aaron Dennis, "Historiography of Science:An American Perspective," in John Krige and Dominique Pestre, eds. *Science in the Twentieth Century* (Amsterdam: Harwood Academic Publishers, 2003), 1–26. For British developments see Mayer, *Roots of the History of Science in Britain, 1916–1950*. For the discussion on the Soviet case see Elena Aronova, "The Politics and Contexts of Soviet Science Studies (Naukovedenie): Soviet Philosophy of Science at the Crossroads," *Studies in East European Thought* 63/3 (2011): 175–202.
20. See Mayer, *Roots of the History of Science in Britain, 1916–1950* (especially the Introduction). On Sarton see Lewis Pyenson and Christophe Verbruggen, "Ego and the International: The Modernist Circle of George Sarton," *Isis* 100/1 (2009): 60–78, and other articles in the Focus section of the same *Isis* issue devoted to Sarton. On the history of Bukharin's Institute in the early 1930s see Alexander Dmitriev, "InstitutIstoriiNaukiiTekhniki v 1932–1936 gg.," *Voprosyistoriiestestvoznanijaitekhniki* 1 (2002): 3–36; on its later transformations see Aronova, "The Politics and Contexts of Soviet Science Studies," 175–202.
21. On Marxist history of science see Robert M. Young, "Marxism and the History of Science," in Olby et al., eds.,*Companion to the History of Modern Science*, 77–86. On the Congress in London in 1931 see Loren Graham, "The Socio-Political Roots of Boris Hessen: Soviet Marxism and the History of Science," *Social Studies of Science* 15/4 (1985): 705–722; Gideon Freudenthal, "The Hessen-Grossman Thesis: An Attempt at Rehabilitation," *Perspectives on Science* 13/2 (2005): 166–193; Gideon Freudenthal and Peter McLaughlin, *The Social and Economic Roots of the Scientific Revolution* (Heidelberg: Springer, 2009), and Christopher A. J. Chilvers, "The Dilemma of Seditious Men: The Crowther-Hessen Correspondence in the 1930s," *British Journal for the History of Science* 36/4 (2003), 417–435.
22. See Gary Werskey, *The Visible College*: *A Collective Biography of British Scientists and Socialists of the 1930s* (New York: Holt, 1978). For a more recent account see Mary Jo Nye, *Michael Polanyi and his Generation*: *Origins of the Social Construction of Science* (Chicago, IL: University of Chicago Press, 2011).
23. Yehuda Elkana, "Alexandre Koyré: Between the History of Ideas and Sociology of Knowledge," *History and Technology* 4 (1987): 111–144. On Butterfield see Nick Jardine, "Whigs and Stories. Herbert Butterfield and the Historiography of Science," *History of Science* 41/2 (2003): 125–140.On the spread of "anti-Marxian ideology" among Cold War historians of science see Ursula Klein, "Kuhn in the Cold War," in Alexander Blum, KostasGavroglu,

ChristianJoas, and JürgenRenn, eds. *Shifting Paradigms*: *Thomas S. Kuhn and the Historyof Science* (Berlin: Open Access, 2016, in print), 115–121.

24. Somsen, "A History of Universalism," 373.On the spaces of "de-politicization" and "politicization" of the history of science and science studies in West and East see Elena Aronova, *Studies of Science Before* "*Science Studies*":*Cold War and the Politics of Science in the U.S.*, *U.K.*, *and U.S.S.R.*, *1950s–1970s.* PhD Dissertation, San Diego: University of California, 2012. See also an important article by Michael Aaron Dennis, "Historiography of Science: An American Perspective," in John Krige and Dominique Pestre, eds.,*Science in the Twentieth Century* (Amsterdam: Harwood, 1997), 1–26.
25. Ibidem.
26. One example of a synthetic account of the intellectual project of science studies that mentions the Cold War as merely a backdrop is John Zammito, *A Nice Derangement of Epistemes*: *Post-Positivism in the Study of Science from Quine to Latour* (Chicago: University of Chicago Press, 2004), chapter 5.
27. George Reisch, "The Paranoid Style in American History of Science," *Theoria. An International Journal for Theory*, *History and Foundations of Science* 75 (2012): 323–342.
28. See discussion in David A. Hollinger,"The Defense of Democracy and Robert K. Merton's Formulation of the Scientific Ethos," *Knowledge and Society* 4 (1983): 1–15. See also Jessica Wang, "Merton's Shadow: Perspectives on Science and Democracy," *Historical Studies in the Physical and Biological Sciences* 30 (1999): 279–306.
29. Elena Aronova, "Big science and 'Big science studies' in the United States and the Soviet Union during the Cold War," in Naomi Oreskes and John Krige, eds.,*Science and Technology in the Global Cold War* (Cambridge, MA: The MIT Press, 2014), 393–429, and Elena Aronova, "The Congress for Cultural Freedom, Minerva, and the Quest for Instituting 'Science Studies' in the age of Cold War," *Minerva* 50/3 (2012): 307–337.
30. On Polanyi see Mary Jo Nye, *Michael Polanyi and His Generation*: *Origins of the Social Construction of Science* (Chicago: University of Chicago Press, 2011).
31. See, for instance, Mayer, "*S*etting up a Discipline II: British History of Science and the 'End of Ideology'" and Nye, *Michael Polanyi and his Generation*.
32. See, for instance, Peter Romijn, Giles Scott-Smith, Joes Segal, eds. *Divided Dreamworlds*? *The Cultural Cold Warin East and West* (Amsterdam: Amsterdam University Press, 2012). For the discussion of how these visions were translated into science policy see John Connelly, *Captive University. The Sovietization of East German*, *Czech*, *and Polish. Higher*

Education, 1945–1956 (Chapel Hill: University of North Carolina Press, 2000); Kristie Macrakis and Dieter Hoffmann, eds.,*Science under Socialism: East Germany in Comparative Perspective* (Cambridge, MA: Harvard University Press, 1999); Natalia Tsvetkova, *Failure of American and Soviet Cultural Imperialism in German Universities, 1945–1990* (Leiden: Brill, 2013).

33. See Aronova, "The Politics and Contexts of Soviet Science Studies," 175–202.
34. See Sigrid Schmalzer, "Self-Reliant Science: The Impact of the Cold War on *Science* in Socialist China," in Oreskes and Krige, eds. *Science and Technology in the GlobalCold War*,75–106.
35. On the reception of Kuhn in the Soviet Union see Aronova, "The Politics and Contexts of Soviet Science Studies," 175–202.
36. See John Krige, *American Hegemony and the Postwar Reconstruction of Science in Europe* (Cambridge, MA: MIT Press, 2006). See also papers in the collections Oreskes and Krige, eds.,*Science and Technology in the GlobalCold War* and Turchetti and Roberts, eds.,*The Surveillance Imperative*. For an analysis of repercussions especially in the intelligence world see Ronald E. Doel, "Scientists, Secrecy, and Scientific Intelligence: The Challenges of International Science in Cold War America," in J. Van Dongen, F.Hoeneveld, and A.Streefland, *Cold War Science and the Transnational Circulation of Knowledge* (Leiden: Brill, 2015), 11–35. On a transnational history methodology in the history of science see the special issue of the *British Journal for the History of Science* 45/3 (2012) edited by S. Turchetti, NèstorHerran, and Soraya Boudia.

PART I

Science Studies in the "West"

CHAPTER 2

Telegrams and Paradigms: On Cold War Geopolitics and *The Structure of Scientific Revolutions*

George A. Reisch

In the summer of 1947, a young Thomas Kuhn was revolutionized and overcome by the intellectual excitement of what he would later call his "Aristotle experience." He had just decided to switch from physics to history of science—a far-reaching career decision made easier by an invitation from Harvard president James Bryant Conant to help teach history-based science courses in the university's new general education program. Conant, a chemist by training, needed Kuhn's expertise in physics to design case studies in dynamics and astronomy. The assignment brought Kuhn face-to-face with Aristotle and his "shock" (a word Kuhn would use often when describing the event) at realizing that "Aristotle seemed a very good physicist indeed, but of a sort I'd never dreamed possible."[1] Aristotle's physics was not wrong, as Kuhn had been led to believe by Whiggish, presentist histories of science and by his education as a physicist. Rather,

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he suddenly realized, Aristotle's physics had never really been understood by the historians and science educators who dismissed it.

Kuhn's career was deeply shaped by the Aristotle experience, the puzzles and problems it raised for him, and the solutions to them he finally offered in his famous book, *The Structure of Scientific Revolutions.* Reflecting on these puzzles in the early 1950s, when he first agreed to write a monograph for the *International Encyclopedia of Unified Science*, Kuhn was led to suppose that his scientific education had somehow blinded him to the possibility of alternative systems of physics like Aristotle's. One reason for this, as he would explain in *Structure*, was that scientific understanding and perception were controlled by "paradigms." To a community of scientists working under one paradigm, he wrote, those working under a competitor may seem to be "suddenly transported to another planet where familiar objects are seen in a different light and are joined by unfamiliar ones as well."[2] This space-age metaphor and the language of "paradigm shifts" helped make *Structure* a sensation inside and outside the academy and made Kuhn himself one of his generation's most important intellectuals.

A year before Kuhn's momentous encounter with Aristotle, another young, ambitious, and highly intelligent Ivy League graduate achieved a similar conceptual breakthrough. It concerned Stalin and the Russians. Until just a few years before, many Western intellectuals and elites admired the great Russian experiment in socialism, especially during the popular front of the 1930s when communists, socialists, and liberals stood united against Nazism and fascism in Italy and Spain. After Stalin's armies finally entered Berlin and subdued Hitler, the United States felt (as Conant himself put it) an enormous sense of "gratitude for the prowess of the armies which finally overwhelmed the Germans on the Eastern Front."[3] To many Americans, therefore, it came as a shock when postwar diplomatic relations with the Soviets became strained and the shared sense of purpose in defeating Hitler evaporated without residue. The distinctive ideals and values of the Kremlin, it seemed, had never really been understood by the historians and diplomats in question.

Some would say this was no revelation, just a reminder of how puzzling and worrisome the Soviets could be. To New York intellectuals like philosopher Sidney Hook, many of whom had originally set out to build careers as intellectual socialists, the diplomatic shock came earlier in the late 1930s, when Stalin began consolidating his power, murdering his rivals, and eliciting incredible confessions of counterrevolutionary treason from seasoned Bolsheviks. The *New York Times* characterized the show trials as a grim festival of irrationality and marveled at “how completely we must abnegate all reason in contemplating the somber record of the Soviet military courts in the last two years.” What was happening in Russia was literally unthinkable for Americans, it seemed, as if “twenty years after Yorktown somebody in power in Washington found it necessary for the safety of the state to send to the scaffold Thomas Jefferson, Madison, John Adams, Hamilton, Jay and most of their associates.”[4] In 1939, the Soviets shocked the West again by turning their back on the popular front and concluding a nonaggression pact with Hitler.

Russia was “a riddle wrapped in a mystery inside an enigma,” Winston Churchill had said as the war began. But after Hitler invaded Russia herself in June 1941, England, the United States, and the Soviet Union would support each other with matériel, intelligence, and open communication to defeat the common Nazi enemy. With success at hand in early 1945, Churchill, Roosevelt, and Stalin met at Yalta to negotiate the postwar political landscape of Europe and divide Germany into zones controlled by the Allies. Roosevelt came away believing that Stalin was a leader with whom he and the West could work. Conant agreed. He rallied his colleagues in science and government to support international controls on the new atomic technologies.[5] With Hitler put down, he reasoned, the new peace in Europe must not be corroded by distrust and suspicions over unequal stockpiles of weapons or bound-to-fail attempts to guard atomic secrets.

But Conant’s hopes for international collaboration did not last. By the time of the Potsdam Conference later in the year, Truman had succeeded Roosevelt and was much more suspicious of Stalin and his motives. When Truman personally informed him at the conference of the new weapon “of unusual destructive force” being developed, Stalin reacted nonchalantly and “showed no special interest”[6]—a reaction that raised questions about what Stalin understood or perhaps (thanks to spies) already knew. Overall, the division of Germany for postwar control among the Allies was proving contentious, and Soviet armies were not leaving the countries they had pledged to leave at Yalta, such as Poland and the Baltics. Stalin seemed to be eyeing Iran, Greece, and Turkey as fertile ground for cultivating communism, as well.

In February 1946, Stalin spoke at the Bolshoi theatre in Moscow and raised the State Department’s eyebrows even higher. The war had proved Russia’s economic and military strength, he said. Soviet scientists were ascendant, too. They would “not only catch up with but also surpass those abroad." The war itself, he added, "arose in reality as the inevitable result of the development of the world economic and political forces on the basis of monopoly capitalism."[7] Was Stalin really thumping his chest and blaming the recently concluded war on the United States, on Western "imperialism"? And in calling for unprecedented levels of industrial output—sufficient, he said, to "consider our country guaranteed against any eventuality"—was he anticipating another war for geopolitical supremacy?

Washington cabled the American embassy in Moscow asking for opinions and analyses of the speech. George Frost Kennan was then a 42-year-old diplomat stationed in the embassy. As a young appointee of Ambassador Averill Harriman, Kennan had helped establish the first American embassy in Moscow in the 1930s. Now, after other posts in Germany and Europe, he was back at the embassy. Knowledge about Russia seemed to flow in Kennan's blood. His grandfather's cousin, also named George Kennan, was an explorer and reporter who "did more than anyone else to shape the image of Siberia—and to a considerable extent that of tsarist Russia itself—as a prison of peoples." Theodore Roosevelt had leaned on the elder Kennan for information and advice about how to understand Russia. Some four decades later, when Stalin gave his puzzling speech, another Kennan was ready and eager to enlighten his superiors about this unique nation.[8]

Washington's cable in hand, Kennan figured that he'd make his superiors sit down and really think about the distinctive psychology that had grown up in the Soviet leaders.[9] It would take some time, too, for the solution to the riddle wrapped in a mystery inside an enigma was obviously not simple. "I cannot compress answers into single brief message without yielding to what I feel would be dangerous degree of over-simplification," Kennan's telegram began. But, he was sure he had some answers. "I hope, therefore, Dept. will bear with me …."[10]

In Washington, a State-Department teletype machine began to whirr and click, not stopping until 17 pages of type about the curious features of the Russian mind had overflowed the machine to curl up on the floor. Kennan's now-famous "long telegram" rang with compelling metaphors, clear language, and an impressive presence of mind. The riddle of the Russian mind, it seemed, had finally been solved. Secretary of the Navy James Forrestal found Kennan's analysis so convincing and important that he made copies and required everyone in the Truman cabinet to read it.[11]

Kuhn's achievement also brimmed with insight and overflowed its boundaries. When, at the end of the 1950s, he finally captured the insights of his Aristotle experience in his new language of paradigms, normal science, incommensurability, and revolutions, his manuscript was too long for the *International Encyclopedia*. But the monograph was so impressive the University of Chicago Press did not hesitate to publish it—both as an entry in the encyclopedia and as a standalone book.[12] In history and philosophy of science, it too rapidly became required reading.

Yet Kennan's long telegram and Kuhn's long monograph are comparable not only because they conquered their respective fields. They are substantially connected. On the one hand, they are connected by Conant, who taught Kuhn how to think about the history of science at the same time that he rubbed elbows with Kennan and other officers struggling to understand and manage the Soviets. They are also connected conceptually by a set of presumptions about the ways in which knowledge, human experience, and natural language interact to form coherent, independent ideologies and worldviews. These presumptions are evident in both Kennan's and Conant's early writings about the Soviet mind as well as in Kuhn's well-known arguments for "incommensurability" and the mutually exclusive qualities of paradigms. For these reasons, as Steve Fuller has argued, *The Structure of Scientific Revolutions* really is an "exemplary document of the cold war era."[13] But its exemplary qualities themselves overflow the student–mentor relationship between Kuhn and Conant that Fuller addresses in *Thomas Kuhn*: *A Philosophical History for Our Time*. They exist also in other aspects of the Cold War experience in America, such as debates and anxieties over ideological mind control and "brainwashing" (discussed in my "The Paranoid Style in American History of Science"[14]).

Here I shall sketch two other arenas in which these presuppositions simultaneously operated: the joint efforts by Washington, Cold War foundations, and American universities—specifically Conant and Kuhn's Harvard—to limn the puzzling inner workings of the communist mind, and Conant and Sidney Hook's campaign to educate American students about the nature and power of communist ideology. Though I have argued elsewhere that the Cold War in the United States deeply transformed American philosophy of science by effectively prohibiting certain kinds of research in the 1950s (such as Marxist philosophy and philosophical research inspired by Otto Neurath's unity of science movement),[15] this network of collaborations among Kennan, Conant, Kuhn, and Sidney Hook during the same years shows that new modes of science studies grew and thrived through their connections to these national and geopolitical concerns. Kuhn's *Structure* was in some ways transitional. Commissioned for the unity of science movement's encyclopedia in the early 1950s, the book turned out to be one of that movement's last gasps. On the other hand, *Structure* has long been praised for breathing new, exciting, historical air into logical empiricist philosophy of science. As much as Kuhn's revolution in science studies was inspired by his historical interests, however, it was shaped and guided by this surrounding discourse about the power of ideology to divide humanity into incompatible communities and by the urgent need for ordinary Americans as well as diplomats to understand the peculiar semantic mechanisms at work in the Soviet mind.

The Long Telegram

Kennan had discovered the fundamental "neurosis" and confusion that seemed to grip the Soviet view of the world. It was a neurosis revealed by history, by the particular Slavic experience of the world joined to the emergence of dialectical materialist orthodoxy. The telegram read,

> At bottom of Kremlin's neurotic view of world affairs is traditional and instinctive Russian sense of insecurity. Originally, this was insecurity of a peaceful agricultural people trying to live on vast exposed plain in neighborhood of fierce nomadic peoples. To this was added, as Russia came into contact with economically advanced west, fear of more competent more powerful, more highly organized societies in that area. But this latter type of insecurity was one which afflicted rather Russian rulers than Russian people; for Russian rulers have invariably sensed that their rule was relatively archaic in form, fragile and artificial in its psychological foundation, unable to stand comparison or contact with political systems of western countries.[16]

The dogmas of Marxism, Kennan explained, were suited for "this land which had never known a friendly neighbor or indeed any tolerant equilibrium of separate powers, either internal or international." After Marxism and its theory of class conflict had "smouldered ineffectively for half a century in Western Europe," it predictably "caught hold and blazed for the first time" in Russia, a land in which conflicts had never been peaceably surmounted.

> In this dogma, with its basic altruism of purpose, they found justification for their instinctive fear of outside world, for the dictatorship without which they did not know how to rule, for cruelties they did not dare not to inflict, for sacrifices they felt bound to demand … Today they cannot dispense with it. It is a fig leaf of their moral and intellectual respectability … This is why

> Soviet purposes must always be solemnly clothed in trappings of Marxism, and why no one should underrate importance of dogma in Soviet affairs.[17]

Having embraced Marxism to soothe and rationalize their insecurities, Soviet leaders were now controlled by it. They routinely engaged in a kind of "self-hypnotism," Kennan wrote, leading them to "believe what they find it comforting and convenient to believe." Their understanding

> is seemingly inaccessible to considerations of reality in its basic reactions. For it, the vast fund of objective human fact about human society is not, as with us, the measure against which outlook is constantly being tested and re-formed, but a grab bag from which individual items are selected arbitrarily and tendentiously to bolster an outlook already preconceived.[18]

Considering the tremendous size of the country, resources, and determination of Moscow to extend communism as a geographical buffer against the encroachments of capitalism, this diagnosis, Kennan admitted, was "not a pleasant picture."[19]

Yet, there was hope, Kennan explained. The first task was to understand better the mindset of the Soviet leaders. "We must study it with the same courage, detachment, and objectivity, and same determination not to be emotionally provoked or unseated by it, with which doctor studies unruly and unreasonable individual."[20] The medical metaphor would soon become commonly used by figures like FBI Director J. Edgar Hoover and Senator Joseph McCarthy to justify their anticommunism. Kennan used it to ask the public and its leaders to shoulder responsibility:

> Much depends on health and vigor of our own society. World communism is like malignant parasite which feeds only on diseased tissue. This is point at which domestic and foreign politics meet. Every courageous and incisive measure to solve internal problems of our own society, to improve self confidence, discipline, morale and community spirit of our own people, is a diplomatic victory over Moscow worth a thousand diplomatic notes and joint communiqués.[21]

Conant and many other postwar intellectuals agreed that America's growth was creating ever more pressure to solve these "internal problems" surrounding civil rights, equality of opportunity, and persistent poverty. Left unsolved, these problems could promote sympathy with communism in the United States. In solving these problems at home, however, the nation would help itself geopolitically, as well. American prosperity and happiness would cast doubt on communist propaganda about American inequality, racism, or poor educational standards. Nonaligned nations would increasingly cast their lot with the United States instead of the Soviet Union.

The X Article

As Kennan and his insights became the talk of Washington insiders, the editor of the widely read journal *Foreign Affairs* asked him to share his new understanding of the Russians with a larger public. Kennan was willing but decided to appear anonymously as "Mr. X" because he currently worked at the State Department.[22] It did not take long for viewers and commentators—in the *New York Times*, for example—to determine who X was. Soon, excerpts appeared in popular magazines under Kennan's name and—though it would be only for several years—Kennan himself became revered among his peers as the guiding architect of American Cold War policy.

Like the long telegram, the X article began with the insight that "Soviet power as we know it today is the product of ideology and circumstances: ideology inherited by the present Soviet leaders from the movement in which they had their political origin, and circumstances of the power which they have now exercised for nearly three decades in Russia." The task at hand, Kennan explained, was to undertake a kind of "psychological analysis"[23] to understand how these two elements have combined to form the Soviet Union of today.

What had to be grasped, Kennan explained, was the worldview of the Soviet leaders. It embraced the historical incompatibility of communism and capitalism and the inevitable victory of communism. It prized unity and conformity and employed all the methods of dictatorship to tamp down dissent. And it involved a deep, functional devotion to Marxism that resulted in "an unshakable stubbornness and steadfastness" in the Kremlin, its allied nations, and among the membership and leaders of the Communist Party—"the whole subordinate apparatus of Soviet Power":

> Once a given party line has been laid down on a given issue of current policy, the whole Soviet governmental machine, including the mechanism of diplomacy, moves inexorably along the prescribed path, like a persistent toy automobile wound up and headed in a given direction, stopping only when it meets with some unanswerable force.[24]

The word “brainwashing” would not be coined for another few years. But Kennan nonetheless portrayed Soviet leaders as ideologically controlled, dogmatic, unable to freely and critically inspect their beliefs, and unable to be liberated from their mental captivity by reason or argument:

> The individuals who are the components of this machine are unamenable to argument or reason which comes to them from outside sources. Their whole training has taught them to mistrust and discount the glib persuasiveness of the outside world. Like the white dog before the phonograph, they hear only the ‘master’s voice’. And if they are to be called off from the purposes last dictated to them, it is the master who must call them off. Thus the foreign representative cannot hope that his words will make any impression on them. The most that he can hope is that they will be transmitted to those at the top, who are capable of changing the party line. But even those are not likely to be swayed by any normal logic in the words of the bourgeois representative. Since there can be no appeal to common purposes, there can be no appeal to common mental approaches.[25]

Both the ordinary Communist Party members at the bottom and their leaders in the Kremlin “at the top” lived in something like an ideological dreamworld. Here, “truth is not a constant but is actually created, for all intents and purposes, by the Soviet leaders themselves” who interpret for their underlings and the masses “the logic of history” that remains invariably friendly to Soviet goals and values.[26]

Conant, Kennan, and the Logic of Containment

Kennan’s lasting diplomatic fame lay in his third way to handle the Soviets—not by waging preemptive war, and not by doing nothing. “The United States cannot expect in the foreseeable future to enjoy political intimacy with the Soviet regime. It must continue to regard the Soviet Union as a rival, not a partner, in the political arena.”[27] While it may not attack this rival, it must prevent the Kremlin’s puppets and toy automobiles from spreading communist doctrine and government wherever they wish:

> Soviet pressure against the free institutions of the western world is something that can be contained by the adroit and vigilant application of counterforce at a series of constantly shifting geographical and political points, corresponding to the shifts and manoeuvres of Soviet polity, but which cannot be charmed or talked out of existence.[28]

The Soviets and their "expansive tendencies" are here to stay. But those tendencies can be checked, he wrote, by "a policy of firm containment, designed to confront the Russians with unalterable counter-force at every point where they show signs of encroaching upon the interests of a peaceful and stable world."[29] What exactly Kennan meant by "containment" and "counter-force" would itself become controversial, but his proposal was taken up in the National Security Council Report 68 (NSC-68), a blueprint for the "containment" of communism for the duration of the Cold War.

Conant shared Kennan's historical and psychological approach to the new geopolitical realities and recommended many of the same courses of action. His book of 1948, *Education in a Divided World*, echoed Kennan's picture of the Soviet Union as a self-hypnotized world unto itself, cleaved linguistically and semantically from the West. Conant also insisted that American citizens, no less than diplomats and policymakers, must understand at least something about the curious logic of this world—both to blunt the alluring promises and exaggerated claims of Marxist fanatics and to avoid the catastrophic risks of military posturing. If the specter of atomic devastation did not tell against those who "advocate of an attack on Russia by the United States," Conant wrote, then surely the predictable aftermath of a successful attack did: how possibly—both Conant and Kennan asked their readers—could the USA manage to successfully occupy and rehabilitate a vanquished Soviet Union? "Can anyone imagine our occupying and policing the vast country for even a few months, let alone a period of years?" Conant asked.[30] "I think military authorities would agree," Kennan later wrote, "that this is not technically feasible even if it were worth one's while to make the staggering effort."[31]

Barring the disaster of a third World War, Conant and Kennan envisioned the same endgame for the standoff. Containing communism, Kennan explained, did not mean simply "holding the line and hoping for the best."[32] For the Soviet leaders were not *fully* isolated from the realities of the world around them. They remained "keen judges of human psychology," especially when it came to propaganda and their hope to make the communist East shine more brightly than the allegedly corrupt, capitalist West. "Facts speak louder than words to the ears of the Kremlin,"[33] Kennan wrote. This was an opening America could exploit if it could

> create among the peoples of the world generally the impression of a country which knows what it wants, which is coping successfully with the problems

> of its internal life and with the responsibilities of a World Power, and which has a spiritual vitality capable of holding its own among the major ideological currents of the time.

Success and unity in America, in other words, would give the lie to the alleged "palsied decrepitude of the capitalist world" that is "the keystone of communist philosophy."

> Even the failure of the United States to experience the early economic depression which the ravens of the Red Square have been predicting with such complacent confidence since hostilities ceased [in 1945] would have deep and important repercussions throughout the Communist world.[34]

Conant was on the very same page: Americans must demonstrate "that there is in fact a strong and vigorous rival to the Soviet views" and show—admittedly not attempt to persuade—the Russian leaders that their confidence in the corruption and fragility of capitalism was misplaced. "What can convince the Soviet leaders, some may ask? Not words but facts," he explained: "the stubborn fact of the successful leadership of the United States among the non-communistic nations."[35]

As proud and competitive Americans, Conant and Kennan even relished the ideological contest at hand. Despite its dangers, they knew this was a historical chance for liberalism to prove itself in a global setting. "The thoughtful observer of Russian-American relations," Kennan wrote, "will find no cause for complaint in the Kremlin's challenge to American society."

> He will rather experience a certain gratitude to a Providence which, by providing the American people with this implacable challenge, has made their entire security as a nation dependent on their pulling themselves together and accepting the responsibilities of moral and political leadership that history plainly intended them to bear.[36]

Conant also called for educators and "all thoughtful citizens" to band together under liberalism and demonstrate that the USA, "this vast instrument of democracy, can be made responsive to the needs of a free nation in a divided world."[37] "Who could ask for more," he asked at the end of his book, "than to be given an opportunity to live at a time when such possibilities lie ahead?"[38]

However historic the opportunity, both Conant and Kennan knew that the situation could be ruined by those who lacked the patience to let liberalism prevail in the long run, and driven by national emotions and insecurities, would push toward a destabilizing military buildup. Conant himself would take that road in the coming years. But in 1948, shortly after Kennan's X article appeared, he tried to persuade his readers away from it, writing that "nothing short of the worst fears of the alarmists can vitiate the endeavors of the citizens of this country to bring our society nearer to our historic goals."[39] Kennan closed his X article by urging the nation to "measure up to its own best traditions." In his long telegram, he had inadvertently ended on another Conantian note—one inspired by the poet Archibald Macleish, whose interventionist poem of 1937 "Speech to the Scholars" (O scholars schooled upon the books/… Rise from your labor now! Enlist) had pressed Conant's liberal buttons the wrong way. Indeed, scholars must not enlist, Conant thundered in private correspondence with Macleish and in his baccalaureate sermon to undergraduates that year. "It is easy," Conant told Macleish, "to lose the very things one wishes to preserve by declaring war in favor of them."[40] What the nation must preserve now, above all, Conant explained, was its liberalism, pluralism, and traditions of scholarship and learning. These distinguished the culture of the West from Stalin's authoritarianism and offered a way forward through the new Cold War. Kennan too warned his State Department superiors that the nation must not become some mirror image of the Soviets. It must not meet Soviet expansionism with Western expansionism, or meet Soviet brutality and authoritarianism with a home-grown counterpart. "The greatest danger that can befall us in coping with the danger of soviet communism," he wrote at the close of his telegram, "is that we shall allow ourselves to become like those with whom we are coping."[41]

National Security Council Report 68

In the autumn of 1947, Conant visited Harvard alumni clubs around the USA, drumming up support for Truman's Marshall Plan to economically fortify Europe and to warn Americans away from alarmist overreaction to the puzzling behavior of the Soviets. An international agreement for the control of atomic energy, he told an audience in San Francisco, "might be the 'just the step required' to convert an 'armed truce' into peace." Such an agreement could be a point at which the Western mind and Soviet mind might meet—just as they had met before over the task of defeating Hitler. The ominous sense of mistrust and unpredictability many now felt toward the Soviets, even the very "idea of an impenetrable iron curtain," Conant explained, might begin "to fade."[42]

Whether or not Conant knew who its author was (though he probably did), he quoted the celebrated "Mr. X" article that challenged the nation to become a society "capable of holding its own among the major ideological currents of the time."[43] In the State Department circles they both moved in—Conant as a member of the Atomic Energy Commission's General Advisory Counsel and Kennan as the celebrated diplomat who had figured out the Russians—they counseled against building up stockpiles of atomic weapons outside of an internationally established framework and against the viability of waging a "preventive war." Conant's arguments caught the ear of Secretary of Defense Forrestal (who had pulled strings to get Kennan's article published in *Foreign Affairs*), who now made phone calls to *Atlantic Monthly* to get one of Conant's speeches on the subject published. To the National War College in the fall of 1948, Conant urged that "for us to develop a Machiavellian foreign policy culminating in our launching a surprise attack on the Soviet Union or declaring war for the sole purpose of waging destruction would negate the very premise on which our culture rests."[44]

A year later, Kennan and Conant each tried to dissuade the Truman administration from pursuing the new thermonuclear "super" bomb. In January 1950, Conant made his case over lunch with Truman's Secretary of Defense Dean Acheson. In April, he met with Kennan, Paul Nitze, and other writers of NSC-68 to argue against the view that the Soviets were eager to invade and conquer western Europe or the USA.[45] He specifically warned Nitze, the principal author, that any plans to "roll back" Soviet control of Eastern Europe would necessarily invite a new world war along with planetary devastation.[46]

By the end of 1950, however, the tumult of the early Cold War persuaded Conant to reverse himself on some of these positions. First, the trial of Archibald Hiss convinced Conant that hysterical claims about communists in Washington were not just signs of a political witch hunt. Then, a series of telling surprises and shocks—the Soviet development of the atom bomb (far sooner than Conant anticipated), the communist revolution in China, and the onset of the fighting in Korea—convinced Conant that he himself had misconstrued what he earlier described as an ideological rivalry. He now saw it as a Soviet quest for military world domination—one that could lead to nuclear disaster if the nation did not follow NSC-68's recommendations to build its military might and station troops strategically around the world to contain and frustrate the Kremlin's actions.[47]

Kennan, however, stood firm and insisted that NSC 68 failed to understand the Russians and what his proposal for containment properly meant. Years later he saw it as a simple but dangerous, if not tragic, misunderstanding:

> …all came down to one sentence in the "X" Article where I said that wherever these people, meaning the Soviet leadership, confronted us with dangerous hostility anywhere in the world, we should do everything possible to contain it and not let them expand any further. I should have explained that I didn't suspect them of any desire to launch an attack on us. This was right after the war, and it was absurd to suppose that they were going to turn around and attack the United States. I didn't think I needed to explain that, but I obviously should have done it.[48]

Nitze and his fellow authors of the report saw no misunderstanding. They simply rejected Kennan's recommendations on the basis of their consensus, to which Conant now subscribed, that Kennan had underestimated the Soviet military threat. While it may be true that "the objectives of a free society are determined by its fundamental values," the report explained, the USA may not hope to win the Cold War solely by cultivating its freedom and internal vigor. It must also protect and maintain "the material environment in which [those values] flourish." As if wagging a finger at Kennan's naïveté, NSC-68 maintained that "Logically and in fact, therefore, the Kremlin's challenge to the United States is directed not only to our values but to our physical capacity to protect their environment."[49] It therefore called for the defense of American interests through the use of "force"—a word Kennan himself had used—in the form of thermonuclear weapons, increased stockpiles of atomic weapons, and "greatly increased general air, ground, and sea strength, and increased air defense and civilian defense programs."[50]

Having rejected the new consensus, Kennan drifted from the corridors of power in Washington. Given his renown and expertise about communism, as well as his knowledge of Germany—besides diplomatic posts in Hamburg and Berlin, Kennan had been interned in Germany for six months when Germany declared war on the USA[51]—some expected that he would become the high commissioner when the job became open in 1953. But Eisenhower's top pick for the job was Conant.[52] Despite a short-lived ambassadorship to the Soviet Union (in 1952) and later Yugoslavia in the early 1960s, Kennan worked increasingly outside of Washington in academic settings where his criticisms of Cold War policy were more welcome. In the early 1950s he joined forces with anticommunist intellectuals in the CIA-funded *Congress for Cultural Freedom* and, by the end of the decade, took a permanent post at the Institute for Advanced Study in Princeton, New Jersey, then headed by Conant's former chief physicist J. Robert Oppenheimer.

When in 1957, he delivered the prestigious Reith lectures in Great Britain, Kennan controversially argued that the Western Allies should withdraw their military presence from Europe altogether, including, of course, Conant's Germany. The nascent student movement was impressed and intrigued. The Student League for Industrial Democracy (SLID) (forerunner of Tom Hayden's Students for Democratic Society) read Kennan's lectures and discussed his proposals with keen interest.[53] Washington and cold warriors like Conant, however, were aghast. In *Time* magazine, Dean Acheson, who as secretary of state under Truman was Kennan's former boss, dismissed not only Kennan's arguments for reducing nuclear tensions but his entire career: "Mr. Kennan has never, in my judgment, grasped the realities of power relationships, but takes a rather mystical attitude toward them. To Mr. Kennan there is no Soviet military threat in Europe."[54] Conant also joined the anti-Kennan chorus by lecturing on campus "against some of Kennan's ideas," *The Crimson* reported. Two weeks later Conant joined a dozen other signatories who, the *New York Times* reported, "assailed the recent proposals of George F. Kennan for the neutralization of Europe."[55]

The Kennan–Conant Consensus

By the end of the 1950s, Kennan no longer agreed fully with Conant and other American cold warriors about how to handle the Soviet threat. But there was little disagreement about the basic, ideological forces that had so divided the world. The Soviets, Kennan had always pointed out, insisted on the ultimate incompatibility of communism and Western capitalism, and the prediction that only one kind of economic system could survive by dominating the globe. NSC-68 emphatically agreed. The two superpowers were deeply incompatible, polar opposites. Of the "Soviet system," it explained,

> No other value system is so wholly irreconcilable with ours, so implacable in its purpose to destroy ours, so capable of turning to its own uses the most dangerous and divisive trends in our own society, no other so skillfully and powerfully evokes the elements of irrationality in human nature everywhere, and no other has the support of a great and growing center of military power.[56]

This was new and unprecedented, the report said. Echoing Conant's own language about the "fanatical faith" of communists in the Kremlin, the report claimed that "unlike other previous aspirants to hegemony" on the world stage, the USSR "is animated by a new fanatic faith, antithetical to our own, and seeks to impose its absolute authority over the rest of the world."[57]

While the growing military industrial complex would take care of the military end of NSC-68's agenda, it remained urgent that diplomats and scholars continue the task that Conant had helped Kennan pioneer—the attempt to decode, understand, and if possible learn to predict events in the peculiar "mental world the Soviet leaders,"[58] as Kennan had put it. In his Reith lectures, Kennan argued that time had not been friendly to the Soviet mind—as if the Kremlin and its inhabitants had drifted far from earth:

> They view us as one might view the inhabitants of another planet through a very powerful telescope. Everything is visible; one sees in the greatest detail the strange beings of that other world going about their daily business; one can even discern the nature of their undertakings; but what one does not see and cannot see is the motivation that drives them on their various pursuits. This remains concealed; and thus the entire image, clear and intelligible in detail, becomes incomprehensible in its totality.[59]

The Soviets had become their own victims "of the abuse they have practiced for so long on the freedom of the mind." It seemed that

> their habitual carelessness about the truth has tended to obliterate in their minds the distinction between what they do believe and what they find it merely useful to say. It would be easier for us if they either believed things entirely or spoke them in utter cynicism. In either case, we would know

> where we stood. As it is, our problem is very difficult indeed; for we can never know, when we encounter their statesments and reactions, whether we have to do with the substructure of sincerely held error which does exist in their minds or with the superstructure of contrived and deliberately cultivated untruth to which they are so committed.

"What we are confronted with here," Kennan added, "is not just misunderstanding, not just honest error, but a habit of mind, an induced state, a condition."[60]

Still, Kennan insisted, that condition was more semantic than pathological or genocidal. "Problematical as I believe the psychology of the Soviet leaders to be," he remarked, "I cannot warn too strongly against the quick assumption that there is no kernel of sincerity" in their communications or overtures for peace. Perceived insincerity, he suggested, was likely the result of the different, clashing ideological frameworks in play:

> Their idea of peace is of course not the same as ours. There will be many things we shall have to discuss with them about the meaning of this term before we can agree on very much else. But I see no reason for believing that there are not, even in Moscow's interpretation of this ambiguous word, elements more helpful to us all than the implications of the weapons race in which we are now caught up.[61]

Three years later, in his book *Russia and the West under Lenin and Stalin*, Kennan continued to probe the semantic confusions that confounded and confused relations with the Soviets. Under the title "Conflict of the Two Worlds," Kennan argued that miscommunication and misunderstanding had marked Soviet relations since the World War I and the Russian Revolution, when Russians and Western leaders failed to understand the pressing realities each faced:

> I think it important that you should have clearly in mind … the image of these two groups of men—the Russian Communist leaders on the one hand, and the responsible Western statesmen on the other—each preoccupied with a different issue, each moving earnestly forward in pursuit of its particular goal, each to some extent deceived as to the feasibility of that goal and the values implicit in it, each nevertheless endowed with a sense of total self-righteousness, having no understanding or respect or tolerance for the issue that preoccupied the other.[62]

The "emotional mental states into which people had now worked themselves," Kennan argued, had led Westerners to mistakenly believe they were suddenly locked in an epic, ideological battle, "a struggle between democracy and autocracy":

> Only this explains the curious kind of exasperation both sides inflicted on each other in 1917 and 1918, by their insistence on talking about the wrong things. People simply talked past each other. The contacts between the two sides, for this reason, tended to have a distracted absent-minded quality. The words they addressed to each other were usually shining examples of irrelevance.[63]

All this history had been rewritten, though, in Soviet historiography. "If you read the Soviet historical material of the present day," Kennan wrote, "you will find the Soviet historian very concerned to persuade you that it was not this way at all." Instead of the West and East engaged with the many complexities of World War I, it will be said that the West

> had no greater concern than the challenge raised for it by the Russian Revolution—that it was wholly absorbed with the problem of how it could counter and destroy this tremendous force of justice and truth, namely the Russian Communist movement, which had just appeared on the world horizon and was threatening to destroy everything dear to the selfish interests of world capital.[64]

The curious "mental world" of the soviet mind, Kennan believed, continued to lie at the center of the Cold War. It dominated not only international relations but, through the ideological tilt of Russian historiography, the Soviet mind's very understanding of itself and its place in the world.[65]

The Russian Research Center

Conant too embraced these metaphors of miscommunication and misunderstanding with the Soviets. In *Education in a Divided World*, he acknowledged the diplomatic land mines the ideological clash had buried. Could it be, he asked, that some Soviets and their advocates are simply lying when they describe their totalitarian system as "democratic" and point their fingers at the capitalist West for destroying human "freedom"? Or, more likely, have the different ideologies that now divide the world scrambled meanings so that common words "like 'freedom', 'democracy', even 'truth' and 'beauty' have entirely different overtones for the two groups" and cleaved the world into "two separate 'universes of discourse'?"

> Without a better understanding of the way Russian rulers think—'how they are wired,' as one American delegate who argued daily with them has said—without a better knowledge of Soviet philosophy and an accurate estimate of its hold on individuals, we are shadow-boxing in many areas.[66]

The delegate in question was most likely General Frederick Osborn, whom Conant knew from both his war work and as a trustee of the Carnegie Corporation, a longtime supporter of Harvard. In 1947, Osborn was head of the American delegation at the United Nations, where early post-war attempts to hammer out an agreement for international control of atomic energy were frustrated by the puzzling, enigmatic reasoning in the Russian delegation. Having enlisted Conant to join a group of outside experts to advise his delegation, Osborn had his ear to vent his frustration about the Russians—particularly Andrei Gromyko, the Soviet deputy foreign minister, who over the course of negotiations in 1947 and 1948 would alternately tantalize, confuse, and puzzle the American delegation (and then tell the press that his American counterparts were to blame for the lack of diplomatic progress).[67]

What was needed, Osborn told Conant, was a "massive scholarly effort to probe Soviet behavior patterns."[68] Through his Carnegie contacts, Osborn had the money; and Conant had the institution and the intellectual talent. Together they established the Russian Research Center at Harvard. According to Carnegie Foundation documents, the center would bring "social psychology, cultural anthropology, and sociology to Russian studies." Experts at the Center would analyze Russia on a "day-to-day" basis along with the rest of the Western world; but its scholars would also more intensively study "attitudes of Russians toward their homeland in relation to the rest of the world," their "attitudes toward Authority, Hierarchy, Suppression of Individual Freedom," and especially the distinctive mind-set of that strange creature, "the Communist Bureaucrat."[69]

"What parts of that ideology are readily assimilated because they simply provide new verbal symbols for old value-systems" and "what parts are unassimilable and remain mere words"?[70] What conditions must be met for the Soviets to appear intelligible and reasonable? What causes simple translations of language to fail so that diplomats end up talking past each other? Carnegie documents outlining the future Center cited

Kennan's X article for inspiration and echoed Conant's remarks about the "separate universes of discourse" that seemed to be at play.

Originally envisioned as a three-year research project, the Center became an enduring Cold War institution. Leading scholars conducted research while government intelligence agencies welcomed "a new, high-powered source of experts and expertise to supplement and collaborate with their own efforts to understand their cold-war adversary."[71] Coverage of the new Center in *The Crimson* repeatedly cited director Clyde Kluckhohn's mission statement—to study "Russian institutions and behavior in an effort to determine the mainsprings of international actions and policy of the Soviet Union."[72] Early publications of the Center addressed the Soviet "national consciousness" and (the title of one of Alex Inkeles' books) *Public Opinion in Soviet Russia: A Study in Mass Persuasion*. The Center's reason-to-be, by outward appearances as well as its founding impulse, was to make sense of the Soviets' strange (and dangerous) behavior and to better understand the ideological and authoritarian mechanisms that instilled and maintained the Soviet mindset among its citizens. A review of Inkeles' book appeared in the *New York Times Sunday Book Review* under the title "The Making of the Russian Mind." The accompanying photograph showed a handful Russian schoolchildren huddled around a composition book while a painting of Lenin teaching a schoolgirl to read hung on the wall behind them. The caption read, "Russian school children study under the eye of Lenin."[73]

A Russian Research Center in Every Neighborhood

The Russian Research Center combined Conant's wish to assist diplomats and military planners in their efforts to keep communism under control with his goal to educate Americans about the nature of communism. Harvard's Center could be a flagship in this effort, its scholarly wisdom trickling down into the larger public in the same way that Harvard's new General Education program would blaze a path for the rest of the nation's colleges and high schools to follow. In *Education in a Divided World,* he called for educators in colleges and universities to teach what was known so far about the Soviet ideology that so captivated its leaders. To defuse one predictable response, Conant used the metaphor of disease to make sure he was properly understood: "Studying a philosophy does not mean endorsing it, much less proclaiming it," he wrote. "We study cancer in order to learn how to defeat it. We must study Soviet philosophy in our universities for exactly the same reason."[74]

Sidney Hook, the staunch anticommunist New York philosopher, could not agree more. He enthusiastically reviewed Conant's book in the *New York Times* and praised Conant as the rare intellectual who understood the power and threat of Soviet ideology. Indeed Conant was correct, Hook wrote, that managing the new Cold War "entails a close study of the philosophy behind Soviet expansion and infiltration—which he would require of all students as soon as they are old enough to understand it."[75] Hook himself had built his career on the understanding of Soviet ideology—first as a sympathetic expert on Marxism aiming to synthesize Marx and American pragmatism and later, after his own political conversion in the late 1930s, as an expert on the mendacious, conspiratorial agenda of party communism.[76] Hook's review commenced a largely tacit but powerful alliance between Hook and Conant—well-known authorities on education and anticommunism in New York City and Boston, respectively—that helped cement the national consensus that communist teachers were unfit to teach. Controlled by their ideology and the ever-changing "Party line" from the Kremlin, the consensus held, communist professors and teachers either used (or were likely to use, Hook insisted) their classrooms, lectures, and lesson plans deceptively to indoctrinate students and advance international communism.[77] As Ellen Schrecker has documented in her book *No Ivory Tower*, the consensus enabled anticommunist trustees, administrators, and politicians to effectively purge party communists from higher education. From the mid-1950s until the student movement and civil rights movements began to gain momentum in the 1960s, communism could hardly be found on American campuses. With highly publicized investigations having taken place at Harvard, Berkeley, and other campuses, the American academy had joined Conant's, Kennan's, and Hook's anticommunist consensus.

The academy's victory over communism, however, did not mean that communism could—or should—be ignored in schools. No longer an article of belief, it remained essential as subject matter. Both Hook and Conant continued to rally the nation for education about communism in the nation's colleges and high schools. In 1954, a year after Conant had assumed his post in Germany, Hook took up the issue in *The New York Times Sunday Magazine* and used Conant as a springboard for making his case. "Some years back," Hook wrote, referring to his own review of *Education in a Divided World*, "Dr. James B. Conant, then president of Harvard University, suggested that so long as we live in a divided world, teaching not of but about communism should be made an integral part of all liberal arts college courses." After all, Hook noted using the popular metaphor, "no one can be infected by the study of cancer or venereal disease."[78] Not content to declare victory over communism in the American academy, Hook wanted to lay into the offending ideology—to expose and dissect it publicly in front of students, to render it powerless through analysis. To an ideology that secured power over its victims by evading scrutiny, by silently controlling their thoughts and reactions, there could be no greater insult than being so dragged into the intellectual light of day. A bold-faced pull-quote appearing with the article put it succinctly: "Hatred and fear of the doctrine are not enough, says an educator. We must know what it really is in order best to combat it."

The study of Russian communism is already underway "in almost every high school and college of the nation as part of existing courses," Hook noted. But it must now "be made explicit and systematic rather than incidental to other themes." For each student, he explained, this means "giving more thought to it, either as an independent study or as part of existing courses in democracy or government or the social science."[79] "It is of the first importance," Hook asserted, "in discovering what the leaders of the Communist movement, the general staff of the greatest mass movement in history, really think, how they intend to get it, and at whose cost."

Like the research being undertaken at the Russian Research Center, Hook envisioned interdisciplinary study in American schools. How was it, exactly, that communist ideas were able to so effectively subvert critical thinking and lead masses of people into conceptual slavery? One key, Hook argued, lay in language and the manner in which the Soviet mind twisted and exploited words and concepts to both subvert the mind's critical faculties and snare other, uninfected minds into communism's conspiratorial web. To meet this need, Hook recommended the development of specialized textbooks. But he admitted that "wherever possible the primary source material should be official statements of communists themselves" to which each course should devote attention to "semantic analysis." Following Kennan and Conant, Hook described modern Marxism as a carnival of semantic corruption:

> Most of its key concepts like 'ruling class,' 'proletariat,' 'dictatorship,' 'the state' as well as its propagandistic slogans like 'peace,' 'freedom-loving' and 'imperialist' are systematically ambiguous. Its central propositions, like the oft-proclaimed concept of 'the withering away' of the state, are either disguised definitions effected by a violent abuse of language, or pious resolutions to interpret events.

"A whole armory of intellectual tools," Hook enthused, was waiting to be enlisted in the continuing fight. They must be "brought to bear and sharpened in a painstaking analysis of communist theory."[80]

Conant resigned the presidency of Harvard the year before to become high commissioner (and then ambassador) to Germany. One of his first post-diplomatic engagements in 1957, however, was a summer school conference at Harvard dedicated to "Teaching the Nature of Communism." He opened the conference with a lecture titled "On Understanding Communism" and closed it three days later with "Education in Europe and the United States." During the conference, he moderated a panel discussion on "The American Philosophy of Education and the Problem of Dealing with the Nature of Communism in the Curriculum." He offered a three-pronged approach, addressing the nature of the totalitarian state, communist education, and the philosophy of Marxism-Leninism. "The Marxist Doctrine," Conant elaborated, consisted of three sub-parts:

> Dialectical materialism, which is a 'world philosophy' that 'answers everything'; historical materialism, which is claimed to be an inevitable result of dialectical materialism; and the Marxist analysis of the capitalistic structure.

The analysis followed *Education in a Divided World*. But having just lived in Germany, Conant's popular writings about the "all-embracing" qualities of Soviet philosophy in the late 1950s and early 1960s were supported and enlivened by anecdotes and personal observations. In some cases, he would report during the days of the conference, there were glimmers of good news: signs that communist "indoctrination" was sometimes failing to take hold in the satellite nations where nationalist sentiments remained strong among citizens.[81]

The Promise (and Problems) of Paradigms

One danger of communist ideology, Conant, Hook, and Kennan agreed, was precisely its power to "answer everything" and thus captivate the mind of the true believer. By studying communism in American schools—led by teachers, of course, who had not themselves succumbed to it—students would become less susceptible to its alluring worldview, less inclined to dogmatism, and more appreciative of the pluralism that comes with democracy and the cultural freedoms denied in the communist East. As Kennan had put it at the end of his long telegram, "We must see that our public is educated to realities of Russian situation."

Embarking on his new career as a historian of science in the late 1940s and early 1950s—he was invited to write a monograph for the *International Encyclopedia*, it appears, in late 1952—Kuhn was surrounded by this national anticommunist agenda. He worked closely with Conant in the General Education program from 1947 to 1950, when Conant returned to full-time administration and left Natural Science 4 and other General Education courses in Kuhn's and others' hands. The two continued to correspond and maintain their collaboration and friendship after Conant left Harvard for Germany and Kuhn left for Berkeley (Conant wrote a foreword, for example, to Kuhn's first book, *The Copernican Revolution*, and Kuhn dedicated *Structure* "To James B. Conant, who started it."). Kuhn's conversion from physics to history of science thus coincided with the nation's conversion to Conant's anticommunist consensus—a consensus that offered Kuhn conceptual resources as he built his new theories of science and scientific revelations in the wake of his Aristotelian revelation. Of course, Kuhn also consulted texts in psychology, philosophy, history, and sociology. But the world of science and its history that he depicted was just like this world of geopolitics. It too was divided by opaque, mutually exclusive worldviews propped up by the psychology of human perception (Kuhn cited research such as Jerome Bruner's and Leon Postman's experiments using anomalous playing cards) and by the semantics of language. Different scientific paradigms control the meanings of words, Kuhn explained, so much so that different theoretical programs amount to "incommensurable ways of seeing the world and practicing science in it." Disputants may therefore often "talk through each other" when defending their very different commitments.[82] Along with the metaphor of "different planets," this imagery and language of ideological isolation and captivity appears throughout the nation's anticommunist consensus. Conant spoke of disconnected "universes of discourse," and Kennan noted that in diplomatic communication with the Soviet Union there can be "no appeal to common purposes [and] no appeal to common mental approaches." James Burnham, Hook's colleague in both philosophy and anticommunism, used a word that Kuhn would make famous as one way to describe this incompatibility: it was the "incommensurate" motives of the communists, Burnham explained, that had left the postwar world "split sharply and decisively into two incommensurate regions, the communist and the non-communist."[83] Kuhn's new image of "normal" scientific communities, destined to miscommunicate with their rivals and to be transformed by historic and winner-take-all revolutions, was painted in geopolitical lines and color.

Should this terminology and imagery be understood merely as a kind of packaging in which Kuhn presented the theory of science his encounter with Aristotle had inspired? To some extent, it can be. Kuhn hoped that his book would reach a wide, scholarly audience that would help to revolutionize the Western understanding of science. He therefore took care to craft *Structure* in an appealing and convincing way. His choice to use these familiar and well-understood tropes of geopolitics could only have helped him reach that goal. But it would be a mistake to conclude that *Structure* makes contact with its Cold War culture only as a means to promote or popularize scholarship that is otherwise unconnected. Elements of Cold War geopolitics are manifest in almost every stage of *Structure*'s gestation. At the very start, for example, Kuhn chose to understand his Aristotle experience not as offering an addition to, or correction of, his knowledge about science. He saw it without question as a kind of total conversion, even a "revelation," that had forever transformed his thinking about science.[84] Kuhn framed his own intellectual development, in other words, in terms of the era's abiding interest in "conversion experiences" at the hands of political ideology. Five years later, in an early précis and editorial correspondence about the monograph that would become *Structure*, Kuhn compared the psychological and sociological functions of scientific theories to those of "ideology." In 1961, just before *Structure* was published, he offered the striking (and, to some, upsetting) claim that *dogmatism*, as opposed to open-mindedness and creativity, is an essential, functional component of "normal science."[85] In its largest aspect, *The Structure of Scientific Revolutions* explicates the nature and course of scientific revolutions by comparing them to political revolutions,[86] so it was not unnatural for for Kuhn to see rank and file "normal" scientists through the lenses of Conant's and the nation's geopolitical preoccupations with ideologies and incompatible worldviews.

Predictably, Conant and some other anticommunist intellectuals were not impressed with Kuhn's theories of paradigms and "normal science."[87] In so adapting the consensus about what was *wrong* with Soviet political culture to explain what was *right* and revolutionary about science and its history, Kuhn had subverted one of the central tenets of Cold War liberal anticommunism: that what is good for society is also good for science and scientific progress. But in shaping Cold War anticommunism as he did, Conant ironically prepared the way for Kuhn's insights. His crusades to help scholars study and understand the Russian mind—simultaneously using the tools of psychology, sociology, history, and semantics on offer within the Russian Research Center—and his popular analyses of a world "divided" by ideology served not simply to make Kuhn aware of this geopolitical consensus. That was hardly necessary, for since he was a child in the progressive 1930s Kuhn was keenly and precociously interested in the politics of liberalism and human progress. Rather, Conant's Cold War politics and the consensus that language, ideology, and human psychology conspired to divide human experience into different, "incommensurate" worlds of experience and belief is best understood as a paradigm (in precisely the original sense Kuhn intended) for Kuhn's own interdisciplinary efforts to understand his Aristotle experience and formulate his theory of scientific revolutions.

To less intellectual anticommunists, such as J. Edgar Hoover, Senator McCarthy, and those who took their proclamations seriously, communists and communism were to be feared as destructive monsters seeking to destroy human civilization and leave the world in cold, totalitarian ruins. Nothing constructive could be learned from Russia's campaign to subdue and control humanity. Neither Kennan nor Conant, nor especially Hook, would object strongly to such a description; but each would recognize it, at least privately, as counterproductive exaggeration. However effectively such anticommunist rhetoric might raise funds or gain votes from the public, it provided no help in solving the urgent diplomatic and educational problems at hand. For Conant and Kennan, especially, the problem of communism was an ineluctably human problem, calling not for comic-book stereotypes but serious scholarship and creative analysis that aimed to understand the peculiar ideological chemistry unfolding within the Soviet Union and international communism.

Kuhn responded similarly to his Aristotle experience. On that "memorable (and very hot) summer day"[88] in 1947 when Aristotle's physics suddenly clicked in Kuhn's mind, he took it not as a sign that he was daydreaming or suffering from heatstroke; least of all did it confirm the conventional wisdom that Aristotle's physics, so different from modern orthodoxy, was primitive or confused. To a young, talented scholar, eager to make his mark as an intellectual, the shock of Aristotle seemed suspiciously like a breakthrough. Aristotle, it promised to reveal, was not a bad physicist after all, just a different kind of physicist who responded to nature in a different way than others had. Kennan would insist similarly that the Soviets are not simply mendacious, much less inhuman, as popular stereotypes would soon come to suggest. They had merely—but crucially—responded to their history and the world around them in a different, non-Western way.

Notes

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2. Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 4th edition, 2012 [1962]), 111.
3. James Bryant Conant, *My Several Lives*: *Memoirs of a Social Inventor* (New York: Harper & Row, 1970), 561.
4. "Moscow's New Purge," *The New York Times*, editorial, 1 March 1938.
5. See James Hershberg, *James B. Conant*: *From Harvard to Hiroshima and the Making of the Nuclear Age*, (New York: Knopf, 1993), ch. 14.
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7. AP and Brooks Atkinson, "Stalin Sets a Huge Output Near Ours in 5-Year Plan; Expects to Lead in Science," *New York Times*, 10 Feb. 1946.
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9. See Nicholas Thompson, *The Hawk and the Dove*: *Paul Nitze*, *George Kennan*, *and the History of the Cold War* (New York: Henry Holt, 2009), ch. 4.
10. George F. Kennan to George Marshall, 22 Feb. 1946, quoted from scanned version held at the Truman presidential library (www.trumanlibrary.org).
11. David Allan Mayers, *George Kennan and the dilemmas of US foreign policy* (Oxford: Oxford University Press, 1988), 100.
12. This and subsequent information about Thomas Kuhn and the writing of *The Structure of Scientific Revolutions* draws on my forthcoming monograph, *The Politics of Paradigms*: *Thomas Kuhn*, *James B. Conant*, *and* the 'Struggle for Men's Minds'.
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15. See, for example, George A. Reisch, *How the Cold War Transformed Philosophy of Science*: *To the Icy Slopes of Logic* (New York: Cambridge University Press, 2005).
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17. Long telegram, 6.
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21. Long telegram, 17.
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24. Ibidem, 574.
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26. Ibidem, 573.
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31. George F. Kennan, *Russia and the West Under Lenin and Stalin* (Boston: Little, Brown and Co., 1960), 390.
32. Kennan, *Sources of Soviet Conduct*, 581.
33. Ibidem, 575, 574.
34. Ibidem, 581.
35. Conant, James B., *Education in a Divided World*, 30.
36. Kennan, *Sources of Soviet Conduct*, 582.
37. Conant, James B., *Education in a Divided World*, 30.
38. Ibidem, 234.
39. Ibidem, 234.
40. Hershberg, *James B. Conant*, 114.
41. Kennan, Long Telegram, 17.
42. Lawrence E. Davies, "Conant Sees Peace Under Atom Pact," *New York Times*, 9 Sept. 1947. See also Hershberg, *James B. Conant*, 338–339.
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45. Ibidem, 480–481, 495.
46. Ibidem, 483–484.
47. Ibidem, 491–501.
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63. Kennan, *Russia and the West Under Lenin and Stalin*, 10.
64. Kennan, *Russia and the West Under Lenin and Stalin*, 11.
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67. On Osborn's appointment, see Hershberg, *James B. Conant*, 328. On the frustrating and puzzling behavior of Gromyko, see for example "Gromyko in Clash on Disarmament," *New York Times*, 7 Apr. 1948.
68. Hershberg, *James B. Conant*, 845, note 81.
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University Press, 2009), chs. 2 and 3, which detail the formation of Harvard's Center and Columbia University's Russian Institute the year before.

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73. David Dallin, "The Making of the Russian Mind," *New York Times*, 2 Jul. 1950. See also Barrington Moore, Jr. *Terror and Progress-USSR* (Cambridge: Harvard University Press, 1954); Alex Inkeles, *Public Opinion in Soviet Russia: A Study in Mass Persuasion* (Cambridge: Harvard University Press, 1950).
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82. Kuhn, *Structure*, 4 and 109.
83. James Burnham, *The Struggle for the World* (New York: John Day, 1947), 73 and 24.
84. See Kuhn's foreword to Ludwig Fleck, *The Genesis and Development of Scientific Fact* (Chicago: University of Chicago Press, 1979), vii.
85. Kuhn discusses "theory as ideology" in a Guggenheim grant application and also in correspondence with Charles Morris in the summer of 1953. Both documents are held in Kuhn's archived papers at MIT, Box 2, Folder 53. On "dogmatism," see Kuhn, "The Function of Dogma in Scientific

Research," in A.C. Crombie, ed., *Scientific Change* (New York: Basic Books, 1963), 347–369.

86. The comparison is direct in the first pages of "The Nature and Necessity of Scientific Revolutions."
87. See Hershberg, *James B. Conant*, 860, note 84. For more detail on Conant's reaction, see my "The Paranoid Style in American History of Science," §7.
88. Kuhn, preface to *The Essential Tension* (Chicago: University of Chicago Press, 1977), xi.

CHAPTER 3

"What's So Great About Science?" Feyerabend on Science, Ideology, and the Cold War

Ian James Kidd

In a 1976 paper, entitled "On the Critique of Scientific Reason", the iconoclastic philosopher of science Paul Feyerabend argued that two fundamental questions ought to be at the core of the philosophy of science.[1] The first question was "What is science?" was hardly surprising, given the importance of the investigation and description of the theories, methods, and practices of scientific enquiry to that discipline. The second question, "What's so great about science?", might be more surprising, depending on how it is taken: if read as a call for clear articulation of the epistemic merits of scientific enquiry, then it is a jocular statement of another typical task of the philosophy of science—namely, the evaluation of those theories, methods, and practices. But coming from Feyerabend, a self-confessed "epistemological anarchist" who was later memorably described as the "worst enemy of science", that second question might set off alarm bells. Indeed,

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the year after this paper was published, Feyerabend published *Science in a Free Society*, where he called for a separation of science and the state, and for a radical demotion of the cognitive and cultural authority of science—on the grounds that its "hegemonic" authority was an active threat to a "free society".[2] At this point in his career, Feyerabend's emerging ambition was "to defend society … from all ideologies, science included".[3]

Such critical claims took on an increasingly political or ideological character from the mid-1970s onwards. In the first edition of the book, *Against Method*, published in 1975, Feyerabend emphasises how "Western rationalism", superlatively manifested in science, has enabled and encouraged policies of intellectual and cultural imperialism. In a set of charges familiar in a "post-colonial" world, Feyerabend declares that "non-Western tribes" are "physically suppressed [and] lose their intellectual independence", while, closer home, the hegemony of science is "restricting the lives … thoughts [and] education of developed world societies".[4] Naturally, these proclamations and denunciations were rapturously received by legions of critics of science and scientific culture and made Feyerabend "a hero of the anti-technological counter-culture".[5] Certainly they resonated within a social and political context characterised by critical revolt against established authorities, ideologies, and traditions and were marked by a growing interest in, and sympathy towards, oppressed and marginalised groups, ideas, and ways of life. Although many of Feyerabend's criticisms were better made by other counter-cultural critics—Herbert Marcuse, say, or Martin Heidegger—his status, during this period at least, as a distinguished philosopher of science indicates that his own critical contributions may be original and worth exploring.

The purpose of this chapter is, therefore, to offer a sympathetic interpretation of Feyerabend's question, "What's so great about science?" and to argue that, despite its proximity to his polemics, it does reflect a sincere and important conviction. My suggestion is that Feyerabend aimed to inspire and enable critical reflection on the cognitive and cultural authority of the sciences in late modern societies—and, indeed, that much of his work from the late 1960s until his death in 1994 can be understood as orbiting around that theme. Such critical vigilance is necessary because any cognitive and cultural authority—political, religious, scientific—is liable to lapse into complacency and dogmatism, certainly in principle, if not in practice. In Feyerabend's dramatic term, the possibility of *tyranny* is always present, and this being so, our best defence is to establish effective means of identifying and interdicting "tyrannous" tendencies. And, as I go on to argue, in the case of science, this defence is best mounted by the discipline of the philosophy of science.

For a core purpose of that discipline, in Feyerabend's view, was that it ought to enable such critical vigilance by providing resources and arena for asking and answering the question, "What's so great about science?"

This conception of the philosophy of science was not original to Feyerabend, and it is enjoying a recent revival in the form of "socially engaged philosophy of science".[6] Such a development is to be deeply welcomed, given the numerous obvious examples in which philosophical reflection on science and its place in society can be illuminating and ameliorative. Many factors fed into its emergence within Feyerabend's own work, including a genuine enthusiasm for "socially engaged" scientists such as Ernst Mach, and growing acquaintance, especially during the 1980s, with cultural anthropology and development studies.[7]

Oddly enough, certain relevant contemporaneous intellectual disciplines and political movements are not cited by him, such as feminist philosophy, critical race theory, or critical development theory. Such blind spots are curious, to say the least, for though Feyerabend was sincerely committed to "philosophical pluralism", a point made persuasively by Eric Oberheim, it is also clear that his pluralism was typically pointed in certain disciplinary directions.[8] There is plentiful discussion of the history of physics, classics, and the history of art, but practically nothing on moral philosophy, say, or economics, despite their proximity to issues—like social justice—that did interest him.

The focus of this chapter, however, is upon one contextual factor that did influence Feyerabend—namely, the Cold War. It is clear that his favoured idiomof "ideology", "tyranny", and so on—was a reflection of the charged ideological conflicts of the mid-twentieth century, and certainly it is clear that Feyerabend's deep concern was with deep themes—liberty, say, and authority—that had a special resonance during the Cold War for academics as much as for "the general public". A further aim of this chapter, then, is to situate Feyerabend in historical context and to explain how the Cold War informed his views on science and its place within late modern societies.

Specifically, I argue that the Cold War played two roles. The first is that the Cold War influenced the agenda of mid-twentieth-century Anglophone philosophy of science by proscribing debate about social and political issues.[9] It was to this narrowed agenda that Feyerabend was critically reacting by explicitly calling for a socially engaged philosophy of science. The second is that *science* played a central role in the ideological struggle of the Cold War. Both the USA and USSR were competing for the cultural prestige of being pre-eminent exemplars of a scientific culture, as well as competing, at a more pragmatic level, for technological superiority. Feyerabend's calls for critical reflection on the prestige of science, therefore, resonated within a wider ideological context: within this framework, to be a scientific culture is to be progressive, "modern", and especially capable of beneficent governance of the world. Therefore, to seriously ask the question "What's so great about science?" is to call into question one of the foundational values central to the ideological struggle of the Cold War. Taken together, these two points indicate that Feyerabend was calling into question both the disciplinary remit of the philosophy of science and a wider set of ideological issues concerning the authority and value of science.

There is also a further aspect, albeit one that Feyerabend tends to leave implicit, which is that the programme of critical reflection that he calls for applies to *all* authoritiesdemocracy just as much as communism, the left wing just as much as the right. A free society, of the sort Feyerabend seems to envisage, is one whose members are freed from unreflective and presumptive commitment to any set of epistemic and political ideals; and this means, in practice, that no special exemptions ought to be extended to liberal democracy and science.

It should be added, too, that Feyerabend's call for active critical reflection upon both the value of science and the aims of the discipline of philosophy of science are not confined to the long-gone days of the Cold War. Though Feyerabend does not put his claim quite like this, his call is, I think, for critical enquiry into prevailing cognitive and cultural authorities as part of epistemically and socially responsible citizenship. Granted, his interests were usually more critical than constructive, and he often lapsed into charged polemics rather than careful persuasion—but, still, once separated from the rhetoric, a sensible and defensible claim can be constructed.[10]

If so, then Feyerabend's conception of the philosophy of science as a discipline that enables and inspires critical reflection on the place of the sciences within late modern societies enjoys a continuing importance. It origins may lie in the Cold War, but its impetus is provided by the fact that ours is, and will likely continue for some time to be, a scientific culture.

Two Fundamental Questions

The significance of the two questions that Feyerabend identifies as fundamental to the philosophy of science can be appreciated once they are placed in the context of the historical development of that discipline. Clearly enough, philosophers have devoted great time and energy to asking and trying to answer the question "What is science?", for instance, by constructing models of science, or studying scientific practice, and so on. Feyerabend, of course, made his own contributions through his studies of scientific realism, methodology, and empiricism, among other topics.[11] But for Feyerabend this first question cannot be separated from the further question of the value and significance of science—not least, since prevailing conceptions of "the nature of science" are often false or distorting, and so liable to lead people to misestimate the scope and value of science. A main claim of *Against Method* was, after all, that the authority of science is typically premised on the idea that it is epistemically privileged by virtue of its employing a single, formalised methodology—"The Scientific Method"—that granted it a special epistemic and social authority. But as Feyerabend argued, methodological monism finds little support in the realities of the history and practice of science, for what one finds are, in fact, a plurality of diverse and changing methodologies. Naturally, he chose to dramatise this by using the label "epistemological anarchism", but the idea is more soberly described in terms of the "disunity of science" and, more recently, "scientific pluralism". (Oddly, though, few advocates of the disunity or plurality of science cite Feyerabend as a precursor to their views, with a few honourable exceptions.)[12]

It was this perception of the neglect of one of its core purposes that led Feyerabend to backhandedly describe the philosophy of science as "A Subject with a Great Past"—but, pointedly, no future.[13] The reason for this harsh judgement was that philosophers of science were failing to engage with the relationship between science and wider social and political concerns, such as education and policymaking. Philosophers of science—of the mid-1970s, at least—were, complained Feyerabend, producing "castles in the air" and had abandoned "the intention of influencing the development of science", thereby "making a contribution to … the world".[14] In section four of that paper, in fact, Feyerabend goes on to praise the philosopher-physicist Ernst Mach as an excellent example of a practising scientist who reflected philosophically on his practice, while also engaging with social and political issues—preserving freedom of thought, say. The philosopher being presented, albeit unsystematically, for admiration and emulation is therefore socially and practically engaged with science.

Contemporary scholarship in the history of the philosophy of science largely corroborates these criticisms of the direction and agenda of the discipline in the mid-twentieth century and the further implicit claim that, prior that period, the discipline had indeed been socially engaged. A good example is the Vienna Circle. Despite the diversity and development of the Circle's views, a shared commitment was to the articulation of a "scientific world-conception" able to "transform, enlighten, and invigorate culture", and so to achieve—as three of its lead figures put it—"a rational transformation of the social and economic order".[15] But by the 1960s, these sorts of robust social and political ambitions had largely disappeared from philosophy of science, owing largely to the changing intellectual and ideological climate.

This is especially clear in the emergence of the "value-free ideal" of science during the 1950s, which has been brilliantly documented by Heather Douglas.[16] A value-free science can be presented as a politically neutral, "objective" science, free from contamination by partisan purposes and prejudices, and this in turn enables the discipline of philosophy of science to present a similarly depoliticised professional self-image. In Douglas' words, "a focus on the logic of science, divorced from scientific practice and social realities, was an increasingly attractive approach for the philosophy of science as the cold war climate intensified".[17] At a time of "McCarthyism" the co-optation of science and academia as battlegrounds in the many-sided conflict with the Soviet Union, the longevity, if not survival, of the philosophy of science was judged to lie in its strict neutrality on political matters. It was stripped of its social and political content and, as Reisch puts it, "effectively forced … to take [an] apolitical, highly abstract form".[18]

My suggestion is that it was this abstract, apoliticised conception of the philosophy of science to which Feyerabend was critically responding. In a neglected 1968 paper, he complained that science had abandoned social and political discussion of "the good life" despite the obvious relevance of science to the topic.[19] Several of his papers during the 1960s and 1970s gesture to socially engaged topics and themes, though usually as asides to some more "technical" point, and it is only really from about the late 1960s that such themes really come to the fore. It is worth asking, then, why Feyerabend kept such a close focus on the social and political themes that so many of his peers in the philosophy of science were neglecting.

There are, I suggest, at least two main points to consider, with the first being Feyerabend's temperamental iconoclasm. It is obvious to anyone who reads his work that he was, if nothing else, attracted to radical stances, devil's advocacy, and deeply resistant to conformism, dogmatism, and "groupthink". It is plausible that if he judged that his peers were reluctant to engage with social and political issues—and likely to be alarmed if he did—then he would enthusiastically do so. Throughout his correspondence with *Imré* Lakatos, one often finds Feyerabend adopting the alternative view to whomever he was debating with—defending the American Constitution against radicals, say, or defending witchcraft against enthusiasts for modern physics.[20] Indeed, where criticism was concerned, the rule does seem to have been *anything goes*. In 1974, for instance, John Krige recalls that Feyerabend began his lecture course at Sussex by posing three questions: "What's so great about knowledge? What's so great about science? What's so great about truth?"[21]

The second point is that Feyerabend enjoyed the professional and institutional freedom to direct his interests as he wished. In 1958, he was appointed as a professor at the University of California in Berkeley, but over the next 15 years he took a succession of visiting professorships at University College London, Berlin, Yale, and Auckland. Such professional mobility and institutional diversity gave Feyerabend a special freedom, and he explains his constant moves partly in terms of a resistance to being confined to any single institution or culture.[22] Moreover, he had the special advantage of being based at Berkeley, a bastion of left-wing liberalism and activism, both then and, albeit perhaps less so, now.[23] It is, of course, much easier to pursue political interests within an institution that is appropriately politically engaged, especially when one has the advantage of easily moving to alternative institutions.

These two points of course pull together in the image of Feyerabend as a temperamental iconoclast who, by achievement and good fortune, enjoyed a professional status that allowed him to pursue his interests untrammelled. During his time at Berkeley, for instance, much of his teaching was highly unconventional: he lectured on church dogma and witchcraft; set essay questions such as "What is the meaning of the "Age of Aquarius?"" and "How was the coldness of the Devil's penis explained by St Thomas Aquinas?"; and invited "guest lecturers", including warlocks, astrologers, and members of the Gay Liberation Front.[24] I suppose that if, at the height of one's career, one can teach at a top American university a philosophy of science course whose syllabus includes the Council of Trent, where students can answer exam questions on an astrological age popularised by the popular musical *Hair*, and hear guest lectures by the daughter of the founder of the Church of Satan, then one must have a suitably secure sense of one's professional and institutional status.[25]

It might be objected that few of these topics pertain to the social and political aspects of the philosophy of science, and that Feyerabend may be retreating into eclectic evasion of a theme which, all the while, was still proscribed by the philosophy of science. To label Feyerabend an "anarchist" or "relativist" is, after all, a standard strategy used by those who want to impugn him—for who would take seriously a criticism of science by a philosopher who, a few pages later, might praise voodoo? Certainly Feyerabend does not always do himself many favours where credibility is concerned, but there was, perhaps surprisingly, a reasoned philosophical rationale for the "anarchistic" topics and pedagogy that Feyerabend employed, and it is related to his conception of the purposes of the philosophy of science.[26]

The core idea is, again, that of critical reflection on the authority of science. Feyerabend played with different arguments to support his call for such critical vigilance, of which I will quickly summarise just four.[27] The first is that judgements about the "excellence" of science—to use Feyerabend's preferred term—are often the results of *assumption* rather than *argument*, and therefore are vulnerable to criticism—though this does not, of course, mean that they cannot subsequently be supported by arguments.[28] The second is that judgements about the excellence of science are often grounded in false, naïve, or otherwise untenable conceptions of the history, methods, or practices of science, such as the "myth" of methodological monism challenged in *Against Method*. The third is that the sciences are, says Feyerabend, "surrounded by an aura of excellence" that tend to "check enquiry into their beneficial effects", owing to their entrenchment within late modern society, and to the latent scientism that this can encourage.[29] During the 1980s, for instance, much of Feyerabend's work drew on examples in which the environmental and agricultural knowledge of aboriginal peoples had been displaced without due consideration of their merits. The fourth is that the scientific establishment can, at least in certain cases, exploit its cognitive and cultural authority to negate or minimise scrutiny, for instance by implicitly defining evaluative criteria in its own terms—"dice-loading", in other words—which Feyerabend suggested was the case with appraisals of Chinese acupuncture by the medical establishment.[30]

Though they have been only quickly presented, it should be clear that these four styles of argument do not constitute a systematic critique of the cognitive and cultural authority of the sciences in late modern societies. They are problematic for several reasons. For a start, the object of these criticisms is, too often, "science" in the singular, rather than specific disciplines, research programmes, and so on. Though Feyerabend embraced a pluralistic, "disunified" picture of the sciences, this did not stop him from regularly invoking it for polemical purposes. Indeed, he explicitly criticises those who talk of ""science" as a single uniform entity", though he also concedes that it can be useful, at least at certain times, for "tactical reasons".[31] Next, certain of these styles of argument are contingent upon contestable presuppositions, for instance, that judgements about the excellence of science are generally presumptive rather than the results of careful deliberation and reflection. Perhaps this is true if one thinks that it is *individuals*—the "man on the street", perhaps—who ought to make these judgements, but this is an absurd view. Typically, judgements about the excellence of particular scientific products, such as theories or technologies, are undertaken by specialist groups such as peer review panels, science policy committees, healthcare bodies, and so on. And finally, Feyerabend generally neglects to do the careful sociological work that is required for a robust appraisal of public attitudes towards the sciences: or rather, of the attitudes of diverse publics to particular sciences, and to the institution of science more generally. "The public" is no more singular than "science", and it is invariably sociologically too naïve to speak, as Feyerabend does, of what "the public" think about science; a good contemporary example is of course the diverse and complex attitudes towards climate change in the USA.

Other concerns could be offered, of course, but my point is that even Feyerabend failed to fully develop many of these arguments; the general claim that appraisals of the value, authority, and "excellence" of science are too insufficiently robust is defensible. In the last few decades, the sorts of concerns that Feyerabend broaches about the values and authority of the sciences have, after all, been taken up by a diverse range of disciplines. Many of the main movements in contemporary philosophy of science surely speak to these concerns, such as political philosophy of science, philosophy of science in society, science and values, and feminist and "postcolonial science and technology studies".[32] These areas of enquiry can be interpreted as different ways of engaging with the question, "What's so great about science?"

The emerging claims, then, are that Feyerabend argued that the discipline of philosophy of science was, at least in the 1960s, abrogating its core imperative to engage with social and political claims about the authority of the sciences. The Cold War had, at least in the USA, helped to confine the agenda of the discipline, and it was this construction that prompted Feyerabend's talk of its having a "great past". The restoration of a socially engaged philosophy of science is now increasingly being achieved, as evidenced by Philip Kitcher's recent remark that an "urgent task" for contemporary philosophers of science is the production of a "theory of the place of Science in a democratic society".[33] Indeed, the title of the book in which Kitcher makes this remark—*Science in a Democratic Society*—is an obvious allusion to Feyerabend's own *Science in a Free Society.*

The first way that the Cold War affected Feyerabend's philosophy of science is, therefore, that it artificially narrowed the agenda of the discipline to proscribe engagement with social and political concerns. Clearly enough, Feyerabend was only partly successful at this, owing to his lapse into polemics, among other things. But it is easy to see how he was reacting against a narrow "technical" agenda when he called for the "separation of science and the state" as part of his ambition of "protecting society from all ideologies". Indeed, such declarations were radical precisely because they directly challenged a disengaged and apolitical conception of the discipline of philosophy of science. In the words of one perceptive commentator,

> What makes his argument relatively unique and provocative is the way he employs his conceptual insights from the history and philosophy of science to defend a view of human freedom and self-determination which is directly at odds with any form of dogmatism or closed society.[34]

In the next section, I discuss a further way in which Feyerabend's calls for a socially engaged philosophy of science was shaped by the Cold War.

Science, Modernity, and Ideology

The call for critical appraisal of the prestige of science can be located within the context of wider ideological debates during the Cold War. A distinguished historian has described the Cold War as fundamentally a "battle of ideas"—of competing ideological visions that consisted of "explicit ideas and implicit assumptions that provided frameworks for understanding the world and defining actions in it".[35] At a deep level, these ideological battles involved competing claims by the USA and USSR to represent a privileged position in the intellectual and cultural development of human societies. Science, of course, played a central role in these disputes in two distinct ways. The first is the instrumental valuation of science as a source of new military and industrial technologies—hydrogen bombs, stealth aircraft, "spy satellites"—and, the second, as a powerful honorific status symbol. The ideological struggle between the Cold War powers was, at a suitably deep level, a struggle for the privileged status of being a *modern* culture—progressive, rational, advanced—which meant, in practice, a scientific culture.

Such ideological deployments of science were not original to the Cold War. During the Second World War, vigorous efforts were made by both the Allies and the Axis powers to associate science with the preferred political ideologies. The American sociologist Robert Merton argued, in an influential series of papers, that science and democracy were indissolubly bound up in a single cultural mode.[36] The virtues of scientific enquiry were intimately related—even "spiritually", perhaps—with the values of a democratic society. In fact, Merton's paper, "A Note on Science and Democracy", has been praised by one American intellectual historian as "one of the most robust and firmly grounded of its era's contributions to the intellectual defence of science and democracy".[37] At the same time, Soviet academicians and leaders made parallel claims. No less a figure than Joseph Stalin took an active role in Soviet academic and scientific life, positioning himself as the "coryphaeus of science", arguing that both Marxism and the sciences were engaged in a common project of identifying the "objective processes which take place independently of the will of man", with physics handing the "laws of natural science" and Marxism dealing with the "laws of political economy".[38]

In the late 1940s and into the 1950s, ideological appeals to the sciences continued unabated, most obviously with the Space Race, and if anything, the ideological import of being—and of being seen to be—a scientific culture only intensified. Perhaps the most forthright statement on the part of the democratic West was offered by the American chemist James Conant, later to be president of Harvard University. "Scholarly inquiry and the American tradition", he wrote, "go hand in hand", for the reason that "science and the assumptions behind our politics are compatible; in the Soviet Union, by contrast, the tradition of science is diametrically opposed to the official philosophy of the realm."[39] Such claims of an intimacy between ideology and science continued unabated throughout the remainder of the Cold War, and it was judged, by many Americans after the collapse of the Soviet Union, that their scientific and technological superiority was proof of their rightful status as *the* pre-eminently modern scientific culture.[40]

The Cold War, then, helped to determine "what science was, what it did, and what it meant", and this shaped both public and political perceptions of the significance of science.[41] Science is, after all, open to a plurality of alternative, often competing perceptions and evaluations, ranging from the vehicle of a technocratic utopianism to a source of spiritual and cultural disenchantment, to offer just two. To be a scientific culture is not simply to enjoy the technological fruits of insights into the nature of reality, but to enjoy a privileged historical and political status—hence, the premium placed upon a proper respect for, and pursuit of, science. It was within this wider and deeper context that Feyerabend's question, "What's so great about science?", resonated: for to ask seriously that question was, even if only potentially, to call into question a major component of competing ideological self-images for which the Cold War had been waged. The West, for instance, projected an ideological vision of a democratic liberal scientific culture—progressive and plentiful, enquiring and emancipated—that, if achieved, would indicate a deep convergence of its definitive epistemic and political values. The citizens and the scientists of a free society would share common ideals and qualities: freedom of thought, unbiased and unprejudiced, free from dogmatic diktat and a corrupting ideology grounded in a false philosophy that found no support in objective scientific enquiry.

Feyerabend also identified certain common features of political and epistemic freedom, including an imperative upon critical scrutiny of prevailing authorities, resistance to self-serving "myths", and an active hostility to dogmatism. A large part of his criticism of Thomas Kuhn's model of science—of paradigms, "normal science", and so on—was, after all, grounded in the worry that contained a dogmatic, authoritarian ideology, disguised as the objective results of historical enquiry.[42] More obviously, there is the immense and genuine admiration that Feyerabend had for John Stuart Mill's essay, *On Liberty*, which he praises as a "magnificent essay", and as the 'outstanding example of a libertarian epistemology.[43] Indeed, Mill is one of the few figures whom Feyerabend consistently and approvingly cites, which is impressive, given his proclivity for retroactive revision of his intellectual debts.[44] Moreover, we know that Feyerabend taught *On Liberty*, alongside essays by Lenin and Mao, during the "student revolution" in Berkeley.[45]

Given the admiration for freedom and liberty, the hatred of dogmatic ideology, and the admiration for Mill, it might be supposed that Feyerabend's political sympathies lay broadly with the West—or, more precisely, with a socially liberal democratic system of government. Oddly enough, things are more complicated, for a couple of reasons. The first is that it is often very difficult to identify Feyerabend's own political views, given his tendencies to exaggeration, polemic, and devil's advocacy; during lectures, for instance, he would sometimes use Millian arguments to call for greater tolerance of fascism.[46] The second is that, if one consults his writings, it is difficult to identify any sustained or overt political commitments or affiliations, beyond a general enthusiasm for freedom and liberty, though even this shades, too often, into rhetoricised calls for "anarchism". (Often, though not always: after all, *Against Method* opens with the declaration that '*anarchism*, while perhaps not the most attractive *political* philosophy, is certainly excellent medicine for *epistemology*, and for the *philosophy of science*).[47] The third reason, perhaps the most important, is that Feyerabend's resistance to membership of groups, schools, or parties was too entrenched a feature of his character to allow him to explicitly align himself with either the left or the right. Around 1948, Feyerabend declined an opportunity to work as the assistant of the poet, playwright, and theatre director Bertolt Brecht, describing this decision, at the time, as "the biggest mistake of my life". But his initial regret dissolved once he discovered, some years later, the "collective pressure of the partly fearful, partly dedicated … group that surrounded him".[48] Likewise, his criticisms of both Popper and the "Popperian school" were often couched in terms of their collective dogmatism: according to his account, they required "declarations of faith" and asked him to "put Popper on every page and into every footnote of everything' he wrote".[49] The accuracy of these reports is not my concern, since these examples are cited to make the point that Feyerabend was deeply and inveterately hostile to identification with, or membership of, groups, parties, or organisations.

The main point to take from these remarks is that Feyerabend's hostility to group membership precluded his commitment to—and certainly participation in—any formalised political cause or party and prevented him from aligning himself with any ideology.

Ideological Monism

Feyerabend's reluctance to endorse or embrace a political ideology—to place himself on either the "right" or "the left", however tentatively—might seem puzzling. It seems clear enough that he had a stable, if unsystematic, commitment to a broadly liberal stance, coupled with an acute sense of, and respect for, the diversity of lifestyles. Clearly, his inspiration here was Mill's liberalism, whose influence becomes increasingly obvious in Feyerabend's later period, even if his reading of Mill is lacking in certain respects.[50]

A good statement of this is the essays collected together as *Farewell to Reason*. These reflect his views of the 1980s, and together defend the claim that "cultural diversity … is beneficial while uniformity reduces our joys and our (intellectual, emotional, material) resources".[51] The concern with praising and protecting cultural diversity in fact helps to explain Feyerabend's reticence about adopting a defined political ideology and his principled indifference to the Cold War conflicts that were, at this time, coming to an end. Early in *Farewell to Reason*, Feyerabend complains that "quarrels" about the competing ideological merits of liberal democracy and communism "shrink into insignificance" when compared to a deeper, underlying phenomena, which one might call *ideological monism*.

As Feyerabend complains,

> This is an international phenomenon; it characterises capitalist as well as socialist societies; it is independent of ideological, racial or political differences and it affects an increasing number of peoples and cultures … What is being imposed, exported, and again imposed is a collection of uniform views and practices which have the intellectual and political support of powerful groups and institutions.'[52]

As this passage makes clear, what Feyerabend was opposed to was not simply this or that ideology, but to the very idea underlying ideological conflict: that there is a single uniform way of conceiving and organising social and political life—just as he opposed the idea of a single, uniform scientific method and Kuhn's monistic model of science.

It might be supposed that Feyerabend might at least guardedly welcome the eventual emergence of the West as the dominant ideological power, given its formal commitment to democratic freedom and liberty. Though Feyerabend quoted Mao, he praised Mill, and his admiration for the former was genuine. Yet later in the passage, Feyerabend goes on to make clear his hostility towards the West:

> By now Western forms of life are found in the most remote corners of the world and have changed the habits of people who only a few decades ago were unaware of their existence. Cultural differences disappear … replaced by Western objects, customs, organisational forms.[53]

The real focus of Feyerabend's political concern was not, then, with the triumph of this or that ideology, but rather with the deeper framework of ideological monism. Though Feyerabend does not, of course, deny the significant differences between the West and the Soviet Union, his constant concern is with the presupposition that there is, or indeed could be, a single framework, ideology, or "form of life" to which geographically and culturally diverse communities of human beings ought to adopt. Moreover, any ideology, no matter how good its intentions and principles, can deteriorate if its members lapse into dogmatism and intolerance.

In fact, it was precisely the perpetual risk of a lapse into dogmatism that Feyerabend perceived not just in the political ideologies of the Cold War, but in any area of intellectual activity and culture. If there is any stable feature of Feyerabend's philosophy, it is that strict dogmatic commitment to any single set of doctrines, theories, or methods is, therefore, a route to myopia, dogmatism, and the implicit elevation of the particular interests of the relevant group, school, or party. The danger, as Feyerabend saw it, was that the enthusiast for a given theory or paradigm or ideology, inevitably, "looks at life through the spectacles of his own technical problems and recognises hatred, love, happiness, only to the extent that they occur in these problems", but given the complexity of "human interests and … human freedom", such a person—whether they be a diehard conservative, or a card-carrying socialist—is "proceeding in the worst possible fashion".[54] An ideological commitment would, on this view, be folly. Indeed, "there is nothing inherent in science or in any other ideology that makes it *essentially liberating*", since any of them, however well-intentioned, can "deteriorate and become stupid religions".[55]

With these remarks in place, I suggest that Feyerabend's stance on the Cold War was doubly critical for it presupposed an ideological monism to which he was temperamentally and philosophically opposed, and at a deeper level both of the competing ideologies shared an imperialistic spirit. In fact, many of his criticisms are directed, not at the USSR or the USA, or "East" or "West", but rather at something more general—usually "Western civilisation", "Western culture", and other cognate terms. As late as the early 1990s, one finds him fulminating against the "onslaught of Western civilization" upon aboriginal communities, and the "elitism which has so far dominated' its intellectual and political culture".[56] Such criticisms, grounded in a grand historical narrative, had in fact been a slumbering project of Feyerabend's for over 20 years. From at least the mid-1970s, Feyerabend was engaged in a never-completed project, a grand historical study of "the rise of Western rationalism", from the ancient to the modern world.[57] Since the study was never completed, its merits cannot be assessed, but the main claim was that the historical development of Western intellectual and cultural development indicates a progressive trend towards “abstraction”, culminating in the modern sciences—with ruinous social, environmental, and spiritual consequences.

Consider, for instance, Feyerabend’s complaint that “Western civilization” has, gradually, “spread all of the world”, driven by, and bringing in its wake, “knowledge … weapons, and monotony”, directed by ideologists with a zeal for “technological projects” aimed at the “rebuilding of cities and countries” despite the protests and preferences of the “wishes and values” of the people whose lives are thereby transformed.[58] This is a classic form of a broad style of critical narrative that became popular in several different traditions throughout the last century—in Theodor Adorno and Max Horkheimer’s *Dialectic of Enlightenment*, say, of the later Heidegger’s fulminations against the “technological” stance that dominates late modernity.[59] The point to make in the case of Feyerabend, however, is that both of the ideological superpowers waging the Cold War were instances of the trends he evidently deplored: the large-scale restructuring of societies, ecologies, and cultures according to a particular ideological conception. Feyerabend was always opposed to monism, whether scientific or ideological, and to anything that smacked of a conviction that despite the world’s ontological complexity and cultural diversity, there exists some single theory, method, or ideology—some grand “abstraction”, in his derogatory sense of that term—that can and should be imposed, firmly and even forcefully, upon that world.

So if Feyerabend was neither on the “left” or the “right” and a champion of neither liberal democracy nor state socialism, it is because of a powerful suspicion of ideological monism, and of people’s tendency to “deteriorate” into a stance of uncritical acceptance of prevailing convictions, values, and ideals—and these suspicions were, in his view, equally applicable on both sides of the Iron Curtain. Crucially, though, these criticisms of dogmatic, ideological monism in the political sphere directly paralleled Feyerabend’s interventions in the philosophy of science—for instance, in his axiom that there is a “totalitarian element” in any doctrine—political or scientific—which aspires to be, or which presents itself as, a “single uniform entity” that can, in principle or in practice, be “universally accepted”.[60]

During the twentieth century, then, the domains of both philosophy of science and of political ideology offered instances of the conflicts premised upon ideological monism. By contrast, what Feyerabend wanted, in both cases, was not to argue for one side against the other; instead, it was to attempt something more foundational, and no less radical: to try to compel people to disengage from their default, unreflective commitment to their cognitive and cultural authorities—the sciences, say, or a particular political ideology—and instead to enable people to become critically reflective.

This sentiment is nicely expressed in a 1976 dialogue, whose tone is both exasperated and sincere, and which offers a clear insight into Feyerabend's character:

> You see—I don't just want to replace maniacs of one kind by maniacs of a different kind—Jews by Christians, dogmatists by sceptics, scientists by Buddhists, I want to put an end to all manias and to the attitudes in people that support manias and make it easy for their prophets to succeed.[61]

Feyerabend therefore urges people to become properly critical, by which he meant being informed, self-critical, and free from "myths", false images—of science, say—and actively alert to their inherited prejudices and presumptive certainties. Though this may sound boringly thin—"Be informed!", "Argue, don't ridicule!"—but in most cases, argued Feyerabend, even these basic preconditions of reasoned enquiry were not fulfilled. Though there are many reasons for this, the one that most concerned Feyerabend was the failure of academics to perform their proper, socially engaged purposes—philosophers of science, say, who peddle false images of science, and who try to isolate their discipline from social and political concerns.

In late modern societies, where science is intimately implicated in ideological disputes, the wilful failure of philosophers of science to become socially engaged was both a professional and an intellectual *debacle*. Yet the Cold War created a climate that was hostile to the possibility both of those debates and the sort of discipline able to initiate and pursue them.

Conclusions

In this chapter, I've argued that the Cold War influenced Feyerabend's philosophy of science in two related ways, both related to his question, "What's so great about science?" The first is that this question is a serious question—an opportunity, not for a perfunctory rehearsal of celebratory sentiments, but rather for sustained critical reflection on the history, methods, scope, and value of the scientific enterprise. But to ask this question properly requires careful engagement with the cognitive and cultural authority of science, including its complicity with social and political values and concerns, and this in turn requires a socially engaged conception of the philosophy of science that had been nullified during the Cold War. It was this artificially delimited conception of the disciplinary remit of mid-twentieth-century Anglophone philosophy of science to which Feyerabend was critically reacting against. The second is that Feyerabend was also challenging a presumptive evaluation of the honorific status of science as a mark of a modern, progressive culture, and so was calling into question a core point of ideological contention between the USA and the USSR.

Moreover, Feyerabend gradually extended this radical critical stance from science to a wider set of political values and ideals, including the ideological monism that sustained the Cold War. The real danger, for Feyerabend, lay not with science or any given ideology, but with a fundamental faith in a single authoritative vision of society, of history, or indeed of reality. Though he doubtless had reservations about particular political ideologies, the deep object of his concern was ideological monism—the faith in the ideal of a single, uniform vision, whether of science, history, society, or reality itself. The role of the philosopher of science was to inspire and enable critical reflection on prevailing conceptions and estimations of science, but the Cold War militated against both this project and this conception of the role of the discipline. As Douglas puts it, the Cold War demanded "strict dichotomies: either one was with the United States, its capitalism and its democracy, or one was with the Soviet Union, its communism, and its totalitarianism."[62] It was such absolutist dichotomising that Feyerabend was so deeply opposed to, and it is this sentiment which flows through his philosophical and political thought.

Modern-day philosophers of science, living in a post-Cold War world, should therefore better appreciate the role that Feyerabend played in helping to restore a socially engaged conception of the discipline of the philosophy of science—a central question of which is, surely, "What's so great about science?"

Notes

1. Paul Feyerabend, "On the Critique of Scientific Reason", In *Essays in Memory of Imré Lakatos*, edited by Robert Cohen, Paul Feyerabend, and Marx Wartofsky (Dordrecht: Springer, 1976), 109–143.
2. Paul Feyerabend, *Science in a Free Society* (London: New Left, 1978). On his conception of a "free society", see my "Feyerabend on Politics, Education, and Scientific Culture", *Studies in History and Philosophy of Science*, 57 (2016): 121–128.

3. Paul Feyerabend, “How to Defend Society Against Science”, *Radical Philosophy* 11 (1975): 3–8.
4. Paul Feyerabend, *Against Method*: *Outline of an Anarchistic Theory of Knowledge* (London: New Left, 1975), 299.
5. John Preston, “Paul Feyerabend”, *The Stanford Encyclopedia of Philosophy* (edited by Edward N. Zalta), available at plato.stanford.edu/archives/win2012/entries/feyerabend/, §2.17.
6. See, for instance, the papers in the special issue of *Synthese* 177/3 (2010) edited by Carla Fehr and Kathryn Plaisance and entitled *Socially Relevant Philosophy of Science*.
7. At several places, Feyerabend offers useful intellectual autobiographical sketches, including *Farewell to Reason* (London: Verso, 1987), ch.12 and *Against Method* (London: Verso, 1993 [3rd edition]), ch.20.
8. Eric Oberheim, *Feyerabend’s Philosophy* (Berlin: Walter de Gruyter, 2008).
9. I say “Anglophone” to mark the fact that philosophical reflection on the sciences is also a feature of other philosophical traditions that are typically, if inelegantly, classified as “Continental”. See, further, Gary Gutting, *Continental Philosophy of Science* (Oxford: Blackwell, 2005).
10. On Feyerabend’s rhetoric and polemics, see Oberheim, *Feyerabend’s Philosophy*, ch.1.
11. These are the topics of the first two volumes of Feyerabend’s collected papers: *Realism*, *Rationalism*, *and Scientific Method*: *Philosophical Papers*, *Volume One* (Cambridge: Cambridge University Press, 1981) and *Problems of Empiricism*: *Philosophical Papers*, *Volume Two* (Cambridge: Cambridge University Press, 1981), respectively.
12. These are John Dupré, *The Disorder of Things*: *Metaphysical Foundations of the Disunity of Science* (Cambridge, MA: Harvard University Press, 1993), 263f and Hasok Chang, *Is Water H2O*? *Evidence*, *Realism and Pluralism* (Dordrecht: Springer, 2012), 269f.
13. Paul Feyerabend, “Philosophy of Science: A Subject with a Great Past”, In *Minnesota Studies in the Philosophy of Science*, Vol. 5, edited by R. H. Steuwer (Minneapolis: University of Minnesota Press, 1970), 172–183.
14. Paul Feyerabend, “Philosophy of Science: A Subject with a Great Past”, 172.
15. Neurath, Carnap, and Hahn, quoted in George A. Reisch, *How the Cold War Transformed Philosophy of Science*: *To the Icy Slopes of Logic* (Cambridge: Cambridge University Press, 2005), 43 and 305.
16. Heather Douglas, *Science*, *Policy*, *and the Value-Free Ideal* (Pittsburgh: University of Pittsburgh Press, 2009).
17. Douglas, *Science*, *Policy*, *and the Value-Free Ideal*, 48.
18. Reisch, *How the Cold War Transformed Philosophy of Science*, 6.
19. Paul Feyerabend, “Science, Freedom, and the Good Life”, *Philosophical Forum* 1 (1968): 127–135.

20. See ImréLakatos and Paul Feyerabend, *For and Against Method: Including Lakatos' Lectures on Scientific Method and the Lakatos-Feyerabend Correspondence*, edited by Matteo Motterlini (Chicago: University of Chicago Press, 1999), 179 and 190.
21. John Krige, *Science, Revolution, and Discontinuity* (Brighton: Harvester, 1980), 106.
22. Paul Feyerabend, *Killing Time: The Autobiography of Paul Feyerabend* (Chicago: University of Chicago Press, 1995), 112–113 ff.
23. An engaging historical study, especially for the period during this period of Feyerabend's career, is W.J. Rorabaugh, *Berkeley at War: The 1960s* (Oxford: Oxford University Press, 1969).
24. See *Killing Time*,137 and *For and Against Method*, 331, 302 and 190.
25. See *For and Against Method*, 249.
26. I say more about views on education and criticism in my "Feyerabend on Science and Education", *Journal of Philosophy of Education* 47/3 (2013): 407–422.
27. Feyerabend, *Against Method*, ch.19.
28. Feyerabend, "On the Critique of Scientific Reason", 112.
29. Paul Feyerabend, "Consolations for the Specialist", in *Criticism and the Growth of Knowledge*, edited by ImréLakatos and Alan Musgrave (Cambridge: Cambridge University Press, 1970), 197–230. Quotation at 209.
30. For a sustained case study and discussion, see Ian James Kidd, "A Pluralist Challenge to "Integrative Medicine": Feyerabend and Popper on the Cognitive Value of Alternative Medicine", *Studies in History and Philosophy of Biological and Biomedical Sciences* 44/3 (2013): 392–400.
31. Paul Feyerabend, *Conquest of Abundance: A Tale of Abstraction versus the Richness of Being*, edited by Bert Terpstra (Chicago: University of Chicago Press, 1999), 264 and 243.
32. See, e.g. Philip Kitcher, *Science, Truth, and Democracy* (Oxford: Oxford University Press, 2001); Harold Kincaid, John Dupré, Alison Wylie (eds.), *Value-free Science? Ideals and Illusions* (Oxford: Oxford University Press, 2007); Heidi E. Grasswick (ed.), *Feminist Epistemology and Philosophy of Science: Power in Knowledge* (Dordrecht: Springer, 2011); Sandra Harding, *Sciences From Below: Feminisms, Postcolonialities, and Modernities* (Durham, NC: Duke University Press, 2008).
33. Philip Kitcher, *Science in a Democratic Society* (New York: Prometheus Books, 2012), ch.1 *passim*.
34. Paul Tibbetts, "Feyerabend on Ideology, Human Happiness, and the Good Life", *Man and World* 9/4 (1976): 362–371.
35. David Engerman, "Ideology and the Origins of the Cold War, 1917–1962",in *The Cambridge History of the Cold War*, edited by Melvyl

Leffler and Odd Arne Westad (Cambridge: Cambridge University Press, 2010), 20–43. Quotation from page 20.

36. David A. Hollinger, *Science, Jews, and Secular Culture: Studies in Mid-twentieth-century American Intellectual History* (Princeton: Princeton University Press, 1996), 81. See also Robert Merton, "A Note on Science and Democracy", *Journal of Legal and Political Sociology* 1 (1942): 115–126.
37. Hollinger notes that Feyerabend is notable for his claim that "science [can be] part of a system of domination". Hollinger, *Science, Jews, and Secular Culture*, 82.
38. Quoted in Ethan Pollock, *Stalin and the Soviet Science Wars* (Princeton: Princeton University Press, 2006), 11.
39. Quoted in Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (Cambridge: Cambridge University Press, 1988), 297. On the Cold War context of American social science, see especially chapters 10–12.
40. See, further, Audra J. Wolfe, *Competing with the Soviets: Science, Technology, and the State in Cold War America* (Baltimore, MD: The John Hopkins University Press, 2012).
41. Mark Solovey, "Science and the State during the Cold War: Blurred boundaries and a Contested Legacy", *Social Studies of Science* 31/2 (2001): 165–170. Quotation from page 168. This issue of the journal is dedicated to science in the Cold War.
42. See, especially, "Consolations for the Specialist".
43. See Feyerabend, *Against Method*, 38; *Farewell to Reason*, 281.
44. Oberheim notes that Feyerabend "systematically removed his acknowledgements to Popper from the two volumes of collected papers published in 1981, sometimes replacing them with acknowledgements to Duhem or Mill". *Feyerabend's Philosophy*, 3.
45. *Killing Time*, 123; *For and Against Method*, 169 and 351.
46. *For and Against Method*, 242.
47. *Against Method*, 9.
48. *Killing Time*, 73.
49. *Killing Time*, 97.
50. See, e.g. Struan Jacobs, "Misunderstanding John Stuart Mill on Science: Paul Feyerabend's Bad Influence", *Social Science Journal* 40/2 (2003): 201–212; Elizabeth A. Lloyd, "Feyerabend, Mill, and Pluralism", *Philosophy of Science* 64/4 (1997): 396–407; Kent Staley, "Logic, Liberty and Anarchy: Mill and Feyerabend on Scientific Method", *Social Science Journal* 36/4 (1999): 603–614. For a summary of this debate, and my own views on it, see my *Pluralism and the "Problem of Reality" in the Later Philosophy of Paul Feyerabend*, PhD thesis, Durham University, 2010.

51. Paul Feyerabend, *Farewell to Reason* (London: Verso, 1987), 1.
52. Feyerabend, *Farewell to Reason*, 3.
53. Feyerabend, *Farewell to Reason*, 3.
54. *Against Method*, 3rd ed., 154.
55. Feyerabend, "How to Defend Society against Science", 4.
56. Feyerabend, *Conquest of Abundance*, 159 and 241.
57. See Paul Feyerabend, *Einführung in die Naturphilosophie*, edited by Helmut Heit and Eric Oberheim (Frankfurt am Main: SuhrkampVerlag, 2009). The "rise of rationalism" is discussed further in Helmut Heit, "Reasons for Relativism: Feyerabend on the "Rise of Rationalism" in Ancient Greece", and John Preston, "The Rise of Western Rationalism: Paul Feyerabend's Story", both *Studies in History and Philosophy of Science* 57 (2016): 70–86.
58. *Conquest of Abundance*, 261–262.
59. See, e.g. Theodor Adorno and Max Horkheimer, *Dialectic of Enlightenment*, trans. J. Cumming (London: Verso, 1979); Martin Heidegger, *The Question Concerning Technology, and Other Essays*, trans. William Lovitt (London: Harper, 1977).
60. Feyerabend, *Conquest of Abundance*, 264.
61. Paul Feyerabend, *Three Dialogues on Knowledge* (Oxford: Blackwell, 1991), 76.
62. Douglas, *Science, Policy, and the Value-Free Ideal*, 49.

CHAPTER 4

Looking for the Bad Teachers: The Radical Science Movement and Its Transnational History

Simone Turchetti

Science studies scholars have gone a long way in reconstructing their discipline's past, but as Sergio Sismondo and others have argued, the reconstruction of its recent history has been overlooked.[1] What has yet to be examined, in particular, is how the political tensions of the late 1960s informed academic work and new approaches. A relatively recent article by Jon Agar reflects on the sea change that, in that decade, led to led to the transformation of the scientists' relationships with the state and the public.[2] But its ramifications into the study of science have not been sufficiently explored. Even more recently, Mary Jo Nye has cast new light on the ascendancy of earlier contributions, such as that of Michael Polanyi, on scholars interested in understanding science as a social phenomenon (or construct) in the 1960s.[3] Yet, her compelling narrative does not pay sufficient attention to significant ruptures deriving from the social context

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in which these intellectuals operated, and especially how political activism informed scholarly production.

The radical science movement was a heterogeneous group of scholars who, from the late 1960s, reappraised the role of science in society as part of the broader political agenda of protesters in university campuses and factories. While the presence of a number of science studies scholars in the activists' ranks is well-known, this article documents the connections between academic work and political activism.[4]

Its main argument is that there is an important legacy worth exploring, especially with regard to how activism propelled novel ideas beyond national borders, through the temporary or permanent relocation of radical scholars in new academic settings, the organization of meetings, and collaboration on specific topics. The circulation of ideas, internationally, informed the debate among science scholars on their methods, even if only a few of them openly endorsed radical stances, and among those who did, fewer still continued to be politically active for more than a few years. This chapter shows how this transnational flow informed the emergence of new approaches within the nascent field of science studies, from the sociology of scientific knowledge to Paul Forman's "distortionist hypothesis."[5]

Drawing on a transnational history methodology, the chapter identifies key transitions at national level and how these changes influenced debates in other countries.[6] The article focuses on three historical cases in Italy, Britain, and the USA, since important connections existed between their scholarly communities. Firstly, their governments had important political and military ties in the Cold War conflict.[7] Furthermore, in the mid-1960s, British and Italian science policy makers criticized their American counterparts for the imbalance between the USA and Western Europe in the funding of novel research, a criticism encapsulated in in the "technology gap" debate and the migration and the migration of European scientists to the USA (the so-called brain drain).[8] Finally, as Italian and British governments adapted R&D policies popular in the USA, this offered comparable ammunition to the critics of contemporary science and technology in each nationally specific setting. These similarities helped activists to forge transnational links and exchange ideas.

1968 and the Radical Science Movement

A radical critique of contemporary science emerged as a consequence of the university protest movement that peaked in the 1968 campaigns. The protest started in the early 1960s in US campuses and spread in the following years, culminating with the events of that critical year.[9] In March, New York University's students demonstrated against Dow Chemical, one of the manufacturers of the napalm—or Agent Orange—used in Vietnam. From April the protest moved to Columbia University, and in August the Democratic National Convention in Chicago was disrupted by five days of antiwar demonstrations.[10] In Western Europe, the students of the University of Madrid rallied against the dictator Francisco Franco and those of the University of Rome "La Sapienza" battled with the police in what is remembered as the largest revolt in Italian campuses to date. On March 17, an antiwar demonstration in Grosvenor Square, London, ended with 86 people injured and 200 demonstrators arrested. Another protest against the Vietnam War ended with an attack on the British Defense Secretary, the Secretary of State for Education, and the Home Secretary. In May, the students of the University of Paris famously halted teaching and research, and eventually French workers joined the protest, bringing the whole country to a standstill. The Cold War, particularly as exemplified in the proxy war in Vietnam, loomed large in these demonstrations as protesters targeted the superpowers' administrations for the quashing of dissent at home and expansionist ambitions abroad. Even those protesters who openly embraced communism showed their dissent from Soviet leaders, especially when their country's tanks rolled into Czechoslovakia to suppress the Prague Spring.

Along with their political protest, the demonstrations brought to the fore a variety of novel themes, including the new conditions of workers in automated factories, the existence of racial and gender divides in modern society, environmental protection, and the imbalanced development of the nations of the world. Science and technology came under scrutiny too. Although the infamous Agent Orange used in Vietnam was the main bone of contention, the military and peaceful applications of nuclear energy and the Space Race were also targeted. The spreading of new, apparently innocent and progressive, technologies such as the television and the computer was additionally seen by the activists as controversial and worth investigating as well.

Scientists and engineers in Britain, Italy, and the USA played an important role in the protest. Pressure groups such as the Union of Concerned Scientists, the Students for Democratic Society, and the Scientists and Engineers for Social and Political Action (SESPA) were established on American campuses. Their key goal was to promote a new science "for the people," stressing the urgency of designing new knowledge which could address the problems of society at large, and especially those of minorities and disadvantaged groups.[11]

Protest groups in Britain focused especially on atomic, chemical, and biological warfare. The International War Crimes Tribunal of 1966, set up with the assistance of philosophers Bertrand Russell and Jean-Paul Sartre, concluded that using napalm constituted genocide. Two years later, a conference on chemical and biological warfare organized in London ended with the establishment of the British Society for Social Responsibility of Science (BSSRS), which focused especially on the controversial uses of CS gas in Northern Ireland.[12]

In Italy and France, the protest led both to strikes in, and occupation of, research institutions. The campaigners demonstrated against low salary levels and complained that Italian laboratories lacked research freedom. They argued against the spread of what they viewed as the adoption of R&D strategies distinctive of US laboratories and rigid hierarchical structures within research centers. Protesters took over some of these facilities for some months, including the Euratom-sponsored Laboratory of Genetics and Biophysics based in Naples.[13]

Factory workers also showed their dissent and joined forces with the academics. Those in the USA sought to undermine those firms who had contracts with the US Department of Defense. The BSSRS supported the campaign of Mike Cooley, a British activist working at the space industry giant Lucas Aerospace. Cooley and his associates staged protests against the company's plans for redundancies, suggesting that the firm should redirect production toward socially useful commodities instead of space gadgets.[14] Researchers in Rome united forces with the workers of a local electrical engineering company calling for greater equality in salary redistributions.[15]

Protesters agreed that the role of science and technology in modern societies needed to be further explored. They thus examined the work of British scientist John Desmond Bernal, who, in his 1939 *Social Function of Science*, argued for the need to introduce planning in order to end the capitalist corruption of science and bring its pursuit in line with socialist ideals and state planning. Bernal's thinking continued to appeal to grassroots protesters, even if scientific developments in the Soviet Union highlighted senses in which planning on its own could not transform science in a tool for social change. The Cold War only stimulated the growth of a military-scientific complex, but granted no social improvements or egalitarian progress, especially for the citizens of the two superpowers. The inflammatory lyrics of black poet and musician Gill Scott-Heron on the Apollo moon landing vividly illustrated these concerns.[16]

The 1964 volume *One Dimensional Man*, written by the German-born philosopher Herbert Marcuse, was popular among protesters and became a reference textbook for the radical science movement. Drawing on Karl Marx's notion of alienation, Marcuse extended its application to technological commodities such as cars, TVs, and kitchen appliances, claiming that these goods were not paving the way to progress but furthering the exploitation of the lower classes. Other heretical Marxist interpretations of the 1950s also became popular, arguing for a new understanding of the production of science and technology in terms of analysis focused on specific and typical workplace practices. Harry Braverman's *Labour and Monopoly Capital* innovatively configured research activities as a labor process: "how things get into the pipeline" in the words of radical thinker and historian of science Robert (Bob) Young.[17] In Italy the essays of Raniero Panzieri, a Sicilian and socialist émigré employed by the car manufacturer FIAT of Turin, appraised the communist dogma that ownership of the means of production granted social progress. His analysis of how machines could redefine the relationships between workers and employers pointed once again to the key role of scientific and technological advance in shaping power relations in the factory and society more generally.[18] Protesters also debated the issue of science and development, while Barry Commoner's *Science and Survival*, published in 1966, famously brought to the fore the question of environmental awareness and protection.[19]

Radical science groups tackled all these issues outside the circuit of traditional academic presses and produced their own newsletters such as SESPA's *Science for the People* and the BSSRS's *Radical Science Journal* (later *Science as Culture*). Debating and writing also helped more precise definition of the meaning of "radical" as (according to historian of biology Garland Allen) "deeply critical of any establishment or *status quo*," although for some radicals the focus was more on the critique of science in capitalist countries than on the establishment as a whole. Throughout their publications the activists rethought the origins and current circumstances of science and technology, which had important repercussions for the historical understanding of science, a theme of interest to activists and scholars alike. It was especially through the settling of a number of American scholars in Britain that this debate led to important historical developments in British universities.

American Radicals in Britain

From the 1930s the foundations of science had been at the center of an important debate in Britain, thanks to leftist scientists like Bernal, whose perspective was opposed by Polanyi's own on the freedom of science and scientists.[20] Britain also hosted a small but vibrant group of Marxist historians of science, some of whom, such as Samuel Lilley, were marginalized within the scholarly community but continued to elaborate critical views throughout the 1950s.[21] In the late 1960s, however, the study of science by and large propounded an optimistic view of modern science and ignored its more controversial contemporary applications. Following the students' protest of 1968, BSSRS activists elaborated new analyses on the development of contemporary science, thus prompting other scholars to reconsider their stances. The presence in Britain of a number of American radical scholars that had already witnessed the turmoil in the USA and had recently left the country played an important role in these debates.

Some, like mathematician Jerry Ravetz, had moved to Britain already in the late 1950s as a consequence of the rampaging anticommunism stirred by US Senator Joe McCarthy. Appointed by philosopher Steven Toulmin at the University of Leeds, Ravetz became founding member and eventually head of the university's Division of History and Philosophy of Science.[22] Others, such as the University of Harvard's graduate Gary Werskey, had worked with historian of biology Everett Mendelsohn, a SESPA activist and one of the most significant contributors to science studies from a radical perspective in the USA. Partly because of Mendelsohn and the growing interest in radical themes at Harvard University, in 1967 a workshop entitled *Social Problems of Contemporary Science* had taken place there, helping to focus and give resonance to the issues of interest to protesters.[23] By then, US scholars were also debating the consequences of the expansion of large-scale research facilities in "big science," something that in 1961 physicist Alvin M. Weinberg had indicated as pathological, and which historian of science J. Derek de Solla Price went on to study extensively.[24]

The presence of American scholars in Britain fostered the debate on science (both big and small) as the subject of political and academic work. Werskey and Ravetz joined the BSSRS and worked with its activists to the completion of new studies. One important result of these synergies was the 1969 book *Science and Society*, written by two British members of the society: Hilary Rose, professor of sociology at the London School of

Economics, and Steven Rose, who taught biology at the Open University. Written in an effort to produce a new narrative for the emergence and development of modern science encapsulating the views defining their authors' political experience, its completion was also made possible by their exchanges with Werskey and Tony Benn, a leading left-wing figure in the Labor Party.

Rose and Rose argued that a substantial change had taken place in the evolution of modern science, which had ceased to be a quiet backroom activity of a cultured few and had become a major industry.[25] The historical narrative centered on the notion of non-neutrality of science—the idea that science is never "done in the abstract, in a vacuum, but in a context which places value judgements upon its goals".[26] This notion had originally found a space in the prewar debates on science but was no longer used by the representatives of the progressive left.[27] Actually, by the mid-1960s they were still hoping to address the social problems of science through the establishment of the "science of science," a new discipline defining a scientific approach to science policy.[28] In contrast, *Science and Society* revisited the history of modern science, recognizing that exactly because science is non-neutral, scientists had been progressively losing their autonomy, thus making their work more responsive to the corporate and state interests. The effect of this transformation was an increasing dependence on external sources of funding combined with the inability of autonomously set research questions.

Roses' précis anticipated the publication of another important book with a similar argument. The 1971 *Scientific Knowledge and Its Social Problems*, written by Ravetz, contended that the scientists' activities entailed "serious social problems of an entirely new order due to the penetration of the industry; the need of technological sophistication and the growing ethical and moral problems it engendered."[29] Ravetz's book led the Physics Department at the University of Leeds to threaten refusal to let their students take history of science courses. Some of Ravetz's colleagues in science faculties considered his book as dangerous and subversive and viewed him as another "bad teacher."[30]

The collaboration between American and British radicals was as fruitful as it was short-lived. Actually, by the time the Roses' and Ravetz's pioneering works were published, the BSSRS was about to collapse. Although its militants agreed that science was non-neutral and had become heavily influenced by the industrial and the military, they disagreed on what was needed to reform it.[31] Ravetz, seeking pragmatically for ways to restore public trust in the work of scientists and to give them a firmer grip on science policies, left the BSSRS and went on to lead, as executive secretary, the newly born Council for Science and Society. The Council's founders, physicist John Ziman and molecular biologist Michael Swann, featured prominently in the British scientific community. In 1968 Ziman had published *Public Knowledge: An Essay Concerning the Social Dimension of Science,* in which it was emphasized that the current problems in science could be overcome by establishing more successful ways to communicate its achievements to the wider public. Now both Ravetz and Ziman looked at Michael Polanyi's work as a key reference to analyze what kinds of reforms were needed.[32]

By contrast, in a hypothetical parliament of science reformers, the Roses positioned themselves to the left of Bernal. They claimed that unless a serious analysis of the role of science in capitalist societies was carried out, any remedial action would not solve the problem. They thus criticized the Council, which they viewed as a restoration project. It was born, they claimed, as a forum from which former members of the BSSRS "could preach unhindered the doctrine of the neutrality of science."[33] By then some of the universities in which the BSSRS had originally found support started to offer other interpretations countering radical propositions. This proved decisive in carving institutional space for novel departments devoted to the study of science and its current problems from a non-radical perspective.

This was especially the case at the University of Edinburgh, where Swann, who was its principal and vice-chancellor, sponsored the establishment of a Science Studies Unit (SSU).[34] 1969 former British Broadcasting Corporation science educator David Edge was appointed as new SSU director, and a rich endowment allowed hiring scholars as members of its staff. A radio astronomer by training, in 1959 Edge had moved into science journalism and ten years later succeeded in recruiting a number of young scholars who could further examine, from a novel perspective, the structure, norms, and values of scientific communities. The team included Barry Barnes (a molecular biologist-turned-sociologist of science) and David Bloor (an experimental psychologist). Edge had also appointed Werskey, who aside from helping the Roses, had been busy with his doctoral project on those leftist scientists, who, like Bernal, had been active in interwar Britain.[35] But Werskey, possibly because of the tensions between BSSRS militants, soon left Edinburgh and moved to Imperial College in London. He was replaced by another American scholar, the University of Pennsylvania graduate Steven Shapin, who ended up researching Edinburgh's nineteenth-century phrenology following the completion of his doctoral thesis on the early Royal Society of Edinburgh.[36] His study paved the way to a distinguished career in the history of science, typified by many pioneering contributions including the widely acclaimed *Leviathan and the Air Pump,* written together with the University of Cambridge historian Simon Schaffer.

From the 1970s SSU scholars introduced more sophisticated versions of the non-neutrality concept, thus attracting the interest of those interested in the study of science while maintaining some distance from the political arena in which the BSSRS activists had been operating until then.[37] Drawing on Karl Mannheim's *Ideology and Utopia*, which contributed to establish sociology of knowledge as a field of academic enquiring, Barry Barnes's approach mandated explaining historical change in science as a function of the redefinition of social norms.[38] In so doing, if on the one hand it reaffirmed science's non-neutrality, on the other hand it downplayed specific circumstances deriving from the rise of modern industry that hinged upon it. Notably, Barnes's analysis of recognition as the currency of scientific societies minimized the centrality of external factors in the production of scientific knowledge.[39] This was something that did not go unnoticed among those who, instead, wanted to keep the notion of non-neutrality firmly within the political arena. According to the historian of science and ex-BSSRS activist Brian Martin, Barnes "was losing touch with the flesh and blood of the struggle in and over science."[40]

It was especially the foundation of the sociology of scientific knowledge (SSK) that left former protesters unimpressed. The SSK, according to Martin, was now refashioned as a "history of ideas, a history of scholastic traditions, with no suggestions that it is also a history of social movements and class, gender and racial struggle."[41] Yet others, like Shapin, eventually pointed out that these novel approaches were not necessarily rooted in an opposition to radical stances, but in the effort to "purify" scholarly work due to disillusionment with the post-1968 trajectory of the radical science movement.[42]

The polemic on the BSSRS's future eventually calmed down partly because a reformation of science studies curricula was by then already ongoing in several research centers, including, for instance, the Liberal Studies in Science Department of the University of Manchester.[43] At the University of Sussex's Science Policy Research Unit (SPRU), the polemic continued but also led—pragmatically—to policy responses to the problems raised by the protesters. Established in 1966 by economist Christopher Freeman, the unit promoted an optimistic vision of science and technology presented as engines of economic growth, a vision encapsulated in Freeman's *Economics of Industrial innovation*.[44] Yet, the SPRU hosted a number of radical thinkers, whose work challenged conservative views. According to former Harvard (and Cambridge) scholar Roy MacLeod, the SPRU forged an alliance between classic liberals and Marxists.[45] Even the unorthodox philosopher Paul Feyerabend spent some time at the University of Sussex writing *Against Method*.[46]

Some scholars continued to develop unusual trajectories to carry forward political agitation work. In 1972 the Cambridge BSSRS section was reformed as *Science for the People* group first and then as *Radical Science Collective*. Its key figure, American radical scholar and University of Rochester graduate Robert (Bob) Young was then a lecturer at the University of Cambridge's History and Philosophy of Science Department. Rather than follow the pragmatic approach of his colleagues in Sussex, in 1976 Young left academia to work as an independent scholar. Following the precepts of Harry Braverman, he eventually launched a manifesto-like appeal from the pages of the *Radical Science Journal* arguing that historians of science and technology could gain substantially from radical approaches. What was needed was

> […] further detailed research in the history of science that laid bare science as social relations, from the 17th century onwards; Reconceptualizing the history of technology as a history of choices and social practices, not just as a history of gears, mules and jennys; Re-sieving the best of conventional STS and other scholarship for theoretical insights and agitational use.[47]

Although many radical science activists now wished to carry out analyses and studies outside of academia, at times in opposition with it, they struggled to inform scholarly debates as much as they wished.[48] Moreover, the radical science groups' growing isolation and divisions paralleled the expansion of the Council for Science and Society, whose initiatives ensured sponsorship for a variety of scholarly activities in science studies. One such initiative was an attempt to systematize teaching in UK higher education institutions through the creation of the joint program Science in its Social Context (SISCON) elaborated at the centers in Manchester, Sussex, and Edinburgh. In the 1980s Ziman went on to sponsor high-level political initiatives, including the setting up of Science Policy Support Group, which became the engine of new patronage activities through a new institutional sponsor for research in the social sciences: the Economic and Social Research Council (ESRC). Meanwhile, the establishment of an academic journal devoted to science studies (*Science Studies*, published by Macmillan—later to become the Sage-owned *Social Studies of Science—SSS*) further consolidated the field. The new journal also provided opportunities to reflect upon changes in the national science policies—the case in point being the *SSS* country reports.

The community of British scholars interested in the study of science changed considerably following the emergence of the radical science movement even when the BSSRS lost its appeal. And while the creation of new academic centers devoted to science studies helped to keep radical positions at the fringes of the academic debate in Britain, these positions eventually gained popularity in other countries. In particular, it was the collaboration between defeated radical scholars in Britain and those in Italy that set the circumstances for a fruitful exchange.

The British Connections of an Italian Radical Science Scholar

In 1968 no chair devoted to science studies existed in Italian universities, and the few practitioners in the field often worked within the realm of philosophical studies. The proponents of radical stances found little space within academia even when a lively debate on science and its current problems developed in Italy. This debate produced a sensation, particularly due to the publication of several articles by communist physicist Marcello Cini. A friend of the aforementioned neo-Marxist intellectual Panzieri, Cini completed postgraduate studies at the University of Cambridge, where he had studied together with British physicist Paul Dirac. He had also participated in the proceedings of the 1966 tribunal set up by Sartre and Russell.[49] Following the Prague Spring, Cini was ousted from the Italian Communist Party for his condemnation of the Soviet invasion in Czechoslovakia. Together with other expelled party members, he went on to establish a radical collective publishing the monthly magazine (later newspaper) *Il Manifesto*.

In June 1969, shortly after the successful completion of the Apollo 11 mission and the first moon landing, Cini's opinions caused nationwide outrage. While the event was celebrated by Communist Party officials (and those of other parties alike) as a landmark achievement epitomizing the progress of humankind, Cini argued that the mission ought to be understood as a manifestation of the scientific arms race between the USA and the Soviet Union.[50] In an article published in *Il Manifesto*, he claimed that American science policy managers prioritized research that could grant propagandistic returns and be reoriented toward military R&D rather than studies focusing on social problems such as pollution, environmental degradation, and scientific literacy.[51]

Cini's denunciation anticipated a broader historical review on the circumstances of contemporary science that he completed with three colleagues of the Physics Department at the University of Rome. These scientists dug deeper into the issue of whether science was non-neutral and, drawing on Panzieri's neo-Marxist approach, argued for understanding how capitalism shaped the production of scientific knowledge. Published in 1976, their main work *L'Ape e l'Architetto* captured the attention of a number of radical scholars in Italy. The title referred explicitly to a famous sentence in Marx's *Capital* indicating that while "a bee puts to shame many an architect in the construction of her cells," it is the architect that "realises a purpose of his own that gives the law to his modus operandi."[52] As the Italian physicists emphasized, it was exactly because scientific artifacts and theories embodied political purpose that the production of scientific knowledge in a capitalist system was attuned to its values even in those fields where there was no clear "business" dimension. They wrote: "science as commodity production […] serves as a model for the style of work even in fields which are not directly concerned with the production of information for sale, such pure sciences as high-energy physics or biology."[53] This led Cini and his coworkers to a critical appraisal of the development of science in both the Western and the Soviet bloc, as it was naïve to believe that alternative economic systems could coexist and promote diametrically opposite science policies. "Proletarian" science was affected by the same malady as capitalist science; it was prone to large-scale investments and unwilling to promote social advance.

Cini and his co-workers also wished to promote these views abroad, and while completing their study established a fruitful exchange with Steven and Hilary Rose, who meanwhile had attempted to stave off the decline of the radical science movement in Britain by strengthening international collaborations. By 1972, when they started exchanging views with Cini, they had already left the BSSRS, criticizing it for being not sufficiently "socialist." Four years later they edited two volumes with the collaboration of activists from other countries, as result of discussions at meetings in universities, trade unions centers, and community groups in the USA, Britain, France, and Italy. The first one, entitled *The Political Economy of Science*, offered a non-orthodox Marxist interpretation of the role of science in contemporary society, paying attention especially to scientific practices, frontier research fields such as neurobiology, and the ongoing commodification of scientific work. The second volume, entitled *Radicalisation of Science*, promoted instead an analysis of how scientists' movements could transform science into a tool for social change.[54]

Cini and his colleagues welcomed the collaboration with the Roses for reasons similar to those of their British colleagues—namely, an effort to counter the decline of the radical science movement in Italy and inform scholarly debates. In the 1970s, the study of science was not a disciplinary field in Italy, and a small scholarly community gravitated around philosopher Ludovico Geymonat (University of Milan) and his colleague Paolo Rossi (University of Florence). These philosophers, mainly focusing on the history of science, argued that history could shed new light on the unfolding of scientific rationalism and progress. Their "neo-enlightenment" (as they called it) found fertile ground within the Italian Communist Party, then a key player in cultural affairs. Geymonat, a former World War II partisan and party member, framed the social problems of science and technology within a "two cultures" approach, in line with what the party preached. Politicians trained in the humanities struggled to understand science, and recounting its history could convince them about investing more in research.

Unsurprisingly, the post-1968 ferment found Geymonat and Rossi unsympathetic. They worried about radical interpretations casting a negative light on science. As Giuliano Pancaldi has recently argued: "higher and higher dykes were built to protect what was perceived as the besieged citadel of science, and the history of science itself, from what was felt as, and occasionally was, an extremely dangerous flood."[55] In an effort to "protect the citadel," however, Geymonat and his pupils eventually propagandized the merits of Soviet-styled dialectical materialism. An edited volume published in 1974 documented their exchanges with Soviet colleagues across the Iron Curtain and their effort to popularize Soviet views.[56]

Thus when *L'Ape e L'Architetto* was published, it became a target for the Italian 'diamats' (the followers of dialectical materialism) since it portrayed contemporary science as warped by Cold War interests in both Western and Eastern blocs. Marxist philosopher Lucio Colletti drew first blood by making pedestrian remarks about non-neutrality being a fantasy. Gravity, he claimed, affected falling bodies in the same way in capitalist and socialist countries.[57] His colleague Giulio Giorello followed suit, dubbing Cini and his colleagues as "Sunday epistemologists," thus alleging that since they researched physics during working days, their analyses of philosophical issues were amateur-like. Geymonat criticized Cini too, arguing that his conception of science was deceptive. Science, he argued, offered "increasingly refined tools to analyze our society and to consider the origins of its contradictions."[58]

The altercation between Cini's and Geymonat's groups was rooted in alternative political propositions. The diamats were endorsed by the Italian Communist Party while Cini's coworkers had ties with its competitors on the left. But it also reflected the effort to keep the Italian centers devoted to the study of science under tight control. Pancaldi has aptly renamed their altercation as the Italian Science Wars. Cini's colleagues continued to carry out their studies in physics departments, while history of science was identified as the disciplinary area associated with the philosophers' studies of science. With the 1980s now approaching, radical thinkers were defeated, although international collaborations helped them to continue debating and exchanging ideas. The marginalized British and Italian scholars found opportunities to travel to the USA where, in quite exceptional circumstances, their inputs would play an important role in forging groundbreaking scholarly work.

Europeans in the USA (and David Noble)

Together with Everett Mendelsohn, many others scholars, including Richard Lewontin, Richard Levins, Lev Levidow, and Garland Allen, continued propounding radical views in the USA throughout the 1980s. So when radicalism was on its descending curve in Europe, it still held strong in some academic centers across the Atlantic. Its longevity was partly a response to Ronald Reagan's presidency as the new US president took the Cold War conflict to a new height and favored the growth of a military-industrial complex, at times through controversial schemes such as the ill-fated 1983 Strategic Defense Initiative (SDI) (or Star Wars).[59] He also supported the equally doomed Superconducting Super Collider (SSC), despite its rising costs and public criticism. In this new funding climate the relations between military, science, and technology propelled new studies aimed at charting the circumstances of defense-oriented research and rejuvenating earlier radical science production. Foreign radical scholars, such as those from Britain and Italy, continued to entertain connections with US academics and visit their research centers. In turn, their presence and ideas informed the production of new narratives.

The case in point is the scholarly work of historian of technology David Franklin Noble. Born in New York, Noble united the impetus of his activism with a momentous intellectual production. Formerly a member of the Students for Democratic Society at the University of Rochester (where Bob Young had also trained before moving to Britain), in 1979 he completed his first book *America by Design. Science, Technology and Corporate Capitalism*. Reappraising traditional histories of technology and drawing on Lewis Mumford's work, Noble showed that the development of science and technology in America was intertwined with the vested interests of the US industry. In particular, he focused on how industrial magnates lobbied for the reformation of scientific and industrial standards, the definition of new patent legislation, the tying of university research into industrial research, and the training of new "scientific manpower" to be employed by the industry.[60]

Noble's 1984 *Forces of Production* went on to argue further that the expansion of computer science and automation evidenced a path of technological development tied to the needs of the US military. The dismissal of *record-play-back*, which memorized and reproduced the worker's movement, and the adoption of *numerical control*, which applied computer-based routines to the production process, demonstrated that the path sponsored by the US Department of Defense had prevailed. Noble claimed that the adoption of new technologies restructured industrial relations in ways that advantaged employers (and military patrons alike).[61]

Noble succeeded in establishing international collaborations and to some extent was responsible for propagandizing, in the USA, the work of European activists. In 1980, the Marxian architects/bees comparison was being reused by aforementioned British campaigner Mike Cooley, who had meanwhile contributed to the Roses' collection *The Political Economy of Science*. Cooley's monograph *Architect or Bee. The Human/Technology Relationship* had—like Noble in the USA, Cini in Italy, and the Roses in Britain—investigated the social consequences of automation, computerization, and the use of robotic devices, especially in factory work. Cooley alleged that this critique was part of a "technological continuum discernible over the last 400 years or so" which had seen technology used to reconfigure industrial relations.[62] Noble was among those who immediately showed an interest in this narrative and sought to introduce Cooley's work to the US public in activists' meetings.

But Noble was notoriously ostracized by US academia. He was refused tenure at the Massachusetts Institute of Technology (MIT) because, as noted by Noam Chomsky, he was "a bit too radical" and even sued the institute's managers in a court case that he lost.[63] When the historian moved to the Smithsonian Institution as a curator, he stirred controversy by proposing an exhibit focusing on resistance to automation. He once again took his employer to court and had to leave, moving to Drexel University the following year.[64] Noble was a typical example of how a radical approach to the study of technology could be rooted in serious scholarly work and yet be marginalized within academia because of its political implications.

But when Noble moved to the Smithsonian Institution, he came to exercise growing influence on the historian of science Paul Forman, with important consequences for the whole discipline and science studies more generally. Forman was not (nor was ever going to be) a radical scholar. But his work in the 1980s was informed by radical ideas as a consequence of the exchanges with Noble—something that is hardly ever fully acknowledged in the literature. First of all, it is important to recall that this was a very fertile period in Forman's career, a time in which the Smithsonian Institution had become a focus for visiting scholars from Western Europe. Some of them had a radical background, such as the physicist turned historian and Cini's coworker Michelangelo De Maria, who visited Forman's institution in 1984.[65] The presence of these scholars stimulated debates at the Smithsonian Institution on many contemporary themes including whether the SSC project should go ahead, the consequences of the SDI initiative, and more generally how the new president Ronald Reagan was busy directing more funds toward defense R&D and basic research in the physical sciences.[66]

It is in this cultural milieu that Forman's distortionist hypothesis first emerged. A number of radical scholars have mistakenly seen his earlier production as exemplifying a radical approach. Martin enlists his 1971 paper on 1920s Weimar physics as one of those inspiring it because in that paper Forman argues that the intellectual atmosphere of Weimar Germany informed its physicists' revolt against classical causality that paved the way to the foundation of quantum mechanics.[67] What was later to be known as "Forman's thesis" resonated with the experience of campus protesters embracing radical science stances. But while there is no doubt that Forman's portrayal of the allegiance and refusal of ideological tendencies by the German scientists was itself informed by his own experiences in the 1960s, Forman was critical of radical tendencies. At the time he was a graduate student at the University of California (Berkeley) but had no sympathy for the “science for the people” approach.[68] Actually, when Forman, then a junior faculty member, met Noble for the first time at the University of Rochester in the early 1970s, he was not sympathetic at all to his political and intellectual orientation either.

Yet he became more interested in Noble’s thinking when he moved to the Smithsonian Institution. Forman’s views on the “distortion” of US science eventually appeared in his 1987 “Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940–1960.” The paper contended that military needs allowed US scientists to gain access to funding and, in turn, molded research directions according to the wishes of their military patrons. It did openly recognize that the research agenda was no longer left in the hands of the scientists but was in those of their patrons. Forman thus neatly reworked something that in the end could be seen as the classical notion of non-neutrality within the new framework of historical studies documenting the growth and transformation of US science as a consequence of military patronage. This was similar to what Noble had done for automation technologies, and in the paper Forman openly acknowledged the importance of Noble’s work in shaping the hypothesis; the historian was “the chief inspiration in its conception and prosecution” of his article.[69]

Historians of science have extensively debated the merits and flaws of Forman’s distortion hypothesis. Yet few appear to have realized that ultimately the paper should be explained as manifesting a legacy with the intellectual production of the radical science movement. That said, toward the end of the 1980s Forman was not alone in pushing toward new directions in US science history. Emphasis on the shaping of science as result of industrial and military patronage appeared in novel works by a number of other scholars, including Robert Seidel, Stuart Leslie, and Daniel Kevles, among others, who also promoted a fruitful debate on Forman’s hypothesis.[70]

Conclusions: Hybrid Knowledge and the Hidden “Radical” Ancestors

As John Krige has recently argued, new knowledge always constitutes a hybrid—the result of the encounters between a variety of knowledge-makers, and the intermingling (or “mongrelisation”) of their original ideas. Examining the hybridization process, Krige stresses, “creates a space for a history of science that recaptures the dynamic interaction between diverse and interconnected sites of knowledge-making."[71] Yet—surprisingly—the self-reflexive effort to examine this interaction in science studies and between practitioners devoted to the production of new theories on the development of science has hardly ever been attempted.

This chapter has endeavored to show the merits of such a project. Some of the approaches and ideas that science studies scholars routinely use today in the classroom and in novel research—from the formulation of SSK to Forman's distortionist hypothesis—are the product of many episodes of mongrelization. Looking at some episodes in three Western countries, Britain, Italy, and the USA (but presumably one could find many more in other countries), I have shown how important the international exchange of ideas between radical intellectuals was to scholarly production in science studies. American émigré scholars in Britain contributed to develop some of the innovative interpretations emerging within the radical science movement, especially on the non-neutrality of science. The collaboration between Italian and British radical scholars helped to advance these novel propositions internationally. The temporary stay of Italian, British, and many other scholars in the USA at the time of the Reagan administration brought further attention and analytical extension to these radical stances.

This transnational flow of radical knowledge led to revisiting and reinterpreting original propositions emerged back in the students' turmoil of the late 1960s in a process of hybridization. The ostracism of radical positions in academic departments made radical scholars more eager to establish international collaborations through which analyses of the contemporary problems of science could proliferate. The ensuing international debate on science and its problems informed institutions and scholarly work in science studies, even if not necessarily because the academics agreed with the radicals. In the case of SSK, the drive to overcome radical predicaments on the non-neutrality of science stimulated the search for novel analyses and theories. It is beyond the ambitions of this paper to establish if SSK represented a move forward in the direction of scholarly research on science, or one backward in the process of critiquing the contemporary problems of science (both propositions may, of course, be true). Yet it is equally important to highlight an overlooked genealogy in the history of SSK. In the case of the distortionist hypothesis the climate of collaboration typifying the Smithsonian Institution in the Reagan era stirred more "mongrelization." Thus Noble's understanding of the development of automation technologies (itself a product of radical thinking) could definitively inform Forman's analysis of military patronage as a key determinant in the trajectory of new scientific disciplines.

Without the input of American radical scholars in Britain, SSK would arguably have been far different from what it is, and Forman would not have come up with such a vivid portrayal of the impact of military patronage on scientific research. Forman's main source of inspiration, Noble, probably would not have elaborated an interpretation of the shaping of automation technologies without the exchanges with Mike Cooley. Nor would have the Italians historians learned to appreciate science studies as they now do without their "indigenous" Science Wars (which were far from being just an Italian affair).

If international exchanges forged new concepts in the study of science, it was partly as a result of the stimulating intellectual production of the radical science movement, which in turn resulted in a process of knowledge hybridization. While this hybridization did not necessarily entail the sharing of political views and aspirations, it was decisive in forging new approaches.

This is not to deny that the radical science movement failed to grasp some of the key transformations that would completely change, in a not too distant future, the science they were busy portraying. Radical thinkers urged a reform of science in a world divided in two blocs, but the collapse of the Soviet Union and the fall of the Berlin Wall was something that they failed to consider. And their bleak forecast of a monolithic computing industry enslaving humans and assembled around a few mainframe processors did not take into account the rise of micro- and home-computing first and the internet (although itself a product of the military-industrial complex) afterward. Furthermore, they underestimated the robustness and flexibility of academic institutions in countering radical views, and the ability of their managers to regain control of the cultural debate on science.

But the radical science movement helped the scholars of a number of countries to focus more on the social dimensions and determinants of science. Although more nuanced theories for the ways in which these determinants operate in the scientific domain have recently emerged, these "bad teachers"(as the radical scholars were known in Italy) deserve more attention. The field of science studies as we know it probably would have been dramatically different without their works.

Notes

1. For an overview see Sergio Sismondo, "Science and technology studies and an engaged program," in *The Handbook of Science and Technology Studies*, edited by Edward J. Hackett, Olga Amsterdamska, Michael E. Lynch, and Judy Wajcman (Cambridge, MA: MIT Press, 2007), 3–32.
2. Jon Agar, "What Happened in the Sixties?" *British Journal for the History of Science* (*BJHS*) 41/4 (2008): 567–600.
3. Mary Jo Nye, *Michael Polanyi and His Generation. Origins of the Social Construction of Science* (Chicago and London: University of Chicago Press, 2011), xix–xxi.
4. There are radical thinkers who have recounted their past experience. See, for instance: Garland E. Allen and Roy M. MacLeod (eds.), *Science, History and Social Activism. A Tribute to Everett Mendelsohn* (Dordrecht: Kluwer Academic, 2001). Gary Werskey, "The Marxist Critique of Capitalist Science: A History in Three Movements?" *Science as Culture* 16/4 (2007): 397–461.
5. This doesn't mean, however, that this transnational flow was the unique determinant for this emergence or responsible, on its own, for a number of "turns" in the research areas we currently associate with science studies.
6. On transnational history of science see Simone Turchetti, Néstor Herran, and Soraya Boudia, "Have We Ever Been 'Transnational'? Towards a History of Science Across and Beyond Borders," *BJHS* 45/3 (2012): 319–339. On the construction of new knowledge through circulation see also Kapil Raj, *Relocating Modern Science: Circulation and the Construction of Knowledge in South Asia and Europe, 1650–1900* (New York: Palgrave Macmillan, 2010).
7. See on this John Krige, *American Hegemony and the Postwar Reconstruction of Science in Europe* (Cambridge, MA: MIT Press, 2006).
8. On brain drain see Matthew Godwin, Jane Gregory, and Brian Balmer, "The Anatomy of the Brain Drain Debate, 1950–1970s: Witness Seminar," *Contemporary British History* 1 (2009): 35–60. On the technology gap see: Carroll W. Pursell, *Technology in Postwar America: A History* (New York: Columbia University Press, 2007), chapter 7.
9. On its origins at the University of California (Berkeley) see Massimo Mazzotti, "Fifty Years of Free Speech at Berkeley," *Times Higher Education* 20 November 2014 (available at: http://www.timeshighereducation.co.uk/features/fifty-years-of-free-speech-at-berkeley/2016966.fullarticle).
10. On the events see: David Farber, *Chicago '68* (Chicago: University of Chicago Press, 1988).
11. "Introduction" in H. Rose and S. Rose, *The Radicalisation of Science* (London: Macmillan, 1976), xvi. On "Science for the People" see especially the set of documents collected on the occasion of the conference

"Science for the People: the 1970s and Today," available at: www.science-for-the-people.org.

12. H. Rose and S. Rose, "The Radicalisation of Science," in Rose and Rose, *The Radicalisation of Science*, 1–31, on 20. See also Alice Bell, "Beneath the White Coat: the Radical Science Movement," *The Guardian* 18 July 2013; On BSSRS see Alice Bell "Science for the People!" *Mosaic. The Science of Life* (available at: mosaicscience.com/story/science-people).
13. Mauro Capocci, "Politiche e Istituzioni della Scienza: Dalla Ricostruzione Alla Crisi", in C. Pogliano and F. Cassata, *Storia d'Italia. Annali 26.Scienze e culturadell'Italiaunita* (Turin: Einaudi, 2011), 266–294, on 280.
14. Werskey, "The Marxist Critique of Capitalist Science: A History in Three Movements?" p. 432.
15. LASER (ed.), *Valle Giulia e La Luna. Lotte dei Tecnici e Critica della Scienza* (Rome, 2000), p. 76.
16. "I can't pay no doctor bills/But whitey's on the moon/Ten years from now I'll be payin' still/While whitey's on the moon." Gil Scott-Heron, "Whitey On the Moon," in *Small Talk at 125th and Lenox*, Flying Dutchman/RCA, 1970.
17. Bob Young, "Introduction," in Les Levidow (ed.). *Radical science. Essays* (London: Free Association Books, 1986), pp. 1–14, on 5–6. The neo-Marxist economist Paul Sweezy exercised growing influence on US protesters. See Garland E. Allen, "Radical Politics and Marxism in the History of Science," in Allen and MacLeod (eds.), *Science, History and Social Activism*, 185–202, on 188.
18. See Raniero Panzieri, *Lotte Operaie Nello Sviluppo Capitalistico* (Turin: Einaudi, 1976). Panzieri's work was never translated in English, but is analyzed in Steve Wright, *Storming Heaven. Class Composition and Struggle in Italian Autonomist Marxism* (London/Sterling (VA): Pluto Press, 2002), 15–46.
19. On Commoner see: Michael Egan, *Barry Commoner and the Science of Survival: The Re-Making of American Environmentalism* (Cambridge, MA: MIT Press, 2007). See also Jerry Ravetz, *Scientific Knowledge and Its Social Problems* (Oxford: Oxford University Press, 1971), 425.
20. On this see Gary Werskey, *The Visible College. The Collective Biography of British Science Socialists in the 1930s* (London: Allen Lane, 1978). The Polanyi-Bernal debate is discussed in Nye, *Michael Polanyi and His Generation*, 218–222.
21. On Lilley see Vidar Enebakk, "Lilley Revisited: On Science and Society in the 20th Century," *British Journal for the History of Science* 42/4 (2009): 563–595.
22. Graeme Gooday, "History and Philosophy of Science at Leeds," *Notes and Records of the Royal Society* 60 (2006): 183–192.

23. Garland E. Allen and Roy M. MacLeod (eds.), *Science, History and Social Activism. A Tribute to Everett Mendelsohn* (Dordrecht: Kluwer Academic, 2001).
24. Alvin M. Weinberg, "Impact of Large-Scale Science on the United States," *Science* 134 (1961), 161–164; John Derek de Solla Price, *Little Science, Big Science* (New York: Columbia University Press, 1963).
25. Hilary Rose and Steven Rose, *Science and Society* (London: Penguin Books, 1969), xvii.
26. Ibidem, 262.
27. It was especially the 1931 International Congress of the History of Science in London that had allowed distinguishing between a corrupted capitalist science and a progressive one configured as proletarian. But, as Rose and Rose have recently argued, at the onset of the Cold War, Bernal and others in Britain "retreated to the conservative ideology of science as a neutral pursuit, to be used or abused by society." See Hilary Rose and Steven Rose, *Genes, Cells and Brains. The Promethean Promises of the New Biology* (London: Verso, 2013), 5. A vivid account of the importance of declaring science non-neutral in BBSRS activities is in Alice Bell, "'Science is not Neutral!' Autumn 1970, when British science occupied itself," *The Guardian*, 8 September 2014.
28. A Science of Science Foundation was established in 1964, and both Bernal and De Solla Price featured in its Advisory Council. See Maurice Goldsmith and Alan Mackay, eds., *The Science of Science* (Middlesex: Penguin, 1964).
29. Ravetz, *Scientific Knowledge and Its Social Problems*, 14.
30. John Christie, Personal Communication to the Author, 19.10.2015.
31. See also Werskey, "The Marxist Critique of Capitalist Science: A History in Three Movements?" 431; H. Rose and S. Rose, "The Radicalisation of Science," in Rose and Rose (eds.), *The Radicalisation of Science*, 1–31, on 21.
32. On Ziman see Michael Berry, "John Michael Ziman, 1925–2005," in *Bibliographical Memoirs of the Fellows of the Royal Society* 52 (2006): 479–491, on 486–490. On Polanyi's ascendancy on Ravetz and Ziman, see Nye, *Michael Polanyi and his Generation*, 293–294.
33. Rose and Rose, "The Radicalisation of Science," in Rose and Rose (eds.), *The Radicalisation of Science*, 1–31, on 28.
34. David Edge, "Acceptance," *Science, Technology and Human Values* 19/3 (1994): 366–385, on 366. Always according to the abrasive Roses, the local BBSRS section turned into "a pleasant lunch club activity, unlikely to raise serious critical issue." Rose and Rose, "The Radicalisation of Science," in Rose and Rose (eds.), *The Radicalisation of Science*, 1–31, on 21.
35. Gary Werskey, *The Visible College: A Collective Biography of British Scientists and Socialists of the 1930s* (London: Allen Lane, 1978).

36. Werskey recently claimed that it was the radical Bob Young who "bowled [him] away" from Edinburgh. G. Werskey, "What Made the SSU Special 50 Years Ago", Unpublished typescript sent to the author on 03/05/2016. Shapin's study was published in several articles and books, including Steven Shapin, "Phrenological Knowledge and the Social Structure of Early Nineteenth-Century Edinburgh," *Annals of Science* 32/3 (1975): 219–243.
37. John Henry, "Historical and Other Studies of Science, Technology and Medicine at the University of Edinburgh," *Notes and Records of the Royal Society* 62 (2008): 223–235, on 225. One additional element recalled by many as critical to the SSU and other centers' establishment was the effort to bridge the gap by civil servant C. P. Snow between humanistic and scientific cultures. C. P. Snow, *The Two Cultures* (Cambridge: Cambridge University Press, 1962 [1959]).
38. See Nye, *Michael Polanyi and His Generation*, 279–283.
39. "The existence of the independent reward system gives the scientific community the opportunity to control its own core research activities [...]. It allows scientists to accumulate findings and develop theories without undue regard for whatever economic and political concerns currently predominate in the wider society". Barry Barnes, *About Science* (Oxford: Blackwell, 1985), 48. Barnes was also dismissive of previous attempts by a number of scholars to "connect the rise of science with the rise of capitalism." See B. Barnes, *Scientific Knowledge and Sociological Theory* (London and Boston: Routledge and Kegan Paul, 1974).
40. Brian Martin, "The Critique of Science Becomes Academic," *Science, Technology and Human Value* 18/2 (1993): 247–259, on 248.
41. Ibid., 251. An alternative view on the SSU merits is in Henry "Historical and Other Studies of Science, Technology and Medicine at the University of Edinburgh". See also Massimo Mazzotti, "Introduction," in M. Mazzotti (ed.), *Knowledge as Social Order. Re-Thinking the Sociology of Barry Barnes* (Aldershot, UK: Ashgate, 2008), 1–13.
42. Steven Shapin, "Discipline and Bounding: The History and Sociology of Science as Seen through the Externalism–Internalism Debate," *History of Science* 30 (1992): 333–369. See also Werskey, "The Marxist Critique of Capitalist Science: A History in Three Movements?" 442.
43. This was mainly thanks to Fred Jevons, whose main contribution to the debate on science was *Science Observed*: *Science as a Social and Intellectual Activity* (London: George Allen and Unwin, 1973).
44. Christopher Freeman, *The Economics of Industrial Innovation* (London: Penguin, 1974). Freeman's team at SPRU went on to elaborate an appraisal of the Club of Rome's recent findings in H. S. D. Cole, C, Freeman, M. Jahoda, and K. L. R. Pavitt (eds.), *Thinking about the Future. A Critique*

of the Limits of Growth (London: Chatto&Windus/Sussex University Press, 1973).
45. Roy MacLeod, communication to the author, 5.3.2012.
46. On Feyerabend see Ian Kidd's article in this volume.
47. Werskey, "The Marxist Critique of Capitalist Science: A History in Three Movements?"439.
48. G. Werskey, "The Social Context of Science," in Allen and MacLeod (eds.), *Science, History and Social Activism. A Tribute to Everett Mendelsohn*, 203–214, on 206.
49. Marcello Cini, *Dialoghi Di Un Cattivo Maestro* (Turin: Bollati Boringhieri, 2001), 97.
50. Similar positive stances typified the PCI's position on nuclear energy. On this see Capocci, "Politiche e Istituzioni della Scienza: Dalla Ricostruzione Alla Crisi," 276.
51. M. Cini, "Il satellite della luna," *Il Manifesto* , September 1969.
52. G. Ciccotti, M. Cini, M. de Maria and G. Jona-Lasinio, *L'Ape e l'Architetto. Paradigmi Scientifici e Materialismo Storico* (Milano: Franco Angeli, 2011 [1976]). Marx's quote is from K. Marx, *The Capital*, Vol. 1, Chap. 7, Part 1.
53. G. Ciccotti, M. Cini, and M. De Maria, "The Production of Science in Advanced Capitalist Society," in S. Rose and H. Rose, *The Political Economy of Science* (London and Basingstoke: Macmillan, 1976). See also H. Rose, "Hand, Brain, and Heart: A Feminist Epistemology for the Natural Sciences," *Signs* 9/1 (1983): 73–90, on 79–80. In 1976, Rose and Rose's edited collection was translated and published in Italy.
54. Rose and Rose, eds., *The Political Economy of Science*, ix; Rose and Rose, eds., *The Radicalization of Science*, xvii–xviii.
55. Giuliano Pancaldi, "Purification Rituals: Reflections on the History of Science in Italy," in M. Mazzotti and G. Pancaldi, *Impure Cultures. Interfacing Science, Technology and Humanities* (Bologna: Università di Bologna, 2010), 233–248, on 239.
56. Ludovico Geymonat, Giulio Giorello, Enrico Bellone, and Silvano Tagliagambe, *Attualità del Materialismo Dialettico* (Rome: Editori Riuniti, 1974). On the Chinese views see Lucy Gao in this volume.
57. Cini, *Dialoghi Di Un Cattivo Maestro*, 110.
58. Geymonat cit. in Capocci, "Politiche e Istituzioni della Scienza," 291.
59. On this historical transition see Daniel J. Kevles, "Big Science and Big Politics in the United States: Reflections on the Death of the SSC and the Life of the Human Genome Project," *Historical Studies in the Physical and Biological Sciences*, 27/2 (1997): 269–297.
60. David F. Noble, *America By Design. Science, Technology and the Rise of Corporate Capitalism* (New York: Alfred A. Knopf, 1979).
61. D. F. Noble, *Forces of Production. A Social History of Industrial Automation* (New York, Oxford: Oxford University Press, 1984).

62. Mike Cooley, *Architect or Bee? The Human/Technology Relationship* (Boston, MA: South End Press, 1980), 45.
63. On this see Thomas Misa. "David F. Noble, 1945–2010," *Technology and Culture* 52 (2011): 360–372, on 365–366.
64. Misa."David F. Noble, 1945–2010," 366.
65. The visit was not directly connected to the study of radical issues, but it was a follow-up from the 1972 International Varenna school organized at Como Lake on the history of physics in the twentieth century. See Charles Wiener (ed.), *History of Twentieth Century Physics. Proceedings of the International School of Physics, 31 July–12 August 1972* (New York and London: Academic Press, 1977).
66. For an overview see G. A. Keyworth, II, "Four Years of Reagan Science Policy: Notable Shifts in Priorities," *Science* 224 (1984): 9–13. See also Kevles, "Big Science and Big Politics in the United States: Reflections on the Death of the SSC and the Life of the Human Genome Project."
67. Paul Forman, "Weimar Culture, Causality, and Quantum Theory: Adaptation by German Physicists and Mathematicians to a Hostile Environment," *Historical Studies in the Physical Sciences* 3 (1971): 1–115. On its understanding as a radical text see Martin, "The Critique of Science Becomes Academic," 250. On the thesis more generally see Cathryn Carson, Alexei Kojevnikov, and Helmuth Trischler, *Weimar Culture and Quantum Mechanics* (Singapore: World Scientific Publishing, 2011).
68. Paul Forman, communication to the author, 2.3.2012.
69. P. Forman, "Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940–1960," *Historical Studies in the Physical and Biological Sciences* 18: 1 (1987): 149–229.
70. For an overview on the debate on Forman's hypothesis and the debate it produced see Naomi Oreskes, "Science in the Origins of the Cold War," in *Science and Technology in the Global Cold War*, edited by Naomi Oreskes and John Krige (Cambridge, MA, and London: MIT Press, 2014).
71. John Krige, "Hybrid Knowledge: The Transnational Co-production of the Gas Centrifuge for Uranium Enrichment in the 1960s," *British Journal for the History of Science* 45/3 (2012): 337–357, on 339.

CHAPTER 5

Thomas Kuhn's *Structure*: An "Exemplary Document of the Cold War Era"?

Hans-Joachim Dahms

I must confess that I was skeptical from the start when I read some ten years ago that Thomas Kuhn's ideas concerning the development of the sciences were influenced by the Cold War. My immediate reaction to these allegations was surely prompted by the fact that it was a leading member of the Socialist Student Association (SDS in German) in Göttingen who advised me—then a newcomer to the leftist scene—to lay aside for a while all the stuff by Rudolf Carnap and Alfred Tarski that I had been so fond of before 1968 and focus instead on Kuhn's *Structure*.[1]

I then realized, however, that recent work explaining the role of public intellectuals and academics in the USA during the Cold War represents an attempt for some scholars to come to terms with the country's bleak past. And writing as a German I can easily explain how difficult this is. The atrocities of the Nazi dictatorship, especially during World War II and the Holocaust left no one of my generation untouched. I remember

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from my school days that there was resistance to all the revelations about Nazi Germany and many in the elder generation tried to draw a so-called *Schlussstrich* (a line), often even before serious investigations began. Nevertheless, some publications from the late 1940s stand out as a beginning of *Vergangenheitsbewältigung* (coming to terms with the past), like Alexander Mitscherlichs and Fred Mielkes' documentation on the Nuremberg doctors' trial or Eugen Kogon's description of the *SS-Staat*.[2]

Whereas in the communist East the history of the Nazi past of important universities like Jena and Leipzig was already written and published by the late 1950s as official contributions to anniversaries, in the democratic West the contribution to the Nazi atrocities in the academic sphere came into focus much later. Here the first publications about individual universities appeared only in the late 1970s and were often written by critics of the institutions where they were raised. German universities that do not carry fully-fledged investigations into their past in the Third Reich are today the exception and no longer the rule.

In this way historical research has cast a critical light on prestigious German scientists and philosophers. Take the instance of Martin Heidegger, who was still considered by many intellectuals as a leading German philosopher after the end of World War II. The first documentation about his activities as a Nazi activist was published only at the beginning of the 1960s. Today after dozens of books and literally hundreds of articles, the questions being debated are only when his Nazi inclinations started, how intense they were, to what extent and in what ways they also influenced his philosophical work, and when they ended.

So I can now see why in recent years the growing historical interest for the time when the USA plunged into its "dark ages" has returned investigations into the actions and role of its intellectuals and politicians during the Cold War period, and especially during the McCarthy era and the time of the Vietnam War. These enquiries have encompassed an examination of the academic sphere, and one of the best books written on this subject is James Hershberg's biography of US science administrator and Harvard University president James Bryant Conant. Hershberg's book describes the life and work of a leading Cold War figure as an academic, a politician, and a diplomat.[3]

Like in Germany, academics that were formerly portrayed—so to speak—as heroes are now depicted as villains. That now applies to Thomas Kuhn, who was an icon during the student unrest of the late 1960s among liberal intellectuals. Kuhn started his academic career thanks to Conant's help. Now in his book Hershberg did not say much about Conant's disciple, and in any case nothing critical. Yet Kuhn is now being portrayed by other scholars as a sort of Cold Warrior and even, as we shall see, as the "American Heidegger."[4] I will treat these interpretations in the following way: I first discuss recent arguments about Kuhn; then I present an alternative explanation for the presence of some conservative traits in his main work, *Structure*.

Fuller and Reisch on Kuhn

Kuhn's theory of the development of science, as presented in *The Structure of Scientific Revolutions*, was certainly written *during* the Cold War. And this is independent of the question of how you define the Cold War and whether you let it end already in the beginning 1970s (say with the beginning of détente and Willy Brandt's *Neue Ostpolitik* between the two Germanys), or only in the historical turn represented by fall of the Berlin Wall in 1989, or the breakdown of the Soviet Union in 1991. But was Kuhn's theory also *shaped* or at least *influenced*, *informed* by the Cold War? For instance, Stephen Fuller invites us to take Kuhn's *Structure* in this way: "I urge that Structure be read as an exemplary document of the Cold War era."[5]

Now the proof for influences of specific historical factors on the content of a text can be given in different ways. You can lean on the production, the reception, and the content analysis of the work in question or any combination of those factors. Fuller focuses mainly on the production. His argument runs more or less like this: Kuhn's most important mentor was James Bryant Conant, and he was one of the most important intellectual figures in the US establishment during the Cold War. So Kuhn is an ideologue of the confrontation between blocs and his book a document of the Cold War era. I do not find this argumentation convincing. Surely Kuhn was led by Conant in his path away from physics and into the history of science.[6] And also Kuhn's first career step into the society of fellows at Harvard University, which made Kuhn's ideas ripen, was sponsored by Conant.[7] So it is small wonder that Kuhn exchanged ideas before the publication and even asked Conant to write the foreword to his first book *The Copernican Revolution*, and dedicated the second one, *Structure*, to him.[8] But all these (and many more) biographical details are not sufficient for the required proof. This is because it has to be shown in the first place that everything that Conant handled was itself affected by Cold War thinking. Next, it has to be demonstrated that Kuhn took these "contaminated" ideas over from Conant without much changes and that there were no other major influences at play.

Fuller, in his second and more popular book about Kuhn, *Kuhn versus Popper*, sharpened his thesis. But in the volume he used even more doubtful arguments in order to shake Kuhn's prestige. Now it was not just what Kuhn wrote in *Structure*, but also what he *avoided to say in public* about political issues (his "conspicuous silence on the politics of science"[9]) that Fuller interprets as a major cause of his allegiance to Cold War values. Fuller's arguments draw comparisons between Kuhn and contemporary public intellectuals like Noam Chomsky, Daniel Ellsberg, and Michel Foucault as positive examples, and the prominent German philosopher Martin Heidegger as a negative touchstone.

Fuller entitled the whole last chapter of his second book "Is Thomas Kuhn the American Heidegger?"[10] The parallel is meant to suggest a positive answer to that question and with it comes a negative evaluation of Kuhn because of Heidegger's role in Nazi Germany. Fuller actually thinks Heidegger had a *negative* responsibility for not having spoken out against National Socialism. These are the last sentences of the book:

> […] the bare fact that Kuhn, again like Heidegger, requires special dispensation from negative responsibility does not speak well about the times in which he lived. Heidegger's defenders derive considerable rhetorical support from an image of Nazi Germany as so oppressive that it could deform so profound a thinker as Heidegger. Is perhaps some similar argument about Cold war America required to add moral ballast to Kuhn's silence? The lack of such an argument to date suggests that we have yet to assess the full moral cost of claiming that Kuhn flourished in—and not in spite of—Cold War America.[11]

I think this comparison is completely misconceived, even when for the sake of argument one grants the legitimacy of cross-country and cross-historical parallels: Heidegger did not have a *negative* responsibility for *staying silent* in the Third Reich, but, to the contrary, for *having spoken out* on many occasions as the leading propagandist for National Socialism in the academic sphere after Hitler's rise to power.[12] Whoever the (unnamed) people may be who tried to defend Heidegger adducing the oppressiveness of the Nazi regime as reason for his "deformation," they too are completely wrong. He performed all his propagandistic activities without being forced. Quite to the contrary, he had the ambition to become the state-acclaimed leading philosopher and, as such, become the spiritual *Führer des Führers*. It is true that Heidegger also remained silent for a while. But that occurred *after 1945* when he was unable to cope with his Nazi past. That was another of his moral failures. On the contrary, whether Kuhn indeed stayed silent in the late 1960s and early 1970s is something we shall see in what follows next.

I cannot and need not treat here the three contemporaries (Chomsky, Ellsberg, Foucault) that Fuller compares with Kuhn, because it seems a bit far-fetched to demand the outstanding lifelong activity of these public intellectuals from everyone in the academic sphere, be she/he as successful as she/he may be. The parallel drawn by Fuller with Daniel Ellsberg is especially illuminating. Fuller writes about him in his comparison with Kuhn:

> One prominent example was the scandal associated with the *Pentagon Papers*, classified documents about the Vietnam War that were passed to the New York Times in 1971 by the prominent decision theorist, Daniel Ellsberg. However, Kuhn was never in danger of crossing that line.[13]

Fuller seems to think that Kuhn should have done something similar in the 1960s. How could Kuhn have achieved that? One precondition for the publication of the papers was that after his academic career Ellsberg joined the Pentagon. Otherwise he never could have gained access to the papers and served as a whistleblower. Could and should Kuhn have tried to do the same? Would the Pentagon in turn have Kuhn's application accepted? Did the Pentagon employ historians of science at all?

But what most people interested in Kuhn don't seem to know is: was he prepared to "cross the line" from science to politics? Kuhn was not especially keen to play the role of a public intellectual. But when he did, he hardly ever sided with Cold Warriors. For example, whereas Kuhn's former sponsor Conant was convinced "that science would not be deformed by military funding," Kuhn had reached the opposite conclusion.[14] Moreover, at the beginning of the 1970s he interrupted his studies for a while in order to take forward a political reform of Princeton University, which led him to a position closer to those of students protesting in the university campus.

This fact is evident from the following episode: Kuhn was for a while engaged in a correspondence with the Hungarian philosopher Imre Lakatos about publishing proceedings of the famous 1965 conference at the London School of Economics (LSE).[15] In one letter Lakatos had in passing complained about "the most frustrating term of my academic career, because of the student disturbances here…" and continued, "However, it seems now that, simply by calm and firm stand, we defeated the first wave of the 'revolution' and, indeed, so much so, that we do not expect now any serious further trouble for a very long time to come."[16]

Lakatos had indeed published his open letter to the LSE president in order to counter the rising influence of students in academic affairs. He criticized in that letter the confusion of the students' *right of exercising criticism* with "demands for student *power* [...] concerning appointments, establishing new chairs, positions, designing syllabuses and, in general, concerning the content of teaching and research."[17] Lakatos found these demands "completely unjustified." But Kuhn took up the remark, claiming that he was prepared to take some of the worries of the protesters very seriously:

> Now it is Princeton that is politicized, and not just the students. I am one of a number of members of this faculty who find it very hard to imagine going altogether back to business as usual while the country remains in its present state. One indirect result of this is that I have just accepted an invitation to head up a major University committee whose assignment is to study the role of government funding of on-campus research and to recommend changes, if necessary, to tie us less closely to the military and policy-making establishment. The job starts at once, and I have cancelled the remainder of my leave and also most of my summer plans in order to take it up.[18]

That committee is named in the archival files of the university as the "Kuhn Committee" after the name of its chairman. It was formed after Richard Nixon's televised speech of April 30, 1970 in which he announced his intention to invade Cambodia and the subsequent shooting of four student protesters at Kent State University demonstrating against this escalation of the Vietnam War. A university council at Princeton set two main tasks for the committee to evaluate: "1) that the University refuse to accept any outside funds for research on campus which is directly and specifically related to weapons and weapons system. 2) That Congress be asked to channel all funds in support of research to universities through civilian departments and organizations such as H.W.W. and the N.S.F. rather than the Department of Defense."[19]

As historian Sarah Bridger has shown in her PhD dissertation and her recently published book, Kuhn was elected chairman of the committee, because "he embodied the committee's mix of scientific expertise and social criticism."[20] The work of the committee cannot be described here in detail. It started with a scrutiny of all military-related research on the

Princeton campus and also asked for information about defense contracts at other universities like Berkeley, Stanford, and Cornell.[21] It discussed important questions prompted by these findings like the difficult distinction of pure and applied research and the "drift" prompted by outside funding, especially in applied sciences. Other questions like whether a restriction of military research might restrict academic freedom and what the purpose of a university in 1970s America should be were also discussed.

The outcome was less spectacular, though. The Kuhn Committee explicitly criticized only the university's renting of its Institute for Defense Analyses to the military and the presence of a classified library on campus and recommended that these facilities be withdrawn. According to Bridger, the advice given by the committee in the end "failed to satisfy many committee members, however, most notably Kuhn himself."[22] And that is perhaps a reason why in his writings and interviews Kuhn never came back to his experiences with the committee. Another reason might have been that applied science and, more specifically, military research and development did not belong to Kuhn's idea of normal science. His former student Paul Forman nevertheless pursued exactly that theme.[23] To sum up this episode, Kuhn was indeed prepared to "cross the line" into political discussions on science, but was perhaps less successful in that endeavor than other public intellectuals of the 1960s and early 1970s.

However, Fuller not only argues that Kuhn was a Cold Warrior, but in his second book he asserts that it was Karl Popper rather than Kuhn who played a role as a progressive leader in the philosophy of science. But in this respect Fuller does not seem to interpret Popper's work accurately enough. He was possibly misled by the fact that Malachi Hacohen's superbly written biography on Popper covers only the first half of his life, including his socialist beginnings.[24] But from 1945 onward, Popper's political orientation was very different. *The Open Society and its Enemies* served, after the defeat of Nazi Germany and its allies, as a sort of ideological document on the Western side of the Cold War (much more, in any case, than everything Kuhn has ever written).[25] Popper had meant it that way, and it was received so on the Eastern side of the Iron Curtain as well, especially in the former German Democratic Republic.[26]

So I would argue that Fuller's thesis should be turned upside down, and I would not keep writing about the issue of Kuhn's role in the Cold War any longer if not for the recent work of a scholar whom I take more seriously than Fuller—namely George Reisch. I find it completely understandable and justified that Reisch, who has published a very good book on the development of philosophy of science during the Cold War up to 1960,[27] now wants to know more about how the Cold War shaped that philosophical discipline afterward. I am also in agreement with Reisch's intentions to focus on Kuhn, because *Structure* was published in 1962 and was the most influential book in philosophy of science not only mid-1960s onward but for the rest of the century.

Reisch's argument also starts with James Conant, and he uses throughout a combination of the production-centered biographical approach and the content-analytical approach (see the discussion above). Here, of course, Conant plays an important role in the formation of Kuhn's ideas. In order to understand science properly, one has to understand not only its structure, but also (and even more so) its historic development. And that has to be done by studying historical examples from different scientific disciplines. Although Reisch emphasizes Conant's important role in the formation of Kuhn's ideas, he nevertheless points to some major divergences. Firstly, according to Kuhn, scientific development is not always cumulative (as Conant thought). Secondly, scientific development—especially in its cumulative phases—is largely dogmatic (as Kuhn underlined and Conant, who thought of a scientist as an always open-minded and critical individual—criticized).[28]

In pointing out these similarities and divergences Reisch's interpretation is more nuanced than Fuller's. But then Reisch adds remarks on normal scientific development and revolutionary change, which perhaps will be received as even more controversial than Fuller's. I have in mind sections in which Reisch writes of scientists being brainwashed into a new paradigm. Brainwashing is what he sees as distinctive of the Cold War atmosphere that informed the writing of Kuhn's *Structure*.

Now nowhere Kuhn does himself use the word "brainwashing." So one would expect some proof for the affirmation that brainwashing played such a big role in forming Kuhn's idea of revolutionary change. Reisch's proof is either anecdotic (as when he cites the fact that a well-known Cold Warrior, Czeslaw Milocs, was in Berkeley at the same time when Kuhn was there) or too all-encompassing (as when he states that the idea of a brainwashing was so pervasive that almost everyone in the USA was overcome by it). It is also not entirely clear to me if Reisch thinks that the notion of brainwashing was unconsciously absorbed by Kuhn or consciously assimilated. If it was absorbed unconsciously, much more has to be done to show that Kuhn was overwhelmed by it. If the thesis is that Kuhn used the brainwashing metaphor consciously, an explanation should be given for the fact that he never used the word in the first place.

Reisch's thesis seems, therefore, grounded on insufficient evidence. Moreover, Cold Warriors in the USA did not *propagate* brainwashing; they tried to exploit the *fear of brainwashing* and were anxious to protect Americans from communist brainwashing. So Kuhn, as a Cold Warrior, should have written something *against* a picture where sudden and irrational changes in beliefs occurred and not *for* such a characterization of paradigm change in science. In that respect Reisch's thesis is also less convincing and one can only hope for clarification in his forthcoming book on Kuhn.

Kuhn and the Sociology of Science

My chapter could come to an end here, but my critique is not meant to be purely negative. My first thesis is instead that where Kuhn's ideas in the characterization of scientific change seem to be most conservative and may seem to have been affected by the Cold War, they actually come from his study of authors in the sociology of science. And my second point is that these ideas have little to do with the Cold War because they had been formulated long before, in the 1930s. Nevertheless, it is noteworthy that Kuhn kept for a while close contact with some leading sociologists of science during the 1950s, which may have further informed a conservative trait in his work during the Cold War period. I proceed in the following way: I first name some traits of Kuhn's characterization of the development of science which seem to me the most conservative ones. Then I go on to identify their most important sources in the sociology of science.

Conservative Traits in Kuhn's Theory of Scientific Development

There are indeed a number of conservative, perhaps even authoritarian and elitist, traits in Kuhn's philosophy, which have been overlooked by many of his followers and are discussed by critics like Fuller, Reisch, and others. Kuhn himself cited with approval the opinion of one of his students, who had characterized *Structure* as a "profoundly conservative book," when he elaborated this evaluation as follows: "And it is; I mean, in the sense that I was trying to explain how it could be that the most rigid of all disciplines, and in certain circumstances the most authoritarian, could also be the most creative of novelty."[29] Most of these conservative traits center on Kuhn's characterization of normal science, especially the thesis of its dogmatism and that of paradigm-monopoly. Kuhn cites a number of examples where outsiders of a scientific community do not find an audience among the insiders, although they contribute important innovations.[30] So should these cases be treated as "collateral damage" of the scientific progress? Kuhn seems to think so, because he writes: "[…] can we be surprised that a scientist resists paradigm-change? What they are defending is, after all, neither more nor less than the basis of their professional way of life."[31]

Kuhn maintains that a mature science (in contrast to a pre-science, or perhaps other academic fields like philosophy) has exactly *one* paradigm. Only in periods of crisis there is a proliferation of competing paradigms in a mature science. It was especially this thesis that prompted Popper and his disciples to criticize Kuhn's theory (and they were responsible for coining the term "paradigm-monopoly-thesis"). Why did they oppose that thesis so much? I think there is a hidden political motive behind this. It has to do with Kuhn's parallel, despite some "vast and essential differences," of scientific and political revolutions.[32] If we translate the paradigm-monopoly-thesis into politics, it would mean that between revolutions we have a sort of one-party-system characteristic of dictatorial political systems like Fascism, National Socialism, and also Communism. That idea seems to me the main reason why Kuhn was so well received in the East and why he was so much attacked by Popper himself and his disciple Paul Feyerabend: the paradigm-monopoly thesis crashes head-on with the political philosophy of open society and its pluralism that they advocated.

At one point Popper and Feyerabend agreed that normal science was something to be criticized. And their battle cry for the area of science became from then on "revolution in permanence," an idea answered by Kuhn in following words: "It isn't that I thought everything was revolutionary—revolution in permanence is a contradiction in terms."[33] Of course, the parallel between progress in science and in politics had been given up by Popper's followers. I now turn to Kuhn and the sociology of science in order to see how much (or how little) of those ideas Kuhn borrowed from that discipline, and will then ask what that has to do with the Cold War.

Kuhn's Encounter with Ludwik Fleck

In his foreword to *Structure* Kuhn writes: "[…] I have encountered Ludwik Fleck's almost unknown monograph, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* (Basel, 1935), an essay that anticipates many of my own ideas […] Fleck's work made me realize that those ideas might require to be set in the sociology of the scientific community."[34]

This statement has long been ignored. Fleck's seldom read book sold only 277 copies till 1959[35] (three years before the publication of Kuhn's *Structure*). It was translated to English (with a foreword by Kuhn) in 1979 and republished in the original German a year later. Only at that point an academic literature began to develop about Fleck.[36] Small wonder that it started with Fleck's influence on Kuhn, because many of Kuhn's ideas developed in *Structure* are already present in that book. I name only three of these as they characterize Fleck's sociological approach:

- A new look on the history of science which should not be based on textbooks but on original sources including non-published ones,
- Taking the scientific community (what Fleck calls a *Denkkollektiv*) and not the individual researcher as the subject of the thought process, and
- Laying emphasis on the thought style (*Denkstil*) of the scientific community and its results, the opinion systems (*Meinungssysteme*).

These very important ideas are common to the sociology as well as to the history of science. But in Fleck's book we also find some more controversial theses, which were later on shared by Kuhn and opposed by others like Karl Popper and his followers:

- The dogmatism of those opinion systems and the weight of tradition and of the initiation of newcomers to those systems; these ideas are worked out in Fleck's book, some even in more detail than in Kuhn's own work.[37]
- The idea of a *Denkkollektiv* (or scientific community).

It is not only the general sociological approach that Fleck and Kuhn have in common, but also some other theses connected with it. Fleck writes about this fundamental concept in the sociology of science: "cognition is [...] not an individual process of any 'particular consciousness'. Rather it is the result of a social activity, since the stock of knowledge exceeds the range available to any one individual."[38] He then goes on to characterize the thought collective as follows:

> If we define "thought collective" as a community of persons mutually exchanging ideas or maintaining intellectual interaction, we will find by implication that it also provides the special "carrier" for the historical

> development of any field of thought, as well as for the given stock of knowledge and of culture. This we have designated thought style.[39]

Interestingly, this relatively general definition of the *Denkkollektiv* covers not only scientific communities, but also communities such as those of the world of football, fashion, and so forth. Fleck also has very interesting ideas about the definition of scientific communities, their complicated inner structure, and their different ways of communication with the outward lay world. When Kuhn commented in 1979 on the book and its impact on him, he seemed to have forgotten these details, because he said:

> I never felt at all comfortable and I still don't with (Fleck's) "thought collective". It was clear it was a group, since it was collective, but (Fleck's) model (for it) was the mind and the individual. I just was bothered by it, I could not make use of it. I could not put myself into it and found it somewhat repugnant. That helped me keep it somewhat at arm's length, but it was very important that I read that book because it made me feel, all right, I'm not the only one who's seeing things this way.[40]

For Kuhn one of the consequences of adopting a paradigm is that a community which embraces it is entitled to only one of them,[41] which was termed paradigm-monopoly-thesis by Popper and his followers. Kuhn says nothing about Fleck's "prediscovery" of the paradigm-monopoly-thesis. But it is clearly present in Fleck's book. Fleck devotes a whole chapter to the dogmatism of the opinion systems in scientific communities. He brings it under the title "The Tenacity of Systems of Opinion and the Harmony of Illusions…"[42] This chapter begins right away: "Once a structurally complete and closed system of opinions consisting of many details and relations has been formed, it offers enduring resistance to anything that contradicts it."[43] He goes on to name examples for this dogmatism not only from his own field of medicine, but also from other sciences.

Now the question is: what has all this to do with Cold War? Well, Kuhn read Fleck's book at the beginning of the 1950s.[44] But Fleck's main ideas were already formed in the late 1920s and his book was finally published in 1935.[45] The editors of his collected works, Sylwia Werner and Claus Zittel, write of an "astonishingly wide reception" of the book. This seems an exaggeration for the whole period between 1935 and the end of the 1970s; there were indeed a number of reviews right after its publication, but only few sold copies. Perhaps the reception would have been different if Moritz Schlick, then head of the Vienna Circle of logical positivists, had succeeded in incorporating his book in the series *Schriften zur wissenschaftlichen Weltauffassung*.[46] But the book was turned down by the Vienna-based Springer publishing house, and so it was published in Switzerland. All this happened more than ten years before the beginning of the Cold War.

After World War II the reception of Fleck's book was almost completely nonexistent. Kuhn came across the book because, as he writes in his autobiographical interview, Hans Reichenbach's *Experience and Prediction*, which was published in 1938, contained a reference to Fleck's work and Kuhn followed it out of curiosity.[47] In the early 1960s, shortly before Fleck died and Kuhn's *Structure* appeared, Fleck's publisher informed him that he had yet to sell half of the remaining copies of *Denkstile und Tatsachen*.[48] Nothing thus reveals a Cold War influence in Kuhn's reception of Fleck's book.

So indeed, many of the leading ideas of Kuhn—and especially his seemingly most conservative ones—have nothing to do with the Cold War, because they were formulated in the early days of the sociology of science. It is interesting and important to note that Kuhn owes many of his ideas to sources which preceded the Cold War. This fact is perhaps overshadowed by Kuhn's tendency to play down these influences later on.

Kuhn and the Sociology of Science

Not much is known nowadays about the extent to which Kuhn followed trends in the sociology of science of the 1950s and exchanged ideas with some of its leading representatives. These trends and exchanges did happen during the Cold War. But were they representative of its ideology? My answer is no.

Kuhn persisted in his effort of reconfiguring the philosophy of science so as to encompass a more realistic picture than the one drawn by leading logical positivists of the Vienna Circle (like Rudolf Carnap) and critical rationalists (like Karl Popper). In pursuing this aim Kuhn studied cognitive psychology, especially the psychology of perception, which led him to incorporate the concept of a *gestalt*-switch in the description of what happens during a paradigm change in periods of scientific revolutions.[49] He was not the only one in realizing the importance of *gestalt* psychology as US philosopher of science Norwood Russell Hanson had also focused on it a few years before Kuhn.[50] On the other hand Kuhn was the first to emphasize the importance of blending this new study with a sociological analysis. So he wrote to Charles Morris, who in 1953 had asked him to contribute a monograph on the history of science for the *International Encyclopedia of Unified Science* (which almost a decade later was published as *Structure*), that "my basic problem is sociological."[51] As Reisch points out, by "basic problem" Kuhn meant how "to reconcile the historical fact of total and revolutionary scientific change with these conservative, internal sociological dynamics that tend to fix practices, methods, and logical and experimental standards."[52]

It has to be noted here that Kuhn had a special conception of "sociology of knowledge" in mind. As his correspondence with Philipp Frank (a physicist and former member of the Vienna Circle, who in 1939 joined Harvard University) shows, Kuhn understood by this term something that might be called a "micro-sociological" view. He wished to focus his attention on the scientific community of each scientific discipline rather than provide a macro-sociological outlook emphasizing broader sociological or "existential" factors that tend to interfere with logic and evidence.[53] So Kuhn insisted, in a draft of the letter to Frank, that

> An examination of those sociological factors which impinge upon an individual scientist not by virtue of his membership in a national community (say the United States), but by virtue of his membership in a narrower professional group (say the American Physical Society), because these factors determine the problems to be attacked, the methods and experiments used to solve them, and the standards by which a proposed solution is regarded as a "proof."[54]

Kuhn went back and leaned on the sociology of knowledge *in this narrower sense* after he had left Harvard, settled in Berkeley, and gone to Stanford for a year.

During the 1950s themes like multiple discoveries, prediscoveries, and priority disputes were the focus of many leading sociologists of science. Kuhn himself contributed to these ongoing debates with the paper "Energy Conservation as an Example of Simultaneous Discovery."[55] In this article he presented a multiplicity of internal and external factors to explain the discovery of energy conservation. Kuhn circulated his papers of that time among leading sociologists of science like Robert Merton and Bernard Barber and so belonged to the "hidden college" of that emerging discipline.

Among the pre-prints he received was a paper by Barber about the "resistance by scientists to scientific discovery," which later on was published in *Science*.[56] Barber's aim in that paper was to show that resistance to scientific innovations was present not only among the usual suspects for that behavior, like religious figures, but *also among scientists themselves*. In his relatively short, but very dense paper Barber provided a taxonomy of all the internal and external factors that informed this resistance and used several examples from different scientific disciplines to illustrate the importance of this phenomenon. The case of Gregor Mendel was particularly important because the resistance to Mendel's discoveries due to both methodological factors (opposition to the use of mathematical techniques in genetics) and personal prejudice against an outsider (Mendel was a monk). The case of Mendel is also of special interest in the history and sociology of science because it is an important example of a prediscovery (his findings were "rediscovered" some 30 years later) and multiple rediscoveries (thanks to three botanists: Carl Correns, Hugo de Vries, and Erich Tschermak, independently one from the others!). Barber was presumably driven by a sort of pedagogical ambition—namely to *downplay* the significance of scientists' resistance to novel discoveries. Kuhn took up Barber's results in his paper "The Function of Dogma in Scientific Discovery" and presented them, even if in a slightly different form, at the Sociology of Science section of the Symposium on the History of Science that took place at the University of Oxford in 1961. The paper was eventually published in the volume *On Scientific Change*.[57] On this occasion Kuhn showed remarkable respect for dogmatism in science:

> [...] In fact, the man engaged in puzzle-solving very often resists substantive novelty, and he *does so for good reason*. To him it is a change in the rules of the game and any change of rules is intrinsically subversive.[58]

So seemingly here (and elsewhere) Kuhn added a normative element to his descriptive stance vis-à-vis the development of science: scientists are by and large *justified* when they resist innovations by outsiders, because they cannot start discussions about possible alternatives at every moment of their research. But again the question is: what has the incorporation and possible transformation of contemporary results of sociology of science to do with the Cold War? Nothing. Unless it can be shown that some of the leading figures and themes in the sociology of science of the 1950s were "infected" by Cold War ideologies themselves.

Kuhn and Herring

After his positive experiences with Barber and Merton, Kuhn even developed plans to promote the not well-established discipline "sociology of science" at American universities. The president of the Social Science Research Council, Pendleton Herring, seemingly had asked Kuhn to write down some remarks on the issue following a presentation in November 1959 that Kuhn had given at a conference on the history of quantification in Stanford. Herring was himself a remarkable figure: born in 1904 he became first professor of political science at Harvard (1928–1947), and helped drafting the National Security Act of 1947, which unified the US armed forces under the US Secretary of Defense and established the CIA. A year later Herring became president of the Social Science Research Council (SSRC, founded in 1923). According to Fred Greenstein and Austin Ranney, "no other figure of his generation […] had as varied and great an impact on social science as Pendleton Herring."[59] Herring always looked out eagerly for new themes and programs the SSRC might sponsor. Kuhn wrote down his ideas for a program in the sociology of science in a long letter dated 21 December 1959:

> For the record, let me try to provide you with a written summary of my main responses to your request for suggestions about ways of promoting the sociology of science. Perhaps I need not elaborate again how very fruitful a research field I think this one could become in a very short time, given a few people working in it. I can think of no other field that so immediately offers so very much unexamined data. Nor can I think of a field in which examination of the data is so likely to promote a new understanding of issues vital to current national policy. You have heard me talk of misleading stereotypes about the nature of science before. Sociology of Science seems to me particularly likely to do away with such stereotypes and to replace them with more viable concepts.[60]

If only one would be able to recruit a small number of people "doing productive work with the nation's best sociology departments," that would produce a snowball effect. Now Kuhn proposed for the inner kernel of a study group in the sociology of science Robert Merton, Bernard Barber, and Thomas Kuhn.[61] I did not find an answer to that letter in Kuhn's papers, but it seems that nothing followed Kuhn's proposal. Whether that had to do with his criticism of quantification in the social sciences (which Herring seemingly championed) or with the instability of the group behind Kuhn's idea is still an open question. Perhaps Herring simply wished to prioritize other programs instead, like "sociolinguistics" and "law and society."[62]

In view of Kuhn's aspirations expressed in the letter to Herring I find it astonishing that Kuhn gave up his close engagement with the sociology of science even *before* the publication of *Structure*. *After* its publication he had his hands full fighting back efforts of self-proclaimed "Kuhnians" to appropriate his ideas in their versions of science studies. From the early 1970s onward the proponents and adherents of the "strong programme" featured as targets of Kuhn's criticism. Merton, one of the leading figures in the sociology of science, a more traditional-minded classic in this field and—last not least—Kuhn's inspirer and discussion partner, gave him good advice to speak out in public: "Je ne suis pas Kuhniste."[63]

In any case Kuhn's quarrel with the promoters of the strong programme is perhaps the reason why his close relationship with the sociology of science before 1962, which informed much of his thinking up to that time, is not so much visible nowadays. In contrast with Kuhn's relation with the Cold War, the connections between Kuhn and a number of sociologists of science should be taken into account more.

Appendix: Michael Polanyi—Another Source for Kuhn's Conservatism

Not everything that went into Kuhn's conservative ideas stemmed from the sociology of science however. In the late 1950s, another influence was represented by Michael Polanyi's works, especially *Personal Knowledge*, which was published in 1957.[64] When Kuhn's delivered his talk "The Function of Dogma in Scientific Research" (and that, by the way, was just a week before the Cold War reached a new height with the construction of the Berlin Wall in August 1961), his remarks instigated an interesting exchange with the Hungarian-born British intellectual.[65]

Kuhn had received an invitation from the organizer of the Oxford symposium to talk on measurement in the social sciences. But he had another idea in mind—namely to present the content of first chapters of his *Structure,* which at that time was not yet finished. Now, the first portion of *Structure* is all about the notion of the paradigm and normal science, the dogmatic side of scientific development. Any talk about extraordinary science and scientific revolution is still missing in the dogma paper.

How much Kuhn's and Polanyi's ideas converged at that time can be seen from the discussion after the talk. Polanyi, the second commentator (after Rupert Hall) said:

> The paper by Mr. Thomas Kuhn may arouse opposition from various quarters, but not from me. At the end of it he says that the dependence of research upon a deep commitment to established beliefs receives the very minimum of attention. I could not agree more, I have tried in vain to call attention to this commitment for many years. I hope that if I join forces with Mr. Kuhn we may both do better.[66]

In his closing remarks Kuhn too emphasized the similarities of his views with those of Polanyi. But he also named a few differences, the most important one being his stress on the scientific community versus Polanyi's more individualistic outlook:

> It is not, after all, the individual who decides whether his discoveries or theoretical inventions shall become part of the body of established science. Rather it is his professional community, a community which has and sometimes exercises the privilege of declaring him a deviant […]. I take the "social implication" of my views more seriously than his [Polany's] commentary suggests.[67]

Notwithstanding minor differences between Kuhn and Polanyi at the Oxford conference, it is perhaps no wonder that the only one of Popper's disciples attending the conference, Imre Lakatos, had the impression that Kuhn was "just a footnote" to Polanyi. But soon afterward things changed dramatically. After the publication of *Structure* one year later, Polanyi himself was relegated to a footnote. And Kuhn, during his next travel to England when he attended the 1965 London conference had his hands full convincing the audience, and especially Popper and his followers, that he did not preach a personalist, elitist philosophy of science. Later on, he took distance from his article "Function of Dogma" and rejected proposals to have it republished.[68]

Toward the end of his life in a long biographical interview, Kuhn did everything he could to downplay Polanyi's influence on his ideas. On the other hand, Polanyi's scholars went on to suggest that Kuhn's theory drew on Polanyi. In view of Kuhn's lengthy occupation with the sociology of science (which was independent of his reading of Polanyi) and its conservative trends, these accusations miss their target as much as those about his connections with the Cold War.

Notes

1. This prominent SDS member, Volkhard Scholten, died some years ago, and I dedicate this article to his memory.
2. Alexander Mitscherlich and Fred Mielke, *Doctors of Infamy*: *The Story of the Nazi Medical Crimes* (New York: Schuman, 1949); Eugen Kogon, *Der SS-Staat. Das System der deutschen Konzentrationslager* (Franfurt/Main: Verlag der Frankfurter Hefte, 1946).
3. James Hershberg, *James B. Conant*: *Harvard to Hiroshima and the Making of the Nuclear Age* (New York: Alfred Knopf, 1993).
4. Steve Fuller, *Kuhn Vs. Popper. The Struggle for the Soul of Science* (Basingstoke: Macmillan, 2003), 193.
5. Steve Fuller, *Thomas Kuhn. A Philosophical History for Our Times* (Chicago and London: University of Chicago Press, 2000), 5.
6. Thomas S. Kuhn, *The Road Since Structure. Philosophical Essays*, *1970–1993*, *with an Autobiographical Interview*, edited by James Conant and John Haugeland (Chicago and London: University of Chicago Press, 2000), 275.
7. Ibidem, 276.
8. T. S. Kuhn, *The Copernican Revolution. Planetary Astronomy in the Development of Western Thought*, foreword by James B. Conant (Cambridge, MA: Harvard University Press, 1957).
9. Steve Fuller, *Kuhn Vs. Popper. The Struggle for the Soul of Science*, 213.
10. Ibidem, 193–215.
11. Ibidem, 215.
12. See Martin Heidegger, *Reden und andere Zeugnisse eines Lebensweges* (Frankfurt am Main: 2000), 81–274 for a collection of his speeches, letters, etc. during his time as *Rektor* of Freiburg University and also some of his writings as professor in the Third Reich (especially the footnotes at p. 308 and pp. 345–347). On Heidegger and Nazism see for instance: Víctor Farías, Joseph Margolis, and Tom Rockmore, *Heidegger and Nazism* (Philadelphia: Temple University Press, 1989); Rüdiger Safranski, *Martin Heidegger*: *Between Good and Evil* (Cambridge: Harvard University Press, 2002).
13. Steve Fuller, *Kuhn Vs. Popper. The Struggle for the Soul of Science*, 207.
14. Ibidem, 206.
15. What was later famously published as Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970).
16. Lakatos to Kuhn, 26 March 1970, in "Correspondence" (13), "Kuhn, Thomas" (512), Imre Lakatos Papers, LSE Library Archive.
17. Imre Lakatos, "A letter to the Director of the London School of Economics," 1968 in I. Lakatos, *Mathematics*, *Science and Epistemology*,

edited by John Worrall and Gregory Currie (Cambridge: Cambridge University Press, 1978), 247–253.

18. Lakatos' answer to Kuhn's letter cited above reads: "To my knowledge, the association of military research with the universities is here (i.e: in GB) regarded as completely out of order and, in fact, when the Defence Ministry offered to finance a position at the LSE in which a man would have had some connection with the Ministry, the proposal was rejected outright". Lakatos to Kuhn, undated, in "Correspondence" (13), "Kuhn, Thomas" (512), Imre Lakatos Papers, LSE Library Archive.
19. Sarah Bridger, *Scientists at War*: *The Ethics of Cold War Weapons Research* (Harvard: Harvard University Press, 2015), 221.
20. Sara Bridger, "Scientists and the Ethics of Cold War Weapons Research," PhD Dissertation: Columbia University, 2011, 340.
21. Ibidem, 346.
22. Ibidem, 343 and 347.
23. Ibidem, 350. On the origins of Forman's theses see also Turchetti in this volume. Bridger does not discuss in detail the relationship between Forman and Kuhn, but concludes her dissertation with an outlook into Ronald Reagan's Strategic Defense Initiative (SDI) program. Today, it would be relevant to look into the enormous defense buildup after 9/11 and also into the "applied" research associated with worldwide surveillance activities of the National Security Agency (NSA) and other agencies.
24. Hac Malachi Haim Hacohen, *Karl Popper. The Formative Years 1902–1945* (Cambridge and New York: Cambridge University Press, 2000). On the origins of Popper's political philosophy in the 1920s see also: Hans-Joachim Dahms, "Karl Poppers erste Schritte in die Philosophie: Leonard Nelsons Paradoxien der Souveränität und Nelsons sowie Poppers Lösungsversuche," in *Karl Popper*: *A Centenary Assessment*, *Volume I*: *Life and Times*, *and Values in a World of Facts*, edited by Ian Jarvie, Karl Milford, and David Miller (Aldershot: Ashgate, 2006), 83–89.
25. Karl Popper, *Open Society and its Enemies* (Princeton: Princeton University Press, 1966).
26. See Popper's 1992 Foreword to the 7th edition of *Open Society and its Enemies*. Popper's "practical-political receipts of reformism" and his idea of an open society were included as "anti-communistic conceptions" into the obligatory curriculum for philosophy students in the GDR. See on this H. J. Dahms, "Philosophie an der Universität Jena 1946–1989," in: *Hochschule im Sozialismus. Studien zur Geschichte der Friedrich-Schiller-Universität Jena* (*1945–1990*), edited by Uwe Hoßfeld, Tobias Kaiser, and Heinz Mestrup (Weimar and Vienna: Böhlau, 2007), 1568–1626, on 1604.
27. George Reisch, *How the Cold War Transformed Philosophy of Science*: *To the Icy Slopes of Logic* (Cambridge: Cambridge University Press, 2005).

28. Kuhn to Conant, 29 June 1961, quoted in G. Reisch, "When *Structure* met Sputnik: On the Cold War origins of *The Structure of Scientific Revolutions*" in: *Science and Technology in the Global Cold War*, edited by Naomi Oreskes and John Krige (Cambridge, MA: The MIT Press), 371–392, on 373.
29. Kuhn, *The Road Since Structure*, 308.
30. T. S. Kuhn, "The Function of Dogma in Scientific Research," in: *Scientific Change. Historical Studies in the Intellectual, Social and Technical Conditions for Scientific Discovery and Technical Invention, from Antiquity to the Present*, edited by Alistair C. Crombie (London: Heinemann, 1963), 347–369, on 348. From my pupils days in Wuppertal I remember Karl Fuhlrott, a gymnasium teacher who found the first skeleton of the Neanderthal man in a valley between Wuppertal and Düsseldorf in Western Germany, but was treated by leading anatomists of his time like Rudolf Virchow with contempt. The bones found by Fuhlrott were described by these "authorities" as those of a disfigured human being.
31. Kuhn, "The Function of Dogma in Scientific Research," 364.
32. Kuhn, *The Structure of Scientific Revolutions*, 92 ff.
33. Kuhn, *The Road Since Structure*, 295.
34. Kuhn, *The Structure of Scientific Revolutions*, VI f.
35. Benno Schwabe & Co publishers to L. Fleck, 8 December 1959 cit. in Ludwik Fleck, *Denkstile und Tatsachen. Gesammelte Schriften und Zeugnisse*, edited by Sylwia Werner and Claus Zittel (Berlin: Suhrkamp, 2011), 594.
36. T. Kuhn, "Foreword," in L. Fleck, *Genesis and Development of a Scientific Fact*, edited by Thaddeus J. Trenn and Robert K. Merton (Chicago: Chicago University Press, 1979), vii–xi. On the lack of coverage on Fleck's work see also Kokowski's article in this volume.
37. One major difference between Fleck's and Kuhn's works is that the discipline from which Fleck takes his examples and especially his main example (syphilis) comes from medicine rather than physics.
38. Fleck, *Genesis and Development of a Scientific Fact*, 38.
39. Ibidem, 39.
40. Kuhn, *The Road Since Structure*, 283.
41. Kuhn, "The Function of Dogma in Scientific Research," 352.
42. Fleck, *The Genesis of a Scientific Fact*, 27.
43. Ibidem.
44. Kuhn, *The Road Since Structure*.
45. Sylwia Werner and Claus Zittel, "Einleitung: Denkstile und Tatsachen," in: Ludwik Fleck, *Denkstile und Tatsachen. Gesammelte Schriften und Zeugnisse*, 9–51, on 30 f.
46. See the correspondence between Fleck and Schlick, in Fleck, *Denkstile und Tatsachen. Gesammelte Schriften und Zeugnisse*, 561–565. That Schlick

recommended Fleck's book to his publisher for incorporation into the book series shows that Kuhn's allegation that Fleck's "Polish-German … (had) been so very difficult" is not entirely correct (see Kuhn, *The Road to Structure*, 283). As a German I have to say that I find Fleck's language very idiomatic and elegant.

47. Hans Reichenbach, *Experience and Prediction. An Analysis of the Foundations and the Structure of Knowledge*, with a new introduction by Alan W. Richardson (Notre Dame: University of Notre Dame Press, 2006 [1938]). Kuhn, *The Road to Structure*.
48. Fleck's correspondence with his publisher Benno Schwabe Verlag in: Fleck, *Denkstile und Tatsachen. Gesammelte Schriften und Zeugnisse*, 592–596.
49. Christoph Limbeck-Lilienau, "Kuhn and cognitive psychology," in *Vienna Circle Yearbook 2017*, edited by Friedrich Stadler (Dordrecht: Springer (forthcoming)).
50. Norwood Russell Hanson, *Patterns of Discovery*: *An Inquiry into the Conceptual Foundations of Science* (Cambridge: Cambridge University Press, 1958). On Hanson see the recent biography: Matthew D. Lund, *N. R. Hanson. Observation*, *Discovery and Scientific Change*, foreword by Hasok Chang (Amherst, NY: Humanities Books, 2010). On the Gestalt-theory see especially 85 ff. and 151–154.
51. Quoted in Reisch, "When *Structure* met Sputnik: On the Cold War origins of *The Structure of Scientific Revolutions*,"375.
52. Ibidem, 375.
53. Ibidem, 375.
54. Cited in Reisch, unpublished draft, 2014, 7.
55. Thomas S. Kuhn, "Energy Conservation as an Example of Simultaneous Discovery," in *Critical Problems in the History of Science*, edited by Marshall Clagett (Madison: University of Wisconsin Press, 1959), 321–356. Reprinted in T. S. Kuhn, *The Essential Tension. Selected Studies in Scientific Tradition and Change* (Chicago: Chicago University Press, 1977), 66–104.
56. Bernard Barber, "Resistance by Scientists to Scientific Discovery," *Science* 134 (1961), 596–602. A copy of the pre-print sent by Barber is in Kuhn's papers collection at the MIT.
57. T. S. Kuhn, "The Function of Dogma in Scientific Research," in *Scientific Change*, edited by Alistair C. Crombie, 347–369.
58. Ibidem, 364, emphasis added.
59. Fred Greenstein and Austin Ranney, "Pendleton Herring (27 October 1903–17 August 2004)," in *Proceedings of the American Philosophical Society* 150/3 (2006): 487–492, on 491. See also the obituary: Matt Schudel, "Political Intellectual Pendleton Herring, 100," *The Washington Post*, 20 August 2004.

60. Kuhn To Herring, 21 December 1959, in Thomas S. Kuhn Papers, Box 23, MC 240, MIT Institute Archives and Special Collections.
61. On this see also K. Brad Wray, "Kuhn's Social Epistemology and the Sociology of Science," in *Kuhn's Structure of Scientific Revolutions—50 Years On*, edited by William J. Devlin and Alisa Bokulich (Boston: Springer, 2015),167–184, on 171.
62. This is what the internet guide to the SSRC papers suggests: Kenneth W. Rose, "A Guide to the Social Science Research Council Archives at the Rockefeller Archive Center," New York, 1999, text above footnote 49 and the list of committee projects. Available at: http://www.rockarch.org/collections/nonrockorgs/ssrcdoc.pdf, accessed 12.05.2015. I further explore some of these connections in Hans Joachim Dahms, "Thomas Kuhn's Place in the Sociology of Science," in *Vienna Circle Yearbook 2017* [forthcoming].
63. "Kuhn must periodically be tempted to exclaim, after the fashion of that Victorian scholar who spent much of his long exile in the British Museum: 'Je ne suis pas Kuhniste'." See Robert K. Merton, "Structural Analysis in Sociology," in *Sociological Ambivalence and Other Essays* (New York: The Free Press, 1976), 109–144, on 134.
64. Michael Polanyi, *Personal Knowledge. Towards a Post-Critical Philosophy* (Chicago: University of Chicago Press, 1958). On Kuhn and Polanyi see also Martin X. Moleski, "Polanyi vs. Kuhn: Worldviews Apart," *Tradition and Discovery: The Polanyi Society Periodical* 33/2 (2006): 8–24.
65. See the first footnote in T. S. Kuhn, "The Function of Dogma in Scientific Research," 347.
66. Commentary by Michael Polanyi on Kuhn's "The Function of Dogma in Scientific Research," in *Scientific Change*, edited by A. C. Crombie, 375–380, on 375.
67. Kuhn, Reply in *Scientific Change*, edited by A. C. Crombie, 394 f.
68. See Reisch, "When *Structure* met Sputnik: On the Cold War origins of *The Structure of Scientific Revolutions*." He agreed to republish instead T. S. Kuhn, "The Essential Tension: Tradition and Innovation in Scientific Research," in *The Third University of Utah Research Conference on the Identification of Scientific Talent*, edited by C. W. Taylor (Salt Lake City: University of Utah Press, 1959), 162–174.

PART II

Studies of Science Behind the "Curtain"

CHAPTER 6

Blind Isolation: History of Science Behind the Iron Curtain

Gabor Pallo

The first history of science and technology conference in Hungary was held in November 1972. Its published proceedings contain 54 papers, but the program was larger as some speakers did not submit their texts.[1] Indeed, in the early 1970s in Hungary history of science took important steps in the road of institutionalization—a road that gradually turned in the opposite direction after the Cold War period. The conference was organized by a newly established committee for history of science and technology in the Federation of Technical and Scientific Societies (FTSS). This federation was established in 1948, right after the beginning of the Cold War, with the aim of coordinating the work of the currently existing 14 scientific societies and to protect their interests. The Committee for the History of Science and Technology defined its scope as the histories of fields represented by these professional societies. Medicine had a

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separate umbrella organization with similar goals to FTSS. It had its own historical section with an activity unconnected with FTSS, although some personal ties secured a loose relationship between the officially unconnected organizations. Accordingly, when speaking about history of science, I disregard history of medicine but include history of mathematics and technology that the FTSS considered to be sciences. By that time, a considerable group of scholars became interested in the past of their fields.

The claim of this chapter is that during the Cold War period, the history of science community had an explicitly non-articulated interest in working in isolation from other STS fields of science studies, rather than in “the confluence of disciplinary streams”—to borrow David Edge’s formulation to characterize the atmosphere of the early period of STS, a policy-centered form of science studies.[2] The consequence was that STS as an intellectual field in its own right and as a social or intellectual movement remained largely, though not completely, unknown in Hungary. This chapter tries to explain the underlying reasons.

Favorable Climate to Institutionalization in the Early 1970s

The early 1970s brought more than the first history of science conference in Hungary. In 1972 the Hungarian Academy of Sciences established a small research group for the history of science in the Department of Experimental Physics at the Technical University of Budapest. A year later, the Hungarian Museum for Science and Technology started its work. In 1974 the Hungarian Academy of Sciences set up a so-called complex committee for the history of science and technology to organize the growing activity in the field. At this point the institutionalization stopped until 1997, when a PhD school was accredited at the Technical University of Budapest.[3]

This institutional expansion cannot be directly attributed to a decisive change in the current intellectual or political climate. The 1970s were one of the Cold War decades during which Hungary belonged to the Soviet sphere of influence. It was a socialist country. The regime seemed stable, almost frozen without any chance of radical change even in the far future. However, some new economic, political, and social elements emerged in the framework of the political regime, which influenced many segments of life, including, indirectly, the position of the history of science. These were the early years of “Gulash communism”—to use the journalistic term applied in the West to characterize the relatively moderate work of Soviet-type socialism in Hungary.

Studying the history of the Soviet satellite countries, historian Ivan Berend T. pointed out that the year 1973 was a turning point in European and world history for many reasons, primarily the 1973 oil crisis—which will be discussed at length later in the chapter. As Berend emphasized, "The year 1973 was indeed the beginning of a new chapter of greater European economic history, which, in the case of Central and Eastern Europe, led to the collapse of their state socialist regimes."[4] Berend's statement is particularly remarkable because he participated in the process of forming the moderate version of Soviet-type socialism in Hungary.

Moderation was a consequence of the Hungarian revolt against the Soviet regime in 1956.[5] After the defeat of the revolution by the Soviet army and the scathing retaliation executed by the illegitimate puppet government lead by János Kádár, a consolidation process started in the mid-1960s. The administration attempted to silent the vehement political discontent by welfare measures. According to a tacit social contract, the political leadership promised a steadily improving standard of living in exchange for the population's passive acceptance of the political realities determined by Hungary's unchangeable position in the bipolar world of the Cold War era. The tacit contract extended to intellectual life. The chief party leader responsible for the field, György Aczél, categorized intellectual endeavors into three groups: to be supported, to be tolerated, and to be prohibited. In the lack of clear criteria of belonging to one or the other, Aczél's personal preferences were decisive. History of science aimed to belong to the supported group instead of the tolerated one. The Soviet Union apparently respected the compromise in the hope of avoiding another revolt. This deal constituted the essence of the Hungarian economic and public policy until Kadar's resignation and the collapse of the whole regime in 1989.

However, the communist government had enormous difficulties in keeping the tacit contract because the centrally planned economy did not give enough potential for a steady growth. By the mid-1960s, the limits of extensive growth seemed to be very near. Extensive growth was based on increasing the number of workers into production by attracting women (housewives) and agricultural workers living in villages to the factories operating in towns. This process could not be continued after practically all able people had been employed. The party decided to turn to intensive growth, which meant increasing the efficiency of work by new institutional organization and technological improvement. The new institutional organization included elements of market economy and individual incentives combined with central planning. This was called New Economic Mechanism, introduced in 1968, a decisive year in world history for many reasons—mainly the student revolts in France and the USA, and for the Warsaw Pact military invasion of Czechoslovakia.

Berend, however, considers the year 1973 more important than 1968 because the oil crisis drastically elevated the price of crude oil causing fast and unexpected slowdown in the world economy. Referring to Joseph Schumpeter, Berend assumes that the crisis fostered a renewal of technology: "After the 1973 oil crisis and the beginning of marked decline, the dawn of a new technological era, a new Schumpeterian 'set of technological changes,' was also signaled. The invention and early distribution of the personal computer in 1974 symbolized the new wave of technological revolution. While the old sectors declined, some of the new ones experienced impressive increase. Electricity output, in contrast to coal production, continued to grow."[6]

For the field of history of science, the growing significance of technology was ideologically instrumental in spite of the radical difference between the technologically backward Hungarian and the Western situation and its interpretations. Indirectly, everything related to the "scientific-technological revolution," in the party's ideological terms, received attention in Hungary.[7]

Although in the early 1970s the Soviets ordered the economic reforms to be stopped and required ideological strictness, some market elements could be kept. Even this little divergence from Stalinist centrality brought social consequences. Some old organizations, like the National Council of Farmers' Cooperative and others, including the Federation of Technical and Scientific Societies or the Hungarian Academy of Sciences wearing communist dresses, exerted a kind of lobby activities.[8] These organizations emphasized their loyalty to the party and the regime, but they represented the interests of a social group (such as farmers or engineers) in negotiations behind the scenes. Open negotiations in the non-democratic society were unimaginable.

These two features of the contemporary political contexts were favorable to the institutionalization of the history of science. They did not "cause" it; they did not even urge it, as historians of science did not consider their field to be a profession or discipline. They were individuals interested in a subject. They aimed to gain more space and more recognition for their activities and some possibilities to have an academic career in the field, and they found some doors open to a certain extent.

Some Features of Historiography of Science in Hungary

Two researchers were particularly active in establishing institutional framework for history of science in Hungary; both were quite advanced in their careers. In 1998 one of them, Ferenc Szabadváry, historian of chemistry, looked back at the early 1970s. He said that although there was no history of science department in any Hungarian university and there was no research institute or a special section in a research institute of the Academy of Sciences doing research in the field, "there have been people working on history of science but they worked besides their jobs in other areas."[9]

Szabadváry (1923–2006) grew up in a family owing a soap factory in Western Hungary.[10] He was expected to take over the family business; this is why he graduated from chemical engineering in Budapest in 1945, instead of following his passionate interest in humanistic fields, mainly history and languages. Soon after he occupied his leading position, the factory was nationalized. Szabadváry received a job at the Department of Analytical Chemistry of the Technical University of Budapest, although he was not a supporter of the new communist regime. While doing respected work in analytical chemistry, as a return to his childhood dream, he started to do research on the past of his discipline as a hobby. He conducted his studies in isolation from other people having similar interests in the history of other scientific or technological fields. Like most of his colleagues in the 1960s–1970s, he worked as an amateur historian, but so successfully that he won the American Dexter prize in 1970 for his book on the history of analytical chemistry. This book was republished in the series of classics in the History and Philosophy Science in Switzerland in 1992.[11] In Hungary, however, he had difficulties in publishing his book and, in general, in publishing history of chemistry articles due to the lack of proper journals and an interested historian community. His colleagues in the chemistry community appreciated his work, though they were not particularly interested. He received an academic degree as a chemist, and the chemistry section of the Academy financially supported the publication and later, the translation of his book.

His historical view on analytical chemistry was in harmony with the naïve positivism of the chemistry community of the time. To a large extent, Szabadváry's book is backward history: it reads history as antecedent genealogy of the current state of the art. This present-centered, Whiggish anachronism penetrates the whole study. It looks for analytical chemistry before its subject had been defined intellectually or sociologically. The largely descriptive chapters include the history of gravimetric and titrimetric methods, spectroscopy, and so on. We can learn about their origins and progresses. For instance, the gravimetry chapter relies mainly on Fresenius's textbook published in 1846, but the chapter also includes early twentieth-century results of, for example, Kolthoff and Njegovan, and then methods introduced in Szabadváry's times, like derivative thermogravimetry.[12] The titmetry chapter, about 20 pages later, starts again with mid-eighteenth-century authors, such as Francis Home, William Lewis, and others, and arrives at methods like complexometric titrations introduced by Schwarzenbach in the 1950s.[13] Disciplinary boundaries are clear-cut—no intellectual traditions, no cultural or sociological context in chapters that return to the periods of the earlier ones. Yet, here and there we can find references to technological demands and general remarks on politics.[14] The volume contains many names, dates, and biographical notes, which makes the book useful, like a lexicon.

This method and view on science characterized Szabadváry's later works, including his history of Hungarian chemistry, coauthored with Zoltán Szőkefalvi-Nagy who wrote about the pre-1848 period, while Szabadváry wrote about later times.[15] This book was very successful again. In 1971, it earned Szabadváry the highest scientific degree, Doctor of Science, given by the Academy, which made him eligible to become full professor at the Technical University and director of the newly established Museum for Science and Technology. His chapters again provide a lot of data, biographies, and descriptions of chemical works and separated chapters on institutions, teaching, journals, and the like. Chemistry is sharply defined again. The history of its subfields like inorganic and organic chemistry, physical chemistry, and others are described in separate chapters as if the boundaries have always been as clear as in the author's times. It gives a scientistic, limited, utilitarian picture of science with nice good people, free from special interests, biases, political activities, religious and cultural commitments, and beliefs. In fact, the book has no unifying narrative either. Professors follow each other in the sequence of time, and their teaching and research activities are described as separate stages of the road that leads to the present. His two main works exemplify the typical histories of science in Hungary, though at the highest level.

Szabadváry published extensively in several languages and in Hungary. Typically, he published in chemistry journals of his field, mostly in *Talanta* and *Journal of Thermal Analysis and Calorimetry*. He wrote about famous analytical chemists, like Berzelius,[16] or analytical methods. In addition, he wrote popular scientific books. The most successful was about Lavoisier, which was translated into several languages.[17]

Szabadváry's counterpart in physics was Jolán Zemplén (1911–1975), who published two volumes about the history of early Hungarian physics. Her father, Győző Zemplén, was an outstanding physicist who died young in the First World War.[18] Her uncle, Géza Zemplén was a legendary professor of organic chemistry, also at the Technical University of Budapest. After graduation, Jolán Zemplén started working in the Department of Physics at the Technical University of Budapest. She did experimental research on spectroscopic subjects and became the third full professor and head of department in the same family. She lived in the main library of the Eötvös Loránd University, in an apartment secured to the director of the library, László Mátrai, husband of Jolán Zemplén. This situation had several advantages. Firstly, Zemplén had an easy access to old books and current literature. Secondly, she enjoyed the company of many people with deep knowledge in various fields of humanities, who gathered around her husband and in the library. She also had inclinations toward humanities, including languages. Thirdly, Mátrai, a Marxist philosopher, member of the Academy of Sciences and professor at the Eörvös Loránd University with important positions and political influence in intellectual circles and the in the Hungarian Academy of Sciences, could help his wife in institutionalizing history of science, a field he also liked, although his specialty was philosophy of literature.[19]

Zemplén started writing about the history of physics while on maternity leave in the 1940s.[20] Her early works served goals of popularization. However, when she turned to history of physics as research field instead of popularization, with few exceptions, she published about exclusively about Hungary-related subjects. Jolán Zemplén's works could be characterized roughly the same way as Szabadváry's. Perhaps she was a bit more engaged in Hungarian political history than Szabadváry; maybe she looked a little bit more on religion and seventeenth-century philosophy than Szabadváry did, but the de-contextualization tendencies are basically the same in both authors' books: science progresses largely in isolation of the sociocultural environments that can sometimes provide favorable or unfavorable external conditions for development.[21] For instance, in harmony with the contemporary Marxist requirements, Zemplén emphasizes the close relationship between science and technology in the eighteenth century: "The relationship between physics and technology became gradually tighter, and by the end of the century the bourgeois class that had been carrying scientific development seized the power."[22] In her books we learn about people, textbooks, schools, and sometimes experimental instruments based on large archival material.

The third book of this genre was written by Barna Szénássy on the history of mathematics in Hungary.[23] Its structure and character were similar to the Szabadváry's and Zemplén's books, but this is the most limited one of the three—the least analytical and the least reflective.

These representative books proved, of course, useful in the hands of other researchers because they contained important basic information about the past of the Hungarian science. They have been used as sources, as compendiums of old thick books, and as descriptions of difficultly available and difficultly readable manuscripts and correspondences. This was a genre in the history of science literature; many authors rely on them even today.[24]

The speakers of the 1972 conference presented a very colorful picture. Their subjects were diverse, touching micro histories with little details of the past of a Hungarian factory, a school, an experiment, a portrait of a man, and so on. All of them were descriptive, comprehensible to those who were familiar with the science and engineering of a given time or a given field. Although all people in the room were somehow interested in the history of science and technology, they felt isolated because of the diversity of fields and times and the lack of common ground that could organize the program. Characteristically, there was no discussion after the papers.

None of the speakers mentioned Kuhn's *Structure of Scientific Revolution*. Only two speakers, László Vekerdi, a polymath librarian trained as surgeon, and Géza Györgyi, a theoretical physicist, mentioned Kuhn's name but not the *Structure of Scientific Revolutions;* rather his project on the historical sources of quantum physics was mentioned.[25] This work was more in harmony with the scope of interest of the historians of science and technology than with some general ideas about the nature of science. I am almost sure that Vekerdi knew *Structure*, while the others did not, and they had probably not read it later either. Popper, Kuhn, Feyerabend, and Lakatos, the central characters of the debate on the nature of science and its history, remained unmentioned and unknown or at least out of the sight of this generation of historians of science in Hungary.

Hungarian Historians of Science Working Outside Hungary

There was a notable exception of the mainstream history of science in Cold War Hungary: Árpád Szabó (1913–2001), historian of ancient mathematics who did not participate in the 1972 conference. Szabó was neither a scientist nor a mathematician. He was a classical philologist who had graduated in Germany.[26] He was appointed as a full professor of classics in 1940, at the age of 27, at the University of Debrecen, East Hungary. In 1948 he was moved to the Eötvös Loránd University in Budapest. He published books on classic literature, philosophy, and politics in Hungarian, and later also some on the history of science.[27] His career was disrupted because of his activity in the 1956 revolt against the Soviet regime. He lost his position in 1957 and received a job in the library of the Institute of Mathematics of the Hungarian Academy of Sciences in 1958. He occupied this position until his retirement in 1983. This institute, and its director, Alfréd Rényi, an excellent mathematician, inspired his investigations on ancient mathematics, which resulted in a very successful book in German on ancient Greek mathematics.[28]

Szabó claimed that the methods of Greek mathematics originated in dialectics, the art of discussion. In addition, the characteristic mathematical argumentation based on deduction came from the philosophical style of Eleatics (Parmenides, Zenon), an aristocratic school of thinking. By this, and by his method based on classics, Szabó transgressed the disciplinary boundaries revered by his historian of science generation in Hungary. He did not participate in the discipline-building activity of his colleagues; rather, he worked as a loner.

While in Debrecen in the 1940s, he became very popular among the students, including Imre Lipschitz, who changed his name to Imre Lakatos after the World War II. They kept friendly contact throughout their life. Lakatos also worked in the library of the Institute of Mathematics after his imprisonment in the early 1950s and admitted that he, a secret police agent, spied on Szabó.[29] Lakatos settled down in Britain after leaving Hungary in 1956 and introduced Szabó to his mentor, Karl Popper, who was interested in Szabó's work.[30] As a result, Szabó was frequently invited to Britain in the 1960s. He visited, lectured, and participated in meetings.

(He also was visiting professor in Heidelberg on the invitation of Hans-Georg Gadamer.) This intensive scientific activity in the West was permitted in very few instances only.

In London, Lakatos (but, to my best knowledge, not Szabó) regularly met two other Hungarian historians and philosophers of science: Michael Polanyi and Arthur Koestler. By the 1960s, Michael Polanyi left his chair of physical chemistry in Manchester to become a philosopher. His basic terms like "personal knowledge," "tacit knowing," and "republic of science" are at least as much part of the science studies' vocabulary as the Lakatosian "methodology of research programs," "rational reconstruction," or "protective belt," yet Polanyi's role in contextualizing science is cited less often than Lakatos's.[31] Arthur Koestler, a science writer, became best known through his novel, *Darkness at Noon*, an analysis of the constructed lawsuits in Stalinism.[32] Koestler (trained as an engineer) was deeply involved in historical and philosophical problems of science. His books on the scientific revolution (*Sleepwalkers*) or on evolution and behaviorism (*Ghost in the Machine*) and others analyzed some crucial points of science in a very enjoyable style. They were widely debated in the 1960s and 1970s, but are largely forgotten now.[33]

Besides their political activities during the Cold War period, Polanyi, Koestler, and Lakatos—in one way or another—significantly influenced the formation of post-positivist view of science and the debates about it, which had started already in the 1950s.[34] In particular, Polanyi and Koestler were among the pioneers of the psychological, cultural, and sociopolitical approach to science. Szabó did similarly in the field of history of mathematics, although he did not publish about the big philosophical debates on the nature and development of science going on in the 1960s and early 1970s. In addition, Polanyi was in close contact with Karl Mannheim, sociologist, pioneer of sociology of knowledge, forerunner of the strong program.[35] Although they all were Hungarians and part of the same intellectual tradition as those living in Hungary, they were isolated from the mainstream Hungarian community of history of science both personally and intellectually.

Uneasy Relations with Philosophy

What was the underlying reason of separation between historians of science living outside and inside Hungary? The personal part is easy to explain. Polanyi, Koestler, Lakatos, and Mannheim both geographically and intellectually lived on the other side of the bipolar world of the Cold War era. The Iron Curtain was difficult to pierce through. Traveling, attending conferences, even the possibility of correspondence was restricted.

In addition, they all fervently criticized the politics and ideology of Soviet-type socialism, including the principles of the socialist science policy. Among many other things, Koestler's *Darkness at Noon* had already been mentioned. Lakatos spoke out against the Lysenko case; Polanyi, an ardent critic of socialism and its Hungarian version, argued against central planning in general and central planning of science in particular. His long debate with Bernal on the distinction between pure and applied science and central planning implied the sharp contrast between the Marxist and anti-Marxist philosophies of science and the two scientists' conflicting relations with Soviet-type socialism.[36] Because of the ideological-political relevance of the debated problems, it involved some amount of danger under the non-democratic Cold War circumstances. Science in general and social sciences in particular were isolated from Western science as if Marxism had created a separate intellectual world in the Eastern side of the Iron Curtain.

Another important reason for the lack of connection with these colleagues living in the West was that most of the mainstream Hungarian historians of science isolated themselves from philosophy in general. Lakatos would say, paraphrasing Kant, that their history was blind because it lacked philosophy. The intentional separation from philosophy could be explained by the scientific education of the authors. They studied science or engineering, and they lacked knowledge and interest in philosophical and sociological issues. In general, a strong bias could be seen against philosophy in the circle of scientists and engineers. Philosophy was considered identical with simplified and canonized textbook Marxism, while all theories born on the Western side of the Iron Curtain were considered bourgeois and hostile to socialist Hungary. All universities had a department of philosophy to teach Marxist–Leninist philosophy, and all students were compelled to learn it. The teachers of these departments were considered to be servants of the Communist Party, while their subject was considered to be an ideology serving communist politics.

The intellectually, but not politically, most influential philosophers, who declared themselves as representing an interpretation of Marxism different from the one supported by the Communist Party did not show much interest in the philosophy of science. The circle gathered around

George Lukács (Ágnes Heller, György Márkus, and others) wrote about philosophical anthropology, social philosophy, ethics, social ontology, and other issues discussed by contemporary ideology and politics. In the early 1970s, when the party ordered strictness in ideology and culture, these philosophers were put from the tolerated category into the prohibited one. As a result, they were fired from their jobs, and some of them were pressed to leave Hungary for the West.[37]

On the other hand, some philosophers, much less known than those belonging to Lukács's circle, were active in philosophy of science. People like Tibor Elek and Judit Fodor at the Technical University, József Horváth at the Eötvös Loránd University, or Antal Müller in the Institute of Philosophy of the Hungarian Academy of Sciences, and others extensively published about the dialectical materialist interpretation of debated areas of science, such as relativity or quantum physics, but they did not focus on questions related to the nature of science and its rationality, progress, cumulativity, or historical character analyzed by Polanyi, Koestler, Lakatos, Kuhn, Feyerabend, and their colleagues in the West. Some members of the young generation of philosophy, starting their careers in the late 1960s, however, were fervently interested in these subjects, but at that time their intellectual impact upon the Hungarian historians of science was negligible in spite of the friendly personal relationships.[38]

Most of the mainstream historians of science isolated themselves from all this political and ideological mess around philosophy. They had many reasons for doing this, including that many, though not all, of them presumably did not support the political regime and its ideology in any interpretation.

Atheoretical Attitude of the Historians of Science

The question arises whether the Hungarian historians of science were really blind in the Lakatosian sense, meaning that they did not follow any philosophical line at all, or whether their work was restricted to factology.[39] The short answer is that most of them did not study philosophy and they did not consider history of science to be an illustration or a store of case studies for philosophical ideas, even less to be rational reconstruction of research programs or paradigms. They did not assume that philosophy is an intellectual precondition for doing history of science. Nevertheless, they unintentionally shared philosophical commitments that they presumed to be plausible beyond any doubt and without any necessity to argue for or against them.

Szabadváry expressed these ideas at the beginning of his book on the history of analytical chemistry: "Technology have always played a decisive part in the growth of human welfare. Technological progress is based on natural sciences. Scientific research, starting the modern times, made the birth of our civilization possible. Thousands of researchers have observed and investigated nature for centuries. One's observation was used by the other, and the third built a theory upon these. This is how our knowledge is ceaselessly augmented."[40] Zemplén's view was similar: history of physics "shows how mankind's relation with nature developed, and with the progress of knowledge how man could use this knowledge to his purposes, then to change nature. The two components: cognition and application show how history of physics is related to the history of other sciences. As it is the science of understanding nature, history of physics is closely connected with the history of philosophy, and, as application, with the history of technology."[41] Philosophy in this context means seventeenth-century natural philosophy.

Neither the histories nor the historians of science were blind in Hungary. They just accepted without any criticism what their culture suggested as a prefabricated way of thinking. According to this thinking science acquires knowledge on nature. Knowledge mirrors nature. Science progresses in the way that knowledge gradually becomes more precise and more extended. Scientific knowledge can be applied to solving technological problems. Technological and scientific progress go hand in hand and serve the progress of mankind.

This position can be considered as the received view on science of scientists in Hungary and in many other countries in the bipolar world of the Cold War. It was the Marxist–Leninist doctrine too.[42] In general, official Marxism shared the simple empiricist, positivist epistemology with the majority of scientists, engineers, and science policy experts such as Vannevar Bush, Marxist historians of science like Hessen and Bernal, and many other historians and philosophers of science.[43] The Hungarian historians of science did not initiate, and they were not interested in any radical criticism against this view. They happily accepted it as something naturally given and they relied on it in their works.

On the other hand, this general approach to science explains the sociological position of the field. Because science was assumed to be closely connected with engineering as its application, in the committees, conferences, publications, and museums historians of science did not separate their subjects from history of technology. The section of technology sent representatives to the Complex Committee of History of Science and Technology of the Hungarian Academy of Sciences, while the section of history, philosophy, arts, or literature did not. It was justified by saying that these fields considered their history as part of their own subject, while science and technology always work in the present without giving any relevance to their past. Characteristically, the committee was headed for a long time by a historian of textile industry, Walter Endrei. Without much need of argumentation, science and technology were supposed to be linked to each other.

Conclusions

Neither science studies nor STS existed in Hungary in the Cold War period either as a movement or as an institutionalized academic subject. History of science gained some ground as an accepted field, but its interests lay in the isolation from social sciences—mainly philosophy that was considered as a kind of "thought police." On the other hand, it was the interest of the historians of science to be closely connected with science and engineering. These fields were reputed to be apolitical, atheoretical, positive, factual, useful, and safe. This stand could easily be accepted by the ideological and political authorities that followed a political line of isolating the masses from politics.

An interesting comparison can be made with the Cambridge situation accounted by Anna Mayer.[44] In Cambridge, after Herbert Butterfield, a historian, took over the leading role in the history of science from scientists, like Joseph Needham, history of science took a turn toward complex historicity. With this, a long debate started about the nature of science and its progress, its role in society, and the like. The Hungarians—Lakatos, Koestler, Polanyi, and Mannheim—directly or indirectly influenced and were influenced by this debate that imbued British thinking on science in the Cold War period. As a result, as Loraine Daston pointed out, political and cultural context became emphatic in science studies, which led to some estrangement between science, engineering, history of science, and science studies on the most-frequented international stages.[45]

In Hungary scientists played the leading roles in the formation of history of science as a field with special identity. They did not feel the attraction of externalism, cultural approach, and other approaches that characterized science studies in the West. They were isolated from historians as much as from philosophers. Historians had no ambitions to interfere in the matters of history of science; in fact, they were very marginally interested in the subject. The big representative book series, *History* *of Hungary*—produced by the Institute of History of the Hungarian Academy of Sciences—focused on past political matters, while history of science was described in a short, separate, not very important chapter.[46]

The views of Hungarian historians of science on their subject were consistent both with Marxism and the received view of scientists and engineers, their closest allies. This situation radically changed around the end and after the Cold War period with the appearance of a new generation.

Notes

1. Rajnai Rudolfné (ed.), *Amagyarországi tudomány-és technikatörténet konferencia. Budapest, 23–25 November 1972* [The history of science and technology conference in Hungary] (Budapest: MTESZ, 1973).
2. David Edge, "Reinventing the Wheel," in *Handbook of Science and Technology Studies*, edited by Sheila Jasanoff et al. (Thousand Oaks, CA: Sage Publications, 1995), 3. The political and intellectual background of the early period of science studies and STS is analyzed in several writings, including Stephen Cutcliffe, "The Historical emergence of STS as an Academic Field," in S. Cutcliffe, *Ideas, Machines, and Values: An Introduction to Science, Technology, and Society Studies* (Lanham: Rowman & Littlefield, 2000), 1–16, and Sal Restivo, "Introduction," in Sal P. Restivo (ed), *Science, Technology, and Society: An Encyclopedia* (New York: Oxford University Press, 2005), ix–xv.
3. As a young researcher I participated in the events described here, and I knew the participants well. Due to lack of adequate archival material, I had to rely partly on my memories, partly on my own correspondences. On the other hand, for personal reasons I do not want to speak about my activities and about people belonging to my generation. The main characters of this account belong to a generation older than mine.
4. Ivan T. Berend, *From the Soviet Bloc to the European Union: The Economic and Social Transformation of Central and Eastern Europe since 1973* (Cambridge: Cambridge University Press, 2009), 8. Berend was a leading historian in Hungary, president of the Hungarian Academy of Sciences and member of the highest bodies and committees of the Communist Party. After the collapse of the regime, he became Distinguished Professor of History at the University of California, Los Angeles.
5. The description of the political situation can be considered as a short summary of the "received view" published in many books, including the most often cited: Ignác Romsics, *Magyarország története a XX. században* (Budapest: Osiris, 1999). Translated in English as I. Romsics, *Hungary in the Twentieth Century* (Corvina, Budapest 1999).

6. Ibidem, 14.
7. A Czechoslovak team published an influential book on the issue of science and technology, in which the term was used. Richta's book was published in Hungary as Radovan Richta, *Válaszúton a civilizáció*: *a tudományos-technikai forradalom társadalmi és emberi összefüggései* (Budapest: Kossuth Könyvkiadó, 1968). Bernal's books *The Social Function of Science* had already been published: J. D. Bernal, *Social Function of Science*. George Routledge and Sons Ltd, London 1939 and J. D. Bernal, *Science in History*. Watts, London 1937.
8. Tibor Valuch points at this institutional differentiation in his article on the social historical situation in the 1970s Hungary. Tibor Valuch, "A 'gulyás-kommunizmus' valósága," *Rubicon* 10 (2001)–1 (2002): 69–76.
9. Ferenc Szabadváry, "Európai tudománytörténeti helyzetkép, különös tekintettel Magyarországra," in *Tanulmányok a természettudományok*, *a technika és az orvoslás köréből* edited by Éva Vámos and Lilly Vámosné Vigyázó (Budapest: Áron Kiadó, 1998), 13.
10. A biographical sketch about him was published in English by the Division of History of Chemistry of the American Chemical Society 2006. See: www.scs.illinois.edu/~mainzv/HIST/awards/Dexter%20Papers/SzabadvaryBioJJB.pdf, accessed 27 March 2013. Another sketch with Szabadváry's bibliographical details appeared on the occasion of his 80th birthday in *Technikatörténeti Szemle* 25 (2001–2002): 5–38.
11. Ferenc Szabadváry, *Az analitikai kémia módszereinek kialakulása* (Budapest: Akadémiai Kiadó, 1960) [translated as F. Szabadváry. *History of Analytical Chemistry* (Oxford: Pergamon Press, 1966)]. See also Ferenc Szabadváry and Zoltán Szőkefalvi-Nagy, *A kémia története Magyarországon* (Budapest: Akadémiai Kiadó, 1972).
12. Szabadváry, *History of Analytical Chemistry*, 186–194.
13. Ibidem, 205–287.
14. The chapter on phlogiston theory is introduced, among other things, in this way: "The power of king limited the political power of noble class. A rising new class, bourgeoisie, received a role of growing importance. […] The many changes meant the start of a new age, capitalism." Ibidem, 50.
15. Szabadváry and Szőkefalvi-Nagy, *A kémia története Magyarországon*.
16. F. Szabadvàry and Robert A. Chalmers, "Jöns Jacob Berzelius (1779–1848) and analytical chemistry," *Talanta* 27/12 (1980): 1029–1036; F. Szabadváry and R. A. Chalmers, "Carl Friedrich Mohr and analytical Chemistry in Germany," *Talanta* 26/8 (1979): 609–617; F. Szabadváry and R. A. Chalmers, "On the Invention of Conductimetric Titration," *Talanta* 30/12 (1983): 991–999; F. Szabadvary, "Über die anfänge der kolorimetrischenanalyse," *Talanta* 5/2 (1960): 108–111.
17. F. Szabadváry, *Antoine Laurent Lavoisier. The Investigator and His Times, 1741–1794* (Cincinnati, OH: University of Cincinnati, 1977).

18. Her relative, Gábor Zemplén, was also a historian of science working at the Technical University of Budapest. He published her biographical sketch: Gábor Áron Zemplén, "Zemplén Jolán," in Margit Baloghand Katalin S. Nagy, eds., *Asszonysorsok a 20. században* (Budapest: BME Szociológiaés Kommunikáció Tanszék, 2000), 99–107.
19. About Mátrai's positions, see the obituaries such as Zoltán Havasi, "Mátrai László 1909–1983," *Magyar Könyvszemle* 100 (1984): 130–131. Zemplén's colleague and friend, Gábor Biró, physicist, historian of physics, party activist helped a lot in the institutionalization process.
20. Jolán M. Zemplén, *A háromezeréves fizika* (Budapest: Franklin-Társulat, 1950) and J. M. Zemplén, *Roger Bacon*, *1214–1294* (Budapest: Művelt Nép Könyvkiadó, 1954).
21. J. M. Zemplén, *A magyarországi fizika története 1711-ig* [History of physics in Hungary until 1711] (Budapest: Akadémiai Kiadó, 1961); J. M. Zemplén, *A magyarországi fizika története a XVIII. században. A fizika szaktudománnyá válik* [History of Physics in Hungary in the 18th century. Physics becomes a discipline](Budapest: Akadémiai Kiadó, 1964).
22. Ibidem, 14.
23. Barna Szénássy, *A magyarországi matematika története* (*a legrégibb időktől a 20. Század elejéig*) (Budapest: Akadémiai Kiadó, 1970). [translated as *History of Mathematics in Hungary until the 20th Century* (Berlin and New York: Springer-Verlag, 1992).
24. A good example of the genre: James Riddick Partington, *A History of Chemistry*, 4 Vols., London: Macmillan, 1961–1970.
25. László Vekerdi, "A honi tudománytörténetírás gondjairól," [On the difficulties of writing history of science in Hungary]in Rajnai, ed., *A magyarországi tudomány*, 467–475 and Géza Györgyi, "A kvantummechanika történetéről és annak néhány Magyar vonakozásáról," [On the history of quantum mechanics and its bearings on Hungary] in Rajnai, ed., *A magyarországi tudomány*, 519–527.Both Vekerdi and Györgyimention Thomas Kuhn, *Sources for History of Quantum Physics*; *An Inventory and Report* (Philadelphia: American Philosophical Society, 1967).
26. Szabó's short biography was published in an obituary: Gábor Kutrovátz, "Szabó Árpád, 1913–2001," *Magyar Tudomány* 6 (2002): 828.
27. See, for instance: Árpád Szabó, *Perikles és kora*: *Történeti és politikai áttekintés* [The age of Pericles: historical and political review] (Budapest: Franklin–Parthenon, 1942); Á. Szabó, *Sokrates és Athén* (Budapest: Szikra, 1948); Á. Szabó (with Erkka Maula Enklima), *Szophoklész tragédiái* [Sophocles's Tragedies] (Budapest: Gondolat, 1985); Á. Szabó, *Untersuchungen zur Frühgeschihte der griechischen Astronomie*, *Geographie und der Sehnentafeln* (Athen: Akademie Athen, Forschungsinstitut für Griechische Philosophie, 1982).

28. Á. Szabó, *Anfänge der griechischen Mathematik* (Munich and Vienna: Oldenbourg, 1969).
29. Szabó spoke about Lakatos's spying on him in an interview to the author.
30. About Lakatos's years in Hungary see Jancis Long, "Lakatos in Hungary," *Philosophy of the Social Sciences* 28/2 (1998): 244–311; Alex Bandy, *Chocolate and Chess: Unlocking Lakatos* (Budapest: Akadémiai Kiadó, 2010).
31. Michael Polányi, *Personal Knowledge: Towards a Post-Critical Philosophy* (Chicago: University of Chicago Press, 1958); Imre Lakatos, *The Methodology of Scientific Research Programmes* (Cambridge: Cambridge University Press, 1978). About Polanyi see William T. Scott and Martin X. Moleski, *Michael Polanyi: Scientist and Philosopher* (Oxford and New York: Oxford University Press, 2005); Mary Jo Nye, *Michael Polanyi and His Generation. Origins of the Social Construction of Science* (Chicago and London: University of Chicago Press, 2011).
32. Arthur Koestler, *Darkness at Noon* (New York: Random House, 1941).
33. A. Koestler, *The Ghost in the Machine* (New York: Macmillan, 1968); A. Koestler, *The Sleepwalkers. A History of Man's Changing Vision of the Universe* (London: Hutchinson, 1964); A. Koestler, *The Act of Creation* (New York: Macmillan, 1965). See also his biography: David Cesarani, *Arthur Koestler: The Homeless Mind* (London: Heinemann, 1998).
34. Lorraine Daston has written, in relation to the origin of STS, that "science studies could and did retrospectively lay claim to a distinguished lineage: not only Karl Marx and Émile Durkheim but also Karl Mannheim's sociology of knowledge, Ludwik Fleck's philosophy-cum-sociology of biomedical research, J. D. Bernal's Marxist approach to science policy, Mary Douglas's cultural anthropology, Ludwig Wittgenstein's philosophical reflections on rules and forms of life, and Michael Polanyi's explorations of 'personal knowledge' in science. But as a self-conscious field of inquiry science studies first came into being in the 1970s, and its touchstone text was a work in the history of science: Thomas S. Kuhn's *The Structure of Scientific Revolutions*." L. Daston, "Science Studies and the History of Science," *Critical Inquiry* 35 (2009): 798–813, on 801.
35. See their correspondence in Éva Gábor, ed., (with assistance of Dezsö Banki and R. T. Allen), *Selected Correspondence (1911–1946) of Karl Mannheim, Scientist, Philosopher, and Sociologist* (Lewiston, NY: Edwin Mellen Press, 2003).
36. See Matteo Motterlini, ed., *For and Against Method: Including Lakatos's Lectures on Scientific Method and the Lakatos-Feyerabend Correspondence. Imre Lakatos and Paul Feyerabend* (Chicago: University of Chicago Press, 1999), 21. On Polanyi's discussions with Bernal on the directions of science see also: Nye, *Michael Polanyi and His Generation*, 183–222, and

Michael Polanyi, *The Logic of Liberty*: *Reflections and Rejoinders* (Chicago: University of Chicago Press, 1951).

37. The measurements, called "suit," against philosophers made noise in the West too. Some of the group's documents were published in a special issue (number 5) of the journal *Filozófus-Per 1973–Világosság* in 1989. About the group's history see also: János Weiss, "A filozófusper és következményei," *Fordulat* 10 (2010): 168–183. On their times see finally: Gábor Boros, ed., *A hetvenes évek filozófiai lehetőségei és valósága* (Budapest: L'Harmattan Kiadó, 2010).
38. The relationship between the historians and sociologists of science in Hungary was somewhat similar to that between historians and philosophers of science.
39. Lakatos's famous *bon mot* goes "Philosophy of science without history of science is empty; history of science without philosophy of science is blind." Imre Lakatos, "History of Science and its Rational Reconstructions," in *PSA 1970*: *Proceedings of the 1970 Biennial Meeting of the Philosophy of Science Association*, edited by Roger C. Buck and Robert S. Cohen [*Boston Studies in the Philosophy of Science* 8 (1970)] (Dordrecht and Boston: Reidel, 1971), 91–136.
40. Szabadváry, *Analitikaikémia*, 5 (my translation). These sentences and some other paragraphs are omitted from the English translation.
41. Zemplén, *A magyarországi fizika története 1711-ig*., 5.
42. There were several books and journal articles explicating this view. See, for instance F. V. Konsztantyinov, ed., *A marxista filozófia alapjai* [*Foundations of Marxist Philosophy*] (Budapest: Kossuth Könyvkiadó, 1963); János Fedor, *A marxizmus leninizmus alapjai* [*Foundations of Marxism–Leninism*] (Budapest: Kossuth Könyvkiadó, 1963).
43. Vannevar Bush, *Science*: *The Endless Frontier. A Report to the President* (Washington, D C: United States Government Printing Office, 1945). Boris Hessen's key work "The Social and Economic Roots of Newton's Principia" has recently been republished in Gideon Freudenthal and Peter McLaughlin, eds., *The Social and Economic Roots of the Scientific Revolution*: *Texts by Boris Hessen and Henryk Grossmann* (Dordrecht: Springer, 2009), 41–101. As mentioned before Bernal's books were translated into Hungarian, but his opponents were not even mentioned.
44. Anna-K. Meyer, "Setting up a Discipline: Conflicting Agendas of the Cambridge History of Science Committee, 1936–1950," *Studies in the History and Philosophy of Science* 31/4 (2000): 665–689; A.K. Meyer, "Setting up a discipline, II: British history of science and 'the end of ideology', 1931–1948," *Studies in History and Philosophy of Science* 35/1 (2004): 41–72.
45. According to Daston, "After Kuhn, both science studies and the history of science deliberately adopted a position of estrangement toward contemporary

science, but they did so for different reasons that ultimately led to divergent understandings of science and how to study it [...]. Historians of science entertained fewer suspicions about the deceptions or self-deceptions of contemporary scientists apropos of current science" (Daston, "Science Studies and the History of Science," 805). Sheila Jasanoff also spoke about the divergence of history of science from science studies in S. Jasanoff, "Reconstructing the Past, Constructing the Present: Can Science Studies and the History of Science Live Happily Ever After?" *Social Studies of Science* 30/4 (2000): 621–631. See also Charles E. Rosenberg, "Woods or trees? Ideas and actors in the history of science," *Isis* 79/4 (1988): 564–570.

46. Pach Zsigmond Pál, ed., *Magyarország története* [History of Hungary], 10 Vols. (Budapest: Akadémiai Kiadó, 1976–1989).

CHAPTER 7

The Science of Science (*naukoznawstwo*) in Poland: Defending and Removing the Past in the Cold War

Michał Kokowski

We know surprisingly little about the history of Polish *naukoznawstwo* (science of science) despite some of its contributors (Kazimierz Twardowski, Maria Ossowska, Stanisław Ossowski, Tadeusz Kotarbiński, Kazimierz Ajdukiewicz, Florian Znaniecki, Ludwik Fleck, Stefan Amsterdamski) having gained international recognition.[1] This leads to a paradox because the fundamental axiom of all historical science is the search for the historical context of the subject under study. So the following questions come to mind: Why is the historical context in which Polish contributions to science of science emerged yet to be fully understood by historians? And more generally: What are the distinctive elements of the Polish history of science of science? When did science of science emerge in Poland? And how can we relate it to political developments in Poland, especially during the Cold War?

To answer these questions on the grounds of historical analyses, we can state what follows. There is an important tradition in science of science in Poland dating back to the early twentieth century, but the scholars who

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were part of this tradition struggled to receive recognition in the post-war years (the late 1940s). The chief reason for this was political. Soviet influence on Polish cultural affairs meant that those references that did not overtly refer to dialectical materialism were removed or omitted. Thus, the very past and legacy of Polish science of science became the bone of contention in establishing it after World War II (WWII). Those who were associated with Soviet intellectuals sought to remove or omit references to the Polish *naukoznawstwo*, and those who challenged this association had an interest in defending (or mobilizing) the past recalling that science studies had autonomously developed in Poland. This happened gradually and especially as a consequence of the open challenges to the communist regime that happened with protest movements of 1956, 1968, and 1980s–1990s with Solidarity. Finally, a new paradox emerged in Poland in the 1990s, when the Cold War ended. Science of science was organizationally significantly weakened for political reasons, because new Solidarity politicians and the organizers of science erroneously equated it with the Soviet ideology.

The Polish Tradition in Science of Science

Naukoznawstwo emerged in Poland in the 1910s–1920s. It was a result of intellectual interactions and cooperation between Stanisław Michalski from the Academic Section of the Józef Mianowski Fund, and, among others, the representatives of the Lvov–Warsaw School of Philosophy, including Kazimierz Twardowski, Maria Ossowska, Stanisław Ossowski, Tadeusz Kotarbiński, Kazimierz Ajdukiewicz, and other thinkers operating on the fringe of this school, such as the philosopher and sociologist Florian Znaniecki.

From 1916 to 1939, the Academic Section of the Józef Mianowski Fund established a center for research into the science of science.[2] It organized congresses (1920, 1927) and a seminar (from June 1928). It also published the specialist journals *Nauka Polska. Jej Potrzeby, Organizacja i Rozwój* (Polish Science. Its Requirements, Organization and Development; from 1919), and *Organon* (from 1936)—all edited by Stanisław Michalski—and established a library and documentation center (managed by Janina Małkowska) while cooperating with many universities in the country and abroad.[3]

Among many enlightening articles published in *Nauka Polska* from 1919 to 1939, two of them have special importance for this article, namely, the two often cited programmatic papers about a new branch of knowledge named *naukoznawstwo*. These were “Przedmiot i zadania nauki o wiedzy” (1925) by Znaniecki and “Nauka o nauce” (1935) by Maria Ossowska and Stanisław Ossowski.[4] Another very interesting contribution to the science of science developed by a Polish author was the work of Ludwik Fleck, a microbiologist, immunologist, historian, and philosopher of medicine, the author of the 1935 *Entstehung und Entwicklung einer wissenschaftlichen Tatsache. Einführung in die Lehre vom Denkstil und Denkkollektiv*.[5]

Before WWII, Fleck’s *opus magnum* was met with quite considerable interest, since about 20 of its reviews were published in Germany, Austria, Switzerland, Sweden, Italy, Belgium, the Netherlands, and Poland. In addition, it was mentioned by Hans Reichenbach’s monograph *Experience and Prediction* published in the USA.[6] These elaborations were written mainly by experts in medicine and biology, and three by experts in philosophy. Nearly all of them assessed Fleck’s monograph very highly.[7] Nevertheless, at that time, his thought was not incorporated into the body of accepted knowledge constituting *naukoznawstwo*, as has been rightly noticed by Gad Freudenthal and Ilana Löwy: “[H]e remained a marginal outsider to all professional groups [of scientists (bacteriologists), historians and philosophers of science, professional philosophers, and the ‘lay’ public] whom he sought acceptance.”[8]

During WWII, the invaders, Germany and the Soviet Union, closed down all Polish universities, scientific societies, and scientific foundations and seized their properties. Despite this, Polish scholars continued to conduct research and teaching activities in secrecy.[9] Many of them were imprisoned, many were killed (especially Jews) or—to protect their lives—fled Poland. It was estimated that the losses amounted to 30–40 % of the Polish scholars. Polish science also suffered material losses. Some scholars developed higher education activities and scientific research in exile.[10]

A cultivation of the science of science was subjected to the same restrictions and obstacles. The invaders dissolved the centers dealing with this subject matter: Lvov University and Warsaw University, where the Lvov–Warsaw School of Philosophy acted (many of its Jewish members were killed), and the Mianowski Fund. Michalski and Fleck were imprisoned in German concentration camps: the former for his conspiratorial research activities, and the latter because he was a Jew. Znaniecki did not return from the USA to Poland. Because of the war, the Mianowski Fund suffered huge material losses, including its seat (located in the Stanisław Staszic palace), publications, warehouses of books, archives and manuscripts, items in print.[11]

Soviet Pressures on Polish Science of Science

After WWII, there was an urgent need in Poland to rebuild its infrastructure, economy, and learning facilities (the Poles had limited possibilities to learn during the war in a systematic way). It was also important to reinstate the Polish intelligentsia, including university staff, and rebuild a system of higher education. The subject matter of the science of science played an important role in this context. The scholars who survived the war and lived in Poland, including the members of the Mianowski Fund, the Lvov–Warsaw School of Philosophy, and Ludwik Fleck, resumed creative work at this branch of knowledge with great commitment.

This interest was also cultivated by a new generation of scholars assembled in the *Konwersatorium Naukoznawcze* (the Science of Science Seminar) of Kraków. The aim of the seminar was "practice of the science of science, a struggle to bring science to the public and the public to science."[12] In order to meet these targets, the members of the group discussed all aspects of the science of science, especially the organization of science in Poland. Moreover, the Seminar published the journal *Życie Nauki. Miesięcznik Naukoznawczy* (Life of Science. Monthly Journal of the Science of Science), playing a crucial role in the debate on the science of science, including the examination of the utilitarian and worldview aspects of scientific research.

The scholars specializing in the science of science (e.g. Mieczysław Choynowski, Kazimierz Ajdukiewicz, Ludwik Fleck) and the representatives of the government authorities (e.g. Bolesław Bierut, Eugenia Krassowska) interested in different aspects of organization of science in Poland published their texts in *Życie Nauki*.[13]

However, all these activities happened in a new political situation caused by the fact that Poland had found itself (like other countries of Central and Eastern Europe) in the sphere of Soviet influence.[14] As a consequence, the entire Polish state was being reorganized in accordance with the political paradigms of the Soviet state, based on the totalitarian communist ideology. The bases of this system were: planned economy, the idea of a classless society, and intellectual, psychological, and physical terror (based on the activities of the security forces and extensive imprisonment of dissidents) against all real or only imaginary enemies.[15]

The sciences played a crucial role in the communist Soviet ideology, since they were considered a necessary tool to build a communist society and to fight with the "bourgeois" culture and the religious superstition.

In this context, new ideas emerged, such as that of planning all the activities of science, and "the frontline of science": a system of management of all sciences including the careers of scientists and humanists.[16] Therefore, "the guiding force of the Polish state" (i.e. the governing parties that accepted the communist–socialist ideology) made a huge effort in order to change the philosophical interpretations of science and to rebuild the entire system of organization of Polish science according to the Soviet rules, which were entirely alien to the pre-war Poland.

From the late 1940s, Polish universities and scientific societies were sought to be aligned with Marxist, Leninist, and Stalinist ideological interpretations of the humanities and social sciences (philosophy, history, linguistics, etc.), as well as the natural sciences (genetics, physics, etc.). Sciences and learning had to be interpreted in accordance with the Leninist principle of party leadership, and was now perceived as a weapon in the struggle for the victory of communism in the world.[17] Fundamental works by Marx, Engels, Lenin, Stalin were now translated in Polish such as those of other Soviet scholars (Zhdanov, Lysenko, and Vladimir Aleksandrovich Fock). Training visits of many of these Soviet scholars to Poland occurred frequently. Polish proponents of Soviet views on science treated them as an authority on scientific and philosophical matters. However, while Lysenkoism found many supporters among Polish biologists between 1949 and 1955, the reception of the Marxist interpretations of quantum mechanics was rather limited among physicists.[18]

The leading role in this transformation of Polish science was played by the Instytut Kształcenia Kadr Naukowych przy KC Polska Zjednoczona Partia Robotnicza (PZPR) (Institute for Education of Scientific Staff at the Central Committee of the Polish United Workers' Party) established in Warsaw in 1950 following a decision by the Central Committee of the Polish United Workers' Party. Professor Adam Schaff was the founder and director of the new Institute.[19]

These regime "thinkers" severely criticized the thought of the leading representatives of the "bourgeois thought" in Poland, especially the members of the famous Lvov–Warsaw School, the Polish Catholic school of philosophy (Neo-Thomism), and Florian Znaniecki's school of sociology.[20] In an analogous spirit, in 1951, the Editorial Office of the journal *Myśl Filozoficzna* (Philosophical Thought), with Adam Schaff as the editor-in-chief, treated the sociological studies developed by the school of Florian Znaniecki, as reactionary, "alien to the working class."[21] Then, in order to "re-educate" Polish academics to the new standards of thinking, the 1st Congress of Polish Science was held in Warsaw in the summer of 1951 and attended by more than 1600 participants. The plan to rebuild the entire Polish science in the image of Marxist ideology, in its contemporary Stalinist version, was "unanimously" accepted and proclaimed.[22]

In the late 1940s, many university scholars (circa 1000), who did not surrender unto the *intellectual* Soviet terror, especially the representatives of the Lvov–Warsaw School of Philosophy, were deprived of the right to educate students or even to work at universities.[23] For example, Tadeusz Kotarbiński, the rector the Łódź University between 1946 and 1949, and head of the Department of Philosophy between 1946 and 1950, was forced to resign.[24] The purges marked the end of the Science of Science Seminar that operated until December 1949. The reorganization of the seminar and *Życie Nauki* by Choynowski was truly a great achievement and he had plans to establish a full institute for researching the science of science. Unfortunately, since his political views were not in "full agreement" with the political orthodoxy of these years, he was forced to resign. As a consequence, the seminar ended its activities, and the journal was moved to Warsaw.[25]

Fleck, another contributor to *Życie Nauki*, returned from the Buchenwald concentration camp in 1945 and in the following years made a brilliant career in science (microbiology and bacteriology). He also continued to cultivate his interests in the theories of scientific knowledge and cognition. He eventually published two papers on these subjects in 1946 and 1947. In one he also formulated a definition of the science of science:

> *Naukoznawstwo* is a separate science based on observation and experiment, historical and sociological investigations. It is a part of the science of thought-styles.[26]

Moreover, in 1947 Fleck presented his views on the science of science and the theory of cognition during a seminar held in Lublin. But in the late 1940s and early 1950s, his thought did not gain greater recognition in Poland since his "relativistic" idea of socially and historically constructed truth could not be accepted either by the rational philosophers from the still existing Lvov–Warsaw school or the dialecticians from the newly founded Marxist school.[27]

Other scholars, however, tried to develop the Polish science of science further in these harsh times. After being released from the Auschwitz II–Birkenau concentration camp, Stanisław Michalski stayed in Kraków to direct the Scientific Division of the Mianowski Fund. Michalski was still the editor of the journals *Nauka Polska* and *Organon* and he had plans to develop the science of science, to organize conferences on the organization of different branches of knowledge, to create a book series entitled *Wiedza o nauce* (The Knowledge of Science), to prepare a university textbook on the science of science, and to start courses on the science of science for academic decision-makers and university students.[28] Moreover, Michalski had a plan to restore the Council of the Fund and make it a sort of "Non-governmental, Social Ministry of Science."[29] Unfortunately, for both economic and political reasons, Michalski was not allowed to implement this idea. He could not resume the activities of the pre-war Circle of the Science of Science of the Mianowski Fund either. Nevertheless, its library was reopened and enabled scholars to produce new research on the science of science.

However, when the 25th (special jubilee) volume of *Nauka Polska* was published, it was condemned by prominent party activists since it was edited in the pre-war spirit and layout, without references to the Marxist, Leninist, and Stalinist ideology. In other words, from the point of view of communists, there was no future in Poland for the Mianowski Fund. As a consequence, the Ministry severely limited the financing of the Fund's activities, although a Science of Science Commission of the Mianowski Fund could be established.[30]

Co-Existence of "Old" and "New" Traditions in Polish Science of Science

During the Cold War years, the science of science continued to be at the center of attention of many Polish thinkers. However, it was increasingly affected by the new political climate. The centers that continued the pre-war Polish tradition of the science of science were dissolved or significantly weakened, while the regime politicians supported a large group of social scientists who began to lay new foundations for the discipline. In their approach, a clear divide between science policy and the theoretical reflection on the science policy began to disappear. It all happened in full agreement with the Marxist–Leninism methodology, even if important corrections were made between 1956 and 1968.

The death of Joseph Stalin and the critique of Stalin's Cult of Personality by Nikita Sergeyevich Khrushchev paved the way to the new Soviet leader's *odtepel'* (thaw). The death of the Polish Communist leader Bolesław

Bierut (on March 12, 1956, in Moscow) significantly weakened the hard-line Stalinist faction in Poland. In consequence, from 1955, numerous Polish thinkers—mainly Marxists and political activists from PZPR (Polish United Workers' Party)—feeling a whiff of freedom, severely criticized the Stalinist terror in Poland. This criticism covered the whole sphere of state activity, including cultural and scientific activities.[31] Among these critics were Adam Schaff,[32] the members of the Warsaw school of history of (political) ideas (Bronisław Baczko, Leszek Kołakowski, Jerzy Szacki and Andrzej Walicki), and the scientifically oriented philosopher Stefan Amsterdamski (1929–2005) from University of Łódź. At the philosophical core of these critiques was an analysis of Marxism–Leninism in the light of the historical method (especially including the idea of historicism). This method showed that the straight diamat approach is a kind of ideology.

The real political breakthrough happened in 1956 with the Polish October (or Gomułka's thaw, from the name of the leader of the Polish Communist Party Władysław Gomulka). The communist government loosened inner politics. A critical discussion of the distortions of the power system in the Stalinist period was taken up in all branches of social life (including science and universities). Victims of political persecution were rehabilitated and the scholars expelled under Stalin returned to work at universities; Znaniecki was restored as the creator of the sociological school; there was also a critique of ideological scientific theories developed in the Stalinist era (including Lysenkoism).

In the spirit of liberalization of all Polish cultural life of the mid-1950s, many prominent scholars, such as, among others, Józef Chałasiński (Deputy Secretary of the Polish Academy of Sciences from 1952 to 1958) and Henryk Jabłoński (Secretary of the Polish Academy of Sciences between 1955 and 1965), criticized the "oppression of Polish science by Stalinism," and in particular the "ideologization" of the system of management of Polish science by Stalinists. They also argued for restoring the autonomy of all sciences in Poland and a reform of the Polish Academy of Sciences.[33]

However, already in 1957, the Stalinist methods of control were restored and moreover a wave of anti-Semitism swept through Poland.[34] In the middle of the 1960s, Gomułka changed its political course, seeking to strengthen "good relations" with the USSR leading to rejecting the demands of the democratic student movement and cleansing the PZPR from revisionist elements. A real Communist–Fascist–anti-Semitic hysteria followed suit.

The repression hampered interesting developments in science studies. For example, the Secretary of the Polish Academy of Sciences, Henryk Jabłoński, in a speech given during the General Assembly of the Academy stated:

> Non-scientific factors determined many issues of science, among other things many scientific concepts. It is enough to mention the imposing to the biologist the concept of Lysenko or dogmatic approach to the concept of Pavlov (I do not judge whether these concepts were legitimated or false, because, in fact, this is of secondary importance); it is enough to mention philosophical, economic and historical sciences, where own research was often replaced by the search for arguments to the theses imposed from the outside; it is enough to mention the reducing to a minimum the possibility of development of sociology under the influence of the theory proclaimed by non-scientific factors, that such learning did not exist.[35]

As a consequence of the critiques of this sort, in 1956, the ("bourgeois") sociology and ("bourgeois") philosophy were recognized as valuable subjects of research. The result was not only the reinstatement of these disciplines at universities, but also the establishment of an Institute of Philosophy and Sociology at the Polish Academy of Sciences.

Adam Schaff, the Institute's director, now employed eminent scholars who had played a role in the pre-war development in science studies—among others, Kotarbiński, Ajdukiewicz, Ossowska, Ossowski, and two prominent postwar sociologists: Józef Chałasiński (student of Znaniecki) and Jan Szczepański.[36] All of them were interested in different aspects of the science of science. Moreover, in 1957, a nationwide seminar on the philosophy of science was organized by the Institute, and Helena Eilstein was appointed as the head of this seminar.[37] And in the same year the *Pracownia Ogólnych Problemów Organizacji Pracy PAN* (Research Centre for the General Problems of Labor Organization, PAS) was established, and chaired by Tadeusz Kotarbiński. *Praxeology*, that is, the theory of efficient action, a field of research on all purposeful human activity, constituted the theoretical basis of this center.[38]

The science of science was also researched with great attention in the Department for History of Science and Technology of the Polish Academy of Sciences, established in 1956 and chaired by former Mianowski Fund associate Bogdan Suchodolski.[39] The Department also employed Wanda Osińska, who worked earlier at the Academic Section of the Mianowski Fund until its incorporation on March 3, 1951 into

Warszawskie Towarzystwo Naukowe (Warsaw Scientific Society) that was dissolved in 1956. She helped to keep alive the memory of the Mianowski Fund in this Department. The Department had even plans to set up a Centre for Research on the Science of Science and a Centre of Theory and Organization of Science. These plans were put into effect in 1964 and 1969, respectively.[40]

Furthermore, in 1963 the Scientific Secretary of the Polish Academy of Sciences passed a resolution establishing the *Komisja Naukoznawstwa PAN* (Commission of the Science of Science of the Polish Academy of Sciences).[41] A planned *Pracownia Badań Naukoznawczych* (Centre for Research on the Science of Science) of the Department for History of Science and Technology, PAS was to provide logistic and financial support of this Commission. *Naukoznawstwo* was defined as "the scientific discipline (a group of disciplines) whose subject is the scientific activity and science as a storage of knowledge."[42] From 1963 to 1968, Ignacy Malecki chaired the Commission.[43] He was interested in the problem of perspective planning of scientific activities and coordination of scientific research, and in 1964 he published a programmatic paper rooted in the Marxist thought entitled *Z zagadnień metodologicznych nauki* (On the methodological issues of science), in which he emphasized the need to develop a practical attitude in the reflection on the science of science.[44]

> "The modern science of science, called […] *naukoznawstwo*, is […] a discipline which puts […] the practical purposes" next to the theoretical generalizations. "The question posed to the methodology of sciences by the economic and industrial management focuses mainly on the research directions that would yield maximum benefit". "When choosing research directions one needs to look […] also for means […], which consists of financial expenditure, academic staff and organizational frameworks […] the rapid development of the modern science of science is corroborated by the fact that now 29 magazines which almost exclusively deal with these issues appear in the world […] *Naukoznawstwo* is not just a […] descriptive theory, but […] becomes an instrument of action, helping people who are in charge of scientific institutions and other scientists".[45]

Other politicians, including the authorities of the Ministry of Higher Education and Science such as Henryk Jabłoński, formulated this sort of practical hints.[46]

This style of thought provided new opportunities to the promoters of the science of science. So, in November 1964, the aforementioned Centre for Research on the Science of Science was established at the Department for History of Science and Technology. It was managed by Aleksander Tuszko until 1 October 1968.[47] The unit conducted research on the current organization of science, its funding and administration, and various applications of science and technology for development purposes. The workers of this unit described these matters objectively, regarding the Soviet Union and the Western countries, as well as international organizations, such as the United Nations Educational, Scientific and Cultural Organization and the United Nations.

Furthermore, on February 18, 1965 Pracownia Badań Naukoznawczych Zakładu Historii Nauki i Techniki PAN (Centre for Research on the Science of Science of the Department for History of Science and Technology, PAS) and Pracownia Ogólnych Problemów Organizacji Pracy PAN (Centre for the General Problems of Labor Organization, PAS) established the *Konwersatorium Naukoznawcze PAN* (The Seminar of the Science of Science). It continued the tradition of the pre-war Circle of the Science of Science of the Mianowski Fund and the postwar Kraków Seminar of the Science of Science. The seminar was managed by Tadeusz Kotarbiński.[48] In his papers, he developed the logical analysis of science in the tradition of the Lvov–Warsaw School of Philosophy. In addition, he positively referred to the Marxist idea to intensify the practical aspects of the science of science.

This was seemingly a climax in the coexistence between old and new tradition in Polish science studies. At this point in time the 11th International Congress of the History of Science, organized by the Department for History of Science and Technology took place in Warsaw and Kraków, August 24–31, 1965. A crucial role of the science of science for the history of science, all culture, and the development of science and technology was emphasized during this congress. Biologist and politician Stanisław Kulczyński stated in his address:

> I have been entrusted with the honorable task of greeting the International Congress of History of Science from the Polish science and Polish politics. I cannot do it from a position of science with its back turned on politics, nor from a position of politics with its back turned on science. I can do so from a position that combines science with politics. This position can be only a common and realistic method of thinking of politicians and scholars.[49]

John D. Bernal and Alan L. Mackay gave a plenary lecture "Towards the Science of Science" at the opening session of the congress.[50] Finally, the International Symposium "The past and future of science" was held at the end of this congress in Kraków. Attended by 100 participants, among its speakers were Bonifatij Michajłowicz Kedrow (USSR), Gennadij Michajłowicz Dobrow (USSR), Derek J. de Solla-Price (USA), René Taton (France), and Ignacy Malecki (Poland).

Polish scholars specializing in science of science were rather well-informed about the developments in the West and the work of international scholars, especially John D. Bernal, Derek de Solla-Price, and the Soviet scholars.[51] They were acquainted with the Soviet science of science not only through Polish–Soviet conferences on this subject matter, but also through the reading of the original works or translations of these works into Polish or English, and the reviews of many these works.[52] On the other hand—as Eugeniusz Olszewski has shown—at least until 1968, the Soviet scholars were rather faintly familiar with the Polish postwar science of science (nevertheless they were positively disposed toward the Polish achievements in this field).[53] Furthermore, contemporary foreign scholars, including the Soviet ones, and even the Polish ones, had rather limited knowledge of the Polish pre-war science of science. In the context of the current reinterpretation of earlier science of science contribution, the original role played by Polish scholars was virtually neglected and replaced—in a sort of rewriting of its history—by the study of Bernal.[54] However, as we have seen, research of this kind, excluding scientometrics, had been developed in Poland in the Mianowski Fund at least from 1916. This limited knowledge of the Polish pre-war science of science among foreign authors was caused in part by the same Polish postwar scholars who insufficiently emphasized Polish contributions in this field. On the other hand, Polish postwar scholars could not proclaim this thesis openly, since all Polish pre-war initiatives in the field of the science of science were made in a non-Marxist spirit, and just this aspect was consequently contested by Polish communist activists from 1945. In other words, there was a political reason for such attitude toward the pre-war Polish achievements. The advocates of Marxism consciously neglected or even not mentioned these achievements and did so in order to "sell" the science of science to the current Polish politicians. Bernal's works, written in a Marxist mode, were good tools in order to realize this aim. Nevertheless, the Polish representatives of the science of science did not forget these achievements.

In 1966, the PAS Department for History of Science and Technology organized a council on the study of the theoretical issues of the science of science. During the council, Paweł Rybicki and Bodgan Suchodolski divided the science of science into two groups of issues: of theoretical nature (researched especially by philosophy of science, sociology of science, and history of science) and of practical nature (skills of rational planning, organizing, and financing the development of science).[55] By contrast, Aleksander Tuszko, who was manager of the Centre for Research on the Science of Science at the Department for History of Science, PAS, was an advocate of a utilitarian approach to the science of science. Therefore, in his article entitled "Science of Science as a Tool for Action" (1966), he treated the science on science

> as a complex scientific discipline which investigates and analyses elements and factors which influence the development of science as well as the efficiency and the effectiveness of research activity.[56]

Old and new traditions in Polish science studies merged in the new approach pioneered by Suchodolski and published in an article in which he postulated an integrated research program of internal and external factors in the development of all sciences, not only exact sciences and natural sciences, but also social sciences. He understood the term *naukoznawstwo* in a broad sense, using it to denote all disciplines examining science (including its organizational aspects and the prediction of its development). However, according to him, this set of branches does not lead to a research program that would have a theoretical value and was practically useful. This is the reason why he postulated that: (1) the term *naukoznawstwo* in a broad sense was best avoided, (2) the term "nauka o nauce" ("science of science") was used to designate this group of sciences, and (3) a threefold distinction between these sciences was made: these were: (a) theory of science (it includes philosophy of science, sociology of science, and psychology of science) identified with *naukoznawstwo* in the narrow sense (which is the right sense), (b) history of science, and (c) science policy. The first two disciplines have cognitive status (as they are engaged in studying reality), and the third has a practical status (because it deals with the transformation of the social reality).[57]

Furthermore, in the 1960s the works of Thomas Samuel Kuhn caught the attention of many Polish scholars. His two books: *The Copernican revolution. Planetary astronomy in the development of Western thought* (1957) and *The Structure of Scientific Revolutions* (1962) were not only reviewed in Poland but also translated, respectively, in 1966 and 1968. Marxists Eugeniusz Olszewski and Stefan Amsterdamski played an especially important role in its reception. The latter translated *The Copernican* *revolution* into Polish and wrote a postscript to this book. Two years later, he helped publishing the translation of *Structure* and wrote a postscript to this book entitled *The History of Science and the Philosophy of Science*.[58]

But the coexistence between new and old hid a far more worrying reality as many protagonists of Polish history of ideas and Polish science of science fell afoul of political leaders. Between 1955 and March 1968, Marxist philosophers and members of the PZPR such as Adam Schaff, Bronisław Baczko, Leszek Kołakowski, and Stefan Amsterdamski were labeled by the Stalinists the (Marxist) "revisionists." It was a very pejorative, essentially political label, regardless of whether the doctrine they preached was philosophically different from the accepted interpretation of Marxism. This accusation meant as much as the Stalinist accusation of *idealism* in the late 1940s and early 1950s.[59] This political fight with the revisionists was connected with an anti-Israeli (anti-Zionist) stance of the Polish government and the PZPR policy, which, in 1967, quickly turned into an anti-Jewish campaign.[60]

As a result of a real Communist–Fascist–anti-Semitic hysteria, after March 1968, thousands of "revisionists" and Jews were removed from jobs in public services, including teaching positions at schools and universities. Among them were Adam Schaff from the University of Warsaw and the Institute of Philosophy and Sociology at the Polish Academy of Sciences, and Stefan Amsterdamski from the Institute of Philosophy at the University of Łódź.[61] This hysteria caused the migration of 15,000–20,000 Polish citizens from Poland since 1968–1972.[62]

The Bleak 1970s

After March 1968, Polish politicians (who were dominated by Communists and were against Polish intelligentsia) lost interest in promoting scientific culture (including the science of science) and became sworn enemies of the so-called *socialism with a human face* (developed by the advocates of non-communist socialism).[63] Since many Polish scholars proved to be opponents of communism and communist socialism, the media sketched the image of the science in the phase of a deep crisis.[64] Moreover, in the 1970s, economic depression and political upheaval was growing in Poland. And in this context, "many experts of the science of science dealt with issues of broader social activities which science is only small part of"; in effect, the majority of scholars studied the science of science only at the margin of their curricula, and thus, new work was dominated by historical and semantic approaches (that were, in principle, deprived of political cogency).[65]

In December 1970, after a political crisis caused by a revolt of the workers on the Coast protesting which was brutally suppressed by the police and the army, a new communist government of Edward Gierek was established. In order to promote socio-economic development, Gierek extensively used foreign loans that produced a dramatic collapse and a huge foreign debt, accompanied by strikes and demonstrations (in June 1976). These were brutally suppressed by the police and the army and produced the creation in 1977 of an independent democratic opposition.

In 1973, the Committee for the Science of Science (chaired by Ignacy Malecki) and the Committee for the History of Science and Technology (chaired by Bogusław Leśnodorski) sought to create an Institute for the Science of Science (at the Polish Academy of Sciences) that was to link the departments of the Polish Academy of Sciences that did research on different scientific disciplines from the meta-scientific point of view (namely, by exploring connections with history of science and technology, history of arts, history of literature, philosophy, etc.). However, the initiative failed:

> I will not disclose a secret by saying that the 2nd Congress of Polish Science disappointed the Committee for the Science of Science and the Committee for the History of Science and Technology by not laying the foundations for the creation of the Institute for the Science of Science.[66]
>
> Perhaps this influenced the debate over the formula of the science of science that was demonstrated at the 2nd Congress of Polish Science. The dispute was the consequence of the interests of the science of science concentrated on the history of science, just as it was cultivated at the Institute of History of Natural Sciences and Technology, Academy of Sciences of the Soviet Union and the science of science based on Kotarbiński's programme, hence the (despised) praxeological provenance. (What should be remembered from that period is the *votum separatum* of professor E. Olszewski during the formulation of the draft resolution of the Congress of Polish Science).[67]

Bogdan Suchodolski played an important role in the course of these events. He was the chairman-rapporteur of the congress discussion group for "the science of science, history of science and technology" and upheld the idea, already formulated by him in 1969–1972, that the science of science is not a consolidated, well-defined discipline, but a constellation of different disciplines that study theory, history, and praxis of science.[68]

Nevertheless, the 2nd Congress of Polish Science adopted resolution postulating the desire to "ensure the development of rare disciplines and specializations, including the science of science." In accordance with the Congress postulates, a research institute for the science of science and a distinct center for theory of science policy were to be organized.[69]

However, something else happened—the Instytut Polityki Naukowej, Postępu Technicznego i Szkolnictwa Wyższego (Institute of Science Policy, Technical Progress and Higher Education), affiliated to the Ministry of Science, Higher Education and Technology, was established. This unit had political not theoretical character and after the Congress, its development coincided with the dissolution of other theoretically-oriented organizations.

In 1973 the separate Department of Praxeology, PAS, employing over a 100-person team, was formally dissolved, and at the same time, after its significant abridgment, included in the ministerial institute.[70]

In a strange coincidence, in 1974, Suchodolski finally suggested that the term *naukoznawstwo* should be identified with a branch of practical knowledge named *polityka naukowa* (science policy) and even proposed that the very term *naukoznawstwo* should be avoided. Instead of it, he promoted the development of specialized disciplines belonging to the science of science, especially history of science.[71]

So, on May 24, 1975, the Pracownia Historii Organizacji Nauki Zakładu Historii Nauki i Techniki PAN (Research Centre for History of Organization of Science at the Department for the History of Science and Technology, PAS), established in that year and managed by Bohdan Jaczewski, held its first meeting.[72] Furthermore, in 1977, the Department of History of Science and Technology became the Institute for History of Science, Education and Technology (in full agreement with Suchodolski's hopes expressed in 1973).

In consequence, in the middle of the 1970s, the interest in the subject matter of the science of science weakened in Poland, especially regarding the practical part of the science of science (i.e. the organization of science, prognostics of science, and politics of science).

The Re-Emergence of the Old Tradition and a Sudden Real Collapse of Research

Although the late 1960s and the 1970s represented, by and large, a difficult period for the Polish society, there was a slow resurgence of themes that were familiar to those who had pioneered the science of science. This was at first an underground and independent movement that was, however, in full swing in the 1980s in connection with the rise of the *Solidarność* (Solidarity) movement.

Amsterdamski, who—as a Marxist revisionist—had been forced to resign from the University of Łódź in 1968, was employed the following year in the Department for History of Science and Technology of the Polish Academy of Sciences.[73] He became the manager of the team researching on the issue of values in science, and such matters as the value judgments about the content of a scientific theory, the role of value systems in cognitive activities of scholars, the ethos of science as a social institution, the influence of the value systems recognized outside the scientific community on the development of science, the "value-killing" and "value-creating" functions of science in social life, the problem of the value of science. Amsterdamski himself linked all these matters with the science of science in a lecture given in 1970.[74] From 1975, he was affiliated to the Pracownia Historii Organizacji Nauki (Research Centre for History of Organization of Science).

During his time at the Department, he thoroughly analyzed the thought of Thomas S. Kuhn, Karl R. Popper, and Imre Lakatos. In 1973 and 1983, he published two monographs on the so-called historicized philosophy of science, respectively, *Między doświadczeniem a metafizyką. Z filozoficznych zagadnień rozwoju nauki* and *Między historią a metodą*: *spory o racjonalność nauki*, which received international acclaim.[75] Since 1977, Popper's works, including *Logik der Forschung* (1934) and *The Poverty of Historicism* (1957), were translated into Polish, and Amsterdamski wrote a preface to the latter.[76] Then, in 1985, he translated into Polish Kuhn's *The Essential Tension. Selected Studies in Scientific Tradition and Change* (1977) and wrote a postscript to this book.[77] Throughout the period when he completed these works, Amsterdamski was still an activist of the political opposition in Poland.

In the late 1970s, the independent democratic opposition was growing in Poland.[78] In the autumn of 1977, the so-called Latający Uniwersytet (Flying University) was formed, and the Towarzystwo Kursów Naukowych (Society for Academic Courses) continued the activities of this university. Both institutions were independent educational associations whose aim was to break the monopoly of the state teaching at the university level and reveal the "white spots of the official Polish culture."[79] One of the leaders of this new cultural movement was Stefan Amsterdamski, who gave three lectures regarding the problems of science between 1977 and 1980.[80]

The period under consideration was a time of great political transformations in Poland. It began from the wave of protests and strikes of the Polish people, which led in September 1980 to the creation of *Solidarność*, the first independent trade union in the socialistic bloc. On September 8, 1981, the First National Congress of Delegates sent "the message to the working people of Eastern Europe" calling for workers of the Soviet sphere of influence to struggle together for the right of freedom of association. Then, on December 13, 1981, Wojciech Jaruzelski, the PZPR secretary and prime minister of the Polish government, declared the martial law in Poland, which lasted until 1983. During this period, activists of Solidarity were imprisoned and the organization was outlawed. In May and August 1988, a new wave of riots and protests spread across Poland. They resulted in the Communist Party deciding to approach the leaders of Solidarity for formal talks.[81] In 1989, Solidarity was legalized again and two years later the first entirely free Polish parliamentary elections since the 1920s took place.

During the period considered, several scholars that played a crucial role in the science of science in Poland passed away, including Kotarbiński (the main promoter of praxeology) and Suchodolski (the author of the first lecture given at the Seminar for the Science of Science in the Mianowski Fund in 1928). Another leader, Malecki, became the honorary chairman of the Committee he had directed in 1990.[82]

Meanwhile, scholars and politicians continued to debate on the science of science. For example, Malecki noted in the article entitled "Dyscyplinowe i problemowe podejście do naukoznawstwa oraz jego przyszłego rozwoju" (Discipline-like and problematic approach to the science of science and its future development) that the subject matters of praxeology and the science of science, developed by Tadeusz Kotarbiński and his students overlap each other:

> The interests of praxeology and the science of science overlap. This applies to that part of the science of science that Tadeusz Kotarbiński called «the practical science of science». We have here a convergence of research purposes, because the main objective of the praxeological inquiries is to provide efficient action, and the science of science has the same interest, although in a slightly different aspect. The works of the science of science and praxeology complement each other. From the point of view of praxeology, the general methodological aspects of conducting scientific research are the most important, whereas the science of science deals mainly with the specific features of these studies.[83]

From an institutional viewpoint, the Research Centre for History of Organization of Science at the Institute for History of Science and Technology, PAS, established in 1975, continued to systematically publish new elaborations. Stefan Amsterdamski was still employed in this department up until June 1991, but he spent several years abroad.[84] Scholars from other departments, centers and teams from this institute continued publishing important works on the history of organization of science, including especially the subsequent volumes of monumental *Historia Nauki Polskiej* (History of Polish Science) edited by Suchodolski.[85]

In 1980, a new unit called the Zakład Prakseologii i Naukoznawstwa (Department of Praxeology and the Science of Science) was established.[86] It was in this context that Bohdan Walentynowicz and his co-workers planned to develop a real center for the science of science (with doctoral studies and research programs with appropriate funds). However, these ideas were not recognized by the authorities of the PAS. When four years later, Walentynowicz died, many scholars left this Department.[87] Another institute devoted to Science Policy and affiliated to the Ministry of Science and Higher Education, was dissolved in 1992. And in the next year—two more units: the Department of Praxeology and the Science of Science and the Research Centre for History of Organization of Science shut down.[88]

In the 1990s, Polish academic authorities wrongly assumed that not only the programs of these units, but also the whole discipline of the science of science were, in principle, directed entirely by the old-fashioned political vision of science which originated in the communist era. In this manner, they had neglected the real achievements of all science of science in Poland from the very beginning of this branch of knowledge until the 1990s.

In the new political situation, some Polish scholars finally dared to point out that the first research of the science of science in the world was carried out in pre-war Poland in the Scientific Section of the Mianowski Fund and the research was revived in Poland after WWII at, among others, the Seminar for the Science of Science. However, Malecki and others emphasized only the pioneering character of the research done in the Scientific Section of the Mianowski Fund, and the fact that they were not known internationally.[89] What they did not add, however, was that they themselves contributed to make it possible for the pre-war Polish researchers to be overlooked in international forums. The reason was very simple. The thought of the pre-war Polish scholars did not grow out of Marxist and communist thought.

On March 29, 1990, during a meeting at the Centre for the History of the Organization of Science, affiliated to the Institute for the History of Science, Education and Technology, PAS, Jan Piskurewicz devoted his lecture to the postwar activities of the Mianowski Fund and the attempts to restore them. This had already happened in 1975 when Stanisław Małkowski, his last president, addressed a special letter to the President of the Polish Academy of Sciences calling for re-establishing the Fund and never received a reply.[90] At the 1990 meeting, a number of academics sought to reactivate this fund as an independent organization and the initiative received support from the Warsaw Society of Arts and Sciences and the Polish Academy of Sciences. On May 20, 1991, the Józef Mianowski Fund, with the additional designation, a Foundation for the Promotion of Science was finally re-established.[91] However, the Fund did not continue *researching* in the field of science of science, but focused only on providing grants to publish books and study abroad.

Another interesting twist in the story concerns Fleck. His views became popular in the Western culture only in the 1970s, when Kuhn made Fleck's monograph one of the main foundations for his world bestseller *The Structure of Scientific Revolutions* (1962). However, only in 1981(!), Fleck's views became popular in Poland, and it was only in 1986 that the first translation of his *opus magnum* was published.[92] The neglect of his work was caused by the incompatibility between postwar philosophical views and his approach. Furthermore, the works of Florian Znaniecki were translated or republished in Poland in the 1980s.[93]

Nevertheless, in the 1990s the integrated (both pre- and postwar) approach of the science of science was, in principle, rejected in Poland, and this field of knowledge collapsed organizationally. It was replaced by studying different sub-branches of the science of science, especially the philosophy of science, the history of science, and scientometrics.

Conclusion

The development of science of science in Poland as a branch of knowledge was shaped by the local political context (which is obvious) and the global one (which is not so obvious). At the beginning of the Cold War in Poland the communist leaders and their advocates postulated the abandonment of the pre-war style of doing science (which succeeded only partially) and caused the closure of centers that promoted the approach, including, among others, the Józef Mianowski Fund and the Science of Science Seminar in Kraków.

The Cold War was responsible for the dynamic growth of the science of science (i.e. an integrated reflection on science), related to the creation of the four active institutions: the Committee of Science of Science, PAS; the Department for History of Science and Technology, PAS (especially, Seminary of Science of Science directed by T. Kotarbiński, and the Research Centre for History of Organization of Science at the Institute for History of Science and Technology, PAS); the Institute of Science Policy, Technical Progress and Higher Education, affiliated to the Ministry of Science, Higher Education and Technology; the Department of Praxeology and the Science of Science, PAS.

The end of the Cold War resulted in a serious limitation of research in the science of science at the Institute for History of Science (part of the employees undertook active practice of science policy in government) and the closing of two other centers: the Institute of Science Policy, Technical Progress and Higher Education, affiliated to the Ministry of Science, Higher Education and Technology and the Department of Praxeology and the Science of Science, PAS.

From the late 1950s to 1980s, as an indirect consequence of the open challenges to the communist regime in the protest movements of 1956, 1968, and 1980s with Solidarity, Polish scholars recognized the achievements of Znaniecki, Maria and Stanisław Ossowski, Kotarbiński and Fleck (in the process the very past legacy of Polish science studies was recalled and accepted) and also learnt more about the English-speaking philosophy of science (Kuhn, Popper, Lakatos). However, in the 1990s, Polish science of science was seriously organizationally weakened. That is to say, it abandoned the integrated approach in the reflection on science both in the style of pre-war and postwar times. These reflections were replaced by a development of particular branches of science studies, mainly of the philosophy of science, the history of science, and scientometrics. These decisions caused a further development of these branches but influenced negatively the further development of the organization of science and higher education in Poland.

Notes

1. On these contributors see Bohdan Walentynowicz, ed., *Polish Contributions to the Science of Science* (Boston: D. Reidel, 1982); Stefan Zamecki, *Koncepcja nauki w szkole lwowsko-warszawskiej* (Wrocław: Zakład Narodowy im. Ossolińskich, 1977); Robert K. Merton, *Teoria socjologiczna i struktura społeczna* (Warszawa: Państwowe, 1982); Jan Woleński, *Logic and Philosophy in the Lvov–Warsaw School* (Dordrecht: Kluwer Academic Publishers, 1989).

2. Founded in 1881, its history is discussed in Jan Piskurewicz, *Warszawskie instytucje społecznego mecenatu nauki w latach 1869–1906*: *Muzeum Przemysłu i Rolnictwa i Kasa imienia Mianowskiego* (Wrocław: Zakład Narodowy im. Ossolińskich, 1990). See also Leszek Zasztowt, ed., *Kasa Mianowskiego 1881–2011* (Warszawa: Kasa im. Józefa Mianowskiego—Fundacja Popierania Nauki, 2011).
3. Zdzisław Kowalewski claims the Fund organized the first cyclic seminar on the science of science in the world. See Z. Kowalewski, "Teoria praktyki naukowej jako nowa wyodrębniająca się nauka," in *Nowe specjalności w nauce współczesnej. Materiały z posiedzeń konwersatorium naukoznawczego Polskiej Akademii Nauk*, edited by Tadeusz Kotarbiński, Wanda Osińska, Eugeniusz Geblewicz (Wrocław: Polska Akademia Nauk, 1977), 87–112, on 109. On the journal see: Grażyna Wrona, "Nauka Polska. Jej Potrzeby, Organizacja i Rozwój, 1918–1939," *Rocznik Historii Prasy Polskiej* 7/2 (2004): 19–47.
4. Translated in English as F. Znaniecki, "The subject and tasks of science of knowledge," in Walentynowicz, ed., *Polish Contributions to the Science of Science*, 1–81; Maria Ossowska and Stanisław Ossowski, "Science of Science," in Walentynowicz, ed., *Polish Contributions to the Science of Science*, 82–95.
5. L. Fleck, *Genesis and Development of a Scientific Fact. Introduction to the theory of thought-style and thought-collective* [translated by Fred Bradley, Thaddeus J. Trenn, and Robert K. Merton with a foreword by Thomas S. Kuhn] (Chicago: University of Chicago Press, 1979).
6. Hans Reichenbach, *Experience and Prediction. An Analysis of the Foundations and Structure of Knowledge* (Chicago: The University of Chicago Press, 1938), 224.
7. See Thomas Schnelle, *Ludwik Fleck—Leben und Denken*: *zur Entstehung und Entwicklung des soziologischen Denkstils in der Wissenschaftsphilosophie* (Hamburg: Universität Hamburg, 1982), 341–342; Leon Chwistek et al., "Przedwojenne recenzje książki Ludwika Flecka," *Studia Philosophica Wratislaviensia* 6/2 (2011): 141–171.
8. Gad Freudenthal and Ilana Löwy, "Ludwik Fleck's Roles in Society: A Case Study Using Joseph Ben-David's Paradigm for a Sociology of Knowledge," *Social Studies of Science* 18 (1998): 625–651, on 644–645.
9. Piotr Hübner, "Polityka naukowa 'państwa podziemnego' w Polsce 1939–1944," *Zagadnienia Naukoznawstwa. Studia i Materiały* 20/4 (1984): 571–580.
10. Andrzej Bolewski and Henryk Pierzchała, *Losy polskich pracowników nauki w latach 1939–1945. Straty osobowe*, (Wrocław: Ossolineum, 1989), 162–163. On the fate of the members of Lvov–Warsaw School see Woleński, *Logic and Philosophy in the Lvov–Warsaw School*, 19–23.
11. Piotr Hübner, Jan Piskurewicz, and Leszek Zasztowt, "Józef Mianowski Fund. A Foundation for the Promotion of Science. History," [translated

by Jacek Soszyński], available at www.mianowski.waw.pl/foundation/history/?lang=en, accessed 10/2/2013.

12. Mieczysław Choynowski, "Przedmowa," in *Bibliografia zawartości* "*Życia Nauki*", *1946–1952*, edited by Krystyna Kowalczyk, Agnieszka Paszkowska, and Julian Wójcik (Wrocław: Zakład Narodowy im. Ossolińskich, 1969), v.
13. Ibidem.
14. Soviet, US, and UK leaders agreed on Poland's postwar circumstances during the conferences of Teheran, Yalta, and Potsdam. The country had the highest percentage (22.2 %) of fatalities in WWII.
15. Stéphane Courtois et al. (translated by Jonathan Murphy and Mark Kramer), *The Black Book of Communism*: *Crimes*, *Terror*, *Repression* (Harvard: Harvard University Press, 1999). Terror in Poland was incomparably smaller in scale than that in the Soviet Union, but changed the attitudes of many Poles, including intellectuals. See Piotr Hübner, *Polityka naukowa w Polsce w latach 1944–1953*, *geneza systemu*, 2 Vols. (Wrocław: Zakład Narodowy im. Ossolińskich, 1992).
16. Siemion R. Mikulinski, "Polityka naukowa krajów socjalistycznych," *Zagadnienia Naukoznawstwa. Studia i Materiały* 14/3 (1978): 343–353.
17. Ryszard Sitek, *Warszawska szkoła historii idei. Między historią a teraźniejszością* (Warszawa: Wydawnictwo Naukowe Scholar, 2000), 30–33.
18. The proponents of Lysenkoism in Poland popularized it from 1949 as members of *Koło Przyrodników Marksistów* (Circle of Naturalists Marxists). One of their opponents, Wacław Gajewski (1911–1997), was forbidden from teaching. See Piotr Węgleński, "Profesor Wacław Gajewski," Instytut Genetyki i Biotechnologii UW (available at: http://www.igib.uw.edu.pl/index.php/start2/about/wspomnienie-o-prof-waclawie-gajewskim/, accessed 10/5/2015).
19. Beata Bińko, "Instytut Kształcenia Kadr Naukowych przy KC PZPR—narzędzie ofensywy ideologicznej w nauce i szkolnictwie wyższym," *Kultura i Społeczeństwo* 40/2 (1996): 199–214. Schaff was the director of the Philosophy Institute at Warsaw University.
20. Sitek, *Warszawska szkoła historii idei. Między historią a teraźniejszością*, 35–45.
21. Józef Chałasiński "Drogi i bezdroża socjalizmu w nauce polskiej (1949–1954)," *Kultura i Społeczeństwo* 1/1 (1957): 7–43; reprinted in *Kultura i Społeczeństwo* 40/3 (1996): 19–44.
22. Barbara Fijałkowska, *Polityka i twórcy* (*1948–1959*) (Warszawa: Państwowe Wydawnictwo Naukowe, 1985), 150–157.
23. Piotr Hübner, "Na froncie nauki," *Forum Akademickie* 3/2004 (available at: http://forumakad.pl/archiwum/2004/12/26-kartki_z_dziejow_nauki_w_polsce.htm, accessed 10/5/2015).
24. Sitek, *Warszawska szkoła historii idei. Między historią a teraźniejszością*, 30.

25. Piotr Hübner, *Polityka naukowa w Polsce w latach 1944–1953*, *geneza systemu*, vol. 1 (Wrocław: Zakład Narodowy im. Ossolińskich, 1992), 382–384.
26. L. Fleck, "Problems of the Science of Science," in *Cognition and Fact. Materials on Ludwik Fleck* (Dordrecht: Reidel, 1986), 113–128, on 127.
27. According to Fleck all philosophical doctrines, such as idealism, positivism (including that developed by the Lvov–Warsaw School of Philosophy), and materialism are socially and historically constructed and have no absolute meaning and validity.
28. Hübner, Piskurewic, and Zasztowt, "Józef Mianowski Fund. A Foundation for the Promotion of Science. History".
29. Hübner, *Polityka naukowa w Polsce w latach 1944–1953*, *geneza systemu*, vol. 1, 388.
30. Zasztowt, *Kasa Mianowskiego 1881–2011*, 139.
31. Barbara Fijałkowska, *Polityka i twórcy* (*1948–1959*), 332–352; Sitek, *Warszawska szkoła historii idei. Między historią a teraźniejszością*, 107 fn.2; Ryszard Herczyński, *Spętana nauka*: *opozycja intelektualna w Polsce 1945–1970* (Warszawa: Wydawnictwo Naukowe Semper, 2008), 155–254.
32. However, in 1953–1954, he was still a Stalinist. See A. Schaff, *Moje spotkania z nauką polską* (Warszawa: Polska Oficyna Wydawnicza "BGW", 1997).
33. Leszek Kuźnicki, "Polska Akademia Nauk w roku 1956," *Nauka* 2 (2006): 137–144.
34. Herczyński, *Spętana nauka*: *opozycja intelektualna w Polsce 1945–1970*, 255–328.
35. Jabłoński cited in Kuźnicki, "Polska Akademia Nauk w roku 1956," 141 [translation—M.K.].
36. Schaff, *Moje spotkania z nauką polską*.
37. Władysław Krajewski, *Polish Essays in the Philosophy of the Natural Sciences* (Dordrecht: D. Reidel, 1982), xiii–xiv.
38. Schaff, *Moje spotkania z nauką polską*.
39. Witold Sygocki, "Instytut Historii Nauki PAN w latach 1952–2003. Kalendarium," in *Instytut Historii Nauki Polskiej Akademii Nauk w latach 1953–2003*, edited by Joanna Schiller and Leszek Zasztowt (Warszawa: Instytut Historii Nauki PAN: 2004), 17–30.
40. Ibidem, 21–22.
41. Bohdan Walentynowicz (in "Profesor Ignacy Malecki. Na 70-lecie urodzin," *Zagadnienia Naukoznawstwa. Studia i Materiały* 14 (1983): 3–6, on 4) claims that this was the first commission of such a kind in socialist countries.
42. Stefan Chaskielewicz, "Information Concerning the Organization of Polish Research in the Field of the Science of Science," *Zagadnienia Naukoznawstwa. Studia i Materiały* 4 (1968): 56–58.
43. Walentynowicz, "Profesor Ignacy Malecki. Na 70-lecie urodzin".

44. Ignacy Malecki, *Problemy koordynacji badań naukowych* (Warszawa: PWN, 1960).
45. Ignacy Malecki, "Z zagadnień metodologicznych nauki," *Nowe Drogi* 18/3 (1964): 11–19. [translation—M.K.].
46. See, for example, Henryk Jabłoński, "Kryteria wyboru kierunków badawczych," *Nowe Drogi* 18/1 (1964): 44–54.
47. Tuszko, a professor of Warsaw University, was dismissed as he did not receive "the authorization of the Minister of Education and Higher Education [Henryk Jabłoński]." Sygocki, "Instytut Historii Nauki PAN w latach 1952–2003. Kalendarium," 21.
48. Tadeusz Kotarbiński, Zdzisław Kowalewski, Eugeniusz Geblewicz, and Wanda Osińska, eds., *Problemy epistemologii pragmatycznej. Materiały z posiedzeń Konwersatorium Naukoznawczego Polskiej Akademii Nauk* (Wrocław: Zakład Narodowy im. Ossolińskich Wydawnictwo Polskiej Akademii Nauk, 1972).
49. Stanisław Kulczyński, "Nauka—polityka—historia. Przemówienie inauguracyjne na X I Międzynarodowym Kongresie Historii Nauki," *Kwartalnik Historii Nauki i Techniki* 10 (1965): 511–517, on 511 [translation—M.K.].
50. John D. Bernal and Alan L. Mackay, "Towards a Science of Science," *Organon* 2/3 (1966): 9–17.
51. By 1967 Derek de Solla Price, *Science Since Babylon* (New Haven, CT: Yale University Press, 1961) and *Little Science*, *Big Science* (New York: Columbia University Press, 1964) had been translated in Polish. So was J. D. Bernal's *Science in History* (New York: Cameron Associates, 1954).
52. On the conferences see, for instance, Ignacy Malecki et al., "Wybrane Materiały Polsko-Radzieckiego Sympozjum Naukoznawczego," *Zagadnienia Naukoznawstwa. Studia i Materiały* 4/ 2 (1968): 3–142. Many works of Soviet scholars were published in the journals *Zagadnienia Naukoznawstwa. Studia i Materiały*, *Kwartalnik Historii Nauki i Techniki*, and *Organon*.
53. Eugeniusz Olszewski, "'Woprosy Istorii Jestiestwoznanija i Tiechniki' w latach 1966–1967," *Kwartalnik Historii Nauki i Techniki* 13/4 (1968): 847–850.
54. See, for example, Józef Chałasiński, "Problem nauki i roli uczonego w społeczeństwie," *Nauka Polska* 1/1 (1953): 164–185.
55. Jerzy Drewnowski, "Narada w sprawie badań nad teoretycznymi zagadnieniami naukoznawstwa," *Kwartalnik Historii Nauki i Techniki* 11/4 (1966): 407–408.
56. Aleksander Tuszko, "Naukoznawstwo jako narzędzie działania. (Artykuł dyskusyjny)," *Zagadnienia Naukoznawstwa. Studia i Materiały* 2/1–2 (1966): 148–156, on 156.
57. Bogdan Suchodolski, "O podstawach polityki naukowej," *Zagadnienia Naukoznawstwa. Studia i Materiały* 4/24 (1970): 3–8.

58. T. S. Kuhn, *Przewrót kopernikański. Astronomia planetarna w dziejach myśli Zachodu* (Warsaw, 1966); T. S. Kuhn, *Struktura rewolucji naukowych* (translated by Helena Ostromęcka), (Warszawa: PWN, 1968).
59. Sitek, *Warszawska szkoła historii idei. Między historią a teraźniejszością*, 68–83.
60. Jerzy Eisler, *Marzec 1968. Geneza, przebieg, konsekwencje* (Warsaw: Państwowe Wydawnictwo Naukowe, 1991).
61. Marek Gumkowski and Maria Ofierska, eds., *Marzec 68* (Warszawa: Otwarta Rzeczpospolita. Stowarzyszenie przeciw Antysemityzmowi i Ksenofobii, 2008).
62. Eisler, *Marzec 1968.*
63. This kind of socialism was at the core of the official program of the Polish Socialist Party. See Bohdan Urbankowski, *Czerwona msza czyli Uśmiech Stalina* (Warszawa: Wydawnictwo Alfa, 1995), 318–320.
64. Piotr Hübner, *Nauka polska po II wojnie światowej—idee i instytucje* (Warszawa: Centralny Ośrodek Metodyczny Studiów Nauk Politycznych, 1987), 165–174.
65. Stefan Chaskielewicz and Aleksander Tuszko, *Polityka naukowa drugiej generacji* (Warszawa: Wiedza Powszechna, 1975), 124–125.
66. Bogusław Leśnodorski, "Historia nauki i techniki wśród nauk o kulturze," *Kwartalnik Historii Nauki i Techniki* 19/3 (1974): 445–461, on 458 [translation—M.K.].
67. Wojciech Gasparski, "O aktualnym stanie badań naukoznawczych w Polsce," *Zagadnienia Naukoznawstwa. Studia i Materiały* 25/3–4 (1989): 381–397, on 383–384 [translation—M.K.].
68. Bogdan Suchodolski, "Historyczne refleksje and istotą nauki współczesnej," *Zagadnienia Naukoznawstwa. Studia i Materiały* 4/20 (1969): 3–18.
69. Wojciech Gasparski, "Przegląd problemów nauk o nauce po latach," *Zagadnienia Naukoznawstwa. Studia i Materiały* 36/4 (2000): 443–459, on 456.
70. Gasparski, "O aktualnym stanie badań naukoznawczych w Polsce," 384.
71. Bogdan Suchodolski, *Nauka a świadomość społeczna* (Wrocław: Zakład Narodowy im. Ossolińskich—Wydawnictwo PAN, 1974), 141.
72. See Sygocki, "Instytut Historii Nauki PAN w latach 1952–2003. Kalendarium."
73. Janina Owczarek, "Pracownicy Instytutu Historii Nauki PAN w latach 1954–2004," in *Instytut Historii Nauki Polskiej Akademii Nauk w latach 1953–2003*, edited by Schiller and Zasztowt, 427–436, on 428. Even if he was a dissident, Amsterdamski was employed in the national academy of sciences.
74. Stefan Amsterdamski, "Nauka współczesna a wartości," *Zagadnienia Naukoznawstwa. Studia i Materiały* 7/1 (1971): 58–73; S. Amsterdamski, "Nauka jako przedmiot humanistycznej refleksji," *Studia Socjologiczne* 2 (1971): 27–54.

75. S. Amsterdamski (translated by P. Michałowski), *Between Experience and Metaphysics. Philosophical Problems of the Evolution of Science* (Dordrecht: D. Reidel, 1983); S. Amsterdamski (translated by Olga Amsterdamska), *Between History and Method*: *Disputes about the Rationality of Science* (Dordrecht: Kluwer, 1992).
76. Karl Popper (preface by S. Amsterdamski), *Nędza historycyzmu*: *z dodaniem fragmentów autobiografii* (Warsaw: Krąg, 1984).
77. T. S. Kuhn (translated and with a poscript by Stefan Amsterdamski), *Dwa bieguny. Tradycja i nowatorstwo w badaniach naukowych* (Warsaw: Państwowy Instytut Wydawniczy, 1985).
78. The election of Karol Wojtyła to the papacy, and his visit to Poland in June 1979 resulted in strengthening of the independent democratic opposition.
79. These institutions took the names of historical institutions that acted in the nineteenth and twentieth centuries. See Ryszard Terlecki, *Uniwersytet Latający i Towarzystwo Kursów Naukowych. 1977–1981* (Kraków—Rzeszów: Instytut Europejskich Studiów Społecznych, 2000).
80. Stefan Amsterdamski, Aldona Jawłowska, and Tadeusz Kowalik, *Język propagandy*, (Warsaw: Niezależna Oficyna Wydawnnicza, 1979).
81. Krzysztof Dubiński, *Okrągły Stół* (Warsaw: Wydawnictwo Krajowa Agencja Promocyjna, 1999).
82. See Walentynowicz, “Profesor Ignacy Malecki. Na 70-lecie urodzin”.
83. Ignacy Malecki, “Dyscyplinowe i problemowe podejście do naukoznawstwa oraz jego przyszłego rozwoju,” *Prakseologia* 101 (1987–1988): 333–343, on 334 [translation—M.K.].
84. Owczarek, “Pracownicy Instytutu Historii Nauki PAN w latach 1954–2004,” 428.
85. Bogdan Suchodolski et al. (eds.), *Historia nauki polskiej*, 9 vols. (Wrocław: Zakład Narodowy im. Ossolińskich, 1970–1992).
86. This unit brought together the Department of Praxiology (which, after the dissolution, in 1976, of the Institute of Science Policy, Technical Progress and Higher Education, was united with the Institute of Philosophy and Sociology, PAS) and the Centre for Research on the Science of Science. Piotr Hübner, “Krótka historia Zakładu Prakseologii i Naukoznawstwa,” *Prakseologia* 104 (1989): 145–169, on 164.
87. Gasparski, “O aktualnym stanie badań naukoznawczych w Polsce,” 385–386.
88. See Sygocki, “Instytut Historii Nauki PAN w latach 1952–2003. Kalendarium”.
89. Ignacy Malecki, “Prakseologia a naukoznawtswo,” *Prakseologia* 1–2 (1989): 17–19, on 17; I. Malecki, “Ewolucja koncepcji naukoznawstwa w ostatnim półwieczu,” *Zagadnienia Naukoznawstwa. Studia i Materiały* 36/4 (2000): 437–442, on 437.

90. Piotr Hübner, Jan Piskurewicz, and Leszek Zasztowt, *Kasa im. Józefa Mianowskiego. Fundacja Popierania Nauki 1881–1991*, Warsaw: Nakł. Kasy im. J. Mianowskiego, 1992, 50.
91. Ibidem.
92. Aleksandra Derra, "Sprawa Hedfors, czyli o tym, co Ludwik Fleck robił w Buchenwaldzie. Niechętnie przedstawiana historia pewnego oskarżenia," *Studia Philosophica Wratilaviensia* 6 (2011/2): 175–180.
93. Florian Znaniecki, *Pisma Filozoficzne*, 2 Vols. (Warsaw: Państwowe Wydawnictwo Naukowe, 1991).

CHAPTER 8

Scientists of the World, Unite! Radovan Richta's Theory of Scientific and Technological Revolution

Vítězslav Sommer

In May 1967, journalist Václav Kotek held a conversation with Radovan Richta (1924–1983), philosopher and head of the Czechoslovak interdisciplinary team of scholars, which published the critically acclaimed collective work *Civilization at the Crossroads* (1966). The book addressed the "social and human implications of the scientific and technological revolution." Its publication was considered an exceptional achievement of Czechoslovak social sciences and the first step toward a more detailed elaboration of the current state as well as future prospects of state socialism in the context of global economic and technological transformations. In an interview with Kotek, Richta talked about his long-lasting interest in the development of human abilities in the condition of a "scientific and technological revolution" (STR). Richta enthusiastically declared:

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> Science, which recently becomes the source of civilizational progress, has, as a productive force, different "human parameters." Every step of science depends directly on the growth of human forces. On the other hand, science simultaneously causes and demands this growth. The more civilization relies on science, the greater the significance of human skills, craft, ability, formability—of, simply, universal development of man.[1]

Asked by Kotek about the current state of knowledge on the STR, Richta answered:

> Our achievement is only a scientific hypothesis and we cannot be satisfied with it. For us, it is an initial phase, a starting point of a research program developing within our team […] In the history of society, the STR is probably the first process which society can and has to grasp from its very beginning.[2]

The short interview shows how Richta conceptualized the STR and its relationship to science at the moment when his scholarship rose to prominence and simultaneously became an important source of the Czechoslovak reform-communist program. Even in marginal texts related to the STR, it was possible to recognize the centrality of science for Richta's thinking about civilizational changes and the future of the communist political project. Fundamental redefinition of the "scientific" was closely connected with the attempts to change the status of science and scientists in the socialist societies. This theoretical reevaluation of science's social functions was based on the belief in the necessity of scientific administration of state socialism. Crucial was the idea that scientists and experts are not only academic supporters of socialist policy but, above all, the most important actors of the general progress toward communism. The most striking aspect was the vision of science as a "direct productive force" and a decisive field of human activity. Science thus occupied a prominent position in Richta's STR conception. According to Richta's explanation in the interview with Kotek, realization of the STR was still the enterprise of the future. And it was science which had to play the dominant role in the remaking of the world in the epoch of groundbreaking technological changes.[3]

The aim of this chapter is to analyze conceptualization of science and "scientific" in Richta's STR research. It seeks to explore how the discourse of scientific and technological progress influenced Richta's examination of possible paths toward a communist future and how his scholarship was situated in the context of Czechoslovak social sciences from the 1950s to 1980s. Czechoslovak STR was a specific kind of scholarship, which analyzed science and technology in social and political context. Rather than involved in research of, for example, epistemological questions, STR highlighted topics related to interaction between science and society. Science and technology were discussed by STR scholars primarily as social phenomena. The STR thus represents influential Marxist attempt to discuss science and technology as a part of research of politics and society.

The following text is divided into three sections. The initial part examines the broader intellectual background of Richta's STR project. It explores the Stalinist theory of knowledge, which affected scholarly praxis and theoretical thinking about science in the 1950s. This body of knowledge was a highly problematic starting point for social sciences in the early period of the socialist dictatorship. Furthermore, later critiques of the Stalinist concept of the "scientific" played a crucial role in the process of establishing reform-oriented research programs and expert teams including Richta's STR project. The second part of this chapter deals with Richta's conceptualization of the STR in the 1960s, primarily with his attempt to formulate a humanistic conception of labor within the Marxist–Leninist theory of social change and economic development. The conceptualization of science in *Civilization at the Crossroads* and related texts written by Richta in his reform-communist period is analyzed in this section. The aim of the chapter's final part is to explore transformations of Richta's examination of science in his late writings. His texts from the 1970s and early 1980s were influenced by the changed situation in Czechoslovak academia after the collapse of the Prague Spring and subsequent introduction of the more authoritarian late socialist governance.

In the Name of "Active Superstructure": Stalinist Theory of Knowledge and Social Sciences

Richta's conception of the STR is usually connected with the effort of reform-communist intellectuals to democratize and modernize the state socialist system in the face of its deep crisis, which fully emerged in the first half of the 1960s. *Civilization at the Crossroads* and Karel Kosík's *Dialectics of Concrete* (1963) were the results of a serious reevaluation of Marxist–Leninist dogmatics under the banner of de-Stalinization and a reform of Czechoslovak social sciences. Richta's first attempts to conceptualize the STR and the role of science and technology in socialist society were based on the critique of the Stalinist concept of the relationship between economy and labor. Other reform-communist social scientific research projects were also based on a more or less open assault on Stalinist scholarship.[4] The rejection of Stalinist thought was thus a starting point for deliberations about different concepts of production of social scientific knowledge.

On the other hand, it is necessary to discuss Stalinist origins of later reform attempts. The purges of the Czechoslovak scholarly community in the late 1940s and the introduction of Marxism–Leninism enabled an expansion of new social scientific cultures deeply rooted in the structures of the communist movement and Communist Party of Czechoslovakia (CPC) apparatus. Universities were transformed from centers of research into solely educational institutions and the Czechoslovak Academy of Sciences (CSAS) was established as the most important research institution.[5] The institutional basis of the social sciences was dramatically changed. Besides the CSAS, new institutions were founded in the Party apparatus complex (Party School of Higher Learning, Institute for the History of CPC). The institutional, personal, and conceptual map of Czechoslovak social sciences was completely rewritten.[6] In the early 1950s, the majority of future reform-communist scholars were active and prominent young supporters of this process. They used the Stalinist conceptualization of Marxism–Leninism as the most important theoretical tool for describing social reality. It was no coincidence that their later grappling with the legacy of Stalinism was not only critical of Stalinist scholarship, but also contained certain traces of continuity with the Stalinist way of thinking about society and its relationship to science.

The Stalinist approach to science emphasized the necessity of its new theoretical and methodological apparatus. Of equal importance was the establishment of new rules of scholarly community and definition of distinctive social and political functions of socialist scholarship. The Stalinist theory of knowledge was elaborated primarily in Stalin's two important essays, "Dialectical and Historical Materialism" (1938) and "Marxism and Problems of Linguistics" (1950). These two texts highlighted the political significance of social scientific scholarship. Social sciences were portrayed as an important actor in everyday revolutionary struggles and subsequent socialist construction. Scholars were primarily political actors who were actively involved in the creation of a new socialist society. The concept of partisanship in science was not Stalin's groundbreaking discovery. It was elaborated in Lenin's texts about the relationship between writers and the revolutionary party, written at the very beginning of the twentieth century.[7] However, the Stalinist theory of knowledge introduced a new type of partisanship and a more rigorous vision of the relationship between science and political activity. The publication of Stalin's infamous "Letter to the Editorial Board of the Magazine 'Proletarskaya Revolyutsia'" in 1931 resulted not only in the purge of Soviet historiography.[8] Stalin's rude intervention into the fierce historiographical debate contained a harsh critique of the alleged attempt of the journal's editors to "draw people into a discussion on questions which are axioms of Bolshevism."[9] There were certain topics, according to Stalin, which had a status of "axioms," and thus it was forbidden to discuss them. The strict rules of scholarly discussion introduced by Stalin's "Letter" served as a starting point for a broader examination of the principles of production of knowledge in the Stalinist dictatorship.

The theoretical definition of the relationship between social sciences and the policy of the communist party in Stalin's "Dialectical and Historical Materialism" justified the direct subordination of the social sciences to the political aims of the revolutionary party. Politically involved Marxist–Leninist scholarship was portrayed as a science based on the faultless scientific theory, which was capable of discovering the indisputable laws of social development. The main aim of the social sciences was to formulate indisputable truths about social reality and provide intellectual support for the revolutionary attempts of the communist movement. It implied that the revolutionary efforts of the Stalinist state were based on unmistakable conclusions of scientific research. The relationship between the social sciences and party policy was characterized by an example from historiography:

> If the connection between the phenomena of nature and their interdependence are laws of the development of nature, it follows, too, that the connection and interdependence of the phenomena of social life are laws of the development of society, and not something accidental. … It follows that social life, the development of society, is also knowable, and that the data of science regarding the laws of development of society are authentic data having the validity of objective truths. Hence, the science of the history of society, despite all the complexity of the phenomena of social life, can become as precise a science as, let us say, biology, and capable of making use of the laws of development of society for practical purposes.[10]

Further elaboration of the Stalinist theory of knowledge was provided by Stalin's other well-known intervention in Soviet scholarly debates. His writings on linguistics, which were published in 1950, introduced the influential concept of "active superstructure." This authoritative revision of Marx's theory, originally elaborated in the preface to *A Contribution to the Critique of Political Economy* (1858–1859), redefined the role of superstructure in the process of socialist construction. Stalin left no doubt about the primarily political tasks of science. The superstructure was not exclusively dependent on changes in the economic base. Marx's statement that "it is not the consciousness of men that determines their existence, but their social existence that determines their consciousness" was not interpreted too rigorously by Stalin.[11] The Stalinist superstructure was a policy actor, which had to take part in the contemporary social and economic transformations of the Soviet state and society. Science was not solely an academic enterprise isolated in the ivory tower of universities and research institutions. Scientists had to actively support policy and from the position of superstructure intervene in the changes of the base. In *Marxism and Problems of Linguistics* Stalin argued:

> The superstructure is a product of the base; but this does not mean that it merely reflects the base, that it is passive, neutral, indifferent to the fate of its base, to the fate of the classes, to the character of the system. On the contrary, no sooner does it arise than it becomes an exceedingly active force, actively assisting its base to take shape and consolidate itself, and doing everything it can to help the new system finish off and eliminate the old base and the old classes. […] The base creates the superstructure precisely in order that it may serve it, that it may actively help it to take shape and consolidate itself, that it may actively strive for the elimination of the old, moribund base and its old superstructure.[12]

Stalin's reinterpretation of the base–superstructure relationship implied that science was a specific part of revolutionary forces. First of all, production of knowledge was a policy instrument. Stalinism introduced an extremely utilitarian definition of the relationship between science and politics. Furthermore, it was not only a theoretical exercise. The Stalinist concept of scientific knowledge determined the everyday praxis of the social sciences. In the summer of 1950, a leading radical Czechoslovak Stalinist theoretician and propagandist Gustav Bareš used the concept of "active superstructure" to describe science as an important initiator of socialist construction. According to Bareš, Stalin's texts about linguistics were a serious appeal to all scholars. Stalin's aim was to encourage them to participate in the building of the new order. Without doubt, only socialism was able to provide conditions for "humanistic and truthful scholarship."[13] Everyday praxis of social scientists was seriously affected by the practical application of this theory. Scholars were directly involved in the activities organized by the Communist Party propaganda apparatus. Their most important task was to contribute to propaganda campaigns and serve as the intellectual avant-garde of the socialist revolution.

The youngest generation of social scientists, including Radovan Richta, launched their careers in the early 1950s as enthusiastic Stalinists. For them, the theory of "active superstructure" and partisanship of science was a set of theoretical and methodological rules as well as a general framework of their everyday existence in the specific institutional networks, which connected academia with the Communist Party apparatus. The Stalinist concept of politically engaged scholarship deeply influenced the social sciences in state socialist Czechoslovakia. The strong belief that scholars had to support communist party policy did not disappear with the decline of Stalinism in the second half of the 1950s. Post-Stalinist rethinking of the "scientific" was performed under the banner of "scientification" and the overcoming of "dogmatism" and the "cult of personality." Social sciences had to abandon simple propagandist activities in favor of a more sophisticated scholarly analysis closely connected with the needs of Communist Party policy. "Scientification" was by no means an ideological regress to "bourgeois positivism" or an attempt to implement "impartial" and "objective" scholarship. The aim was to establish a close connection between Marxist–Leninist partisanship and scholarly analysis, between party decision-making and scientific research. According to the post-Stalinist conception of science, social scientists were experts whose aim was to create a scholarly background for Communist Party policy. "Scientification" meant a serious Marxist–Leninist analysis of the contemporary world. The policy of the Communist Party had to be based on scientific expertise, which was considered a crucial condition for the successful administration of future phases of socialist construction. However, cooperation with propaganda was still an important task of the social sciences, which had to serve as an expert advisor for the communist party's propaganda activities.

In 1957, a leading Marxist–Leninist theoretician Pavel Reiman characterized the relationship between science and propaganda as an interaction between two closely interconnected parts of one scientific–political complex: "Typical for Marxist communists is that they do not consider science to be an enterprise of a closed circle of chosen ones providing research in their ivory towers at times of great struggles for the new society. Real communist scientists draw lessons from praxis … and pass the results of their research … to the masses and nations, which they serve. Building barriers between propaganda and science has nothing to do with Marxism-Leninism."[14]

The Stalinist conception of the social sciences as an immediate political actor was replaced by emphasizing their expert role. The post-Stalinist conception of the "scientific" was reflected in everyday research praxis. Scholars became oriented toward activities closely connected with the legitimization and consolidation of socialist dictatorship after the revolutionary turmoils of the early 1950s. Significant were attempts to invent social scientific agendas dealing with the various aspects of the state socialist system. As early as 1955, the discussion about the character of the Czechoslovak socialist revolution began. Political and legal scientists discussed the historical roots of Czechoslovak socialist statehood and attempted to explain discrepancies between the Marxist–Leninist theory of revolution and the specifics of Czechoslovak development.[15] Later they tried to theoretically define "people's democracy" and a socialist state.[16] Attempts to summarize the theoretical foundations of state socialism resulted in the establishment of *scientific communism*, a new discipline researching topics like the theory of the revolutionary party, the national question, the agrarian question, or "class relations." The institutional basis of *scientific communism* was the Charles University Institute of Marxism-Leninism for Universities, which published a series of textbooks for universities and higher learning institutes.[17] In the course of the late 1950s and early 1960s, *scientific communism* scholars from the Charles University Institute focused on the problems of socialist society and participated in the rebirth of sociology in Czechoslovakia.[18] Historians attempted to formulate a grand narrative of state socialism and find its historical legitimization.[19] In 1961, a synthesis of Communist Party history was published, which served as an authoritative account of historical development toward socialism.[20]

The social sciences addressed several aspects of Communist Party governance. In 1960, a new Czechoslovak constitution was adopted, which officially claimed that socialist construction in Czechoslovakia had been successfully completed. Nevertheless, this official proclamation could not fundamentally affect Czechoslovak social and economic reality. In the following years, new acute and highly politicized research topics emerged.

The need for the cultivation of state socialism was combined with the evident necessity to deal with an obvious economic and political crisis. It caused a thematic shift toward a reform agenda and a further emphasis of the importance of scholarly expertise for the functioning of the state socialist system. In the early 1960s, not only early post-Stalinist descriptions of the socialist state and its functions, but also plans of its further development and related reform proposals were important. Precisely at that moment, Richta's early texts on the STR were published. The future of socialism seemed to be fatally dependent on the capacity of science to resolve crucial questions of the contemporary world. Radovan Richta entered the discussions about the future of socialism with a theory, which was based on an analysis of science as a decisive actor of historical progress toward communism.

Direct Productive Force and Post-Industrial Labor

The Czechoslovak reform-communist movement was closely interconnected with the expert culture, which was established as a result of the partial de-Stalinization of the social sciences after 1956. Expertise in the field of economy, political sciences, historiography, or sociology was seen as an important part of the reformists' efforts. The Czechoslovak reform started in the early 1960s and reached its peak in the 1968 Prague Spring. This period became the golden age of social scientific expertise.[21] The main aim of scholars was to define the developmental strategies of socialism in accordance with global political, social, and economic changes. Future-oriented scholarship was important as it examined the possible paths of socialist development. The reformist group in the communist establishment and prominent scholars in academia and Communist Party institutes shared the conviction that it was necessary to formulate socialist policy on the solid ground of scientific research.

This emphasis on scholarly expertise was a response to the problems of state socialism affected by the economic and political crisis. Nevertheless, it also reflected the reformists' belief in the possibility of a new socialist policy based on a rational and wide-ranging scholarly analysis of the contemporary world. Scholars were also attracted by the vision of scientists as agenda setters and intellectual leaders of the modern socialist society. According to František Šorm, biologist and president of the CSAS from 1962 to 1969, science under advanced socialism had to be based on careful planning of research and a much broader involvement of science and scientists in policymaking. In 1967, Šorm wrote, "In advanced communist society real scientification of the whole of social life will take place."[22] Expert culture in the 1960s was structured around research teams dealing with the most important topics of the period. Scholars were mobilized by the reform-communist vision of the possibility to invent a new socialist policy. Social scientific expertise of the reform-communist period was ambitious, self-confident, and well-organized—and was seen by contemporaries as the most advanced stage of engaged socialist scholarship. Social scientists entered the political arena as important actors actively involved in academia as well as in the backstage of Communist Party policymaking.

During the first half of the 1960s, several expert teams were established. These groups were usually managed by important scholars and were closely connected with the reform personalities in the communist party leadership. A short overview of these scholarly teams documents the extent of their expert activities. The team of economists led by Ota Šik, who was head of the CSAS Institute of Economics and a leading personality of the reform-communist faction in the Communist Party, became a laboratory of economic reform based on the idea of market socialism.[23] In 1966, an interdisciplinary team at the CSAS Institute of State and Law was established, which provided research on the development of the political system and democracy in socialist society. The team was headed by Zdeněk Mlynář, who was a member of the Communist Party law commission and an influential actor in the negotiations and struggles in the party apparatus.[24] The main task of an expert team led by sociologist Pavel Machonin was sociological research of contemporary Czechoslovak society. Sociologists from the CSAS Institute of Sociology and the Charles University Institute of Marxism-Leninism for Universities analyzed the "structure of Czechoslovak society."[25] Social scientific expertise was active also in the Czechoslovak Army. Scholars at the Klement Gottwald Military Political Academy dealt with the elaboration of Czechoslovak military doctrines. Their important material from 1968 was entitled *Memorandum: To Formulate and Constitute Czechoslovak Interests in the Military Field* and discussed the necessity of formulating an independent Czechoslovak foreign policy and military strategy with regard to the specific position of Czechoslovakia in a divided world.[26] During the first half of the 1960s, teams of historians usually called "committees" were established. The most important one was the Czechoslovak Committee for the History of Antifascist Resistance. The task of the "resistance committee" was to analyze the "Czechoslovak revolution" as the initial phase of a specific "Czechoslovak road to socialism."[27] Top-secret scholarly expertise was provided in the "rehabilitation commissions," which worked in the communist party apparatus and were composed of historians, political scientists, lawyers, and Communist Party *apparatchiks*. The commissions were charged with analyzing the Stalinist show trials of the early 1950s and collecting materials usable for the legal rehabilitation of victims.[28]

Radovan Richta's project on STR research was a part of this "expertisation" movement. Richta, born in 1924, was a communist activist already during the Second World War when he joined the communist resistance group *Předvoj* (Avantgarde). He survived imprisonment by Nazis and became member of the Communist Party in 1945. Richta studied philosophy and natural sciences at the Charles University. At the same time, he was active in a radical Stalinist wing of the CPC and served as a chief-editor of cultural and political journal *Tvorba* from 1948 to 1953. His early philosophical writing was concerned with the critique of Czech "bourgeois philosophy." Initially in the late 1950s and early 1960s, research on the interaction between man and technology in socialism was Richta's personal enterprise. In 1963, he published two influential works which laid the foundations of Czechoslovak STR scholarship: *Man and Technology in the Revolution of Our Days* and *Communism and Changes of Human Life* (*On the Nature of Humanism in Our Time*).[29] His later examination of the STR was based on the philosophical framework which was elaborated in these two early texts.

Richta's theory of science was formulated especially in *Man and Technology in the Revolution of Our Days*. It was based not only on J.D. Bernal's concept of science as a "direct productive force" but, above all, on Marx's examination of the future shift from manual labor to new kinds of production and changes in the relationship between man and labor, which was elaborated in *Grundrisse der Kritik der politischen Ökonomie* (1857–1859). Richta predicted a radical change in the structure of productive forces. Traditional manual labor or "abstract labor" was dominant in modern capitalism and early state socialism and, more generally, was typical of the industrial mode of production. According to Richta, the STR meant the substitution of industrial factory labor by fully automated production. This truly epochal and revolutionary change required a new structure of productive forces based not on manual skills or physical performance, but also on advanced human abilities, above all, the capacity of scientific work. Richta wrote: "Science enters production not only as a direct productive force, but also as a decisive actor. On the other hand, direct production seems to be the rather conscious application of natural sciences, its technological use."[30] Important for this process was the emergence of a new type of man, who was not an element of labor force but rather a subject of productive forces and a conscious ruler of the automated process of production.[31] The gap between labor and science, which was typical for the industrial epoch, could be overcome only by the universal development of human abilities. Science, research, and management were characterized as a prevailing form of labor in the technologically advanced future.[32] The anticipated result of this integration of labor and science, which was rooted in scientific and technological progress, was the overcoming of alienation and the fusion of labor with the permanent (self-)cultivation of man.[33] Science and technology were thus sources of a new humanity, which was based on creative work and the ability of man to fully participate in the process of the STR.

The revolution in productive forces, enabled by scientific progress, was crucial for the future of the communist political project. Richta criticized politically naïve visions, which characterized socialist revolution solely within traditional political activities of the communist movement. The STR changed the very nature of the socialist revolution: "The last battle of communism is taking place right before our eyes. Its stage is production and its weapons are science and technology."[34] The worldwide victory of communism was dependent on the changes in productive forces. Richta mentioned the "technological appeal of the communist revolution" as the most important phenomenon of the present historical period. His concept of STR thus offered Marxist version of the post-industrial or post-Fordist theory, which reflected the post-Stalinist economic doctrine of a more sophisticated relationship between industrialization and the quality of everyday life in socialism.[35]

Emphasis on the importance of science was followed by a critique of technophobia and Stalinist "theoretical blindness" toward intellectual labor. Richta characterized science as the new center of communist revolutionary efforts: "It is not possible to win the 'last battle' with capital without a scientific and technological revolution."[36] In the conclusion of *Man and Technology in the Revolution of Our Days*, the heroism of former revolutionaries was compared with the everyday efforts of scientists and engineers. Battles in production, education, and science were the "decisive human content of our epoch." Richta closed his book with a pathetic and imploring call: "Do we still behave as revolutionaries?"[37]

In the epoch of the STR, a new form of revolution emerged—revolutionaries were not insurgent workers or communist militants fighting in violent uprisings. Revolutionary subjects were scientists, engineers, and other experts involved in the revolutionary transformations of modernity. The humanistic aspects of the STR were further elaborated in *Communism and Changes of Human Life*. In this text, Richta also explored changes in the structure of labor and their consequences for the (self-)cultivation of man. The importance of scientific and technological knowledge for "creative labor" was emphasized, which was highlighted as an "axis of communist life."[38] Richta characterized the STR as the revolutionary epoch which constituted the framework of future "real human community." Science was a crucial actor in this civilizational process, which was, in Richta's words, characterized by the "social development of man."[39]

In 1965, Richta became head of an interdisciplinary team for the research of STR at the Academy of Sciences' Institute of Philosophy.[40] Philosophers, economists, sociologists, psychologists, and scholars from other fields sought to analyze contemporary technological and scientific changes and define strategies for the future development of mankind. The most important text of Richta's interdisciplinary research group was the work *Civilization at the Crossroads* (1966), which sought to explore these changes in the context of the fundamental economic and social transformations wrought by new technologies and the changed structure of productive forces.[41] *Civilization* was based on Richta's theoretical evaluations of the STR and on several empirical analyses produced by his numerous collaborators.[42] In April 1966, the first version of *Civilization* was discussed and authorized by the Presidium of the Communist Party.[43] The book was published in the same year. It became a social-scientific bestseller and was translated into several languages.[44]

The theoretical framework of *Civilization* was based on Richta's earlier works. Philosophical considerations about civilizational changes were supplemented by empirical examinations of the social, economic, and political conditions of the STR. Science occupied a prominent position in the book's narrative. The concept of science as a "direct productive force" mentioned in Richta's early works was connected with certain practical conclusions. First of all, the changed position of science in the social structure of modern societies was observed. It was no longer a product of civilizational development but its producer. The STR required the direct involvement of science in the process of "creating social relationships and human life." Richta predicted the existence of a "scientific civilization" and the functioning of science as a "specific form of modern human existence."[45] Unlimited possibilities of knowledge in terms of its growth and the importance of science for the rapid modernization of almost all aspects of human existence were emphasized. *Civilization* called for the further construction of scientific infrastructure integrated in a transnational circulation of knowledge. Richta drew an optimistic picture of Czechoslovakia transformed into the "arsenal of scientific and technological progress" and "one huge laboratory."[46]

The domination of science in the structure of productive forces was crucial for the future transformation of state socialist governance. Richta predicted fundamental changes in the political system. Stalinist dictatorship and post-Stalinist bureaucratic centralism were strictly hierarchical forms of governance which were typical for industrial production. "Scientific civilization" required decentralization and broader space for the participation of society in decision-making. Specifics of scientific work such as self-fulfillment by labor or individual responsibility and initiative were described as the basis for a future functioning of society composed of self-confident and participating subjects. "Broadening socialist participation in the development of civilization" was emphasized, and thinking about the future was characterized as a social process based on wide discussion and dialogue resulting in a "long-term collective creation of perspectives." Richta called for the democratization of planning to prevent the "harmful accumulation of responsibility for the direction of social development."[47] New forms of socialist governance were characterized as a policy based on scientific expertise. Broader participation of science in decision-making processes and the simultaneous construction of a huge scientific institutional apparatus were required. The aim was to replace political voluntarism and arbitrariness by a policy based on an exact scientific analysis of reality.[48]

Civilization contained two potentially contested concepts of the social role of science—democratic science based on public participation, and technocratic expertise producing knowledge usable by political authorities in the process of scientific administration of society. The future was portrayed as a hyper-technological world governed and managed by scientific rationality. The danger of technocratic governance by experts and managers was immanent in the vision of a completely predicted and controlled path toward a communist future. Tensions between technocratic and emancipatory elements were more than evident in Richta's crucial work. *Civilization* tried to find a convenient compromise between state socialist high modernism and Marxist humanism. This attempt required an optimistic vision of totally self-fulfilled individuals freed from all forms of alienation as well as an image of a well-organized society coordinated on the basis of exact plans and models elaborated by a huge apparatus of experts. The inner contradictions of Richta's concept were well recognized by his contemporaries. Philosopher Karel Kosík criticized the technocratic elements of the STR, which were interpreted as an obstacle to real emancipation.[49] Sociologists in Machonin's team described the STR as an unrealistic vision. According to them, Richta ignored the real structure of Czechoslovak society, which was far from his wishful thinking about "scientific civilization."[50] Regardless of these critical voices, STR gained influence in the late 1960s as a general theoretical framework of the state socialist modernization efforts and Richta's project was officially supported by the communist party authorities from the mid-1960s.[51] In 1968, Richta co-authored the most important policy document of the Prague Spring, the so-called "Action Program of the CPC."[52] It seems that STR was recognized as an official theory of future development toward communism by reform-minded party policymakers as well as by more moderate and ideologically more cautious party functionaries. As will be shown in the next section of this text, Richta's STR contained both emancipatory and humanist concept of "scientific civilization" and technocratic vision of technologically advanced state socialism.

Contradictions contained in Richta's STR were highlighted also by "Western" observers. Although David Rodnick's 1973 review described *Civilization* as "an extraordinarily brilliant analysis of the variety of social changes which man-kind is and will be facing in the coming decades" and a "pioneering research team study which needs to be repeated over and over again," there was still space for highlighting the problematic aspects of Richta's scholarship.[53] Daniel Bell in his *The Coming of Post-Industrial Society* provided an original interpretation of the STR concept as presented in *Civilization*. Bell ignored Richta's humanist claims about the importance of men's self-fulfillment and broader social participation in civilizational changes. On the contrary, Bell emphasized an analysis of scientific governance. According to him, "the stratification of the new society inevitably will emphasize the dominance of the professional and technical classes," so the following question emerged: "If the production and maintenance of the scientific mastery of the future society requires the presence of highly trained research elites, supported by large technical staff, does not all this define the attributes of a new potential ruling class?"[54] Bell predicted a decline of political importance of the working class and a rise of "cleavages between professional elite and the mass."[55] The result was a "post-socialist society": "The age-old socialist dream of a harmonious new society, thus, is doomed to frustration. Instead, the new society itself will generate new conflicts and new struggles not necessarily along the old lines of class and power, but of attitudes to change and to science itself."[56] In this concrete case, the STR was used as a theory from the other side of the "Iron Curtain," which supported Bell's own concept of a post-industrial society. Nevertheless, Bell precisely exposed the technocratic elements which were presented in *Civilization* and which were close to the reality of post-1968 socialist governance in Czechoslovakia. Bell's analysis highlighted topics, which played an important role in Richta's subsequent writings on science in the 1970s and early 1980s.

Richta's Late Works: Science in Late Socialism

Czechoslovak social sciences were profoundly influenced by the collapse of the reform-communist political project after 1968. The demise of reform-oriented policies was followed by personnel purges in academia, first and foremost in the social sciences. Several institutions as well as expert teams were dissolved and numerous scholars lost their jobs. In order to purge Prague Spring "revisionism," a much more restricted institutional and epistemological regime was established.[57] Social scientific theoretical and methodological frameworks were reformulated in order to overcome the infiltration of Czechoslovak scholarship by "bourgeois theories."[58] Richta continued to hold a preeminent position among leading social scientific theoreticians after 1968. But during the post-Prague Spring period, he pragmatically left the reform-communist agenda and took part in the "consolidation" of the social sciences. After the forced dissolution of two crucial social scientific institutions, the Academy of Sciences' Institute of Philosophy and Institute of Sociology, Richta became a director of the newly established Institute for Philosophy and Sociology. His research team was the only scholarly group established in the 1960s, which remained active after the "consolidation" purges.[59]

The changed political environment required a reformulation of Richta's STR conception. This revision of Czechoslovak STR scholarship was provided in close cooperation with Soviet colleagues. In October 1970, the Third Czechoslovak-Soviet Symposium of Philosophy took place. Czech and Soviet scholars discussed the STR and decided to collaborate on the "Marxist analysis of the STR" and prepare a collective volume about the relationship between man and technology.[60] The result was the book *Man-Science-Technology*, which was published in 1973 and served as a representative outcome of the collective efforts of Soviet and Czech STR experts.[61] This collective work documented substantial shifts in STR theory. Czech scholars authored four chapters dealing with the STR and political systems, the STR and changes in the social structure, the STR and organization and management, and the relationship of the STR in creating the "ways of life." First of all, *Man-Science-Technology* was a heated and ideologically rigorous polemic with "bourgeois theories" of society and the future (Daniel Bell, Raymond Aron, Herbert Marcuse, and others). A critical analysis of state socialism, which was important in *Civilization*, was completely missing in this book. It was an outcome of active self-disciplination of Richta's project and pragmatical revision of his theory. In short, *Man-Science-Technology* was official ideological self-critique of Czechoslovak STR scholarship. This politics of self-denouncing enabled that Richta occupied prominent position in academia also after the collapse of reform-communism and his research team was not entirely dissolved. On the other hand, it required Richta's conscious involvement in the post-1968 purges of "revisionists" in academia and in the forced institutional changes like the dissolving of institutions. Moreover, Richta was willing to seriously simplify his theory according to requirements of the "consolidation" regime. The former call for broader social participation in forecasting and planning was replaced by a formal emphasis on scientific prediction, which was described as an integrated and centralized expert system capable of controlling the future. Collaboration between Soviet and Czech scholars was highlighted and served as legitimation of the continued existence of STR scholarship and proof of the fact that reform-communist "deviations" had been overcome. Also in the following years, construction of a correct and authoritative account of the STR required involvement of Soviet scholars.[62]

The reform-communist STR emphasized science as a crucial agent of social and economic changes. The aim of science was to actively participate in policymaking and create opportunities for broader social participation in the prediction of the future and in civilizational changes caused by scientific and technological progress. In the 1970s, STR theory was conceptualized primarily as a technology of governance, which was applied from above by the Communist Party authorities. The division between scientific expertise and policymaking was striking. Science was not an active actor of development but a policy instrument constructed by experts for the purposes of centralized governance by the party elites. STR theory of the 1970s proposed a top-down application of scientific knowledge. Society was excluded from the process of knowledge production and from the political application of science. Scientists' status also changed. In the earlier accounts of the STR, scientists played the role of the avant-garde of technological progress and related social changes. According to the 1970s STR, however, scientists had to produce knowledge and provide it to the communist authorities. The STR became the official developmental theory of late socialist dictatorship and science had to serve as a tool for the careful cultivation of the state socialism.

What remained from 1960s STR theory was an emphasis on the importance of science for the functioning of modern societies. At the same time, as the STR became more of an empty ideological catchword than a serious theoretical framework of social and economic development, Richta and his collaborators focused increasingly on the theory and sociology of science. Former interdisciplinary research on the "social implications of the STR" was reduced to an examination of science and its defense against critiques of scientific rationality. In the 1970s, Richta's writing addressed primarily the situation of science in the age of growing distrust in scientific and technological progress and in the ability of science and technology to resolve crucial problems of mankind.

In his late texts, Richta wrote about a serious crisis of science resulting from its subordination to financial capital and the military.[63] This critique was aimed exclusively at science in capitalist countries. Problems of modern science were thus discussed only as problems of science in capitalism. According to Richta, science was under the pressure of monopolies, which were interested in science only because of its ability to produce financial profit. In capitalism, science was incorporated into the structures of private business. The second serious threat for the future of science was military research. Richta characterized military research programs in the USA as "the greatest deformation in the development of science in human history."[64] The negative image of science in capitalism served as a legitimation of the socialist concept of scientific research and was in perfect accordance with official Communist Party doctrines. Richta highlighted the "humanization of science" in socialism. Coordinated and planned research was emphasized, as well as the harmonious development of science, which was not to be subordinated to capital but to the interests of the whole of society. The contemporary crisis of science and its lack of credibility was, according to Richta, evidence of the general crisis of capitalism.[65] It was necessary to complete a transformation of the relationship between science and society. Proposed was the complete control of science by society and Richta wrote about the "vital necessity for society to ensure united and purposeful social control of the development of science, constituting a component of the general strategy of social development and derived from such a general strategy."[66]

In his paper presented at a UNESCO conference about the STR, held in September 1976 in Prague, Richta wrote about the incompatibility of scientific and technological progress with the capitalist system.[67] The current situation of Western scientific thought was characterized as a "crisis of perspective," which led to the inability of social sciences to formulate positive visions of the future (most importantly for developing countries) as well as a failure to analyze the fundamental social changes brought by the STR. The crisis of science could be resolved only in socialism, which was able to realize "planned development in the sphere of the human components of productive forces."[68] In socialism, a completely different framework of scientific research existed, which was capable of producing knowledge usable in the process of general development toward communism. Richta characterized the main objectives of modern science thus:

> The key issue of science today, and its most difficult practical task, is the purposeful control of social processes, connected with the building up of a socialist society based on general mutual co-operation and geared towards man's general development … Society at present is not confronted with the task of clipping the wings of science or technology; on the contrary, its vital interest is to expand science and its application into new dimensions, capturing the entire complex of the transformation of nature and society.[69]

Richta remained optimistic about scientific and technological progress and its ability to positively affect the future of modern societies. However, his refusal to countenance pessimistic visions of modern civilization was not only connected with his passionate defense of science and its importance for the coordinated development of a society free of military threats and supremacy of capital. It was no coincidence that serious evaluation of the current state of science in state socialist countries was completely missing in Richta's late work. There is no doubt that the schematic dichotomy of declining science in capitalism and a promising socialist "new type of science" was part of late socialist legitimizing discourse. If the future development of mankind was in the hands of science and technology, only well-organized and systematically planned socialist scholarship, freed from military and commercial pressures, was able to fulfill these promises. Richta's late theory of science thus served not only as an argument against techno-skeptics and critics of scientific rationality. At the same time, it represented a theoretical justification of the late socialist political system and its promise of harmonious development toward a technologically advanced future in the ideological, as well as governmental, framework of late socialism.

Conclusion: Radovan Richta's Theory of Science Between Stalinism and Late Socialism

Radovan Richta was one of the most important Czech social scientists in the postwar era and a scholar whose research gained international attention on both sides of the "Iron Curtain." If there was any element in Richta's thought that was a sign of continuity in his work, it was surely his firm belief in the decisive influence of science on the development of modern societies. The starting point for Richta was the Stalinist theory of knowledge, which influenced a whole generation of young communist intellectuals. The radical concept of science as an "active superstructure" was important for their conviction about the necessity of politically engaged scholarship. The concept of science as an agent of social and political change constituted a continuity between Stalinist, reform-communist and late socialist scholarship. On the other hand, the critique of the attempts to subordinate science to everyday policymaking and the rejection of Stalinist dismissive attitudes toward scientists and intellectuals was a crucial component of the reform-communist effort to formulate a new theory of scientific knowledge.

Refutation of these aspects of Stalinism served as the starting point for Richta's theory of STR, which placed science and technology at the forefront of the process of historical change. His original reception of Marx resulted in the concept of science as a "direct productive force." According to this interpretation, science had the ability not only to produce technological progress but also to substantially transform modern societies. In his 1960s writings and especially in *Civilization at the Crossroads*, Richta predicted a "scientific civilization" enabling full self-fulfillment of man by means of science and technology. A critique of Stalinism was more than evident in his emphasis on broader social participation in the STR. Science was a crucial actor in the revolutionary process of the social emancipation. The future was in the hands of scientists, who were the true revolutionaries in the age of advanced socialism. Moreover, his intellectual project gained a positive attention of communist party authorities. It seemed that reform of the state socialism required massive involvement of scientists, experts, and scientific knowledge in governance of the socialist future.

After 1968, Richta's theory of science was fundamentally influenced by political changes in Czechoslovakia. Introduction of more authoritarian form of governance was accompanied by the massive purge of "revisionists," which had serious consequences especially for social sciences. What was required was a thorough elimination of all remnants of the Prague Spring. STR scholarship was also "consolidated" and subordinated to the needs of the "normalization" regime. The STR project was ruthlessly simplified in close collaboration with Soviet scholars. Richta consciously and pragmatically reduced his concept to a schematic developmental theory and tailored the STR according to the requirements of late socialist governance. All emancipatory elements contained in the 1960s STR theory were carefully removed. Technocratic expertise was favored, not the leading role of scientists in the process of social change. The STR became an official narrative of the future and gradually lost its intellectual significance. Richta thus reduced his research program to an examination of science and its functioning under different conditions of socialism and capitalism. His late writings were polemics against critiques of science and technology and with pessimistic accounts of modernity. His aim was to promote socialist science as a theoretical as well as political model of research, which was capable of overcoming the growing skepticism toward science, technology, and their ability to positively influence the future. However, Richta's complete rejection of capitalist science and obligatory lack of critical evaluation of scholarship in state socialist countries strengthened the legitimizing aspect of his work.

Radovan Richta died in 1983, just before the first critical impulses resulting from Soviet *perestroika* and *glasnost* entered the social scientific arena in Czechoslovakia. The political and economic crisis of the "Eastern Bloc" raised a painful question about the future of the communist political project. This would have most likely raised several issues for Richta as a theoretician of science in socialism. It is exciting to imagine Richta's potential activities in the late 1980s. What would have been his agenda? A return to the emancipatory aspects of his scholarship and rethinking of the STR in order to create a theoretical background for Czechoslovak *perestroika*? Or a cultivation of his research on science in the more liberal environment of the 1980s, which would have resulted in the establishment of Czechoslovak *science studies*? What is sure, in the course of the 1980s, thinking about the future went in different directions. Richta's concept of the STR, as well as his theory of science, was not in the spotlight during the political and social transformations which took place in the last years of the socialist dictatorship.

Notes

1. Václav Kotek, "Logika změn naší doby. Hovoříme s dr. Radovanem Richtou, laureátem státní ceny Klementa Gottwalda," *Kulturní tvorba* 21 (1967): 1. All original text in Czech translated in English by the author.
2. Ibid., 3.
3. For a scholarly examination of STR, see Arnold Buchholz, "Wissenschaftlich-technische Revolution (WTR) und Wettbewerb der Systeme," *Osteuropa* 5 (1972): 321–390; Arnold Buchholz and T. J. Blakeley, "The Role of the Scientific-Technological Revolution in Marxism-Leninism," *Studies in Soviet Thought* 20/2 (1979): 145–164; Julian M. Cooper, "The Scientific and Technical Revolution in Soviet Theory," in *Technology and Communist Culture*: *The Socio-Cultural Impact of Technology under Socialism*, edited by Frederic Fleron (New York: Praeger, 1977), 146–179; Erik P. Hoffmann, "Contemporary Soviet Theories of Scientific, Technological and Social Change," *Social Studies of Science* 9/1 (1979): 101–113; Erik P. Hoffmann, "Soviet Views of 'The Scientific-Technological Revolution,'" *World Politics* 30/4 (1978): 615–644. Stefan Guth, "One Future Only. The Soviet Union in the Age of the Scientific-Technical Revolution," *Journal of Modern European History* 13/3 (2015): 355–376. See also Erik P. Hoffmann and Robin F. Laird, *Technocratic Socialism*: *The Soviet Union in the Advanced Industrial Era* (Durham, NC: Duke University Press, 1985).
4. For de-Stalinization of philosophy, historiography and sociology see Michael Kopeček, *Hledání ztraceného smyslu revoluce*: *zrod a počátky marxistického revizionismu ve střední Evropě 1953–1960* (Prague: Argo, 2009); Vítězslav Sommer, *Angažované dějepisectví*: *stranická historiografie mezi stalinismem a reformním komunismem* (*1950–1970*) (Prague: NLN, 2011); Michael Voříšek, *The Reform Generation*: *1960s Czechoslovak Sociology from a Comparative Perspective* (Prague: Kalich, 2012).
5. See Alena Míšková, "Vývoj mimouniverzitní vědy v Československu a ČSAV po roce 1945," in *Bohemia Docta. K historickým kořenům vědy v českých zemích*, edited by Alena Míšková, Martin Franc, and Antonín Kostlán (Prague: Academia, 2000), 418–493.

6. Markéta Devátá, Doubravka Olšáková, Vítězslav Sommer, and Peter Dinuš, *Vědní koncepce KSČ a její institucionalizace po roce 1948* (Prague: Ústav pro soudobé dějiny AV ČR, 2010). From comparative perspective, see John Connelly, *Captive University: The Sovietization of East German, Czech, and Polish Higher Education, 1945–1956* (Chapel Hill and London: University of North Carolina Press, 2000); Gyórgi Péteri, *Academia and State Socialism: Essays on the Political History of Academic Life in Post-1945 Hungary and Eastern Europe* (Boulder, CO: Social Science Monograph/Columbia University Press, 1998).
7. For Lenin's theory of "partisanship" (*partiinost'*) in social sciences, see John Barber, *Soviet Historians in Crisis, 1928–1932* (London and Basingstoke: Macmillan, 1981), 1–5.
8. Ibid., 126–136. The broader socio-political implications of Stalin's letter are analyzed in Sheila Fitzpatrick, *Everyday Stalinism: Ordinary Life in Extraordinary Times: Soviet Russia in the 1930s* (Oxford: Oxford University Press, 2000), 24–28.
9. Iosif V. Stalin, "Some Questions Concerning the History of Bolshevism: Letter to the Editorial Board of the Magazine 'Proletarskaya Revolutsia,'" in J. V. Stalin Internet Archive, available at: www.marxists.org/reference/archive/stalin/works/1931/x01/x01.htm.
10. Iosif V. Stalin, "Dialectical and Historical Materialism," in *The Essential Stalin: Major Theoretical Writings 1905–52*, edited by Bruce Franklin (London: Anchor Books, 1972), 311–312.
11. Karl Marx, "A Contribution to the Critique of Political Economy," in Karl Marx and Friedrich Engels, *Collected Works* (London: Lawrence & Wishart, 1987), 29: 263.
12. Iosif V. Stalin, "Concerning Marxism in Linguistics," in Franklin, *The Essential Stalin*, 408–409.
13. Gustav Bareš, "J. V. Stalin o jazykovědě," in *Ohlas článku J. V. Stalina "O marxismu v jazykovědě" na našich vysokých školách. Stenografický záznam diskuse o článku J. V. Stalina "O marxismu v jazykovědě" konané 29. června 1950 na Filosofické fakultě Karlovy university v Praze* (Prague: Rovnost, 1951), 24.
14. Pavel Reiman "Za důsledné uplatnění historického materialismu v dějinách Komunistické strany Československa, " *Nová mysl* 3 (1957): 246.
15. *Otázky národní a demokratické revoluce v ČSR. Sborník příspěvků přednesených na konferenci Historického ústavu ČSAV 28–30. IV. 1955 v Liblicích* (Prague: ČSAV, 1955).
16. Ivan Bystřina, *Lidová demokracie* (Prague: Nakladatelství Československé akademie věd, 1957); Jiří Houška and Karel Kára, *Otázky lidové demokracie* (Prague: Státní nakl. politické literatury, 1955); Michal Lakatoš, *Otázky lidové demokracie v Československu* (Prague: Nakladatelství Československé akademie věd, 1957).

17. See the ten volumes series Miloš Kaláb et al., eds., *Vědecký komunismus* (Prague: SPN, 1958–1962).
18. Pavel Machonin, *Cesty k beztřídní společnosti: Třídní vztahy v období výstavby socialismu a komunismu* (Prague: SNPL, 1962). Discussions about sociology and scientific communism are analyzed in Voříšek, *The reform generation*.
19. Adam Hudek, *Najpolitickejšia veda: slovenská historiografia v rokoch 1948–1968* (Bratislava: Historický ústav SAV, 2010); Sommer, *Angažované dějepisectví*.
20. For the so-called "History of CPC textbook" see Sommer, *Angažované dějepisectví*, 201–229 Martina Winkler, "'In unteilbarer Einheit mit unserer Geschichte': Die Geschichte der KSČ als neue Meistererzählung," *Comparativ* 10/2 (2000): 61–80.
21. Vladimir V. Kusin, *The Intellectual Origins of the Prague Spring* (Cambridge: Cambridge University Press, 1971); Gordon H. Skilling, *Czechoslovakia's Interrupted Revolution* (Princeton: Princeton University Press, 1976), 90–133; Karel Kaplan, *Kořeny československé reformy 1968*, 4 Vols. (Brno: Doplněk, 2000–2002).
22. František Šorm, *Věda v socialistické společnosti. Poznámky k otázkám společenské funkce vědy, řízení a organizace vědecké práce* (Praha: Academia, 1967), 13.
23. Ota Šik, *Ökonomie, Interessen, Politik* (Berlin: Dietz, 1966); Ota Šik, *Plan and Market under Socialism* (New York: International Arts and Science Press, 1967); Ota Šik, *Ekonomika a zájmy: Jejich vzájemné vztahy do socialismu* (Praha: Svoboda, 1968). Šik's memoir was published as Ota Šik, *Prager Frühlingserwachen: Erinnerungen* (Herford: Busse Seewald, 1988).
24. Zdeněk Mlynář, *K teorii socialistické demokracie* (Prague: Státní nakladatelství politické literatury, 1961); Zdeněk Mlynář, *Stát a člověk: Úvahy o politickém řízení za socialismu* (Prague: Svobodné slovo, 1964); Milan Čermák, Eduard Kučera, and Zdeněk Mlynář, *Základní otázky teorie státu a soudobé typy státu*, 3 Vols. (Prague: Vysoká škola politická ÚV KSČ, 1968). Mlynář's memoir was published as Zdeněk Mlynář, *Night Frost in Prague: The End of Humane Socialism* (London: C. Hurst, 1980).
25. Pavel Machonin et al., *Sociální struktura socialistické společnosti: Sociologické problémy soudobé čs. společnosti. Sborník* (Prague: Svoboda, 1967); Pavel Machonin et al., *Československá společnost: sociologická analýza sociální stratifikace* (Bratislava: Epocha, 1969).
26. "Memorandum třiceti vědeckých pracovníků Vojenské politické akademie a Vojenské technické akademie s návrhem československé vojenské doktríny, zaslané náčelníkem VPA V. Menclem prvnímu tajemníkovi ÚV KSČ A. Dubčekovi jako příspěvek k tvorbě základních dokumentů pro XIV. sjezd ÚV KSČ (Dokument 34)," in *Vojenské otázky československé reformy*

1967–1970. Vojenská varianta řešení čs. krize (*1967–1968*), Vol. 1, edited by Antonín Benčík, Jaromír Navrátil, and Jan Paulík (Prague and Brno: Doplněk 1996), 137–144.

27. Sommer, *Angažované dějepisectví*, 280–294.
28. For the exile publication of the 1968 rehabilitation commission's final report, see *Das unterdrückte Dossier: Bericht der Komission des ZK der KPTsch über politische Prozesse und "Rehabilitierung" in der Tschechoslowakei 1949–1968*, edited by Jiří Pelikán (Vienna: Europa, 1970).
29. Radovan Richta, *Komunismus a proměny lidského života*: (*k povaze humanismu naší doby*) (Prague: ČSŠPVZ, 1963); Radovan Richta, *Člověk a technika v revoluci našich dnů* (Prague: ČSŠPVZ, 1963).
30. Richta, *Člověk a technika*, 13.
31. Ibidem, 50.
32. Ibidem, 55.
33. Ibidem, 65–70.
34. Ibidem, 28.
35. For the Soviet "New Course" in economic doctrine, see David C. Engerman, "The Romance of Economic Development and New Histories of the Cold War," *Diplomatic History* 28/1 (2004): 23–54. For post-Fordism and post-industrialism, see Ash Amin, ed., *Post-Fordism: A Reader* (Oxford: Wiley-Blackwell, 1994); Bob Jessop, "What Follows Fordism?: On the Periodization of Capitalism and Its Regulation," in *Phases of Capitalist Development: Booms, Crises and Globalizations*, edited by Robert Albritton, Makoto Itoh, Richard Westra, and Alan Zuege (Basingstoke and New York: Palgrave, 2002), 283–300.
36. Ibidem, 79.
37. Ibidem, 80.
38. Richta, *Komunismus a proměny lidského života*, 28.
39. Ibidem, 51.
40. For the institutional history of the STR research team, see Jiří Hoppe, "Radovan Richta a mezioborový tým pro výzkum společenských a lidských souvislostí vědeckotechnické revoluce: Proč a jak vznikala Civilizace na rozcestí", in „O nový československý model socialismu."Čtyři interdisciplinární vědecké týmy při ČSAV a UK v 60. letech, edited by Jiří Hoppe (Prague: Ústav pro soudobé dějiny AV ČR 2015), 44–76.
41. Radovan Richta et al., *Civilizace na rozcestí: společenské a lidské souvislosti vědeckotechnické revoluce* (Prague: Svoboda, 1969 [1966]). The first edition of the book was published in 1966, but I am using the third Czech edition. The book was translated as Radovan Richta et al., *Civilization at the Crossroads: Social and Human Implications of the Scientific and Technological Revolution* (New York: International Arts and Sciences Press, 1969). For a recent philosophical analysis of *Civilization*, see Stefan

Bollinger, "Der 'Richta Report'—Vergessene Marxistische Alternativen in Zeiten der Produktivkraftrevoluiton," *Sitzungsberichte der Leibniz-Sozietät* 76 (2005): 75–90.

42. National Archives in Prague, Presidium of the CPC 1962-1966, folder 142, archival number 149, point 8, Presidium of the CPC meeting May 3, 1966, *Společenské a lidské souvislosti vědeckotechnické revoluce* (Social and Human Implications of STR).
43. *Společenské a lidské souvislosti vědeckotechnické revoluce* (Social and Human Implications of STR), Presidium of the CPC meeting April 25, 1966, National Archives in Prague, Presidium of the CPC 1962-1966, *svazek* (volume) 142, *archivní jednotka* (archival number) 149, *bod* (point) 8, Presidium of the CPC meeting May 3, 1966, *Společenské a lidské souvislosti vědeckotechnické revoluce* (Social and Human Implications of STR).
44. Although the title of the book was similar to that of *Science at the Cross Roads*, that is to say collection of papers presented by Soviet delegates at the London International Congress of the History of Science and Technology in 1931, it seems that this similarity was not deliberate. *Science at the Cross Roads* was not one of the sources of the STR theory in Czechoslovakia.
45. Richta et al., *Civilizace na rozcestí*, 234–235.
46. Ibidem, 253.
47. Ibidem, 302–303.
48. Ibidem, 257–259.
49. Karel Kosík, "Our Current Crisis," in *The Crisis of Modernity: Essays and Observations from 1968 Era*, edited by James H. Satterwhite (Lanham: Rowman and Littlefield, 1995), 17–51. For Kosík's critique in the broader context of the reception of Richta's concept of STR, see Aant Elzinga, "The Rise and Demise of International Council for Science Policy Studies (ICSPS) as a Cold War Bridging Organization," *Minerva* 50/3 (2012): 299.
50. Voříšek, *The Reform Generation*, 228–231.
51. For discussion among members of communist party Presidium officials, see *Společenské a lidské souvislosti vědeckotechnické revoluce* (Social and Human Implications of STR), Presidium of the CPC meeting April 25, 1966.
52. For the English translation of "Action Program," see Robin Alison Remington, ed., *Winter in Prague: Documents on Czechoslovak Communism in Crisis* (Cambridge, MA: MIT Press, 1969), 88–137.
53. David Rodnick, "Review: Civilization at the Cross-Roads: Social and Human Implications of the Scientific and Technological Revolution by Radovan Richta," *American Anthropologist* 75/2 (1973): 441.

54. Daniel Bell, *The Coming of Post-Industrial Society: A Venture in Social Forecasting* (New York: Perseus Books, 1973), 109.
55. Ibidem, 110.
56. Ibidem, 111.
57. For the "consolidation" of social sciences in Czechoslovakia after 1968, see Libora Oates-Indruchová, "The Limits of Thought? The Regulatory Framework of Social Sciences and Humanities in Czechoslovakia (1968–1989)," *Europe-Asia Studies* 60/10 (2008): 1767–1782; Alena Míšková, Miroslav Šmidák and Hana Barvíková, *Československá akademie věd 1969–1972: restaurace komunistické moci ve vědě* (Prague: Ústav pro soudobé dějiny AV ČR 1998). For case study dealing with Charles University Faculty of Arts in the 1970s and 1980s, see Jakub Jareš, Matěj Spurný, Katka Volná et al., *Náměstí Krasnoarmějců 2: učitelé a studenti Filozofické fakulty UK v období normalizace* (Prague: Univerzita Karlova v Praze, 2012).
58. The canonical work of the official critique of "bourgeois theories" was Ladislav Hrzal and Jakub Netopilík, *Ideologický boj ve vývoji české filozofie* (Prague: Svoboda, 1983).
59. For a critique of Richta's activities in the 1970s, see Karel Mácha, *Glaube und Vernuft. Die Böhmische Philosophie in geschichtlicher Übersicht. Teil IV/2, 1953–1989* (Brünn: Mährische Landesbibliothek, 1998), 203–214. A philosopher who was purged from academia after 1968 and left Czechoslovakia to West German exile in 1978, Mácha characterized Richta's post-1968 development as a shift from "prominent thinker" to "unsympathetic institute director" and academia *apparatchik*. Mlynář, *Nightfrost in Prague* (133–134) portrays Richta as a coward and cynical traitor of Prague Spring.
60. The conference papers were published in *Filosofický časopis* 19/1 (1971).
61. *Člověk – věda – technika: k marxisticko-leninské analýze vědeckotechnické revoluce* (Prague: Svoboda, 1973). For an English translation see *Man Science Technology: A Marxist Analysis of the Scientific-Technological Revolution* (Prague: Academia, 1973).
62. A second product of Czechoslovak–Soviet research collaboration on the STR was published in 1982. See Radovan Richta, Semen Romanovitch Mikulinskiĭ et al., *Socialismus a věda* (Prague: Academia 1982). Translated in English as Radovan Richta, Semen Romanovitch Mikulinskiĭ et al., *Socialism and Science* (Prague: Academia, 1983).
63. Radovan Richta, "Science in Danger," *Teorie rozvoje vědy* 5/8 (1981): 7–30. The article was presented as a paper at the 16th International Congress of History of Science in Bucharest in 1981.
64. Ibidem, 15.

65. Radovan Richta, "Is There a New Type of Science Emerging?" *Teorie rozvoje vědy* 1/4 (1977): 7–44. The article was presented as a paper at the 15th International Congress of History of Science in Edinburgh in 1977.
66. Ibidem, 19.
67. Radovan Richta, "The Role of the Social Sciences," in *The Social Implications of the Scientific and Technological Revolution*: *a UNESCO Symposium* (Paris: UNESCO, 1981), 44–47.
68. Ibidem, 51.
69. Ibidem, 60.

PART III

National Agendas of the Studies of Science Beyond the "Two Blocs"

CHAPTER 9

The Cold War, Political Neutrality, and Academic Boundaries: Imprints on the Origins and Early Development of Science Studies in Sweden

Aant Elzinga

I have spent most of my academic life in Sweden where I was part of the emergence and development of science studies as a field of research and teaching. The review that appears below is therefore necessarily personal and is not meant to be comprehensive. It reflects an insider's perspective on the early societal context and different tensions regarding the preferred orientation of a field that promised new knowledge on the dynamics of science relevant for policymakers but also a source of criticism of the power elites.

As I have written elsewhere, the field of science studies has a double legacy.[1] On the one hand, there was the influence of scholarship in history and philosophy of science, as well as science policy studies. On the other hand, there was the international solidarity of the peace movement and

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Pugwash in the late 1950s, anti-imperialism, and the spirit of 1968 and "Science for the People," all linking back to an older movement of social responsibility of science that had existed since the 1930s.

A decade and a half after the termination of World War II, the legacy of the 1930s social responsibility movement in history and philosophy of science was eclipsed by a "debate" on internalism–externalism, a "turn" that in these fields also informed research agendas in Sweden. The Marxist historian Sam Lilley's long essay, "Social aspects of the history of science," appearing in a UNESCO-supported journal, was soon forgotten as the internalism in the historiography of science crowded out the so-called externalism, which became a dirty word because it resonated with Marxian precepts.[2] Significant for the matter of the present account was also Sweden's position as a neutral country embarked on a "third road" that combined elements of capitalism and a reformist mode of socialism. The so-called Swedish model was founded on an historical pact between labor and capital dating back to the mid-1930s. The liberal corporativist social contract facilitated capitalist economic growth combined within a framework of government regulations to foster a welfare state for the benefit of all citizens.[3] This model had implications for science policy in as far as it entailed the Western division between policy for science and science for policy, albeit with a stronger emphasis than usual on the latter. Publicly funded research, moreover, was the domain of the universities—national institutions subject to public accountability and transparency.

In Sweden, science studies largely followed the same pattern found in other Western countries, but there was greater awareness of scholarship in Eastern Europe and the Soviet Union. In the late 1960s, the radical science movement's critique of the role of Big Science in both the USA and USSR became important. The critical discourse that emerged had some bearing on science policy studies but in practice it had little effect on the actual policies in Sweden.

In the following discussion, the emergence and development of science studies and its various problematics in five different locations are highlighted. Owing to the historically longer continuous trajectory at the University of Gothenburg and the early creation of the Research Policy Program at Lund University, as well as the author's own affiliations with these two institutions, the stories of the scenes in Lund and Gothenburg are given more space than those of the other sites.

Sweden's Place on the Science Map of the Nascent Cold War

Sweden did not participate in World War II but maintained a neutral stance, trying to balance between the two hostile camps during the war. Nazi Germany was allowed a railway corridor for troop transports to the occupied Norway, and Swedish iron ore was also shipped to Germany. When it became clear that the Allied Forces would win, contacts with Britain increased and it became easier for persons in the Danish and Norwegian resistance movements to find a safe haven in Sweden. After the war, an attempt was made to create a neutral pan-Scandinavian pact, but this failed as both Denmark and Norway joined the North Atlantic Treaty Organization (NATO). Sweden then opted for a policy of non-alignment and armed neutrality, albeit covertly combined with military coordination with NATO via Danes and Norwegians. With its industry intact during the war there was ample opportunity to export industrial goods to war-ravaged European countries. In this context, the armed neutrality meant building up a strong armed force, with the SAAB company designing and constructing several generations of advanced fighter planes.

The immediate postwar decades forged a sense of technological optimism. In November 1955, a conference with 400 participants was held in a corporativistic spirit in Stockholm. Called by the Social Democratic Prime Minister Tage Erlander, the *Rigoletto Conference*, as it was named after the cinema theater in which it was held, involved top-level scientists, industrial leaders, and trade union representatives. The conference's focus was on Technology and the Society of Tomorrow, and it projected an amazing technocratic vision of growth and development based on advances in science and engineering.[4] The only vocal skepticism expressed came from the Social Democratic Women's movement that came out strongly against the plans for a Swedish nuclear weapons capability and was also skeptical of some other high technology projects.

Sweden already had a nuclear research reactor and intended to develop an electrical power capacity using a heavy water reactor, hoping that this would provide the possibility of upgrading domestic uranium in order to develop an atomic bomb. This project was however terminated as a result of the pressure from the USA to abstain combined with the commercial availability of much more cost-efficient light-water technology from the other side of the Atlantic. Thus in 1968, Sweden signed the international Non-Proliferation Treaty. In practice, Swedish freedom of action associated with its status of political neutrality was soon reduced even further. Thus, the fighter plane *Viggen*, for example, incorporated many parts manufactured in the USA so that the possibility of selling this advanced aircraft to other countries was constantly frustrated. The American demand of a blockade against strategically important technology being transferred to the USSR and the Eastern European bloc was also enforced and reluctantly accepted. In return, Sweden could secretly count on being covered by the NATO shield in case of an attack from the East. In practice, then, Sweden leaned toward the Western side in its neutrality. So, in Swedish military games the enemy was always in the East, never in the West. And NATO planes were permitted to pass over Swedish territory on spy missions in the Arctic regions.

The pursuit of basic research in Sweden was not wholly indifferent neither to the country's long-term economic and military potential, nor to its strategic geopolitical position between the spheres of influence of the two superpowers, the USA in the West and the USSR in the East. With ready access to a highly militarized Arctic Sweden, it was thought, might become a future battlefield utilizing a new generation of high-technological weapons in the air, over land, and in the icebound seas. A clear reminder of the country's sensitive geopolitical location in northern Europe can be found in a now declassified report on Swedish science written in 1949 when the Cold War conflict between East and West was intensifying. Commissioned by the US Central Intelligence Agency (CIA) the 60-pages report (stamped "Secret") presents a very readable and penetrating précis of Sweden's scientific capability.[5] Indeed, it constituted the best overview of Sweden's scientific and technological resources as well as its research policy machinery. In the country itself, there was no comparable overview done at the time. The report identified Sweden's leading researchers and research administrators who were active in strategically important areas of science, and who, as the report stated, should be immediately lifted out of the country and moved to the USA in the event of the Soviet invasion:

> It is clear that the totality of Swedish scientific effort will not seriously add or subtract from the US potential …. Nevertheless, in view of the great capabilities of certain Swedish scientists, among them M. Siegbahn, A. Tizelius, and T. Svedberg (see Appendix B for a more extended list), it would be of advantage to have them working in the US side. Probably nothing essential in the way of increased scientific contribution would be gained by forcing

> Sweden to join the Western Alliance. In the case of hostilities, however, it is felt that a considerable advantage would be gained by evacuating the top 20–40 or more Swedish scientists to deny long-range benefits to the USSR.[6]

The list of scientists in the report's Appendix B included 25 outstanding personalities in several cutting-edge specialty areas: bacteriology, biochemistry, cellulose chemistry, engineering science, physiology, nuclear physics, and tele-technology. Also included was a philosopher and a social democratic politician Malte Jacobsson, an onetime Rector of Gothenburg University College and the Chairman (1947–1956) of the Swedish Atomic Energy Company.

Given the context described above, the science policy obviously supported Swedish national needs and interests, focusing on high-energy physics, the new areas of research in chemistry, nuclear physics, materials science, and the life sciences. The Swedish Defense Research Agency (FOA), founded in 1945, served to some extent as a think tank in the 1960s. It was there that the systems theory, operations research, technological forecasting, and new ideas in research policy, which were developed elsewhere, were monitored and discussed with an eye to their implications for Swedish policy. The Rand Corporation and Herman Kahn's futurological scenarios with an anti-communist thrust influenced FOA's analysts such as Eskil Block.[7] The Swedish Royal Academy of Engineering Sciences (IVA) also played an important role when it came to stimulating Swedish initiatives in science policy studies and "futurology," as will be noted below.

However, it was not only the relevance pressures of Sweden's military–industrial complex that informed the research policy agendas and stimulated science policy studies. There was also, as already noted, the country's character as an advanced welfare state and its corporate mode of governance, the so-called Swedish model of development predicated on a "third way" that combined capitalism with elements of socialism. This gave Swedish research policy a distinctive *dirigist* style that was much more prominent here than in most other countries in the West.

Having pointed out all of the above, it must also be remembered that because of its overt neutral position in a conflict-filled world, the Swedish capital in 1966 became the site where the Stockholm International Peace Research Institute (SIPRI) was established. The idea originated two years earlier when Tage Erlander suggested commemoration of Sweden's previous 150 years of unbroken peace by creating an independent international think tank for understanding preconditions for a stable peace and for peaceful resolution of international conflict. SIPRI worked through its enabling the experts to go to Sweden without representing their governments' viewpoints but their own understanding of issues. This was decisive, for instance, in the debate on seismic verification on nuclear tests.[8] Over the past 46 years, the annual *SIPRI Yearbook* has been an invaluable source of credible data and analysis in security studies. Additionally, since 1901, Stockholm is the home of the Nobel prizes for physics, chemistry, and medicine or physiology, which makes that institution's archives an attractive place for historical studies on the vagaries of science and internationalism in a past that lies before the 50-year secrecy veil.[9]

Sweden's neutrality during the Cold War thus catered to the building of bridges between East and West. This became an important context for the evolution of science policy studies since it, additionally, implied a greater awareness of similarities between trends in the USSR and the USA. It is not accidental that the uncovering of the "science of science" as something similarly informing both East and West happened in Sweden, as we shall see.

As elsewhere, in Sweden the Cold War renewed an anxious debate on the social role of science as well as urged to search new analytical categories to critique it. This led to a revamp of the existing analyses on the social responsibility of science dating back to the 1930s. A reaction to the Cold War first took form with the Stockholm Appeal of 19 March 1950 to Ban the Atomic Bomb, but thereafter the opposition against the imperialism of both superpowers grew markedly. This culminated in a particularly strong grassroots solidarity movement in support of the Vietnamese National Liberation Front (FNL) and against the US-led aggression. Students' revolt against university reforms, the Spirit of '68, radicalized the student movement, parts of which also criticized Sweden's own military–industrial complex. The Cold War politics thus evoked its own counterpoint, a dialectics of liberation, in which the social movements sought alternatives, an outcome being a stream of radicalism that continued for a long time in science studies both in Gothenburg and Lund, even after the field was professionalized *qua* science and technology studies (STS). This is another part of the storyline that will be sketched below, but first it is necessary to identify some facets that linked the new radical science movement to the older one of the 1930s since the latter's legacy was also important for the post-World War II trajectory of science studies in Sweden.

A Legacy Lost and Retrieved: The Social Responsibility of Science

During the 1930s, the threat of fascism, the events in Italy, Germany, China, and elsewhere had a consciousness-raising effect on many scientists, especially the natural scientists.[10] A physicist at Lund University, Torsten Gustafsson was among those active in opposing a notable Nazi trend in academe. In 1942, he was one of ten Lund professors who published a collection of essays on science and society.[11] In his piece, Gustafsson raised concern about one of the consequences of a narrow specialism. As he argued, scientists lacking political consciousness can be gullible and naïve regarding what had happened in Nazi Germany. Unable to resist the "suggestions of the times," some of them, he notes, fell for astrological and other mystical conceptions, which are incompatible with the outlook of modern science. They are schizoid in their worldview, combining, on the one hand, their learning in the natural sciences with, on the other hand, the most absurd and obscurantist positions in as far as social questions are concerned. As an antidote, he argued, academics need to be aware of the bigger picture, to look beyond the narrow confines of their specialism and gain a better understanding of science in context, that is, the social relations of science. Much later, professor Gustafsson was instrumental in bringing to Sweden a Cold War refugee, Steven Dedjier, who—in 1966—began the Research Policy Program (RPP) in Lund (see below). Gustafsson's standpoint cited above, along with his actions, resonate with the international social responsibility of science movement with its better-known representatives such as the so-called Bernalists in Britain.

The radical science movement in the 1960s and 1970s differed from and criticized the earlier one of the 1930s and late 1940s. Bernalism and the technocratic mode of the "science of science" promoted in Eastern Europe was now a subject of criticism for its lack of reflexivity.[12] Steven Shapin in his 1992 article revisiting the internalism–externalism (non)-controversy after 30 years pointed out that it was left behind without ever having been resolved.[13] From a different perspective, Steve Fuller found that the background and context of Thomas Kuhn's seminal work *The Structure of Scientific Revolutions* (1962) bore a strong imprint of James B. Conant's post-World War II policy of using the history of science to instill a specific ideologically motivated understanding of science in society. Kuhn helped to reinforce an internalist self-understanding of science in which the concept "Society" did not extend beyond the scientific community.[14] In Sweden, Kuhn's work was well received but for some young scholars associated with the radical science movement it also became a foil for extending the category of the "social" to include a critique of the political economy and the "ideology" of science. Their chief target became science's nexus with the military–industrial complex. Later, during the 1970s, the other pillar of the country's research policy, the welfare state, and the "sectorial doctrine" associated with it, also came under fire, as some of its sectors put academic research on tap in ways that induced an exceptionally strong push from basic to applied science. This pressure eroded academic integrity, causing what I have earlier called "epistemic drift," a concept used in critical science studies.[15]

The next two sections briefly outline the two main strains of Swedish science policy and how they spurred science studies, that is, general policy for science, and science for policy (also called sectorial policy). Thereafter, I trace more specifically a number of university-based trajectories in science studies. These accounts will also (in separate sections) touch upon the critical views associated with the radical science movement that also played a seminal role in the emergence of science studies at a couple of university sites.

Science Policy Studies Stimulated by the Military–industrial Complex and OECD

During the late nineteenth century and during the 1920s and 1930s, Swedish universities were largely oriented toward German traditions of knowledge. After World War II, it was Anglo-American rather than German intellectual traditions that became the source of new inspiration for many academics and policymakers. In Britain, the prophet of a new role for science in the corridors of power, C.P. Snow, had in 1956 launched his thesis of the "Two Cultures" comparing a progressive and forward-looking ethos of science with what he saw as the conservative backwaters of the humanities.[16] He argued for an urgent need to educate large numbers of the postwar students for participation in an ongoing "natural scientific-technical revolution." The thesis was taken up in Sweden a few years later. In Uppsala, on the initiative of the Liberal Student Association Verdandi, an academic home for anti-Marxism, Snow's famous little booklet (1959) on the same topic was translated into Swedish and appeared in 1961 as the first number of the Verdandi-debate series. It was recommended to readers in a spirit of "winning the competition with Communism."[17]

With the development of a science policy doctrine in the West under the auspices of the OECD, Sweden was quick to adopt that organization's leading concepts and categories, such as the linear model of innovation with its distinctions between basic research, applied research, and development. The Atomic Research Council and the Natural Sciences Research Council jointly set up a committee for the study of the economics and organization of research (*Kommitteen för forskningens ekonomi och organization*—FEK). This committee, managed by Göran Friborg, funded a series of studies that became important early sites for the dissemination of these ideas reaching out to the administrators involved in the public management of research.[18] Topical issues were the brain drain, the role of basic research in innovations, science policy in the USA and Soviet Union, the assessment of the increasing volume of R&D in Sweden and other countries, and the means of increasing the efficiency of scientific information retrieval systems. Furthermore, the Engineering Sciences Academy together with FOA saw systems theory and futurology (*futurologi*, later called *framtidsstudier*) as tools for conceptualizing and orchestrating research efforts with an eye to rationalizing innovation processes.

Both FEK and FOA stimulated science studies within a multidisciplinary framework broadly conceived as "Research on Research" (*Forskning om forskning*). Considerable attention was devoted to back-casting the studies done in the USA in the latter part of the 1960s, respectively, by the US Department of Defense and the US National Science Foundation (NSF), regarding the ultimate effectiveness of basic research and inventions for the production of innovative products and military technologies—namely, the projects Hindsight (launched by the DoD) and Technology in Retrospect and Critical Events in Science (TRACES) (launched by NSF to challenge the DoD's negative findings).[19] In 1966, a new popular journal, *Forskning och framsteg* (Research and Progress), started by the three basic research councils (natural science, medicine, and technical science), became a handy kind of Readers Digest that regularly reviewed new developments regarding scientific discovery, funding, and policy. The editor was a well-known professional science journalist with advanced research experience in earth sciences, Eric Dyring. Göran Friborg was also a regular contributor to the journal, which featured the work and interests of many leading researchers.[20] The news about science studies projects funded by FEK also appeared on the journal's pages. The funding from both FEK and FOA created the opportunity spaces for early science studies, which developed in several university settings, such as Uppsala, Lund, and Gothenburg, in the 1960s. In the mid-1970s, the impact of sectorial research policy afforded new patronage circumstances in the fields of science and innovations studies.

Swedish Research Funding Reforms and the Origins of the Mode 2 Metaphor

In 1977, a reform of the higher education was introduced in Sweden. As in the USA earlier on, the reform implied a further move from university to multiversity. Among other things, it brought a range of "new" professional training programs (like nursing) into academe, which called attention to the problem of *forskningsanknytning* (connecting-to-research). This had an immediate impact on opportunities for science studies since the National Board of Universities and Colleges (UHÄ)[21] now created a special program (*Forskning om högskolan*) to stimulate and fund research on the emergence of new disciplines as well as the structure and norms of universities more generally in past and future. For many years a significant number of grants from this program went to scholars in the history of ideas, theory of science, organizational sociology, sociology of higher education, and also political science.[22]

The Swedish research council system was also reconfigured in 1977. One aspect of this change helped spur the field of science studies. To meet an increasing demand for funding of interdisciplinary activities at the universities, for example, a (now defunct) Council for Co-ordination and Planning of Research (*Forskningsrådsnämnden*—FRN) was created. It had several sub-committees—one was concerned with stimulating Future Oriented Research, another provided grants for popularization to promote what later became known as Public Understanding of Science, a third took over FEK's role to help promote science policy studies. One of the first international publications produced by FRN was a report by sociologist of science Stuart Blume, *Science policy research: the state of the art and implications for policy* (1981).[23]

In the mid-1980s, Roger Svensson, responsible for FRN's science policy studies program, initiated a project that intermittently over the course of five years brought together a small group of international researchers. Led by Micheal Gibbons, this group was asked to investigate the changing forms and conditions of research (interdisciplinarity, heterogeneity, globalization, and a shift closer to the sites of application) whereby—as Svensson put it—“the universities seemed to be losing their monopoly on advanced knowledge production.” A rather informal six-person task force engaged to probe the matter was not pressed by deadlines or reporting demands. The result became the most influential publication that FRN ever produced, viz., Michael Gibbons et al.’s now classical book, *The new production of knowledge*: *the dynamics of science and research in contemporary societies* (1994), outlining a well-known Mode 2 thesis.[24] Ultimately, the outcome was made possible thanks to the aforementioned new patronage circumstances in Sweden that brought with them a focus on interdisciplinarity. The new funding modes afforded the opportunity spaces to revisit the internalist–internalist debate as well as that on the social responsibility of scientists. Nevertheless, scholars who chose to pursue science studies at Swedish universities were still constrained by the matrices of their individual home disciplines.

Science Studies Framed Within Political Science (Uppsala and Stockholm)

In terms of the disciplines within universities, some elements of science studies emerged from the history of ideas, political science, sociology, business administration (particularly industrial economics and organization) and theory of science. In Uppsala and Stockholm it was within political science that science policy studies took form. Influential was the pioneering work of Sverker Gustavsson who later combined an academic career with an intermittent involvement in Sweden’s national science policy at the level of (a social democratic) government.

In his first study, a Licenciate thesis in 1966, entitled *Autonomi och heteronomy. Ett perspektiv på den forskningspolitiska idédebatten* (Autonomy and heteronomy. A perspective on the science policy doctrine debate), Gustavsson picked up the trail of Samuel Lilley and reviewed the debate between Liberalism and Marxism regarding science policy following upon Michael Polanyi’s and the Society for Freedom in Science’s famous critique of Bernal’s *The Social Function of Science* (1939). He also summarized the postwar discussions right up to the debate on criteria for scientific choice in the Edward Shills’ newly founded journal *Minerva* in the 1960s.[25] Thus, he highlighted both historiographical and research policy issues that lay at the core of the internalism–externalism debate. The publication was financed by FEK.

In his doctoral thesis in 1971, Gustavsson dealt with the integrity of scientific advice as a precondition for well-founded political planning and decision-making.[26] Inspiration came from the writings of a Harvard professor Don K. Price who coined the phrase “science speaks to power” as central to a model of the relationship between science and politics. Price built on Max Weber’s concepts of bureaucracy, civil servants, and professionalization, and he introduced the notion of “scientific estate” situated at the pure rationalist pole of a spectrum where the other pole is the personally embodied knowledge and “charisma” of politicians. This view, also adopted by Gustavsson, was challenged by the latter’s more radical young colleagues (especially at the universities of Lund and Gothenburg) who participated in the anti-authoritarian Students for a Democratic Society (SDS) movement of 1968.

Gustavsson for his part was active as a member of the editorial board of the Social Democratic Party theoretical journal *Tiden*, and later was for a while the deputy minister for science in a social democratic government (1986–1991). In the debate on science and society, he consistently followed the neo-Mertonian position according to which the issues of organization and production of scientific knowledge must be dealt with separately from those regarding epistemic content and modes of validation. In the 1990s, he was a strong critic of the so-called new social contract for science based on the notions like Mode 2 knowledge production and Triple Helix fusion of academic science, state power, and industry.

At the University of Stockholm, a group focused on the study of higher education and science policy was built up within political science. The supportive role of Sweden’s nester in political science and friend of Olof Palme, Professor Olof Ruin, was important for this development. Several members of the group produced influential studies on Swedish science policy (e.g. Rune Premfors and Björn Wittrock).[27] Wittrock later became the main director of the Swedish Collegium for the Study of Social Sciences (SCASSS) in Uppsala,[28] a forum that has facilitated several comparative science policy studies relating to developments in different countries.[29] The initiatives both at Uppsala and Stockholm were strongly tied to individual scholarly efforts and did not immediately lead to any longlasting institutionalized science studies unit. Institutionalization of science studies on the other hand appeared in Lund, Gothenburg, and Linköping. Nowadays however there is an STS center in Uppsala with its administrative seat at the Department of Economic History.[30] At the

Royal Institute of Technology (KTH) in Stockholm studies in the (social) history of science, technology and the environment extend into STS.[31]

Science Studies Linked to Sociology Helped by the Cold War (Lund)

At Lund University, science policy studies were institutionalized in a setting formally linked to sociology but also supported by senior researchers in history, physics, and political science. The history of Lund's unit illustrates how the country's neutrality could be used to advantage as a platform where one could navigate between and learn from both blocks, linking science policy studies directly to the Cold War and the fact that Sweden occupied a geopolitical space between the two rival and mutually hostile superpowers.

A Research Policy Program (RPP) at Lund was formally established in 1966, the same year that the Science Policy Research Unit (SPRU) was founded on the University of Sussex campus. The director of the RPP in Lund was a refugee from Tito's Jugoslavia, Stevan Dedjier who had practical research policy experience in his home country. He was acquainted with both John D. Bernal and Christopher Freeman (the director at the Sussex unit), and with an excellent command of both Russian and English, he had a unique overview of trends in both East and West. The first thing he did was to build up an extensive library with the relevant documents, books, and journals from the countries on both sides of the "iron curtain." The unit, later called the Research Policy Institute (RPI), was affiliated with the university's department of sociology.

Dedjier went to the USA from his native Serbia to study physics where he received his degree from Princeton in 1934. Since the Depression years, he was also close to political movements on the Left. He stayed on to become a journalist, a communist activist, and an organizer in the Pittsburg mining district, and later became a General Maxwell Taylor's bodyguard in the American airborne division that parachuted into Arnhem during World War II. At the time he became temporarily involved in the intelligence work with the CIA's predecessor organization. After the war, he settled in his homeland Yugoslavia where he returned to physics and became the head of that country's nuclear research institute (1952–1954).[32] Due to a conflict with the Tito government he was sacked and counted himself as a nomadic internal exile before ending up in Copenhagen (1961) with Niels Bohr.

The physics professor at the University of Lund, Torsten Gustafsson (whom we already met—see above), who was a friend of Niels Bohr, helped ease Dedjier's way into neutral Sweden where our émigré first ran a weekly science policy seminar in Stockholm. The topic was science policy and the seminar was sponsored by the Engineering Sciences Academy. On many occasions, the guest lecturers (among them Derek J. de Solla Price) were invited to speak in this forum, which became an important medium for the dissemination of the concepts, models, and approaches developed in the USA and elsewhere. It was here too that the political science student Sverker Gustavsson, while preparing his Licentiate thesis in Uppsala (see above), participated in 1964 and 1965, and received important feedback from both Dedjier and other seminar participants.[33]

Torsten Gustafsson, it must be added, was a childhood friend of the Swedish Prime Minister Tage Erlander and his informal science advisor, a relationship that helped incorporate Dedjier into the Swedish academic scene in Lund (1962). After the advanced studies in science policy and invitations (intermittent periods in 1963 and 1965) to Harvard and Yale (collaborating with de Solla Price)[34] as well as short periods in India, Dedjier became an Associate Professor in Lund. An early institutionalization of the field of science policy studies in Sweden was thus supported from the highest political level while the opportunity spaces like the seminar in Stockholm were afforded by the Engineering Sciences Academy and the military think tank FOA.

The indirect translation of Swedish political neutrality into science policy studies in this country can be discerned in Dedjier' publications. Some of these reflected national policy thinking in the Swedish government, but he took advantage of his own "neutral" location to stimulate a dialogue across the East–West divide. In 1964, Maurice Goldsmith and Alan Mackay published a little anthology commemorating the 25th anniversary of Bernal's classical book. They emphasized once again the importance of scientists' responsibility to the world in which we live. The volume was revised and reissued as a popular Penguin pocketbook in 1966 under the title *The Science of Science*. Contributors included Joseph Needham and de Solla Price. Dedjier also had a contribution, in which he discussed "the ideology of national research policy" as an international phenomenon evidenced by quotations from statements of leaders in different countries. The first quotation he lists is from Tage Erlander, and Dedjier argued that research planning was becoming very much part of scientists' social reality but, paradoxically, there was no requisite knowledge base regarding how research develops or what it does or could do to advance the achievement of national goals.[35] The same year, now in the journal *Minerva*, Dedjier took a cue from international discussions at the time, not least in Eastern Europe, and publishes “a programme and a plea” for promoting the science of science.[36] By this time, he already had a significant publication record in the field of science policy studies.[37]

It may be noted that two years earlier he had been impressed by Stephen Toulmin’s stocktaking paper in *Minerva* (also analyzed in Gustavsson’s thesis) on the debate regarding the “criteria for scientific choice” and wrote a short piece in the journal’s correspondence section under the heading “The Unity of Science Policy.” He had been collecting and surveying the Soviet literature on science policy (then called “scientific policy”) and noted how similar themes appeared both there and in the West but while authors on both sides of the East–West divide dealt with the same problems in very similar ways, they disregarded each other’s contributions to the discussion “of a problem common to all societies.”[38]

In fact, as he put it, “An iron curtain … hangs between the students and practitioners of scientific policy in liberal countries and those in communist countries.”[39] In the Soviet Union, he noted, “thought on the nature of science and on scientific policy is sloughing off its earlier dogmatism.”[40] Thus, the time was right to try and stimulate an exchange of experiences and communication on science policy across the Cold War divide. This idea appears to have fit in well with one of Edward Shill’s ambitions at the time since Dedjier’s appeal for a unified approach to science policy appears in the same volume of *Minerva* that reprinted Maria Ossowska and Stanislaw Ossowski’s classical paper on the science of science written in 1936.[41] Subsequently *Minerva* also reported on papers presented in Warsaw during the 1965 International Congress on History of Science held where J.D. Bernal and Alan Mackay delivered a paper entitled “Towards a science of science,” later reprinted in Russian in a history of science and technology journal.[42]

The Spirit of ‘68 and Radical Science Critique

Apart from catering to Swedish military–industrial interests in monitoring international trends in research policy and promoting science policy studies in this vein, the RPP also proved to be an incubator for graduate students with radical ideas that were critical of these same trends. Several young scholars used Dedjier’s excellent library to write critical studies, for example, Ronny Ambjörnsson, Gunnar Andersson, Jan Anderstedt, Aant Elzinga, and Andrew Jamison.

In 1968, the issue of scientists' social responsibility was a particularly hot topic when Lund University during celebrations of its tercentenary (in a town heavily policed to "calm" student protests against feting monopoly capitalist co-optation) hosted an international symposium on Scientific Research and Politics.[43] This was attended by a number of radical figures in the world of social science, like Nicos Poulantzas and André Gunder Frank. From Lund itself there were several participants including Dedjier, Joachim Israel, and Göran Therborn, and from Gothenburg two doctoral students, Ronny Ambjörnsson and Aant Elzinga.[44] The symposium and the resulting book was important in further introducing new ideas about the role and function of science in society into Sweden. Like the ongoing international debate, these discussions reflected the tension between participants who expressed a liberal philosophy of science and those who held a more radical view.[45] Together with Stevan Dedjier's career, these events further illustrate the distinctiveness of the Swedish trajectory toward science studies, indicating how Swedish neutrality helped create an ecumenical space for a dialogue across an ideological divide perpetuated by the Cold War.

Dedjier also helped raise awareness about the problems facing the so-called Third world countries. One of his areas of expertise was the issue of brain drain from the underdeveloped world (as it was then called) to the industrialized West. Later, he developed the idea of science studies as a form of business intelligence in the context of promoting innovation. The concept was similar to that of research foresight but with much more emphasis on information processes and structures. He was succeeded in 1986 as director of RPI by Jon Sigurdsson who had an engineering background and came to science policy during his time as a science attaché at the Swedish Embassy in Beijing. When he came to Lund the link between Science, Technology, and Innovation (STI) and development studies grew again. So did a focus on Japan and the Japanese approach to science and technology policy that was much discussed in those days.

Early on Andrew Jamison working at RPI forged links between STS and environmental policy/sustainable development. Jamison did his *Filosofie Doktorat* in theory of science in Gothenburg University.[46] The arrangement in Lund for a long time was one where advanced studies could be pursued at RPI but doctoral degrees had to be awarded within a mainstream university discipline. This is similar to the model that originally existed for science studies at Cornell University in the USA. It is only recently that the institute in Lund has been given the right to produce its own PhDs. Despite the fact that RPI did not have a PhD program until 2007, a large number of graduate students did their doctoral work within the Institute (while formally they belonged to another department). RPI has produced over 15 doctorates to date.

Systems Theory and Futurology, New Impulses in Philosophy of Science (Gothenburg)

At the time of writing theory of science at the University of Gothenburg is part of a larger academic department that also includes philosophy and linguistics. The discipline has a complicated history, initially emerging from philosophy 50 years ago as an independent discipline, separating from the philosophy department in the late 1960s, and then after the turn of the century it was clustered together with first the history of ideas and now philosophy and linguistics. In the course of this entire 50-year period, 30 persons have obtained their Fil Dr and five their Filosofie Licentiate exams in theory of science.

In principle, science studies also encompasses philosophy of science. Traditionally, however, in the school of analytical philosophy the latter discipline is by definition excluded from the cluster of disciplines that in the 1950s were grouped under “science of science.” This was made clear in the widely read textbook edited in 1953 by two University of Minnesota-based philosophers of science, Herbert Feigl and May Brodbeck. In her Introduction, Brodbeck, referring to the “science of science,” explained that “‘sociology of knowledge’ and ‘history of ideas’ are current names for this/broader/study, which is interesting and important .... Yet, systematically, the science of science belongs to sociology and psychology, to the behavioural sciences. It is a way of engaging in scientific activity rather than a way of talking about that activity. Hence, it is not part of philosophy of science.”[47] Feigl & Brodbeck’s narrow approach to science could be found among most scholars at philosophy departments across Sweden, from Uppsala to Stockholm and Lund, and at first also in Gothenburg.

The Swedish name for the discipline is *vetenskapsteori*, comparable to the German term *wissenschaftstheorie*. It was established at the University of Gothenburg with a personal chair for Håkan Törnebohm in 1963.[48] He chose to emphasize that the new subject, in keeping with the fact that the Swedish term *vetenskap* is broader than the English term “science,” was not limited only to studies on physics or the natural sciences (which had inspired the logical positivist tradition) but also covered meta-theoretical studies in and on the humanities and social sciences (e.g., the schools of hermeneutics and critical theory). He moreover had close ties to history of science—a field that in Sweden had largely been cultivated as a part of the history of ideas and learning or "intellectual history."

Second, following Albert Einstein's precept that philosophers should not only look at what scientists say but much more at what they do, he soon introduced an "empirical turn," which opened up the relevance of historical (and later, also socio-politically-oriented) case studies to explore and develop models of the "growth of science."

Third, when it came to developing heuristic models of research processes, inspiration was drawn from cybernetics and systems theory, fields that matured in military contexts during World War II and flourished in military–industrial-oriented think tanks like Rand Corporation in the USA and FOA in Sweden.

Fourth, this new intellectual trajectory in theory of science captured the attention of and soon also funding initially from FOA but also from FEK[49] and thereafter from FRN and other granting agencies for a series of multidisciplinary colloquia that ran continuously from the mid-1960s onward for about ten years.

The early Cold War setting in the case of theory of science was thus mainly reflected in a certain resonance with FOA's interest at the time in monitoring new developments in systems theory, research policy, and forecasting. For our part as academics it afforded an opportunity space and the development of informal inter- and transdisciplinary networks. Colloquia were begun that gathered not only scholars from different disciplines but also research managers and the occasional policymaker or planner.[50] One of FOA's futurologists, Eskil Block also participated and eventually enrolled as a doctoral student with an intention of writing a thesis comparing different paradigms in futures studies, an unending project that was never completed. Nevertheless his project was the clearest confirmation of how the foregoing development can be read as a disciplinary diversification that was close to the Cold War agenda of having more predictive power, but for that very same reason it also evoked its own counterpoint, views, and critical approaches closely associated with the new radical science movement that emerged in opposition to the Cold War agenda.

What assisted both these trends in Gothenburg was the fact that conceptually the theory of science was put on new foundations that entailed an "empirical turn," that is, a shift from a logical to an empirical study of sciences, which in turn implied a distinct deviation from May Brodbeck's way of drawing boundaries.[51] Consequently the term "theory of science" itself, with its earlier accent on "logic and methodology" was locally translated into English as "theory of science and research." Logical analysis and methodology was in part replaced by heuristics and systems theory; theory of science was reconfigured into a modeling of "inquiring systems" conceived as changing ensembles of knowledge (often hypotheses), attended by problems and instruments with feedback loops and internal as well as external "regulatives" (ideal of science, world picture, values, ethical considerations, etc.) viewed in context.[52] In other words "theory of science and research" was understood as an active integrative ingredient in a broader multidisciplinary area we called "research on research," something on par with science of science. The latter formulation also appears in some early papers written by Törnebohm and his students Gerard Radnitzky and Göran Wallén.[53]

Internal tensions within the Department of Theory of Science existed because a few PhD students were committed to the new radical science movement and tended to be more concerned with external factors (how science in society was being steered) and questioned some of systems theory's managerial perspective and its obvious resonance with military–industrial interests. Still, everyone shared a common denominator: critique of positivism and logical empiricist philosophy of science. That a critical trend could develop under the same roof owed much to Törnebohm's genuinely democratic ethos and non-authoritarian leadership style. The research environment at the department was very inspiring and permitted fruitful debate as well as critical analysis of science in society in a Marxist vein. Consequently *dialogue* focused on research processes (which was the rationale for the colloquia) extended a notion of *paradigm theory* parallel to Thomas Kuhn's to the task of uncovering "steering factors" *both* internal to scientific work, so-called ideals of science, views of nature and man, and concomitant methodological precepts, *and* "steering factors" external to science (economics, politics, ideology, etc.). Further, Imre Lakatos' concept of research programs was also articulated in a number of case studies.[54] A focal point was the emergence of new disciplines, interdisciplinarity and, later, also scientific controversies. Characteristically Törnebohm, however, liked to distinguish between knowability (*vetbar*—what can be known) and know-worthiness (*värt att veta*—what is worth knowing), and pointed out that logical empiricism's internalistic normativity tended to focus only on the first of these criteria whereas oftentimes it is just as important (if not more) to ask the second question. This approach also gave a new twist to the internalism–externalism discussion and encouraged both prongs in an effort to critique science.

One of the first doctoral theses was Gerard Radnitzky's *Contemporary Schools of Metascience* (1968). It provided an overview of logical empiricism, hermeneutics, and critical theory (Habermas) and contained a critique of the ideal of science promoted by analytical philosophy. Mainstream philosophers in Sweden rejected the work as belonging to sociology rather than philosophy of science. This led to a schism and Törnebohm leaving the department of philosophy to set up his own unit for the theory of science. Formal independence from the mother discipline did not come until much later, institutional change in universities being slow and difficult, especially when economic resources are involved. It was a case of a Swedish version of the famous Popper-Adorno debate on the virtues and discontents of positivism, known as the positivism-struggle (*postivismusstreit*).

Radnitzky subsequently moved to Bochum and then Trier where he was professor in philosophy, became a friend of Friedrich von Hayek's, joined Franz Joseph Strauss' conservative think tank, and in the German debate on science policy became a sharp critic of the finalization thesis.[55] This is the thesis that builds on Kuhn's paradigm theory by adding a post-paradigmatic phase, that is, the idea that when a fundamental theory in science becomes "closed" or "mature" it is "finalized," which means that it is no longer internally driven and henceforth easily steered from outside by social interests. The Starnberg group (Gernot Böhme, Wolfgang van den Daele and Wofgang Krohn) responsible for this three-stage model of the growth of science thus suggested a possibility of linking the advance of science to external mission orientation. A misleading claim made by the extreme libertarian Radnitzky but also other anti-finalists like Gunnar Andersson was that the finalization theorists, if they gained influence in science policy, would signify "the end of free basic research"; comparisons were made with the loss of autonomy through ideological and political steering in the Soviet Union. It is interesting to note that Törnebohm's distinction between internal and external "steering factors" in the growth of science actually paralleled the scheme of internal and external "regulatives" or determinants identified by the Starnberg group.[56]

The same year Radnitzky got his degree, Aant Elzinga completed a Licentiate thesis entitled *Normal and revolutionary science*, *with special* *reference to traditions*, reviewing a large number of historiographical models of science and classifying these as basically autoletic (i.e., valuing science for its own sake) or heteroletic (valuing science for some external utility).[57] This scheme derives from Törnebohm's epistemological distinction while incorporating Sverker Gustavsson's organizational and policy-related concepts, "autonomy" and "heteronomy."

Gunnar Andersson, who also studied with Törnebohm, built further on Gustavsson's critical analysis of the debate about science and society between Marxists and Liberals, first producing an essay (1972) and then a doctoral dissertation (1975) on this topic.[58] In 1972, Andersson followed Radnitzky to Germany where he became an authority on Karl Popper's critical rationalism before returning to Sweden in the late 1980s to join philosophy of science at Umeå University.

Political Activism and its Sublimation in Science Studies

Research in Gothenburg on internal and external steering factors in science was important for the emergence of new perspectives in science studies that was informed by the radical science movement of the late 1960s and early 1970s. This social basis coincided with similar developments elsewhere. The Harvard historian of science Everett Mendelsohn in an interview in the summer of 2000 recalled the importance of a political activism back in the late 1960s and early 1970s that also led him to an interest in science policy. "Part of what I did do was to go off into work on social studies of science—policy things. Some of the activism carried successfully over into the scholarly world."[59] It was part of a wider movement for the social responsibility of science (with both a social liberal and a socialist wing) that had roots going back to the late 1930s and in which natural scientists played the most significant role.[60] "I was not the only one doing that," recalls Mendelsohn;

> Pugwash started doing a number of things, and we had some interactions. Anti-nuclear weapons, anti-testing, this overlapped with Soviet-American (meetings) because very often they were interested in the weapons as the Americans wanted to talk to Soviets about it. That involved a kind of activism, petitions, campaigns to stop nuclear weapon testing; issue by issue, they were focused and they were operational.[61]

At the University of Gothenburg, the students' radicalism found one of its stronghold in sociology. Here critical studies on the impact of science and technology were undertaken as a part of labor sociology encouraged by Edmund Dahlström. At the departments of history of ideas and theory of science, respectively, a science studies connection was academically articulated in two critical studies in a Marxist vein in the crucial years 1968–1969. One was a history of science text that integrated externalist and internalist approaches, the other was a debate book on science policy in Sweden, the Soviet Union and the USA.

The history of science text, inspired by a modified version of Bernalism, was at first frowned upon by senior scholars but later became popular as a textbook and was widely read both inside and outside academe.[62] The science policy book, on the other hand, being highly critical of the use of science in imperialist ventures and superpower politics both East and West, also challenged the Swedish model of the third way based on an historical pact between capital and labor.[63] We criticized the model as being too conciliatory to the hegemony of monopoly capital interests and by extension too much influenced by developments in the USA. The Soviet order of science and society was also criticized. Because of its radical critique of the social and political relations of contemporary science this book was mostly ignored.

A few years later inspiration however came from abroad where similar critical studies were being pursued. Particularly welcome were in 1976 Steven and Hilary Rose's two well-known publications, *The Radicalization of Science* and *The Political Economy of Science*. By 1980, issues relating to science and technology in the Third World were also taken up, including the earlier impact of the Chinese cultural revolution on science in that country.[64] This raised the question of the significance of anti-science trends and conditions assuring the integrity of science in the face of strong external relevance pressures from whichever direction they came, be it multinational corporations, national governments and state and university bureaucracies, or Maoist imaginaries of a grassroots peoples' science. It was what we later more generally called the issue of "keeping science straight."[65]

In 1976, to mark the culmination of ten years of developments in research on research and 10th anniversary of the establishment of RPI in Lund, a large conference was organized in Gothenburg under the title *Ten Years Research on Research*.[66] The intention was to gear up for a consolidation of this multidisciplinary field in Gothenburg in the form of a Center for Research on Research. It was the beginning of a long uphill process.

The immediate next step was to organize, for two years in a row, a series of rotating colloquia hosted by the various departments in Sweden where science studies was developing. The first year these meetings led to familiarization with each other's research and some amount of cross-fertilization. During the second year some leading researchers in science studies were invited from abroad to help in our efforts to consolidate the new field on a national basis. Everett Mendelsohn and John Ziman played a prominent part in this context. For my part, I had generated a comprehensive overview of Swedish science studies that was later published (1980) in the journal *Social Studies of Science* as a country report.[67]

The experiences summarized in the country report identified several barriers to the development of science studies. One of these was the disciplinary structure in place at Swedish universities, particularly in relevant areas of scholarship. A second factor that reinforced fragmentation was the nonexistence of sustained funding from research councils. Third, it was found that the term "research on research" was being rhetorically highjacked by policymakers and research administrators but without facilitating bottom-up growth of the area as a field of scholarly pursuit. Rather the term entered the science policy discourse as yet another category associated with societal sectorialization—in this case tending to hitch science studies to a technocratic perspective of the growth of science and technology in society.[68]

By this time we were already using the term "science studies" in English, and in order to mark our own critical perspective and epistemological focus it was translated into Swedish as "vetenskapsstudier," although a textbook produced in 1984 still has the title *Forskning om Forskning eller konsten att beskriva en elefant* (Research on research, or the art of describing an elephant).[69] Funded by UHÄ (see above), it is an anthology that in the Swedish context introduced contemporary research in theory of science, historiography of science, sociology of science (the Strong program and Actor Network Theory), Stuart Blume's program for a political sociology of science, science policy studies, and more.

Interdisciplinarity: Tema-T (Technology and Social Change, Linköping)

A site where STS is thriving today is the University of Linköping. That university is a rather new institution dating back to 1967 when a branch of Stockholm University was placed in Linköping. It was granted full status as a state university in 1975. At the time inter- and multidisciplinary research was much in vogue. This led to an experiment. Instead of the traditional academic structure a Department of Thematic Studies was created in 1980, having traditional chairs but focusing these around a theme—for example, "Water" (Tema-V, water resource utilization and regulation), "Health" (Tema-H), or "Technology and Social Change" (Tema-T), to which further areas were later added, for example, "Gender" (Tema-G). The new mode of organization was a novel feature in the Swedish academic landscape and it has facilitated strengths in various areas of STS and STI in Linköping.[70] Instrumental in developing the Tema-T section was Lars Ingelstam who prior to coming to Linköping as professor headed a research group on planning theory at FOA (1968–1972) and was director (1973–1980) of the Secretariat for Futures Studies that was created 1973 in Stockholm at arm's length from (a social democratic) government after a white paper on the subject written by Alva Myrdal and meant as a counterweight to the IVA-FOA mode of futurology since it mainly served only industrial and military interests.[71]

From its beginnings in 1980 Tema-T had four full professors (with roots in history, sociology, literature, and innovation studies) each with ample faculty funding for fostering doctoral students. The sociology and innovations studies orientations have traditionally counted for most overlap with STS and STI. Today, Tema-T is headed by professor Claes-Henrik Helgesson who combines an economics background with a strong interest in STS with focus on medical knowledge and how it is validated in clinical settings on the one hand, and on the other hand the roles of artifacts and markets, how these contribute to shaping scientific knowledge and knowledge-validating practices. Teaching and research staff counts 40 persons, among them six professors, five lecturers, four assistant professors, and three postdocs, and currently there are about 20 fully funded doctoral students, most of them with some kind of STS or STI orientation.

Although Tema-T has not existed as long as the theory of science unit in Gothenburg or the science policy research unit in Lund, it has been more successful in terms of institutional expansion as a science studies enterprise. This is probably because as an institution it emerged at a time when and in a setting where interdisciplinary integration was felt to be important and consequently received favorable consideration from the side both of policymakers and the local university administration. Expansion of STS in Gothenburg and Lund has on the other hand been built on temporary ad hoc alliances attended by periods of consolidation, constriction, and dispersion.

Concluding Remarks

A landmark in the development of STS in Sweden has been the hosting of the quadrennial joint-4S/EASST (European Association for the Study of Science and Technology) conference by Gothenburg University in 1992. The theme was "Science, Technology and Development" and Vandana Shiva and Everett Mendelsohn featured as keynote speakers. Subsequently, as a president of EASST for six years during the 1990s, I further emphasized the science policy connection.[72] Hosting the conference in Gothenburg was part of a strategy from 1986 onward to successively expand science studies in Gothenburg and internationalize. In this period, a large number of graduate students completed PhDs, several of them with an explicit STS orientation.[73] By that time, at the University of Gothenburg we also had, apart from the theory of science department, several other units with science studies involvements, for example, human technology (a field that sprung out of human ecology) that was initially led by historian of science and technology, Mikael Hård, who is now professor at Darmstadt TU. Later it gave rise to a section for Technology and Science Studies headed by Hans Glimell (now incorporated into the Sociology Department at Gothenburg).[74]

A network for the study of scientific controversies (controversy studies)[75] was run jointly by theory of science scholars and colleagues at the sociology department from 1986 onward.[76] This network held international summer schools at the Dubrovnik University Center, involving many participants from a number of European countries.[77] Additionally there existed in Gothenburg at the time a Center for Research Ethics headed by the philosopher Stellan Welin, initially sponsored and funded for ten years by the Gothenburg Royal Society of Arts and Sciences.[78] The establishment of a science studies journal *VEST*, *Tidskriften för vetenskapsstudier* served as a platform for interaction on a broader level in Sweden and Scandinavia. In several of the intellectual environments, gender aspects were also brought into STS. All these ad hoc arrangements and the networking involved temporarily helped consolidate a local STS environment, but in the long run it was impossible to sustain, since institutional barriers within and across faculty structures and lack of substantial funding took its toll. Fragmentation took hold time and again. Momentum continued during the 1990s, but the idea of creating a large STS department did not materialize. STS remained a distributive field.

In Gothenburg where the days of the *postivismusstreit* (and student radicalism) belong to the past one now sees some collaborative projects with colleagues in practical philosophy, for example, around medical ethics and the impact of new technologies in medical science and hospital care, among others in a research program for Studies of Medicine, Expertise, and Controversy (SMEC).[79] Other projects deal with the advent of terrorism and its influence on conditions of research, surveillance practices and images of society,[80] the sociology and politics of fisheries resources in Europe,[81] the history and epistemology of science policy and public understanding of science in Sweden.[82] STS-oriented teaching packages regularly purchased by the faculties of natural sciences and medicine has spun off a Master's program in evidence-basing of decision-making and planning in health care and community services settings. A relatively recent area of research is bibliometric methods currently in use by policymakers and university administrators and their impact on academe.

Overall, the story of the emergence and development of science studies in Sweden is perhaps most interesting for the distinctive trajectory that, early on, enabled scholars in the field perhaps more than elsewhere to learn that some of the problems they were concerned with were fairly similar on both sides of the Iron Curtain. This is borne out in the cases of early studies in Uppsala, Lund, and Gothenburg and particularly confirmed by Stevn Dedjier's career. An important background factor in this regard was the country's position as a neutral state between the two mutually hostile spheres of influence dominated by the USA on the one hand and by the Soviet Union on the other. The official position of neutrality on the geopolitical arena during the Cold War enabled interaction with as well as bridge-building and mediation between the two sides. The international expertise assembled at SIPRI in Stockholm is a distinct example of this and the early establishment of an internationally oriented program for science policy studies in Lund may be seen in the same light.

Swedish political non-alignment was based on a principle of armed neutrality with a leaning toward the West, NATO (covertly) and the OECD (prominently). It was sustained by economic muscles enabled by a contract of class conciliation between capital and labor, a corporativist state with a high class military–industrial complex in which science and technology played an important role. This made it possible to successfully navigate in turbulent waters during the Cold War era. National research policy was founded on two basic principles—to boost the country's economic prowess and to build a strong welfare state. Promotion of science and innovation for the military sector was kept under cover behind closed doors under the auspices of the Ministry of Defense and its think tank FOA, and therefore never open for the kind of critical lens that could scrutinize academic research.

Another point to be made is that Cold War agendas do not translate mechanically into intellectual agendas for science studies at universities. The process is indirect and mediated, constrained and enabled by the mangle of various circumstances and, as I have tried to show, gelled in a complicated mix of micro-politics of academic disciplines and the roles of leading personalities. What happened in Sweden, among other things, was the emergence of the opportunity spaces created for pursuing systems theory, futurology, and science policy studies. How these opportunity spaces came to be utilized depended very much on the resourcefulness of a number of individuals. Contacts with academic colleagues in the Western world, particularly Britain and the USA, were also important, and conceptually the ideologically tinted internalism–externalism "debate" influenced early studies in diverse ways.

The linkages to the Cold War setting both in terms of funding opportunities and intellectual agendas for "Research on Research" also generated tensions and debates under this common heading which—when it was officially high-jacked by the national science policy bureaucracy—was later replaced by the term "science and technology studies." This is evident in the cases of Lund and Gothenburg. At both these sites the student movement also played a role, giving rise to a counter-culture in science policy studies. In the long run, however, this more radical intellectual culture was been marginalized. It runs against the grain of mainstream trends in business and politics in society at large, and it deviated from the micro-sociological turn that internationally became a hallmark of a high church "professionalization" in STS.

With the end of the Cold War the new political drivers began to motivate scholars' agendas. A current tendency is accommodation to the mainstream trend of privatized globalization as Philip Mirowski calls it. This mode of globalization, in a sense, plays a role in the cultural framing of the perspectives and concepts that are considered relevant today in ways that are analogous to what the Cold War regime did for a long time in the previous geopolitical era.[83] At this conjuncture, as once before, the social responsibility of STS—in my view—is not to join the culture of compliance but to go against the stream, to sharpen critical capacities in opposition to facile representations of science, technology, and society.[84]

Notes

1. Aant Elzinga, "Some notes from the past," *EASST Review*, June 1997, available at: easst.net/wp-content/uploads/2012/04/Some-Notes-From-EASSTs-Past.pdf, accessed 19 July 2013.
2. Samuel Lilley, "Social Aspects of the History of Science," *Archives Internationales d' Histoire des Sciences* 2 (1949): 376–443; see also S. Lilley, "Cause and effect in the history of science," *Centaurus* 3 (1953–1954): 58–72.
3. A. Elzinga, "Forskningspolitik i Sverige," in Ronny Ambjörnsson, Gunnar Anderson & Aant Elzinga, *Forskningspolitik i Sverige*, *Sovjet och USA* (Stockholm: Aldus/Bonniers, 1969), 124–211; A. Elzinga, "Science Policy in Sweden, sectorization and adjustment to crisis," *Research Policy* 9/2 (1980): 116–146; A. Elzinga, "Forskningspolitiken och den liberala korporativism," *Sociologisk Forskning* 19/2 (1982): 39–63. The 1969 book was written when its authors spent a month in the library that Stevan Dedjier was building up in Lund.
4. Per Lundin and Niklas Stenlås, "Technology, state initiative and national myths in Cold War Sweden," In *Science for welfare and warfare*, edited by Per Lundin, Niklas Stenlås and Johan Gribbe (Sagamore Beach, MA: Science History Publications/USA), 1–3.
5. CIA, *An Estimate of Swedish Capability in Science* (Washington, DC: CIA/OSI, 1949). I wish to thank Ronald E. Doel for drawing my attention to a copy of this report (the original is in the Harry S. Truman Library).
6. Ibid., 4—emphasis appears in the original.
7. FOA expanded rapidly during the Cold War, starting with 130 employees (among them 55 researchers) in 1945, reaching 280 employees in 1947, 750 in 1955, and over 1000 in in 1958 to peak at 1583 in 1971. This made it by far the biggest research organization in the country, employing many more researchers than any of the universities or private companies, and gives an indication of the relative size and importance of Sweden's own state-led military–industrial complex. See Stenlås, "Military technology, national identity and the state," pp. 61–84.
8. Jozef Goldblatt and David Cox, eds., *Nuclear Weapons Tests. Prohibition or Limitation?* (Oxford: SIPRI, CIIPS, Oxford University Press, 1988).
9. Robert Marc Friedman, *The Politics of Excellence. Behind the Nobel Prize* (New York: Times Books, 2001); A. Elzinga, *Einstein's Nobel Prize. A glimpse behind closed doors, the archival evidence* (Sagamore Beach, MA: Science History Publications, 2006).
10. Cf. Patrick Petitjean, "Introduction. Science, politics, philosophy and history," *Minerva* 46/2 (2008): 175–180; Patrick Petitjean, "The joint establishment of the World Federation of Scientific Workers and of UNESCO after World War II," *Minerva* 46/2 (2008): 247–270; Anja

Skaar Jacobsen, *Physics, Philosophy and Politics in the Twentieth Century* (Singapore and London: World Scientific Publishing, 2012).

11. Torsten Gustafsson et al., *Tidspegel. Aktuella uppsatser om vetenskap och samhälle av tio Lundaprofessorer* (Stockholm: Bonniers, 1942).
12. A. Elzinga, "Bernalism, Comintern and the science of science: critical science movements then and now," in *From research policy to social intelligence. Essays for Steven Dedijer*, edited by Jan Annerstedt and Andrew Jamison (London: Macmillan, 1988), 87–113; on the emergence and early development of the EASST see Elzinga, "Some notes from the past."
13. Steven Shapin, "Discipline and bounding. The history and sociology of science as seen through the externalism.-internalism debate," *History of Science* 30 (1992): 333–369.
14. Steve Fuller, *Thomas Kuhn: A philosophical history of our times* (Chicago, IL: University of Chicago Press, 2000).
15. Cf. Björn Wittrock & Aant Elzinga, eds., *The university research system: the public policies of the home of scientists* (Stockholm: Almqvist & Wiksell, 1985).
16. *New Statesman* 6 October 1956.
17. C.P. [Charles Pierce] Snow, *De två kulturerna* (Uddevala: Bo Cavefors, 1961).
18. Cf. Göran Friborg, *Brain drain and brain gain of Sweden. The findings of four studies by the Committee on Research Economics (FEK)* (Stockholm: Natural Sciences Research Council/NFR, 1972). One of the problems discussed in Europe at the time was the so-called technological gap vis-à-vis the USA, a notion introduced in Jean- Jacques Servan-Schreiber, *The American Challenge* (New York: Atheneum, 1968).
19. Project Hindsight looked back 20 years while TRACES identified chains of scientific events dating back to 1853.
20. Göran Friborg, "Grundforskningens roll i innovationerna," *FoF* 3 (1969): 2–3; G. Friborg, "Forskarnas informationsproblem," *FoF* 6 (1969): 32; G. Friborg, "FoU volymen under 1960-talet," *FoF* 4 (1970): 26–28; G. Friborg, "Utveckling i Sverige av sektoriell forskning," *FoF* 8 (1972): 26–29. The newly introduced state-mandated Contractor–Purchaser principle entered academe and stirred up opposition and heated debates throughout the 1970s.
21. UHÄ = Universitets- och högskoleämbetet.
22. One of the outcomes was a UHÄ-funded project that produced a frequently cited little volume: Jan Bärmark, Aant Elzinga & Göran Wallén, *Nursing care research. The emergence of a new scientific specialty* (Gothenburg: Theory of science department, 1981). A later outcome was: Björn Wittrock & Aant Elzinga, eds., *The university research system: the public policies of the home of scientists* (Stockholm: Almqvist & Wiksell International, 1985), which introduces Elzinga's concept of epistemic drift.

23. Earlier Blume was asked to review the field; cf. Stuart Blume, *Towards a political sociology of science* (New York: Free Press, 1974).
24. See Dick Kasperowski and Fredrik Bragesjö, *Bilda och samverka* (Gothenburg: Institutionen för filosofi, lingvistik och vetenskapsteori, 2011), 45, footnote 6. For a recent analysis of the Mode 2 thesis, see Gregor Schiemann, "An epoch-making change in the development of science? A critique of the epochal-break thesis," in *Science in the context of application*, edited by Martin Carrier and Alfred Nordmann (Dordrecht: Springer Verlag, 2011), 431–453.
25. Elena Aronova, "The Congress for Cultural Freedom, *Minerva*, and the quest for instituting 'Science Studies' in the Age of Cold War," *Minerva* 50/3 (2012): 307–337.
26. Sverker Gustavsson, *Debatten om forskning och samhället* (Stockholm: Almqvist & Wiksell, 1971).
27. Rune Premfors, *Svensk forskningspolitik* (Lund: Studentlitteratur 1986).
28. SCASSS: http://www.scasss.uu.se/.
29. E.g., Sheldon Rothblatt & Björn Wittrock, eds., *The European and American university since 1800. Historical and sociological essays* (Cambridge: Cambridge University Press, 1993).
30. http://www.sts.uu.se/about_Uppsala_STS/?languageId=1
31. At the School of Architecture and the Built Environment: www.kth.se/abe/om-skola/organisation/int/philhist/historia.
32. Dedjier has written a fascinating autobiography, Stevan Dedjier, *My Life of Curiosity and Insights. A Chronicle of the 20th Century* (Lund: Nordic Academic Press, 2009). See also Dedjier's festschrift: Annerstedt and Jamison, eds., *From Research Policy to Social Intelligence*. And Andrew Jamison, *The Making of Green Engineers. Sustainable Development and the Hybrid Imagination* (San Rafael, CA: Morgan & Claypool Publishers, 2003), 11–12.
33. Sverker Gustavsson, *Autonomi och heteronomy* (Uppsala: 1966), ii.
34. Derek de Solla Price's wife was Danish; consequently, he frequently visited Scandinavia.
35. Stevan Dedjier, "Research policy—from romance to reality," in *The Science of Science*, edited by Maurice Goldsmith and Alan MacKay (Hammondsworth: Penguin, 1966), 262–284.
36. Stevan Dedjier, "The science of science: A programme and a plea," *Minerva* 4/4 (1966): 489–504.
37. S. Dedijer, "Measuring the Growth of Science," *Science* 138 (1962): 781–788; S. Dedijer, "Underdeveloped Science in Underdeveloped Countries," *Minerva* 2/1 (1963): 61–81; S. Dedijer, "The Unity of Scientific Policy," *Minerva* 3/1 (1964): 126–129; S. Dedijer, "Migration of Scientists: A World-Wide Phenomenon and Problem," *Nature* 201 (1964): 964–967.

38. S. Dedijer, "The Unity of Scientific Policy," 126; the developments on the Soviet side are discussed in E. Aronova, "The Politics and Contexts of Soviet Science Studies (*Naukovedenie*): Soviet Philosophy of Science at the Crossroads," *Studies in East European Thought* 63/3 (2011): 175–202.
39. Ibid., 127.
40. Ibid., 126.
41. Maria Ossowska and Stanislaw Ossowski, "The Science of Science," *Organon* 1/1 (1936): 1–12 [reprinted in *Minerva* 3/1 (1964): 72–78].
42. AAVV, "The 'Science of Science' symposium," *Minerva* 4/1 (1965): 121–122. See also Linda Lubrano, *Soviet Sociology of Science* (Columbus, OH: American Association for the Advancement of Slavic Studies, 1976), p. 9; and A. Elzinga, "The rise and demise of the International Council for Science Policy Studies (ICSPS) as a Cold War bridging organization," *Minerva* 50/3 (2012): 277–305.
43. The setting, the mood and the conflict with the "stakeholders" is described in Elzinga, "Bernalism, Comintern and the science of science."
44. Taking their cue from Jürgen Habermas's critical theory and the notion of three knowledge constitutive interests, Ambörnsson and Elzinga prepared "The repressive function of science in advanced capitalist society" published in Lars Dencik, ed., *Scientific Research and Politics* (Lund: Studentlitteratur, 1969).
45. Cf. A. Elzinga, "Objectivity and partisanship in science," *Ethnos* 1–4 (1975): 406–427 [available at www.autodidactproject.org/other/aant1.html, accessed 19 July 2013]. See also A. Elzinga, "The growth of science: romantic and technocratic images," in *Progress in Science and its Social Conditions*, edited by Tord H. Ganelius (Oxford: Pergamon Press, 1986).
46. In 1974 Jamison began the journal *Natur och samhälle* (Nature and Society) and 1981–1984 he promoted the Lund-based research program on Technology and Culture; see e.g., Aant Elzinga and Andrew Jamison, *Cultural components in the scientific attitudes to nature: Eastern and Western modes?* (Lund: RPI & Committee for Future Oriented Research, Technology & Culture, 1981). Jamison is now with Aalborg University in Denmark (see http://people.plan.aau.dk/~andy/Academic%20publications.htm, accessed 20 July 2013.
47. May Brodbeck, "The nature and function of philosophy of science," In *Readings in the Philosophy of Science*, edited by Herbert Feigl & May Brodbeck (New York: Appleton-Century-Crofts, 1953), 3–7, on 3.
48. Håkan Törnebohm began his career as a specialist in philosophy of science focusing on Einstein's theories of relativity. He was inspired by the work of Hans Reichenbach whose seminar at the University of California, Los Angeles (UCLA) he attended. After teaching in Gothenburg and Stockholm, Törnebohm was appointed head of the philosophy department at the University of Khartoum (Sudan).

49. A FEK-funded project conducted by Göran Wallén concerned scientific information retrieval systems. See Göran Wallén, "Aktiva informationscentraler," *Forskning och Framsteg*, 6 (1969): 33; G. Wallén, *Kunskapsutveckling och organisation i tvärvetenskapliga grupper*, Göteborg: Rapport från institutionen för vetenskapsteori, 1979; G. Wallén, *Vetenskapsteori och forskningsmetodik*, Lund: Studentlitteratur, 1992.
50. During a ten-year period from 1966 to 1976 two volumes containing proceedings from the colloquia were published as FOA-reports.
51. Gerard Radnitzky, "Toward a theory of research which is neither logical reconstruction nor psychology or sociology of science," *Quality and Quantity*: *International Journal of Methodology* 6/2 (1972): 193–238.
52. A parallel development occurred at the University of California, Berkeley where the philosopher C. West Churchman employed the notion "inquiring systems." See C. West Churchman, *The design of inquiring systems* (New York: Basic Books, 1971).
53. Gerard Radnitzky, Håkan Törnebohm, and Göran Wallén, "Wissenschaftstheorie als forschungswissenschaft," *Journal of General Philosophy of Science* 2/1 (1971): 115–119; Håkan Törnebohm, *Science of Science. Lectures upon invitation by the JRC Euratom Ispra Establishment* (Karlsruhe: Euratom, 1976).
54. E.g. A. Elzinga, *On a research program in early modern physics*, *with special reference to the work of Chr. Huygens* (New York: Humanities Press, 1972). Curiously, although Lakatos sought to immunize the growth of science from its societal context, his own dialectical view resonated with his personal history of participation in the Hungarian movement. See Alex Bandy, *Chocolate and Chess* (*Unlocking Lakatos*) (Budapest: Akadémia Kaidó, 2010), and John Kadavy, "Book Review of *Chocolate and Chess*," *Philosophy of the Social Sciences* 20 (2010): 1–11.
55. Wolf Schäfer, "The finalization debate. A reply to our critics," *Finalization in science*, edited by Gernot Böhme and Wolf Schäfer (Dordrecht: Reidel, 1983), 275–299. The group completed their work at Starnberg's short-lived Max-Planck Institute for the Life Conditions in the Scientific–Technical World headed by Carl Friedrich von Weizsäcker and Jürgen Habermas.
56. The two categories of regulatives identified by the Starnberg group are listed by Tues de Jong, "Het internalisme-externalisme-debat en het werk van de Starnberg-groep," *Kennis en Methode. Tijdeschrift voor wetenschapsfilosofie en methodologie* 3/1 (1979): 142–156. The special issue (edited by Lolla Nauta and Gerard de Vries) is entirely devoted to the internalism–externalism debate.
57. This work was later expanded, appearing as A. Elzinga, *The growth of knowledge*: *Some notes on possible historiographic models* (Gothenburg: University of Gothenburg, Department of Theory of Science, 1972 [expanded and

revised in 1979]). At the neighboring History of Ideas Department, University of Gothenburg, Ronny Ambjörnsson translated into Swedish Hessen's classical paper on the social and economic roots of Newton's Principia. See R. Ambjörnsson, ed., *Idé och klass. Texter kring den kommersiella revolutionens England* (Stockholm: PAN/Norstedts, 1972).

58. Gunnar Andersson, *Vetenskapens frihet och nytta* (Gothenburg: University of Gothenburg, Depatment of Theory of Science, 1975).
59. Cited in Garland Allan and Roy MacLeod, eds., *Science, history and social activism: A tribute to Everett Mendelsohn* (Dordrecht: Reidel, 2001), 13.
60. Elzinga, "Some Notes on the Past." See also Aant Elzinga and Catharina Landström, eds., *Internationalism and Science* (London: Taylor and Graham, 1996).
61. Allan and MacLeod, eds., *Science, history and social activism*, 14.
62. Ronny Ambjörnsson, Aant Elzinga and Anna Törngren, *Tradition och revolution. Huvuddrag i det europeiska tänkandet* (Staffanstorp: Bo Cavefors Bokförlag, 1968).
63. Ronny Ambjörnsson, Gunnar Anderson and Aant Elzinga, *Forskningspolitik i Sverige, Sovjet och USA* (Stockholm: Aldus/Bonniers, 1969).
64. Erik Baark and Aant Elzinga, "Rebirth of the Science of Science in China," *Science of Science* 2/3–4 (1981): 243–259; A. Elzinga, "The Science of Science in China: Report by a Specialist in Science Policy," *Science, Technology, and Human Values* 6/35 (1981): 18–21.
65. Andrew Jamison, ed., *Keeping Science Straight. A critical look at the assessment of science and technology* (Gothenburg: Department of Theory of Science, University of Gothenburg, 1988).
66. Jan Bärmark, ed., *Tio år forskning om forskning* (Göteborg: Inst. för vetenskapsteori, 1978).
67. A. Elzinga, "Science studies in Sweden: a country report," *Social Studies of Science* 10 (1980): 181–214.
68. See my work on epistemic drift: A. Elzinga, "Research, Bureaucracy and the Drift of Epistemic Criteria," in Björn Wittrock and Aant Elzinga, eds. *The University Research System. The Public Policies of the Home of Scientist*s, Stockholm: Almqvist & Wiksell International, 1985.
69. Jan Bärmark, *Forskning om forskning eller konsten att beskriva en elefant*, Stockholm: Natur och kultur, 1984. The allusion to an elephant has to do with the old Indian story of ten blind men who try to describe an elephant, one feeling the animal's trunk, another a leg, a third the tail, and so on, each claiming radically different notions of what they are investigating.
70. Tema-T: http://www.tema.liu.se/tema-t?l=en
71. The Secretariat pursued a different line in futures studies than the more technocratic approach of the Engineering Sciences Academy, which was inspired by Herman Kahn (Rand Corporation). Ideological differences made it impossible to unite the two approaches under one and the same roof.

72. Elzinga, "Some notes from the past."
73. More on this period is outlined in A. Elzinga, "Drömmar om en elefant," *Vi vet något. Festskrift till Jan Bärmark*, edited by Margareta Hallberg (Göteborg: Institutionen för idéhistoria och vetenskapsteori, 2008).
74. See www.sts.gu.se/forskning/teknik–och-vetenskapsstudier–sts-/, accessed 20 July 2013.
75. Thomas Brante and Aant Elzinga, "Towards a theory of scientific controversies," *Science Studies* 2 (1990): 33–46.
76. Ibidem.
77. Cf. Aant Elzinga, Jan Nolin, Rob Pranger and Sune Sunesson, eds., *In science we trust?: moral and political issues of science in society*, Lund: Lund University Press, 1990).
78. Welin is now professor of biotechnology, culture and society at the Institution for Medicine and Health, Linköping University.
79. See www.flov.gu.se/english/research/projects/studies-of-medicine–expertise-and-controversy–smec-/.
80. See Mats Fridlund: www.flov.gu.se/om/personal/mats-fridlund/.
81. Sebastian Linke: www.flov.gu.se/english/about/staff/sebastian-linke/.
82. Dick Kasperowski: www.letstudio.gu.se/members/dick-kasperowski/; Fredrik Bragesjö: http://www.flov.gu.se/english/about/staff/fredrik-bragesjo/.
83. Philip Mirowsky and Esther Mirjam Sent, "The Commercialization of Science and the Response of STS," in *The Handbook of Science and Technology Studies*, edited by Edward J. Hackett et al. (Cambridge, MA: The MIT Press), 635–689.
84. For a critical take on the present situation see A. Elzinga, "Features of the current science policy regime: Viewed in historical perspective," *Science and Public Policy* 39/4 (2012): 416–428.

CHAPTER 10

What does a “National Science” Mean? Science Policy, Politics and Philosophy in Latin America

Federico Vasen

Recent scholarly work on the history of the reflections about twentieth-century science and technology have pointed out that a research agenda, which consolidated in the 1970s under the umbrella of science and technologies studies (STS), has its historical roots in discussions going back to the 1930s. The recent history of the STS is mainly associated with the notion of social construction of knowledge that emerged in radical interpretation of Thomas Kuhn’s work, which was furthered by bringing in sociological and anthropological approaches and methodologies (i.e. Latour and the ethnography of modern laboratory). But this recent work points to another tradition: Aronova and Elzinga, among others, argue that STS were originally studies on science policy.[1] John D. Bernal’s seminal work in the 1930s and then the debates in the journal *Minerva* in the 1960s can be identified as constituting the first stage in understanding the relationship between science, politics, and society, in which key scientists

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and intellectuals such as Alvin Weinberg, Michael Polanyi, and Stephen Toulmin, took part. This preceded further debates on social nature of scientific knowledge, which have taken center stage in later years.

It is important to recall this political dimension in the longer history of science studies before proceeding to the matter of this chapter—the history of the reflections on science and technology in the so-called "peripheral countries" during the Cold War period. Among the philosophers of science and science studies scholars, there is a growing interest in reconsidering the links between philosophy of science and science policy.[2] Yet, these discussions have been mainly centered on the Global North and, mainly, the Western context. By having presented the previous discussion, this essay sheds light on how these issues have been analyzed by the actors on the fringes of global political and science system.

My intention is to portray the distinctive features that these reflections had in Latin America where they were entangled in the local political debates. Although these reflections on science resembled those in the USA, Western Europe, and within the Socialist Bloc, they were also driven by a specific set of issues related to Latin America's positioning in the Cold War, which constituted a space ripe for tensions and competition between the two superpowers. The 1959 socialist revolution in Cuba and guerrilla groups operating in the region on one hand, and the CIA operations in Central America and the US-led *Cóndor Operation* aimed at weakening pro-socialist governments on the other, transformed Latin America into a Cold War battlefield.[3] This political situation set the stage for intellectuals' debates at the time.

The role of science and technology in "development" was one of the issues discussed by the intellectual elite. Yet, these debates were not restricted to science policy in a narrow sense. They included themes that would normally be studied by philosophers of science. This close interrelation between political and philosophical aspects is reminiscent of the "Unity of Science" philosophy of science's movement during the 1920s and 1930s. The de-politicization process that the historian of philosophy George Reisch described in that context was not fully accomplished in Latin America up until the military dictatorships and the ideological persecution of the late 1970s.[4]

This chapter examines the debate on what was termed "national science" and the relationship between national goals and scientific research. In what follows I describe the development of science policy in Latin America from the 1930s on and its relationship with debates on global science policy. Then I focus on the debates on "national science" in

Argentina in the late 1960s and early 1970s, and the ways these debates informed both philosophical and political perspectives. Argentinean intellectuals such as Mario Bunge, Gregorio Klimovsky, and Oscar Varsavsky, among others, contributed to this discussion, mainly through the journal *Ciencia Nueva*.

A Policy for Latin American Science

Similarly to what happened in other regions of the world, the institutionalization of science policy in Latin America occurred after World War II (WWII). Before that, there were some initiatives in the 1930s led by scientists who sought to advance in "pure" scientific research. For instance, in 1933 the Argentinean Association for the Progress of Science (AAPC) was created and its first chairman was Bernardo Houssay. Years later, Houssay received the Nobel Prize in Physiology and Medicine for his research on glucose regulation and the pituitary gland. This Association promoted private funding of science through local philanthropy, mirroring what occurred in the USA during the interwar period.[5] It also advocated establishing private universities in order to boost scientific research activities, especially since public universities focused on the training of professionals and did not engage in scientific research consistently. The project to create new universities failed, but the AAPC did manage to receive funding from international philanthropists like the Rockefeller Foundation. The authorities behind the 1943 military coup and Peron's government that was elected in 1946 did not agree with Houssay's emphasis on freedom of research and he and his associates were excluded from universities. Their research activities were then carried out in private research institutions and funded mostly by the Rockefeller Foundation and local philanthropists. This situation changed after 1955 when a new military coup changed government's perspective on science. Houssay was then appointed as head of the newly created National Research Council in 1958.[6]

In the years between 1945 and late 1950s, science policy was inspired by the ideology derived from the "linear model of innovation," a theoretical framework that resurfaced in the aftermath of the WWII. The idea that improving technological and socioeconomic development first required strong capabilities in basic research was well received by Latin American scientists in need of resources. The linear model also provided a rationale for demanding public support for science, since it acknowledged basic research as too risky an investment for the private sector. As it occurred globally, the 1950s in Latin America represented a time for trust in the power of science.

Its potential for war had been tragically demonstrated in the previous years and it was now time to show its potential for peace and economic development. This "policy for science" in Latin America was sponsored by a new international organization: the United Nations Educational, Scientific, and Cultural Organization (UNESCO). During its first general conference in 1946, a network of regional offices for scientific cooperation in South-East Asia, Middle and Far East, and Latin America was created. The network assisted scientists that were isolated from research centers and searching for updated information. It also helped them to be in contact with colleagues.[7] The UNESCO's policy was shaped by a universalist worldview, promoting science as part of a Kantian cosmopolitan project of global understanding, in tune with the postwar cultural atmosphere.[8]

At the 2nd UNESCO general conference, which took place in Mexico in 1947, its representatives organized a meeting of experts in order to assess how the organization could better contribute to the "advancement of science in the region." Bernardo Houssay chaired the session, and held a follow-up meeting in Montevideo in 1948. Following its recommendations, a Science and Technology Regional Office for Latin America and the Caribbean was established in that city in January 1949. The office would play a very important role in the expansion of science policy studies in the coming years.[9] The first national institutions to promote scientific activities were thus created. In 1950, Argentina established the National Directorate for Scientific and Technological Research (that was re-organized as CONICET in 1958), Brazil created the National Research Council (CNPq), while Mexico set up the National Institute of Scientific Research (INIC).[10]

The Emergence of a Regional Scientific Bureaucracy

By the 1960s, the science policy scene started to change. The trust in the linear model and the *science push* framework weakened while the *national* dimension gained prominence. Universalism yielded to an emphasis on localism and realpolitik. Based on the management of "big science," Weinberg's well-known article "Criteria for scientific choice," published in 1963, showed a shift on science policy perspectives.[11] According to Weinberg, internal criteria were based upon the readiness for exploitation and scientists' competence in the field. On the other hand, external criteria referred to scientific merit (relevance to neighboring areas of science), technological merit (the feasibility of a desired technological aim), and social merit (relevance to human welfare and the values of man). This discussion showed the desirability of criteria for scientific choice that went beyond the assessment of academic peers. Internal criteria were deemed necessary but not a sufficient condition for funding. The resource shortages and the need to prioritize certain research domains introduced an idea of relevance that favored *national* interests over academic communities and the individual scientists' aspirations. The discussion of these issues would later be included in what Weinberg called "trans-science," a realm in which questions can be posed by science but whose answer science cannot provide. The coordination of scientific activities and its linkage to national goals was one of the main issues raised in the debate developed in *Minerva*. Edward Shils, the journal's editor, argued that "every country which has a substantial amount of scientific activity, even many of those which have very little, has something like an empirical science policy or, perhaps it would be more accurate to say, science policies. It is not, however, unjust to say that none has a rational and comprehensive science policy."[12]

International organizations played a key role in promoting the construction of a *rational* science policy at a national level in the developing world. In Latin America, UNESCO and the Organization of American States (OAS) helped in the diffusion of new institutional models for the organization of scientific research. As Finnemore argued, in the late 1950s the political changes inside UNESCO resulted in greater power given to the member states.[13] Thus, in the sciences, UNESCO consulted those countries that sought to consolidate their scientific institutions, as it was the case for Belgium in 1957. However, by the beginning of the 1960s the organization refocused its attention on the developing countries ceasing to work on-demand. Instead, it started an active campaign aimed at convincing the national governments that every country, regardless of their size and economic and scientific development, needed a research bureaucracy. In Latin America, UNESCO sponsored a seminar held in Caracas in 1960 to discuss the organization of scientific research (Houssay, 1960). In its final declaration, known as "Carta de Caracas" it was concluded that "taking into account the benefit they brought to the nations were they exist, the creation of National Councils of Scientific and Technical Research must be encouraged."[14]

By the 1960s, UNESCO's policy was not a mere follow-up on the previous initiatives to promote national research councils as instruments of local scientific communities. The new bureaucracies that were part of the new UNESCO policy for developing countries as different as Zambia, Brazil, and Lebanon, were not regarded as just an instrument of scientific communities. Rather, the new policy sought to integrate these scientific institutions with the objectives of national development planning. These initiatives were in tune with the pro-American stance and the theoretical framework of modernization theory, which stated that a country should go through pre-established stages in order to attain development.[15] Even to the countries with already established scientific institutions, such as Brazil, Argentina, and Mexico, UNESCO's representatives suggested that their councils should adopt new strategies based on research plans. In 1969, Argentina created a new institution, National Council of Science and Technology (CONACYT), to coordinate all research institutions in the country.[16] In Brazil, CNPq changed its name from the former National Research Council to the National Council for Scientific and Technological Development (though keeping its acronym unchanged). In Mexico, a new CONACYT replaced in 1971 the former INIC. Between 1960 and 1970, new scientific institutions were created in other Latin American countries such as Peru, Colombia, Ecuador, Costa Rica, Uruguay, Bolivia, and Venezuela.[17]

The OAS also played an important role in the development of science policy in the region. OAS activities were closely related to the Alliance for Progress, a regional initiative of the Kennedy administration presented in 1961 to help Latin America's development and contain the expansion of communism.[18] Science and technology would play an important part in this initiative as being considered key factors for national developing planning. OAS's main activities included the funding of science policy studies in academic centers and international methodological seminars. It also sponsored a Conference on the Application of Science and Technology to the Development of Latin America (CACTAL) in Brasilia in 1972.[19]

The Intellectuals' Reflections on Science Policy in Latin America in the 1960s

With these developments of the science policies, the critical voices followed, fomenting the beginning of the local tradition in science policy studies. These reflections are usually referred as the Latin American Thought on Science, Technology and Development (PLACTED), which gained in!uence in the late 1960s. Jorge Sabato, Amílcar Herrera, and

Oscar Varsavsky are pointed as the main minds behind the movement.[20] These intellectuals advanced a critical reflection and the reappraisal of the first science policies in the region, which promoted scientific activities, regardless of local knowledge demands. They referred to those policies as "offertist" in that they concentrated on producing ("offering") basic scientific knowledge and neglected the local demands that could have led to research agendas closer to the actual needs.

Against this backdrop, Sabato proposed a model named "IGE triangle." Each of the corners represented an actor needed for technological development: scientific infrastructure (I), government (G), and industrial structure (E). According to his diagnose, Latin American countries had weaknesses in each vertex of the triangle, and lacked articulation between them. He concluded that the interaction between the different sectors should be increased.[21] Sabato also remarked the importance of "technological autonomy" and claimed that Latin American countries should have a leeway to define which technology they want to import and which to produce locally. But "autonomy" must not be confused with autarky or self-sufficiency, which is not desirable—Sabato argued. It is not about producing *every* needed technology locally, which is not always viable. It is rather about having the political, economic, and technical means not to lose decision-making power.[22]

Amílcar Herrera was an Argentinean geologist who, after the 1966 military coup, went into exile in Brazil. At the State University of Campinas, he founded the Department of Science and Technology Policy, one of Latin America's leading academic institutions in the field. He critically discussed the first stage of institutionalization during the 1950s and claimed that the early pioneers of Latin American science did not address the issue of relevance of research to local needs. In his main work, *Science and Politics in Latin America* (1971), he stated that:

> The structure of the current development of science is determined by the directions imposed by the needs of the most advanced countries and not by a sort of "natural law" that inexorably determines the modality of scientific growth. Intending to blindly imitate those models of developments means to become subsidiaries of systems conceived for other necessities and resources.[23]

Imitation does not just imply that a scientific institution would fail to integrate with a local network. It also means that it could effectively be integrated into an international network and contribute to a knowledge production system ruled by foreign interests.

> To contribute to the general advancement of science, Latin American countries have no obligation to obsequiously follow research directions and developed countries' agendas. On the contrary, they can accomplish a much more effective action by picking subjects in accordance to their own needs. That would help the advancement in research areas that might be neglected because main international players lack interest in them.[24]

While globally the 1960s can be characterized by the emergence of a perspective that valued the importance of organizing scientific research attuned to national goals, in Latin America this discussion gained greater attention since it was not just about reorganizing and allocating resources more efficiently. In the periphery of the global science system, it was actually a question of power and colonialism. Research conducted in a developing country was regarded as functional to the research agenda of developed countries. It was then not about moving from a universalist to a local view. Rather, it was about becoming aware that the universalist framework concealed an imperialist relationship of domination. Sabato and others thus advocated a process that enlarged the autonomy of local actors and encouraged freedom from foreign agendas.

Their ideas also related to the development of a Latin American critique of the modernization theory in the social sciences, or dependency theory. According to this framework, Latin American underdevelopment does not constitute a "previous stage" in the path toward development, as Rostow might have argued.[25] On the contrary, it was a structural condition of peripheral economies that are disadvantageously inserted in the international markets as providers of raw materials. This situation was not independent from the position of developed countries but functional to it, since they had taken advantage of those terms of trade. Traditional *desarrollistas* (developmentalist) recipes that did not acknowledge this dependency relationship were regarded as unsuitable. New pathways for a different integration in the world trade system should then be sought.[26] This issue remarkably erupted during the discussion on foreign research funds granted by the Ford Foundation for developing social research. Scholars discussed whether it was possible to carry out research that was critical of capitalism with US funding.[27]

A Spectrum of Critiques: The Debate About a "National Science" in Latin America

By the end of the 1960s, political radicalization was a global issue. The opposition to the Vietnam War, the emergence of a pacifist movement, environmentalism, feminism, and the confirmation of a critical stance inside academia are some main examples of changes occurring in the cultural scene. A more radical agenda around science emerged with authors like Paul Feyerabend that sought to introduce relativism with regard to scientific truth and contributed to the appreciation of other knowledge traditions.[28]

In Latin America, the radicalization processes had specific regional dimensions compared to what happened in other countries. In the case of Argentina, the 1966 military coup deeply impacted the scientific field. A number of researchers and university scholars who had developed material and symbolic conditions for scientific research had to exile both inside the country and abroad. The USA sought to expand its presence through military governments, trained in the "national security doctrine," and claimed that armed forces should be used in the persecution of dissident persons and organizations in their own countries.[29]

US policymakers tried to contain resistance to capitalism that arose in Latin America as part of the "expansion wave" of the 1959 Cuban Revolution in countries like Nicaragua and Chile, which could mean the enlargement of Soviet influence in the region. The discussion about science and society in Latin America cannot be isolated from this political context, in which anti-US imperialism and the role of developing countries in the global political system were central to the agenda. Political, economic, and cultural liberation from dependence became one of the main objectives for progressive intellectuals. Issues broadly related to "quality of life," like environmentalism, derived from the radicalization in the developed countries, were not, although often mentioned, a priority in the politically contentious Latin America.[30]

The debate on science and politics started as a reaction to, and a critique of, "offertist" policies advanced by research councils. It gained a bolder geopolitical localization by the beginning of the 1970s. The universal status of research agendas was deeply criticized and a controversy about the concept of "national science" developed. Is it possible to have a "national science?" What does this mean? Is this about science dealing with local problems using universal methodologies? Or should a national science also criticize the methods and propose specific methodologies for every national reality? Analyzing the concept of national science added a philosophical dimension to the previous political discussion about research agendas, since it was philosophy of science the discipline that dealt before with the rationale of research methodology.

Different stances regarding the link between the science's methods and economic and cultural dependence were thus formulated. The central texts in this discussion are taken from the book *Ciencia e ideología. Aportes polémicos* that contains articles originally published in the early 1970s in *Ciencia Nueva*.[31] The journal published regularly between 1970 and 1973 and worked as the voice for organizations like the *Grupo de trabajadores de la ciencia* [Science workers group] that held a leftist view on science, politics, and society. At the same time, the journal achieved to receive the radical perspectives from developed countries through the publication of Spanish translations of articles from the US radical journal *Science for the people*.[32]

Assuming the risk of losing the specificity of each author, I have ordered these stances in three groups: (a) universalist perspective that prefers to avoid local issues and agendas, (b) recognition of the importance of local agendas but defense of universal methods and, (c) intention to reform scientific methodology according to historical–political conditions.

A Universal Science

When it comes to identify different positions in the debate about a national science, one should begin by describing one of the extremes: universal science. This position denies that local characteristics shape scientific activities. Accordingly, there cannot be something like a "national science." On the contrary, science is one, and should be practiced with the same methods, standards, and agendas worldwide. Thematic interests are determined by mankind's common search for truth and the enlargement of the endless knowledge frontier, and not by local characteristics or demands. These assertions echo Polanyi's "Republic of science" in which scientists are represented as citizens of one unique global republic of researchers.[33] As I mentioned, during the first stage of the institutionalization of science policy in the 1950s, this was the dominant ideology in Latin America. Later, even with a change of conceptions within organizations like UNESCO, scientific communities kept this ideological framework alive through and their participation in research councils. Houssay, the leading representative of this universalist stance, remained influential in the region until his death in 1971. Mario Bunge, an Argentinean philosopher of science based now at McGill's University (Montreal, Canada), also shared a similar perspective when he claimed that "science is universal, or it is not science but folklore (…) the requirement to restrict research to native issues results in a dramatically lower research quality, since the objective of science is to find knowledge of the general, not to describe idiosyncrasies."[34]

From a philosophical perspective, this universalist view was fully compatible with the logical-positivist consensus in philosophy of science. What singles out science is its method, and it is universal. Local characteristics may arise in the context of discovery but they should be clearly absent in the context of justification that deals with the relationships between theory and empirical data.[35] From a political perspective, the linear model of innovation and the emphasis on universal science might also be regarded as a form of cultural containment of communism. Individual scientists acting in a framework of "freedom of inquiry" and "free enterprise" were more akin to the image of capitalism than planning and priority setting in science.[36]

A Science With "National Traits"

Universal science represented one of the poles within the range of possibilities in the 1970s debate about science and nation. A second choice was a position that can be described as science with "national traits." For supporters of this position, science keeps working with a universal method, but engages too with an agenda on local issues. The importance of science in dealing with local problems is then recognized, but solutions only follow from a universal method.

Many intellectuals shared this point of view. Amílcar Herrera, to whom I referred earlier, remarked that:

> The idea of scientific development oriented by national needs should not be confused with the absurd conception of a 'nationalistic' science in the narrow sense of this expression, which deals only with local problems and is more or less isolated from the international context. The methods and the aim of science are effectively universal, and the continuous exchange and a close connection with the world science system is the only guarantee of a quality level that suits modern scientific work. There cannot be a 'Latin American' science. What might exist and should exist is a science whose orientation and general objectives are in line with the need to solve the multiple problems that the development of the region demands.[37]

This text clearly shows the distinction between the two dimensions mentioned earlier: one related to the research agendas, legitimately interwoven with local interests, and a second one related to research methods that are regarded as universal. In addition, Herrera highlighted the importance of not losing the connection to the international scientific system, since it is the sole guarantee of local production's quality. In a similar vein, some years earlier, Mario Bunge stated that:

> The election of problems of national interest should be stimulated but it should also be insisted that they are treated in an international level. It would be absurd to miss the chance to measure cosmic rays in Chacaltaya, to do biology of tropical areas in the Amazon, or to study Motilon indigenous people in Venezuela. National characteristics should receive special attention, both for the enrichment of universal knowledge and for their possible utilization. But all object or typical problem must be treated with the method and the universal aim of science. (…) In sum, a science with national traits, yes; nationalistic science, no.[38]

A discussion about time dedicated to local issues and the importance of global agendas also emerged. But the general stance maintained that science should not be completely taken by local issues and it should keep some room for basic science in a classical fashion. Bunge, for example, declared that "whoever claims to restrict scientific activity of some area to the study of its typical features and forgetting the universal, is claiming actually the return to previous centuries, when autonomous disciplines within science existed. This provincialism is a thing from the past."[39] Gregorio Klimovsky, an Argentinean mathematician and philosopher of science argued that:

> If I am asked about the social responsibility of scientists (…) I think the ideal could be represented by somebody who dedicates 50 % of his time to basic research but invests the other 50 % in the gathering of information related to the national problems that exist, how they have been resolved before and what could be done to frame them in new ways.[40]

Many authors agreed in that this "science with national traits" should not only be involved with local problems but also deal with universal issues, as a way of not losing contact with the standards of science in developed countries. But some authors like Klimovsky went further in their justification of the importance of basic science. He believed that there could also be a political (anti-imperialist) justification for basic research. Reflecting on the trajectory of the Department of Computer Science at the University of Buenos Aires he commented that:

> Some companies took over the studies of scientific computing, changing the contents of the degree. It was no longer aimed to educate applied mathematicians at a very high level, not only in computing but also in all fields of numeric calculus. It became a career that just trained a person in some techniques of programming, since that is the only knowledge of interest for those companies. Undoubtedly, they are not going to promote the teaching of certain things they keep for their headquarters and not for the colony they think we are.[41]

In this quote, the "geopolitical" perspective appears as part of the criteria for making choices about research agendas. Klimovsky's argument was used to support the claim that a focus on basic knowledge can be more politically liberating. Local demands might be too narrow and not require higher technical or scientific skills but only applied and low level knowledge. In this context, the development of basic research contributed to Latin American autonomy and liberation, even if it is not directly linked to local demands. Transnational companies to which Klimovsky referred to could indeed keep advanced research activities near headquarters and only transfer to third world countries simple technical needs. It was then necessary to promote research in its highest depth in Latin America and not just be restricted to applied research, which even if it was close to some local demands, might in the long run create even more dependence.

In another passage, Klimovsky insists in the same argument of Herrera and Bunge about national science:

> Regarding so-called "national science" I think it is useful to make a distinction. If by it we refer to special methods to design research, test hypothesis or infer conclusions from premises, methods that relate to our idiosyncrasy and national spirit, then this idea is absurd. Not only absurd, it is also dangerous, as Hitler's ambitions for German science. But if "national science" means awareness about our problems, the study of techniques that might solve them, the detection of hypothesis and theories that might help us, then this idea matches the characterization of the tasks a scientist must perform in our time.[42]

As the quote demonstrates, the philosopher Hans Reichenbach's distinction between the "context of justification" and the "context of discovery" was clearly accepted by Klimovsky. In his view, the only national element that might be present in science relates to the local selection of research problems according to national priorities in a vein similar to that of Alvin Weinberg's external criteria for scientific choice. By contrast, there can be no local or national distinctive feature when we consider the methods used for testing hypothesis—which would fall within Reichenbach's "context of justification." Klimovsky expressed this idea using this same terms: "I do not find ideological aspects that affect objectivity of knowledge, from the perspective of the context of justification."[43] He was in fact one of the scholars who introduced logical-positivism in Argentina, together with Mario Bunge.[44] Moreover, he was interested, especially in light of the wrongdoings of Nazi science, in defning national science as aligned to a strategy for Latin American liberation and autonomy. In a similar vein, Jorge Sabato, in his introduction to a volume about science, technology, development, and dependence in Latin America, maintained that the discussion about national science was like dealing with "old problems in new words." He referred to Johannes Stark's *Nationalsozialismus und Wissenschaft* (1934): "Jews have particularly helped in the diffusion of the idea that science is international. No, science is not international, it is as national as art."[45] Another polemist, Thomas Moro Simpson also linked this discussion with the Lysenko affair and the protection of theories based on patriotism and nationalism.

The positions of intellectuals like Herrera, Sabato, and Klimovsky about the issue of national science can then be summarized as follows. They held that the methods and the agenda of scientific research could be considered separately. Regarding methods (falling within the realm of the "context of justification") there can be no characteristics that emerge from geopolitical situations. Nevertheless, local needs and knowledge demands can—and should—influence the selection of research topics. This does not mean that basic research should be neglected. On the contrary, it is the key to autonomy and development. Although the national needs are important in shaping the research agenda, a radical stance should be avoided since it resembles Nazi science and the Lysenko affair.

National Science and Revolutionary Projects

Oscar Varsavsky's figure gained prominence as the most radical position in the debate on national science. Varsavsky obtained his PhD for his work on the foundations of quantum statistics and held a professorship at the Department of Mathematics at the University of Buenos Aires. In the early 1960s, he was one of the most radical critics of the *desarrollista* (developmentalist) position. *Desarrollistas* were those who willingly accepted foreign research funds and thought that science in a peripheral country should reach developed nations’ level of achievement with the same standards. To accomplish that, *desarrollistas* supported the education of scientific graduates in traditional American and European universities, and insisted on their return to Argentina to share the knowledge gained during their stay abroad. Varsavsky objected to this usual practice within the scientific community. He claimed that this practice strengthened the cultural dependence and was a proxy for the reproduction of research agendas unrelated to local realities. He also distrusted the ideological commitments embedded in foreign funding. In the late 1960s the Camelot Project, a social research study funded by the US Army on domestic armed conflicts that was going to be implemented as a pilot in Chile, had caused great unease in the academic community.[46] Scholars and researchers perceived the study as similar to previous cases at the University of Buenos Aires when research grants had been made available by the Ford Foundation.

After the 1966 military coup in Argentina, Varsavsky was exiled in Venezuela, from where he started to reflect about his previous experience and developed his critique of *desarrollismo*. The *locus classicus* for this critique was his book *Ciencia*, *política y cientificismo* (“Science, Politics and Scientism”), published in Buenos Aires in 1969. He had discussed some of his ideas before in a short article published in the *American Behavioral Scientist*.[47] Varsavsky claimed that the only way to break the colonial links that the *desarrollismo* (also referred as “scientism”) position builds was through a radical change in how science is done. He developed the notion of *styles* of scientific development. In his book *Hacia una política científica nacional* (“Towards a national science policy”), he distinguishes between three scientific styles: neocolonialism, developmentalism, and creative socialism. Neocolonialism implies that less developed countries would support basic science even though it was “useless” for technological application and social and industrial development.[48] Varsavsky believed the pursuit of this type of research as equating to colonialism insofar as the agenda for basic research is disguised in less developed countries and rooted in the needs of developed countries. Only their epistemic objectives are explicitly mentioned. In other words, to put it simply, it is colonial because it is “sold” to underdeveloped as basic research whereas in fact it has an agenda that is being disguised and invisibly shaped by the needs and interests of the companies and governments of developed countries. In contrast, *desarrollismo* recognizes the importance of science for development, but takes the USA as social model and praises the postwar industrial development based on technological innovation. But “its hidden premise is that men and society will remain essentially as in current developed countries and the possibility of a New Man in a new society is not even mentioned.”[49] The third option, creative socialism, states that current science is more value-laden than what the *desarrollistas* acknowledge. It includes some pro-capitalist agendas (e.g. social psychology for advertising) and excludes other more revolutionary ones (as research about social transformation and the New Man).[50] In this respect, he points to the need to develop social sciences under a revolutionary perspective. It is in the social sciences where the main breakthroughs still have to be made. He also critiqued the technological development in the Socialist bloc, claiming that the USSR had achieved great advances in natural science and engineering (the Sputnik, the elimination of hunger and epidemics) but “many of us still have doubts that the Soviet man, now in its second generation, is indeed ‘new’.”[51]

If neocolonialism represents in general terms a universalist vision of science, the *desarrollista* approach recognizes the importance of science for development but does not make a broader critique of society or the internal structure of science, as alluded earlier. For Varsavsky, who endorsed a Marxist radical viewpoint, science should become part of a project of revolutionary political change. He critiqued the perspective of Klimovsky and Herrera, since they still maintained that science is a universal tool that could be used for different political objectives. In his view, science should be completely transformed for revolutionary purposes. The economic, political, and cultural requirements of socialism in developing countries demand a new scientific style that can be termed as a “national science.”[52] In contrast with the views of the authors previously discussed, this style does not only refer to agenda-setting issues. It includes also considerations about the context of justification:

> Scientists now accept without great questioning the ideological uses of science, which were dramatically demonstrated by the atomic bomb. They reluctantly also accept that the selection of research proposals and subjects can be determined by extra scientific considerations (…) but by no means

> they can accept that a doubt be cast on the neutrality of "scientific method", of truth criteria, of everything that in "empiriologic" language is called "context of justification", as different from the contexts of discovery and application. We will see however, that with a closer look to the elements and components of this scientific method many different possibilities immediately appear for each one, all of which are not easily adapted to all styles of development.[53]

In open opposition with Bunge and Klimovsky, Varsavsky claimed that the scientific method can have variability according to the development style of a given society. The standards for hypothesis validation will not be the same for a neocolonial, developmental, or socialist style. Varsavsky's critique was mainly directed to the social sciences. The methodology used in those disciplines carries out biased assumptions and does not promote social change, whereas social sciences able to serve a socialist style make use of a methodology fitting that objective. "I am not saying that the methods usually used are bad for everything. They are bad to deal with the problem of social transformation. Neither am I maintaining that they necessarily bring about false results. The point is that the methods might not be the most adequate and that their utilization delays the results."[54] Criticizing the social sciences' methodology, he pointed out, for instance, to the uncritical use of statistics, the reliance on "trendy" theories (game theory, cybernetics) and the simplistic application of mathematics and topology to the understanding of society. According to Varsavsky, society is more complex and these frameworks fail to capture its complexity. His proposal was not to deny the importance of quantitative methods. He proposed instead to redefine them. Until his death in 1976 he worked in "numerical experimentation" methods to help the planning of a socialist society.

In addition, Varsavsky focused on another issue, which was a key to his conception of justification of scientific theories. According to the logical-positivist view, the acceptance or rejection of a theory or hypothesis should be based in the analysis of empirical data and its logical relations to theory. Varsavsky, on the contrary, maintained that this decision should be made on the basis of three criteria: "importance, ethical value and credibility (a more sincere word than 'truth' or 'probability'), which is only possible if the hypothesis is integrated with others in a system, which is immersed in an environment."[55] Rather than separating the contexts, as Klimovsky did, Varsavsky intended to join the philosophical epistemic dimension with ethics and politics. In his holistic system, a hypothesis could not be assessed separately from its context; its credibility was always related to its importance and ethical value.

Finally, Varsavsky confronted Klimovsky's emphasis on basic science as a means to achieve political autonomy. He boldly rejected "recreational" basic research: "The definition of a technological policy is previous to any consideration of science policy: the latter depends on the former. Only after deciding—in general terms—the style and technological strategy, it is possible to judge the functionality of scientific research and assign priorities."[56] The definition of a style of society or a "national project" (as he calls it in further works) happens first, and then, based in that decision, it will follow the adoption of a suitable technological and scientific style.

Philosophy and Science Policy: Concluding Remarks

The debate about national science has many relevant aspects. On the one hand, it draws attention to the role of political drivers in the first wave of the reflections on science, technology, and society in the twentieth century. It also shows a thematic confluence of the issues discussed in the same years in the USA and Western Europe; for instance, the questions of the priorities for scientific research and the role of public funding in it. The feature that distinguished the Latin American debate was the emphasis on the regional situation and the analysis of the impact of imperialist practices and "imitative behaviors" on science funding and research communities. In the intellectual scene of the Cold War, discussions about science and politics were interwoven with the conflict between the capitalist and the socialist blocs. Latin America was deeply entrenched in the global power dispute between the USA and the USSR. While the former funded and supported pro-American military regimes, the latter supported a guerrilla war through Cuban influence. Within this framework, for someone like Varsavsky, "national" science that would take part in the liberation of Latin America from economic and cultural dependency was inseparably linked to the vision of a revolutionary political change. The transformation he envisioned was deemed to overcome the shortcomings of the Soviet model as it advocated the idea of a "new man"; an anthropologic shift that did not take place in the Soviet Union. Other authors like Klimovsky and Herrera did not see the need of a revolution, or even a philosophical reform, as they believed it possible to reform science and include developmental instances without compromising its universality.

The debates examined in this chapter also had influence on the actual policies that were developed in Latin America. At the university level, during the early 1960s in Buenos Aires most professors in the research schools adopted the approach I termed "a science with the national traits." Importance was given to the improvement of material conditions for research (more full-time professorships, scholarship for postgraduates, funding for equipment) and also social commitment and political involvement with national issues was praised and encouraged through popular education and university outreach.[57] More radical approaches such as Varsavsky's emerged within the context of the discussion of foreign funding for academic research by the Ford and Rockefeller Foundations.[58] Varsavsky helped to increase awareness about the ideological load of some of the research policy proposals, targeting mainly the social sciences. Till today, Varsavsky's figure is an icon of anti-imperialist science studies in the region and he is quoted as an inspirer of socialist science policies by Hugo Chávez's administration in Venezuela.[59]

Moreover, technical accomplishments can be seen as a consequence of reflections on autonomy and anti-imperialism. The local development of a research reactor and the inclusion of local suppliers for the construction of the first nuclear power plant in Argentina could not have been possible without a strong political commitment.[60] However, it should be also noted that the changing political landscape in Latin America during those years prevents from making a global analysis. The military and civil administrations that held office had different views on science and technology that were specific for each country and technological field.

Besides the political issues that this discussion raises, the inclusion of the philosophical dimension is particularly interesting. It is not usual in the field of science policy even nowadays to discuss research priorities and political uses of research along with the nature of scientific method. On this, I believe that the Latin American debate was ahead of its time. Today only a few philosophy of science would accept that non-epistemic values play a role in the context of justification.[61] In fact, although it may share some holistic framework with Kuhn, Varsavsky's view is closer to this current research agenda than to the radical views of the philosophers of his time (like Feyerabend). His opponents discuss with him and defend the objectivity of the context of justification; they attack him as if he were a representative of irrationalism or relativism. But his critics did not hit the target since Varsavsky was not contesting the whole building of science and its rationality. He maintained the need to recognize variability in the context of justification and reconsider how we assess empirical evidence. So his philosophical perspective on science was not as radical as his political views. While he claimed that Latin American societies needed a deep political (anthropological even) transformation, that in the realm of science should not be so deep.

Summing up, the Latin American discussions of national science give an important insight on the larger context of the political and philosophical debates in the late 1960s and early 1970s, providing an interesting comparative dimension to our current research agendas. These discussions show that the first wave of the reflections about science and technology was driven by political and geopolitical issues. These political concerns had set the stage for the intellectuals' philosophical analyses. With the professionalization of science policy along with the apolitical drift in philosophy of science these connections between philosophical analyses of science and the political concerns became less and less tangible. Yet, as this chapter shows, the philosophical debates were started as the response to the ongoing debate on the role of science in the development of Latin American nations.

Notes

1. Elena Aronova, "The Congress for Cultural Freedom, Minerva, and the Quest for Instituting 'Science Studies' in the Age of Cold War," *Minerva* 50 (2012): 307–337; Aant Elzinga, "The Rise and Demise of the International Council for Science Policy Studies (ICSPS) as a Cold War Bridging Organization," *Minerva* 50 (2012): 277–305.
2. See Philip Kitcher, *Science, Truth and Democracy* (New York and Oxford: Oxford University Press, 2001); P. Kitcher, *Science in a Democratic Society* (New York: Prometheus Books, 2011); Heather E. Douglas, *Science, Policy and the Value-Free Ideal* (Pittsburgh: University of Pittsburgh Press, 2009).
3. Gilbert Joseph and Daniela Spenser, eds., *In from the Cold: Latin America's New Encounter with the Cold War* (Durham: Duke University Press, 2008).
4. George Reisch, *How the Cold War transformed Philosophy of Science* (New York: Cambridge University Press, 2005).
5. Robert E. Kohler, "Science, Foundations and American Universities in the 1920s," *Osiris* 3 (1987): 135–164.
6. Diego Hurtado and A. Busala, *Ideales de la universidad científica. Elitismo y función social de la ciencia, 1931–1959* (Buenos Aires: Libros del Rojas,

2003); Diego Hurtado and María José Fernández, "Institutos privados de investigación pura versus políticas públicas de ciencia y tecnología en Argentina (1943–1955)," *Asclepio. Revista de Historia de la Medicina y de la Ciencia* 65/1 (2013): 1–17.

7. Adriana Barreiro and Amílcar Davyt, *Cincuenta años de la Oficina Regional de Ciencia y Tecnología para América Latina y el Caribe de la Organización de las Naciones Unidas para la Educación, la Ciencia y la Cultura (ORCYT/UNESCO) Un análisis histórico de la cooperación en la región* (Montevideo: UNESCO, 1999).
8. Martha Finnemore, "International Organizations as Teachers of Norms. The United Nations Educational, Scientific, and Cultural Organization and Science Policy," *International Organization* 47 (1993): 565–597. See also Perrin Selcer, "Patterns of Science. Developing Knowledge for a World Community at UNESCO," PhD dissertation, University of Pennsylvania, 2011.
9. Barreiro and Davyt, *Cincuenta años de la Oficina Regional de Ciencia y Tecnología*, pp.26–30.
10. Óscar Gustavo Retana Guiascón, "La institucionalización de investigación científica en México," *Revista UNAM Ciencias* 94 (2009): 46–51. See also Diego Hurtado, *La ciencia argentina: un proyecto inconcluso* (Buenos Aires: Edhasa, 2011); Rafael de Brito Dias, *Sessenta anos de política científica e tecnológica no Brasil* (Campinas: Editora da UNICAMP, 2012).
11. Alvin Weinberg, "Criteria for scientific choice", Minerva I (1962), 158–171. Aronova and Elzinga; Aronova, "The Congress for Cultural Freedom, Minerva, and the Quest for Instituting 'Science Studies' in the Age of Cold War," 307–337; Aant Elzinga, "Features of the current science policy regime: Viewed in historical perspective," *Science and Public Policy* 39 (2012): 416–428.
12. Edward Shils, ed., *Criteria for Scientific Development. Public Policy and National Goals* (Cambridge, MA: MIT Press, 1968), x.
13. Finnemore, "International Organizations as Teachers of Norms. The United Nations Educational, Scientific,and Cutural Organization and Science Policy," 565–597. This does not mean that cosmopolitanism and universalist ideology disappeared. A group of international experts, members of an elite that shared norms and values, did emerge. But the "world community" did not become the popular and vivid representation of unity of mankind that was originally envisioned. See Selcer, *Patterns of Science. Developing Knowledge for a World Community at UNESCO*, 415–420.
14. Quoted in Barreiro and Davyt, *Cincuenta años de la Oficina Regional de Ciencia y Tecnología*, 46.
15. Walt Whitman (W.W.) Rostow, *The Stages of Economic Growth. A non-communist Manifesto* (Cambridge: Cambridge University Press, 1960).

Selcer points out that it cannot be sustained that UNESCO did itself become an instrument of American foreign policy. American foundations, such as Ford and Rockefeller fulfilled that purpose more explicitly. P. Selcer, "The View from Nowhere. Disciplining Diversity in post-World War 2 International Social Science," *Journal of the History of the Behavioral Sciences* 45/4 (2009): 309–429.

16. Adriana Feld, "Planificar, gestionar, investigar. Debates y conflictos en la creación del CONACYT y la SECONACYT (1966–1969)," *Eä Journal* 2/2 (2010): 1–43.
17. Barreiro and Davyt, *Cincuenta años de la Oficina Regional de Ciencia y Tecnología*, 50.
18. Michael Latham, "Ideology, Social Science and Destiny: Modernization and the Kennedy-era Alliance for Progress," *Diplomatic History* 22/2 (1998): 199–229. See also Diana Marcela Rojas, "La Alianza para el Progreso en Colombia," *Análisis Político* 23/70 (2010): 91–124.
19. A. Feld, "Las primeras reflexiones sobre la ciencia y la tecnología en la Argentina (1968–1973)," *Redes* 17/32 (2011): 185–221.
20. R. Dagnino, H. Thomas, and A. Davyt, "El Pensamiento Latinoamericano en Ciencia, Tecnología y Sociedad. Una interpretación política de su trayectoria," *Redes* 3/7 (1996): 13–51. See also O. Galante, M. Marí, R. Carnota, F. Vasen, and O. Benso "La Escuela Latinoamericana de Pensamiento en Ciencia, Tecnología y Desarrollo: proyecto de recuperación histórica y documental," Paper presented at the 13th *Encuentro ALTEC*, Cartagena de Indias (Colombia), 2009.
21. Jorge A. Sabato, and N. Botana "La ciencia y la tecnología en el futuro de América Latina," in *El pensamiento Latinoamericano en la problemática ciencia-tecnología-desarrollo-dependencia*, edited by J.A. Sabato (Buenos Aires: Biblioteca Nacional, 2011 [1975]), 215–234.
22. This distancing from autarky could be showing a critical appraisal of traditional anti-imperialist approaches like those of Franco's Spain or Fascist Italy (see Norton Wise and Tiago Saraiva, "Autarky/Autarchy: Genetics, Food Production, and the Building of Fascism," *Historical Studies in the Natural Sciences* 40/4 (2010): 419–428). Since the reflection is taken from a text of 1982, it could also point to a retrospective critique of the more radical positions that also emerged during the 1960s in Latin America. Jorge A. Sabato, "Reflexiones sobre ciencia y tecnología," *Informe Industrial* 70 (1983): 14–17.
23. Amílcar O. Herrera, *Ciencia y política en América Latina* (Buenos Aires: Siglo XXI, 1971), 72 [my translation].
24. Ibid., 97.
25. Rostow, *The Stages of Economic Growth. A non-communist Manifesto*, 4–16.

26. Fernando H. Cardoso and Enzo Faletto, *Dependency and Development in Latin America* (Berkeley: University of California Press, 1979 [1968]).
27. Silvia Sigal, *Intelectuales y poder en Argentina. La década del sesenta* (Buenos Aires: Siglo XXI, 2002); see also María Elina Estébanez, "La modernización en Exactas: los subsidios de la Fundación Ford durante los años '60." in *La construcción de la ciencia académica. Instituciones, procesos y actores en la universidad argentina del siglo XX*, edited by Carlos A. Prego and Oscar Vallejos (Buenos Aires: Biblos, 2010), 253–268.
28. On Feyerabend's radicalism, see the chapter written by Ian Kidd in this volume.
29. J. Patrice McSherry, "Tracking the origins of a state terror network," *Latin American Perspectives* 29: 1 (2002): 38–60.
30. These issues were not completely absent from the pages of the journal *Ciencia Nueva*. Feld and Kreimer point out that environmental discussion did have its place (Adriana Feld and Pablo Kreimer, "La science en débat en Amérique Latine. Perspectives « radicales » au début des années 1970 en Argentine," *Revue d'anthropologie des connaissances* 6 (2012): 29–58). It can also be reminded that the Latin American World Model developed by Herrera et al. in 1976. Amílcar O. Herrera et al., *Catastrophe or New Society? A Latin American World Model* (Ottawa: IDRC, 1976) openly contested environmental arguments put forward in *Limits to Growth*. See Dennis L. Meadows et al., *The Limits to Growth. A report for the Club of Rome's Project on the Predicament of Mankind* (New York: Universe Books, 1971). But environmental issues remained marginal to the political debate on science.
31. Gregorio Klimovsky et al., "*Ciencia e ideología. Aportes polémicos*," Buenos Aires: Ciencia Nueva, 1975.
32. Feld and Kreimer, "La science en débat en Amérique Latine. Perspectives « radicales » au début des années 1970 en Argentine," 29–58.
33. Michael Polanyi, "The Republic of Science: its Political and Economic Theory," in *Criteria for Scientific Development. Public Policy and National Goals*, edited by E. Shils (Cambridge, MA: MIT Press, 1968 [1962]), 1–20.
34. Mario Bunge, "Filosofía de la investigación científica de los países en desarrollo," in *El pensamiento Latinoamericano en la problemática ciencia-tecnología-desarrollo-dependencia*, edited by J. A. Sabato (Buenos Aires: Biblioteca Nacional, 1975), 75–85, on 78. [my translation].
35. The context of discovery relates to the origin and construction of scientific hypothesis. In contrast, the context of justification deals with the process of acceptance or rejection of hypothesis based on logical reasoning and empirical data. According to the traditional philosophical point of view, while the discovery process can be influenced by non-epistemic values, justification remains value-free. See Paul Hoyningen-Huene, "Context of Discovery and Context of Justification," *Studies in the History*

and Philosophy of Science 18 (1982): 501–515; Jutta Schickore and Friedrich Steinle, eds., *Revisiting Discovery and Justification*: *Historical and Philosophical Perspectives on the Context Distinction* (Dordrecht: Springer, 2006).

36. Naomi Oreskes, "Science, Technology and Free Enterprise," *Centaurus* 52 (2010): 297–310.
37. Herrera, *Ciencia y política en América Latina*, 97. [my translation].
38. Bunge, "Filosofía de la investigación científica de los países en desarrollo," 83. Bunge can be situated in an intermediary position between the universalist stance and this second perspective. He acknowledges the existence of a local agenda, but denies the existence of a relationship of "domination" of the peripheral science by the center. He also distrusts an excessive localism [my translation].
39. Ibid., 83.
40. Gregorio Klimovsky, "Ciencia e ideología," in *Ciencia e ideología*. *Aportes polémicos*, edited by G. Klimovsky et al. (Buenos Aires: Ciencia Nueva, 1975), 11–37, on 35. [my translation].
41. Ibid., 30.
42. Ibid., 37.
43. Ibid., 28.
44. Alberto Cordero, "Philosophy of Science," in *A Companion to Latin American Philosophy*, edited by Susana Nuccetelli, Ofelia Schutte, and Otávio O. Bueno (London: Wiley-Blackwell, 2010), 370–382.
45. J. Sabato, "Nota introductoria a la sección 'ciencia e ideología'," in *El pensamiento Latinoamericano en la problemática ciencia-tecnología-desarrollo-dependencia*, edited by J. Sabato (Buenos Aires: Biblioteca Nacional, 2011[1975]), 33–37, on 34. [my translation].
46. George E. Lowe, "The Camelot Affair," *Bulletin of the Atomic Scientists* 22/5 (1966): 44–48. See also Marc Solovey, "Project Camelot and the 1960s Epistemological Revolution Rethinking the Politics-patronage-social Science Nexus," *Social Studies of Science* 31/2 (2001): 171–206.
47. Oscar Varsavsky, "Colonialism in the Hard Sciences," *American Behavioral Scientist* 10 (1967): 22–23; O. Varsavsky, *Ciencia*, *política y cientificismo* (Buenos Aires: Centro Editor de América Latina, 1969).
48. O. Varsavsky, *Hacia una política científica nacional* (Buenos Aires: Periferia, 1972).
49. Ibid., 46.
50. The idea of the New Man is part of Ernesto Che Guevara's revolutionary humanism, who claimed that it was necessary to build a new revolutionary subjectivity, deprived from all individualism. Michael Löwy, *The Marxism of Che Guevara*. *Philosophy*, *Economics and Revolutionary Warfare* (Lanham: Rowman & Littlefield, 1973).

51. O. Varsavsky, *Hacia una política científica nacional*, 50. [my translation].
52. O. Varsavsky, “Bases para una política de ciencia y tecnología,” in *Ciencia e ideología. Aportes Polémicos*, edited by G. Klimovsky et al. (Buenos Aires: Ciencia Nueva, 1975), 52–57. [my translation].
53. Varsavsky, *Hacia una política científica nacional*, 59. [my translation].
54. O. Varsavsky, “Ideología y verdad,” in *Ciencia e ideología. Aportes Polémicos*, edited by G. Klimovsky et al. (Buenos Aires: Ciencia Nueva, 1975), 41–51. [my translation].
55. Ibid., 44.
56. Varsavsky, “Bases para una política de ciencia y tecnología,” 56. [my translation].
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CHAPTER 11

From Dialectics of Nature to STS: The Historical Evolution of Science Studies in China

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What is known as "science studies" in China would have been identified, at least up until the 1980s, with the "dialectics of nature." The field was established in China during post-World War II (WWII) social and political transformations and coincided with the establishment of the communist regime. The new discipline was christened after Friedrich Engels's unfinished book under the same title first published in 1883. However, the Chinese version of dialectics of nature encompassed a broader range of topics than those discussed in the Marxist theorist's work.[1] The dialectics of nature was originally imported in China from Soviet Russia, where in its early incarnation as ideological and philosophical approach it had already occupied an important space within the Soviet intellectual landscape and functioned as a political wedge in the definition of new science policies.[2] In China, it was introduced in the late 1930s, attracting the interest of a number of scholars and occupying the niche in between natural sciences

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and philosophy. It was promoted and further developed by Mao Tse-Tung who played an instrumental role in the proliferation of the works drawing on Marxist–Leninist analyses, and in making dialectics of nature part of academic curricula.

Importantly for the theme of this volume, dialectics of nature played a decisive role as the ideological harness for China's scientists during the Cold War, shaping approaches to the meta-discussions of science and its role in society. It also aimed to reach beyond the scientific community, to inform the activities of the other members of Chinese society.

While the Chinese communist administration recognized the importance of developing science and technology to advance national security and state building, the intellectuals were identified as a class of "petty bourgeois" who needed to be transformed by labor practice and the learning of Marxism. During the first ten years of China's communist-led regime, 1954 Chinese students and scientists returned to the Far Eastern country. Sixty-four percent of them came back from the USA, including the physicist Hsue-Shen Tsien who went on to lead China's missile program.[3] These "returnees" represented a treasured scientific manpower, but the government harbored suspicion, especially because of their training in Western countries. Teaching them Marxism, and especially the *Dialectics of Nature*, meant to yield their allegiance to the regime. The Chinese case thus allows reappraising recent and less recent studies on Cold War science showing that while the global Cold War was about using patronage to direct scientists toward the completion of new equipment such as advanced weaponry and delivery systems, it was also about finding ways to secure their political allegiance and compliance to state-sponsored ideological precepts.[4]

Considering the merits and shortcomings of the dialectics of nature is also decisive for deepening our understanding of the ways the studies of science resurfaced as a new bourgeoning field during the Cold War. Even more so as non-Western contexts, and especially China, have been largely overlooked.[5] The Chinese version of "science studies"—the dialectic of nature—presents a historical trajectory of the field that differs drastically from that of the Western science and technology studies (STS) tradition. This article aims therefore at shedding light on its significance and shortcomings while examining the content and the context in which this discipline emerged and thrived.

While the chapter's main focus is on the relationship between science and politics in China, as well as the ways in which the Cold War informed the development of dialectics of nature, it also documents the downfall of dialectics of nature at the end of the Cold War. The political reforms and the opening up of China to Western cultural influences since the 1980s led to the deep transformations of the dialectics of nature. In the last decade of the Cold War, Chinese "science studies" scholars argued for the need of reconsidering ideological impacts on their studies and introduce more freedom in moving across the constructed boundaries of their discipline. With the Cold War's end, they embraced the Westernized philosophy of science and technology. While in the West the constructivist approaches were attacked during the "science wars" of the 1990s, in China these approaches found a welcoming community of scholars, replacing the space that dialectics of nature occupied in the previous 50 years.[6]

The Discussion of Science and its Role in Society in China Before World War II

The search for philosophical approaches that could help to better understand the development of science and technology derived primarily from the effort of finding ways of modernizing China—a country threatened by its aggressive neighbors and enslaved by European colonial powers. In the nineteenth century, the foreign missionaries in China had helped to divulge and popularize key scientific works published in Europe. However, at the beginning of the twentieth century, the political turmoil prevented the organization of scientific societies and scientific activities.[7] The "May Fourth" era, which takes its name from the 1919 demonstrations against China's treatment at the Versailles conference ending World War I (WWI), was a transitional period in modern Chinese history when the collapse of the imperial state, the prominence of Confucian ideology, and the massive influx of Western ideas spawned competing visions of a reunified Chinese polity. The shaky alliance between the nationalists and the communists consolidated the project and the vision of a modern Chinese nation. Yet, there was a lack of consensus on its political, social, and cultural drivers.

In these circumstances, a new generation of scholars and progressives in China endorsed the Western ideals of democracy, understood as a form of enlightened government and political thought, and as a scientific methodology aligned to this worldview.[8] Democracy and Science (德先生与赛先生) were the flagships of the cultural movement seeking to modernize China that influenced both professional and scientific communities as well as the wider public. Chinese scholars began to acquaint themselves with Western literature. Among other doctrines, Marxism–Leninism occupied an important space in their sets of readings. Scholars close to the Chinese communist party, established in 1921, were particularly receptive to the cultural influences from Soviet Russia welcoming Marxist approaches.[9] In particular, scholarly and activists' journals such as *New Youth* (新青年) elaborated the theory of materialism in the natural sciences, and explored its facets in politics and culture. Marxism–Leninism formed the core of the set of theories guiding the political revolution, and it was in this context that the dialectics of nature was first introduced into China.

In the 1920s, Chinese left-leaning intellectuals began to translate the works of Karl Marx and Friedrich Engels, including the chapters from *Dialectics of Nature* and Marx's articles on natural philosophy.[10] Engel's unfinished book was written in an effort to conceive a theory of nature consistent with Marx's principles of dialectical materialism, and thus it sought to forge a synthesis between the analysis of historical and natural processes. The book drew on a variety of examples in physics, chemistry, biology, and history aiming to demonstrate that the very same principles can be successfully applied to both natural and social domains.

Engels's unfinished book was published in the Soviet Union in 1925. Three years later, a chapter of the book, "Labour in the Transition from Ape to Man," was translated into Chinese by the scholar Lu Yuanyi. Shortly thereafter, the entire book was translated and published under the same title (自然辨证法) by Du Weizhi, a Trotskyite who played a significant role in spreading Marx's theories on the philosophy of nature and science in China.[11] Vladimir I. Lenin's *Materialism and Empirio-Criticism* was also translated around the same time.[12]

These translations forged the beginning of the debate on the merits of dialectics of nature in China. The communist scholars argued that Engels's precepts could find successful application in the process of the transformation of China from an agricultural nation to an industrialized power. This view was not shared by the Chinese nationalists, however. And in 1927, their leader, Chiang Kai-Shek, ended the alliance with the communists, paving the way for the organization of the independent Red Army and, from 1933, its military retreat from nationalist forces (the Long March).

The civil war marked the watershed in the cultural debate on the drivers of China's modernization. Many scholars who had received their education in the Western countries now intended to contribute to this debate, drawing on their experiences abroad. Those who had spent time in Russia were particularly eager to propagandize the dialectics of nature as a philosophical approach and an ideology. Its spreading was especially prominent in Shanghai, China's largest city and port where the civil war had polarized the debate. Not coincidentally, the first center specializing in dialectics of nature was established in Shanghai.

In 1937, China entered into a new conflict with imperial Japan, which ended the civil war and restored the alliance between communists and nationalists. Shanghai was now attacked by the Japanese troops and, as a result, the scholarly activities had to move to other Chinese cities. Major research centers for dialectics of nature were established in Yan'an (延安) and Chongqing (Yan'an was near the end point of the Long March).

In Yan'an, the dialectics of nature flourished partly due to the interest of the leader of the Chinese communist party. Mao Tse-Tung published several articles, such as "On Practice," "On Contradiction," "On materialism," "On movement," and "On space-time," which quoted Engels's book.[13] These publications were instrumental in connecting the advancement of science in China to the application of dialectics of nature principles. As he stressed in his talk in 1940, "natural science is one of man's weapons in his fight for freedom. For the purpose of attaining freedom in society, man must use social science to understand and change society and carry out social revolution. For the purpose of attaining freedom in the world of nature, man must use natural science to understand, conquer and change nature and thus attain freedom from nature."[14] Drawing on Engels's teaching, he indicated that "the transformation of nature by natural sciences has to be guided by the social sciences."[15] Mao's teaching and personal involvement was decisive in promoting the discipline, which received funding and support from the party.

Yu Guangyuan and the Consolidation of Dialectics of Nature

The Chinese economist Yu Guangyuan was the most important patron of the dialectic of nature in China. Graduated from Physics of Tsinghua University, Guangyuan established a research school devoted to the dialectics of nature and promoted the new discipline throughout his life. Since 1936, he joined the Study Society of Natural Science, a secret communist organization in Shanghai which not only united the progressives from the scientists to promote anti-Japanese war but also organize discussion about Marxist new philosophy. His activities within the society were closely linked to the cultural reforms instigated by the Communist Party of China, of which Guangyuan was an active member.[16] Along with Mao, Guangyuan believed that the dialectics of nature could guide Chinese scientists in developing new methods of researching and practicing scientific activities. Drawing on Engels's and Mao's analyses of the relationship between social and natural forces, Guangyuan sought to develop new ways of thinking and resolving the problems associated with the development of a national industry and the economic transition in China in this period.

In the late 1930s, several workshops were organized in Yan'an to discuss the dialectics of nature. Following these discussions, in 1940, the Society for the Research of the Natural Science (延安自然科学院) was established.[17] While the majority of its members were scientists, the society also included philosophers and social scientists interested in the natural sciences. Yu Guangyuan was active member in the society and he prepared a new translation of Engels's *Dialectics of Nature* using the German edition, which was eventually adopted by the society as a textbook.

In 1943, in the midst of WWII, the Chinese communist party had decided to put an end to the alliance with the nationalists. The breakup had immediate consequences for the dialectics of nature. While Mao agreed to set up a research group focused on the subject, in Yan'an, the teaching of the dialectics of nature was discontinued despite the fact that Guangyuan and Mao's teacher Xu Teli continued to promote the new discipline. Both regarded Engels's doctrine as the resource to counter nationalist views on Western science, corrupted, as they saw it, by the capitalist ideology. Thus, they maintained that nationalism in China would only allow foreign industrial concerns to set foot in China while preventing its own development. While recognizing the ideological nature of capitalist approaches to science, Yu Guangyuan and Xu Teli were at the same time oblivious to the ideological nature of Engels's precepts, understanding them as uncorrupted, *true*, science.

That said, the dialectics of nature helped to reinforce a view that science was a means to overcome the underdevelopment in China. It also provided the opportunity for debating the foundations of science.[18] At the same time, Chinese scholars also became acquainted with Western studies on science and society, which cast a critical perspective on its development—most prominently Bernal's *The Social Function of Science*.[19]

All in all, the political, scientific, and intellectual communities that drove the Chinese revolution forward were well-versed in the dialectic of nature and interested in applying this approach to both science and society. The Chinese scientists and scholars reappraised the Western discussion of the philosophical foundations of science, and they also considered social issues associated with its advance. To be sure, the dialectics of nature movement was closely linked to broader cultural sentiments: the aspiration for the liberation from Japanese invaders, corrupted nationalist leaders, and European colonial powers by modernizing China and departing from feudal culture. At the end of the revolutionary process, the new ideology became deeply engrained in the science and technology institutions in China.

On October 1, 1949, the establishment of the People's Republic of China marked the end of the two decades of unrest in the Far Eastern country. The new regime opened new possibilities for dialectics of nature, which was officially endorsed as a discipline. These developments coincided with the beginning of the Cold War, opening a complex geopolitical game of alliances and divisions spanning across the globe.

The Cold War and the Politics of Dialectics of Nature: Building a Discipline

With the onset of the Cold War, the pursuit of knowledge became a matter of states' concern on both sides of the Iron Curtain.[20] In China, the emphasis of sponsoring novel scientific research did not derive entirely from the political confrontation with USA and Soviet Union, but also by the need of building and stabilizing a new country following the world conflict and the civil war. The new imperative for Chinese leaders was no longer "saving the country through science" (科学救国) in the spirit of the May Fourth movement, but "building the nation through science" (科学兴国). Mao advocated self-reliant science, and focused on application of science, which was of obvious practical importance for China as an impoverished developing country.[21] The same urgencies created the circumstances for the state and party support of the dialectics of nature, forging the new developments in the period from 1949 to 1966.[22]

In the context of this new political agenda, the dialectics of nature represented an innovative interpretative approach for the philosophical reflection on scientific method and a tool for the modernization of the country. But it also became a device in the hands of the Chinese state administrators for the management and control of the scientists. Dialectics of nature came to be seen as a harness to align scientists' activities with the leading ideology and the needs of the communist party.

Young Chinese scientists read Marx and Mao's classics, while Engels's book was singled out as one of "the twelve compulsory books of the party cadres."[23] In 1951, Mao's earlier articles "On Practice" (实践论) and "On Contradiction" (矛盾论) were reprinted in order to instruct scientists on the merits of dialectics of nature. Meanwhile, Mao's own doctrines were propagated in academic disputes to assert the superiority of Chinese approaches. For instance, the mathematician Hua Luogeng published a review of Mao's papers in the *Chinese Science Bulletin*, in which he drew on Mao's work criticizing the dogmatism and empiricism in Western mathematics.[24] The development of Chinese dialectics of nature also included the adoption of a quantitative statistical analysis of large scientific trends and science developments—taking into account criteria such as class and the social origins of scientists. Only the proletariat science, which typified the communist society, was portrayed as good science.[25]

Mao's "On Practice" played an important role in cultivating the tendency to censor approaches to science alternative to those embracing the dialectics of nature. The first part of this paper emphasized that genuine, "transformative," knowledge could only be developed from practice, while in the second part Mao stressed that the theories must serve practice and get tested by practice.[26] Practice thus became the chief criterion in the search for truth, and in distinguishing between alternative scientific approaches favored by the bourgeois and proletariat scientists. The emphasis on practice, however, downplayed the importance of theoretical knowledge planting the seeds of anti-intellectualism in China.[27]

In the meantime, the building of the political alliance between China and the Soviet Union, especially in the period of Sino-Soviet cooperation (1952–1956), was accompanied by the wholesale importation of cultural approaches and methods. In this period, the Chinese Party's Central Committee introduced the program *Learning from the Soviet Union*, which entailed acquiring educational materials from Russia and receiving training under the guidance of the Soviet experts. This period saw the rise of translations from Russian into Chinese. Not only theoretical works on dialectical materialism were translated but also a number of propagandistic studies discussing the role of science and technology in the construction of a socialist state. Thus, the hybridization techniques of the Ukrainian agronomist Trofim Lysenko were endorsed by Chinese agricultural experts, while Mendelian genetics was rejected as idealistic and reactionary.[28]

Mao's dialectical interpretation on the nature of elemental particles informed the study of the universe and Chinese approaches to physics. Mao enjoyed having conversations with atomic scientists about the nature of matter. According to physicist Qian Sanjiang, in January 1955 Mao presided one of their meetings and used the dialectics of nature to explain that matter was infinitely divisible. That was the reason, he argued, for the recent discovery of many new types of sub-atomic particles:

> So long as we have people and resources, all sorts of miracles can be performed [...] An atom is a unity of opposites, the proton and the neutrons. One divides into two—this is a universal phenomenon. Protons, neutrons, as well as electrons are also divisible. Now this has not been proved by scientific experiment; yet under advanced experimental conditions, it will be proved. Chairman Mao added with a smile: Do you believe this? If you don't, anyway I do.[29]

As Edward Friedman has shown, Mao read the translated articles of Schoichi Sakata the Japanese physicist who was the first to develop a new model for the interaction of sub-atomic particles and whose work had been informed by Engels's *Dialectics of Nature*. In 1963, Sakata discussed his research in an article published in the Chinese *Journal of Dialectics of Nature*.[30] Mao went on to further affirm a dialectical views of sub-atomic particle and invited some of the Chinese leading philosophers of science, Zhou Peiyuan and Yu Guangyuan, to discuss the implication of Sakata's work.[31] Mao's philosophy also stimulated and influenced the studies of the Elemental Particles Research Group (北京基本粒子理论组) established in Beijing in 1965. Its members contributed to elaborate the "straton model" which resembled the quark model elaborated independently by Murray Gell-Mann and George Zweig in 1964 and was eventually embraced by the international community.[32]

But it was not only the scientific community that embraced the dialectics of nature. Mao believed that the key tenets of this philosophical approach should inform the society at large. Farmers and researchers thus collaborated in the "Eight-Point Charter for Agriculture"—a number of dialectics-based techniques for increasing the farming output. Armed with Mao's "little red book" of quotations, they pioneered novel terracing techniques and hybridized strains of rice in what was intended as a new type of "scientific" farming.[33] In medicine, doctors discussed the combination of traditional Chinese medicine and Western medicine, and the dialectics of treatment for patients.[34] Factory workers carried out research on the dialectical laws of technological development. According to Gong Yuzhi, workers, cadres, and technologists employed for the construction of China's oil field, in Daqing, benefitted from the reading of Mao's "On Practice" and "On Contradiction." As Yu Guangyuan stressed in 1964, dialectics of nature was meant to building an alliance between philosophers, workers, farmers, scientists, and engineers.[35]

But in the aftermath of the death of Josef Stalin, Chinese scientists reconsidered the merits of dialectics approaches. A brief period of liberalization of Chinese society, which started in 1956 and was heralded by the *Letting Hundred Flowers Bloom*, *Letting Hundred Schools of Thought* campaign (百花齐放,百家争鸣), was marked by the promotion of liberal values in both the sciences and the arts.[36] Yu Guangyuan, who organized a nationwide academic conference in Qingdao, now openly criticized the Ukrainian agronomist Trofim Lysenko who had been responsible for designing some of the methods deployed in Chinese agriculture. Guangyuan now forewarned about the risks to be derived from letting Chinese politicians decide about scientific matters:

> To carry out the policy of "letting a hundred schools of thought contend," the Communist Party has decided not to make any resolutions on academic issues. Let the scientists themselves discuss these issues. Even scientific institutions should be very cautious in drawing conclusions, let alone the Communist Party. The party should lead academic and guarantee academic development, but it would be better not to draw conclusions about academic issues.[37]

About the relationship between philosophy and science, he pointed out:

> We cannot solve scientific problems by philosophical inference. Philosophy studies the laws of thinking, not the laws by which nature and society develop […] Philosophy is not omnipotent. If it can provide scientists some help in ideological method, then it plays its appropriate role.[38]

Other scholars in the dialectics of nature played an active role in this reappraisal of Soviet methods. For instance, Gong Yuzhi of Peking University, who was Mao's adviser and the president of the Chinese Society for Dialectics of Nature, published a paper in which he argued that philosophical approaches could help in solving scientific problems but only in so far as the scientists were left free to consider their merits: "the task of philosophy isn't to block or limit science, but to provide a wider vision for the science research."[39]

But on the whole the *Letting Hundred Flowers Bloom*, *Letting Hundred Schools of Thought* movement produced two, seemingly antithetical, results. On the one hand, it disenfranchised Chinese dialectics of nature studies from Soviet influences. On the other hand, it further regimented Chinese scholars to make dialectics of nature the main interpretative framework for the study of science. In 1956, Mao and his government issued "Marching on Science" (向科学进军), a program of scientific development encapsulated in the Twelve year Long-term Science and Technology Development Plan. Designed as the blueprint for China's future science and technology investment, it drew heavily on dialectics of nature. Yu Guangyuan, who was appointed a vice-director of the draft panel that prepared the plan, suggested reforming not only natural sciences but also the social sciences in Chinese academia. He advocated the introduction of dialectics of nature as a separate discipline. The Chinese Premier Zhou En-lai endorsed these suggestions. The dialectics of nature was inaugurated as an independent discipline. It comprised nine main subjects: (1) the dialectical materialist approach to the basic concepts of mathematics and the natural sciences; (2) the scientific methodology; (3) the movement in nature and the classification of sciences; (4) the historical development of mathematics and the natural sciences; (5) the critique of idealism in mathematics and the natural sciences; (6) the philosophical problems of mathematics; (7) the philosophical problems of physics, chemistry, and astronomy; (8) the philosophical problems of biology and psychology; (9) the natural science as a social phenomenon.[40]

Following the approval of the plan, a new education program started. In 1956, the Department of Philosophy at Peking University enrolled six postgraduate students to be tutored by philosophy and science teachers to form a new dialectics of nature research unit.[41] Although the research group was dissolved two years later, several other universities established similar training programs. In the same year, the *Journal of Dialectics of Nature* was established. For several decades, the journal was the most important scholarly publication venue focusing on meta-discussions of science. And in the 1960s, the journal functioned as a forum for the debates on science, its methods, and its policies.

Yet, even though by the late 1950s the Chinese dialectics of nature came of age and helped to forge an alliance between Chinese scientists and scholars, the implementation of the 12-Year Plan proved to be more difficult. Moreover, while the *Letting Hundred Flowers Bloom*, *Letting Hundred Schools of Thought* movement mitigated the communist regime tendency to censor Western science, after the campaign ended the dialectics of nature's scholars turned once again to discussing its limitations, in an effort to further unite Chinese practitioners and highlight the novelty of their approaches.

The transformation of the dialectics of nature from its original function as a methodological device into a tool for party propaganda was also the outcome of the growing demand for allegiance that the Chinese political leaders sought to obtain from their scientists in order to align the scientific community with national security needs. By the time the 12-Year Plan was approved, Mao had given his consent to the plans for developing the atomic weapons program. The first successful test of China's nuclear bomb took place in 1964.[42] Three years later, Mao and Zhou En-lai announced the beginning of the space program, recognizing the importance of giving to communist China a leading edge in science. In this period, the dialectics of nature, as an ideological harness, complemented what Evan Feigenbaum has characterized as China's techno-nationalism: the construction of a nation-state through the funding of its R&D structures with the shared understanding that these structures would play a decisive role in modernizing the country.[43] The resurgence of the anti-Western sentiment and the effort to align the dialectics of nature with pro-Chinese ideologies echoed Mao's effort to reinvigorate the communist regime through the Cultural Revolution. Emphasizing the need to purge the bourgeois elements that infiltrated the Chinese society and to restore a system in which the true communist ideology prevailed, he and other Chinese leaders turned their attention to dialectics of nature as a means to "cleanse" the cultural debate on science.

In the context of the Cold War, due to its competition for science and armaments, scientific manpower constituted a strategic asset. Dialectics of nature attained a renewed importance as a harness for the scientists that aligned their thinking to Chinese precepts, especially when the Cultural Revolution was in full swing. While before 1956 the dialectics of nature lacked the institutional dimension and political backers, after the 12-Year Plan and Mao's fight against "revisionism," the discipline, its publications, and training programs were by and large shaped by the national security imperatives and the need of Chinese administrators to build the consensus among Chinese scientists.

Beyond the Cold War—From Dialectics of Nature to STS

Mao's death in 1976 marked the end of the Cultural Revolution and the beginning of a transitional period in which the sciences continued to thrive under a regime. This marked a significant change for the Chinese dialectics of nature too, as the existing censorship of Western scientific thought was no longer considered viable and, albeit slowly, more works were introduced casting a new light on the advancement of science in both the East and the West. This paved the way to the introduction in China, during the 1980s, of the new wave of Western analyses on science emerging under the science studies umbrella.

This significant shift in programs and goals was endorsed by Mao's successor, Deng Xiaoping, who also personally approved the establishment of a new Chinese Society for Dialectics of Nature. The society was formally founded in 1981 with Yu Guangyuan as chairman, and other figures within the Chinese Academy of Sciences, also including the leaders of its atomic and space programs, as members.[44] The society also published its own journal, *Studies in Dialectics of Nature*, which helped to take forward the reformation that Xiaoping advocated. It was again Yu Guangyuan, appointed in 1975 as the head of the Political Research Office of the State Council and the first director of the Economic Research Institute of the State Planning Commission, who was among the authors of the speech delivered by Deng at the 1978 Plenum of the Communist Party's 11th Central Committee. In this speech, Deng advocated greater openness in Chinese society and a move toward market economy, marking a watershed in the history of modern China.[45]

The third Chinese Science and Technology Development Plan (1978–1985), in line with Deng and Yu's ambition, stressed the new set of priorities for dialectics of nature scholars. They were now tasked with researching more on the history of science and technology, finding solutions to the practical problems related to the role of dialectics of nature in Chinese science, and starting to translate Western books and articles offering new solutions and interpretations pertaining to science policy.[46]

Meanwhile, the polemic article "Practice is the sole criterion for testing truth," appeared on the pages of the *Guangming*. While the author of the article was indicated as Nanjing University's scholar Hu Fuming, it was actually written by a number of scholars and party leaders including the incoming Communist party chairman Hu Yaobang (who was close to Guangyuan). The article emphasized that the lack of freedom had prevented China from competing with other, more developed nations. The dialectics of nature scholars organized a seminar on the themes raised in Fuming's essay and while their conclusions represented a compromise between the critics and advocates of Mao's earlier claims, they stated that freedom of thought was essential for the progress of science.

Shortly after, however, Deng was ousted out of power by the Gang of Four and the reformist project mothballed. Dialectics of nature thus continued to play a role as a harness even in this transitional period. Despite the end of Cultural Revolution, Mao's designated successor Hua Guofeng put forward what was labeled as the "two whatevers" principle—namely that whatever Mao had said was correct and whatever instruction he had given, ought to be implemented. This entailed the further advancement of classic dialectics of nature studies and the consolidation of the discipline as a key resource for the formation of Chinese scientists.

In the early 1980s, the Chinese Ministry of Education issued a new directive indicating that all universities should provide dialectics of nature courses to the students majoring in science and technology. These courses could be elective for the undergraduates, but were made compulsory for postgraduates. Since there was a lack of teachers trained in dialectics of nature, new master and doctoral programs were also set up in the universities across the country. As a result, many students who majored in natural sciences switched to dialectics of nature. For instance, one of the current leading scholars in this field, Li Zhengfeng, learned chemistry as an undergraduate, before pursuing a master degree in dialectics of nature and eventually starting a career as a specialist in dialectic of nature studies.

Many new textbooks were also published that standardized the content of Chinese dialectics of nature studies across the universities and consolidated their role in the education system. The content of these publications became more prosaic, and increasingly unimaginative. Replicating Engels's theory and Mao's analyses, they now encapsulated three universal laws deemed to be essential in constructing a unified picture of natural processes. These were the law of the transformation of quantitative changes into qualitative changes (qualitative leaps result from accumulated quantitative changes), that of unity and contradiction of opposites (all entities are a combination of oppositions), and that of negation (each stage of development contains within itself the seed of its own destruction and replacement).

At the same time, the end of the Cultural Revolution helped to stir the dissent on these philosophical tenets and unleashed an important debate on their validity within the Chinese scientific community. With Deng's return to power, and consistently with his and Guangyuan's earlier recommendations, more Western works on science were published and debated by the Chinese dialectics of nature scholars. The discussed works included Western studies on the philosophy of science and technology, and the new studies on the sociology of science. Although David Bohm's review of Kuhn's *Structure* was translated in Chinese already in 1965, only in 1980 did the whole volume appear in Chinese.[47] In 1981, Jin Wulun (who in

2003 translated Kuhn's book for the fourth time), organized a seminar on the theory of scientific revolutions. Kuhn's views were praised even though the theory itself was rejected as unable to explain the dynamics of scientific progress.[48] In the early 1980s, other important works by Western philosophers such as Popper, Lakatos, and Feyerabend, were translated in Chinese and examined by dialectics of nature scholars.[49]

The introduction of Western literature was a decisive factor in shaking the traditional teaching education system founded on dialectics of nature. During the 1980s, finding a convincing balance between the teaching and researching of traditional Marxist philosophical approaches and the new Western views on science had become increasingly more difficult. The gap between competing versions of "science studies" had become wider and the introduction of the Western version of science studies stirred a conflict on the trajectory of dialectics of nature as a discipline.

Leading Chinese scholars devoted to this discipline now agreed to debate on the nature and key points of disagreement between competing approaches. They argued that while both dialectics and modern philosophical studies were founded on solid logic principles, Western studies provided less normative basis. H. Lyman Miller has pointed out that some of the scholars involved in this debate, such as Fang Lizhi, Xu Liangying, and Dong Guangbi, eventually opted for the "new" studies on science, while Zha Ruqiang and He Zuoxiu, among others, remained loyal to Marxism.[50]

Behind the debate on the trajectory of dialectics of nature was also a dispute on the future of the discipline and more generally on the level of autonomy of Chinese scientists and intellectuals, something that became dramatically apparent during the 1989 Tiananmen Square student protest (as Wang has noticed).[51] Interestingly, Guangyuan was reprimanded after the crackdown that followed the protest as he showed to be among the few leaders sympathizing with the students.[52]

The argument on the true foundations of science virtually ended in 1990 when the Ministry of Education released a new catalogue of academic disciplines. In the new catalogue, as a second-tier discipline of philosophy, dialectics of nature was now replaced with "philosophy of science and technology (dialectics of nature)." From 1997, dialectics of nature no longer featured, not even within brackets. Always in the 1990s, a variety of new theories of sociology of science and technology were introduced in China including the Sociology of Scientific Knowledge and Actor Network Theory. Constructivist approaches to the study of science became popular and, as a consequence, education centers devoted to dialectics of nature had to adjust. For instance, the Department of Dialectics of Nature at Tsinghua University changed its name to Center of Science, Technology and Society. Also, the last three annual meetings of the Society of Dialectics of Nature of China were entitled, respectively, "Heading for STS," "Approaching STS," and "Into STS". STS became a new umbrella term for research on science policy, sociology of science, philosophy of science and technology.

The last 30 years have thus been typified by a massive shift defining the reorganization of dialectics of nature as part of STS (broadly construed). That said, however, prominent dialectics of nature scholars continued to be involved in key science and technology policy decisions in China, creating a somewhat paradoxical situation. The massive project on the Three Gorges Dam, the assessment on the merits of traditional Chinese medicine, and the analysis on how to build an indigenous innovation nation (just to mention a few examples) have all involved researchers in this field. The close relationship between dialectics of nature scholars and policy-makers thus continued, creating a unique synergy whose implications are, to these days, still difficult to grasp.

Conclusion: Dialectical Roles of Dialectics of Nature

In today's China, with its fastest raising economy, science is recognized as granting spectacular successes in the process of explaining the natural world, and, through technology, in the making of it. Especially after the beginning of the period of opening up to Western capitalism, the old communist precept that "science and technology are the primary productive forces" (科学技术是第一生产力) for a nation's prosperity is, seemingly, widely accepted. Science has now acquired a vital position in the development of an independent Chinese economy.

Yet, today the communist legacy of China's appreciation of science seems to have vanished, while—as this chapter has shown—it was actually a decisive factor in the construction of the Chinese nation through the teaching of Engels's and Mao's theories. Dialectics of nature was pivotal in forging this appreciation from the 1920s onward, and constituted a formative element in the political processes that led to China's revolution and the emancipation from foreign powers. In the ideology of class struggle, scientists were presumed to be a reactionary class, especially in the early years of the Chinese communist party. Dialectics of nature helped to create a coherent set of principles to further advance science in China and unite its scholars.

But, following the laws of dialectics of nature, we can conclude that there are always two sides to any issue. While dialectics of nature practitioners such as Yu Guangyuan were inspired by the vision of modernizing the country, their discipline became a tool for their government to control and manage China's growing scientific community. The need of modernizing and rapidly creating an efficient system of scientific production regimented the education of Chinese scientists and catered for the creation of the comprehensive framework for organizing and uniting these scientists under a unified set of ideological principles. Dialectics of nature lost its innovative character as an intellectual project and was transformed into a set of norms used to reinforce rhetorically the existence of the key differences in the pursuit of science in the West and in China.

But was China's dialectics of nature only a derivative ideology associated with Soviet socialism? This paper shows that the answer is clearly no. Under different names, the study of science emerged as an academic field and a distinct area of professional expertise in its own right before and during the Cold War years in a number of states on both sides of the Iron Curtain. The promotion of the studies of science as a politically relevant area of expertise, undertaken within existing powerful institutional structures outside academia, helped to legitimize the disciplinary identity of science studies in the age of the Cold War.[53] There is thus no reason to believe that the Chinese dialectics of nature had less merit of Western approaches or was less important. Actually, analyzing the historical evolution of dialectics of nature in China helps us to broaden our understanding of the history of science studies. It legitimately extends the analysis of similarities and differences between different countries in constructing normative frameworks for the analysis of science, given its mounting importance in twentieth-century nation-building, and reveals their political and social determinants.

Notes

1. Friedrich Engels, *Dialectics of nature* (London: Wellred, 2012 [1883]).
2. On dialectics of nature in Soviet Russia see David Joravsky, *Soviet Marxism and Natural Science*: *1917–1932* (New York: Columbia University Press, 1961); Joseph M. Bocheński, Nicholas Sollohub, and Thomas J. Blakeley, *Soviet Russian Dialectical Materialism* (Dordrecht: Reidel, 1963).

3. Zuoyue Wang, "The Cold War and the reshaping of transnational science in China," in *Science and Technology in the Global Cold War*, edited by Naomi Oreskes and John Krige (Cambridge, MA: MIT Press, 2014), 343–370.
4. An overview on the historiographical debate on Cold War science is in N. Oreskes, "Science in the Origins of the Cold War," in *Science and Technology in the Global Cold War*, edited by Oreskes and Krige, 11–30.
5. As noted in Zuoyue Wang, "Science and State in Modern China," *Isis* 98 (2007): 558–570. On the history of science in China see also: H. Lyman Miller, *Science and Dissent in Post-Mao China*: *The Politics of Knowledge* (Seattle: University of Washington Press, 1996); Chujuan Nancy Wei and Darryl E. Brock, eds., *Mr. Science and Chairman Mao's Cultural Revolution*: *Science and Technology in Modern China* (Plymouth: Lexington, 2013); Dong Guangbi, *Zhongguo jinxiandai kexuejishushi lungang* [An outline of science and technology in modern and contemporary China], (Changsha: Hunan Education Press, 1992); Dong Guanbi, ed., *Zhongguo jinxiandai kexuejishushi* [A history of science and technology in modern and contemporary China] (Changsha: Hunan Education Press, 1997).
6. See Chen Fan and Chen Jia, "LujishudeshehuihuayuSTSde zhongguohua [The Socialization of Technology and the Chinization of Science Technology and Society Studies (STS)]," *Ziranbianzhengfa Yanjiu* [*Studies in the Dialectics of Nature*] 29/2 (2013): 37–41. Chen Fan, "Woguodangdai kejiyushehui (STS) yanjiuxianzhuangjifazhan [The Status and Development of STS in Modern China]," *Studies in the Dialectics of Nature* 27/12 (2011): 15–19. Zhao Wanli and Hu Yonghui, "Dangdai STS Yanjiudeshehuixuejinlujiqizhuanxiang [The Sociological Approach to STS and Its Theoretical Turn]," *Science and Society*, 1/1 (2011): 80–93.
7. Lu Jiaxi, *Zhongguokexuejishushi* [A History of science and technology in China] (Beijing: Science Press, 2003).
8. He Ganzhi, *Jindai Zhongguo Qimengyundong Shi* [The history of Chinese Enlightenment] (Shanghai: Shenghuo Shudian, 1938), 109.
9. Jen Ch'o-Hsüan, "The introduction of Marxism-Leninism into China," *Studies in East European Though* 2 (1970): 138–166.
10. See F. Engels (translated by Cheng Song), *Congyuandaoren* [From Ape to Man] (Shanghai: Taidong Tushuju, 1930); F. Engels (translated by Du Weizhi), *Ziranbianzhengfa Yanjiu* [Dialectics of Nature] (Shanghai: Shenzhouguoguangshe, 1932); F. Engels (translated by Qian Tieru), *Fan Dulinlun* [Anti-Dühring] (Shanghai: Kunlun Shudian, 1930). V. Lenin (translated by Di Qiu), *Weiwulun he Jingyanlun Pipan* [Materialism and Empirio-Criticism] (Shanghai: Mingri Shudian, 1930).
11. On Du Weizhi see Gregor Benton, ed., *Prophets Unarmed*: *Chinese Trotskyists in Revolution*, *War*, *Jail and the Return to Limbo* (Leiden: Brill, 2014), 126–136.

12. Lenin, *Weiwulun he Jingyanlun Pipan*.
13. See the booklet first published in August 1937 and that Mao used during the Marxism lecture in Yan'an: Mao Tse-Tung, *Bianzhengweiwufa Tigang* [The outline of Dialectical Materialism] (Tianjin: Tianjin Renmin Press, 1958).
14. Mao's speech at the inaugural meeting of the Natural Science Research Society of the Border Region (February 5, 1940). Reproduced in Mao Tse-Tung, *Quotations*, chapter 22 (Methods of Thinking and Methods of Work), available at: www.marxists.org/reference/archive/mao/works/red-book/ch22.htm, accessed 10/2/2015.
15. Gong Yuzhi, "Zhongguo Ziranbianzhengfa Shi [The history of Dialectics of Nature in China]," *Studies in Dialectics of Nature* 2 (1991): 16.
16. Yu Guangyuan, *Yigezhexuexuepai Zhengzaixingqi* [One philosophical school is on the rise] (Nanchang: Jiangxi Science and Technology Press, 1996).
17. Gong Yuzhi, "Zhongguo Ziranbianzhengfa Shi [The history of Dialectics of Nature in China]," *Studies in Dialectics of Nature* 2 (1991): 8–14.
18. For instance, a prominent member of the dialectics of nature community now promoted a reappraisal of Newton and Galileo. See Li Jun, "Weishenmeyaojinian Jialilue he Niudun [Why we should remember Newton and Galileo]," *Qunzhong Weekly* 7 (1942): 12.
19. Ma Haiping, "Kangzhanshiqi Chongqing Ziranbianzhengfa de Xuexiheyanjiu [The learning and research of the Dialectics of Nature during anti-Japanese War in Chongqing]," in *Zhongguo Ziranbianzhengfa Yanjiu: Lishi yu Xianzhuang* [The studies of Dialectics of Nature: history and present status] (Beijing: Zhishi Press, 1983).
20. Elena Aronova, "Studies of Sciences Before 'Science Studies': Cold War and the Politics of Science in the U.S., U.K. and U.S.S.R., 1950s–1970s," PhD Dissertation, University of California, San Diego, 2012, p. 2.
21. Sigrid Schmalzer, "Self-Reliant Science: The impact of the Cold War on Science in Socialist China," in Oreskes and Krige, eds., *Science and Technology in the Global Cold War*, 75–106.
22. Gong Yuzhi, *Ziranbianzhengfa zaizhongguo* [Dialectics of Nature in China] (Beijing: Peaking University Press, 2005).
23. *Renmin Daily*, 9 January 1951.
24. Hua Luogeng, "Yigeshuxuegongzuozhe Xuexi 'Shijianlun'de Tihui," [The understanding of *On Practice* of a mathematician] *Chinese Science Bulletin* 7 (1952).
25. Hu Huankai, *A selected collection of documents on animadvert on science in China during 1950s–1970s* (Jinan: Shandong Education Press, 2003), 105.
26. Mao Tse-Tung, *Selected Works of Mao Zedong*, Vol. 1 (Beijing: Renmin Press, 1991), 180.
27. Jin Guantao, "Shijianlun yu Maliezhuyirujiahua [On Practice and the Confucianization of Marxism-Leninism]," text available on: www.aisixiang.com/data/69280.html, accessed 2/10/2014.

28. Wu Chongzhen, Liang Qianwen, and Shen Xiaofeng, "Jianguoshiqinian Ziranbianzhengfadexuexiheyanjiu [The learning and research of Dialectic of Nature from 1949 to 1966]" in *Zhongguo Ziranbianzhengfa Yanjiu* (Beijing: Zhishi Press, 1983). See also Laurence Schneider, *Biology and Revolution in Twentieth-Century China* (Lanham, MD: Rowman & Littlefield, 2003).
29. Qian Sanjiang, "Cherishing memories of Chairman Mao," *Peking Review* 42 (1977), 22; cit. in Edward Friedman, "Einstein and Mao: Metaphors of Revolution," *The China Quarterly* 93 (1983): 51–75, on 52–53.
30. Schoichi Sakata and Jiben Lizi de Xingainian, "The new concept of elemental particles," *Journal of Dialectics of Nature* 1 (1963): 7–14.
31. See Friedman, "Einstein and Mao: Metaphors of Revolution," 53.
32. On the straton model see: Ding Zhaojun and Hu Huakai, "A historical account of the accomplishment of the "Straton Model" in China," *Journal of Dialectics of Nature* 29/4 (2007): 62–67. One young physicist, Liu Yaoyang, proposed an idea similar to quark but since his theory would not fit well in Mao's ideas about infinity, his research was never published. Yinghong Cheng, "Ideology and Cosmology: Maoist Discussions on Physics and the Cultural Revolution," in *Mr. Science and Chairman Mao's Cultural Revolution*, edited by Nancy Wei and Brock, 197–233, on 204.
33. Schmalzer, "Self-Reliant Science", 87–89.
34. Gong Yuzhi, *Ziranbianzhengfa Zai Zhongguo* [Dialectics of Nature in China] (Beijing: Peaking University Press, 2005), 33.
35. Ibidem, 35.
36. Sun Xiaoli, "Ziranbianzhengfa: Kuaruwenlijiaorong de Guangkuolingyu [Dialectics of Nature: Into a vast field which combines art and science]," *Studies in Dialectics of Nature*, 27/12 (2011): 3–5.
37. Yu Guangyuan, "Speeches at the Qingdao Genetics Conference of 1956," in Fan Dainian and Robert Cohen, eds., *Chinese Studies in the History and Philosophy of Science and Technology* (Dordrecht and Boston: Kluwer Academic Publishers, 1996), 27–40, on 31.
38. Ibidem, 39.
39. GongYuzhi, "Kaideluofuzhu Lun'engesidezhuzuo Ziranbianzhengfa Yishujianping [The book review of Kedrov's *On Engels' Dialectics of Nature*]," *Journal of Dialectics of Nature* 1 (1957): 67–69.
40. "Ziranbianzhengfa (shuxueheziranke xuezhongde zhexuewenti) Shiernian-yanjiuguihua Cao'an [The draft of 12-Year Plan of Dialectics of Nature (Maths and the philosophical issues in natural sciences)]," *Journal of Dialectics of Nature* 0 (1956): 1–6.
41. Feng Ding, Yu Guangyuan and Wang Zisong featured as philosophers in the unit, whereas Zhou Peiyuan (a former student of Werner Heisenberg and the principal of Peiking University), Xu Guangxian, Wang Zhuxi, and Shen Tong featured as the scientists.

42. On the Chinese nuclear program see John W. Lewis and Xue Litai, *China Builds the Bomb* (Stanford, CA: Stanford University Press, 1988).
43. Evan A. Feigenbaum, *China's Techno-Warriors*: *National Security and Strategic Competition from the Nuclear to the Information Age* (Stanford, CA: Stanford University Press, 2003).
44. These included the physical chemist Lu Jiaxi (1915–2001, the president of Chinese Academy of Science from 1981), the physicist Li Chang (1914–2010, vice-president of Chinese Academy of Science from 1975 to 1982), the father of Chinese rocketry Hsue-Shen Tsien (1911–2009), and the Chinese nuclear physicist Qian Sanqiang (1913–1992) who contributed to the Chinese hydrogen bomb program.
45. This was partly a response to a mass protest that took place in 1976 following the death of Mao and that found Deng and Yu sympathetic. Cary Huang, "Yu Guangyuan, the economist who inspired Deng Xiaoping's reforms," *SouthChina Morning Post* 27 September, 2013.
46. Sun Xiaoli, "Ziranbianzhengfa: Kuaruwenlijiaorong de Guangkuolingyu [Dialectics of Nature: Into a vast field which combines art and science]," *Studies in Dialectics of Nature*, 27/12 (2011): 3–5.
47. The book review's translator appeared with the pseudonym Yaohui. The first Chinese edition of Kuhn's work was translated by Li Baoheng and Ji Shuli. On the reception and publication of Kuhn's work see the articles in the panel discussion "The Semicentennial of *The Structure of Scientific Revolution* and East Asian STS," *East Asian Science*, *Technology and Society* (*EASTS*) 6/4 (2012): 519–567; especially Bin Li and Dingchen Ren, "The First Fifty Years of *The Structure of Scientific Revolution* in Mainland China," 527–532.
48. Jin Wulun, "Tuomasi Ku'en Kexuezhexuexueshutaolunhui Jianjie [An introduction of the workshop of Scientific philosophy of Thomas Kuhn]," *Philosophical Researches* 3/2 (1982). Wulun went on to publish a study on Kuhn's incommensurability theory (Jin Wulun, "On Thomas S. Kuhn's Thesis of the Incommensurability of Theories," *Journal of Dialectics of Nature* 14/3 (1992): 11–18), and a monograph on Kuhn (Jin, Wulun, *Thomas Kuhn*, Taipei: Yuan-Liou, 1994).
49. After 1976, many influential books by Western scholars were published. In 1977, Xu Liangying translated Einstein's works (on this see Danian Hu, *China and Albert Einstein. The Reception of the Physicist and his Theory in China*, *1917–1979*, Harvard: Harvard University Press, 2005). From 1979, Fan Dainian contributed to translate Lakatos's works in Chinese. See Fan Dainian, "Imre Lakatos in China," in Kostas Gavroglu, Yogos Goudaroulis and P. Nicolacopoulos, *Imre Lakatos and Theories of Scientific Change* (Boston and Dordrecht: Kluwer Academic Publishers, 1989), 59–67.
50. Lyman Miller, *Science and Dissent in Post-Mao China*, 238–260.

51. Wang, "Science and State in Modern China," 559.
52. Cary Huang, "Yu Guangyuan, the economist who inspired Deng Xiaoping's reforms," *SouthChina Morning Post*, 27 September 2013.
53. Aronova, "Studies of Sciences Before 'Science Studies': Cold War and the Politics of Science in the U.S., U.K. and U.S.S.R., 1950s–1970s."

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