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AND BIOLOGICAL PSYCHIATRY

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Volume 28

Peter Zachar

Psychological Concepts and Biological Psychiatry
A philosophical analysis

PSYCHOLOGICAL
CONCEPTS AND
BIOLOGICAL PSYCHIATRY

A PHILOSOPHICAL ANALYSIS

PETER ZACHAR

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*Dedicated to John Naumann
for his independence, intelligence, and irreverence*

Acknowledgments

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Table of Contents

Preface: What this book is about	xv
PART I: THE ATTACK ON PSYCHOLOGY	1
1. Psychology In Trouble	3
1. Introduction	3
2. The Confabulation Problem	5
3. The Devil's Advocates	13
4. Replacements for Psychology?	15
5. Plan of the Book	17
2. Trouble From Psychiatry: Biomedical Materialism	21
1. Introduction	21
2. The Brain as the Substrate of Psychological States	22
3. The Brain as the Organ of the Mind	27
4. A Short History of Biomedical Materialism	30
5. Contemporary Biomedical Materialism: The Neo-Kraepelinians and Beyond	36
6. Biomedical Psychiatry and Humanitarianism	41
7. The Anti-psychiatry Critique of the Biomedical Model	45
8. Conclusion	48
3. Trouble From Philosophy: Eliminative Materialism	51
1. Introduction	51
2. A Short History of Eliminative Materialism	51
3. All Language/Knowledge is Theoretical	55
4. Scientific Realism	60
5. Folk Psychology is a False Theory	61
6. The Potential Advantages of Eliminativism	66
7. Comparing Biomedical and Eliminative Materialism	68
8. Conclusion	74

PART II: THE ROBUSTNESS OF PSYCHOLOGY	77
4. Why There is No Such Thing as “Folk Psychology”	79
1. Introduction	79
2. Folk Psychology as a Conceptual System	79
3. Folk Psychology as a Pejorative Term	82
4. Folk Psychology: Common Sense Defined as Consensus	84
5. Folk Psychology as Superficial Appearances	91
6. Folk Psychology as Self-Evident	93
7. Folk Psychology as a Degenerating Research Program	97
8. Folk Psychology as Antithetical to the Natural Sciences	104
9. Conclusion	105
5. A Critique of Anti-anthropomorphism	107
1. Introduction	107
2. Behaviorism as Eliminativism	107
3. The Construction of Anti-anthropomorphism in Psychology	109
4. Ethology’s Rejection of Eliminativism	113
5. Eliminativism, Science, and Scientism	115
6. Narrow Strategies Versus Narrow Philosophies	120
7. Conclusion	121
6. The Anchors of Psychology	123
1. Introduction	
2. Psychology Is a Level of Analysis Between Internal and External Worlds	124
The concept of levels and the framework of psychology	124
Substrates as tautologies and as advancements	128
Levels of analysis and explanatory pluralism	130
What is a molar account?	132
Psychology as an integrative framework	134
3. Psychology Is Explanation with Reference to What Is in the Head	135
Head versus world debates in clinical psychology	137
Head versus world debates in philosophical psychology	140
4. Psychology Is Understanding with Reference to the Self	144
The concept of self as fundamental	144
The genesis of self-other distinctions equals the growth of psychological mindedness	145

	The ad hoc versus the proper hoc account of beliefs and desires	152
5.	Conclusion	155
7.	Materialism Without Physicalism	157
1.	Introduction	157
2.	Mary, The Blind Neuroscientist	159
3.	Eliminativist Arguments Against Jackson	160
4.	What Subjectivity Contributes	164
5.	Psychology and Subjectivity I: Knowing Ourselves	165
6.	Psychology and Subjectivity II: Knowing Others	173
7.	A Brief Note on the Issue of Ineffability (privacy)	177
8.	Conclusion: Subjectivity at Least Confers a Cognitive Edge, and Probably More	179
	PART III: THE PSYCHOLOGY IN PSYCHIATRY	181
8.	Diagnosis, Behavior, and First-Person Information	183
1.	Introduction	183
2.	Adjoining Levels of Analysis Cross-Fertilize Each Other	184
3.	Systematic Diagnosis is not Co-extensive with Biomedical Materialism	187
4.	Psychological Approaches are not Anti-diagnostic or Anti-operational	189
5.	Behavioral Evidence and the Psyche	191
6.	DSM-III and DSM-IV Utilize First-person Information	195
7.	Psychiatry's Psychological Core	199
	Management Issues	199
	Secondary reactions	200
	Pathogenic reactions	203
8.	Conclusion	208
9.	Evolution, Adaptation, and Psychiatry	211
1.	Introduction	211
2.	Adaptation Does Not "Belong" to the Physiological and Genetic Levels of Analysis	212
3.	Bottom-up Neuroscience and Psychoanatomy Versus the Internal Ecology Model	215

4.	A Note on Psychology and The Internal Ecology of the Brain	221
5.	From the Dualism of “Organic Versus Functional” to Biopsychosocial Pluralism	222
6.	Conclusion	226
10.	Psychiatry, Science, and Anti-essentialism	227
1.	Introduction	227
2.	Kinds of Kinds	228
3.	The DSM Does Not Assume Natural Kinds	230
4.	Two Approaches to Classification, Both Arguably Anti- essentialistic	232
	Approach One: The Medical Model	232
	Diseases are not natural kinds	235
	Species are not natural kinds	239
	Maximize taxonomic advantages, compensate for dis- advantages	243
	Approach Two: Psychological Measurement	245
	Statistics require assumptions	246
	Mathematically confirmed folk constructs are not natural kinds	247
	Personality traits having a biological basis are not natural kinds	250
5.	Conclusion	251
11.	Psychiatry and Reality	253
1.	Introduction	253
2.	Having Neurological Effects Does Not Make Psychology Neuroscience	254
3.	Literally Changing the Brain Is Not the Only Way to Change Psychology	257
4.	Psychiatry and Practical Problem Solving	264
5.	So Where Is Reality?	265
6.	Science and Realism	267
7.	Conclusion	269
12.	Psychiatry and the Rhetoric of Morality	273
1.	Introduction	273

2.	Stigmatization Is a Psychological Problem That Is Independent of Etiological Models	274
	Stigmatization and blame are psychosocial processes	275
	Reducing blame, rejection, and isolation are psychological problems	276
	Psychological understanding reduces blame and stigmatization	279
	Inaccurate biological explanations have also been iatrogenic	281
3.	Biomedical Explanations Do not Guarantee Better Access to Care	285
4.	Moral Understanding	288
5.	Conclusion	293
13.	Reflections	295
1.	Introduction	295
2.	Procrustean Science?	295
3.	The Psychology of Biomedical and Eliminative Materialism	302
4.	Conclusion	304
	References	307
	Name Index	331
	Subject Index	337

Preface: What this book is about

The importance of consciousness as a topic of scholarly concern began, not with human evolution in the Pleistocene era, but with the scientific revolution in the 17th century. After René Descartes taught us to think of the body as a machine, the modern search for the rules of how this machine works began. If the body, including the brain, is an organic machine, then understanding how it creates *sentience*, *sapience*, and *selfhood* are the ultimate biopsychological questions. Only in this context did consciousness become an important philosophical and scientific problem.

Descartes himself could not imagine how the brain might create sentience, sapience, and selfhood, so he lumped them into the category of “the mental” and defined them as something non-material. He even thought that nerves were hollow tubes through which *animal spirits* flowed. The pineal gland was supposedly where mind and body interacted, and it was also akin to a psychological heart — pumping animal spirits through the neural tubules.

The prospects for biopsychology improved once we traded speculative physiology for experimental physiology. Thinkers such as Galvani, Helmholtz, Dubois-Reymond, and Brücke proved that nerves are not hollow tubes filled with animal spirits. Rather they are completely organic, and nerve impulses are both electrical in nature and measurable. In the context of the new experimental physiology, Helmholtz’s former assistant, Wilhelm Wundt, initiated an experimental psychology. About the same time, Brücke’s student, the Viennese neurologist Sigmund Freud, introduced the world to psycho-analysis. At the dawn of the last century the future looked good for psychology.

The problems of consciousness, however, were difficult to resolve. For example, the debates among Wundt’s students about whether or not *imageless* thought exists, and Freud’s theories about dream interpretation and a dynamic unconscious seemed to some less like science, and more like the kind of pedantic philosophical speculations that psychologists had hoped to leave behind. The scientific revolution created the concern with consciousness, but consciousness was more than scientific psychology could cope with. Some

psychologists could not imagine scientific psychology ever adequately understanding consciousness.

When confronted with problems that exceed our ability to deal with them, a judicious option is often to retreat, and work on problems that are more resolvable. The thinkers in psychology who were willing to cut their losses and abandon the problems of consciousness were called the radical behaviorists.

The radical behaviorists such as John Watson and B. F. Skinner subscribed to *anti-anthropomorphism*. According to anti-anthropomorphism, beliefs, desires, intentions, and other states of conscious experience are not appropriate topics for experimental study. The radical behaviorists believed that psychology's concern with consciousness and the mind was a remnant of theological speculation about souls. As Richard Rorty has argued, Descartes transformed "soul-talk" into "mind-talk," and although the mind seemed to be less spiritualistic, it really wasn't. As far as the radical behaviorists were concerned, it was time for psychologists to realize that the war between science and religion was over. Science had won, and we no longer needed to bother with minds. Psychologists were supposed to get on with the business of scientific research, and figure out how to make the world a better place.

As John Naumann once told me, radical behaviorism is a seductive viewpoint. Who wouldn't be for science, truth, objectivity, and progress? For most of the twentieth century, psychology and related disciplines were seduced. Talk about minds, subjective reactions, emotions, and especially consciousness was regarded with great suspicion. Many scholars remained willing to write about the phenomenological viewpoint and a dynamic unconscious, especially in clinical and counseling psychology, and they may have even gained some popularity — but for the most part they were not respected as scientific psychologists. No matter what their orientation, two whole generations of scientific psychologists were most comfortable using behavioristic language — especially the language of reinforcement and extinction.

The situation began to change in the 1980s with the growing dominance of cognitive psychology. At this time, the explanation of behavior was reinserted into the head. As a matter of fact the psychologists who began their education in these years tend to be most comfortable with information processing language, not behavioral language. Cognitive psychology did not, however, consist in a return to consciousness.

The return of consciousness to science in the 1990s was heralded by neuroscientists and philosophers. Neuroscientists such as Francis Crick,

Gerald Edelman, and later Antonio Damasio began proposing theories of consciousness. While Paul and Patricia Churchland showed philosophers that neuroscience is a necessary part of any adequate philosophy of mind, thinkers such as Daniel Dennett, John Searle, and Ralph Ellis wrote books about the nature of consciousness as a biological phenomenon. This very series, *Advances in Consciousness Research*, begun in 1995, represents an important part of the return to consciousness movement.

Even though neuroscientists and scientifically-minded philosophers have helped make the problems of consciousness more respectable, anti-anthropomorphism is still a seductive position — and consciousness remains a suspicious topic among many in the scientific community. In that context, this book does not present a theory about the nature of consciousness as a biological phenomenon, rather it examines the role that psychological concepts such as belief, desire, intention, and subjectivity should play in the scientific study of human behavior. Of course some of these concepts, especially subjectivity, include consciousness.

It would be incorrect to claim that the return to consciousness movement represents a return to the psychology of the early 20th century. There is no returning to the days of innocence that preceded radical behaviorism. Nor should we want to return to those days. In the past one hundred years there have been great advances, not only in psychology, but in neuroscience, and especially in the philosophy of science and the philosophy of mind. The defense of psychological explanation and understanding that is possible today would not have been possible for Wundt, James, or Freud one hundred years ago.

From the standpoint of intellectual history, radical behaviorism has been as good a friend to psychology as fundamentalist Christianity has been to Darwin. With respect to evolution, a few creationists have tirelessly pointed out legitimate holes in Darwinian theory, and the many successful responses to the creationists on the part of the Darwinians have made the theory of evolution by natural selection much stronger than it would have been otherwise. The radical behaviorists have played a similar role with respect to psychological understanding and explanation. Seduction is not always a bad thing.

As the 21st century dawns, anti-anthropomorphism is most prevalent in biological psychiatry and in materialistic philosophy. According to certain thinkers in both groups, the explanation of human action with respect to beliefs, desires, and intentions is false and radically misleading. They hold that

a scientifically accurate psychology would be a specialty within neuroscience. In their shared model, the job of the psychiatrist is to heal “broken brains.”

To some extent, this perspective is marketed to the public by the pharmaceutical industry, which has a financial interest in promoting a positive public opinion about the biological basis of psychiatric disorders. Anyone who knows about Prozac has a common sense understanding of biological psychiatry and materialistic philosophy of mind. Future advances in genetic technologies will make complicated questions about the biological basis of behavior even more relevant.

The point is that even though we have begun the return to consciousness movement, lingering scientific doubts about psychological explanation and understanding remain. As the table of contents and Chapter one indicate, I am going to systematically examine anti-anthropomorphism in both psychiatry and philosophy. I will not examine these positions only to critique them later on. As stated, there is no returning to the innocence of the early 20th century, and the psychology that emerges on the other side of the anti-anthropomorphic critique is better because of that critique.

After systematically examining the anti-anthropomorphic or “eliminativist” arguments, I will focus on philosophy to develop a model about what the framework of psychology should be like in a scientific world. *Psychological Concepts and Biological Psychiatry* provides readers an integrated framework for thinking about the problem of psychology-and-the-brain. Instead of considering physiology, but not evolutionary biology, or considering the scientific method, but not the history of science, or considering a theoretical model, but ignoring their own conscious experiences, readers will learn how to consider all these variables when thinking about psychology-and-the-brain.

I will then use that framework to examine the arguments of the biological psychiatrists. Not only do some biological psychiatrists think that paying more attention to the brain requires paying less attention to the psyche, some people who believe in the importance of consciousness and subscribe to mentalism think that paying more attention to the psyche requires paying less attention to the brain. Both groups are mistaken. Psychiatrists and clinical & counseling psychologists can safely accept that psychological states are brain states. While it is important to understand what is going on in brains, it is equally important to understand what brains do and how they got to be the way they are. This is called *the ecology of neuroscience*. In this evolutionary context I use clinical examples to demonstrate why, if we did not already have the

disciplines of both cognitive and psychodynamic psychology, we would have to invent them to better understand brains — including the very contemporary notion of consciousness as a biological phenomenon.

PART I

THE ATTACK ON PSYCHOLOGY

CHAPTER 1

Psychology in Trouble

1. Introduction

Reflecting a view that has become common among philosophers and cognitive scientists, Stephen Stich (1983) argues that the concept of belief “*ought not* to play any significant role in a science aimed at explaining human cognition and behavior” (p. 5). He would also banish the explanatory role of emotions, desires, and thoughts. In Stich’s view, there is a strong possibility that *psychological* explanations of behavior might turn out to be radically wrong. If Stich is correct, the philosopher Jerry Fodor (1987) claims it would be “the greatest intellectual catastrophe in the history of our species”(p. xii).

It seems preposterous, however, for someone to claim that psychological explanations such as “I bought a house because I *believed* the tax deduction would save me money,” “I hit him because I was *angry*” or “I went to the store because I *wanted* a Hershey’s chocolate bar” are illegitimate explanations, and expect to be taken seriously. It seems preposterous because denying that beliefs and desires are causes of behavior contradicts common sense. Claiming that beliefs and desires do not exist seems even more nonsensical.

Of course, common sense may not be worth defending. Any historian of science will tell you that contradicting common sense can just as easily be considered a virtue as a fault. Claiming that the earth was round rather than flat contradicted the common sense of almost everyone living in the 15th century. As Kalat (1998) notes, the concept “color blindness” also contradicted common sense. Those living at the dawn of the scientific revolution didn’t think it was possible to see an object without seeing what color it was. (In our time the phenomenon of motion blindness seems equally strange. How can one see a moving object but not see that it is moving?) The theory that humans and alligators share a common ancestor also challenged common sense, as did the

claim that “white” is what one sees when an object reflects back all the colors in the visible spectrum. So contradicting common sense is itself not a fault. We have to be open to the possibility that the world may be very different than we think.

Partisans of psychology frequently claim that the belief-desire framework is more than a common sense theory. For example, the fact that psychotherapy works, they claim, proves that psychological explanations have validity. There is, however, a problem with this response. Even if we grant that psychotherapy works, it isn’t clear that its working has anything to do with the validity of psychological explanations. In fact, success in therapy does *not* depend on the training of the therapist. As Robyn Dawes (1994) shows, psychologists have been unable to demonstrate outcome differences between master’s level and doctoral level clinicians. Clinicians with master’s degrees usually react to these findings with some satisfaction, until someone points out that there are not any outcome differences between master’s level and bachelor’s level clinicians either. For both debilitating and less debilitating kinds of problems, all degree levels seem to do equally well. If therapists actually use expert level psychological knowledge to help people change, those with more training should do better, but they don’t.

Readers trained in psychotherapy will be understandably skeptical of these findings. As one friend has commented, “If that was true, how come we never heard about it in school? If these studies are experimentally sound, our research-oriented professors wouldn’t have ignored them!” In fact, these findings haven’t been ignored by training programs, just reinterpreted. For example, professional psychology’s interest in the “common principles” shared by all successful therapies began as an attempt to account for these findings. Common principles include qualities such as empathy, persuasion, and the instillation of hope. Rather than ignoring the findings reported by Dawes, training programs modify their implications. They use the common principles analysis to explain why the theoretical orientation of the therapist does not seem to make a difference in outcome. In doing so they ignore the main point, that training *per se* does not make a difference in outcome.

However unpleasant the implications, intellectually responsible practitioners generally admit the *possibility* that learning to apply sophisticated psychological concepts is irrelevant. They also know that they can’t avoid the implications of psychotherapy outcome research by invoking the platitude that clinical psychology is a young science which has to be given time to develop.

Genetics is an even younger field, and it has already developed into a mature science.

The claim that psychological explanation and understanding is in trouble is a plausible one. As we will see in the next section, claims that much of our self-knowledge is the result of “confabulation” rather than observation are even more damaging to psychology.

2. The Confabulation Problem

Prince: Pray God you have not murd’red some of them?
 Falstaff: Nay, that’s past praying for. I have pepper’d two of them. Two I am sure I have paid, two rogues in buckram suits. I tell thee what Hal, if I tell thee a lie, spit in my face, call me horse. Thou knowest my old ward: here I lay, and thus I bore my point. Four rogues in buckram let drive at me.
 Prince: What, four? Thou saidst but two even now.
 Falstaff: Four Hal, I told thee four.
 Poins: Ay, ay, he said four.
 Falstaff: These four came all afront, and mainly thrust at me. I made me no more ado but took all their seven points in my target thus.
 Prince: Seven, why there were but four even now.
 Falstaff: In buckrum?
 Poins: Ay, four in buckram suits.
 Falstaff: Seven by these hilts or I am a villain else.

(William Shakespeare, Henry IV, Part 1)

“Confabulation” refers to augmenting one’s memory with inaccurate details. It is most common in chronic alcoholics who have developed Korsakoff’s syndrome and in persons with traumatic brain injuries. Unlike Sir John Falstaff, people who confabulate are not lying to interviewers, rather, they supplement their answers with extra details. Nor do they just make up details. Confabulation sometimes involves the retrieval of actual memories, but those memories either have nothing to do with the question asked or are distorted. Confabulators also tend to be confident about the accuracy of their reports.

For example, Moscovitch (1995) relates the story of a sixty-one-year-old stroke victim who had been married for more than thirty years, but incorrectly reported in an interview that he had been married for only four months. He also correctly reported that he had four children ranging in age from 22 to 32

years. When confronted with the inconsistency between having been married for only four months but having four adult children, he claimed that the children were adopted and proceeded to make up stories about when those adoptions occurred.

Confabulation in social and developmental psychology

Although the term “confabulation” is primarily used in medical contexts, research by social psychologists suggests that confabulation may not be limited to neurological disorders. It may be a normal process. In an influential literature review, Nisbett and Wilson (1977) claim that even though people readily answer questions such as “why do you like your job,” “why did you get married,” and “what made you stop going to church,” their answers to these questions are often fabrications, no matter how certain they may be of their accuracy. Wilson (1985) also shows that asking people to think about the reasons for their behavior actually *reduces* the predictiveness of self-assessment, almost as if introspection invites confabulation. If this is true, then the time-honored notion that we have direct conscious awareness of our attitudes, beliefs, and desires may be a philosophical mistake.

Some of the research reviewed by Nisbett and Wilson involves exposing people to conditions that lead them to change their attitudes about a particular topic, such as desegregation or affirmative action. In these studies, people tend to be unaware that their attitudes have changed. Developmental psychologists such as Astington and Gopnik (1988) have shown that two-year-old children cannot report having held beliefs that they have rejected as recently as five minutes ago; and most professionals are surprised to learn that adults make the same errors when the time span is shifted from minutes to days.

In a study using high school students, Goethals and Reckman (1973) assessed participants’ pre-existing attitudes before exposing them to an attitude change process. The specific attitudes targeted involved attitudes about bussing students to different school districts. After the participants’ attitudes were changed in the expected direction (pro or con), Goethals and Reckman asked each participant to recall what their attitude had been before the experiment. Surprisingly, the students tended to incorrectly report that their new attitude had been their attitude all along. Even when the experimenters informed the students that they had in fact changed their opinions, many students denied it.

Ross (1989) hypothesizes that in recalling past attitudes, people begin by assessing their current attitudes. As long as they don't have any reason for thinking that their attitudes have changed, they assume that their past attitudes correspond to their present attitudes. Often, these assumptions are wrong. Following George Herbert Mead, who claimed that every generation rewrites its own history, Ross and Buehler (1994) claim that every individual rewrites his or her own personal history as well. These rewrites help make past events consistent with present concerns and current knowledge.

Rather than being a case of mere forgetting, we actively distort our past to support what we want to believe. To illustrate, Conway and Ross (1984) note that study skills programs have not been shown to be effective in improving student performance, even though we tend to assume that they work. In this context, they asked a group of students about to begin a study skills class to evaluate their own skills. At the end of the course they asked the students to recall their original evaluations, and found that students tended to recall the original evaluations as having been worse than they actually were. This helped them believe that they had improved. Furthermore, although their grades did not improve, six months after the semester ended, the students also recalled their grades that semester as having been higher than they were. In contrast, a comparison group of students who did not take the study skills class tended to more accurately recall their skill evaluations and their grades. Students who took the class distorted their memories in order to justify their belief that the effort put into improving study skills was worth it.

Although Conway and Ross were primarily concerned about the implications for pop-psychology programs such as self-esteem workshops, these results should be sobering for clinicians who have been impressed with *Consumer Reports*-like surveys which show that most people who have been in therapy believe that therapy has been helpful to them. Since we actively distort our own histories, these surveys may only show that people want therapy to have been helpful. Rather than concluding that long term therapy is better than short term therapy as Seligman (1995) did, an alternative conclusion suggested by the Conway and Ross study is that people who invest more time and money in therapy are going to want to remember it as having been helpful.

Another example of normal confabulation is found in research studying people's willingness to tolerate electric shock. Before administering electric shock, Nisbett and Schachter (1966) gave one group of participants a placebo

pill, telling them that its effects included hand tremors, heart palpitations, shallow breathing, and butterflies in the stomach. These so-called effects actually describe the panic-like responses commonly associated with receiving electric shocks. Finding that people in the placebo condition tolerated higher doses of electric shock than average, Nisbett and Schachter claimed that they attributed their unpleasant sensations to the pill and not to the shock.

In 1977 Nisbett and Wilson described an additional finding not reported in the original 1966 article. When asked to explain why they were able to take so much shock, participants in the placebo condition made up explanations such as "I once worked with radios, so am used to shock." They claimed that they never thought about the pill. Even when informed of the nature of the experiment and how they had been manipulated into making a false attribution, they still claimed that they did not attribute any of the unpleasant sensations to the pill, and stuck to their original (confabulated) explanations.

In a similar study, Storms and Nisbett (1970) showed that when given a placebo and told that its effects included arousal symptoms such as racing thoughts and increased heart rate, people with insomnia attributed their symptoms to the pill and fell asleep on average 12 minutes faster than normal for them. Those who were given a placebo but told that it induced relaxation, took 15 minutes longer than normal to fall asleep. Nisbett and Wilson (1977) report that when asked to explain why they were able to fall asleep quicker, the insomniacs in the "arousal" condition made up answers such as "I sleep better later in the week" and "I'm not as worried as I was before." When explained the nature of the manipulation, participants denied that attributing their insomnia symptoms to the pill had anything to do with their ability to fall asleep quicker. They tended to be convinced of the accuracy of their original explanations, even when confronted with disconfirming evidence.

Claiming that we make up explanations for our behavior rather than introspectively examining the contents of our own consciousness is consistent with a hundred years of research in psychology showing that we have limited access to higher order cognitive processes, and are usually aware only of the results of those cognitive processes. For example, if I ask you to tell me your mother's maiden name or ask you what $10 + 17$ equals, the answer just comes to you. We have access to the answer but not to the processes used to find the answer. If I were to ask you how you came up with that answer, because you have no conscious access to those processes, any answer, if not based on an inference from some theory, would be a confabulation.

In scientific psychology's earliest days, this was called imageless thought. The psychologists of the Würzburg school thought that many cognitive activities had no content. In current day terms, they thought that decision making involves non-conscious processes. Similarly, in the United States, William James, John Dewey, and Hugo Munsterberg all developed a version of what Leahey (1992) calls "the motor theory of consciousness." (p. 280) The motor theory states that we do not normally think and then behave, we just behave. Thoughts and behavior are part of the same act. Our experience of thinking or willing as causes of behavior is a misremembering; rather, thinking and willing are inserted into the causal chain after the fact.

In this tradition, Nisbett and Wilson claim that answers to questions such as "why do you like your boss" are given in conformity to common sense theories about the causes of behavior. Even when we accurately report the causes of our behavior, those reports tend not to be based on conscious awareness of cognitive processes as much they are based on common sense models that happen to be accurate. For example, in the insomnia study, worry is a common explanation of insomnia. When asked by Nisbett and Schachter to explain why they got to sleep faster, persons with insomnia used the explanations supplied by common sense, *I slept better because I was not as worried as usual*, whether or not they were true. According to Nisbett and Wilson, if we ask someone why they enjoyed the party last night, rather than inspecting the contents of their consciousness and formulating an answer, most people will just consult their implicit theories about "Why people enjoy parties," and pick an appropriate response. Once they pick, "I talked to some interesting people," they remember their experience so that it corresponds to their answer. After we add experiential content to explanations, we tend to be very confident about their accuracy.

Even in clinical psychology, self-knowledge depends as much on consulting external criteria as it does on the introspection of the contents of consciousness. Interpersonal theorists such as George Herbert Mead and Harry Stack Sullivan claim that self-concepts are constructed through a series of reflected appraisals. They both believe that we develop self-concepts based on others' reactions to us and on what they say about us. This is the same thing Wilson (1985) is talking about when he writes that children are often told what they think and feel, and tend to accept these claims as accurate. For example, if Mom and Dad continually say: "Gee little Kevin, you sure do love that pony!," little Kevin eventually comes to believe that he loves that pony — even if that

was not his initial reaction. Sometimes, as adults we begin to realize that as children we had some thoughts and feelings that were not integrated into our working self-concepts. For example, it is possible that an adult Kevin might come to say “You know, I now realize that I hated that damn pony.” From the interpersonal perspective, our conscious experience can be as much confabulation as genuine reaction. The possibility of “false consciousness” is also what motivates existentialists to focus on the importance of “authenticity.”

The claim that children represent their internal experiences based on external interactions is also supported by the research of developmental psychologist Robyn Fivush (1994). She shows that when conversing with a child about past events such as vacations, parents have to remember for the child. Parents feed their children memory cues so that the children can recall the events too. Children can’t elaborate by themselves, but they can follow their parents’ stories. By participating in these mutual recall experiences, children not only practice remembering their past, they also learn what kinds of details they are supposed to remember.

According to Fivush’s research, parents elaborate more on emotions with girls than with boys. They also emphasize different emotions for each gender. For example, in helping their children recall frustrating experiences, parents emphasize the importance of sadness with girls and the importance of anger with boys. Even when anger is discussed with both genders, the interpretation differs. The retaliatory inclinations of little boys are constructed as normal reactions, whereas with girls, parents construe their decision to not act on retaliatory feelings as the normal reaction. Just as Mead and Sullivan claimed, children construct their internal worlds and their experiences of self based on external feedback. Part of what we call conscious content is taught to us, rather than simply given to us in experience.

Confabulation in cognitive psychology

Confabulation as a normal process is also supported by research in cognitive psychology. Beyond any reasonable doubt, psychologists now know that a good deal of our memory is reconstructive. Contradicting common sense, we do not encode accurate pictures of events and store them in memory for later retrieval. In encoding, we often fit new information into pre-existing structures, which requires some modification of the new information. In the retrieval process, we take partial recollections of events, combine them with

assumptions about how types of events in general tend to unfold, and reconstruct a memory of the event in question. Elizabeth Loftus (1983) has specifically shown that one's degree of confidence in a memory is unrelated to the accuracy of that memory. Memory construction as filling in the gaps combined with subjective certainty about accuracy is very similar to confabulation.

In a typical example of the new psychology of memory, Neisser and Harsch's (1993) critique of flashbulb memories convincingly rebuts the common sense idea that memories are stored in the brain as accurate representations of what really happened. Flashbulb memories refer to perfectly detailed memories of events, and these memories supposedly do not degrade over time. They are among the most impressive kinds of accurate memory. An example would be asking an American how they heard that President Kennedy was shot. People who were old enough at the time always seem to be able to answer this question.

It turns out, however, that these kinds of memories are often inaccurate. In a clever spur-of-the-moment idea, Neisser asked people the day after the Challenger explosion how they heard the news. Three years later he asked them the same question again, and found out that forty percent of the responses people gave at that time contradicted the responses they gave the day after the event. Their confidence ratings in the accuracy of their memories were, however, high, and some of them were reluctant to believe that their flashbulb memories were wrong, even when shown their original answers.

Perhaps the most famous modern memory experiments are those of Elizabeth Loftus and her colleagues on eyewitness testimony (Loftus & Loftus 1980; Loftus, Miller & Burns 1978; Loftus & Palmer 1974). Educating the psychological community about the fragility of memory, Loftus has shown that if people are given plausible but false information when they are asked to recall a single event, they tend to insert that information into their recollection of what really happened. Loftus, Feldman, and Dashiell (1995) note that using only suggestive questions, researchers can influence participants to remember seeing a stop sign when they really saw a yield sign, remember seeing a hammer when they really saw a screwdriver, and remember seeing a curly-haired culprit when the culprit really had straight hair.

More dramatically, people can be persuaded to believe that they have had experiences which they in fact never had. For example, Loftus and Coan suggested to research participants that they got lost when they were five years old. They had family members of research participants help implant this false

memory. Once the memory was implanted, the participants tended to elaborate on it with each retelling. One person, who was told by his brother that he got lost in a mall, remembered feeling terror, remembered being rescued by a balding man wearing a flannel shirt, and remembered his mother's reaction when they were reunited (cited in Loftus, Feldman, and Dashiell 1995).

Ceci (1995) reports similar findings with preschool children. After he and his colleagues implanted an absurd memory about getting a finger caught in a mouse trap and having to go to the hospital to get the trap removed, they found that the children would elaborate on the memory with each retelling. For example, children recalled how they got to the hospital and who was with them at the time. These preschoolers' confabulated reports were so detailed and internally consistent that child clinicians consistently evaluated them as accurate. Furthermore, after being informed that their memories were not true, a quarter of the children continued to insist that the event really did happen.

Modifying a point made by Neisser (1994), research in cognitive psychology indicates that it is important to distinguish between (a) the actual event, (b) the event as it was experienced by the person at the time, (c) what else is going on when the person is trying to remember events (emotions, suggestions), and (d) what they actually remember. We cannot assume that (d) what people remember is merely going to represent (a) what actually happened.

I'm sure readers can readily connect the new psychology of memory to their own experience. Most of us have had debates with friends about "what really happened" or "who said what first." Families often have "you said, I said" discussions as well. Sometimes these discussions develop into arguments because everyone is sure that it happened the way they remember it. The irony is that it probably didn't happen exactly like anyone remembers it. Even if the facts can be reconstructed, they have to be put into some narrative form to make sense, and that requires interpretation.

What psychologists call social skills partly involves being able to remember the mundane facts of life in an interesting way. In answer to the question "How's it going?" some people can tell riveting stories about their lives while others can't say much more than "Fine" — even though each of their lives might be equally interesting. As Brunner (1994) notes, Henry James made a similar point when he said "adventures happen to people who know how to tell it that way" (p. 48). Personal histories are much more than factual reports about what happened.

So what does the possibility of everyday confabulation say about psychological understanding and explanation? If I ask you why you picked the career you did, why should I think that your answer is based on an accurate memory rather than an inference, a distortion, or even a blatant confabulation? The same skepticism should be applied to memories of why you got married, why you had children, how you raised your children, or how your parents raised you. If the findings from social and cognitive psychological research are accurate, psychologists and psychotherapists may be mistaken in developing explanations of behavior based on self-reported beliefs and desires.

To take one last example, following research by experimental psychologist Gordon Bower (1981 1992) which indicates that personal memories are mood dependent, Lewinson and Rosenbaum (1987) claim that parental rejection is not a cause of depression, but a mood dependent construction. They support their hypothesis by showing that, when depressed, people tend to selectively recall memories of parental rejection, but only when they are depressed. Once people recover from depression, they evaluate their parents no differently from the way other people do. Theories about the etiology of psychopathology based on a client's self-report may therefore be based more on confabulation rather than on real events.

What is most damaging about the idea that our own psychological explanations are confabulations is that it comes from inside psychology. It emerged from research on psychological processes. This research can be found in social, cognitive, developmental, and clinical psychology, and is not just the product of anti-psychological scientists committed to the behavioral perspective. It also stretches back to scientific psychology's earliest days. When different research traditions, working relatively independently over a long period of time converge on the same basic conclusions, there has to be something there worth pursuing.

3. The Devil's Advocates

Most psychologists would not agree with the psychology-in-trouble claim, and many of them could compellingly critique the idea that psychological explanations are confabulations. In addition to emphasizing how accurate self-report and long term memories can be, they might also reject using a value-laden term such as "confabulation." Less pejorative terms such as

“interpretation” or “construction” would be more descriptive.

Richard Nisbett and Lee Ross’s (1980) observation that any causal process, be it physical or psychological, is necessarily a construction is friendlier to psychology than the more pejorative confabulation analysis. For example, in explaining why a stone dropped to the ground, a child may say “because it wanted to” and a Newtonian physicist may say “because it existed in a field of forces of which the most relevant were the mass of the stone, the earth’s gravitational pull, and the relative insubstantiality of the intervening medium, namely, air.” (p. 205). According to Nisbett and Ross, neither the child nor the physicist observes these causal processes; they both infer them based on some theory. To say that psychological causes are inferred rather than observed is therefore not such an insult.

We cannot, however, just dismiss the fact that we don’t have conscious access to many of our higher order cognitive processes, that we distort our present and past attitudes, and that confidence in a memory is unrelated to the accuracy of a memory. Since we often either lack the relevant information or have false information about our cognitive processes, our causal inferences are vulnerable to being chronically inaccurate. These facts are not going to go away. Burying one’s head in the sand while waiting for relevant research support is only a temporary solution.

The defense of psychological understanding and explanation from within psychology may also be corrupted by a self-serving attempt to protect guild interests. It is primarily philosophers such as Churchland (1984), Bechtel (1988), and Dennett (1987) who push the confabulation interpretation of social psychological research. Nisbett and Wilson (1977) never use the term. Although having invested a considerable amount of time, money, and identity in psychological explanation is going to make psychologists reluctant to consider chucking the project, many informed thinkers outside psychology are more skeptical. Taking seriously the psychology-in-trouble claim requires carefully considering the arguments of the thinkers who are most skeptical about psychological explanation, especially those who think that psychological explanation needs to be replaced and have strong ideas about what it needs to be replaced with. These thinkers are prominent in both philosophy and psychiatry. Taking *them* seriously is what this book is about. By taking them seriously, even if we don’t end up agreeing with their conclusions, we may agree with enough of the criticisms they make to alter our ideas about the nature of psychological understanding and explanation.

4. Replacements for Psychology?

We are all confronted with reactions and behaviors that seem to have no clear explanations or reasons. These include our own inexplicable thoughts and feelings, or the communications and actions of others. From intrusive thoughts such as wanting to harm others, to subtle fears, benign shifts in mood from glad to sad, and inexplicable attractions/revulsions to other people, we search out reasons for these phenomena.

A twenty-year-old male is referred for counseling after being hospitalized seven times in the past year. He is on probation for writing bad checks, he sometimes cuts his arms with a razor blade to get attention, and he complains of both premature ejaculation and getting angry at women for insignificant reasons.

An elderly woman flies back to the Midwest from California to visit her family. For months she has been talking about moving back home and has indicated to her eldest daughter that she intends to search for an apartment while on her trip. After arriving, she becomes convinced that her family wants to keep her from going back to California and repeatedly accuses them of plotting behind her back.

When psychologists look to the history of past relationships and current feelings about the self and when biological psychiatrists look to brain neurotransmitter levels, they both seek to explain what is not obvious to common sense. They also sometimes disagree about what counts as a good explanation. Such disagreements can be so strong that one group devalues the kinds of explanations offered by the other group. For example:

This shift in perception suggests that we need not look to theoretical constructs of the "mind" or to influences from the external environment in order to understand how people feel, why they behave as they do, or what becomes disturbed when people develop mental illness. Instead, we can look directly to the brain and try to understand both normal behavior and mental illness in terms of how the brain works and how the brain breaks down. (Andreasen 1984, p.138)

In all sectors of life, people are increasingly thinking about their behavior as a consequence of brain functions. Claiming that the brain is the substrate of psychological states has been central to Western thought since the 17th century, and it is claim that I will not refute. Understanding more about the biological basis of our behavior is an important advance. Some people, however, think an increased understanding of the relationship between brain

function and behavior must go hand in hand with a decreased reliance on psychological explanations.

The most determined advocates for disregarding psychological explanations are the *biomedical materialists* in psychiatry and the *eliminative materialists* in philosophy. Biomedical materialists believe that psychiatry is a medical science whose primary object of concern should be the brain. They would prefer to abolish the distinction between psychiatry and neurology. Eliminative materialists reduce psychology to common sense, what they call “folk psychology.” Folk psychology refers to the psychology of beliefs, desires, intentions, and emotions. They claim that folk psychological explanations are false and defective ways of understanding human behavior, and, as such, should be eliminated from our scholarly discourse and replaced by explanations using concepts drawn from neuroscientific research. Edward Shorter (1997), a historian and self proclaimed neopologist for biological psychiatry has even claimed that biological and psychosocial explanations are “polar opposites.... both cannot be true at the same time” (p. 26).

Both biomedical and eliminative materialists share an attraction to *anti-anthropomorphism*, the view that psychological explanations are conceptual artefacts from humanity’s pre-scientific history. Proponents of anti-anthropomorphism believe that concepts such as belief, desire, feeling, and consciousness are primitive concepts. This perspective became popular in early 20th century American psychology. It began with the study of animal behavior where scientists developed an increased awareness of the errors of anthropomorphism — attributing human characteristics such as beliefs and desires to animals. Those thinkers who came to be called the radical behaviorists applied anti-anthropomorphic methodology to human beings, arguing that it was illegitimate to attribute beliefs and desires to both animals and humans. To reject beliefs, desires, attitudes, consciousness and their like is to reject psychological explanations.

Rather than being extremists, the biomedical and eliminative materialists are among the leading scholars in their respective fields. They have reasons for believing what they do. When asked questions such as “*If depression is a brain state, what must we do about it?*” their unqualified answer is “*Modify the brain.*” Not only do they adopt the psychology-in-trouble thesis, they have a limited commitment, personally and professionally, to the continued proliferation of psychological explanations. They view the defense of psychology as a conservative attempt to hold onto a familiar but depleted model, and are

more interested in exploring the revolutionary implications of new models for self-understanding.

If we take both the psychology-in-trouble thesis and the claims of the biomedical and eliminative materialists seriously, the world becomes a dangerous place for psychology. Not only could psychological explanations of ourselves and others be explanatory fictions, there may be better explanations out there telling us what is really happening. The promise of new and better ways of understanding ourselves is appealing. Since these improvements depend on future scientific discoveries, there is no way to make the world safe for psychology once and for all. What can be done is make the world safer for psychology. Making the world safer for psychology will involve exploring biomedical and eliminative materialism much more carefully and systematically than I have done in this introduction. Once we understand the biomedical and eliminative materialists more completely, get into their heads so to speak, we will begin to see what of psychology can survive their critique. Much of traditional psychological conceptualization will survive, however, some of it may also have to be abandoned.

In addition to an increased understanding of the relationship between psychology, science, and common sense, this book will make it easier for readers think psychologically about themselves and others without having to reject biomedical approaches as too narrow

5. Plan of the Book

In the rest of *Part I: The Attack on Psychology*, I systematically describe the principles of biomedical (**Chapter 2**) and eliminative materialism (**Chapter 3**). I take biomedical and eliminative materialism seriously and would like to help the reader to take them seriously as well.

Part II: The Robustness of Psychology begins my defense of psychological understanding and explanation, where I systematically critique eliminativism. I accept a large part of what eliminativists say, both about philosophy and psychology, but what I disagree with will have implications for their attack on psychological understanding and explanation.

In **Chapter four** I explore the concept of “folk psychology,” arguing that for most philosophers “folk psychology” is a synonym for “psychology-in-general.” Philosophers also assume that folk psychology belongs to the realm

of common sense, and not to science. I show that a strict dichotomy between science and common sense cannot be supported. Readers will come to see that the robustness of eliminativism depends on common sense assumptions about the brain, and that folk psychology has much more systematic evidence and explanatory power behind it than the eliminativists acknowledge.

In **Chapter five**, I addresses *anti-anthropomorphism*, the century-old idea that psychological explanations are conceptual artefacts left over from humanity's prescientific history. I demonstrate that anti-anthropomorphic arguments are based on a view of history that is more myth than fact. As a matter of fact, the development of eliminativism in scientific psychology did not happen the way psychologists have been told it happened. I show that the "scientific assumption" that we should eliminate mentalistic terms such as belief and desire from our lexicon is heavy on the assumption side and light on the science side.

In **Chapter six** I reject the eliminativists' views on the framework of psychology. Instead of agreeing with them that the framework of psychology is explanation with reference to beliefs, desires, and intentions, I offer an alternative framework that is more fundamental and less eliminable. At least no one has yet suggested eliminating the dimensions that I claim define the framework of psychology. These include the notion of psychology as a level of analysis between internal and external worlds, psychology as explanation of behavior with reference to what is in the head, and psychology as understanding with reference to the self. A thoroughgoing eliminativism would have to eliminate these "anchors of psychology," and I don't think that is possible.

Chapter seven presents the second part of my proposed framework for psychological explanation and understanding, focusing on consciousness. In this chapter I examine the role that subjectivity plays in psychological understanding and explanation. Although eliminativists claim that all information can be reduced without remainder to third-person descriptions, they admit that many of the concepts we use to understand ourselves and others have subjective elements. I show that without those first-person elements, the concepts would not have the meanings that they do. The information that we get from knowing what states such as anger, sadness, and guilt feel like makes an important difference in how we understand both ourselves and others. I illustrate the importance of adopting "Materialism without Physicalism" with examples drawn from clinical psychology.

Part III: The Psychology in Psychiatry takes what was learned from Part II and applies it to the analysis of biomedical materialism. As with eliminativism, I show that biomedical materialism partly rests on a 19th century view of science, particularly biological science and its relation to psychiatry. This 19th century view is *essentialistic* and surprising pre-Darwinian in tone.

The purpose of **Chapter 8** is to show that first-person information is central to the practice of psychiatry — including both psychoanalytic and biomedical approaches. This leads to the conclusion that a purely third-person or objective psychiatry does not exist. As a matter of record, in their own writings the biological psychiatrists acknowledge that psychiatrists can be materialists, but not physicalists. Consciousness is still a part of biological psychiatry, and should remain so. These considerations call into question the biomedical materialist assumption that the more exclusively genetic and physiological psychiatry becomes, the more scientifically valid it becomes.

Chapter 9 shows that alongside the science of physiology, any adequate psychiatry must also consider the science of evolution. This is a consequence of my own model of the framework of psychology and also a consequence of the heavy emphasis placed by psychiatrists and psychologists on the concept of “maladaptive” in understanding what makes certain psychological states pathological. Adaptation cannot be reduced to the physiological or genetic levels of analysis, or explained from the bottom-up perspective. Even for those who focus on biological etiology, the *ecology of neuroscience* invalidates an exclusive focus on physiology and genetics. Contrary to popular assumptions, evolution is not just about what happened in the past. My framework for psychology will help readers think in terms of both physiology and evolutionary biology when considering the brain’s role in psychiatry. The brand of evolutionary anti-essentialism that I further develop in this chapter will also be an important theme in the next two chapters.

Chapter 10 undermines the biomedical materialist assumption that understanding the pathological process as a biopathological process is a way for psychiatrists to “carve nature at the joints.” It undermines this assumption by critiquing the concept of “natural kinds.” Many scientific psychologists and psychiatrists mistakenly think that if a category can’t be conceptualized as a natural kind, it is an arbitrary category. I show that conceptualizing psychiatric disorders essentialistically is inconsistent both with medicine’s understanding of *disease* and evolutionary biology’s understanding of *species*. In contrast to natural kinds, I introduce the concept of practical kinds, which are stable

patterns that can be identified with varying levels of reliability. It will become clear to readers that thinking psychometrically and conceptualizing psychiatric disorders as practical kinds is more consistent with a scientific view of the world, which rejects the conservative defense of current models in favor of “open concepts.”

Chapter 11 critiques another faulty assumption of biomedical materialists, specifically the assumption that if psychological states are brain states, then describing what is going on in the brain is a truer description of what is happening. They also assume that psychological explanations automatically gain more credence if they can be described in terms of the brain. I show that although assumptions about the brain’s being what is “really real” appear to be common sense conclusions, they do not follow from the brain-as-substrate thesis. I also introduce Kuhn’s idea of scientific exemplars to demonstrate that biomedical solutions to the problems of psychiatry are not always the most direct or most elegant way to help people get better. The point is not to critique important biomedical exemplars, but to show that psychiatrists and psychologists need multiple exemplars. Having multiple exemplars in one’s conceptual repertoire is the best defense against simplistic “psychology versus biology” thinking. I defend this strategy in terms of evolutionary epistemology, specifically William James’ pragmatic theory of truth.

Chapter 12 undermines the melodramatic arguments used by biomedical and eliminative materialists in defending their views, specifically the arguments that simply state: “Biological medicine good, psychological understanding and explanation bad.” I show that the more committed proponents of biomedical materialism compare biomedical successes with psychological failures, and then (they) draw conclusions about the moral worth of each treatment strategy. I also show that it is just as easy to make the argument “Biological medicine bad, psychological understanding and explanation good.” A less exaggerated analysis suggests that the biomedical and psychological perspectives are inherently neither good nor bad.

In **Chapter 13** I conclude with a series of reflections about how stubborn all of us, including biomedical and eliminative materialists, can be about philosophical and scientific opinions. I observe that psychiatrists who try both to think about patients psychologically and to prescribe medication are undertaking an intellectually daunting task. Given the limits of human information processing, the convenience of thinking just psychologically or just biologically is understandable. Even though, as I shall argue, it is often wrong.

CHAPTER 2

Trouble from Psychiatry

Biomedical materialism

1. Introduction

In the following pages I define the basic principles of biomedical materialism using Kuhn's (1970) concept of "scientific paradigms" as a framework. Biomedical materialism is an integrated view of psychiatry which includes assumptions about what counts as a good problem, what counts as good evidence, and what counts as a good solution. It is also a view about the nature of science and science's relation to psychiatry. In addition, biomedical materialism contains an ethics and provides a moral justification for professional practice based on biomedical principles.

Biomedical materialism is a collection of views in psychiatry tied together by a belief that the brain should be the primary target of psychiatric interventions. Biomedical materialists claim that psychiatric disorders are best conceptualized as brain diseases. Many of them think that psychological analyses are irrelevant for understanding the nature of psychiatric disorders. Thinking that psychiatry should approach its professional problems as scientifically as possible, they usually downplay the distinction between psychiatry and neurology. Some would prefer that these two specialties be merged. Biomedical materialists are also scientific realists, and believe that having an accurate understanding of the biopathological process underlying psychiatric disorders will best help psychiatrists achieve the humanitarian goals shared by all medical professionals.

As a world view, biomedical materialism is espoused by numerous thinkers in medicine and psychology. Like any world view, not all to whom I

attribute it will agree with it in every respect. Some thinkers in psychiatry make statements indicative of a strong acceptance of biomedical materialism and then make statements that conflict with their earlier assertions.

Other thinkers such as Samuel Guze and Donald Klein have an evolutionary view of biology which, in certain respects, moves them beyond biomedical materialism. I will present a broader view of psychiatric conceptualization in Chapters 8 through 11 when I examine how the biological psychiatrists differ from the eliminative materialists. This difference can be attributed to the fact that the psychiatrists' professional problems are grounded in the concrete and tangible complaints of their patients, and their interventions are expected to make a practical difference in people's lives.

My description of biomedical materialism is straightforward and should be easily understood by most readers. As stated in Chapter 1, I think these arguments are persuasive. For those readers who approach the biomedical model with some skepticism, I suggest that you read this chapter with an attitude of open mindedness. The psychiatrists and psychologists who advocate biomedical materialism do so for reasons. I challenge you to join my attempt to get inside the heads of the biomedical materialists and try to understand why they think that their position is a viable and warranted view.

2. The Brain as the Substrate of Psychological States

In examining biomedical materialism, I have chosen to discuss the most compelling evidence first, i.e., the scientific evidence which supports the common sense belief that we think with our brains. If we do not think (and feel) with our brains, biomedical materialism would be as mistaken as the Ptolemaic theory of the solar system. My description of the major kinds of evidence for the brain as substrate thesis will be brief. Many readers are likely to have a background that goes far beyond this survey in both breadth and detail, but in outlining the basic *kinds* of evidence for the brain as substrate thesis, briefer is better. With a concise presentation, the reader will more clearly see both how powerfully the data fit together and how compellingly they support the brain as substrate thesis.

The major categories of evidence that I wish to review are as follows:

- a. Organic Diseases That Produce Symptoms of a Psychological Nature
- b. Chemical Substances That Alter Consciousness

- c. Brain Injuries That Alter Personal Consistency and Psychological Unity
- d. Anatomical Studies of Major Mental Illness
- e. Genetic Studies of Major Mental Illness and Personality

My strategy will be to provide typical examples of the type of evidence contributed by each of these categories.

Organic diseases that produce symptoms of a psychological nature.

Thomas Kuhn's concept of an *exemplar* refers to an example of a successful problem solution used by members of a particular discipline. Some examples are so successful that they become models or prototype solutions which disciplines take as their ideals. Members of the discipline teach these ideals to their students. When confronted with new problems, students learn to solve them using criteria illustrated in exemplars. The equation $F=ma$ in Newtonian physics and famous case studies in psychoanalysis are exemplars. The most important exemplar of the biomedical model is *general paresis of the insane*.

In 1900, general paresis accounted for one quarter of admissions to psychiatric hospitals (Pichot 1983). Rosenhan and Seligman (1995) claim that general paresis is the worst epidemic of madness in recorded history. Its symptoms begin with weakness but as it progresses, the symptom pattern can include depression, irresponsibility, slovenliness, mania, and delusions of grandeur. The eventual outcome is paralysis followed by death.

At the turn of the century, the role of syphilis as a possible cause of general paresis had been suspected for more than fifty years, but could not be proven. Following studies by Jepersen, Krafft-Ebing, Wasserman, and Noguchi & Moore between 1874 and 1913, the causal role of syphilis was conclusively demonstrated. By 1909 Paul Ehrlich had discovered the first treatment for syphilis, and in 1918 Julius Wagner-Jauregg introduced a successful treatment for general paresis (for which he received a Nobel Prize in 1927). Rosenhan and Seligman (1995) claim that general paresis was eradicated in a generation. Being able to prove a physical basis for general paresis, and to treat it, fueled the hope that applying the biomedical model to other psychiatric problems would result in equally stunning successes.

The facts about general paresis and other degenerative brain disorders such as Alzheimer's disease and Huntington's disease strongly support the brain as substrate thesis. They demonstrate that a disease process in the brain can produce symptoms of a psychological nature. The resulting disorder of the mind is a result of a disorder in the brain, and the best way to treat these

disorders is to fix what is broken in the brain.

Chemical substances that alter consciousness. Chemical substances have been used to modify and affect consciousness throughout human history. Examples in this category are so well known and obvious that one has to be surprised at the extent to which some people have seemed unwilling to accept that psychiatrists have discovered drugs which can make people less depressed, less anxious, or less psychotic.

One of the most utilized chemicals has surely been alcohol. In addition to the effects of intoxication, once someone becomes physiologically dependent on alcohol, withdrawal can be associated with psychological symptoms such as depression, restlessness, and hallucinations. Hallucinogenics represent another group of drugs which have been used in numerous cultures throughout the ages. These drugs include LSD, psilocybin (e.g., mushrooms), and mescaline (e.g., peyote). They can be associated with depersonalization, enhanced affect ranging from ecstasy to despair, perceptual illusions in multiple sensory modalities, synaesthesia, delusions, paranoid ideation, and flashbacks. Another recreational drug, cocaine, can cause euphoria, paranoid ideation, tactile hallucinations, and increased self-confidence. Obviously, drugs cause psychological changes by altering brain chemistry.

Brain injuries that alter personal consistency and psychological unity. In addition to the slower action of viruses and the relatively quick action of psychoactive substances, the brain can be altered directly through either accident or surgery. Both traumatic brain injuries such as the Phineas Gage story and other cases of frontal lobe injury (Stuss & Benson 1984; Stuss 1991) and surgically-induced brain injuries, such as the split-brain studies (Gazzaniga 1967, 1983; Sperry 1968, 1982) are prominent in this category.

Traumatic brain injuries (TBI) are sometimes subtle. A traumatic brain injury can be an unusually difficult disability to have because people with a brain injury often look and sound physically unimpaired. Although looking normal, the problems they have with impulse control, concentration, memory, comprehension, and emotional regulation have disastrous effects on their lives. In an important sense, TBI's are physical disabilities with psychological symptoms.

One of the more psychologically intriguing phenomena in the TBI category is called *contralateral neglect syndrome*. Kupfermann (1991) reports that if someone receives a lesion in the posterior parietal cortex (Brodmann's areas 5 and 7), that person's awareness of bodily space is altered, especially if

the lesion is in the non-dominant lobe (usually the right lobe). What happens in these cases is that people almost completely ignore the left half of their body. For example, they fail to clean or groom the left side of their body, as if it did not exist. Their left arm may hang limp, and if queried about the arm they will even deny that it is their arm! In drawing figures they may also leave one half of the body undrawn. These injuries indicate a concrete neural substrate of one's sense of self. If anything penetrates to the core of psychology and the concept of the mind, it is the sense of self. Brain injuries of this type indicate that our most essential psychological characteristics depend on a properly functioning brain.

Anatomical studies of major mental illness. According to Kandel (1991b), studies have demonstrated that some schizophrenic patients have anatomical abnormalities, particularly: (a) enlarged lateral ventricles, (b) enlarged third ventricles, both reflecting general deterioration of tissue, and (c) a widening of the sulci, which reflects degeneration of frontal lobe tissue. Studying monozygotic twins, Weinberger (1987) found ventricular enlargement is demonstrable in the twin having schizophrenia and not present in the twin who is normal. For the most part, when ventricular enlargement is found in people with schizophrenia, those people had a significant prodromal period. (Prodromal period refers to the time in their life before the identifiable onset of the illness, but in retrospect can be identified as the beginning.) Ventricular enlargement is also observed in people with Alzheimer's disease, in chronic alcoholics, and in people with bipolar disorder. These studies demonstrate that in some cases, gross brain anatomy is correlated with psychological disturbance.

Genetic studies of major mental illness and personality. Genetic studies provide strong evidence for a biological component to psychiatric disorders, including schizophrenia, depression, bipolar disorder, and alcoholism (Kety 1982). The program of studies investigating the genetics of schizophrenia has been especially thorough. Kallman (1938) demonstrated that while 1% of the general population suffers from schizophrenia, if the population sampled includes only close relatives of people diagnosed as having schizophrenia, the incidence rises to 15%.

Torrey, Bowler, Taylor, and Gottesman (1994) claim that the concordance rate of developing schizophrenia between monozygotic twins (who share the same genetic structure) is 28%–40%, whereas among siblings (including dizygotic twins) the concordance rate is 6%–15%. Given that the

concordance rate is 1% in the general population, a logical conclusion to draw is that a shared genetic endowment among family members is responsible for the higher concordance rate between siblings and twins. The more the overlap in genes, the higher the concordance rates.

Torrey et al. actually correct previous overestimations of the genetic component to schizophrenia. Interestingly, they note that the more conservative pairwise statistical methods which indicate that there is a 28% concordance rate for schizophrenia also indicate that there is a 56% concordance rate for bipolar disorder. This finding reflects many clinicians' experiences of bipolar disorder, which seems to be much less variable from person to person than other disorders. Psychiatric disorders with stereotyped presentations are often considered to have substantial somatic components. In bipolar disorder, the depression is a very physically evident state, and the mania tends to overwhelm personality characteristics that often mold the expression of other disorders.

One problem with the above studies is that in addition to a shared genetic endowment, family members also share similar environments (psychosocial influences). In the context of the shared environment problem, the most compelling evidence for a genetic contribution to schizophrenia comes from a study by Kety, Rosenthal, Wender, Schulsinger, and Jacobsen (1975). They demonstrate that children of schizophrenic parents who are adopted by non-schizophrenic families are 10–15% more likely to develop schizophrenia than adopted children whose biological parents are free of the illness.

With respect to personality, researchers at the University of Minnesota have spent years studying identical twins who were put up for adoption and subsequently adopted by different families. Some examples of traits that are strongly related among identical twins reared apart (and not strongly related in the general population) include IQ, traditionalism, religiosity, well being, delinquency, and emotional stability. (Bouchard, Lykken, McGue, Segal & Tellegan 1990; Bouchard & McGue 1990). The evidence that certain psychological states are hard-wired into the brain at birth appears to be strong.

To summarize this section, studies of brain diseases, brain biochemistry, brain injuries, brain anatomy, and genetics all suggest that psychological events are dependent on the brain. One implication of these data is that if we have a thought or a feeling or any complex psychological mixture of thoughts and feelings, something is happening in our brains. If I am intelligent or I am an extrovert, I am that way because of the way my brain works. If I feel happy

one moment and sad the next, something has changed in my brain. If I believed in God yesterday, and am an atheist today, something has changed in my brain. As we will see, biomedical materialists also believe that if we want change thoughts and feelings associated with psychopathology, we should look to the brain because we will then be going directly to the source of the problem.

3. The Brain as the Organ of the Mind

Psychiatry is often defined as the branch of medicine that treats disorders of the mind. A potential consequence of the brain as substrate thesis, and one that biomedical materialists are willing to accept, is that since psychological states are brain states, professions charged with restoring “psychological” health and well-being should focus their attention on the scientifically demonstrated root of psychology, the brain. The psychiatrist Guze (1992) summarizes the biomedical position well, claiming that “the brain is the organ of the mind” (p. 129) and “No modern psychiatry can ignore the substrate of the mind’s operation: the brain.” (p. 131).

Biomedical materialists believe that since all psychological experience is dependent on the brain, no matter what psychological state we consider, a significant proportion of its variance can be accounted for by studying the brain. Whatever “causes” we may hypothesize to be the determining factors of psychological states — including (a) genetics, (b) environmental influences such as the family, culture, and other interpersonal relationships, or (c) environmental influences such as nutritional deficiencies, blows to the head, and viruses — these causes all exert their influence by affecting the brain.

Even if one believes that component psychological states such as low self-esteem and abandonment fears also determine the overall psychological state of the individual, since these component psychological states are in some form or another brain states themselves, the brain is still potentially the best candidate on which to center our attention. As Guze (1992) writes, “one’s feelings and thoughts are as biological as one’s blood pressure or gastric secretion: feelings and thoughts are manifestations of the brain’s operations just as blood pressure reflects the operations of the cardiovascular system and gastric secretion the stomach’s function” (p. 130).

Considering the brain to be the organ of the mind extends the brain as

substrate thesis one step further. If psychological states are attributes of the brain, then they can be thought of as really being brain states. If we want to understand the nature of human psychology, we have to learn brain physiology. The brain as the organ of the mind tenet introduces a form of scientific realism into biomedical materialism. In their view, the neuroscientific hypotheses about the brain with respect to psychological disorders are hypotheses about what really exists and through neuroscience we will find out the truth about psychology.

Some thinkers explicitly claim that since psychological states are really brain states, the effects of psychological interventions can be most accurately understood in terms of their effects on the brain. For example, two psychologists, Lickey and Gordon (1991), state that “psychotherapy can properly be thought of as a biological technique that fits neatly into the medical model of mental illness.... Whatever the details, effective psychotherapy has to be considered a method of changing synaptic transmission” (p. 362).

Mohl (1987) claims that psychiatrists should be willing to move between the neurosciences and behavior as scientists move between subatomic physics and chemistry. One reason they do not readily accept the connectedness of the biological and the psychological is intellectual convention. Mohl suggests adopting a new convention: “medication, dream interpretation, and empathy simply become different ways to alter different neurotransmitters, presumably in different parts of the brain” (p. 325).

In addition to stating that psychological interventions are effective because they affect the brain, biomedical materialists conceptualize the nature of pathological processes in terms of the brain. They take it as axiomatic that the pathological process is really a brain process. For example, with respect to making differential diagnoses among the subtypes of schizophrenia, Andreasen, Flaum, Swayze, Tyrell, and Arndt (1990) suggest two possible models. In one model, which they call the “neurosyphilis” model, a single mechanism affects different brain regions at different stages of the illness. In the second model, called the “mental retardation” model, multiple mechanisms affect a variety of brain regions but lead to a final common syndrome. The neurosyphilis model is based on the general paresis exemplar. The mental retardation model is based on the notion of polygenic traits (as opposed to monogenic or single-gene traits). Andreasen et. al. (1990) indicate that some combination of both models is a possibility.

When we conceptualize the pathological process as a brain process,

psychological symptoms become secondary. For example, in the following passage, Andreasen and Olsen (1982) apply the biomedical model of pathology to the problem of diagnosing schizophrenia.

[If] one assumes that diagnostic subtypes reflect differing underlying causes, phenomenological characteristics may be a poor way to identify etiological subtypes...Once we know the cause (or causes) of schizophrenia and have laboratory tests for making the diagnosis, the use of cross sectional phenomenology to define subtypes may well appear meaningless. (p. 794)

This critique of phenomenology has its root in the well-known distinction between symptoms and the content of symptoms. Symptoms include phenomena such as hallucinations and delusions. The content is what the symptom is about. For example, I was once awakened at 3:00 A.M. by a client who informed me that (a) the Mafia was stalking him and (b) a person staying in the same house as he was their spy. These were delusions. The mafia and the spy were the content of the delusions. Ten hours earlier, I had been dealing with the so-called spy, who was furious because he thought that the police and officials at the community animal shelter were conspiring to torture his dog. His fear about the dog being tortured was also a paranoid delusion, but the content of his delusion was different from the content of his housemate's Mafia delusion.

Bruetsch (1974) describes how the delusional content of those with general paresis changed from time to time and from country to country. Hospital records indicate that in turn of the century France, patients with general paresis thought they were Napoleon; in Germany they thought they were the Kaiser; in Russia they thought they were czars and grand dukes. In the 1920s, many Americans imagined themselves to be Henry Ford. During World War II, patients claimed to be dive-bomber pilots and the owners of battleships. Napoleon or Henry Ford? What matters is not the phenomenological content, but the fact that the patient is delusional.

Not only is the nature of the symptom more important than the content of the symptom, symptoms themselves have to be understood in the context of syndromes. For example, a fever can be symptomatic of many different disorders. In the psychiatric realm, persons with dementia, schizophrenia, and major depression can all have delusions. The syndrome not the symptom is the reason for the problem.

According to biomedical materialism, the syndrome is really a biopathological process. Psychological and social variables may have an influ-

ence on the development of the problem, but the etiology, with respect to a cause that has to be there in order for the disorder to be present, is biological. Hence, Andreasen and Olsen's claim that psychological content may be irrelevant to treating the actual problem. Although the content is interesting and may be meaningful to the individual, it is less important to the medical treatment of the syndrome itself. It is not diagnostically important if someone suffering from general paresis thinks that their brother is plotting to take over the family business. Discovering what fantasies and self-object configurations are being expressed in the delusion will not cure general paresis. What is important is treating the underlying brain disease that is the cause of the delusional symptom.

In a similar vein, Guze (1992) refers to arguments by Whitehorn (1952) who compared undefended psychoanalytically symbolic material in schizophrenic speech to the disclosure of intimate housekeeping details after the collapse of the facade of a bombarded house. Whitehorn claimed that observable primitive conflicts in persons with schizophrenia are results of the schizophrenic decompensation, not its cause. Understanding the decompensation does not require insight into the primitive conflicts. As Guze writes:

Thus, if one considers the possibility that many, if not most, psychiatric disorders result from some variation in brain function, the possibility clearly arises that the patient's "disclosures" may not be specific to the disorder under study (p.16)

In conceptualizing the brain as the organ of the mind, biomedical materialists have focused attention away from abstract psychological complexities to the solid bedrock of neuroscientific reality. Shifting from psychology to neuroscience is an especially important part of the biomedical materialist's professional identity. I will further clarify this point in the following section where I examine the history of biomedical materialism.

4. A Short History of Biomedical Materialism

Since the time of Francis Bacon (1561–1626), Western thought has viewed intellectual progress and the growth of knowledge to be progress and growth toward an increasingly scientific understanding of phenomena. From Ptolemy to Copernicus, from The Book of Genesis to evolutionary theory, from demonology to microbiology, we believe that science has given us truer and more useful beliefs about the world.

The experimental study of physiology did not become an integral part of the scientific picture until the 19th century. When physiology became an experimental discipline in the middle of that century, an observation-based view of organisms replaced a speculative view of bodies animated by spirits. For example, work by Galvani, Dubois Reymond, and Helmholtz demonstrated that nerve impulses are electrical and that the electrical impulse has a measurable speed. Their studies put an end to Descartes' view that nerves were hollow tubes through which animal spirits flowed. The progenitors of contemporary biomedical materialism did their writing in the context of this new experimental physiology.

Although biomedical materialism is grounded in the *evidence* supporting the brain as substrate thesis, the movement itself began with *speculation* about the brain's being the substrate. A prominent thinker in this tradition was the anatomist F. J. Gall (1758–1828) who advocated the neuroscientific hypothesis that personality is a function of brain anatomy. Since different brain structures hypothetically accounted for different aspects of personality, measuring and mapping these structures became important. On the philosophical side, Gall conceptualized personality as a collection of mental faculties. His neuroscientific hypothesis and his philosophical theory combined to form the phrenologist's research program.

Before Brodmann described the cellular architecture of the brain, our understanding of neuroanatomy was limited to visible structures such as various fissures and gyri. According to the phrenologists, relative sizes of these brain structures were correlated with individual differences in personality. The phrenologists also hypothesized that brain anatomy would be reflected in the shape of the skull, so researchers began measuring bumps on the skull.

Since these hypotheses were often assumed to be true without being subjected to experimental tests, phrenology is associated with 19th-century pseudoscience. As is well known, the idea that bumps on the skull are correlated with brain shape was mistaken. But the primary thesis that gross anatomy correlates with psychological function is still prevalent among some biomedical materialists. For example, the psychiatrist Nancy Andreasen (1984) claims that as our understanding of localization of function progresses, we will begin to speak of "psychoanatomy" (p. 92).

By the mid-19th century, experimentally-minded thinkers hoped to create a psychiatry having a firm basis in anatomy and physiology. Ellenberger (1974) writes that before it became an independent discipline within medicine,

scientific psychiatry underwent a conceptual shift, giving up the humoral theory of mental disorders for a theory of disease rooted in disturbances of the nervous system. It was in this systematic disease framework that William Cullen coined the term “neurosis.”

In psychiatry, as elsewhere, the financial support provided to universities for research and graduate education in German-speaking countries made them the world’s leaders in scholarly activity. In the mid-19th century the leading university-based neurologist¹ was Wilhelm Griesinger. In 1845, Griesinger declared that mental disorders are brain diseases. According to Shorter (1997), Griesinger established the tradition of teaching psychiatry based on both scientific research and professional practice. He also firmly believed that psychiatry should be re-integrated with the rest of medicine. Pichot (1983) notes that Griesinger was originally the leader of a smaller group of psychiatrists called the somatocists, but his phrase about mental disorders being brain diseases became a credo accepted by the whole German school of psychiatry.

A sampling of statements made preceding and during this period can hint at the flavor of early psychiatry. Scull (1979) quotes an 18th century medical journal: “Insanity is purely a disease of the brain. The physician is now the responsible guardian of the lunatic and must remain ever so” (p. 165). Benjamin Rush, the founder of American psychiatry in 1812 claimed that “The cause of madness is seated primarily in the blood vessels of the brain” (Shorter 1997, p. 15). In 1869, the American George Beard wrote that neurasthenia (anxious fatigue) was caused by “dephosphorization of nervous systems” and M. Krishaber, in 1873, termed an anxiety state, “cerebrocardiac neuropathis” (Wolman 1984, p. 46). In 1912, Southard wrote that “tangles and twists” in the mind are relatively unimportant and psychiatric attention should be directed at “blots and spots” on the brain (Saas 1992, p. 378).

Constructing explanations exclusively within the framework of biological variables was important to early psychiatrists, as seen in the following 1874 statement by the British psychiatrist Maudsley:

It is not our business, it is not our power, to explain *psychologically* the origin and nature of any of [the] depraved instincts [manifested in typical cases of insanity] (quoted in Sass 1992, p. 378).

1. At this time, psychiatry and neurology were not distinct specialties. Shorter (1997) points out that psychiatrists (alienists) were originally concerned with “madness,” leaving milder neurotic disorders such as anxiety and compulsiveness to neurologists.

The terminology has changed and the knowledge-base has expanded considerably, but many of these opinions are reflective of positions taken by contemporary biomedical materialists. Shorter's (1997) *History of Psychiatry* even claims that it was neurologists who began using psychotherapy as a mode of treatment, whereas psychiatrists (or alienists) were too committed to their biological orientation to see the benefits of attending to psychological factors.

Even though there are many similarities between these mid-19th century views and contemporary psychiatry, the most important thinker of this period, with respect to his influence on biomedical materialism, was Emil Kraepelin. He was important for several reasons. First, Kraepelin introduced systematic medical diagnoses into psychiatry, specifically classifying groups of symptoms into common syndromes. He conceptualized these syndromes as distinct disease entities. Second, in 1883 he wrote the first edition of an influential textbook, called *Handbook of Psychiatry*, in which he proposed that the syndromes identified as mental illnesses are the result of heredity, chemical imbalances, and metabolic irregularities. This book went through nine editions and was the standard text for years. Third, as an administrator, Kraepelin built a department of psychiatry at Munich composed of historically distinguished neurologists such as Brodmann, Nissl, and Alzheimer.² Although he was more interested in understanding the prognosis rather than the cause of a particular problem, Kraepelin provided an institutional structure for the development of the biomedical model.

Kraepelin offered physicians labels for categorizing mental illnesses into schizophrenic and manic-depressive types, but he was unable to provide useful treatment suggestions. Since there were no identified chemical imbalances, the hypothesized "causes" in Kraepelin's theory were not much more confirmed than the classical theory of humoral imbalances. In addition, the medical treatment of psychiatric disorders, which included practices such as purgatives, bloodletting, and emetics had not been proven successful. Especially in the United States, because the more treatment-oriented psychoanalytic perspective dominated the clinics, physicians employing a biomedical model were in the minority.

The widespread importance of the biomedical perspective for physicians in general can be dramatically illustrated by the extent to which the most

2. Shorter (1997) claims that Nissl and Alzheimer popularized the neurology-psychiatry distinction. They spent years looking for the anatomical basis of specific disorders, and those that could not be given a clear anatomical basis were classified as belonging to psychiatry.

psychological of thinkers, Sigmund Freud, was also committed to preserving the biological basis of scientific medicine. Sulloway (1979) quotes from a letter Freud wrote to Jung, complaining about Bleuler's willingness to accept a psychology without sexuality. "In the sexual process we have the indispensable "organic foundation" without which a medical man can only feel ill at ease in the life of the psyche" (p. 90). This was not a view he abandoned. In 1938, near the end of his life, Freud wrote "the future may teach us to exercise a direct influence, by means of particular chemical substances, on the amounts of energy and their distribution in the mental apparatus"(p. 182). Even for Freud, the basic tenets of biomedical materialism made sense, but the evidence favoring an adoption of the model was absent.

The development of biomedical treatments. As we have already discovered by examining general paresis, the impotence of biomedically-based treatments was short lived. Among the first attempts to apply the general paresis model for treating the mind by treating the brain were insulin coma therapy (Kalinowski 1975), electro-convulsive therapy (Pulver 1961), pre-frontal lobotomies (Freeman & Watts 1942), the manufactured sedative chloral hydrate (Shorter 1997), and the prescribed inhaling of chemicals such as nitrous oxide, carbon dioxide, and ether (Ludwig & Surawicz 1975). Barbiturates came into use at this time as well. These treatments met with mixed success. The fortunes of biomedical materialism as a general approach to practice did not begin to gain momentum until chlorpromazine (Thorazine) was offered as a treatment for severe psychiatric problems in the 1950s.

Braden-Johnson (1990) states that in 1937, French physicians isolated the synthetic antihistamine phenothiazine called promethazine in order to relieve severe allergies. Promethazine's most noticeable effect was to make people less anxious. Although extremely calm, people taking promethazine were not sedated. According to Barton (1987), the surgeon Henri Labroit used it on his patients to prevent blood pressure from dropping during surgery, and hence reduce shock. Attempts to construct variations on the chemical structure of promethazine by the chemist Paul Charpentier led to the synthesis of the phenothiazine called chlorpromazine, which was observed to have even greater calming effects on patients (as reported by Labroit in 1951).

Because it calmed but did not sedate, Labroit began suggesting that chlorpromazine be tried on psychiatric patients. An early attempt by some of his colleagues to treat a manic patient was unimpressive. As the news of Labroit's drug spread through medical circles, the psychiatrists Jean Delay

and Pierre Deniker tested chlorpromazine on agitated psychotic patients. Their clinical trials indicated that, in addition to calming people down, chlorpromazine initiated a marked reduction in florid psychotic symptomatology such as hallucinations and delusions.

In 1954 chlorpromazine was marketed in the United States under the trade name Thorazine as a treatment for mania, depression, schizophrenia, obsessive-compulsive disorder, and hypochondriasis (Andreasen 1984; Braden-Johnson 1990). Experience has shown that it works best with the schizophrenias. This class of drugs is often associated with Delay and Deniker's label, "neuroleptic," referring to reduction in nervous activity. Although originally marketed by American drug companies as a "major tranquilizer," its targeted effect is the reduction of hallucinations and delusions. (All drugs used to treat psychological disorders by eliminating psychological symptoms are called "psychotropic" drugs.)

Connections between the major classes of drugs and particular symptoms were all discovered accidentally. The first of the "tricyclic" antidepressants,³ imipramine, was synthesized in experiments with the chemical structure of Thorazine. These experiments were being performed by a Swiss pharmaceutical company in order to find a new anti-psychotic treatment. The German psychiatrist (and psychoanalyst) Roland Kuhn, who had been searching for a treatment for schizophrenia that was less sedating than Thorazine, found that rather than removing psychotic symptoms, imipramine elevated patients' moods (Andreasen 1984; Barton 1987; Shorter 1997). In 1955, they decided to try imipramine out with depressed patients, and were amazed at its effectiveness.

The effectiveness of lithium as a treatment for bipolar disorder was discovered during the Australian J. Cade's (1949) investigations into the toxic effects of manic patients' urine on guinea pigs. To see if the toxic substance in the urine was uric acid, Cade used lithium urate because it had traditionally been used as a solvent for uric acid. He found that the guinea pigs became very lethargic, and no longer had a toxic response to the urine of manic patients (Kramer 1993). This led him to administer lithium to manic patients. All patients in his original trial were noticeably improved. Maxmen (1985) notes that even though the therapeutic effects of lithium were discovered before the other psychotropic drugs were identified, it was not used commercially in the

3. "Tricyclic" refers to a three carbon ring chemical structure.

United States until 1971.

The pharmacological treatment of psychiatric disorders has progressed considerably since its inception in the middle of the 20th century. According to Braden-Johnson (1990), as early as 1970, psychoactive medications brought 500 million dollars a year to drug manufacturers. Cowley (1994) reports that by 1994, just one drug, Eli Lilly's antidepressant Prozac, achieved worldwide sales of 1.2 billion dollars a year. Whitaker (1992) reports that, in 1990 the *American Journal of Psychiatry* devoted 311.5 of its initial 469 advertisement pages to drug ads. By having treatments which were viewed as effective, the biomedical model could move beyond speculations about possibilities and begin the job of both applying the model and researching its effects. It had reached what Kuhn (1970) calls the stage of normal science. The once absent evidence that led thinkers such as Freud to reject a consistent biomedical treatment philosophy became widely available in the 1970s and 1980s.

5. Contemporary Biomedical Materialism: The Neo-Kraepelinians and Beyond

Besides revolutionizing practice, in the past twenty years (a) the abilities of pharmacological treatments to remove symptoms with increasing efficiency, and (b) the many studies investigating the physiological mechanisms affected by these drugs have created an entire research field. With advancing techniques for studying the living brain, a new generation of biomedical psychiatrists whom Klerman (1978, 1983) named the "neo-Kraepelinians" came into existence. Klerman claims that this movement is exemplified by a concern with psychiatry as a branch of medical science, a commitment to the importance of systematic diagnosis, a belief in the qualitative difference between those who are sick and those who are healthy, and an intention to discover the biological basis of mental illness.

According to Klerman, the neo-Kraepelinian approach was first articulated by Meyer-Gross, Slater, and Roth (1954). In a later work, Slater and Roth (1969) claimed that the "foundations of psychiatry have to be laid on the ground of the natural sciences" (p. 1). In the early sixties, a group of psychiatrists at Washington University in St. Louis, including Eli Robins, Samuel Guze, and George Winokur, developed a similar philosophy. Eli Robins had worked under Stanley Cobb, who Shorter (1997) refers to as the founder of

biological psychiatry in the United States. Blashfield (1982) notes that after Winokur moved to the University of Iowa in the early seventies, the St. Louis/Iowa group became an invisible college in psychiatry. By the phrase "invisible college," Blashfield suggests that these thinkers were connected by a common mission. That mission was to make psychiatry a scientific discipline.

In the broadest sense, the scientific approach to medicine involves (a) the systematic identification and classification of syndromes, including a detailed description of what happens to people who are "infected" and (b) the experimental investigation of the biological basis of the syndrome, beginning with anatomy and physiology, and progressing to biochemistry and genetics. The desired outcomes of this process are verified etiological hypotheses and the development of effective treatments. Treatment is paramount, as is indicated by Guze (1992) in *Why Psychiatry is a Branch of Medicine*:

Like the rest of medicine, biological psychiatry is based on the belief that increased knowledge about the anatomy and physiology of the body, in this case especially the brain, is vital for improved practice (p.56).

Guze states that this assumption is not based on current knowledge as much as it is based on the belief that this way of thinking will lead psychiatrists to discover the relevant knowledge.

Van Praag, Lader, Rafaelson, and Sachar (1979) claim that psychiatry was not a part of the movement in which science became the basis for medical practice in the twentieth century. They state that, instead of being scientists, early twentieth century psychiatrists agonized over questions that resembled "medieval disputations rather than scientific arguments" (p. ix). They cite Watson and Crick's (1968) research into the structure of DNA as an example of a scientific approach. Van Praag and colleagues claim that some farsighted psychiatrists have attempted to keep psychiatry linked with the biological sciences.

One of these psychiatrists, The University of Iowa's Nancy Andreasen (1984) refers to psychiatry as a "prodigal son" which has recently "returned home to its place as a specialty within the field of medicine" (p. 8). She argues that the popularity of psychoanalysis and behaviorism delayed the development of psychiatry as a branch of medicine which specializes in the diseases of the brain. With this statement, we come upon another central tenet of biomedical materialism, i.e. *anti-anthropomorphism* or the devaluation of psychology. In addition to the brain as substrate thesis and the claim that the brain is the organ of the mind, biomedical materialists believe that being more scientific

means being more biological. More biological also means less psychological. According to the biomedical materialist point of view, not only are psychologically-based treatments becoming superfluous, the more we learn about the brain, the more superfluous they will become.

Andreasen's contention that a scientifically inspired focus on the brain entails a shift away from psychodynamic and behavioral psychology has been echoed elsewhere. In 1974 MacAlpine and Hunter wrote:

The lesson of psychiatry is that progress is inevitable and irrevocable from psychology to neurology, from mind to brain, never the other way around. Every medical advance leads to the list of diseases which may cause mental derangement. (cited in Hill, Murray, & Thorely 1986, p. 39)

Describing his own entry into psychiatry, Winokur (1981) reports that, at first, he held a psychosocial perspective on psychiatric illnesses. After being involved in studies indicating that early separation experiences were irrelevant to the development of depression, and that psychiatric illnesses ran in families at a better than chance level, he decided that psychiatric illnesses were best studied via the perspective of the medical model. For Winokur, medical model means physiological/anatomical, not psychological.

Winokur describes how he was influenced by Robins' belief that since psychiatrists were doctors, they should function as doctors. Reflecting on the philosophical/psychological interests that led him and many other physicians to enter psychiatry, Winokur writes that he does not believe he understands the mind any better now than he did before entering psychiatry. He also does not believe that his psychiatric colleagues really understand motivation and the roots of behavior and mental life either, no matter what they may claim.

Winokur's position, especially regarding depression, is that we should conceptualize it as a disease like any other disease. "Only a troglodyte would not recognize that pharmacotherapy is the preferred treatment of depression" (p. 115). After demonstrating how Tolstoy was able to eloquently write about his own depression, and noting that Tolstoy's son considered his father's depression to be, among other things, an expression of disillusionment, Winokur offers his own opinion. He claims that since there was no unhappy event in Tolstoy's life at that time, it is "possible that his state of mind was a simple manifestation of an illness" (p. 2).

To further illustrate his views, Winokur (1981) writes about a psychiatrist who interviewed a woman suffering from anxiety attacks. During the interview, the woman remarked that the psychiatrist appeared less professional

because he had muddy shoes. The psychiatrist interpreted this as a sign of good ego strength and sent her home. On a second visit the woman apologized for having made rude remarks about the psychiatrist's shoes. The psychiatrist concluded that the woman was weaker than he first thought, and fearing decompensation on her part, recommended hospitalization. After a year in the hospital, she was discharged and continued to have anxiety attacks. According to Winokur, "The idea of making a diagnosis on the interpretation of motivation or behavior is foolishness" (p. 147).

Wender and Klein (1981) also use the case history method to describe how a woman with panic attacks went through years of psychoanalysis followed by months of behavior therapy, both at great personal and economic cost, with little improvement. The prescription of an antidepressant allowed her to return to a normal life style within a matter of months. The near immediate efficacy of pharmacological treatment, contrasted with years of protracted and minimally effective psychotherapy, is also described in case studies offered by Kramer (1993). Wender and Klein claim that "Some forms of psychiatric illness should no more be treated by some supposedly well-established therapies than should cancers be treated by faith healers" (p.14).

From the standpoint of intellectual history it is clear that radical behaviorism and biomedical materialism take a similar position with respect to psychology. The radical behaviorists relegated psychological explanations to the status of *ad hoc* hypotheses. For example, Skinner (1956) explicitly argued for the *ad hoc* nature of psychological explanations. He claimed that Freud invented the mental apparatus of psychoanalysis rather than discovered it. Skinner also compared id, ego, and superego to other "metaphorical devices" (p. 78) such as essence, force, phlogiston, and ether, all described as explanatory fictions. He claimed that metaphorical constructs have caused trouble in the past and continue to do so, and suggested that these metaphors create problems which are solved by the invention of new constructs. The new constructs create their own unreal problems for which we have to invent more unreal constructs. According to Skinner, in addition to theoretical constructs such as the ego, we can also call into question the reality of sensations, ideas, feelings, and other states of conscious experience.

Andreasen's (1984) definition of psychological or social concepts such as "loss of ego-boundaries" and "lack of self esteem" as "metaphors used to describe biological processes" (p. 132). is congruent with Skinner's position on the reality of psychological states. Although they differ with respect to

what kinds of observations constitute the foundation of scientific evidence, both radical behaviorists and biomedical materialists subscribe to a scientific realism that includes a thoroughgoing anti-anthropomorphism.

Biomedical materialism has progressed beyond the small group of beleaguered neo-Kraepelinians to arguably become the dominant perspective in American psychiatry. As biomedical psychiatry gained prominence in the 1960s and early 1970s, the psychoanalytic model was still the dominant approach. Under the rule of psychoanalytic hegemony, many thinkers wrote off biological treatments in an *a priori* manner. Since that time, the reputation of the psychoanalytic approach has been so eroded that, currently, some psychoanalysts are fighting to make their perspective relevant within the hegemony of the biomedical perspective; e.g., in Gabbard's (1990) article about psychodynamic psychiatry in the "decade of the brain," he claims that one value of psychological approaches is that they can help people be more compliant with respect to taking their medicine.

Further evidence of the growing influence of biomedical materialism in psychiatry can be found in the 1994 revision of *The Diagnostic and Statistical Manual of Mental Disorders* (DSM) which is the official diagnostic system of the American Psychiatric Association. Previous editions of the DSM made a distinction between organic and functional disorders. "Organic disorders" referred to disorders that were the result of a physical cause, such as a brain disease (e.g., Alzheimer's disease). "Functional disorders" referred to disorders involving an interaction between the organism and the environment (e.g., Posttraumatic Stress Disorder). In the DSM-IV the organic/functional distinction was eliminated. What use to be called organic disorders are now called "disorders due to a general medical condition." The organic-functional distinction was rejected in the DSM-IV because it implied a distinction between mental and physical disorders, referred to in the manual as a "reductionistic anachronism of mind/body dualism" (p. xxi). Apparently some psychiatrists would have preferred to forgo the term "mental disorder" altogether, but no acceptable alternative could be found.

The biomedical psychiatrists make the importance of the brain and its relation to science explicit. Adopting an objectively inspired scientific realism leads them to define biological concepts as the ultimate evidential basis of psychiatry. They consider psychological concepts to be secondary at best and explanatory fictions at worst.

Yet being "scientific" is not the most persuasive argument the biomedical

materialists offer in support of their position. Their most persuasive argument relates to the fact that they are physicians and that their task is to alleviate suffering. It is to this part of the argument for biomedical materialism which I now turn.

6. Biomedical Psychiatry and Humanitarianism

In what she labels a “social manifesto,” Andreasen (1984) opens *The Broken Brain* with a moving appeal to our humanity. She describes how the mentally ill have been punished, tortured, locked up and killed throughout history, claiming that these abuses resulted from explanations of the illness in terms of possession by the devil and weak wills. Misunderstanding the true nature of mental illness has led us to react to the mentally ill with “fear, embarrassment, shame, and guilt” (p. 1). Andreasen argues that even though we no longer use punitive physical methods for controlling the mentally ill, we still engage in subtle versions of stigmatization and prejudice.

Andreasen (1984) tells the story of a pediatrician who experienced a serious bout of depression initiated by loss, including the deaths of his father and his first wife, rejection and abandonment by his second wife, and rejection by his professional community. His professional community ironically rejected him because of his depression. As a result, he committed suicide. According to Andreasen, if this man had been stricken with cancer or a heart problem, his family and friends would have surrounded him. Because he had a mental illness, they rejected him. She writes that if we could learn to see that mental illness is an illness such as cancer, our tendency to blame the mentally ill for their problems would be attenuated. If society would adopt the model of medical science, people with mental illness would more likely be treated with the compassion and understanding they need in order to get better.

Other writers have offered similar analyses. Writing about schizophrenia, Torrey (1983) seeks to dispel the myths and “alleviate the millstone of guilt which families have been condemned to carry by mental health professionals” (p. XV). Torrey contends that we have to trade our mystical mentality and our heritage of “examining entrails” (p. XV) for a view of schizophrenia as a brain disease. He claims that non-biological analyses have led to extreme guilt and blame, with depression, divorce, and suicide being the result. He refers to non-biological perspectives on schizophrenia as the “Original Psychiatric Sin.”

According to Torrey, if people would be willing to learn about the inner workings of the schizophrenic brain, they could trade fright for sympathy and concern.

Biomedical materialists also believe that misunderstanding the nature of psychiatric disorders influences public policy in a harmful way. For example, Guze (1992) claims that people with psychiatric disorders are discriminated against with respect to health insurance. He asserts that, compared to other illnesses, health insurance companies fail to provide adequate coverage for psychiatric disorders. This policy results in higher deductibles, higher co-payments, fewer days of hospitalization, and fewer outpatient visits. He claims that the insurance companies are able to discriminate against patients with psychiatric disorders primarily because the general public believes that psychiatric disorders are not real illnesses. According to Guze, it makes no sense to discriminate against patients whose hallucinations and delusions are the result of a brain disorder called schizophrenia while at the same time providing for patients whose hallucinations and delusions are the result of encephalitis. He argues that if psychiatric disorders were recognized as true illness, the insurance companies would have no justification for their policies of discrimination.

In addition to blaming patients, when attributing causes to psychosocial events, we also implicate their families. Torrey's (1983) *Original Psychiatric Sin* refers to the stigmatization of the family as much as it refers to the stigmatization of the patient. Writing about depression, Winokur (1981) says that the family should not take the blame for the illness because there is no reason to believe that the family did anything to cause the illness. Although, in ignorance of the organic nature of problems it may be natural to explain them psychologically, Winokur thinks such explanations are wrong.

According to the biomedical materialists, it is inhumane to not realize when problems are biological in nature. All we need do is examine a problem having an obvious biological basis to see how correct they are. For example, many people with traumatic brain injuries (TBI) have serious short term memory problems. A TBI survivor can easily forget what happened only minutes ago.

As part of her rehabilitation related to a traumatic brain injury, Sally was learning the city bus system. One particular day, when she and her counselor came to her usual bus stop, the concrete was torn up and under repair. Because time was short, they quickly walked a half a block down the street to an

alternate bus stop. After arriving at the new stop, Sally sat down to write herself a note. About one minute later she looked up in confusion and noted with some alarm that they were not at her bus stop. When asked to remember why they were not at her stop, Sally thought for a few seconds but found no answer. After looking down the street and seeing the construction work, she remembered why they were at a different stop.

Sally was not lazy, preoccupied, emotionally upset or in a state of general confusion. Her memory loss was not an example of the everyday absent-mindedness seen in persons without a brain injury. The mechanisms in her brain responsible for storing or retrieving short term memories were simply not functioning adequately. They will probably never function adequately again. Expecting her to remember non-routine facts is unrealistic, and blaming her for forgetting them would be a mistake.

When we attribute causes to impersonal physical mechanisms, people are left free of blame. For example, we don't blame people for their leukemia. To do so would be unjust. Leukemia is a condition thrust upon them. If someone with leukemia is too ill to function, telling them they have to get better or live with the consequences would be cruel. Exploring what they did to create the leukemia or exploring the subjective reasons they may have for being ill would be terribly insensitive on the part of a medical professional. These fundamental principles about the *sick role* have similar implications for our attitudes toward the mentally ill if we view them as also having a disease.

A second dimension to the biomedical materialists' humanitarian analysis is offered from within the framework of their professional mission: to create a psychiatry that is an integral part of scientific medicine. That scientific approaches to solving problems in medicine have been successful is important to them. Their examples of the successful application of scientific medicine includes the research which isolated the polio virus and led to a means for immunizing human beings against this virus. The acknowledged importance of research into the AIDS virus is a contemporary example of scientific medicine's relevance to our well-being. Experimentation, replication, and systematic elimination of possible alternatives in solving problems have made medical science a success.

The biomedical materialists believe they have an ethical responsibility to apply the scientific model to their own professional problems and they have great hope for the future of their discipline, as seen in this quotation from Nancy Andreasen (1996)

Psychiatry is currently the most pioneering and challenging discipline within medicine. We have made enormous strides in disease definition and in treatment during the recent decades, and the care we give our patients is vastly better than it was 50 years ago. Accomplishments equivalent to the discovery of insulin in the 1920s or penicillin in the 1940s are still before us and will change the lives of our patients just as those discoveries did. (p.2).

The biomedical materialists believe that physicians should approach psychiatric problems as they would other medical problems, focusing on their scientific assessment and treatment. The ideal biomedical psychiatrist communicates to his or her patients a belief that their problems are the result of a brain disease and, although they are very upsetting, such problems should not be used as grounds for blaming either themselves or other people in their lives.

The current practice approach of many psychiatric consultants is as follows. An initial interview may last up to two hours, allowing the psychiatrist to determine which diagnostic category the person's symptoms fit into, followed by the prescription of appropriate medication and a possible referral. Follow-up interviews with the psychiatrist can occur anywhere from twice a month to twice a year. These interviews last fifteen to thirty minutes and include direct questions about symptoms and side effects, possibly resulting in an alteration of the dosage or sometimes a change of medication. The interview may last longer if the patient presses for attention and the psychiatrist is willing.

While taking a client to a psychiatric clinic for a voluntary hospitalization, I once had the opportunity to observe part of a psychiatric consultation. A husband and a wife, both in their thirties, had already been through a diagnostic interview. As they were sitting in the waiting room, their psychiatrist sat down with them and related some information about obsessive compulsive disorder, which he called "OCD." He also said he was going to write a prescription. The psychiatrist explained that this medicine was usually prescribed for depression, but it had been found to be very effective with OCD.

He was matter of fact, but respectful. His assurances seemed to give the couple hope: "With medication you should have no further problems." The husband inquired if this was something his wife would be on for a long time, and the psychiatrist, with a slight nod of his head, indicated that she would. He told them that this kind of treatment was normal and many people had followed it, so it was nothing to catastrophize about. After talking about participating in a study which would provide them cheaper medication, and the possibility for a behavior therapy referral, the psychiatrist made an appointment for them to come back in one month and sent them on their way.

An important advantage to this approach from the perspective of biomedical science is that, once a treatment is prescribed, if the patient follows the treatment regimen, success or failure is attributed only to the treatment. Success or failure in psychotherapy is dependent on many factors in addition to the treatment. In psychotherapy, if the treatment didn't work, it is because the person wasn't ready to do the work, or they were not psychologically minded, or they couldn't develop a trusting relationship with the therapist allowing them to talk about their deeper concerns, or their problems had secondary gain value, or the people in the person's life had a need to keep them sick. When failures occur, people are often told that they may be more able to do the kind of work they need to do at some future date, or that they may find another therapist who is a better match for them. These explanations subtly blame people for not getting better. Such excuses are not available to the biomedical practitioner. The more distant relationship between biomedical practitioners and their patients can, paradoxically, be more respectful.

Because the humanitarian argument offers extremely persuasive reasons for favoring the biomedical approach, let me summarize it before we move on. Medical science has shown that many psychological symptoms are the result of organic dysfunction, and future advances in medical psychiatry should uncover the biological basis of other psychological symptoms. To the extent that psychological explanations confuse results with causes, they are no better than explanations in terms of demons and other mystical entities. With greater biomedical understanding, instead of attributing the responsibility for illness to a person, we will be able to attribute responsibility to impersonal brain mechanisms. This would be an important advance for two reasons, (a) it is cruel and unjust to blame people for something for which they are not responsible and (b) having an accurate true understanding of the nature of the pathological process along with effective treatments can go a long way toward eliminating suffering on the part of those people whose lives are afflicted with psychiatric disorders.

7. The Anti-psychiatry Critique of the Biomedical Model

The biomedical model has not gone uncriticized by other physicians. One group of critics, called the anti-psychiatrists, became prominent in the early 1960s. They were held together by their opposition toward the *medicalization*

of the mental. In order to provide a complete argument for biomedical materialism, we need to understand its response to the anti-psychiatrists, which I think has been successful. I will briefly summarize the two main hypotheses of the anti-psychiatry movement regarding the unreality of mental illnesses, and then review the biomedical materialist's response to them.

The first anti-psychiatrist hypothesis stated that the attribution of a mental illness to someone is social in nature; more specifically, mental illness is socially constructed solely on the basis of arbitrary opinions about what constitutes "normality." The anti-psychiatrists generally considered mental illness to be a synonym for unconventional. On the outermost end of this movement, Szasz (1961) argued that mental illness is nothing but a value-laden code word for behavior that contradicts conventional social norms. Szasz said that there is no identifiable (objective) disease in schizophrenia as there is in cancer. He also makes a point that biomedical materialists might be inclined to agree with; that is, if what we call mental illnesses are diseases of the brain, then they are not *mental illnesses*, and probably need to be given back to the neurologists. Strictly considered, there are no *mental illnesses*.

The second anti-psychiatrist hypothesis stated that if there is something which the people having mental illnesses contribute to the generation of a mental illness, whatever they contribute is not related to a biological disease process. The sociologist Scheff (1966) and the psychologist Rosenhan (1977) thought that the mentally ill do contribute something, i.e., a reaction toward being labeled mentally ill. Laing (1967) considered mental illness to be a protest and rebellion against social lies (e.g., the "happy" family). He further stated that, rather than being a mental disorder, schizophrenic behavior is a psychological strategy for coping with an insane world.

Biomedical thinkers find little validity in either of these arguments. As we have seen in reviewing evidence for the brain as substrate thesis, several "insane" behaviors have been related to general disease processes, and there is evidence that there are more specific disease processes not yet identified. The patterns found across many genetic studies have also convincingly demonstrated to biomedical materialists that there is an inherited component to schizophrenia and depression. (Kandel 1991a, 1991b; Kety, Rosenthal, Wender, Schulsinger, & Jacobsen 1975).

Furthermore, arguing that psychiatric diseases are social constructions does not necessarily lead to the conclusion that they are myths. Klerman (1978) points out that all medicine is based on the "disease concept" and the

“sick role,” both of which are social constructions. These concepts are just as valid as are other social constructions such as “the rights of man”, “the electron”, and “the University”. Klerman’s response is very damaging to antipsychiatry. He agrees with their social constructionist analysis, but claims that such an analysis does not distinguish psychiatry from the rest of medicine.

Roth and Kroll (1986) argue that physicians have been willing to treat certain syndromes as diseases even before lesions were identified, for example, diabetes and epilepsy. According to the anti-psychiatrist’s model, before the discovery of the HIV virus, AIDS could not be validly called a disease. Noting that there are syndromes which were once considered to be the result of specific lesions and are not considered so today, for example — anorexia nervosa is no longer considered a pituitary disease, Roth and Kroll state that the boundary between disease and non-disease is more indefinite than Szasz’s dichotomous thinking admits.

The anti-psychiatrist’s analysis can be questioned for other reasons as well. Almost anyone who has had an opportunity to observe a large number of persons with mental illness realizes that mental illness is something more than a protest against convention (Kay 1991). Whether it be schizophrenia, an affective disorder, or some kind of serious personality disorder, there can be a great deal of pain involved. Guze (1992) claims that psychiatrists who spend their lives working with psychiatric patients and their families consider the labeling theory “fundamentally ludicrous” (p.14).

To the extent that social reality is a function of consensus, people who question convention are often labeled as strange, absurd, irrational, and even unstable. Like such “strange” and “absurd” individualists, the behavior of some people with mental illness also contradicts prevailing social norms (for various reasons). Leaping from the observation that protesters are judged as weird, to the claim that all bizarre behavior represents a protest, is a logical fallacy. The syllogism, which can be considered faulty due to an undistributed middle term or the fallacy of affirming of the consequent, looks like this.

1. Social protestors are weird and unconventional.
2. This behavior is weird and unconventional.
3. This behavior is a social protest.

This syllogism takes the same form as: *Elephants have big ears; Donkeys have big ears; Therefore donkeys are elephants*. Both are equally fallacious.

An alternative explanation for the unconventional behavior exhibited by

persons with schizophrenia is that they sometimes lack the social skills to know how to conform. A point overlooked by some anti-psychiatrists is that most mentally ill people do not even begin to question conventional institutions such as their religion, their nation, or their culture.

Contemporary proponents of the antipsychiatry movement claim that biomedical treatments do not always work and that they have unpleasant and dangerous side effects (Breggin 1979, 1983, 1991; Johnstone 1989), but biomedical materialists can point out that such problems are common to all medical specialties. Physicians do not yet have effective treatments for most types of cancer, AIDS, Alzheimer's disease, or muscular dystrophy, and the medications used to treat many disorders have unpleasant side effects. People do not enjoy undergoing chemotherapy or taking AZT and protease inhibitors. Physicians are always working on removing these side effects, but the presence of unpleasant consequences is endemic to medical treatment. Biomedical materialists can also argue that the treatment of psychiatric disorders can be more realistically compared to the treatment of diabetes than it can be compared to the treatment of small pox. Physicians can manage diabetes, but they can't cure it. The same is true for certain kinds of depression and most psychotic states; they can be managed but not cured.

With respect to questioning the medicalization of the mental, biomedical materialism has survived the attack from antipsychiatry. As a matter of fact, with the current biomedical dominance of the field, the antipsychiatry movement has been marginalized to the point of virtual nonexistence.

8. Conclusion

Here ends my definition of the basic tenets of the biomedical materialist paradigm. From both a scientific and a humanitarian perspective, the biomedical materialists offer compelling reasons for believing what they do. Upon finishing this chapter, those readers favorably predisposed toward embracing scientific and biological explanations as the best kinds of explanations can understandably feel confident. Biomedical psychiatry may be just another step forward in the long history of scientific progress. Those readers who are suspicious of the biomedical model have probably at least experienced some self-doubt, asking themselves "what if the biomedical thinkers are right?" They may even have sinking feelings that the biomedical approach is the best

and most responsible approach for psychiatry to take. There are so many reasons supporting biomedical materialism that the committed mentalist has to feel a little overwhelmed.

Unfortunately for the beleaguered mentalists, the argument for materialism is just beginning. Many of the biomedical materialists' beliefs are implicit rather than explicit. They are acted on rather than argued for. This is not the situation in philosophy, where beliefs have to be given detailed and logical justifications. The eliminative materialists in philosophy advocate neurophysiological explanations and assail psychological explanations with great force. Their arguments are complex in nature and explicitly made.

In 1939 the psychologist S.S. Stevens observed, with some amazement, that natural scientists in the form of physicists (operationalists), philosophers (logical positivists), and psychologists (behaviorists) had begun to engage in a common effort with respect to understanding the nature of science. Calling this a triumph of self-consciousness, Stevens took the agreement among these separate disciplines to suggest that, although each movement was just an optimistic program, they were on the right track. In many ways this situation is being repeated today, except that the protagonists engaging in the common effort are neuroscientists, eliminative materialists, and biological psychiatrists/psychologists. It is to the arguments of the philosophers that we now turn.

CHAPTER 3

Trouble From Philosophy

Eliminative materialism

1. Introduction

As with the biomedical materialists, in this chapter I want to convey to the reader why the eliminative materialists believe that their position makes sense. Their thesis that folk psychology is nothing more than a theory, and one that is false at that, may seem absurd to some, but it is the result of very careful thinking drawn from two generations of investigation in the philosophy of science. The eliminativists have complex reasons for saying what they do. Their claim that psychology can and must be replaced by neuroscience is not as implausible as it may appear. As a matter of fact, as neuroscientists construct more sophisticated models, the eliminativist position becomes more persuasive.

Philosophy matters. In many ways, what makes the work of the biomedical materialists so interesting in the first place are the implicit “big questions” they address. Because the big questions (e.g., do we have “minds,” are psychological explanations justifiable, what does it mean for a concept such as “ego strength” to be about something real) are also important questions, psychiatrists and psychologists should not ignore the work of those thinkers who specialize in studying them. Philosophical analysis is an important part of developing a more complex understanding of human behavior.

2. A Brief History of Eliminative Materialism

The *radical behaviorists* in psychology such as Skinner (1938, 1956) held that a scientific explanation of behavior should not include references to unobserv-

able entities residing in the head, such as thoughts, beliefs, desires, and intentions. They also considered such “mentalistic” terms to be explanatory fictions.

Challenging radical behaviorism with respect to “sensations” — for example, feeling pain or seeing red — were the *mind-brain identity theorists* (Feigl 1950, 1958; Place 1956; Smart 1959). Proponents of the identity theory held that sensations could be given an objective (third-person) description if we considered them to be identical with brain states. The identity espoused by the identity theorists was a contingent identity — an identity discovered by science. In their view, we could learn that sensations are brain states just as we learned that salt is NaCl. From this perspective, sensations as brain states occur in the head and can be understood to be causes of behavior. By identifying the mind with the brain, identity theorists made the mental a legitimate cause of behavior.

This neat solution did not last. For example, Herbert Feigl, one of the leading identity theorists, was a classical empiricist, and therefore believed that sensations such as seeing red or hearing a bell were the ultimate kinds of evidence, the foundation of all observation. He considered this kind of evidence to be *self-evident*. For example, we don’t ask someone for their evidence supporting a claim that the grass is green.

This version of classical empiricism was critiqued by the philosopher Wilfrid Sellars (1956), who claimed that the classical empiricist’s self-evident propositions such as “the grass is green” depended on a host of corollary assumptions. Because those assumptions could be called into doubt, neither they nor anything depending on them could be called self-evident.

More specifically, Sellars admitted that we do have inner episodes such as sensations of red and green. He agreed that we know our own sensations better than anyone else knows them, but he also thought that these inner episodes were not just given to us in experience; so we could be mistaken about them. For example, to make an observation report such as “the grass is green” I have to know a lot of other things as well, such as the difference between something’s really being green or just looking green.

Furthermore, an internal state, such as the state our brains are in when we experience green, is not an object of knowledge. An object of knowledge is something we report in propositional form, such as “X is Y.” Propositions depend on concepts — and concepts are learned. So all observation reports, such as “this is green” depend on concepts, and we can be mistaken about any conceptual entity

A third way Sellars put it was to say that the notion of “a green sensation” as a mental entity that I perceive is analogous to an “electron” in physics — it is a theoretical construct. It is not just given in experience.

These arguments became important in philosophical psychology because many defenders of psychological explanation claimed that the existence of beliefs, desires, and pains as causes of behavior is self-evident. Philosophers, however, had come to believe that very little is self-evident. If something as basic as a report of a sense experience, such as reporting that the grass is green, requires concepts that can be called into question, then reporting what psychological states we are experiencing and knowing that these states are causes of behavior may also depend on concepts that can be called into question.

In the 1960s, Sellars’s ideas were used by the philosophers Paul Feyerabend (1962, 1963a, 1963b) and Richard Rorty (1965) to further undermine the traditional understanding of mental states. Following Descartes, most philosophers considered mental states such as feeling pain to be *non-material*, *non-spatial*, known best by the person who has them (*private*), and known immediately (*non-inferential*). They were also considered to be *incorrigible* (something you cannot be mistaken about). For example, my pain is mine, not yours; I do not have to think about being in pain, I know it as soon as it happens; and I am the best source for deciding if I am in pain or not. Feyerabend and Rorty both claimed that it is possible that the “meaning” of mental terms could change so much that they could eventually be seen to be *material*, *public*, *inferred*, and *corrigible*. If so, then they really could be brain states.

Feyerabend was a student of the history of science who studied periods in physics when old theories were replaced by or reduced to new theories. Before Feyerabend, and following Nagel (1949), most philosophers believed that when old theories are reduced to new theories (e.g., Galileo’s and Kepler’s laws being reduced to Newton’s laws), scientists can deduce the explanations of the old theory from the principles of the new theory. In order for this derivation to be logically valid, the meaning of terms from theory to theory has to remain stable. Feyerabend (1962) labeled this the doctrine of “meaning invariance,” a doctrine which he rejected. According to Feyerabend, rather than being fixed or stable, meanings change all the time. As a matter of fact, advances in knowledge consist in changes of meaning.

For example, Feyerabend showed that Galileo’s and Kepler’s laws cannot be deduced from Newtonian principles because some of their terms, such as “acceleration” have incompatible definitions — they do not mean the same

thing. Furthermore, the Newtonian concept of “mass” (a constant property: $F=ma$) was changed so much by Einstein’s theory (a relative relation $e=mc^2$), that Einstein’s notion of mass would not make sense to someone working within the Newtonian framework. Feyerabend was particularly found of these kinds of revolutionary changes.

In philosophical psychology, one of the most-used arguments supporting psychological explanations is that since we have been using beliefs, intentions, and desires to understand ourselves for thousands of years, these concepts have a robustness, sophistication, and richness that cannot be matched by sterile materialistic accounts. For Feyerabend, this defense reduces to a conservative claim that psychological explanation is superior because we are familiar with it. He thought that because we are so familiar with thinking about ourselves psychologically, we are prejudiced against potential alternative ways of understanding and explaining our behavior. Furthermore, some of these alternative explanatory frameworks may be so different from psychological explanation, that they would not make any more sense to us than Einstein’s concept of mass as a relative property would make to a Newtonian. Even if they were better.

In this same tradition, Rorty (1965, 1970) distinguished between the disappearance form of the identity theory and the translation form of the identity theory. In the disappearance form of the identity theory, once mental states are understood to be identical with brain states, talk about mental states can just disappear and be replaced with talk about brain states.

More specifically, Rorty argued that common sense would have us believe that sensation terms such as pain refer to mental entities which are naturally given to us in experience. He claimed that although we currently understand sensation reports as referring to mental entities, we could come to see them as referring to brain states — especially if physiological rules about pain improve our ability to categorize its different facets. If this happened, we would come to believe that mental entities never existed.

According to Rorty, we all agree that a witch doctor who reported seeing demons after taking peyote would really be referring to the content of a hallucination, which is a brain state. If the witch doctor could be helped to see this as well, he would come to believe that the demons never existed. Both the content of the report and the referent of the report would be understood in terms of brain states. As it is for demons, so it is for mental states. We could give up talking about pains and start talking about the firings of C-fibers. Once

we learn to do this, we could come to believe that pains as mental entities never existed. Rorty holds that although this change in the way we talk may never happen, it is a logical possibility.

Both Feyerabend and Rorty were tentative proponents of eliminativism. Before espousing his materialist critique of the “common idiom”, Feyerabend suggested only that we have to give a materialist language a chance to develop. Rather than claiming that the vocabulary of introspection is illegitimate and that the materialist vocabulary will triumph, Rorty (1970) merely asserted that, if Sellars’s critique of the Given is correct, the speculation that sensation terms could be replaced by neurophysiological terms is perfectly coherent. He rejected adopting the dogmatic scientific realist stance that sensations are illegitimate and therefore ought to be replaced. As we will see, in Paul and Patricia Churchlands’ account, Feyerabend’s tentativeness and Rorty’s diplomacy have been eliminated.

3. All Language/knowledge is Theoretical

Paul Churchland’s (1979, 1984, 1995) work is firmly placed within the philosophical tradition shared by Sellars, Feyerabend, and Rorty. The basic principles Churchland draws on in articulating his position are (a) the network theory of meaning, (b) the rejection of the theory/observation distinction, (c) the pragmatic/causal theory of observation, and (d) the plasticity of perception thesis. He uses these principles to propose a generalized scientific realism as the framework for an integrated philosophical system. I will briefly describe these principles along with Churchland’s version of scientific realism before turning to his argument for eliminative materialism.

The logical positivists believed that theoretical terms cannot be defined only with respect to observation; they also have to be defined *implicitly* with respect to other theoretical terms. For example, the meaning of “superego” partly depends on knowing something about “the unconscious mind” and “the Oedipus complex” because the meaning of superego is specified in sentences such as “the superego is formed as part of the resolution of the Oedipus Complex” and “the superego is partly unconscious.” In developing this model of how language is structured, philosophers began to describe it using the metaphor of a web or a network.

This doctrine of *implicit definition* creates problems for those who want

to clearly specify the meaning of theoretical concepts such as superego, because related concepts such as the Oedipus complex and the unconscious also get their meaning by implicit definition. An individual theoretical concept is one point on the web, but its meaning has to be understood with respect to other points on the web. The meaning of those “other points” also has to be understood with respect to still other points, so meaning becomes a function of the whole network (or theoretical framework). Rather than being fixed, the meanings of theoretical terms are fluid and dynamic. Not surprisingly, the idea that theoretical concepts are implicitly defined by all the sentences of the theory that contains them is called the **network theory**.

An important consequence of the network theory is that it is difficult to correlate theoretical concepts with observation in a smooth manner. For example, let's say that part of my understanding of depression is that depressed people derive little enjoyment from life. If I observe Joe, who I thought was depressed, truly enjoying a turkey sandwich, does that mean I should start believing that he is getting better? Not necessarily.

Assume I have just read Wender and Klein's (1981) analysis of the difference between enjoying the pursuit of pleasure (the chase) and enjoying the experience of pleasure (the feast). Some people who are depressed lose the ability to anticipate pleasure, so they do not pursue it; but if given opportunities, they may enjoy the experiences presented to them. Not pursuing them, they will rarely have them, but they can enjoy them.

With my expanded understanding of enjoyment, I have to modify my understanding of depression. Enjoyment is part of the implicit definition of depression, and if its meaning changes, the meaning of depression can change as well. That shift in meaning affects whether or not my observation that Joe is enjoying a turkey sandwich is to be taken as evidence for or against his depression. Joe may still be depressed in the same way he was before, but I now know that the relation between depression and enjoyment is more complex than I previously thought.

One conclusion philosophers have drawn from this kind of analysis is that since theoretical expressions do not have their meaning in isolation, they cannot confront observation in isolation. Quine (1961) said that “our statements about the external world face the tribunal of sense experience not individually but only as a corporate body” (p.41). In this model, the meaning of empirical evidence also becomes problematic because the evidence alone does not determine how we evaluate individual hypotheses. As was the case

with Joe's depression, whatever judgement we make about the facts involves a dynamic interaction between our conceptual framework and the evidence.

Those who agree with Quine hold that in understanding the interdependence of theoretical terms with both other theoretical terms and observation, we come to see that there are no logical truths that are complete unto themselves and foundational with respect to the rest of our knowledge. Justifying principles by referring to the meaning of terms, e.g., "mass can't change relative to the speed of an object because mass is a constant," is undermined. Meaning can always be modified in the light of experience.

Equally important to Churchland is the rejection of the logical positivists' **radical distinction between observation and theory** by thinkers such as Hanson (1958). On the logical positivists' own account, since theoretical terms are implicitly defined by other theoretical terms, they cannot be reduced without remainder to observation. The positivists also believed that good theories help us make predictions about new observations.

Extending these principles a bit further, Hanson argues that observation is dependent on what theories we possess. We see what our training equips us to see. To illustrate, both a lay person and a physician can look at an X-ray of a person's chest, but what a lay person sees is a bunch of shadows, whereas a physician sees a meaningful pattern that tells him or her something about the state of a patient's internal organs. The physician has a set of concepts that leads him or her to see something different.

The common sense rebuttal to Hanson's argument is what Suppe (1977) calls the sensory core view. For example, most Americans in the late 20th century know how to read. When presented with the stimuli on this page, they see words and sentences and paragraphs. Someone who does not know how to read could be presented with this page, but they wouldn't see words and sentences etc., they would see a bunch of meaningless lines. In the sensory core view, both the literate and illiterate are presented with the same neutral stimulus. Those who can read draw the inference, "these are letters and words and sentences and paragraphs" whereas those who can't read draw no inferences.

The sensory core view, however, is precisely what Hanson argues against. He claims that in seeing this page, those of us who can read don't draw any inferences; we directly see what is written. We don't observe neutral stimuli. We observe objects — in this case letters, words, sentences, and paragraphs. Objects come to us organized by what we know. There is no such thing as simple generalization from direct experience. Everything is mediated

by concepts, even observation/perception. As Churchland (1979) puts it, the idea that theoretical knowledge is artificial and observational knowledge is natural must be rejected

Something novel happens when we put the network theory and the rejection of the theory-observation distinction together. Hanson shows us that observation is structured by concepts and propositions. As conceptual, observation terms are embedded in a semantic network and they get their meaning from that network. What we know is therefore up for grabs. We can't ground knowledge in the logical analysis of meanings, because meanings are fluid rather than fixed. Part of their fluidity involves matters of factualness (as opposed to logic alone). Nor can we ground knowledge in raw observation because observation is theory dependent, and therefore its meaning is also fluid.

Philipse (1990) points out that these conclusions pose a problem for someone who wants to assert the importance of empirical observation, which Paul Churchland clearly wants to do. The statements of science are supposed to be justified by empirical evidence; that is what distinguishes them from statements about God's intervention in the world. Yet, how can we take observation to be our primary evidence if it has no meaning apart from a semantically unstable theoretical framework in which it is embedded? What criteria do we have for saying that the theories which we call "observation" are more primary than other theories?

One solution to this problem is to adopt a **pragmatic/causal theory of observation**. Observation sentences are sentences we are trained to utter in response to certain sensory states which have been causally induced in us. For example when I learn to say "I feel pain," I am learning to utter a certain expression in response to a certain causal context. One potential causal context is having a needle stuck in your arm and feeling something. As children, we learn to respond to this event with the words, "I feel pain," but what pain means depends on a wider conceptual network. Pains are not in chairs, pains make your eyes water, pains are associated with avoidance behavior, etc. These (interpreted) meanings which are central to pain come later — with the gradual adoption of a theoretical network in which "pain" is imbedded. "I am in pain" is a sentence which we learn to utter non-inferentially in response to certain conditions. If we can all agree that this sentence is to be applied in certain conditions, it can be taken as observational.

What if, instead of saying "I feel pain!," children were taught to say, "C-fiber 241 is firing!?" This would be what they learn to utter in a certain causal

context. We would obviously teach children such expressions based on well corroborated knowledge of neurophysiology. C-fiber 241 firing is not necessarily the same as pain. It still does not occur in chairs and it makes you wince, but it covers only a needle stuck in your arm and does not include headaches, burns or slaps on the face. Those are different C-fiber states. If we learned to report on our experience using such a different framework, what we observe would be different. Paul Churchland calls this the **plasticity of perception thesis**. With respect to meanings, reports about C-fibers would refer to physical C-fibers firings at the same time they refer to subjective experiences.

Defining observation causally is part of Churchland's critique of both classical empiricism and mentalism. For the classical empiricists, observation is direct and immediate, no prior knowledge is needed. Another way of saying this is that the meaning of simple observation is given in sensation, i.e., red means "redness" and pain means "painfulness." The essential meaning of sensations supposedly lies in their "subjective quality." Defining a term by pointing, in this case by pointing to an experience, is called an *ostensive definition*. With respect to sensations, we learn to attach the right word to the appropriate mental state.

For the mentalists, our own mental states, particularly feeling pain or seeing red, are things we know directly. As Feigl expressed it, pain is both evidence and evidenced (empirical data and semantic referent). The important point for mentalists is: "Even if I am able to come up with a neurophysiological correlate of pain, pain known neurophysiologically is mediated rather than direct. As such, objective neurophysiology cannot replace subjective psychology."

One way in which Churchland (1979, 1992) questions definitions made by reference to direct experience is to point out that even the meaning of so called directly known states such as pain are mediated by concepts, seen in the fact that there is too much variation within the subjective quality of our experience of pain to clearly help us specify its meaning. Pain can be stabbing, burning, piercing, etc. Why call all these different things pain? We obviously have reasons for doing so, but those reasons cannot be contained in raw experiences because it is the raw experiences that are different. So the meaning of pain is probably given in a more complicated way than direct ostension indicates. Therefore pain cannot refer just to painfulness. It would be better to say that pain refers to a functional state such as: being injured, feeling bad, and engaging in avoidance reactions.

There is one other important conclusion that Churchland (1979, 1992) draws from these principles. Most of the arguments I have described in this chapter have been made with reference to sensation terms such as redness and pain. Arguing for the theoretical nature of our awareness of phenomenal properties such as color and pain is the hard argument to make, but once it is made we see that both observations of others (you are in pain) and observations of ourselves (I am in pain) have a theoretical character. In addition to knowing about people with respect to their pains and hunger, we also know them in terms of their beliefs and desires, i.e., their propositional attitudes. The behaviorist critique taught us that attributing beliefs and desires to other persons requires making inferences from behavior and therefore has a theoretical character as well. Because knowing about beliefs and desires is also theoretical, all self-observation, first-person and third-person, is theoretical. For Churchland, all the psychological concepts that we use to explain and predict behavior constitute a theoretical framework.

4. Scientific Realism

Paul Churchland (1979) also defends *scientific realism*. Those who subscribe to scientific realism point out that science has been extremely successful, revealing many hidden mysteries to us. We can navigate through the world in a way that we could not have even imagined in earlier times. The best way to account for the success of science over superstition and metaphysical belief is to claim that science has helped us learn about the truth of the world. Another aspect of scientific realism is essentialism, the belief that ultimately we can expect that our theories will correspond to the true nature of things.

Within this general tradition, Churchland (1979, 1992) specifically proposes a post-positivistic scientific realism. His primary philosophical premise is that such a framework will allow us to solve many important problems that have plagued philosophers for generations, including persistent questions in both the philosophy of mind and the philosophy of science.

Realism is an important problem because we don't directly observe electrons, gravity, or ego strength — we have to infer them. Scientific realists say that some of these inferences are true. One basic strategy for defending realism is to claim that terms such as “electron” refer to the causes of our sensations. This is a particularly popular strategy for materialists, who believe

that, ultimately, physical things such as atoms, neurons, tables, and planets are prototype examples of what counts as real.

An important argument in the defense of scientific realism is the claim that the physical is not picked out by pointing to observations that refer to some substantial entity, but is picked out by placing it in a causal chain. If something is either causal or caused, it is tied in with the physical universe. In the end, thinking about theoretical terms as being caused by the world allows us to believe in their reality.

To solve the problem about what observation terms mean if they are not defined ostensively, Churchland applies the answer used to defend realism about traditional theoretical entities to defend realism about traditional observation terms. His point is that a general scientific realism solves both problems. Just as electrons are real because they are caused by something in the world, following the pragmatic/causal theory, sensation is real because it is the end result of impingement of physical objects on various parts of the body.

Both “observation” and “theory” get meaning from a theoretical network, and they both to refer causal situations. Since both are theoretical, any appeal to observation is an appeal to a theory. The primary criterion for deciding about truth and reality is neither logical analysis nor reference to raw observation, rather it is global excellence of the theory. Global excellence consists of qualities such as explanatory power, coherence, and simplicity. Churchland proposes that the most excellent theories we have are the theories of the natural sciences. For Churchland, in order for any psychological theory to achieve global excellence, it will have to cohere with other theories in the natural sciences.

5. Folk Psychology is a False Theory (Eliminative Materialism).

If all knowledge is theoretical, then different genera of discourses can be conceptualized as competing theoretical frameworks. Following Feyerabend and Rorty, Paul Churchland applies these principles to the problem of the mental and the physical. For years mentalists and materialists have argued about how to best describe ourselves. Churchland characterizes this argument as an argument between competing theoretical frameworks, each having a distinct *ontology*. The important question becomes which framework is preferable? The answer is going to be the framework that has the most scope and allows us to do the most. Deciding this is an empirical question.

In setting up their competing frameworks argument, both Paul and Patricia Churchland define two competitors or candidates for self-understanding. The first candidate is neuroscience and the second candidate is what they call “folk psychology.” In addition, and very importantly, both Churchlands hold that folk psychology is a common sense theory and not a scientific theory. The better known aspect of their argument is their claim that the neuroscientific framework is ultimately going to be the preferable one. Their main point, with which most everyone agrees, is that an explosion in our self-understanding in the next few years is going to come from an increased understanding of how the brain works. Similar to the biomedical materialist’s extension of the “brain as substrate thesis” to the “brain is the organ of the mind” doctrine, the Churchlands also extend the “knowledge explosion prediction” into a claim that the reason there is going to be an explosion is because neuroscience tells us about how we really are — it mirrors our essential nature.

Although the Churchlands’ current work involves applying computational neuroscience to specific problems, such as the nature of discrimination, explanation, and moral reasoning, they got to this point by arguing against folk psychology.⁴ Just what is folk psychology? Patricia Churchland (1986) defines folk psychology as generalizations and rules of thumb which we use to “explain behavior as the outcome of beliefs, desires, perceptions, expectations, goals, sensations, and so forth” (p. 299). Most of us would readily admit that in psychological explanations, beliefs and desires are causes of behavior. Explaining my drinking coffee with reference to my *desire* for stimulation and problems with impulse control, or claiming that I *think* about what I write before I write it are simple psychological explanations. Folk psychology’s explanatory laws include statements such as “people who are in pain tend to wince.”

According to Paul Churchland (1984), the defining feature of eliminative materialism is the view that folk psychology is “a false and radically misleading conception of the causes of human behavior and the nature of cognitive activity.” (p. 43). Within the framework of his scientific realism, getting the causes correct is how we know about reality. The way we know we have the causes correct is when we gain the ability to predict and control our domain of study. Folk psychology, which uses three classes of variables (causal events, beliefs and desires, and behaviors) is not very good at letting us do this.

4. They believe that they have defeated folk psychology in principle and now want to show that it can be defeated in practice (which will be the real defeat).

Another way he puts the argument is to claim that the brain is organized according to its own rules, not the rules of folk psychology. Since there are no neat neurological correlates for psychological concepts such as belief and desire, belief and desire are not natural kinds. It is a mistake to try to understand ourselves using these hypothetical constructs, because they misrepresent the actual physical nature of conscious intelligence. As demonstrated by the case of a pseudoscience like phrenology, rather than squeezing our understanding of the brain into the framework of folk psychology, we should try to understand the brain on its own terms (as a product of evolution, as biochemical, as physiological, etc.).

The problem, however, is not just how to understand the brain, it is how to understand ourselves. The Churchlands claim that as a framework for self-understanding, folk psychology is a massive failure. They begin their devaluation by comparing the framework of folk psychology to other frameworks in the history of science. For example, Paul Churchland (1984, 1992) places folk psychology in the same general class as (a) the phlogiston theory, (b) pre-Copernican perceptions of the night sky, (c) vitalism, (d) alchemy and (e) demonic possession as an explanation of mental illness. He also argues that the folk psychologist's claim that "belief" and "desire" are self-evident parallels the Medieval cosmologist's claim that the earth's placement in the center of the universe was self-evident. Following Feyerabend and Rorty, he rejects any appeal to self-evidence. In his view, appeals to self-evidence are a conservative emotionally-based defense of what we are familiar with and nothing more.

Patricia Churchland (1986) compares folk psychology with what she calls "folk physics". She describes folk physics using the following example, based on an experiment performed by the psychologist McCloskey (1983). If we give college students the task of jogging down a path and dropping a golf ball on a stationary target, many students do not drop the golf ball until they are over the target. Their belief that the golf ball will drop straight down can be called folk physics. The student's explanation of what happens in terms of the Aristotelian concept of impetus is also a part of folk physics.

According to Patricia Churchland, folk physics is intuitively obvious, but demonstrably false. A scientific physics, such as Newtonian theory, would suggest that the students should let the ball drop before they were over the target because the force of gravity will not overcome the ball's forward motion immediately. Her claim is that folk theories look intuitively plausible, but are

falsified by more sophisticated scientific theories. Folk psychology is the same as other folk theories. As intellectual history progresses, like all other folk theories, it is doomed to fail.

Arguments from analogy are only suggestive. Realizing this, Paul Churchland (1984, 1992) argues that folk psychology is a false and failing framework by specifically examining the robustness of folk psychology as a theory, similar to the way we would examine other theoretical frameworks in science. He presents three reasons why folk psychology is a theory that we should reject, (a) its explanatory failures; (b) its lack of potential for growth; and (c) its inability to be smoothly integrated into the framework of natural science.

Among the list of phenomena folk psychology has failed to explain, Churchland includes visual illusions, the nature and dynamics of mental illness, the nature of learning and memory, the faculty of creative imagination, and the purpose of sleep. He claims that while not demonstrating folk psychology to be false, these failures show it to be highly superficial. Churchland claims that since it is likely that a more accurate account of things such as the nature of sleep and of learning will be given in the framework of neuroscience, we should admit that folk psychology is being applied beyond its appropriate domain with respect to these processes.

Regarding its potential for future development, Churchland notes that folk psychology could be considered a degenerating research program in a Lakatosian sense. In other words, it has no potential for development. Lakatos (1970) based his critique of Karl Popper's falsificationist theory of science on the Duhem-Quine observation that refutations of single hypotheses are not taken as refutations of a whole theory. For example, Brown (1977) points out that in the early 19th century, astronomers realized that the observed orbit for Uranus did not match the predictions given by Newtonian mechanics. Rather than rejecting Newtonian mechanics, astronomers predicted that the calculated orbit would match the observed orbit if there was an unknown planet in a particular place exerting a particular gravitational force. Their resulting prediction of the existence of Neptune before it was ever seen is one of the great successes of the Newtonian model. The fact that observation falsified what the model predicted did not lead to a rejection of the model. Lakatos argues that only when a theory has massive failures and rival programs are proving their superiority is it abandoned.

Churchland holds that what he calls the Neolithic legacy of folk psychology has massive failures in this respect, that it is degenerating. For example,

he observes that although primitive cultures once used the folk psychology framework to understand the elements of nature itself, its application is currently restricted to the explanation of higher animal behavior. As far as the explanation of higher animal behavior is concerned, Churchland contends that the folk psychology (FP) framework has not advanced in two or three thousand years. "The FP of the Greeks is essentially the FP we use today, and we are negligibly better at explaining human behavior than was Sophocles" (1992, p. 8). He holds that the future explosion of knowledge lies with neuroscience, not with folk psychology. A philosopher of science such as Laudan (1977) would indicate that the potential for progress is an important factor in deciding what research programs to pursue, and because Churchland sees limited hope for progress in folk psychology, he does not think that folk psychology research programs should be continued.

Nor does Churchland believe that folk psychology advances the unity of science. For Sellars (1956), an important function of theoretical terms is their role as potential candidates for integration into the total picture. Sellars believed that the potential for consistency among different levels of analysis in both science and common sense is great. Integrating various disciplines into a common framework is an important goal and we should try to understand how things in general hang together rather than defending our own domain of analysis in a partisan manner.

Shifting Sellars's argument slightly, Churchland argues that because the concepts of folk psychology fail in terms of smoothly integrating with well-established theories in the natural sciences, we should reject the folk psychology framework. One of his implicit criteria for a scientific theory is that it is formulated mathematically. The laws of physics, chemistry, and neuroscience can all be given in terms of mathematical equations that can potentially be derived from or related to one another- the so-called "autonomous" laws of folk psychology cannot.

As they work through their competing framework argument, the Churchlands' debt to Feyerabend is obvious. In particular, they use Feyerabend's (1962) critique of the logical positivists' model of scientific reduction, what is now called the problem of intertheoretic reduction. Intertheoretic reduction asks about how theories from different domains and different levels of analysis relate to each other, particularly how laws at higher domains, such as psychology, can be explained using laws/principles from lower domains, such as neuroscience.

As we have seen with respect to the problem of meaning invariance, contra Nagel (1949), Feyerabend held that (old) reduced theories are often false, so they cannot be entailed by new theories. If the ontology and concepts of old theories are relatively isomorphic with the concepts and ontology of new theories, reductions of old to new are labeled smooth. In many cases, however, the old theory's ontology is merely discarded (or eliminated). For example, we used to think that heat was a fluid-like substance called caloric. Once the kinetic theory of heat was proposed, heat was seen to be a function of molecular motion and the substance of caloric was eliminated. Just as the caloric theory could not be smoothly reduced to the kinetic theory, the Churchlands are skeptical about a potential smooth reduction of folk psychology to neuroscience. This conclusion is important to the Churchlands' philosophical project because, by eliminating folk psychology, we engage in the kind of ontological simplification that makes a general scientific realism easier to achieve.

The Churchlands also utilize Feyerabend's (1970) and Kuhn's (1970) views on the existence of incommensurable discourses with respect to the problem of intertheoretic reduction. They claim that the identity materialists' belief in the possibility of reduction translates into a belief that the phenomenal and the biological conceptual systems are commensurable, i.e., they are understandable by a common standard. The identity theorists think that the common standard is biology. The eliminative materialists, however, argue that the folk psychological and the biological conceptual systems are incommensurable. They cannot be identified.

6. The Potential Advantages of Elimination

One of the longest running problems in the history of scientific psychology is a question about what kind of a science it should be, a natural science or something standing in between a natural science and a human science. Is psychology a *Naturwissenschaften* or a *Geisteswissenschaften*? Although some early leaders in the field such as Wundt and James vehemently rejected conceptualizing psychology as only a natural science, other prominent thinkers such as Ebbinghaus, Müller, Pavlov, and Watson specifically argued that psychology can best advance by becoming a natural science just as physics and physiology did. Paul Churchland (1979) stands firmly in the line of

thinkers who argue for a natural science model. Forgoing “psychology,” his name for this domain is “the natural science of epistemic engines.” For this new natural science, we need to use the best theory we can find.

Even though he is making the same argument as John Watson and others, it is not exactly the same old argument. The same old argument is that adopting a natural science model constitutes the most reliable way for us to gain prediction and control of whatever domain we are studying. If we could gain control over what we can now only passively endure, the benefits to humanity would be great. Churchland does make this argument, but he adds a twist to the promises made by the other thinkers in the tradition. This twist is possible for him because, with his post-positivistic framework, he is not as bound to naive ideas about the value of objectivity and the danger of subjectivity as earlier thinkers in the tradition were.

Echoing an argument made by Carnap (1956), Churchland readily agrees that our ability to discriminate between our internal states is important. Knowing the difference between hunger and thirst, or between hot and cold is important to our survival. In his model, discriminating between internal states constitutes *inner perception* whereas knowing about what is out there in the world constitutes *outer perception*. He defines all perception as follows: “Perception consists in the conceptual exploitation of the natural information contained in our sensations” (1979, p.2). He thinks that natural science has worked so well for outer perception that we should use it for inner perception as well.

The amount of information entering the brain is enormous. In knowing about the world, we take in billions of bits of information and process them into meaningful elements. Believing that we are extremely inefficient at utilizing all the natural information present in our sensory states, Churchland (1992) claims that learning how to use a more sophisticated theoretical framework with respect to inner perception could dramatically increase our discriminative powers.

He asks us to consider the difference in what an untrained child hears when listening to an orchestra and what a conductor, who utilizes a more sophisticated conceptual framework hears in listening to the same music. Symphony conductors can hear *Am7* chords and pick out the melody played by the flutes within, what to a child, is just a wall of sound. Another relevant comparison involves considering what an expert wine taster experiences in drinking a bottle of wine compared to what a novice wine taster would

experience drinking the same wine. With their sophisticated conceptual abilities, expert wine tasters can taste glycol, ethanol, fructose, tannin, etc., whereas novices cannot tell the difference between a Cabernet and a Merlot. His conclusion is that we have inborn discriminative capacities that we never use and may not even know about.

In a similar fashion, instead of training ourselves to discriminate our internal states on the basis of folk psychology, with a matured neuroscience in our possession we could train ourselves to make discriminations on the basis of neuroscientific concepts. Churchland speculates that we could gain non-inferential knowledge of dopamine levels in the limbic system, spiking frequencies in neural pathways, and inhibitory feedback to the lateral geniculate nucleus. Once we learned how to do this, we could really know ourselves for the first time.

By not accepting a radical dualism of the objective and the subjective, Churchland helps us see that it is not just objective knowledge that we can gain by learning more about neuroscience; we can gain a new subjective perspective as well. The potential explosion of self-understanding is enormous! If we could successfully adopt a natural science framework for introspection, “psychology” would be so radically different that future members of the species would hold that the paltry folk psychology of the past was eliminated in favor of something much more powerful.

7. Comparing Biomedical and Eliminative Materialism

The overlap between biomedical materialism in psychiatry and eliminative materialism in philosophy is, in many respects, extensive. Although it will become clear that there are important differences between practicing psychiatrists and academic philosophers, at this point I am focusing on their commonalities. In addition to materialism, which I here define as the belief that entities such as trees, rocks, planets, and electrical forces are prototype examples of what counts as real, I will highlight their similarities in terms of four themes. The common tenets of biomedical and eliminative materialism are:

- a. Believing that the brain is the substrate of psychological states.
- b. Professing a scientific realism that is Comtean and physicalistic.
- c. Endorsing anti-anthropomorphism.
- d. Asserting a Utopian humanitarianism.

The *brain as substrate thesis* is simply the idea that psychological events are dependent on the brain. This dependency is an ontological one, if there were no brain, there would be no psychological states. When discussed under the rubric of supervenience, it means that there is no mental difference without some physical difference. Psychiatrists and philosophers draw on the same research evidence to support the brain as substrate thesis, which was summarized in Chapter 2.

In Chapter 2, I also discussed the “brain is the organ of the mind” doctrine, the view that, if we want to understand the real nature of psychological events, we have to understand the brain. This doctrine, a corollary of the brain as substrate thesis, is commonly held by materialists. It is a corollary that Paul and Patricia Churchland accept so strongly that they assume it rather than argue for it. The “organ of the mind” doctrine actually flows into the scientific realism tenet, underlying the Churchlands argument that to find out about how the brain works is to find out about how we really are.

When Paul Churchland (1984) writes about the importance of finding out about the causes of mental illness, he suggests that those causes are going to be found by neuroscience. He also asserts that the fact that imipramine controls depression, lithium controls mania, and chlorpromazine controls schizophrenia supports the conclusion that “the victims of mental illness are the victims of sheer chemical circumstance, whose origins are more metabolic and biological than they are social or psychological” (p.145).

He also holds views which accord well with the biomedical materialist’s belief that the best way to create change with respect to curing mental illness is to modify the underlying substrate. For example:

If we can discover the nature and the origin of the complex chemical imbalances that underlie the major forms of mental illness, we may be able to cure them outright or even prevent their occurrence entirely (p. 145).

In philosophy, *functionalism* is the view that mental states are to be defined in terms of casual roles (inputs, outputs, and implicit relations with other states), also called functional states. Functionalists have pointed out that if there were Martians and they were silicon-based organisms, both Martians and humans could be in pain while at the same time being in radically different brain states. What they would share is their general reaction to having a needle stuck in their arm. Since the same functional state can be instantiated in a number of different physical states, understanding any functional state can proceed autonomous of understanding its particular physical instantiation.

Making a pragmatic argument against the irreducibility claims of the functionalists, Churchland (1992) notes that, contrary to the claims of alchemy, we can make real gold, not by manipulating superficial syndromes in something like lead, but by learning how to modify the underlying substrate.⁵ Claiming that functionalism is a modern day version of alchemy, he says we must focus cognitive science on the underlying substrate as well. He suggests that the idea of altering cognition by trying to alter combinations of beliefs, desires, and intentions is a clever trick for preventing the elimination of an outdated perspective. It is a “smokescreen for the preservation of error and confusion” (p.14). Obviously, psychotherapy would not fare much better under Churchland than functionalism does, and would probably also be labeled “short-sighted and reactionary” (p.11) or “an outrage against reason and truth” (p.13)

As a matter of fact, we can glimpse his views on psychotherapy in his claim that neuroscience is unlikely to find the sentences in the head that folk psychological explanations depend on, such as “He is afraid that people will laugh at him” or “She believes that the abuse was her fault.” Churchland (1992) argues that we need an entirely new kinematics and dynamics for comprehending human cognition. He states that once we do this, folk psychology will certainly “be put aside in the lab and in the clinic, and eventually, perhaps, in the marketplace as well.” (p.125).

In a chapter called “The brain in trouble” Churchland (1995) explicitly addresses psychiatric issues, largely from a standpoint of neurology, echoing the biomedical materialist’s inclination to de-emphasize the distinction between neurology and psychiatry. The chapter in question is basically an abridged version of Andreasen’s (1984) *The Broken Brain*. The major difference is that Churchland uses traumatic brain injury as his prototype for understanding psychiatric disorders whereas Andreasen seems more comfortable with the traditional exemplar of general paresis.

He also explicitly discusses talk therapy, specifically psychoanalysis. Contra Paul Churchland’s view, broadly psychodynamic approaches still constitute important therapeutic models. The major alternative systems, especially the cognitive and behavioral approaches, continue to use basic analytic

5. His strongest argument against functionalism is that the facts of multiple instantiation have no consequences for the possibility of a reduction to a physical explanation. They only require that reductions have to be domain-specific. Heat might reduce to one thing in the domain of gases and another in the domain of plasma.

premises. Their creators such as Aaron Beck, Albert Ellis, and Joseph Wolpe are trained analysts, and their training still subtly influences the processes of their “therapy.” Churchland’s view on analysis proper, that it is a “mostly empty and confabulatory art” (1995, p.181), could be applied to most talk therapies. He thinks that Freud’s system is “commonsense psychology relocated one level down” (p. 182) and claims it was a mistake for Freud to apply the explanatory model of folk psychology to describe unconscious processes and abnormal behavior. His proof for these assertions consists of a statement that psychoanalytic technique is chronically feeble in both explanatory power and therapeutic efficacy, especially when compared to biological psychiatry. Although he acknowledges some vague role for talk therapies, he claims that we cannot “fix a genuinely broken brain just by talking to it” (p.183).

Scientific realism is articulated by the Churchlands and assumed by the biomedical materialists. Both groups of materialists are full-blown scientific realists, accepting each of the “realisms” defined by Hacking (1987). The first, realism about entities, claims that the entities talked about by scientists exist. The second, realism about theories, claims that theories of scientists are true or false independent of what we know. Science is supposed to be able to tell us about the true nature of the world, defined as a material world. Biomedical and eliminative materialists also believe that the more physical an entity is, the more real it is, and that science tells us about what the material world is actually like. Any explanation that refers to purely physical processes is therefore the best candidate for achieving correspondence with reality. Both the biomedical materialists and the eliminative materialists associate being more scientific with being a more respectable academic discipline.

The scientific realism of the biomedical and eliminative materialists is also Comtean. They see human intellectual history as evolving toward a higher state, with greater scientific understanding being the defining characteristic of progress. Consistent with this paradoxically anti-Darwinian focus on progress and perfectability,⁶ biomedical and eliminative materialists refer to those views which have been replaced by more accurate scientific knowledge as “primitive” and part of “common sense.”

Anti-anthropomorphism, the third common tenet, depends on scientific

6. The idea of evolving toward a perfect state, and especially the use of adjectives “higher” and “lower” reflects a pre-Darwin theological view of creation that placed God at the top, followed by various species of angels, then humans and the other animals. Technically there is no higher or lower when natural selection guides evolution.

realism. Until eliminative materialism was introduced in the 1960s, the most articulate proponents of this doctrine were the radical behaviorists, especially J. B. Watson and B. F. Skinner. Watson (1913) had an anti-philosophical temperament; arguments among his fellow psychologists about both how to legitimately infer the mental from the behavioral and about the ambiguities of introspection bored him. Skinner (1938, 1948, 1974), who had a talent for philosophical thinking, was more interested in the philosophy of mind and its practical implications. He provided the initial arguments about psychological concepts being hypothetical fictions/metaphors analogous to pseudoscience, a strategy we have seen used by both the biomedical and eliminative materialists.

The basic identity theory thesis about the brain's being the substrate does not entail anything about the legitimacy of psychology broadly construed, yet both the biomedical materialists and the eliminativists adopt the spirit of the behaviorist devaluation, suggesting that continued use of psychological analyses constitutes an impetus to progress. Their anti-anthropomorphism has been amply demonstrated in both this chapter and the preceding chapter.

Although I'm not convinced that the reasons for favoring anti-anthropomorphism are uniform among its various proponents, I wish to highlight a commonality derived from the history of science as constructed by Comte. I refer to Comte's idea that scientific progress consists in progress away from subjective, personalized, anthropomorphic conceptualizations of the physical world. All the proponents of anti-anthropomorphism have the goal of replacing psychological explanations with explanations using the constructs, variables, and methods of the natural and the biological sciences.

Those who accept anti-anthropomorphism often justify it in the name of parsimony, which provides an important check on our explanatory tendency to posit abstract entities that may not exist. For those who value parsimony, it is a greater error to talk about something that does not exist than to ignore something that does exist. With respect to scientific realism, the principle is a remnant of the war between science and religion in the 17th and 18th centuries. Hume stated the basic idea succinctly when he said that anything which does not concern mathematical regularities or observable matters of fact should be committed to the flames, because it contains nothing but sophistry and illusion.

Another apparent commonality between these groups of materialists is accepting the anti-anthropomorphic implication of the continuity of species argument so important to the behaviorists. The implication is that we should

use a unified set of concepts and methods to understand both humans and animals. Churchland (1979) uses this argument in support of eliminative materialism to great effect, making a much more persuasive case than the behaviorists ever did. He begins by noting that animals, human children, and human adults are all epistemic engines, presumably operating on the same basic principles. They all have a kind of rationality that allows them to survive. In philosophy, rationality is usually construed by logicians as the appropriate manipulation of propositions.

Churchland correctly points out that projecting folk psychological propositions such as “If I cry my mother will feed me” into an infant’s or an animal’s head is illegitimate. It may make their behavior intelligible, but those propositions are not really there. Furthermore, given the continuity of species doctrine, the fundamental nature of intellectual activity cannot be propositional, because only one group of epistemically motivated species possesses sentences. His conclusion is that an accurate model of rationality itself requires rejecting propositional folk psychology in favor of some theory about what brains are really doing.

Churchland (1992, 1995) believes that these principles have already been discovered with respect to artificial neural networks and offers persuasive explanations of the nature of discrimination, explanation, and learning. His ideas about high dimension vector representation and the potential of artificial neural networks constitute fascinating epistemological speculations. Excited about the possibilities for progress promised by cognitive neurobiological/connectionist AI models, he goes so far as to label, as *antiquarian*, the language of “observation statements,” “logical inferences,” “rational beliefs,” and “truth” (1992, p.249).

A final similarity that biomedical materialists and the eliminative materialists share is scientific progressiveness, which relates to the fourth tenet, *Utopian Humanitarianism*. This tenet expresses a belief that neuroscience will increase our ability to predict and explain dramatically. In addition to their expectation of an explosion in knowledge, they express the hope that we will use this advanced knowledge for the good of humanity.

The eliminative and biomedical materialists’ progressive belief in the great things to come is partially derived from their scientific essentialism, thinking that neuroscience is finding out about how the world really is. With respect to psychological problems, knowing their true nature will help us eliminate a lot of suffering. The humanitarianism of the biomedical material-

ists has been sufficiently articulated in Chapter 2; the only point I want to repeat here is that they have a tendency to identify their position as the moral one and suggest that opposing positions are the enemies of humanitarian interests.

The eliminative materialists' arguments are surprisingly reflective of biomedical materialism, seen in Patricia Churchland's (1986) assertion that the replacement of folk psychology could constitute not a loss of something necessary for our humanity, but a loss of something merely familiar and well known. She claims that we may even come to see our current psychological myths as *inhumane*.

In his own defense of eliminativism, Paul Churchland (1984) notes that if we could transform our private introspection by appealing to neuropharmacological and anatomical states, the benefits to humanity would be great. By understanding the causes of mental illness, the factors involved in learning, and the neural basis of emotions, intelligence, and socialization, he claims that the sum total of human misery might be reduced and we could create a more peaceful and humane society. His 1995 chapter on psychiatric disorders echoes the basic biomedical belief that only by finding out about the true (biological) causes of mental illness can we reduce the suffering it causes.

8. Conclusion

At the end of Chapter 2 I observed that the evidence favoring biomedical materialism suggests that the biological approach is the best and most responsible approach for psychiatry to take with respect to solving its professional problems. That conclusion looks even stronger at this point, being backed up as it is by theories about the nature of meaning, evidence, truth, and the facts of intellectual history. As a matter of fact, the biomedical materialists' claim that psychological approaches to mental illness have been iatrogenic is a predictable consequence of the radically false nature of folk psychology as suggested by the eliminative materialists.

On the surface, the case against psychology is overwhelming. It may be that those who choose to defend psychology against biomedical and eliminative materialism are playing a cultural role analogous to that played by Cardinal Bellarmine, who defended the legitimacy of scriptural evidence in support of the Ptolemaic theory, and argued against Galileo's scientific evi-

dence which favored the Copernican theory. Analogical arguments are slippery though, and the Bellarmine-Psychology analogy is not flawless. I now turn to an examination of the eliminativist arguments. Paul Churchland suggests that we cannot defend folk psychology without making a reactionary retreat to question-begging arguments. As we examine his position point by point, it will become evident that he has not accounted for all the variables in self-understanding that he thinks he has. I think we can accept many of his claims about neuroscience and philosophy, without having to reject folk psychology in the process. Who the real conservatives are remains to be seen.

PART II

THE ROBUSTNESS OF PSYCHOLOGY

CHAPTER 4

Why there is no such thing as “Folk Psychology”

1. Introduction

In this chapter I argue that the notion of a common sense psychology or “folk psychology” is less coherent than is usually acknowledged, primarily because folk psychology has multiple meanings. In addition to being a term of derision in the same way that common sense is often a term of derision, folk psychology has at least five meanings according to its critics. It is a common sense conceptual system, it describes superficial appearances, it is supposedly self-evident, it is a degenerating research program, and it is antithetical to the natural sciences. A systematic consideration of these criticisms shows that they have limited validity at best.

2. Folk Psychology as a Common Sense Conceptual System

In contemporary philosophy, “folk psychology” names the conceptual system we use to explain behavior with reference to beliefs, desires, expectations, goals, and sensations. For example, explaining my drinking coffee with respect to my *desire* for stimulation or claiming that I apply for a grant because I *hope* to get funding for research are folk psychological explanations. Patricia Churchland (1986) refers to folk psychology as both common sense psychology and intuitive psychology. It is “the rough-hewn set of concepts, generalizations and rules of thumb we all standardly use in explaining and predicting human behavior” (p. 299). As described in Chapter 3, the eliminative materialists hold that folk psychology provides ultimately false explanations of human behavior.

A significant portion of Paul and Patricia Churchland's critique focuses on the work of *functionalists* such as Jerry Fodor who are trying to rationally reconstruct the nature of human reason in terms of propositional attitudes, what Paul Churchland (1992) calls "sentences in the head" (p.125). Two examples of a sentence in the head include *John believes that: the Klingons are about to break into the room* and *John wishes that: O'Brian would get that damned transporter fixed*. Behavior is supposedly a function both of the propositions/representations in the head, and of the attitude (belief, doubt, wish, etc.) the person takes toward those propositions. Using folk psychology, we would explain John's behavior by claiming that he is agitated because he has an attitude of belief with respect to the proposition that the Klingons mean him harm.

The anti-propositional attitude aspect of the eliminativist project is exemplified in Paul Churchland's argument from evolution where he states that since only one epistemically motivated species, namely humans, possesses sentences in the head, studying propositions cannot help us describe the true nature of intelligence. Closer examination, however, indicates that much more is meant by folk psychology than is given by Fodor's description of a certain kind of casual analysis.

First, folk psychology also entails a consideration of subjectivity and consciousness. An important critique of the propositional attitude functionalists is that they leave subjectivity out of their accounts, exemplified by what is called the absent qualia problem. The functionalists' definition of pain in terms of causal roles such as being pricked and saying "ouch" leaves out the subjective quality of feeling that pain. The Churchlands have spent the better parts of their careers arguing against any kind of explanatory role for subjectivity, in essence defending this aspect of functionalism.

Borrowing the arguments against Givenness developed by his teacher Wilfrid Sellars, Paul Churchland (1979, 1992) has argued that there is too much variation in the subjective quality of our experience for it to play an important role in determining meaning. Since the meaning of something like pain cannot be given in the raw feeling of pain alone, it has to be given in terms of the role that pain plays in a linguistic economy. It is what is experienced when someone is stabbed, it is what is experienced when someone is burned, it is what is experienced when someone is cut, etc. The meaning of pain has to be understood in terms of the standard conditions in which pain is experienced. Raw subjectivity is meaningless.

Second, in asserting that our inability to understand the nature of learning, intelligence, and mental illness constitutes some of folk psychology’s explanatory failures, Paul Churchland (1984) suggests that cognitive psychology, abnormal psychology, and the psychology of personality are a part of folk psychology. In claiming that the set of variables appropriate to folk psychological explanation includes (a) behavior, (b) beliefs/desires, and (c) external events, he addresses all variations of psychodynamic psychology, interpersonal psychology, and the theories of the social psychologists. So both clinical psychology and experimental psychology are a part of folk psychology.

Third, in stating that we are not much better at explaining human behavior than was Sophocles, Paul Churchland (1984, 1992) indicates that the less systematic psychological observations offered by scholars in the humanities also form a part of folk psychology. In her own discussion of folk psychology, Patricia Churchland (1986) notes that Henry James employed a more sophisticated psychological theory than Ernest Hemingway.

Philosophers pay almost no attention to what a psychologist would mean by folk psychology. For a psychologist, folk psychology refers to the actual reasons persons-in-the-street use to explain behavior. One example of research studying “folk psychology” can be found in social psychology. Attribution theorists such as Heider (1958) and Kelley (1967) wanted to find out how lay people explain behavior. This is an empirical question. To find out we have to ask them. For example, to study lay theories about what makes a good marriage a researcher would ask people: “what makes for a good relationship.” As Fletcher (1995) points out, we would get many different kinds of answerers, including “There must be complete honesty” and “Your partner should be your best friend.” These micro-beliefs are what Horgan and Graham (1993) call “folksy psychology” (p. 293).

Fletcher (1985) points out that lay models also encode more abstract models of cognitive processes. Pushing the term a little, these processes constitute another “folk psychology.” Questions about cognitive processes include whether there are such things as unconscious beliefs, whether the brain stores information which we can then retrieve, whether introspection is a function of consulting one’s own experience, and whether reason and emotion are in conflict with one another. Since these are the kinds of questions that scientific psychology also studies, folk psychology slides into cognitive psychology. What might be called cognitive folk psychology, however, is more than just a lay model.

A similar point is made by Tellegen (1993) with respect to personality theory, where he distinguishes “folk concepts” from “psychological concepts.” Tellegen defines folk concepts as common sense ideas about personality that are encoded in the natural languages people use on a daily basis. When these natural language concepts are used in a scientific theory to explain personality, they have to satisfy criteria such as internal coherence, external testability, and consistency with the known facts. If they satisfy these criteria, they become systematic psychological concepts. He claims that psychological concepts such as negative emotionality (neuroticism) and positive valence (extroversion) deserve to be called systematic psychological concepts. Personality theory is therefore more than a lay model.

These various distinctions are missing from the philosopher’s uniform concept of folk psychology. In holding that folk psychology refers to the very idea that there are such things as thoughts, beliefs, emotions, and desires, eliminativists inappropriately extend an analysis of folksy psychology to the so-called framework of psychology-in-general. For them, there is no such thing as folk psychology separate from “psychology.” Recognizing the general nature of “folk psychology” is important because in making their targeted arguments, many writers attribute a narrowness and specificity to the folk psychology concept itself. This is a mistake.

3. Folk Psychology as a Pejorative Term

Defining folk psychology as psychology-in-general is a more accurate description of the term, but still fails to capture its meaning as used by the eliminativists. More important to them than the term “psychology” is the adjective “folk.” As stated, this usage borrows from the anthropological literature and refers to a product of the common people. More important, it follows the Comtean tradition in the history of science where common sense is conceptualized as a synonym for primitive, i.e., prescientific.

According to Comte (1853/1896), the only things we can be sure of are things that are publicly observable, which is what science studies. He thought that science should describe lawful relationships among physical phenomena. These relationships should be then used to predict and control events. In the Comtean scheme, scientific thinking characterizes a stage in human history

which had been preceded by two other stages, the theological and the metaphysical stages. These stages are distinguished by mutually exclusive philosophies for understanding natural events. Comte describes the stages as follows:

- a. *Theological stage* (fictitious) — explanations based on superstition and mysticism, particularly anthropomorphic/animist explanations of the physical world. Also called anti-science.
- b. *Metaphysical stage* (abstract) — explanations based on unseen essences, principles and laws. More depersonalized. Also called proto-science or pseudo-science.
- c. *Scientific stage* (positive) — description is more important than explanation, and prediction and control are the primary goals. More mathematical and experimental.

In an ironic twist, the Comtean model is more compatible with the 19th century natural theologians view of evolution than it is with Darwin’s view. The natural theologians held that nature is evolving toward a state of perfection, and later stages are more perfect or higher than earlier stages. This model was important to both Aristotle and Thomas Aquinas. The theologian’s scale of nature places God at the top, followed by the various species of angels, humans, animals, and plants. Those higher up on the scale are more perfect. In this model, science is much closer to a God’s eye view than is anthropomorphic thinking. Related to this pre-Darwinian assumption is the notion that we will someday have a utopian science.

As is well known, Darwin rejected natural theology for a view of evolution as providing a temporary adaptive advantage rather than increasing “perfection.” Divorced from these 19th century assumptions, scientific theories should be seen as providing a temporary adaptive advantage, rather than a step on the path to a God’s eye view of the world.

In claiming that folk psychology is a degenerating research program, Paul Churchland (1992) states that although primitive cultures once used the folk psychology framework to understand the elements of nature itself, its application is currently restricted to the explanation of higher animal behavior. Churchland’s “animistic explanations” used by primitive cultures refers to Comte’s theological stage where storms and floods were thought to result from intentional actions by the gods. The unstated thrust of Churchland’s claim that *the folk psychology used by the Greeks is essentially the folk psychology used by us today* is that folk psychology is a relic of prescientific

modes of human understanding, including both theistic animism and metaphysical essence-oriented “sciences” such as alchemy. In this Comtean model, developing a truly mechanistic, non-intentional, non-subjective theory of persons is part of the natural progress of intellectual history.

True to the spirit of 19th century positivism, folk psychology as pre-scientific “common sense” is one of the eliminativist’s favorite pejorative terms. Especially for Paul Churchland, folk psychology is “radically false” (1989, p.1), “fundamentally defective” (1989, p.1), and “constitutionally incapable of addressing even the most basic mysteries” (1989, p.7). Furthermore it is “profoundly imperfect” (1989 p.7), it “suffers from explanatory failures on an epic scale” (1989, p.9) and “the conception of rationality it provides appears limping and superficial” ((1989, p.16). In addition to terms such as “explanatory poverty” and “cockeyed” (1984, p. 46), he also refers to folk psychology, in good Comtean tradition, as our neolithic legacy. Patricia Churchland (1986) claims it is “folkishly inept, soft and narrow” (p. 395) and “much-muddled” (p.397).

In my experience, philosophers tend to be less bothered by the eliminativist’s insults of psychology than are psychologists. In addition to understanding the eliminativists’ reasons for saying what they do, they can more easily excuse the language as dramatic bluster. It can, however, be rather effective drama, especially when it provokes implicit intellectual prejudices and directs them at psychology.

One of the prejudices I refer to is the science versus common sense prejudice. Even if we ignore the bluster, the simple dichotomy between science and common sense suggested in eliminativist language deserves critical attention.

4. Folk Psychology: Common Sense Defined as Consensus

Now that we have a better understanding of what is meant by folk psychology, we are in a position to examine its fruitfulness as a concept. I begin with the distinction most fundamental to the concept, the distinction between science and common sense. To be sure, we have to admit some kind of a distinction between science and common sense. Common sense supposedly tells us that the earth is flat, but science tells us it is round. Common sense supposedly tells us that the earth is the center of the universe, but science tells us that the earth travels around the sun, which is the center of a solar system.

When we examine criteria on which the distinction between science and common sense is made, one of the first that comes to mind is the idea that common sense refers to consensus, something that everyone agrees on. This person-in-the-street meaning is what most scientific research into folk conceptual systems is about.

The consensus criteria would not get the eliminativists very far, however, because what everyone agrees on is not necessarily in opposition to science. For example, a belief in the brain’s role as the organ of thought is also a part of common sense. Its robustness is probably derived from its relation to what I call **folk materialism**. By folk materialism I mean common sense beliefs about the physical world that are extremely impractical to doubt. It turns out that any kind of materialism, including eliminativism, borrows its robustness from common sense opinion.

A common sense understanding of the physical world is primary not only with respect to that fact we all utilize it, but also with respect to the fact that it is among what we first learn — we even learn about it before we can formulate concepts! According to research in developmental psychology, infants act in the world and receive feedback which they use to modify their behavior, but the knowledge they have is procedural (reflex-like know-how). They quickly begin learning how to navigate in the physical world, but lack representational knowledge of the world (Mandler 1984). Cohen and Strauss (1979) argue that rudimentary concept development begins very early. One path that an infant’s cognitive development takes is differentiating self from objects, then learning about properties of the physical world including movement, spatiality, solidity, and eventually the independent existence of objects. Along this path, the infant’s relation to the physical world undergoes a transformation from knowledge based only on immediate impressions, to knowledge based on abstract representations of an invariant world. (Flavell 1985; Gibson & Spelke 1983).

What infants learn includes the concepts philosophers use to illustrate common sense understanding of the physical world. For example, humans seem to be sensitive to movement at birth (Bronson 1974). According to Spelke (1985), by the age of three months, infants perceive objects as both separate from their backgrounds (i.e., distinct) and as unified entities. Small (1990) notes that young infants (four months) also organize their experiences as events in a three-dimensional world. Baillargeon (1986) has demonstrated that by six to eight months, infants know that a solid object cannot move through a space occupied by another solid object.

Piaget's (1930, 1954) work traced how infants develop the conceptual understanding that objects have an existence independent of them. He called this *object permanence*. According to Small (1990) and Flavell (1985), subsequent research has validated the importance of object permanence, but also indicates that it occurs earlier than Piaget realized, beginning in some form at the age of five months. Clearly by the time they are a year old, children have already developed a basic understanding of the physical world (movement, depth, unity, solidity, and the separate existence of objects).

Once objects are represented conceptually, children begin to analyze them in terms of parts and wholes. By the time they are of preschool age, they can undertake part-whole analyses for events, collections, and objects (Small 1990). Bullock, Gelman, and Baillargeon (1982) have shown that preschoolers also have a basic understanding of cause and effect relationships. Understanding that quantitative properties such as number, quantity, weight, and volume are maintained (or conserved) when both superficial appearances and context change constitute another major development in our understanding of the physical world.

Belief in the physical world is such an ingrained a part of common sense that those who challenge it will quickly have their rationality questioned. For example, here is developmental psychologist James Flavell (1985) speaking for common sense with respect to Piaget's object permanence concept:

In the first place, the concept is so utterly basic and fundamental. If any concept could be regarded as indispensable to a coherent rational mental life, this one certainly would be. Imagine what your life would be if you did not believe that objects continued to exist when they left the field of your vision. Worse yet, imagine how things would be if *nobody* believed it. (p.34)

Flavell suggests that we cannot doubt the independent existence of physical objects without inviting chaos into our world.

Illustrations of not conforming to common sense standards with respect to the physical world can be glimpsed in some forms of dementia. For example, as he approached his ninetieth birthday, my paternal grandfather used to suffer small strokes. Every time he had one of these strokes, he was convinced that he was in the middle of a card game. Regardless of where he was, and no matter what he had in his hand, a belt buckle, a piece of pie, or a postcard, he was convinced that he was holding a hand of cards — with no meld. Though he was not psychotic, my grandfather's dementia clearly led him to make some ridiculous assertions.

Others have offered more eloquent expressions of the contention that doubting physical reality leads to questions about the doubter’s rationality. Descartes (1641/1959) wrote that outside of his methodical doubt, had he believed his hands and body were not really his, he would have been very similar to:

those who are insane, and whose brains are so disturbed and clouded by dark bilious vapors that they persist in assuring us that they are kings, when in fact they are in extreme poverty; or that they are clothed in gold and purple, when they are in fact destitute of any covering; or that their head is made of clay and their body of glass, or that they are pumpkins. They are mad; and I should be no less insane were I to follow examples so extravagant (p. 29).

Here is Descartes, master doubter of the physical world, asserting the irrationality of not believing simple physical facts. The association between folk materialism and rationality adds an even more compelling reason for our intractable belief in it; we are not recognized as a functioning member of society if we reject it. In the above passage, Descartes will continue his argument, calling the physical world into doubt, but his “I don’t really doubt this” declaration partly serves the protective function of reminding the reader that, most of the time, he does believe that his hands are his hands.

Because human beings are physical, the common sense beliefs which I have referred to as folk materialism are naturally applied to persons. For example, I am sitting at my desk. I have limbs, a torso, and a head. I am wearing a sweater and dark pants. My body is made up of a central nervous system and various organs. In my head, as a part of my central nervous system, is my brain. My brain can be analyzed into components. These components include Brodmann’s areas and anatomical regions such as the posterior parietal cortex, Broca’s area, and the reticular formation.

Once we start talking about internal organs and the brain, we are no longer in the domain of folk materialism, but internal organs and the brain are logically connected to folk materialism. The transition from one to the other makes sense, but knowledge of the brain is not primary in the way that knowing “my body and a brick wall cannot occupy the same space and the same time” is primary. They are connected enough, however, that if I went around literally claiming that I had two hearts or that my brain is made of Jell-O, my rationality would be questioned.

There is yet another transition that we make between domains of common sense. We saw how Descartes referred to brains clouded by dark bilious

vapors to account for psychotic mental states. To the extent that the brain is the physical organ of cognition and affect, thinking that the brain contains what makes us psychologically distinct individuals is also common sense.

This version of common sense has been an integral part of western civilization since the Enlightenment. Ellenberger (1974) reports that Voltaire defined insanity as “A brain disease that keeps a man from thinking and acting as other men do”(p. 18). The common sense belief in the brain’s importance can also be found in the Sherlock Holmes stories of Arthur Conan Doyle (1892–1927/1988). As a turn-of-the-century writer, Conan-Doyle articulated ideas central to 20th century culture.

In *A Study in Scarlet*, explaining his own rational psychology to a Watson who was incredulous about the detective’s ignorance of the Copernican theory, Holmes claimed:

I consider that a man’s brain originally is like a little empty attic, and you have to stock it with such furniture as you choose. A fool takes in all the lumber of every sort that he comes across, so that knowledge which might be useful to him gets crowded out, or at best is jumbled up with a lot of other things, so that he has a difficulty in laying his hands on it. (p. 21)

Holmes here clearly identified the brain as the organ of thought. In *The Final Problem*, Holmes described his nemesis, Professor Moriarty, as the Napoleon of crime, a genius, a philosopher, and an abstract thinker, with the conclusion: “He has a brain of the first order.” (p. 471). Later, in *The Adventure of the Norwood Builder*, Holmes would mourn the late lamented Professor Moriarty, whose “great malignant brain” made London such an interesting city. Holmes illustrates an idea common to us all, i.e., the brain is central to each person and is part of what makes each distinct.

Reclusive American poet Emily Dickinson writing in 1862 also expresses how much importance common sense understanding places on the brain’s role:

The Brain-is wider than the Sky-
For-put them side by side-
The one the other will contain
With ease-and You-beside-

The Brain is deeper than the sea-
For-hold them-Blue to Blue-
The one the other will absorb-
As Sponges-Buckets-do-

The Brain is just the weight of God-
 For-Heft them-Pound for Pound-
 And they will differ-if they do-
 As Syllable from Sound-

Johnson and Wellman (1982) have shown that by the time children are four or five years old, they know that the brain is located inside the head and that people think with their brains. By the time they are five, children claim that we would be unable to think without a brain. Awareness that the brain is associated with thinking even precedes awareness that it is also associated with bodily movement. In our society, the importance of the brain is common sense opinion. Children are not little identity materialists because they don't think that thoughts are brain states, but they do clearly believe that we think with the brain.

Outside very basic levels, such as the domain of folk materialism, common sense opinion changes over time. For thousands of years, the belief that the earth is flat was a part of common sense — and the proposition that the earth is round was considered to be ridiculous. But in this day and age, there is something very uncommonsensical about people who persist in believing that the earth is flat. Common sense changed. Many ancients, including Aristotle, thought that the heart and the guts were the centers of consciousness and considered the brain to be an organ for cooling the blood. Speculation about the brain's importance by Alcmaeon, Hippocrates, and Galen were not readily accepted. But in this day and age, the brain's role as the organ of consciousness is common knowledge.

One professional hazzard of being a psychologist is that people often wonder if you are *analyzing them* or *reading their minds*, but I have had people ask me if I was *reading their brain*! The common sense nature of the mind-brain relationship was dramatically brought home to me when I had a hard time helping some students understand what seemed, to me, to be a basic distinction between British empiricism and French sensationalism. In this class, we had already studied Descartes' mind-body dualism. We had also studied how the British empiricists had grounded knowledge in sense experience, such as impressions of red and green and cold. When I told my students that the French sensationalists reduced sense experience to brain states and therefore reduced mind to the brain, they were not sure what I was talking about. This became evident when some of the brighter students came to me and asked me to explain it further. I found out that they could not understand what I meant by “reducing mind to brain” because they thought of mind and

brain synonymously. The lights didn't go on in their eyes until I reminded them that following Descartes, people have thought of mind and brain as being distinct things.

From a clinical perspective, anyone going motorcycle or bike riding should wear a helmet because, in a non-fatal accident, a head injury could be catastrophic. Persons with a brain injury often appear physically unimpaired, but a mild to moderate brain injury can cause people to lose their abilities to express thoughts or even to think the way they thought pre-injury (Burke 1988). Their abilities to plan, initiate, and monitor activities are often disturbed (Prigatano & Fordyce 1986). They may also lose control of their emotions, becoming very impulsive and labile (Lezak 1978). The sequelae of the brain injury would be a loss of the self they know.

In a very significant way, we are our brains. The indisputable importance of taking precautions during bike riding makes it ridiculous to deny the brain's importance. Someone might refuse to wear a helmet because they believe that the chances of a serious accident are slim, but to actually believe that a brain injury would not have serious psychological consequences would be foolish.

When the biomedical psychiatrist Nancy Andreasen (1984) writes "The brain is the source of everything that we are. It is the source of everything that makes us human, humane, and unique. It is the source of our ability to speak, to write, to think, to create, to love to laugh, to despair, and to hate" (p.83), it is not possible to argue against her by claiming that the brain is not the source of those things. It is common sense that we would be unable to think, love, or create without our brains. Just as our language and educational system implicitly teach us that the earth is not flat (via globes, pictures from space, and phrases such as "Around the world in 80 days") so we are taught that we think with our brains. Denying it is irrational.

So defining common sense as what people agree on does not seem to be the kind of thing that eliminativists want to identify as unique to folk psychology, because it fails to distinguish their own beliefs about the brain from common sense. Common sense is not as separate from scientific progress as phrases such as "folk psychology" or "folk physics" would have us believe.

A final mistake made by some thinkers is to associate philosophical problems with common sense. Rorty (1979) is correct when he says that mental entities are the invention of philosophers, but he underestimates how much they are also limited to philosophy. One of the subtle difficulties involved in understanding his argument about the Antipodeans is his defini-

tion of pains as “things in the head,” which makes sense to philosophers more than to people in the street. People will say they have “a pain,” but if we were to ask them, do you mean that you *have a pain* or that *you are in a state of pain*, they would not see the difference. If you explain what a philosopher means by “having a pain,” they would likely say that they mean they are in pain. It would take non-philosophers several readings to figure out that Rorty talks about pain as a mental entity/thing. The same is true for “I have a belief” and “I believe,” even though we may be more inclined to think that we really have things called beliefs rather than being in belief states. Most people don’t think about such things and they would not necessarily prefer the common sense “philosophical” theory to alternative philosophical theories if they were introduced to them at the same time.

5. Folk Psychology as Superficial Appearances

A more subtle argument on the part of the eliminativists is prominent in modern philosophy (and in Platonic essentialism). I refer to the notion that appearances are not necessarily good indicators of what is really there. For example, it may appear that apples are really red or that a table is really solid, but science tells us that this is not so. As Philipse (1990) argues, what modern science tells us is really there goes against common sense appearances. Although many offshoots of the appearance-reality distinction could be pursued, I examine the idea that the truths of science are often counter-intuitive, which is the basic definition used by Patricia Churchland (1986) in her discussion of folk physics.

Being counter-intuitive does not help us separate folk psychology from neuroscience. As any teacher of psychotherapy can tell you, expert-level psychological analyses are counter-intuitive in the extreme. One of the more challenging aspects of supervising graduate students learning to do therapy is that you can communicate only so much of the therapeutic process to them. If you tell them what you actually see, they are likely to either think you are making this up or be so overwhelmed that they feel too helpless to go back into another session.

Let me illustrate with a clinical example. Certain clients, with a personality structure referred to as borderline, have a difficult time maintaining a coherent and integrated sense of self. As a result, they depend on others to

provide them a sense of consistency, and have ever-present fears of abandonment. Often they have been abused as children. Related to the abuse, which was a boundary violation, these people have extremely inappropriate boundaries themselves. They want to get too close too fast and will alternate desires for enmeshment with aggressive pushing away.

What sometimes happens when clients with borderline personality disorder enter therapy and begin to form a relationship, is that they start to test the boundaries to see how safe the therapist is. The alert therapist will realize this and respond accordingly. It can be difficult to tell a graduate student therapist, when a client calls up to reschedule their morning appointment for one later in the day because they have to take their mother to the doctor, that the best thing to do is to refuse their request and offer them another appointment at the regularly scheduled time next week. It sounds rigid and unempathic, especially to the student who wants to be helpful. The supervisor's advice to the graduate student is counter-intuitive.

If the graduate student makes the common sense "empathic" response and offers them a later appointment, they have in effect demonstrated that they cannot be trusted to maintain stable boundaries. For some clients, boundary violations raise the specter of re-victimization. In the next few weeks, this action will often result in a series of missed appointments followed by numerous crisis calls as the client attempts to test the limits even further. In a sense, offering to reschedule has not helped this client feel safe in the same way that setting limits and establishing boundaries would. We can conclude that defining common sense as what reality appears to be, and science as what is counter-intuitive, fails with respect to separating folk psychology from science

I specifically chose a clinical example because it constitutes a domain of interaction that most people have little experience with. Expert-level psychological concepts can be counter-intuitive enough that it takes a few years of *in vivo* experience to begin to be able to use them. Paul Churchland (1995) has referred to psychoanalytic conceptualization in particular as a mostly empty and confabulatory art. Even though he does not acknowledge this, what holds for psychoanalytic folk psychology holds for all psychotherapy. There is no way for the clinical thinker to win this debate as it is set up. At the entry level, folk psychology is common sense and primitive, unchanged since the time of Sophocles. Yet, if some clinical thinker comes along and proposes a theory that is not immediately intuitive or common sensical, it is proclaimed confabulatory.

Another problem with rejecting “appearances” because they are unscientific is that, in the Churchlands’ own account, appearances are relative to conceptual systems. Rorty and Sellars clearly showed that the key issue in appearances is the idea of non-inferential knowledge, what we immediately see. Paul Churchland believes that we could gain non-inferential knowledge of dopamine levels in the limbic system. Rorty (1979) has argued that we could directly experience pain in terms of C-fiber firings. The thrust of Paul Churchland’s (1979) plasticity of perception thesis is that appearances are not necessarily unscientific. So the science-common sense problem is not one of appearances and never was. The issue is the conceptual systems in which things appear.

6. Folk Psychology as Self-Evident

It is in Paul Churchland’s (1979, 1992) often brilliant articulation of his post-positivistic scientific realism that we find another reason to be suspicious of folk psychology. This suspicion is aroused in reaction to thinkers who claim that the existence of pains and beliefs are self-evident. In this sense, self-evident propositions are propositions that depend on no other knowledge and are themselves the foundation for all knowledge claims. The self-evident involves what we cannot doubt.

Churchland disagrees with this analysis. His thinking is derived from Sellars’s (1956), Feyerabend’s (1963a, 1963b) and Rorty’s (1965) critiques of the idea that we possess self-evident propositions. For example, Feyerabend rejected the idea that the truth of what he called the “common idiom” could be established as a result of being confirmed by experience, i.e., self-evident. The fact that we have successfully used psychology to describe and explain ourselves for thousands of years means that our perceptions are prejudiced in favor of confirming psychology. Unfortunately for psychologists, confirmation works for astrology as well. It is a second rate principle of justification. The truths of psychology have to be tested in a framework that is not so tautological. Hence Feyerabend’s well-known claim that we need to develop alternative frameworks. Paul Churchland is so opposed to self-evidence that he almost suggests that anything appearing to be self-evident can’t be true, especially since it has been around for a long time.

I agree with the critique of self-evidence, but think that Churchland is

inconsistent in his use of Feyerabend. As shown in Chapter 3, the eliminativist contention that folk psychology might be a false perspective originated with Feyerabend's (1962) work in the philosophy of science. Paul Churchland's (1992) claims that (a) it is a historical accident that we humans currently use precisely the conceptual framework that we do use, (b) that there are an infinite number of frameworks potentially adequate to common experience, and (c) that we can eliminate the ontology of old theories in favor of new and better theories, all follow from his acceptance of Feyerabend's views, especially Feyerabend's rejection of meaning invariance in favor of a radical contingency of meaning thesis.

If the rejection of meaning invariance holds, however, theoretical terms in folk psychology should have undergone meaning shifts analogous to the meaning shifts demonstrated by Feyerabend for theoretical terms in physics. The forces leading to incommensurability for folk psychology are at least as complex as the forces creating incommensurability in physics. On Feyerabend's (1963b) principles, common idioms are:

intimately connected with the fears and hopes of the community in which they occur; if they are defended and reinforced with the help of powerful institutions; if one's whole life is somehow carried out in accordance with them—then the language representing them will be regarded as most successful (p. 52).

An implication of this characterization is that the theoretical network of every day folk theories is influenced by historical and cultural conditions. Historical shifts, such as the elimination of monarchies by democratic republics, or the evolution from feudalism to open market capitalism have to be considered in examining folk theoretical networks. The rise of science in opposition to religion, creating battles between evolutionary theory and the Book of Genesis, surely influenced the folk psychology network. When the network changes, the meaning of individual terms change.

Putting aside the important question of whether meaning shifts are continuous or discontinuous, exploring the effects these shifts have had on our self-conceptualization is central to the project of many recent continental philosophers. Foucault's (1973, 1979) genealogical examinations of the human sciences' construction of modern subjectivity (e.g., the concepts of mental illness and sexuality) is one example of an attempt to elucidate the contingency of what the Churchlands might refer to as folk psychology. The eliminative materialist's claim that what seems self-evident to us now will not necessarily be self-evident in the future has a parallel in Foucault's claim that what seems self-evident to us now was not even evident in ages past.

In asserting incommensurability, Feyerabend implied that our current understanding of concepts may not be isomorphic with the understanding of people using a different conceptual framework. Labeling this form of incommensurability "dissociation," Hacking (1987) illustrates it by quoting the seventeenth century thinker Paracelsus, "Nature works through other things, such as pictures, stones, herbs, words, or when she makes comets, similitudes, halos, and other unnatural products of the heavens" (p. 70). According to Hacking, we can understand the words perfectly well, but the sentence makes no sense because it is based on a system of categories that are unknown and unintelligible to us.

With respect to psychological processes, no one seems to think that our brains have evolved so much that we differ from the Greeks in any important way. For eliminativists, however, cognition, emotion, and desire are theoretical constructs. If we accept incommensurability as defined by Feyerabend, and the eliminativists clearly do, it is unlikely that the psychological concepts learned by someone living in ancient Greece would be the same as the psychological concepts learned by a psychoanalytically-influenced modernist. On the eliminative materialist's conception of a stagnant folk psychology, Aristotle and Plato should be able to empathize with Faulkner's Quentin Compson. But if we take seriously their views on (a) the contingency of meanings, (b) the network theory of meaning, and (c) the possibility of a folk network, it is unlikely that Aristotle and Plato could even adequately comprehend Quentin Compson, let alone empathize in the same way that we can empathize with him.

In the end, there is nothing about folk psychology that requires self-evidence. Since psychoanalytic theorists demonstrated that we can be unaware of our basic desires and emotions (Freud 1900/1961b; 1901/1961c), no psychologically-minded person would ever claim to know with certainty their exact desires, emotions, or opinions. Attitude research in social psychology has also demonstrated the contingency of belief (Bem 1972; Festinger 1957; Gergen 1965; Parke 1974). We can be vague about our beliefs and surprisingly ignorant of why we hold them. The very possibility that we are influenced by unconscious desires and unquestioned culturally defined beliefs makes psychological states far from self-evident. We can harbor prejudicial beliefs without knowing it, we can wish for something without admitting it to ourselves, and we can react to details about the environment without have any conscious awareness that we have done so.

In Dennett's (1978) terms, belief does not have a phenomenology, meaning that there is nothing that it is like to believe that x is y . Another way he puts it is to claim that "we have conscious access to the *results* of mental processes, but not to the processes themselves" (p. 165). The fact that introspection of thoughts is a matter of inference and intuition rather than "observation" is asserted anew each generation, e.g. Kulpe, Munsterberg, Dewey, Rorty, Mischel, and Dennett. It is one of psychology's most consistently rediscovered truths. From the standpoint of the history of psychology, our psychological states are clearly not self-evident. We can doubt them and we can be wrong about them. That is the whole point of associating Socratic dictums such as "know thyself" with wisdom. The failure of self-evident propositions is not a failure for psychology.

In the following sections I will explore more subtle reasons for being an eliminativist. One reason that I will not discuss is the reliability and validity problems with analyses based on introspection. This problem was originally an issue in the imageless thought debate between the Wundtian and the Wurzburg schools at the beginning of the 20th century. Psychological science, which used introspection as a method, could not agree on whether thought had to have some kind of sensation-based content or whether it could be automatic. For example, does adding $2+2$ really involve a mental image? This debate made the new scientific psychology look like it was trapped in one of the endless kinds of philosophical debates that experimental methodology was supposed to eliminate. The problems of introspectionist methodology constituted an important inspiration for Jamesian functionalism and behavioristic eliminativism.

Nisbet and Wilson's (1977) review of research in social psychology indicates that we cannot reliably report on the *causes* of our own beliefs. We will make up causes if asked, but those causes are demonstrably false on many occasions. Loftus's (1983) research in cognitive psychology indicates that memory is so constructive that self-report with respect to personal history and biography is very unreliable. Using arguments developed by Paul Meehl (1954, 1986), Robyn Dawes (1994) claims that clinical judgement based on clinical experience is extremely unreliable. Not only is the person's self report questionable; the clinical interviewer collecting the data does not even know how to appropriately separate wheat from the chaff in order to assess which piece of information is most important.

I will not discuss this reliability/validity problem because it is just another version of the whole self-evidence argument. Since our own psychological

states are not self-evident, self-reports are always going to be problematic. This difficulty does not justify the claim that there is no validity in psychology. The fact that we can't accurately reconstruct the psychosocial determinants of beliefs does not mean that there are no psychosocial determinants, or that there are no beliefs. Just because our memories of important events can be reconstructions does not mean they we have not been influenced by important events. The fact that developing psychological mindedness is difficult and perfectly accurate self-knowledge is unachievable does not justify eliminativism.

7. Folk Psychology as a Degenerating Research Program

Looking at what psychology has done with its framework, the eliminativists contend that it constitutes a degenerating research program. This claim is not without serious problems. Paul Churchland's three degenerating-research-program claims: (a) the hypothesis of explanatory retreat, (b) the observation of no explanatory advance in 2000 years and (c) the hypothesis of widespread explanatory failures, are all questionable. In the following section, I explore the three latter claims, and attempt to fill out the picture in more detail than has been done by writers such as Horgan and Woodward (1985) or Heil (1991).

Explanatory Retreat. In making his explanatory retreat claim, Paul Churchland assumes that folk psychology as a theory was developed to explain everything. In his view, over time, folk psychology has been reduced to explaining only the behavior of the higher animals, a fraction of what it was originally developed to explain.

Churchland's historical reconstruction would not be endorsed by most people asserting the legitimacy of psychology. An alternative reconstruction would hold that the concepts of belief, desire, and emotion were formulated based on both subjective experience and the intersubjective observation of human behavior. They were later egocentrically extended to other phenomena. Although primitive people may have projected their concepts for understanding themselves onto natural phenomenon such as thunderstorms and seasonal changes, these projections were illegitimate, analogous to the egocentric projections of children. Any “retreat” occurred only after the illegitimate projection was made. Logically speaking, early humans failed to adequately understand the extension of beliefs and desires.

Before we should accept Churchland's claim that folk psychology's

elimination from the explanation of clouds and seasons is a retreat rather than a correction, he has to offer evidence that the concepts of belief and desire were constructed based on observation of clouds at least as much as they were constructed on the basis of human behavior, and therefore were constructed in order to explain both the behavior of clouds and human beings. There is no evidence that they were so developed.

There is no empirical reason for choosing between the “explanatory retreat” or “correction of a projection error” hypotheses. It is even possible that intentional and subjective conceptualizations were applied to both humans and the rest of nature as they were developed, but there is no evolutionary reason why seeing clouds and rain animistically would confer any survival value. From a standpoint of making predictions about others, even the Churchlands admit that there was an evolutionary advantage for developing psychology. The eliminativists’ Comtean idea about the origin of psychology is wholly implausible. Without this origin, their degenerating research hypothesis loses force.

Folk Psychology is Radically False. This brings us to another pillar of the Churchlands’ (1984, 1992) position. It is the false and radically misleading nature of the folk psychology conceptual system that is the problem. Unfortunately, their assertions on this issue are inconsistent. In their account, folk psychology is analogous to the phlogisten theory or to alchemy or to the explanation of mental illness with respect to demonic possession — explanations that have been falsified by subsequent thinking. Except that folk psychology is even worse, suffering explanatory failure on what is called “an epic scale” (1992, p.9)

When claiming that folk psychology has not advanced, eliminativists have to mean that there has been no advance within the framework of folk psychology itself. In other words, they have to claim that since the time of the Greeks, no theory has been advanced which allows us to explain what the Greeks could not already explain. Let us examine this question by exploring two areas of “soft psychology” in which progress has occurred (a) developmental psychology and (c) clinical psychology.

Developmental Psychology. The concept of psychological development from birth to death was slowly accepted by twentieth century psychologists. The many different developmental lines and stages that have been elucidated — including the development of “interpersonal attachment,” “the self-concept,” “gender identity,” and the child’s “theory of mind” — all constitute advances

(Ainsworth, Blehar, Waters, & Wall 1978; Bowlby 1988; Brooks-Gunn & Matthews 1979; Damon & Hart 1982; Astington, Harris & Olson 1988).

For example, Damon and Hart (1982) and Harter (1983) claim that as children develop, their self-descriptions progress from a focus on a physical self, to an active self, a social self, and finally a psychological self. Rosenberg (1979, 1986) notes that shifts in self-understanding occur so that young children think like behaviorists, older children become trait theorists, and adolescents begin to resemble Freudian analysts, concerned with emotions and motivations.

Examples of self-descriptions with rough age ranges include, *physical self-description*: "I have brown hair" (3 to 9 years); *active self-description*: "I play baseball" (age 9 to 12); *social self-description*: "I'm clever and make people laugh" (age 12 to 15); and *psychological self-description*: "I am motivated to succeed and believe in helping people who are less fortunate" (beginning at age 15).

According to Susan Harter (1988), developmental research demonstrates that children are not naturally interested in the self. They are more directed to the outer world rather than the inner world, and show little interest in analyzing internal events such as thoughts and motives. As a matter of fact, children are not fully able to think about their own thinking (metacognition) until adolescence. The obvious conclusion is that if children cannot think about themselves reflexively, then insight-oriented approaches may not be the best psychotherapeutic strategy for them.

Harter describes how these principles applied to the case of a nine-year old client of hers. This boy had problems with truancy and school phobia. According to Harter, his problems were related to his devaluation of women; he wanted nothing to do with either his teacher or the principal, both of whom were women. His feelings toward all women were based on a realistic perception of one woman, his mother. In therapy, he treated Harter as incompetent and helpless as well. Because his attitude constituted a clear transference reaction, it should have been useful to the therapy, but Harter's insight-oriented interpretations of this boy's perceptions of her failed to help him change. The suggestion that he was inappropriately acting toward his teacher and therapist as if they were just like his mother had no effect on his behavior. His ideas about what people are like were based on what they do, not on some abstract psychological model of motivations and traits.

One day, after going home and finding his mother missing, the boy came

back to Harter's office. She spent the next hour helping the child actively search for his mother, leaving messages with the appropriate people, writing a note on the apartment door, and calling the police. It turned out that his mother was passed out in the apartment resulting from some combination of drugs and alcohol. After taking the child to get some supper, Harter once again tried to focus him on his feelings. At this point he looked at her with admiration and said, "Wow, I didn't think you knew how to do all that stuff" (p. 149). Although her technically elegant insight-oriented interventions had little effect on this nine-year old's attitudes, seeing her competently perform active coping behaviors made sense to him. This helped him understand that she was not like his mother, and opened the door for Harter to help him generalize this view to other women, especially his teacher. His school attendance finally began to improve. The point is that knowing about stages of self-concept development and calibrating clinical interventions to match a person's developmental level is important for helping them change.

One of Paul Churchland's (1985, 1995) requirements of "advancement" is being able to help people overcome psychological difficulties such as depression and thereby reduce their misery. Harter's example shows that knowledge of the different levels of self-concept development can be important for making interventions which help people change in a positive way.

The developmental perspective in general, which is prominent in counseling psychology, represents an important advance in knowledge. The basic idea of the developmental perspective is that personal relationships, careers, and families all systematically change over time, and that certain behaviors are more likely to occur at different developmental points. It is important to understand behavior in the context of a person's various developmental lines. For example, getting an advanced degree and starting your career can create certain challenges that will be experienced whether you are twenty-nine or forty-nine. The anxieties of writing a dissertation and negotiating new work demands take place in the context of normal career development, and need to be labeled as common developmental challenges. Psychologists have learned to consider the different "developmental lines" that continually converge in people's lives as a way to understand the context of their behavior. Often, a the therapist trying to help a client with what may seem like overwhelming difficulties can be effective by isolating the client's anxieties, such as starting a family and starting a career at the same time, and labeling both as developmentally normal. The act of *isolating* and *normalizing* these anxieties is itself a powerful therapeutic intervention.

Clinical Psychology. The psychoanalytic elucidation of defense mechanisms by Freud (1926/1961), A. Freud (1936), and Vaillant (1977, 1986) could be considered an advance upon the Greek's conception of human psychology, particularly the more subtle mechanisms such as projective identification, object splitting, and conversion of passive to active. Most students are not introduced to these ideas until college, and they often find them more than an elucidation of common sense. Although a controversial area, it is rather easy to show that there have been advances even in the domain of specialized clinical theories as well.

As psychodynamic therapists gained more experience in the middle of the twentieth century, they began to write about a category of disorders lying in between the psychoses and the neuroses. Most of these thinkers agreed with Freud's view that personality exists on a continuum from neurotic to psychotic. The mid-continuum disorders which they described at this time have come to be known as the class of personality disorders. Some examples of personality disorders that were recognized very early are the schizoid personality and the psychopathic personality (later called “sociopathic” and then “anti-social” personality).

Following Fairbairn (1954), Melanie Klein used the term “schizoid” to designate a developmental stage characterized by the splitting defense. According to Klein (1935/1964, 1945/1964, 1946/1975), young children do not perceive the ambiguity of good and bad in one person (or object); rather, they dualistically split the external world (and their selves) into good and bad part-objects. This process is largely influenced by internal drives such as love and hate. Developmentally, children need the splitting defense in order to protect their experience of the good from being devoured by their experience of the bad (their rage). Fixations at this stage supposedly led to serious psychopathology that could be classified as psychotic or neurotic at different points in time.

As thinking about these kinds of disorders progressed, therapists recognized a general category of personality known as borderline schizophrenia (Grinker, Werble & Drye 1968; Knight 1953; Stern 1938) or the “as if” personality (Deutsch 1934). By “borderline,” psychiatrists referred to a group of persons with an unstable identity and labile moods. These people were especially prone to rage, often suicidal, self-mutilating, and impulsive, but they could also be very competent, especially in their careers. Their suicidality and self-mutilation entailed an approach toward psychotic behavior, but it was transitory. They were thought to be on the borderline between the psychotic and the neurotic.

The “as if” label referred to apparent stability, people who looked as if they were functional, as if they had a consistent identity, and as if they had an integrated affect life. These types of clients entered therapy, formed a working alliance and appeared to make good progress, looking as if they were getting better. Inevitably the person experienced crises which time and again demonstrated that they had not made any deep gains. In the early seventies these concepts coalesced into what is called “borderline personality disorder” (Kernberg 1975; Kolb & Gunderson 1980). Persons with a more schizophrenic-like presentation were placed in a separate category called schizotypal personality disorder.

Psychological research has challenged older observations with respect to the borderline category, particularly the idea that persons with borderline dynamics are stabilized at a pre-oedipal stage, being more integrated than a psychotic (very early fixation) but less integrated than a neurotic (post-oedipal stage). Westen (1991) points out that people are vulnerable to developing borderline dynamics through adolescence. These dynamics also occur in response to trauma and therefore do not follow the simple fixation-regression model suggested by ego psychology theorists, such as Mahler (1971) and Jacobson (1964). Westen has also shown that in some ways (e.g., projecting malevolence) persons with borderline personalities perform worse than persons with psychosis, but in other ways (e.g., being psychologically minded) persons with borderline personality are more advanced than groups of normal individuals.

Familiarity with development psychology and studies of the normal child also allow Eagle (1984) and Peterfreund (1978) to point out that fixation-regression models which characterize child psychology in terms of adult psychopathology and characterize adult psychopathology in terms of child psychology are mistaken. The child is not an arrested adult, and adults, however disturbed, have experiences, cognitive skills, and emotional pressures that are not a part of normal childhood. Many clinicians now realize that, while fixation-regression models are valuable for understanding certain kinds of problems, they cannot be used as theory for explaining psychopathology in general. This is clearly an advancement in our knowledge.

In conclusion to this subsection: Although physicists no longer read Aristotle’s physics, philosophers still read Aristotle’s ethics. Paul Churchland, who suggests that psychologists should be more like physicists and reject archaic concepts, should be happy to know that most psychologists don’t

know much (if anything) about Aristotle’s or Plato’s psychology. I think it is also fair to hold that psychologists can at least be a little like philosophers. The reasons that Aristotle’s ethics still have value today is because some of the practical problems they were developed to negotiate still exist. Any commonality between Greek psychology and modern psychology can also be traced to common practical problems that both psychologies attempt to solve.

Contrary to what the Churchlands claim, there have been advances in psychology, not only in the last 2000 years, but in the last 50 years. New theories change both what we attend to and what problems we seek to solve. Analyzing someone’s level of self-concept development in order to enact meaningful interventions or trying to developmentally situate someone with borderline dynamics in relation to more psychotic and more neurotic kinds of disorders would not have been problems for the Greeks. Aimed with powerful measurement strategies, psychological researchers ask and test detailed questions that also would not have occurred to the less technically advanced Greeks.

Explanatory Failures. The third hypothesis in the degenerating research program argument refers to folk psychology’s supposed explanatory failures. When examining the explanatory failures offered by the Churchlands (e.g., the nature of mental illness, memory, and sleep), it is obvious that the only kind of psychology which could meet their criteria for a non-degenerating research program would be a near utopian psychology that can successfully explain everything. Their criteria are far too stringent. Few would claim that medicine has failed because it cannot completely explain the nature of diabetes, cancer, and multiple sclerosis. Nor does medicine’s inability to cure the common cold, something we have been plagued with since the time of the Greeks, mean that it is a failed research program.

The eliminativists favor neuroscience because they believe that it will *someday* reach a utopian stage of development and they reject psychology because it is not *currently* at a utopian stage of development. Their standards of comparison are not comparable. In the final analysis, it is difficult to defend psychology from a neuroscience that does not exist. Ironically, a more adequate defense of psychology from the eliminativists’ promissory arguments awaits advances in neuroscience. Even with the advances made in the past few years, we can see that eliminativists have had to retreat from some of their stronger claims.

8. Folk Psychology as Antithetical to the Natural Sciences

The most important criterion eliminative materialists use to separate psychology from science is its inability to be smoothly integrated with theories drawn from the natural sciences (their unity of science argument). Since there are supposedly no neat neurophysiological correlates for concepts such as belief and desire, they are not natural kinds. They do not, in Hempel's (1965) phrase, carve nature at the joints. I address the notion of natural kinds more fully in Chapter 10.

The unity-of-science critique was also important to a turn of the century eliminativist, John Watson. For Watson (1913) the defining features of natural science are experimentalism and objectivism. He thought that since we cannot observe conscious content, it cannot be a legitimate part of scientific psychology. In Watson's words, "This suggested elimination of states of consciousness as proper objects of investigation themselves will remove the barrier from psychology which exists between it and the other sciences" (p. 177).

The problem is that modern eliminativists need to offer us more than the claim that they cannot *imagine* how folk psychology can be integrated with neuroscience as it is currently understood. Their guesses about how higher order dimensions in neural networks will cluster themselves is based on ideology more than it is based on empirical research. For example Greenwood (1991) points out that describing actions as *aggressive* or *helpful* would be considered folk psychological descriptions, but those descriptions presume no kind of explanation. One could give a connectionist explanation of aggression. Various thinkers have even offered connectionist explanations of child development (Elman, Bates, Johnson, Karmiloff-Smith, Parisi & Plunkett 1996), social reasoning and social behavior (Read & Miller 1988), person perception and stereotyping (Smith & DeCoster 1998), hysterical personality disorder (Lloyd 1994) and Freudian psychoanalysis (Olds 1994).

Churchland's own description of the EMPATH network's ability to recognize emotions in facial expressions, including delight, pleasure, relation, boredom, and anger supports this point. We even see Churchland discussing the causal role of emotions, claiming that *grief is a response to the loss of a loved one*, which is the basis of Freud's theory of depression! It seems that some of the "thumb worn" categories of folk conceptual systems have been already inserted back into bottom-up neuroscience. To his credit, Churchland knew that this would be a possibility early on:

If the thumb worn categories of folk psychology (belief, desire, consciousness, and so on) really do possess objective integrity, then the bottom up approach will eventually lead us back to them (1984, p. 97).

For the sake of argument, assume that belief states and desire states will be given a respectable neuroscientific substrate in about 50 years. Once that happens, we will have to conclude that belief and desire had “objective integrity” all along, we just didn’t know it. Churchland’s proposal that we ignore these concepts until they are so confirmed parallels Thomas Szasz’s (1961) proposal that because there is no empirically identified brain lesion causing schizophrenia, all other observations suggesting that schizophrenia is a valid syndrome must be rejected. Szasz was mistaken, and so are the Churchlands, for the same reasons.

9. Conclusion

When eliminative materialists attack folk psychology, their defenders often claim that they are only attacking an inferior form of psychology and their actual intention is to improve psychology. Many psychologists would point out that if beliefs, desires, intentions, thinking, feeling, memory, and perception are eliminated, there isn’t much psychology left to improve. The attack on folk psychology is an attack on psychology in general.

I have argued that the thrust of the attack, the notion that folk psychology is a part of common sense and not science, assumes a greater dichotomy between science and common sense than is warranted. Not only does common sense change over time, it incorporates scientific findings as it changes. Many of our most cherished scientific beliefs gain robustness from common sense in the form of what I referred to as folk materialism. Furthermore, if scientific concepts are defined in terms of internal consistency and predictive validity, numerous folk psychological concepts can be considered to be scientifically respectable.

Finally, I addressed criticisms of folk psychology such as the claim that it has not advanced in 2000 years and that it is a degenerating research program. If we take “advance” to mean we can do something that the Greeks could not do, or ponder questions that would not have occurred to them, it is clear that psychology has advanced considerably in the past 50 years, let alone the past 2000 years. Not only have the more experimental branches of psychology

advanced, but areas of soft psychology such as development and clinical psychology have also increased our ability to understand human behavior and cognitive processes.

CHAPTER 5

A Critique of Anti-Anthropomorphism

1. Introduction

In the previous chapter I argued that the notion of “folk psychology” as a primitive, prescientific conceptual system that is rooted in common sense rather than systematic scholarly inquiry is a muddled concept. Common sense “folk psychology” is a simplification, being more of a rhetorical slogan than an accurate description of “psychology” and “psychologists.”

In this chapter I argue that, rather than studying the relevant research and concluding that the facts of neuroscience require a devaluation of psychology, the eliminativists belong to a tradition in the philosophy of science which takes the devaluation of psychology to be an important goal *a priori*. The reasons for the Churchlands’ position reduces to a sophisticated post-positivistic restatement of the anti-anthropomorphism made prominent by psychologists such as John Watson and B. F. Skinner, who were the original eliminativists. Eliminativism (or anti-anthropomorphism) is rooted in philosophical values. It derives from scientism, not science.

2. Behaviorism as eliminativism

In making their arguments, the Churchlands, if nothing else, are traveling the same road as the radical behaviorists. Although anti-anthropomorphism is usually associated with the radical behaviorists, Watson and Skinner, it is equally descriptive of eliminative materialists such as Paul Churchland. Eliminativism whittled down to its essentials is the application of anti-anthropomorphism to human psychology.

Even though the Churchlands are not radical behaviorists, the radical behaviorists were eliminativists. For example, John Watson (1930) traced the genesis of purposes, sensations, images, emotions, and will to the concept of soul, and through the soul to “folk lore.” He described folk lore as “a heritage of a timid savage past” (p.3). Similar to Paul Churchland, he traced psychology back to prehistory and magical thinking, and also referred to psychological concepts such as thought, desire, and perception as “medieval.” Watson further stated that he developed behaviorism because of his dissatisfaction with the contrast between progress in the natural sciences and the barren results of the so-called scientific psychology. For Watson, anti-anthropomorphism was part of the maturation of scientific thinking in both ethology and psychology.

The single most important criterion for being an eliminativist is making an analogy between psychological concepts and “eliminated” scientific concepts such as phlogiston and ether. Before Churchland, Feyerabend, or Rorty, B. F. Skinner (1938, 1956, 1974) compared psychological concepts to what he called “metaphorical devices” such as essence, force, phlogiston, and ether, all described by him as convenient explanatory fictions. Besides eliminating theoretical constructs such as ego, he held that we could also call into question the reality of sensations, ideas, feelings, and other states of conscious experience. He stated that these concepts are part of the clumsy vernacular, and we would do better by our children if we stopped teaching them mentalistic language. His commitment to anti-anthropomorphism led Skinner (1974) to describe himself as a radical behaviorist, not a methodological behaviorist.

In addition to the phlogiston analogy, another reason for claiming that Skinner was an eliminativist is his claim that whatever beliefs and desires are, they are never causes of behavior. According to Skinner, those objects of “introspective” self-observation that are not fictions, are just states of the body. Skinner’s (1957, 1974) definition of private events as states of the body mirrors the eliminativist line of Rorty (1965, 1979) or, to be more accurate, Rorty mirrors Skinner. In their common view, the introspection of internal states of the body is too inaccurate to be of scientific use. The public language for describing these states is either metaphorical (e.g., depression, tension) or a description of an external cause (e.g., stabbing pain). Better knowledge of these states awaits advances in physiology that will tell us what is really going on. In the meantime we need to make inferences, but not to beliefs and desires and intentions.

Skinner was not opposed to physiology either, but he thought that much of it was too speculative, especially in 1938. Critiquing the early behaviorists for substituting consciousness with speculative physiology, he followed Jacques Loeb in thinking that we had to understand behavior with reference to behavior as a whole, at a molar level of analysis. Reductionistic physiology just takes us away from behavior by explaining it away. The difference between him and Paul Churchland is that Churchland thinks that we can have non-speculative neurophysiological explanations. Churchland thinks that the best answer to “why” questions can be given with respect to neurophysiology, and Skinner thinks that the best answers to “why” questions can be given with respect to the contingencies of reinforcement. In short, Paul Churchland is an eliminativist-internalist and B. F. Skinner is an eliminativist-externalist

3. The construction of anti-anthropomorphism in psychology

Skinner himself (1938) published one of the earliest articulations of the received view regarding anti-anthropomorphism in psychology. According to Skinner, John Watson was trained in the Lloyd Morgan-inspired tradition of thinking scientifically about animal behavior. C. Lloyd Morgan’s Canon states that *it is improper to interpret an act in terms of a higher mental process if that act can be interpreted as the outcome of a process which is lower on the psychological scale*. In this model, automatic behaviors such as programmed instincts are lower on the psychological scale than those behaviors attributed to reason, will, and self-consciousness. As a graduate student Watson was exposed to these cutting edge ideas in ethology and, in 1913, took the important step of arguing that these ideas should be applied to research on human behavior as well.

The notion that anti-anthropomorphism began with Lloyd Morgan’s rejection of the anthropomorphism of George Romanes, was itself institutionalized by Skinner’s dissertation director E. G. Boring (1950) in his landmark *History of Experimental Psychology*. Boring’s book dominated the history of psychology for well over thirty years. Although Boring himself was vague about the degree of Romanes’ guilt, he did state that Morgan’s canon was a principle of parsimony and compared it to Occam’s razor. Occam’s razor is usually taken to mean that we should not make up entities to explain our phenomenon of interest if we can explain that phenomenon without reference

to those entities. For example, we don't need divine intervention to explain AIDS if we can explain it with reference to physical variables, such as retroviruses. In psychology, a commitment to anti-anthropomorphism is nearly synonymous with a commitment to parsimony — we don't need complex states such as beliefs and desires to explain a fish's behavior if we can explain it in terms of simpler states such as hunger.

It is still common for history of psychology textbooks to describe Romanes as the thinker who was guilty of using a method called "introspection by analogy," which was thoroughly anthropomorphic. They illustrate this point with examples, such as Romanes attributing feelings of affection, sympathy, and pride to birds. (Hergenhahn 1997; Schultz and Schultz 2000). Recent historical scholarship, however, contradicts these claims and suggests that Morgan's Canon does not prescribe parsimony. Neither does it prescribe anti-anthropomorphism. If this is true, then anti-anthropomorphism may not have been part of the inevitable maturation of scientific psychology, as is claimed by the radical behaviorists.

In contrast to the traditional story, Thomas (1997) shows that attributing a naive "introspection by analogy" methodology to Romanes is a misreading. Romanes was interested in collecting observational evidence of animal behavior and was willing to publish other people's observations, especially if he could corroborate them with a third observer's account of the same phenomena. In the service of objectivity, he quoted people's observations verbatim, including their interpretations. Romanes did not endorse interpretations such as "jealous fish," he was just interested in the descriptions of behavior provided by what he called the "anecdote mongers" (Boakes 1984, p.25). Boakes (1984) states that Romanes' goal was to bring systematic classification to comparative psychology, and notes that he was quite critical of popular books on animal behavior.

Boakes claims that Romanes was willing to attribute conscious cognition to animals if their behavior showed some evidence of being affected by past experience. As a matter of fact, "affected by past experience" was *the* late 19th century definition of intelligent action. Romanes held that since animals modify their behavior when conditions change, they are not just inflexible automatons as Descartes thought. His point was that we can attribute conscious cognition only to animals that show this kind of flexibility.

Costall (1993) shows that Morgan's Canon was developed, not to support the behaviorist model, but, just the opposite, to set up some rules for determin-

ing when conscious cognitive processes could be ascribed to animals. Morgan originally wrote in the tradition of Darwin's "evolution of mind" project and, like Darwin, clearly believed that subjective experience was an important category of evidence available to the researcher. Similar to Romanes, Morgan rejected the Cartesian idea that animals are merely automatons or reflex machines. In his discussion of supervenience, Jaegwon Kim (1994) specifically identified Morgan as one of the first emergent materialists, who believed that consciousness was dependent on the brain, but not reducible to it.

Boakes (1984) claims that Morgan, like Darwin, thought that we need to coordinate subjective and objective evidence with each other. He also believed that it would be unscientific to categorically deny one kind of evidence. Morgan merely prescribed more conservatism with respect to animal consciousness than Romanes did, but their goals were the same. Rather than denying conscious cognitive processes, Morgan thought it a mistake to *assume* that animal cognitive processes are the same as human cognitive processes.

With respect to the principle of parsimony, Costall (1993) and Thomas (1997) both claim that Morgan specifically rejected the idea that simple explanations are the best explanations. Dennett (1978) similarly critiques Skinner for confusing his simpler explanations for being necessarily better explanations. Since Copernicus's heliocentric theory was so much simpler than the Ptolemaic theory, it often seems that simpler is better, especially because simple models are easier to understand. It is perfectly reasonable, however, that a more complex explanation could be the correct one. Simplicity is no guarantor of truth.

The real issue with Morgan's Canon is not complex versus simple, but *higher versus lower*. Lower does not mean simpler. It means less cognitive, and also less metacognitive, and therefore less self-aware. The goal of the early ethologists was not to eliminate cognition, but to avoid making cognition more sophisticated than it actually is. For example, since infants don't have the cognitive capacities to attribute beliefs to others, we can't say that they cry in order to make their mothers think that they are hungry. Five-year old children, however, do have a capacity to attribute beliefs to others, so we have reasons for attributing their actions to more sophisticated cognitive mechanisms, for example, hiding the broken figurine so that their mother will not draw the relevant conclusion about how it got broken. This is how Morgan thought about animal behavior. It was a prodigious leap for the radical behaviorists to claim that anti-anthropomorphism in psychology is the natural consequence of his model. The roots of anti-anthropomorphism lie elsewhere.

E. G. Boring's history of behaviorism as: "science wins the war in ethology" was probably influenced by E. L. Thorndike (1898, 1911). Boakes (1984) suggests that Thorndike insulted both Romanes and Morgan in order to make a name for himself. His scornful deprecation of Romanes can be traced back to his doctoral dissertation which was published in the *Psychological Review* in 1898. This article sets up a contrast between what Thorndike calls the anecdotal school and his own experimental approach. In giving an excellent critique of the problems with case study methods, Thorndike highlights Romanes' published anecdotes, particularly the problematic ones. He also refers to Morgan as the *sanest* writer on comparative psychology, thereby indirectly denouncing everyone else and suggesting a greater separation between Morgan's and Romanes views than there in fact was. In reworking this article for a 1911 book chapter, Thorndike became openly scornful. This made his approach seem both reasonable and original. These career-making exaggerations were taken as the literal truth by subsequent scholars.

When we study those scholars who inspired the early behaviorists, it is also useful to consider the reasons why they did not adopt a thoroughgoing anti-anthropomorphism. Essentially, they thought it was too extreme. For example, Costall (1993) shows that rather than being unaware of the complexities of interpreting animal behavior, Romanes argued that Morgan's objective methods for preventing illegitimate inferences about mind from behavior, if used consistently, would need to be applied to the study of human behavior as well. He appreciated Morgan's point, but rejected interpreting it in its most extreme form. Morgan agreed with this caution and did not think what came to be called "radical behaviorism" was warranted.

Even Thorndike, upon hearing the first public presentation of John Watson's 1913 "Psychology as a Behaviorist Views It," commented that he feared it could become a restrictive orthodoxy (Joncich 1968). Dewey (1918, 1919), although calling himself a behaviorist, was also critical of Watson. He thought that a study of consciousness was compatible with a behavioristic standpoint, and suggested that "experimental behaviorists" willfully denied "the existence of certain facts...merely because their technique [was] not yet developed to the point of dealing with them" (p.15). He also considered Watson's tendency to ignore social reality to be narrow-minded.

Until the 1940s, a majority of psychologists thought that extending scholarly caution regarding inferences about subjectivity into a principle of radical anti-anthropomorphism was mistaken. Nor were they ignorant of the

history of science; they just rejected the behaviorist version of that history. Following William James, turn of the century psychologists were willing to accept a degree of methodological pluralism. Research about expectations, hopes, fears, and other states of conscious experience was at least potentially legitimate. The pragmatic openness of the James-inspired turn of the century psychologists was incompatible with the targeted focus of radical behaviorism.

4. Ethology's rejection of eliminativism

Ethologists have long rejected physiological reductionism because lower level physiological explanations would be far too complex and unwieldy to use as working explanations of behavior. Tinbergen (1954) stated that to try to understand the causes of behavior by leaping into the study of neurons or neuron systems is "extremely harmful" (p.115). Dawkins (1986) noted that detailed neuron models would be "baffling in the extreme" (p.970), making it difficult to see how the whole organism works. Kennedy (1992), while sympathetic to an increased understanding of neurophysiological substrates, holds that evolutionary explanations utilize a different model of causality than physiological explanations.

Ethologists have traditionally followed Skinner in seeing behavior as having a set of rules that can do considerable explanatory work on their own. For example, understanding why abstract reasoning confers survival value does not require understanding the biochemistry of cognition. Kennedy also suggests that another reason ethologists have not embraced physiology is that evolutionary explanations are more fun to develop.

The argument for eliminativism/anti-anthropomorphism partly turns on the extent to which a thinker accepts either a discontinuity between species or a radical continuity between species. Those who accept discontinuity are less likely to be eliminativists. They tend to see humans as qualitatively unique (e.g., Kennedy 1992) and hold that ethologists study animals for their own sake. They believe that it is perfectly appropriate to talk about human beings in intentional language, but a mistake to uncritically apply this language to animal behavior. Examples of unique human attributes include self-knowledge and the capacity for symbolic thought (language). In this view, anthropomorphism is the error of projecting *uniquely* human desires and cognitive processes into animals, not the error of attributing any beliefs and desires to animals at all.

Eliminativists hold a continuity-of-species position, as exemplified by J. B. Watson, B. F. Skinner and Paul Churchland. Watson (1930) even believed that his commitment to the continuity-of-species was the reason for people's early hostility to behaviorism. The primary goal of continuity-of-species thinkers is to find general rules for understanding behavior. Watson specifically suggested that by eschewing consciousness, we could develop a unified set of principles to explain animal *and* human behavior (even though he expressed some skepticism about this possibility). The thrust of Churchland's argument against propositional attitudes is that all life on this planet shares the same genetic material. He notes that many species are intelligent, but since only human beings possess propositional attitudes, propositional attitudes cannot describe the (biological) nature of intelligence. For eliminativists, the error of anthropomorphism is not one of projecting human intentions into animals, rather, the very concept of intentionality itself is a mistake.

Unfortunately for the Churchlands and their like-minded colleagues, the continuity-of-species argument itself cannot justify eliminativism. As early as 1939, W. H. Roberts pointed out that continuity cuts both ways. If the continuity-of-species argument is true, some of the psychological concepts we use to describe humans should be appropriate for animals which are similar to us. Costall (1993) claims that Darwin himself adopted exaggerated anthropomorphic interpretations in order to argue for the continuity between species. Eliminativists cannot beg the question and say that continuity requires a rejection of folk psychology, because accepting folk psychology *and* the continuity-of-species may require a limited "anthropomorphism" of some animals. This latter version of the continuity-of-species was the theoretical context in which Morgan's Canon was formulated.

A distorted view of the history of science also influences some thinkers' commitments to anti-anthropomorphism. The distortion begins with an attempt to understand the relationship between Aristotelianism and the scientific revolution. Aristoteleans conceptualized an object such as an acorn as having a purpose — an acorn seeks to fulfill its nature. This view was inspired by the facts of biological maturation. According to Aristotle an acorn grows into a tree because that is its nature (or design). Design is what Aristotle calls a *formal cause*.

Unfortunately, Aristotle failed to distinguish between types of objects, especially between biological organisms and inanimate objects. For example, he stated that a rock falls to the earth because that is its natural place. A falling

rock is fulfilling its nature, which is to be at rest on the earth. The behavior of animate objects was therefore understood with respect to fixed natures (or essences).

The seventeenth century physicists made a significant contribution to human understanding by disproving the physics of Aristotle. For example, Newton's first law argues that motion is constant and linear. A moving rock naturally travels at a constant velocity, and unless acted on by another force, rectilinear motion (not rest) is its natural state. The seventeenth century physicists de-biologized inanimate objects, teaching us to think of them mechanistically, what Aristotle called explanation with respect to *material* and *efficient* causes (matter in motion). The distorted view that some thinkers hold is that all objects which are the target of scientific investigation have to be de-animated.

The solution to this quandary is to admit that Aristotle was overly inclined to think of everything biologically. As Daniel Dennett (1978) and Ralph Ellis (1995) have noted, any entity with a design that possesses information can be understood as intentional or goal-directed. Biological organisms can still be understood to have an internal design (a genotype), and their behavior can partly be explained with respect to that design (with Darwin providing a mechanistic account of how the design came about). Because complex organisms are designed by natural selection for information-mediated adaptation, we can keep the notion of action, or goal-directed behavior. Therefore "science" does not require the total elimination of purpose and goals, i.e., it does not require radical anti-anthropomorphism.

5. Eliminativism, Science, and Scientism

In this subsection I argue that eliminativist and anti-anthropomorphic arguments are manifestations of an unwarranted and conservative understanding of the scientific process.

Scientism and religion. Eliminativism is a position taken by those who are inclined to adopt scientism. Scientism is the (implicit) presumption that, in addition to the superiority of scientific methodology, the more rigorously and exclusively we use the scientific approach in any endeavor, the more superior the product. Putnam (1994) would add the more scientific the thinker, the more Utopian their actual expectations regarding future scientific achievements.

Instead of listing a set of necessary and sufficient conditions for someone's being scientific, scientism is best defined by a cluster of attitudes involving (a) a commitment to the unity of science, (b) believing in the practical benefits of prediction and control and (c) accepting a materialistic/realistic ontology. None of these beliefs themselves are scientific. More subtly and more importantly, (d) those adopting scientism continue to conceptualize science in terms of its 17th and 18th century cultural war with religion. In this story, the hero is the scientist and the villain is the defender of a traditional view.

Sorell (1991) defines scientism as the deprecation of the non-scientific because it is unscientific. He suggests that scientism is usually a reactive position, taken in response to those thinkers who maintain common sense prejudices when those prejudices are called into question by scientific findings. The most popular exemplar of this kind of prejudice is Cardinal Bellarmine defending the Aristotelian cosmology from Galileo's observations with the telescope. When the metaphysical technicalities of the argument are ignored and the debate is framed as "how many bodies actually exist in the solar system," we all want to see ourselves as being on Galileo's side.

A reliable correlate of the extent to which someone adopts the science versus religion narrative is the extent to which their intellectual history is Comtean history. It should be clear to readers that eliminativist history is Comtean history, i.e., "intellectual history consists of a process of evolution from the theological to the metaphysical to the positive/physicalistic stage of science." The war with religion involved a battle with not only supernatural concepts, but metaphysical concepts as well. Comte's idea of evolution as nature perfecting itself also underlies claims about science's potential contribution to society — Skinner's and Churchland's Utopian heaven-on-earth arguments. Comte himself was an extreme example of someone who adopted the scientific narrative in pure form — where the scientist hero replaces the priest villain in the position of power.

Another indicator of conceptualizing science in terms of its 17th and 18th century battle with religion is critiquing any supposed non-scientific perspective by associating it with theology. Churchland (1984) aptly notes that the use of religious arguments has a sorry history, leading people to ignore the facts of nature in the name of orthodoxy. With respect to the 17th and 18th century culture wars, this charge constitutes a powerful rhetorical strategy because any explanation that can be associated with theology can be written off as a non-explanation.

A prominent example of a scientific thinker using this strategy is Bunge (1977), who held that the mentalistic vocabulary was actually coined by religion and dualistic philosophers. Bunge stated that any psychology which cannot be accounted for in terms of neuroscience is supernaturalistic. He also believed that ordinary knowledge is popular superstition. As would be predicted, Bunge also held that psychology has to become a natural science.

Suggestions that “mind equals soul,” “psychology equals spiritualism,” and related ideas such as “ego strength as a cause of mental illness equals demonology,” or a comparison between psychology and medieval metaphysics can be found in the writings of those thinkers most committed to eliminative and biomedical materialism. (P.M. Churchland 1984, 1992; Watson 1930; Skinner 1953, 1956; Andreassen 1984; Torrey 1983,1992). Even folk psychology realists such as Horgan and Graham (1993) in a tongue-and-cheek article identify the groups participating in the argument about the truth of folk psychology as secularists, churchmen, agnostics, and southern fundamentalists.

The most prolific wielder of psychology/theology analogies has been Rorty (1979, 1982). For example, he has compared the argument between mentalists and materialists to an argument between inspired theists and inspired atheists. The mentalists and theists just know that there are such things as God and pains that play causal roles in the world, and the materialists and atheists think that, since those things don't have any incremental validity with respect to our explanatory power, we don't need to believe that they exist. Another example can be found in his 1982 comparison of the contemporary intuition that “there is more to pain than brain states” with 18th century intuitions such as “if God does not exist, everything is permitted”; “man's dignity consists in his link with a supernatural order,” and “one must not mock holy things” (p. xxix–xxx).

By conceptualizing science as still fighting the battle with religion, anyone who rejects subsuming all knowledge to current theories in the natural sciences is seen as a villain in the Bellarmine sense. Those who defend the unity of science are heroes, much like Galileo. Although effective rhetoric, reducing discussions about what constitutes legitimate evidence in the modern world to a battle between science and religion is unacceptable.

Understanding the limits of conceptualizing science within the framework of the 17th and 18th century culture wars suggests a reason for rejecting scientism. By virtue of growing up in a society founded upon the victory of science in the war with religion, we are all trained to accept scientific explana-

tions as the most basic explanations — no matter what else we may believe. Science and common sense are not mutually exclusive, psychologically speaking. Ask people what AIDS is and they will tell you it is a virus. No matter what else they believe, they believe it is a virus first. To take another example, explaining starvation in Somalia and rape gangs in Bosnia with “because Jesus wants it that way” just doesn’t work for us.

Society has changed enough in the past two hundred years that taking the side of science no longer equals a critique of the current social order. Since the late 19th century, taking the side of science has constituted a defense of the current social order. To borrow what I think is an effective rhetorical strategy: Before Christianity became the official religion of the Roman empire, being a Christian was dangerous. As the philosopher Kierkegaard might point out, after the establishment of Christendom, not only was it safe to be a Christian, in some places it was dangerous to *not* be a Christian. Just as Christianity is most dogmatic when its proponents inappropriately continue to think of their religion in terms of being fed to the lions (or the government), science is most dogmatic when its proponents think that disagreeing with their version of the unified scale of nature is automatically an expression of Medievalism.

As a result of science’s current cultural role, I am unimpressed with the reactionary defense of science. When we come to a point in society when even our philosophers think that in order to be respectable they have to be “cognitive scientists” and engage in their own empirical work, and fundamentalists defend Genesis by calling it “creation science,” science is safe enough.

Scientism and conservative naturalism. There is a corollary epistemological root to scientism, i.e., naturalism. The argument of the scientists in their battle with religion was that we have to try explain the natural world on its own terms. Using God as an answer is fatalistic because it prevents us from figuring out the physical mechanisms of things such as earthquakes and schizophrenia, and therefore prevents us from changing the world for the better. Put in the form of a principle, naturalism versus supernaturalism becomes very similar to Occam’s razor: “do not multiply entities beyond necessity.” As a committed naturalist I agree with this principle, but still think that a distinction can be made between pragmatic and conservative naturalism.

The most prominent philosopher in the tradition of conservative naturalism is David Hume, who in 1748 made the point in a rather dramatic way:

If we take in our hand any volume; of divinity or school metaphysics, for instance let us ask, *Does it contain any abstract reasoning concerning*

quantity and number? No. Does it contain any experimental reasoning concerning matter of fact and existence? No. Commit it then to the flames: for it can contain nothing but sophistry and illusion (in P. S. Churchland 1986, p. 245).

In other words, if we adopt caution, it is a greater error to talk about something that does not exist than to ignore something that does exist. Psychologists call this a concern for Type I as opposed to Type II error.

Eliminativists are not Humeans nor do they secretly subscribe to Hume's views in the philosophy of mind. They do, however, subscribe to his epistemological conservatism, exemplified in Hume's quote about avoiding illusions. Hume was clearly important to Watson, being one of the few philosophers he read and appreciated. Skinner himself explicitly preferred the Humean positivism of Ernst Mach to the logical positivism of the Vienna circle.

The Churchlands, of course, standing on the shoulders of Quine, Feyerabend, and Kuhn, have a complicated relation to Hume. To the extent that Hume is a foundationalist who believes in both the Given and the analytic-synthetic distinction, they are anti-Humeans. They actually make almost no reference to him, except for Patricia Churchland (1986), who favorably quoted Hume's phrase about committing non-mathematical and non-experimental reasoning to the flames. She also states that this principle helps assure that

respectable, worthy discoveries in science could be distinguished from mere speculation and idle theorizing, and from "authoritative" deliverances of sages and theologians (p. 245).

Though their philosophy is not Humean, their spirit is. Although the Churchlands prefer to think of themselves in the revolutionary spirit of Paul Feyerabend, rather than being anarchists, their eliminativist attack on psychology is restrictive and limiting. Eliminativists transform Feyerabend's claim that we ignore many perfectly good facts because they are not formulated in our preferred theory into a claim that we should ignore certain facts because they are part of a particular theory, i.e., folk psychology.

Broadly speaking, naturalism is compatible with many different models of nature, even psychoanalytic models of nature. As a committed naturalist, Freud (1927, 1930) clearly considered religion the enemy. He held that religious beliefs were illusions, and eventually characterized them as delusional. In 1930, this Jewish psychiatrist associated the evils of the growing Nazi movement with the same psychological tendencies that gave rise to

organized religion. He would not have agreed that his ideas about the repetition compulsion and primary process thinking were theologically-based. The repetition compulsion may not be easily explained in terms of genetics or connectionist networks, but that does not make it supernaturalistic.

As stated, the difference between pragmatic and conservative naturalists can be found in their attitude toward error. The conservative approach tends to avoid what experimental psychologists call Type I error — they don't want to say anything untrue or make mistakes. The pragmatic approach is more concerned with Type II error — they don't want to dismiss possible truths or hamper progress. A concern with Type I error is more restrictive.

Many proponents of therapy will argue that although their models and techniques may be imperfect, until they are conclusively proven to be harmful and until workable alternatives are offered, their first priority has to be potential client gain. Those clinicians who oppose eliminativist arguments claim that their first priority has to be, not conformity with the current state of the natural sciences, but the development of new and useful concepts and techniques.

6. Narrow strategies versus narrow philosophies

When anti-anthropomorphism was becoming popular in the late 19th and early 20th century, those thinkers sympathetic to it were charged by their opponents with being narrow. This debate was related to an influential argument by the German thinker Haeckel who held that all life was ensouled (Boakes 1984). Those taking the Leibnizian-Haeckel position thought that all forms of life possess a kind of consciousness, and they saw the ethologists who eliminated consciousness from animal research as narrow-minded. The same basic charge was made by the Vitalists in response to the Mechanists.

These critiques were mistaken. They were mistaken because narrowness and skepticism with respect to evidence is extremely important to scientific progress. For example, it was valuable for psychologists to study only behavior and suspend assumptions about what internal processes may be mediating that behavior. No responsible psychologist would teach students about learning theory without discussing the contributions of behaviorism. Any reasonable research strategy involving suspending firmly held beliefs is worth a try. In the same vein, no one should disagree that narrowness with respect to

learning more about the workings of the brain is also going to be a necessity.

Newton famously did not explain gravity. He considered his law of gravity to be a mathematical summary of past observations, and offered no further explanation. March and Freeman (1962) note that, personally, Newton thought a lot about why two objects exert an attraction on each other. He decided that some invisible medium, called spirit (or ether), must fill the entire universe, and gravity worked through that medium. In his role as a physicist, however, Newton did not allow the spirit-ether hypothesis into his professional model. He refused to assume a hypothesis about gravity.

Skinner's argument for reinforcement worked much like Newton's argument for gravity. Skinner defined reinforcement as any event that increases the probability of a response. He did not explain why a specific event increased the probability of a response, he just observed that it did. Also like Newton, matters were different for Skinner on a personal level. I once heard a psychologist tell a story about seeing Skinner on an elevator at a conference, and when he pushed the button to close the elevator doors and the doors did not close, Skinner claimed "these things have a mind of their own." In his role as a radical behaviorist, however, he did not allow mentalistic constructs into his professional model.

With respect to the question of anti-anthropomorphism, it is important to distinguish between the technically-oriented scientist who is answering his or her own detailed research questions and the "big idea" thinker who writes about the nature of science, mind, and reality. When a narrow researcher's professional model becomes a normative standard or template for a personal worldview (when bracketing becomes widespread elimination), we move beyond, "these are the limits we need to accept in order to answer this research question," to, "anyone who does not accept these limits will not find out about how the world really works." This is a big leap. It makes it impossible even to try solve Sellars's problem of learning how things, in the broadest possible sense of the term, hang together, in the broadest possible sense of the term.

7. Conclusion

In conclusion, the argument of the past two chapters suggests that, although accepting the epistemological critiques of Quine, Hanson, and Feyerabend, the Churchlands have kept the most restrictive assumptions of the positivists

intact. Their idea of folk psychology depends on a more strict distinction between science and common sense than is warranted. This dichotomy can be traced to various scientific beliefs, including an assertion of a Comtean history of science, Humean parsimony, and identification with the scientists who battled religious orthodoxy in the 17th and 18th centuries. If we reject these beliefs, science and common sense become less mutually exclusive, and the range of data acceptable to a theorist is increased.

I don't wish to be so libertarian that we start talking about God and angels as causes of natural events again, but beliefs, desires, and consciousness are at least as naturalistic as electrons. One would hope that the Churchlands eliminativism could evolve in the same way that Richard Rorty's did. Although they think of themselves as liberal revolutionaries, eliminating one area of discourse from the conversation in *a priori* manner is a more conservative strategy than we should be willing to adopt.

CHAPTER 6

The Anchors of Psychology

1. Introduction

In Chapter 4 I argued that the eliminativists' notion of psychology is muddled. They claim to reject only folk psychology, but their definition of folk psychology is so broad that it covers every area of psychology except radical behaviorism. Their attempt to critique the framework of psychology because its plausibility is grounded in common sense ignores the fact that the plausibility of their own beliefs about the brain's importance in understanding behavior is also grounded in common sense. This undercuts the radical distinction between science and common sense that is so important to their position.

To a limited extent, the eliminativists' muddle is not their fault. Because it is not clear what the term "psychology" covers, it carries some inherent linguistic muddle all by itself. Although most thinkers assume that there is a common core to clinical, counseling, social, developmental, cognitive, and biological psychology, that core is rarely specified in a systematic way. To make the world safer for psychology, however, it is important to understand what we mean by psychology. In this chapter I outline a more basic framework for thinking about psychological explanation and understanding. I call it the anchors of psychology framework. My anchors of psychology are less certain than foundations, but sturdier than the to be eliminated "metaphorical fictions" of Skinner, Andreasen, and Churchland.

My justification for these anchors is similar to Theodore Millon's (1991, 1994) justification for his fundamental dimensions of the human personality. According to Millon, in the history of clinical psychology, certain concepts have emerged in theory after theory, even though each theorist has different names for them. These robust concepts, important to the "classical" thinkers in psychoanalytic psychiatry, "rediscovered" by the social behaviorists in the

1960s, and prominent in the programs of experimental cognitive psychologists, by their perseverance deserve to be called “fundamental.”

In the same way, specific dimensions of analysis reoccur again and again in the writings of any “big idea thinker” who attempts to develop comprehensive models of human behavior, no matter what his or her philosophical orientation with respect to “psychology.” I propose that these robust dimensions of analysis are better suited than beliefs and desires to describe the fundamental framework of psychological understanding and explanation. The anchors I will discuss include (a) psychology as a level of analysis between internal and external worlds (b) psychology as explanation with reference to what is in the head and (c) psychology as understanding with reference to the self.

2. Psychology Is a Level of Analysis Between Internal and External Worlds

Thinkers such as Langs (1999) have proposed that psychology should be understood to be a subspecialty of biology — especially if consciousness can be considered to be a biological phenomenon. In this section I argue that psychology is intimately related to biology, but it is best understood as a speciality in its own right.

The concept of levels and the framework of psychology

One of the most persistent issues in the philosophy of mind is trying to decide whether psychology is in any way autonomous from neuroscience. At a minimum, the autonomy of psychology means that the physiological is not the only correct level of analysis for conceptualizing the mental.

Hillary Putnam’s (1975b) example about the physics of round pegs and square holes is the simplest example of more than nominal autonomy. According to Putnam, no one has found it very important to give an account of why square pegs do not fit into round holes with respect to the elementary particles which constitute the pegs and the hole. But ultimately, the elementary particles are what is really there. Even if we had a subatomic account of why square pegs do not fit into round holes, it is unlikely we would use it as an explanation when the higher level of analysis (square shape, round shape, solidity) is more practical. This is probably because our brains did not evolve

to deal with “trillions of subatomic particles” in a non-inferential manner. Sometimes, what counts as a “satisfying description” is limited by what we can reasonably use.

Dennett (1995) points out that extreme reductionism is so silly that no one really believes it. He does this by showing how extreme reductionism might lead to works such as “A comparison of Keats and Shelly from the molecular point of view” or “The role of oxygen atoms in supply-side economics” (p. 81). Dennett’s *reductio ad absurdum* effectively critiques what he calls greedy reductionism.

Another useful metaphor for understanding the psychology-physiology relationship is the mirror-brain analogy. The brain as the substrate of psychological states is analogous to a mirror as the substrate of reflections. Using the mirror-brain analogy, many biologically oriented thinkers believe that because there will be no reflection if we take away the mirror, the mirror causes the reflection. Thinkers such as the psychiatrist Nancy Andreasen (1984) in effect argue that because the reflection is understandable in terms of being a state of the mirror, all we need to do is look to the mirror. Paul Churchland’s (1995) views on psychiatry are similar.

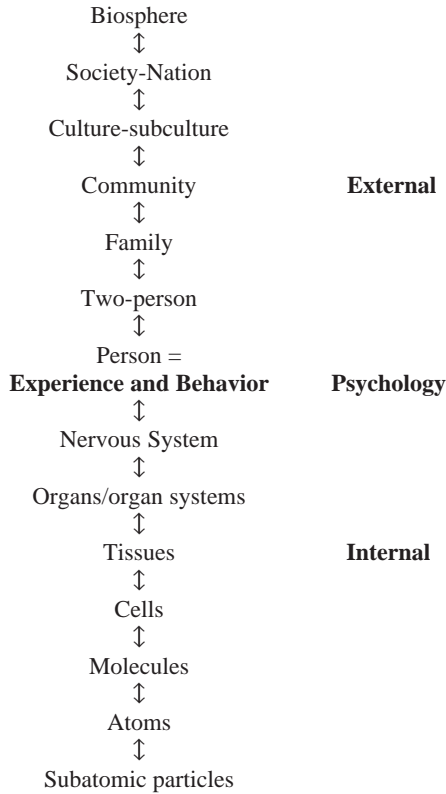
Focusing on the exclusive importance of the substrate requires holding that a sufficient explanation of the reflection can be given with respect to the physical properties of the mirror when it is in a particular reflecting state, e.g., the physical state that the mirror is in when reflecting a person in front of it versus the physical state the mirror is in when reflecting only the bathroom wall. We can also change the reflection by modifying the mirror, i.e., make the person look tall or thin, or give every reflection a green tint.

The problem is that although we can give an explanation of the reflection by describing the state of the mirror, in doing so we ignore the person standing there fixing his hair, we ignore the bathroom wall, and we ignore the lighting in the room. These are also important causal influences on the reflection. Since the person, the wall, and the illumination are important elements of the reflection, there is more to sufficiently understanding the phenomena than understanding the substrate. If the analogy holds, the faults of focusing only on the mirror parallel the faults of focusing only on the brain.

One advantage to the levels of analysis perspective as used in philosophy by McCauley (1996) or in psychiatry by Engel (1977, 1980) is that it helps us avoid the common sense dualists’ mistake of formulating the question about explaining behavior as one of psychology or biology.

In Engel's formulation, nature is organized on a hierarchical continuum (or overlapping levels of organization). An example of a low level of organization would be the subatomic level. Above it is the atomic level, the cellular level, the organ level, the organ system level, the psychological level, the social level, and the cultural level. In a levels of analysis approach, there is interaction between levels and the closer together the levels, the greater the interaction. For example, the cellular level will have more direct implications for the organ level than it does for the social level. We can therefore acknowledge important interactions between biology and psychology (the brain as substrate thesis), without having to eliminate one level of analysis.

Engel's Hierarchy



Wimsatt (1976) offers a more complex analysis of levels than Engel. Although Engel's diagram is easy to understand and illustrates the basic point well, it is probably too neat. It is unlikely that there are equal intervals between levels or even that they are best ordered on a single vertical line. Wimsatt thinks of a level as "local unity" or a point of maximum regularity and predictability, where events naturally cluster together. This makes more sense than the individual node approach of Engel, but Engel's model is the clearest way to introduce the idea.

Not surprisingly, Patricia Churchland and Terry Sejnowski (1992) betray a stubborn disregard for psychology in a footnote to their own diagram about levels of analysis in the nervous system, indicating that a more accurate diagram would include a social level above the neuroscientific level. The psychological level is not even a part of their picture. Having a list that jumps from biology to sociology, with nothing in between further supports my contention in Chapter 5 about the Comtean roots of eliminativism.

In any model, what levels we pick to be on the continuum are of course open to debate. Sometimes two levels work together so well that what Darden and Maull (1977) call an *interlevel theory* is created. In addition to individual theories that span two or more domains, systematic interlevel contexts such as biochemistry, neuropsychology, molecular genetics, and certain aspects of social psychology are domains in their own right. In social psychology, for example, it can sometimes be hard to know whether you are doing psychology or sociology, with different researchers holding different opinions on the issue.

In this context, we can think of psychology as a level of analysis existing between the biological and the social on Engel's hierarchy. If psychology was not already a part of our disciplinary geography, we would have to invent it as an interlevel context. We would have to propose such an interlevel context because any epistemic engine's models of reality and sources for action are going to be a compromise result of an interaction between internal biological and external sociological factors.⁷ From the standpoint of the biological individual trying to explain his or her own behavior, psychology therefore becomes a level of analysis between *internal* and *external* worlds.

Thinking about psychology as a level of analysis where internal and

7. Skinner would reject this. He says that the only level of analysis we need to understand behavior is the behavioral level of analysis. Mentalistic psychologists should argue that we also need the physiological level, with psychology being the resulting compromise formation or interlevel context.

external interact is the beginning of an alternative model for describing the framework of psychology. Rather than being defined in terms of contents such as belief and desire, the framework of psychology is more realistically defined as an interlevel context involving an internal versus external world interplay, with its resulting problems, types of evidence, and methods. One of these problems is disagreement about whether psychology should be more like biology, or more like sociology.

This interlevel context is so important that it has become a level of analysis itself. To eliminate psychology you have to eliminate that entire level of analysis — not just reconfigure it using alternative concepts. Any domain at this level of analysis will be a psychology. This idea closely matches a suggestion by Clark (1996), who wrote that all folk psychology commits us to is the idea of causally important internal states.

Substrates as tautologies and as advancements

The most obvious response to someone who wants to talk about psychology only in terms of neurophysiology is the infinite regress critique; i.e., if psychology is really the activity of the nervous system, then neurophysiology is really the result of biochemical interactions, which in turn are really the activity of subatomic particles. If sensations are “really” brain processes, then brain processes are “really” actualized genetic programs, which are “really” incredibly complex arrangements of atomic particles. Ultimately, everything will have to be eliminated in favor of subatomic physics. Scientific thinkers are most vulnerable to this regress because physics is presumably more scientific and therefore more real than biology or psychology.

The regress is such a ridiculous consequence that eliminativists have to admit multiple levels of analysis. They merely want to make separate levels more consistent with each other, their famous unity of science goal.

Thinking in terms of levels of analysis also presents problems. One of these problems is a thorny question regarding the relationship between a particular level of analysis and its lower level substrate. In this respect, one of the most common errors in psychology and psychiatry is a kind of psycho-neural dualism which leads people to think that identifying a psychological state as having a neurophysiological substrate is news. More people than not confuse what is actually a tautology with an advance in knowledge.

For example, the-brain-as-substrate thesis is a necessary consequence of

any “levels of analysis” analysis. Therefore, no matter how a state of depression develops or is maintained, it is a brain state. Even the great peripheralist Skinner believed that, because how the organism behaves is a result of natural selection, all behavior is inherited. He just thought that what was inherited was a capacity to be reinforced by certain conditions. Unfortunately, when some psychiatrist announces that people have a biological predisposition for depression, we tend to think that something new has been said. It hasn’t. An effective way to point out the emptiness of meaningless brain talk in psychiatry and psychology is to ask the question “*Why doesn’t a rock get depressed?*” The answer is “Because it doesn’t have a biological predisposition for depression.” We readily see the emptiness of the answer in the case of the rock, but fail to see it with respect to depression, anxiety, dissociation, etc, in humans.

From a materialist standpoint, since the brain is the organ of the mind, any psychological account is going to have a neural substrate. For any conceivable psychological process, some kind of a neural grounding can be given. In the same vein, if connectionism is a valid model of brain functioning, no matter what kind of explanation a psychologist proposes, cognitive scientists can always say that connectionism can partly account for it. That is just a fact about the relation between lower and higher levels of analysis. As a substrate argument, a connectionist account can be integrated into any central process account discussed in introductory psychology.

Although common sense dualism mistakenly leads us to think that something new has been said when we identify depression as having a physiological substrate, the important issue is not knowing that depression has a substrate, it is knowing what the substrate actually is. That is new and important.

The biomedical exemplar of general paresis works well here. Any physiologically-minded psychiatrist at the turn of the century would have believed that general paresis has a physiological substrate. Discovering that there was a substrate was not important, what was important was discovering what the substrate was, i.e., syphilitic infection. In the same way, discovering the biological substrate of psychological states will be equally important. Eliminativists of course say that once we discover how the brain works, common sense psychology will be abandoned.

The fact that one particular disorder, general paresis, could be reconceptualized as existing primarily on the biological as opposed to the psychological level is not the same thing as eliminating a whole level of analysis. If most of the causal force of a particular state of mind proceeds from the bottom up, we

will be better able to reconceptualize it biologically. Bipolar I disorder (manic-depression) may be one of the best candidates for this kind of reconceptualization. But when there are import sociocultural and psychological level forces operating, such as in dissociation, rationalization, or a temper tantrum, the psychological level of analysis will remain a parallel level of analysis.

Furthermore, the faults of any psychological explanation will be evident enough at the psychological level of analysis given time. As a matter of fact, the most relevant and direct critiques of psychological explanations come from psychology. Although the *co-evolution* of levels of analysis is important, psychological theorists should still be able to offer the most sophisticated critiques of psychological theories. For example, Harry Harlow's studies showing that infant monkeys attach to terry cloth mothers rather than to hard-wire mothers with bottles falsified Freud's idea about the primacy of orality as well and any theory could.

Levels of analysis and explanatory pluralism

Endorsing what he calls "explanatory pluralism," McCauley (1996) suggests that different levels of analysis make separate explanatory contributions, with each level having its own internally consistent legitimacy. Part of this legitimacy involves a unique research tradition, with research techniques, and specific kinds of professional problems to solve.

Explanatory pluralism echoes Collingwood's (1940) description of multiple levels of analysis with respect to causality:

For example, a car skids while cornering at a certain point, strikes the kerb, and turns turtle. From the car driver's point of view the cause of the accident was cornering too fast and the lesson is that one must drive more carefully. From the county surveyor's point of view the cause was a defect in the surface or camber of the road, and the lesson is that greater care must be taken to make roads skid-proof. From the motor manufacturer's point of view the cause was defective design in the car, and the lesson is that once must place the center of gravity lower (p. 304).

For a psychological example, here are some possible explanations of Joe's being depressed:

- a. His mother died and he is experiencing extreme unremitting grief.
- b. He has lowered levels of serotonin and norepinephrine in his brain.
- c. He has a biological predisposition to become depressed; it happens to many people in his family.

- d. He was emotionally dependent on his mother and fears he can't live without her support.
- e. He was secretly resentful of her, feels guilty about it, and is depressed as a result.
- f. He gave up an exciting job to stay with his mother, and now that she is gone, he feels the emptiness of his life.

Validating each of these explanations requires different methods, different kinds of evidence, and entails different explanatory problems. The point of explanatory pluralism is that each of these explanations can be valid at the same time.

In this respect, Edward Shorter's (1997) *A History of Psychiatry*, although impeccably researched, fails as a potential definitive work. Not only does his self-proclaimed preference for biological psychiatry lead him to ignore important developments in both psychoanalysis and psychotherapy, his failure to appreciate the complexities of multiple levels of analysis leads him to say some unfortunate things. In addition to Churchland-like pejorative "descriptions" of psychoanalysis, he makes some bold claims, such as:

For if the neurons of the brain itself were making people ill, the theoretical structure of psychoanalysis flew out the window" (p. 222)

The neuroscience version is usually called biological psychiatry; the social-stress version makes great virtue of the "biopsychosocial" model of illness. Yet even though psychiatrists may share both perspectives, when it comes to treating individual patients, the perspectives themselves are really polar opposites, in that both cannot be true at the same time. (p. 26)

Anyone who claims that the psychoanalytic theoretical structure is anti-biological is simply uninformed. The individual for Freud was always a biological organism and "constitutional factors" were often considered to be important aspects of etiology. Furthermore, the claim that biological and psychosocial explanations cannot not be true at the same time applies only if the brain is a closed system, and if psychiatric illnesses represent genetic time bombs that go off in people's heads so that physiology explains 100% of the variance.

In response to the "levels of analysis" analysis, Paul Churchland (1996a) claims that it is perfectly coherent to believe that psychology and neuroscience could come to be seen as the same level of analysis. He backs this up by noting that in the 16th century, terrestrial mechanics and celestial mechanics in physics were considered to be two different levels of analysis, with different

rules appropriate to each level, only to be integrated into the same level of analysis by Newton. In a similar way, he claims that it is possible that psychology will become the “Neuroscience of Large and Intricate Brains” (p.224), thereby denying that the psychological and neuroscientific levels of analysis are distinct. For Churchland, anyone who claims that psychology is a distinct level of analysis has already decided the question in favor of psychology, a prime example of question-begging.

What is a molar account?

Paul Churchland (1996a) has recognized that his early formulation of eliminativism was mistaken and now claims to be giving a molar account of the brain, as opposed to just an account of the substrate. By molar, he means an activation pattern across a neural network as opposed to micro processes such as action potentials and long term potentiation. Rather than being a reduction to a lower level of analysis, connectionist (or parallel distributed processing/PDP) models supposedly provide an alternative upper level account of cognitive processes. With solid Paul Churchland rhetoric, he states that those who claim that he is merely giving an account of the substrate are engaging “in one of the great head-in-the-sand episodes of 20th century science” (1996a, p.225).

Ramsey, Stich, and Garon (1991) state that some connectionists think they are offering an account of the substrate, and others think they are offering an alternative theory at the psychological level. Philosophical eliminativism clearly began as the substrate kind of model. The whole idea behind eliminative materialism is that our models of self-understanding should be based on an accurate view of what brains are really doing. As Paul Churchland (1984) put it:

The basic idea is that cognitive activities are ultimately just activities of the nervous system; and if one wants to understand the activities of the nervous system, then the best way to gain that understanding is to examine the nervous system itself. (p. 96).

Rumelhart and McClelland’s (1986) initial excitement about parallel distributed processing was based on the idea of building a computer that works like a brain. The Churchlands themselves consistently redefine psychological problems as problems about the brain, making them the biggest question beggars in the business. According to Patricia Churchland (1986), the very fact that folk

psychology cannot be reduced to neuroscience is enough to suggest that it is radically misconceived and possibly dead wrong. Paul Churchland's (1992) basic reason for rejecting propositional attitude psychology is because it is not a very good model of how the brain is organized.

Whether or not a connectionist account can be considered a psychological level theory, Churchland's idea of a molar account is still not what psychologists and ethologists mean by a molar account. For Churchland, molar account means a molar account of brain activity, which he reasonably assumes will be an alternative theory of cognition. For psychologists, however, a molar account refers to an evolutionary and ecological account of the organism.

Asserting a molar as opposed to a molecular focus was exactly what led another eliminativist, B. F. Skinner (1938, 1989) to claim that radical behaviorism was independent from physiology. Skinner thought that what happens inside the skin of an organism does not explain what the organism does in the space around it.

If the nervous system (or, better, the whole organism) is the product of the evolution of the species and of what has happened to the individual during its lifetime, and if what the organism does is a product of current processes in the nervous system (or, better, the whole organism), then what the organism does is the product of natural selection and of what has happened to the individual, and that is what ethology and the experimental analysis of behavior are all about (1989, p. 130).

As we saw in Chapter 5, a concern with molar processes has traditionally led ethologists to reject biological reductionism. Kennedy (1992) expresses this view by making a distinction between two kinds of causes of behavior, proximate (physiological) and evolutionary (functional) causes. Lorenz (1950) discusses a similar issue.

If particular behaviors, such as (a) *scanning the environment for predators* or (b) *running away* confer adaptive value, then natural selection favors the survival of that behavior in a species' behavioral repertoire. Elks who can run fast will survive. Those who survive pass their physiological make-up onto the next generation. This is an evolutionary cause. Natural selection also supports the machinery that produces behavior, e.g., the physiology of arousal, scanning, and running in response to certain triggers. Searching for predators and escaping are the functions of the behavior whereas the physiological mechanisms are the concrete causes of the behavior.

In this model, feeding is caused by changes in internal physical variables, such as activity in the lateral and ventralmedial areas of hypothalamus in response to blood glucose levels. Feeding is not caused by being hungry (unless you are an identity theorist!). At the same time, if we want to understand the pattern of the behavior or its purpose, physiology is only part of the answer. To understand molar behavior, we also have to refer to history and ask why that behavior survived as part of the species' or the individual's repertoire. These evolutionary-historical factors are also causes of behavior. At a certain point, social conditions probably exert selection pressure on a species repertoire as well (Dennett 1991). Physicalistic realists such as Churchland therefore have an extremely narrow view of what counts as a molar level analysis.

To the extent that we want to give a purely evolutionary analysis of the functional causes of behavior, we can be Skinnerian behaviorists. If we want to talk about functional causes but suggest that they also emanate from within the organism, we are committed to a model that is by definition broader than radical behaviorism. The functional (searching, hunting, escaping) is not the physiological, but when talking about it as emanating from within the head, it is more than the peripheral. Some call it the intentional, which is part of what thinkers such as Tinbergen (1951/1967) and Lorenz (1950) refer to as psychology (the subjective is the other part).

Modern day eliminativists think that higher level neuroscience can move into the level of analysis now occupied by psychology, but still be called neuroscience. If I am correct, once neuroscience gets to the psychological level, new and complex problems endemic to that level will emerge. These include perennial problems indigenous to psychology that no comprehensive model at that level of analysis can escape.

Psychology as an integrative framework

To summarize the main point of the first anchor, one of the most persistent problems faced by those who develop psychological explanations of behavior is how to weigh the importance of internal causes versus external causes. On one end of the continuum would be the neuroscientists who explain behavior primarily with respect to how it is made possible by what happens in the brain. On the other side of the continuum would be the behaviorists who prefer to explain behavior with respect to the reinforcement value of environmental variables.

A compromise position that attempts to focus on *both* internal and external is psychology. Like the brain, psychology is in the head (or the body), but it is also about the external world. Presumably, our behavior is going to be the result of complex interactions between brain and society, and in order to understand that interaction we need a common standard. *Psychology is that common standard.* As a theoretical framework, it allows us to talk about what goes on in the head as a reaction to both the social world and our own internal processes. This is part of Freud's still useful idea of personality as a compromise formation between the individual and civilization, between brain and society, or between internal and external worlds.

3. Psychology Is Explanation With Reference to What Is in The Head

In seeking their explanations, counseling and clinical psychologists are typically concerned with finding out what internal factors about the person contribute to the behavior in question. They ask: "What about this person can help us understand his or her behavior?" Understanding socio-cultural influences and environmental contexts is crucial for comprehending behavior and being empathic, but the *sine qua non* of the psychological approach is asking how the behavior is influenced by personal factors. I refer to this as explanation with reference to what is in the head.

An 18-year-old woman enters therapy because she is extremely anxious about being pregnant. A novice counselor would probably think that her being pregnant makes her anxiety understandable, but more experienced counselors would not so quickly make such a common sense assumption. They would know that not everyone is upset about being pregnant and ask "why is this particular woman upset?" Exploration might reveal that what really upsets her is having to tell her father about being pregnant. A few counselors may be satisfied with this explanation, but not all. Not everyone would be anxious enough about their father's reaction to enter therapy. Asking "why is this woman so anxious about what her father will think?" and learning that she has always feared disappointing her father or finding out that this is the third time in the past 12 months that she has been pregnant is getting much closer to understanding the personal factors contributing to her reaction, and much closer to a psychological explanation.

As noted in the previous section, ethologists make distinctions between

physiological and evolutionary causes of behavior. If I run away from a lion to escape, the function of escaping is best understood in terms of evolutionary variables, which are environmental and historical. It is still *me*, however, who ran away. The source of the escaping is also *me*, doubly so if I am self-conscious enough to know what the function of the behavior is, e.g., being in an escape situation, knowing that I need to escape, and knowing that I am going to escape before I actually flee. The biological and situational and historical and cultural and evolutionary are all important kinds of causal analyses, but they get put together on the inside. To borrow and modify a Skinnerian point, the person is the locus of interaction for the relevant casual variables.

Going back to a previous example, “Why is 40-year old Joe depressed?”

- a. He has lowered levels of serotonin in his brain (physiological).
- b. His mother died (situational).
- c. His father died when he was 10 and he is reliving that trauma, plus he is now alone (historical.)
- d. Joe’s full name is Jotaro and he is Japanese. Family is important to him and he feels he failed his ancestors by not having a family of his own. The family line may die (cultural).
- e. Joe’s need for others has survival value for the species. He is more likely to procreate than some schizoid organism who has no interpersonal needs and does not react to interpersonal loss (evolutionary).

How these influences interact is what psychology is all about. Psychology does its integrative job by looking at how everything gets put together on the inside. For example, no external historical event or cultural fact ever caused anything without eventually causing it from the inside. That they only exist for Joe as represented in him is an old Albert Ellis (1962) point: events do not cause emotional consequences, our interpretations of events cause emotional consequences. So psychology is inherently a representational framework, be it the schemes and prototypes of cognitive psychology or the self-and-object representations of psychodynamic psychology.

The idea of an inner world is therefore central to psychology. There are several overlapping descriptors for this fundamental dimension of psychology, such as representation, imagination, fantasy, imagery, scheme, perspective, etc. Each descriptor has a slightly different connotation. The most important aspect of the anchors framework is the general concept of the internal world as somehow different but related to the external world — and the assumption that what is inside actively influences our understanding of

what is outside. As a matter of fact, developmentally, internalization in the form of an ability to pretend or imagine precedes the development of psychological mindedness.

Although I follow counseling and clinical psychologists in using the term representation to refer to the content of the internal world, a good deal of internal activity probably involves no representations at all. For a psychologist, the “representational world” or “internal world” refers to all the processes (cognitive and emotional) that contribute to the formation of internal models of external objects, not just to “representations” themselves.

Head versus world debates in clinical psychology

One of the most interesting implications of psychology as explanation with reference to what is in the head is that psychology can be seen as evolving from brain-based explanations. For example, Descartes’ *cogito* is an epistemological version of the tendency to prioritize what goes on in the head. It is correlated with the strong beliefs about the brain that we saw Descartes express with respect to insanity in Chapter 4.

In modern times, as soon as a neuroscientific thinker, influenced by evolutionary theory, begins to conceptualize the brain as an open system, psychological problems emerge. The most famous example of a tradition fitting this model is psychoanalysis. Similar to Paul Churchland, Freud wanted to create a psychology that was consistent with how the brain worked. In his 1895 *Project for a Scientific Psychology*, he attempted to write a psychology completely isomorphic with the (19th century) physiology of the nervous system.

The tendency of psychoanalytic thinkers to base their explanations of behavior on psychological processes occurring in the head began with Freud’s commitment to the biological end of the continuum. We see this commitment in Freud’s focus on the importance of “instincts” and his idea that drives create *objects*. The instincts themselves are no more experiential than are the firings of individual neurons — they are biological factors.

For Freud, the meaning any *external object* has for us is a function of how we invest it with emotional energy. In psychoanalysis anything invested with positive or negative emotional energy is called an object. The strongest emotional energy we have is essentially biological in nature, belonging to what Freud called “constitutional factors.” What money, family, or career means to

you depends on how you invest those things with emotional energy. For example, anyone living in a free market economy knows that people differ with respect to how much emotional energy they invest in the pursuit of money. Those who are not concerned about making as much money as possible have invested their emotional energy elsewhere. Freud believed that all emotional investment could be traced back to primary drives — to the body.

The young Freud differed from Churchland in that he had to deal with the practical problems of adaptation. As his neurology practice moved from focusing on hysteria to the treatments of depression, which has an interpersonal focus, and psychosis, which is defined with respect to loss of contact with reality, the quickly maturing Freud adopted a greater concern for the role external objects played in psychic life. For Freud, fantasies, the first psychological manifestation of the instincts, do not always get us what we want. Successful adaptation requires taking reality considerations into account. External objects are important because we have to adapt to them. For example, students cannot just fantasize about getting a degree. They have to apply to a school, go to class, and study for tests. In any area of psychology, evolutionary theories and their focus on adaptation always bring us out of the head and into the world.

Eagle (1984) notes that Freud never strayed from grounding his theory in biology and saw the move out of the head and into the world as something we reluctantly do in order to obtain satisfaction. As a committed biologist, Freud wanted to keep the focus in the head, but as a Darwinian, he couldn't.

The first major psychodynamic excursions out of the head into the world were called ego psychology and object relations theory. A more recent and radical excursion into the world is called the interpersonal/relational perspective. As a matter of fact, one useful way to conceptualize the evolution of psychoanalytic throughout the 20th century is to view it as an ongoing argument about the importance of head versus world in psychological explanations.

The early object relations theorists such as Fairbairn (1952) saw the child as motivated to seek relationships with others, rather than motivated to seek tension reduction. He thought that instead of being secondary, the need to form relationships with others is primary. For object relations thinkers, what we find in the head are not just biologically-based drives, but internalized representations of relationships. In Kernberg's (1975) model, representations of the self, of the other, and of the emotional connection between them serve as our model for interpreting the world.

Object relations theorists also consider adult relationship patterns to be manifestations of what is in the head, but what is in the head is always, in part, an internalization of something external. For example, the lack of trust that an incest victim may have with respect to intimate relationships follows from the internalized incest relationship in their head. They cannot separate the *plot* of the incest relationship (I am being violated) from current life events (I am having sex with my partner). Object relations therapists try to help them experience feelings based on the current external relationship rather than continuing to react primarily to the internal relationships that are in the head.

The interpersonal/relational school (Sullivan 1953; Greenberg and Mitchell 1983) takes this process one step further. Following William James, they hold (a) that behavior is adaptive, and (b) that ideas have to be tested by being put into action. You only know what is in the head by studying actions. Based on a concern for staying close to observable data and not getting sidetracked in metapsychological speculation, interpersonalists never stray from actual relationships and their adaptive significance. As an American style pragmatist, Harry Stack Sullivan held that in addition to manifestations of a biological disease, the symptoms of schizophrenia also can be understood as attempts at adaptation.

For Sullivan and his followers, the interpersonal context is the setting in which all behavior occurs, and the setting can never be ignored. Not only are personality and self manifested in relationships, they are constructed by them and interpretively inseparable from them. The originators of the relational perspective, Greenberg and Mitchell (1983), state that Sullivan rejected the strategy of focusing on what is “in the head.” For interpersonalists, when it comes to knowing others, we have three sources of data.

- a. Observing what they do.
- b. Observing ourselves in interaction with them.
- c. Listening to their own reports of interactions and experiences.

Following George Herbert Mead (1934), rather than focusing on the meaning of objects as a function of idiosyncratic emotional investment (drive cathexis), interpersonalists focus on the meaning and significance we attach to ourselves as discovered in relationships with others. This claim, that we discover ourselves in an attempt to relate to others, makes perfect sense. Think of Henry Higgins in the musical version of Shaw’s play talking about how, when he enters a relationship with a woman, he finds a self who he does not like, rude,

demanding, etc., so for peace of mind, he avoids female companionship.

In contrast to the classical psychoanalysts, the interpersonal/relational school does not emphasize the causal path from “body” to behavior, rather it emphasizes the always embedded self as a manifestation of interactions. Internal representations are so thoroughly constituted by external relationships that they cannot be meaningfully isolated from them.

According to Mitchell (1995), classical analysts who focus on the drives see the body as a foundation upon which social relationships are built. It is there first and can be isolated as the prime mover, i.e., evolution proceeds from within. In other words, drive theorists want to ground conflicts in variables arising from biology (pleasure and pain). Relational thinkers critique Freud’s almost solipsistic insistence that the ultimate root of motivation comes from the body — that external objects are just containers for drives. Those with relational leanings want to ground conflict in internalized relational configurations (good mother, bad mother), claiming that the content of motivation comes from interactions between the self and others.

Although no one in the so-called relational school has given up a focus on what happens in the head, holding that relationships and what is outside are more than a secondary derivative of what is inside has led to the counter-critique that relational thinkers are antipsychological peripheralists, confusing what is essential with what is a mere elaboration (Bachant, Lynch & Richards 1995; Murray 1995). These debates in psychoanalysis are debates about where on the continuum from brain to world psychology should be placed.

Head versus world in philosophical psychology

The debate between the more traditional Freudians and the relational school parallels a debate in continental philosophy between the defenders of the isolated Cartesian subject and the Heidegger-inspired critique of representationalism. As Charles Taylor (1989) claims, what is in the head is a result of being in the world, of acting in and interacting with the world — and it is a continual process. Focusing solely on what is in the head, what Taylor calls disengaged subjects and punctuated selves, is to focus on philosophical creations. No theory of representation can escape this problem: *we cannot form representations without being engaged in the world.*

Interestingly, the eliminativism of Churchland (1995) occupies a position somewhere near the end of the brain-world continuum that Freud took as

opposed to Skinner's peripheralist position. Bechtel (1996) notes that Churchland's connectionism emphasizes what goes on in the head to the exclusion of external considerations, including social relations. Bechtel thinks this is unfortunate because these external factors also have to be taken into account in order to understand ourselves. Using language as an example, Bechtel persuasively argues that not all representations are in the head. We also interact with so-called "external representations."

For example, written language is an external representation system. It is a product of a cooperative effort between individuals. It has adaptive value because what is written down can endure rather than degrade in the way that biologically-based "memories" degrade. Language also extends our cognitive capacities, seen in the fact that a well thought out book can integrate more information at the same time and in greater detail than a brain can — that is one reason why it takes books so long to write! Bechtel's point is that one cannot have an adequate philosophy of science without taking external representations into consideration, but this could easily be extended to moral philosophy, aesthetics, and philosophical psychology. It is also a version of a more molar account of human behavior.

Bechtel's arguments have led to a significant admission on Churchland's part, kicking and screaming though he may be. Churchland (1995) claims that language is a form of "extrasomatic memory," a kind of information storage that exists outside any individual brain (p.270). Brains are embodied and bodies are part of an external physical and social ecosystem. As a result, one cannot just focus on the organism. Behavior takes place in a social context, and social facts are also facts. Churchland admits that in addition to living in a physical space, we live in a social space of

obligations, duties, entitlements, prohibitions, appointments, debts, affections, insults, allies, contracts, enemies, infatuations, compromises, mutual love, legitimate expectations and collective ideals (1995, p.123)

At the same time, Churchland (1996B) holds that, although external reality considerations are important, what goes on in the head is more important. Even if language extends our theoretical capacities, it does not constitute them. For everyone, those external systems exist only as represented in our heads anyway, so it always comes back to the body, to *constitutional factors*. Both Churchland with respect to the brain and the classic Freudians with respect to instincts consider these internal variables to be unconstructed reality. Commenting on the classical Freudians, Merton Gill (1995) summa-

rized their view as being that “the innate is explanatorily superordinate to the experiential (sometimes reductively so)” (p. 92)

Like the defenders of classical Freudian instinct theory, Paul Churchland takes what is in the head to be the primary account and social relations to be the secondary account. Important, but still an elaboration. His ultimate locus of explanation is in the skin, with the brain being the final common pathway of all that is human. Similar to Bechtel, John Dewey was critical of the in-the-skin focus of another prominent eliminativist, John B. Watson. He refused to accept what he called Watson’s “subcutaneous” approach to behavior, meaning Watson’s narrow focus on what goes on under the skin, dismissing the importance of interactions with the world. According to Dewey, explaining *behavior* requires information about the external world as well as information about what is going on in the head.

I also think that “in the head” is loosely an issue for Skinner. One of the unfortunate consequences of pedagogical simplification of theories in introduction to psychology textbooks is that they are rarely corrected later in a student’s education. In introductory courses, Skinner is described as the person who believed in operant conditioning. He thought all behavior was learned, that learning was a matter of reinforcement, and that multiple schedules of reinforcement are available for shaping behavior.

Rather than describing Skinner’s approach to explanation, the textbooks describe the facts he discovered. In contrast, his explanatory approach is called the functional analysis of behavior. Skinner believes that behavior and events are highly correlated. If we know the correlations between behavior and types of events, given control of events, we can control behaviors. The goal of his research program is to estimate the correlations. The problem is that the correlations vary, so one event, say giving a boy chocolate after he cleans his room, does not always increase the probability of the target behavior in the same way. Other events also influence the behavior, and chocolate does not have the same relevance for all boys. With respect to the concept of reinforcement value, Skinner had to introduce the idea of individual differences. Although he defined individual differences externally in terms of “history of reinforcement” this history is still loosely contained in the organism and therefore in the head or the body.

Contra peripheralists such as Skinner or radical constructivists such as Kenneth Gergen, my sympathies lie with Paul Churchland with respect to keeping a focus on internal variables and the individual, but the problem is

viciously circular. Contra Churchland, I take the implications of both natural selection and Jamesian explanatory pluralism seriously, where trying to understand what is in the head always brings us back to the external world. Churchland thinks he avoids the chicken and the egg problem by saying that brains were here first and there would not be social and cultural facts without brains, but this is a corollary to his water thin molarism. Since our brains are already in the middle of this social and cultural world, and since behavior is an attempt to adapt to that world, we cannot partial the world out as easily as Churchland thinks. At the same time, even if individuals are constructed by their social relationships, the brain is still the final common pathway where competing social forces are integrated. The purest kind of psychology would be one that stands squarely in between the brain and the world, without systematically favoring one end of the continuum over the other.

We cannot take “in the head” to literally be the brain, because the brain is an organ of reaction to the world and the self. The world is not a mere elaboration. Dennett (1991) even holds that the brain cannot store all the information that we get by walking around a room or exploring a statue from many angles. The world is actively in our head, going beyond what a brain can invent from moment to moment.

Looking at the head-versus-world continuum as a whole, Skinner’s peripheralism is the most coherent eliminativism. The biology of survival is his ontological fundamental. For example, if we claim that we eat *because* we feel hungry, Skinner will retort that we feel hungry *because* that condition has survival value, so we lose no accuracy by eliminating hunger and just saying that we eat *because* it has survival value. His only qualification is that short term contingencies of reinforcement need to supplement more stable contingencies of survival. As long as Skinner can establish a stable correlation between events and behaviors, he can account for adaptation without psychology. Churchland’s focus is bought at the cost of minimizing the role of natural selection. If Darwin is to be respected, any in-the-head focus has to consider internal-external relations, and hence become psychology. With a widened focus in terms of a truly molar view of organisms, what mental materialism would become is no more similar to Paul Churchland’s anti-anthropomorphism than object relations theory is to Freud’s pre-psychoanalytic *Project for a Scientific Psychology*.

4. Psychology Is Understanding With Reference to The Self

In this section I claim that the framework of psychology also includes considering the self. I specifically discuss the importance of making a distinction between *self* and *other*, and also discuss how that distinction becomes more cognitively complex over time. Making a distinction between self and other (and the related distinction between internal and external) is part of our evolutionary endowment, and the belief-desire framework evolves out of this endowment. According to the evolutionary endowment model, thinking about ourselves and others with respect to beliefs and desires is biological in the way that language is biological.

The concept of self as fundamental

According to Feigl (1971), the framework of psychology contains three fundamental dimensions: *sentience*, *sapience*, and *selfhood*. Of these three dimensions, selfhood is the most fundamental. It is so fundamental that psychological conceptualization would probably be impossible without some concept of self. The concept of self does not have to be an essential self or a true self or a unified self; many contemporary theorists don't believe in such an entity. But the concept of self is such an important aspect of the framework of psychology, that anyone who uses this concept has an implicit psychological theory.

As soon as anyone grasps the distinction between themselves and another, they are thinking psychologically — and *everyone* makes this distinction — even eliminativists. As a matter of fact, one of the Churchlands' most persuasive justifications for eliminative materialism is its potential contribution to our self-understanding. For example, Patricia Churchland (1986) claims that although the excitement about neuroscience relates to the fact that it is a *science*, just as important is the fact that its discoveries have to do with *ourselves*. With neuroscience we are “discovering what we are and how to make sense of ourselves” (p. 10). Paul Churchland (1995) opens *The Engine of Reason, the Seat of the Soul* with: “This book is about you and me” (p.3). At the end of the book, after an involved exploration of connectionist accounts of cognition, he states that he is trying to develop a more accurate theory regarding “the nature and ground of the human self” (p.305).

The genesis of self-other distinctions parallels the development of psychological mindedness.

As an anchor of psychology, the concept of self begins developmentally in the context of an organism's biologically-programmed ability to make self-object distinctions. Rudimentary self-object distinctions occur in all species and clearly in primates. Dennett (1991) claims that rudimentary self-other distinctions are necessary for any organism designed to preserve itself.

The self-other distinction appears very early in development, existing at first in implicit form. Meltzoff and Moore (1983) have shown that by the time they are 12-days-old, human infants will imitate the simple behavioral act of another person, such as sticking out their tongue and turning it to the left. If this is only a reflex, it is a complex one because infants will attempt, in the absence of the original stimulus, to imitate the action if they are delayed from trying it at the time the stimulus is present. They will also practice this action until they get it right. This suggests (a) the presence of an implicit body schema, (b) an implicit orientation toward external objects, and (c) primitive self-consistency.

In a review of what infants know about the world of objects, Mehler and Dupoux (1994) show that a few months after they are born, infants are able to recognize visual and auditory stimuli. This recognition includes spatially locating an auditory stimulus (Clarkson, Clifton & Morrongiello 1985), combining the input from several sense organs and attributing them to the same object (Spelke 1976, 1981; Spelke and Owsley 1979), and understanding the three-dimensional nature of the visual world (Held, Birch & Gwiazada 1980). Some of these abilities first appear in a reflex manner, disappear, and reappear later under conscious control (by the age of four months).

There is also evidence that infants' innate recognition mechanisms are preferentially oriented toward other human beings and social interactions. For example, Field, Cohen, Garcia, and Greenberg (1984) show that within a week of their birth, babies are able to (implicitly) recognize their mothers' faces, even though they cannot (explicitly) recognize faces in general until about two months. DeCaasper and Fiefer (1980) demonstrate that infants prefer human speech over other sounds, prefer female speech to male speech, and prefer their own mother's voices to all others.

In addition to recognizing objects, including human objects, infants begin to learn about themselves. Child psychiatrists such as Winnicott (1965) claim

that self-understanding probably begins with an infant's growing knowledge of the internal-external distinction. According to Winnicott, one of the most important tasks of infancy is learning to accurately distinguish sensations coming from inside the body from sensations coming from outside the body — for example knowing that the sensation of hunger originates inside the body while the sound of a voice originates outside the body.

According to Mahler, Pine, and Bergman (1975) visible proof of the internal-external distinction is seen when the five-month-old baby, in its mother's arms, pulls away to get a better look at her. Infants also begin to prefer the primary caretaker to other persons in their life. Mahler calls this the *differentiation* subphase. The child does not just differentiate itself from the mother, it differentiates the mother from others in terms of emotional preference. Highlighting this aspect of development, Bowlby (1969) called it attachment. Obviously, differentiation and attachment are interrelated processes. Attaching to the mother requires being able to differentiate her from self and others.

Based on research with non-verbal infants, another visible milestone in understanding the internal-external distinction is the ability to discriminate the self-propelled movement of organisms (animistic movement) from both the externally propelled movement of objects and random movement (Poulin-Dubois & Shultz 1988; Golinkoff 1983). Premack (1990) states that infants have basic animistic concepts before the age of two. In a related discussion, Johnson (1988) suggests that the intuitive ability to discriminate between being an actor (turning the head) and being acted upon (being picked up) marks the beginning of the experience of being an "I." Piaget (1954) held a similar view.

Dixon (1957) showed that at 4 months, an infant recognizes its mother's reflection in the mirror, but displays no interest in its own reflection. By 6 months an infant reacts to its own reflection as if it is another infant. Lewis and Brooks-Gunn (1979) show that if the self-image is a live video that moves as the child moves, basic self-detection begins as early as 9 months. In addition to understanding that the reflection is contingent upon their own movement (i.e., coordinating internal and external evidence), by 15 months, children know what their faces look like and can recognize them in pretaped videos and still photographs. According to Meltzoff (1990), by the age of 14 months children know when their own actions are being imitated by an adult — which also requires some ability to coordinate internal and external evidence.

The most famous studies of self-recognition are Amsterdam's (1972) rouge experiments which showed that at 20 months, infants develop conscious self-recognition, measured by the act of touching their own nose in response to a red spot on the nose of their reflection in a mirror. Interestingly, the mirror self-recognition paradigm was first used by Gordon Gallup (1970) in studies with chimpanzees. Comparative psychologists have demonstrated that explicit self-recognition occurs in orangutans as well. (Moses 1994). As a matter of fact, some of the creative research designs that developmental psychologists use to study pre-verbal children were originally developed to study animal cognition.

In addition to self-recognition, comparative psychologists have demonstrated that chimpanzees develop relatively sophisticated concept about others — especially concepts about others as subjects. The internal-external distinction helps them make a distinction between self other, but it can also be turned around. Turning it around involves seeing others as having an internal world as well. For example, Povinelli (1993) shows that chimpanzees can develop a model of what another subject sees, and make practically relevant predictions about what that subject knows, whereas rhesus macaque monkeys cannot. Povinelli shows that chimps know that a person who sees a piece of food being hidden under one of three cups knows where the food is, and a person who did not see the food being hidden does now know where the food is. They consistently rely on the help of the person who saw the food being hidden to find the food. Because this research is conducted in the tradition of Darwin's attempt to understand the evolution of mind, similar questions about the how human beings develop an understanding of another's subjectivity (or consciousness) is called *theory of mind* research.

A better label, however, for what developmentalists are studying in the theory of mind paradigm is *psychological mindedness*. Applebaum (1973) defines psychological mindedness as "A person's ability to see relationships among thoughts, feelings and actions, with the goal of learning the meanings and causes of his experiences and behavior" (p. 26). Except for leaving out the importance of understanding others, and the role of external events, Applebaum's general definition is a good one. The development of psychological mindedness is a process of learning to make more complex discriminations with reference to the internal states self and others.

Traditionally, developmental psychologists payed scant attention to the psychological mindedness of infants and young children because Piaget's

concept of egocentrism suggested that young children lack the ability to see the world independent of their own perspective. Piaget is generally considered to have underestimated young children's abilities. Developmental psychologists now believe that preverbal infants and young children, to turn Nisbett and Wilson (1977) on their heads, *know more than they can tell*.

For example, Meltzoff (1995) believes that body imitation is the root of psychological mindedness. It clearly involves understanding the similarity between an implicit "I" and an implicit "you." He thinks psychological mindedness begins with an innate ability to imitate simple actions, which develops into a non-inferential understanding of behavioral acts as intentions — as internally propelled attempts to bring about particular states of affairs.

According to Meltzoff's research, by the age of 18 months children already situate people within a psychological framework involving intentions. Meltzoff shows that, at 18 months, children will see a behavior as an attempt to bring about a particular state of affairs. By this age, children can complete an incompleting behavior of an adult, indicating that they saw the behavior in terms of what the adult intended. For example, an 18-month-old child can observe an adult attempt to drape a nylon hoop over a prong stuck to a vertical board, see the adult's attempt interrupted, and then successfully complete the behavior themselves. Philosophers have traditionally referred to these types of behaviors as "actions." At 18 months, however, although infants see *behaviors* as intentional, they do not yet see *intentions* as cognitively formulated goals in a person's head.

Repacholi and Gopnik (1997) show that 18-month-old children can also implicitly infer an emotion from someone's facial expression (e.g., disgust) and make predictions about that person's preferences even if the person's desires differ from what the child him or herself wants. They can understand that an adult may want broccoli instead of a cookie, even if they personally would prefer the cookie. Repacholi and Gopnik report that this finding is congruent with the fact that two-year-olds will examine their mothers' expressions in ambiguous situations, implying that children read facial expressions to gather information about subjective reactions and behavior. Social referencing studies like those of Hornick, Risenhoover, and Gunnar (1987) show that even year-old infants will use their understanding of their mother's facial expressions to inform their own preferences for objects such as toys. They actually read their mother's expressions to learn how they should react — to know what they should feel. (The importance of emotions for psycho-

logical mindedness is explored more fully in Chapter 7).

Leslie (1988) shows that between 18 and 42 months, children develop the ability to pretend, which he considers to be a forerunner of psychological mindedness. For example, children can pretend that a banana is a telephone. Pretending depends on an ability to have multiple models in the head for a single object in the world. One model is for fun (telephone) and the other is real (banana). Leslie classifies this type of pretending as *object substitution*. Sometimes the child pretends that objects have properties that they lack, and sometimes children pretend that an object is present when it is not, such as pretending to talk on the phone when there is nothing there but the child's empty hand. Leslie thinks that pretending is the beginning of a theory of mind because the child is able to suspend the normal reference relation between representations and objects, and just report on the representations *in their head*, i.e., to automatically understand cognitively complex internal-external distinctions.

Wellman (1988) holds that once they can represent the physical as a thing *out there*, infants also have an ability to distinguish between the mental and the physical. For example, they know that thoughts, memories, and dreams about physical objects are different from actual physical objects. They understand that physical objects are real and representations of those objects are just in the mind or brain. Between the ages of two to four they also begin to understand behavior in terms of *wanting, hoping, wishing, knowing, remembering, and thinking*. They learn to talk about wanting before learning to talk about thinking.

About this time, children also learn that other people are subjects who have feelings and an epistemological perspective of their own. Flavell (1988) and Wellman and Gellman (1992) state that by the age of two-and-a-half, children understand the connection between seeing and knowing. For example, they will point to an object in an attempt to get a parent to attend to it, and be concerned that the parent see the object with their own eyes. These children also know that they can "see" their own mental images, but that others cannot. They are what Dennett (1978) would call second-order intentional systems.

As noted earlier in reference to Povinelli's work with chimps, aspects of this developmental process are not limited to humans. Although for humans, psychology-talk is something we are taught, research with non-linguistic animals suggests that a fundamental psychological mindedness is part of our natural endowment and not just culturally transmitted. Premack and Woodruff (1978) refer to it as part of the structure of experience.

Developmental psychologists have lively debates about when a theory of mind actually emerges. Wellman (1988) believes that children possess a fundamental theory of mind by the age of three, whereas Johnson (1988) believes that what three-year-old children possesses is an intuitive psychological know-how rather than a theory in the literal sense (meaning something that has been deduced and inferred). These debates are motivated by disagreements about what counts as a theory, disagreements about how to best distinguish between perception, representation, and metarepresentation, and even disagreements about the relationship between *knowing that* and *knowing how* (or explicit and implicit knowledge). Personally, I think the distinctions are fuzzy enough that perception and representation or explicit and implicit are not absolutely distinguishable.

Although a child's understanding of another's actions can also be explained on behavioral grounds, it is likely that as children's knowledge of others grows, both behavioral mechanisms and psychological mindedness operate. During the two- to four-year-old stage of development when psychological mindedness is maturing, anti-anthropomorphic explanations may be consistent with the data but inconsistent with a molar conceptualization of what is happening to the child. When implicit psychological knowledge is at the point of becoming explicit, it is going to be a judgement call as to what the child actually knows. Individual differences should also be at their greatest during transitional ages. In a related discussion, Dennett (1987) notes that any single instance of intentional behavior can often be reduced to a purely behavioral explanation, but it is difficult to continue reducing intelligent actions to dumb tropistic behaviors when their occurrences begin to pile up.

Whatever the case, it is clear that between the ages of two to four, children's abilities to use psychological concepts to understand their own and others' behavior is consolidated, with three probably still being the typical age for what Mahler, Pine, and Bergman (1975) called the psychological birth of the human infant. By psychological birth Mahler and her colleagues refer to (a) the development of a coherent sense of self, and (b) the acceptance of a subjective gap between themselves and others, and (c) an easily-evoked internal representation of another, toned with a positive emotional relation to the self. With respect to psychological understanding, according to Wellman and Bartsch (1988) three-year-olds understand the difference between beliefs and desires, and are aware that both beliefs and desires effect behavior. Lillard and Flavel (1990) show that by the age of three, children *prefer* to describe

behavior in psychological terms rather than in behavioral terms. (Technically children still describe people behaviorally — dispositional descriptions represent an addition to behavioral descriptions and not a replacement.)

In addition to rudimentary mentalism, psychological mindedness includes an ability to conceptualize the representation of beliefs, what Gopnik (1993) calls a representational understanding of the mind. Gopnik believes that, although the psychological understanding of young children is impressive, what they do not understand is equally impressive. For example, Perner, Leekam, and Wimmer (1987) and Wimmer and Perner (1983) show that until age four, children's psychological mindedness is limited by an inability to understand *false beliefs*. Knowing about a false belief means being able to think of another person as holding a belief about the world that does not conform to how the world really is, e.g., understanding that another person could believe that there are cookies in a cookie jar when the child knows that there are really pencils in the cookie jar.

Gopnik (1993) suggests that three-year-olds think of belief as “knowing.” For them, to have a belief about cookies being in the cookie jar is to know they are there. By the age of four, belief becomes “belief that,” in which they understand that their beliefs about the world may be wrong. For example, they may believe that there are cookies in the jar, but they know that they could be wrong. Understanding beliefs as representations about the world, and understanding the possibility of *misrepresentation*, is what allows them to understand false beliefs.

As a matter of fact, until children understand beliefs as representations of the world, they don't even have an ability to report on their own immediate past beliefs; they are trapped in what Gopnik calls an “eternal now.” If you show them a cookie jar and ask them what they believe is in the jar, they will say “cookies.” If you open up the jar and show them that there are really pencils in the jar, and then ask them what they believed was in the jar before you opened it up, three-year-olds will say “pencils.” Four-year-olds will more correctly report that they thought there were “cookies” in the jar before it was opened. Interestingly, three-year-olds do better at reporting their past intentions, pretenses, and desires, but they still make errors.

Congruent with my view that making self-other distinctions is an anchor of psychology, Gopnik and Meltzoff (1994) present convincing evidence that how we think about self and how we think about others co-evolve — one is not more primary than the other. Children progressively understand pretense,

imagination, perception, desire, sources of belief, and false beliefs with respect to self and others at the same time. They also begin learning to think about self-with-others. The area of developmental psychology that studies how children develop the competencies to think about self-with-others is called social cognition.

The ad hoc versus the propter hoc account of beliefs and desires

The traditional alternative to the maturational-evolutionary account of psychological mindedness is to explain it as originating in adults' attempts to solve particular problems. In the problem-driven view of psychology, psychological concepts have an *ad hoc* status, being inventions proposed to solve theoretical anomalies. For example, Freud (1920) proposed the *repetition-compulsion*, the need to repeat unpleasurable experiences, in order to account for the clinical fact of traumatic flashbacks where people seemed to passively relive painful experiences. Flashbacks were inconsistent with Freud's first rule of psychic life, i.e., psychology is a fantasy-driven attempt to recreate past experiences of satisfaction. By resolving the anomaly between the pleasure principle and PTSD symptoms, the repetition compulsion became the dominant principle in the psychoanalytic theory of neurosis.⁸

In *ad hoc* psychology, every time a new problem arises, we can invent a new construct to solve it, but the result is a mess because we are left with an invention (e.g., repetition compulsion) that solves a problem about a prior invention (e.g., the pleasure principle). It is hard to see how fictional constructs proposed to solve anomalies about prior fictional constructs bring us closer to an accurate theory of human behavior. The most famous examples of *ad hoc* science are Ptolemy's concepts of epicycle, retrograde motion, equants, and deferents. These fictional concepts, proposed to resolve problems in geocentric astronomy, did not help us get closer to understanding the actual organization of the solar system.

Skinner (1956) suggested that psychological constructs have an *ad hoc* nature when he claimed that beliefs, desires, and wishes are explanatory

8. Meltzoff and Moore's (1983, 1989) work on infant imitation and the extent to which infants are internally driven to repeat states of affairs even after a long delay is an interesting early precursor of Freud's more psychological notion of the repetition-compulsion.

fictions. This *ad hoc* theme also runs through Sellars's (1956) account of beliefs and pains as theoretical inventions which entered the natural world of behaviorism in an attempt to explain the actions of others with respect to private inner episodes that are immediately known but not self-evident. In Sellars's story, the neo-Rylean Jones, analagous to a scientist, comes up with a creative strategy for ascribing intelligence to both verbal and non-verbal behavior, i.e., by constructing an idea of inner speech or "thought."

In philosophical psychology, the idea that psychological constructs arose in an attempt to explain the conceptual anomalies of behavioral observation is an unstated assumption of the "theory" theory. Eliminativism obviously rests on the assumption of a problem-driven *ad hoc* psychology. As theoretical constructs, beliefs and desires can be rejected in favor of concepts that solve the relevant problems in better ways.

The idea that psychological concepts are *ad hoc* constructions used to make sense of behavior is also a prominent view in some areas of psychology, particularly among social psychologists such as Nisbett and Ross (1980) and Fiske and Taylor (1984). A preponderance of studies in social psychology suggest that attributions of internal states in an attempt to solve problems about behavior are based on observations of behavior and not on access to our own psychological states. They hold that introspection is not a form of perception and therefore not a reliable category of evidence. In this view, explanations based on introspective awareness of belief and desire are probably constructions.

In contrast, I reject the idea that the natural state of behaviorism has been contaminated by an invented psychology. The maturational-evolutionary view suggests that psychological mindedness is a consequence of an inborn capacity to make internal-external and self-other distinctions, which become more cognitively complex as the brain matures. Distinctions between Me and You lead to good me/bad me, good you/bad you, I want, you want, I believe, you believe, etc. Rather than being *ad hoc* inventions, the psychology framework is a *propter hoc* consequence of a biologically enabled, referential, meta-representational capacity.

Psychological mindedness, not behaviorism, is our natural state. As Graham (1987) points out, social cooperation, which has clear adaptive advantages, requires coordination. Coordination is more likely if creatures have the cognitive capacity to predict and understand each others' dispositions. In the service of cooperation, natural selection probably favored individuals

whose brains were subjectively coordinated with one another.

In rejecting the idea that the state of nature is a behaviorist state, I am not rejecting the basic argument of the “theory” theory and the important experimental work on the nature of belief and its relation to introspection. The maturationally progressive distinctions in the internal-external and self-other frameworks evolve into a kind of belief-desire-intentional psychology, but although the *capacity* to think psychologically is given to us, we still have to learn to be psychologically minded.

Proponents of the *ad hoc* view might retort that knowing why and how we develop psychological mindedness does not tell us whether that way of thinking about people is justified in terms of being an accurate model of human behavior. We may have evolved with a predisposition to accept a false framework. The framework may have functional significance, but our cultural metaphors for understanding the framework, such as *I feel as if you can read my mind*, may be inaccurate. Here the tried and true analogy between psychology and theology can be put to work again — just because people seem naturally inclined to believe in God does not mean that God exists.

I agree, with two qualifications. My first qualification is that although the maturation-evolutionary account does not establish the truth of the matter, it does establish that psychological mindedness is more than a mere invention. Contra Neurath, Skinner, and Churchland, it is not just made up and we can’t just teach our children something else. With respect to cultural metaphors, believing in the worthiness of psychological understanding and explanation does not mean that our current ways of talking cannot be improved upon. The possibility of increasing our self-understanding is what makes psychology so interesting.

My second qualification is that all evolution-based brain centered (in the head) theorists develop dispositional explanations — otherwise they would be radical behaviorists. Some kind of dispositional explanations must be true if the origin of species by natural selection has any scientific validity. Dispositional explanations also have Darwinian validity. For example, any mammalian species that couldn’t implicitly learn that some “others” such as lions and tigers and bears have dispositions to eat them, could not survive the trials of natural selection. For those species able to develop an explicit awareness of cognitive processes such as memory, thinking, feeling, and imagination, dispositional understanding will become more psychological, e.g. thinking that the body over there is a subject whose behavioral consistencies come

from the inside. The dispositional framework should have even increased survival value for anyone living in a socially organized world. As Dennett (1987) claims, even though the risks of occasional confabulations are great, adopting the intentional stance is usually worth the risk because the pay-off is so high.

5. Conclusion

I have argued that psychology is anchored in a series of robust explanatory strategies, and that eliminating psychology would require eliminating these strategies. It is a mistake to define psychology in terms of content such as beliefs and desires or to think that eliminating beliefs and desires is to eliminate psychology. Instead, I have argued that psychology is best defined as a level of analysis between internal and external worlds. If we did not have a psychology, we would have to invent one just to account for brain-world interactions. Psychology is also explanation with reference to what is in the head. Psychological explanation requires identifying the personal factors that are responsible for an individual's behavior. The interesting thing about explanation with reference to what is in the head is that psychology naturally evolves out of an attempt to explain behavior with respect to the brain. Finally, psychology is understanding with reference to the self. Understanding with reference to the self follows from the levels of analysis anchor, and conceptualizes psychology as an attempt to find some common standard between brain and world. The most robust aspect of this internal versus external interaction is the self-other distinction. I have reviewed research which persuasively argues that self-other distinctions are maturationally programmed into several species, and in humans they are developmentally elaborated into psychological mindedness, i.e., some kind of folk psychology.

CHAPTER 7

Materialism Without Physicalism

1. Introduction

My framework for psychological explanation and understanding would be incomplete without addressing the importance of consciousness. I specifically address consciousness as subjectivity — or “knowing-what-it-is-like.” I argue that knowing what a brain/body state is like constitutes *information* that cannot be reduced without remainder to something written down in a book. “Knowing what it is like” is a prerequisite for reasonably adequate self-knowledge and for a reasonably adequate understanding of other people. What a brain/body state is like can be written down and communicated, but the effectiveness of the communication is going to depend on the extent to which the receiver of the information has experienced the relevant state described. When looked at from within the anchors framework, specifically the internal-external dimension, subjective states constitute an important kind of information that no one seriously suggests that we should eliminate, and to the extent that subjectivity is important, psychology is important.

Because my use of physicalism and materialism, although historically justified, is not congruent with current understanding, I’ll begin by defining what I mean by these terms.

Materialism. The *ontological* view that to be real is to be material. Material can refer to matter, energy, or some pattern that matter-energy takes.

Naturalism. Naturalism is the belief that we should try to explain the world without recourse to supernatural entities such as God and angels. Scientists are supposed to be naturalists and try to understand the world on its own terms.

Physicalism. The *epistemological* view that, in theory, we can explain anything we want with reference to intersubjectively confirmable observation.

(Third person descriptions). Sometimes understood as the view that, ultimately, we can reduce all scientific explanations to the explanations of physics.

My use of physicalism follows its original meaning in the philosophy of science, what might be called *unity of science physicalism*—or Carnapian physicalism. The point of Carnap’s physicalism was that science has to be grounded in intersubjective facts, which linguistically becomes *third-person* description. Carnap refers to the language of science as a physicalistic thing language. For positivists such as Carnap, limiting the language of science to third-person description is advantageous because a common, publically verifiable language makes the unity of science more achievable.

Third-person descriptions grounded in intersubjective facts will of course be descriptions of physical objects. Therefore, “physicalism” is often taken as a synonym for “materialism,” the view that *everything that exists is material*. This means that there are no non-material substances — such as angels. Denying the existence of non-material substances, however, does not require one to accept that all information can be reduced to third-person descriptions.

A corollary to materialism is “naturalism,” the view that we should not refer to supernatural entities such as God, angels, and souls to explain the world. Because this definition of naturalism is similar to a definition of materialism as the claim that *everything has a physical referent*, naturalism is sometimes taken to be a synonym for materialism. Even if the difference between naturalism and materialism is subtle, it is important.

One can be a scientist and believe that the job of the scientist is to explain the world on its own terms — as a matter of principle. This describes the position of thinkers such as Isaac Newton, who believed in God, but rejected reference to divine intervention in understanding how the world works. Qua science he was a naturalist. As a matter of scientific principle, it would be *immoral* to explain the bubonic plague solely in terms of God’s will instead of seeking the material causes of the plague and preventing its re-occurrence. The same is true of AIDS, sudden-infant death syndrome, and leukemia. Adopting naturalism qua science is, however, not logically inconsistent with some kind of belief in the existence of God.

Carnap’s physicalism also included a proposal that all laws of nature are physical laws. This second type of physicalism could be labeled “causal physicalism.” Causal physicalism is the metaphysical belief that *all causes are physical or physical effects must have physical causes*. Smart (1979) calls

attention to the difference between Carnap's third-person description (epistemological) and materialistic (metaphysical) definitions of physicalism, stating that Carnap used physicalism in the second metaphysical sense as a synonym for materialism. Physicalism-as-materialism is what Paul Churchland means by physicalism. It forms a part of his justification for unity of science physicalism, which he thinks is the claim that all talk of causes can be given in third-person descriptions.

In my view, important distinctions between physicalism, materialism, and naturalism have been lost, although it is the materialism-physicalism conflation I am concerned with here. I claim that by dissociating Carnap's first definition of physicalism from his second definition of physicalism, I can dispute the completeness of third-person descriptions without having to reject the idea that all causes are physical causes. Whether or not we accept this distinction makes a difference for how we think about psychology.

2. Mary, the Blind Neuroscientist

Most debates about the limits of physicalism center around an argument given by Feigl (1958), Meehl (1967), and Jackson (1982). Jackson's version of the argument is formulated with respect to Mary, a blind utopian neuroscientist. Mary is a blind neuroscientist who is deprived of all experiences of seeing colored objects, but develops a comprehensive understanding of the neurophysiology of vision. She knows all there is to know about the neurophysiology of vision. According to Jackson, although Mary has all the physical information about color perception she could have, she still does not know what it is like to see red. Since Mary knows all that can be known from a third-person perspective, and still not know what red is like, physical description is incomplete. There is information belonging to the subjective perspective which is not captured in the physicalistic thing language. Jackson makes it clear that he is arguing against physicalism in Carnap's first sense, as third-person description, which he describes as the restricted view that the terms of science describe everything without remainder.

The philosopher Nagel (1974) has a similar argument. In Nagel's memorable argument, he states that it is logically possible that human scientists could learn all about the neuroanatomy and neurophysiology of a bat's brain, including the bat's ability to navigate through the world by sonar. Once they

had this knowledge, however, they would still not *know* what it is like to be a bat. If there is something that it is like to be a bat, neuroscience is incomplete.

Those who argue for or against Jackson and Nagel often take them to be supporting some kind of mind-body dualism. Chalmers (1996) takes Jackson's arguments as an argument for dualism. Paul Churchland (1995) also perceives the specter of dualism, stating that the conclusion to be disputed in the Mary argument is "there must be a nonphysical dimension to one's conscious experience" (p.201). Claiming that there are natural phenomena or facts or dimensions of experience that do not refer to physical events would constitute a rejection of what I call "materialism."

Although entertaining reading, the battle with dualism is a red herring, diverting materialists from the important question posed by the Jackson-Nagel argument. To isolate the important question, let us assume that materialism-for-psychology is true. In other words, psychological states are brain/body states. There is no psychological difference without a neurophysiological difference. If I am sad today and happy tomorrow, then something has changed in my brain. If I believed in God when I was in college and do not believe in God today, then something has changed in my brain. If we accept basic materialism, we still have a problem with respect to Mary. Mary the utopian neuroscientist does not know all that she can know, meaning that being in certain brain states provides us with unique *information*. The important question to ask is the following: "is there any information that we possess by virtue of being in a particular brain state that is different from the information we possess when we are knowing about that brain state?" If no, then materialism is physicalism. If yes, one can be a materialist but not be a physicalist. I claim that the answer is yes, and what we gain by being aware of certain brain/body states is a form of psychological knowledge.

3. Eliminativist arguments against Jackson

A majority of Paul Churchland's arguments about Mary are aimed at trying to prove that sensations are brain states. For him, Mary possesses a set of sentences written down in neuroscience textbooks. These sentences describe what happens in our brains when we are seeing red. They are coded in a particular part of Mary's brain. What people with normal color vision possess is a representation of a "redness" in their visual pathways, which we call

knowing what it is like to see red. Mary, who has a set of sentences describing brain states and the person who knows what it is like to see red both have knowledge of the same state, they are just representing that state in different ways. This is called the identity theory.

This argument, which is predicated on being a scientific realist, was made popular by Herbert Feigl (1958) and Paul Meehl (1967), who both thought that there is only one world and that no matter what kind of evidence we use to find out about that world, we should ultimately find out the same things. This means that behavioral and physiological evidence is all we need to gain complete knowledge (by description) of the physical world and human psychology. With the appropriate aids, a blind scientist can learn all that a seeing person can learn about the physical world. By analogy, a Martian having an entirely different subjective makeup could, with enough time spent learning human language and observing human behavior, develop the same kinds of theories about our emotions and psychological states that we have developed. Scientific theories are all about the same thing, no matter what kind of evidence we use to discover and justify them.

Jackson's (1982) argument, however, is the claim that having a representation of the red object in the color pathways of the brain constitutes a kind of information (that still needs to be conceptualized). Unless Mary also has a similar representation, unless she has the brain state in question, she lacks information. Mary's not having a representation of redness in her visual pathway in no way supports the dualists' claim that the perception of red is not a brain state. At the same time, admitting that the perception of red is a brain state does not mean that Mary has all the information had by a person with normal color vision.

Churchland (1992) concedes this point when he states that if Nagel's bats have brains with a computational machinery that human brains simply lack, then the subjective character of some of the bat's internal states will "be beyond human imagination" (p. 66). The same is true for Mary. If she were born blind, lacking a normal visual cortex, no amount of third-person information will give her the knowledge of what it is like to see red. Churchland (1995) actually puts it very nicely, claiming that certain physical phenomena are both objective and subjective (p. 225). The objective (third-person) view is not all there is with respect to knowing the material world.

When Paul Churchland claims that Mary, upon being presented red for the first time, could immediately recognize it as red, all he is claiming is that

she could deduce that it has to be red. She would have collateral information about spiking frequencies of 90 Hz and other information about firing patterns in her occipital lobe, and immediately conclude that this must be red. We can and should grant him this. Mary's being able to immediately deduce what red is as soon as she sees it, however, is different from knowing what red is before she experienced it or any color remotely similar to it. I also doubt that Mary's recognition of red on her first seeing it would be as non-inferential as Churchland and Dennett think it would be.

Churchland also speculates that Mary could imagine red beforehand, just as an expert wine taster could imagine an unknown wine if it were properly described. The problem with this argument is a problem related to a flaw in the "ability argument" as used by Churchland and others. The ability argument states that Mary lacks a physical ability, but she does not lack knowledge. Seeing red is supposedly a physical ability, such as eating with chopsticks or wiggling one's ears (Lewis 1983, 1990; Nemirow 1980, 1990). No amount of information can give me the ability to wiggle my ears, but once I can do it, I have a new ability, but not new information. By analogy, since no information from neuroscience can help Mary see red, seeing red must be an ability. If I know what red is like, I have the ability to recognize red and I have the ability to imagine red. It is something we can do, but not some *thing* we know.

I can, however, imagine what it would be like to wiggle my ears or eat with chopsticks even if I have never done these things only because they are similar to other physical movements I have performed. The same is true for expert wine tasters. They can imagine a new wine because they have tasted all its components in other wines. Being able to do so requires that the wine taster has already had all the component subjective experiences, and Mary, having experienced only shades of white, black, and grey, does not have those component experiences. If we make Mary really blind and deny her any visual experience at all, the case against physicalism is even stronger. This disanalogy between ear wiggling, wine tasting, and Mary's seeing red is fatal to Churchland's argument.

Dennett (1991) agrees with Churchland's "we can't imagine what it is like for Mary to know everything" argument, noting that the friends of Jackson make the mistake of thinking of Mary as knowing a lot, rather than knowing everything. Dennett holds that failure of imagination is responsible for many philosophical paradoxes, just as failure of imagination was responsible for vitalists claiming that it is impossible for material mechanisms to

control the growth of a fertilized egg into a human infant. "I can't imagine it" is too often translated into "it's impossible." Although, Dennett establishes that "knowing it all" is difficult to imagine, his own articulation of what Mary would be able to do still shows only that she would use other kinds of first-person awareness in order to triangulate certain states and deduce what their effects on her might be.

What is really important to the folk psychology eliminativists such as Paul and Patricia Churchland is that Mary's conceptualization of seeing red may be radically different from ours. How someone educated in Utopian neuroscience experiences brain states called "seeing red" may be as different from our experience as the experience that Miles Davis has listening to his modal jazz recording "Flamenco Sketches" is different from the experience that a two-year-old child has listening to that same recording. It is hard to understand what modal jazz is, let alone hear it. According to the Churchlands, being practiced in the application of expert-level knowledge is a form of know-how. Even if, however, the Churchlands are correct and we can reconfigure our subjective and automatic awareness along more neuroscientifically accurate lines, Mary is still in trouble. Whatever first-person awareness that your run-of-the-mill Utopian neuroscientists would have when they are in the brain state we call "seeing red," it would not be had by their blind colleague Mary. Physicalism would still be incomplete.

The paradox of admitting the importance of subjective information but wanting to remain a physicalist is not easily resolvable. It backed one philosopher, Herbert Feigl into a corner, making him an easy target for some intellectual teasing from his colleagues Paul Feyerabend and Wilfrid Sellars. According to Paul Meehl (personal communication) Sellars used call him *Feigl: Monday, Wednesday, Friday* and *Feigl: Tuesday, Thursday, Saturday*, the idea being that Feigl's opinions shifted like the day of the week. Feigl's only out was that he usually held that subjective content was a mere experience, not knowledge. Because Paul Churchland rejects the idea of raw experience separated from conceptual understanding, he cannot retreat to the mere experience argument. He should give up the defense of physicalism and realize that all the information about the material world cannot be completely reconstructed by third-person descriptions.

4. What subjectivity contributes

That being in certain subjective states constitutes information is an idea familiar to us all. Teenagers who have never had sex believe that being in an orgasmic state is information, and they are often very curious to discover it. Curiosity with respect to finding out what it is like may be a common reason for people to try marijuana, heroin, cocaine, or LSD. Few parents devastated by the death of a child are going to be persuaded that their celibate priest or childless next door neighbor “knows how they feel.”

Paul Churchland’s point, that we do not have an intimate causal connection to all of our inner states, is still an important one, with implications, i.e., it is a mistake to take all subjective states and classify them as equally informative. Let me provide examples of different kinds of first-person knowledge, some of which have been used by others.

- a. A golfer knows what it is like to swing a club properly
- b. A golfer knows how to describe a proper golf swing
- c. I know what it feels like to lift my arm above my head
- d. I know what it looks like when my arm is above my head
- e. I know what red looks like
- f. I know what functional state my brain is in when I am experiencing the color red
- g. I know what it is like to be in a state of panic
- h. I can describe the neurophysiological substrate of panic

For each pair of statements, one of them refers to a first-person/subjective perspective and the other refers to an third-person/intersubjective perspective of the same physical state. The type of example used in the golf swing event is the type of example used in the ability analysis. “Knowing how to swing a golf club is an ability and we could resolve a lot of philosophical problems about subjectivity if we took it as a model for *every* subjective-objective distinction.” It is easy to believe that knowing what a golf swing feels like is not a fact because it does not tell us anything important about the world. Any information it provides is so trivial that we hesitate to call it information. There is no difference that makes a difference requiring us to reject the ability argument when we use these kinds of examples.

The arm lifting example is similar to the golf swing example. This is the kind of example where the “identity theory” analysis works well. In knowing what it feels like to have my arm lifted above my head and knowing what it looks like to have my arm lifted above my head, I am knowing the same thing. They are identical. Lifting my arm is an event and I can know it under two different descriptions. In the arm example, it is easier to see that there is some information I get in knowing what it feels like to raise my arm, but it is also trivial information.

As was true for the arm lifting example, we can say in knowing what red is like and knowing the brain state that is the substrate of seeing red, we are knowing the same event. However, this case differs in an important way from the golfer example in particular because knowing what red is like seems to tell me something important about the world. It matters to us. As infants, we are naturally inclined to discriminate this kind of information. This type of subjectivity is what is referred to by the argument about Mary the blind neuroscientist.

The example about feeling panic is the most important one, dealing with the kinds of subjective experiences that are crucial for having a psychological understanding of both ourselves and other people. Taylor (1995) notes that a defining characteristic of those who lack psychological-mindedness is an inability to know about themselves as revealed in their emotional reactions and fantasies. Affective information is non-trivial in the extreme. It makes all the difference in the world for psychology.

5. Psychology and subjectivity I: knowing ourselves

Psychologists generally separate emotions into physical, behavioral, cognitive, and subjective components. These elements are all a part of what Averill (1980) calls an emotional syndrome. William James (1890) described the *physical component* as follows “*If we fancy some strong emotion, and then try to abstract from our consciousness of it all the feelings of its bodily symptoms, we find we have nothing left behind*” (p. 451). The physical obviously includes the physiological. The *behavioral component* refers to both the fact that we express emotions (crying, clenched fists, etc.) and the growing consensus in psychology that emotions serve the purpose of preparing us for some kind of action. As one prominent theorist puts it, emotions indicate a change in “action readiness” (Frijda 1988). The *cognitive component* focuses on the role of

evaluation or appraisal, for example Albert Ellis's (1989) idea that events do not cause emotional consequences, beliefs do. The person who feels devastated at being turned down for a job has to believe that the rejection is a catastrophe in order to have the reaction they do. Part of cognitive appraisal involves evaluating the relevance of events to the self, which involves a personal "subjective" reaction. The word "feeling" is often used to refer to the *subjective component* of emotions. The Jamesian idea that emotions give "color" and "warmth" to experience captures the subjective/phenomenal component of emotions. In addition to feelings, we also experience moods, which are enduring low intensity feelings that have no specific object.

Blind scientists, emotionless Martians, and androids with positronic brains allow philosophers and psychologists to talk about differences between someone who experiences subjective qualities and someone who does not. Although useful for making logical points, fictional examples may not be necessary. People who are depressed cannot remember what it is like to feel happy. People who are manic may have no access to experiences of being sad. These extreme cases have less dramatic parallels in everyday psychology as well. Many people have no strong experiences of anger, sadness, or loneliness that they can access. Emotions such as shame are subtle enough that people do not always recognize them, while others simply do not feel them. In the real situation of the human psyche, the importance of experiencing subjective states for gaining information about ourselves and others is an everyday concern.

Alexithymia. Clinicians even have a name for a condition in which people cannot recognize that they are in particular affect states. Sifneos (1972) labeled it *alexithymia*. Linden, Wen, and Paulhus (1995) define it as "an impairment in the recognition and utilization of emotions" (p. 52). Krystal (1988) believes that alexithymia is an important determinant of failure in psychotherapy because it prevents clients from receiving and using the maximum amount of information that subjective awareness of affect can bring. Using the concept of "affect tolerance," he speculates about the importance of being able to consciously experience affect as a requirement for emotional growth and maturity. Interestingly, Krystal even describes alexithymia as analogous to being color blind, but the information to which the person is blind is affective rather than visual. Like color blindness, the originators of the alexithymia concept (Sifneos 1972, 1973) and Nemiah (1977) all believed it had identifiable neural substrates — so there is no specter of dualism here.

In their comprehensive review of the research, Linden, Wen, and Paulhus (1995) report that persons with alexithymia cannot distinguish between emotions, localize feelings in their bodies, or properly label what most people would consider an “emotional expression” as an emotion. When asked to describe their feelings, they often describe external events. They also have a muted fantasy life. As might be expected, their interpersonal relationships are aloof and indifferent. To illustrate, Westen (1996) describes an alexithymic patient who told his therapist about a strange “event” he experienced earlier in the day, specifically, finding himself shaking and tearing up. This made him wonder whether he was crying. In addition to not recognizing “crying,” this person had no clue that his reaction might have been related to the negative biopsy results he had recently received. Westen’s example shows that although not independent of other components of emotion, first-person awareness is our most natural and consistent indicator of emotional states.

The borderline personality. Emotions subjectively understood are an essential part of the conceptualization and treatment of various psychiatric disorders. Marsha Linehan (1987, 1993) has developed a cognitive-behavioral therapy protocol for persons with borderline personality based on the idea that an inability to tolerate strong states of negative affect is a central feature of borderline dynamics. Like Kernberg (1975), she thinks that the development of borderline personality depends on both internal factors, such as unusually strong negative affect, and environmental factors involving inappropriate responses to that affect by significant others. The common theme of these responses is that they invalidate “private experiences.” People who grow up in invalidating environments do not learn consistent labels for their private experiences, especially those experiences with less obvious public accompaniments. People who do not learn to trust their internal experiences and reactions compulsively search for external cues about how to act, think, and feel. They also fail to develop a coherent sense of self. Linehan’s model is supported by developmental research showing that lack of emotional attunement between infants and care givers leads infants to become what clinicians call “disorganized,” unable to minimally regulate their affect states (Oatley & Jenkins 1996).

Because of the importance she places on external validation, Linehan does not see affect as an essential subjective quality that is just given to us. We do, however, experience the subjectivity of emotions, and how we do so has implications for the extent to which we have either integrated or fragmented

psychological lives. Linehan's dialectical behavior therapy treatment regime seeks to help patients learn to tolerate strong negative affect states rather than acting them out in self-destructive ways, e.g., suicide, self-mutilation, and impulsive sensation seeking. The early phase of treatment is focused on eliminating problem behaviors, but it is solidified by teaching the person the skills to cope with negative affect such as anger, loneliness, and depression. This includes teaching appropriate labeling, interpretation, and action. Learning that their emotional reactions make sense and are understandable, even if they are dysfunctional, helps these patients achieve some self-coherence. Research indicates that this model is effective in reducing anger, acting out, social maladjustment, and occupational dysfunction (Linehan, Armstrong, Suarez, Allman & Heard 1991; Linehan, Heard and Armstrong 1993).

Psychopathy. We can see another example of the importance of the phenomenal character of our subjective lives with respect to psychopathy. Whereas borderline personality involves overly intense feelings, psychopathy involves a "dramatic" unintensity of feeling. One consensus description of psychopathy is that those meeting criteria have minimal experiences of anxiety and guilt (Lykken 1957; Fowles & Missel 1994). They may know how to use words such as worry and guilt, but they never feel those emotions except in a very minimal way. With such limited experiences of suffering, persons with psychopathy tend to be rather shallow. Talk therapy has not been useful in helping the person with psychopathy feel certain emotions intensely enough, and in not doing so, they interact with the world in disturbing way.

The emotion-laden information state in question is a complex one, not a simple one. Although there is evidence of a general physiological under-responsiveness by persons with psychopathy (Zuckerman 1990), their deficit may just as likely be a cognitive failure to anticipate the anxiety (Fowles & Missel 1994). This cognitive hypothesis explains why persons with psychopathy can feel panic — and then lie to avoid immanent punishment. Dennett's (1991) claim about not being able to isolate a phenomenal state or feeling from everything else that is going on is relevant here. Using a particular affect state as information involves several cognitive competencies, so there is no raw anxiety or suffering in itself.

Even if the alexithymia of the person with psychopathy originates in a cognitive subsystem, the "deficit" still involves the subjective reaction not progressing in a normal fashion. Subjectivity is not factored out with a cognitive etiology. Just think about the complexity of the experience of

suffering after experiencing the sudden and unexpected end of a committed romantic relationship. There are the intense initial feelings, the sickness in the stomach, ringing confusion and fear, followed by the more chronic dread, physical heaviness, and feeling as if you have a rock in your stomach as opposed to the earlier experience of nausea. Those subjective states are contingent on an interpersonal context where a particular person is important to you. They are important for reasons particular to your identity/self concept, and the rejection has imagined consequences for you because some explicit or implicit expectation has been thwarted. The initial interpersonal context and the imagined interpersonal/personal consequences are an integral part of psychological suffering. Even if the problem for the psychopath is not literally in the substrates of subjective feeling, the resulting lack of intensity of feeling is the psychologically important symptom.

A radical materialist's response to the psychopathy example is that the problem in question is really some kind of neurophysiological deficit. There is supposedly a neurophysiological deficit which is the substrate of the subjective deficit; what we really need to do is fix that neurophysiological deficit and then persons with psychopathy will have the appropriate subjective states. This common sense response, however, misses the point. It is a perfect example of switching to a different question in the middle of an argument, something eliminativists do all the time.

The philosophical problem was determining what kinds of evidence or data or information we need to find out about the world. The Jackson-Nagel argument says that there is information that neuroscience alone cannot give us, so neuroscience has limits. Describing the neural deficit to the psychopath will not help them suffer. They could probably care less. The psychiatric problem, biomedically defined, was learning how to put somebody in different kind of brain state. The answer to the psychiatric problem is that there are many ways to put somebody in a brain state, but let's say that physical manipulation of the brain is the most direct (but not always the most effective or most ethical). This answer: "we can do all we need to if we know enough neuroscience," is not an answer to the philosophical problem. As a matter of fact, the reason we want advanced neuroscience to help us put persons with psychopathy in a different brain state is to give them information that they cannot get from the third-person neuroscientific descriptions alone. This third problem, the psychological problem, involves helping them experience socially appropriate guilt and empathy.

Panic. More concrete examples of subjective states providing information can be found in examining feelings that indicate panic. In therapy, the first step in helping people with panic disorder is teaching them that they are experiencing panic. Often they are only vaguely aware of their symptoms and you need to ask them to notice their shallow breathing, racing heart, dizziness, and fear of going crazy. Sometimes they know about their symptoms, but don't identify them as indicating panic, making their panic attacks even scarier. Once they understand that they are having panic attacks, some clients even interpret felt symptoms incorrectly, such as thinking that their racing heart at the top of the long flight of stairs is a panic symptom. Fearing the initiation of a panic attack tends actually to bring on a panic attack — fear breeds fear. Educating people about what things feel like and the context in which we feel them is important to psychological therapy.

Emotions as signals. Everyone's self-understanding is enhanced by the extent to which they can experience and label particular subjective states. We could call the information bearing role of subjective states the *signal theory of emotion*, a view common to Aristotle, Descartes, Freud, Tomkins, Damasio, and a host of others. Being able to make distinctions between anger, sadness, fear, happiness, disgust, surprise, shame, guilt, etc. has a great practical advantage. We can make finer distinctions as well; for example, Fischer, Shaver and Carnochan (1990) separate anger into emotions such as jealousy, annoyance, contempt, or hostility. Unlike what psychologists call "basic emotions,"⁹ these subtle emotions are not biological universals, but they provide information to those who have them.

Abnormal psychology is not the only context for demonstrating the importance of subjective information. Harriet Lerner (1985) specifically discusses the importance of emotions as signals with respect to normal functioning. To simplify, both clinical lore and pop psychology hold that men are socialized to be more comfortable with anger than sadness, and women are socialized to be more comfortable with sadness than anger. In cliché form, clinicians say that women turn anger into tears and men turn tears into anger. Being uncomfortable with certain kinds of emotion prevents both genders from using all the information available to foster healthy adaptation. Lerner's

9. If (or when) neurophysiology demonstrates unique physical substrates for basic emotional states, such as research showing exactly how the peptide cholecystokinin is important in panic, its contribution to the psychology of emotion would be immense, especially since there is disagreement about which emotional states are "basic."

(1985) focus is on helping women with anger.¹⁰

She notes that many women are taught to fear anger in order to not be labeled as “strident,” or “castrating bitches,” or “shrews.” If their model of femininity discourages anger, in many situations they just don’t feel it — often to protect a relationship. Some women also have non-adaptive experiences of anger that take the form of angry outbursts, what Lerner calls nonconstructive expressions of anger. She thinks that just as physical pain tells us to take our hand off the hot stove, anger tells us either that we need to separate from something or just that something is not right. Tavis (1989) notes that the “something” is usually the interpretation that someone is not behaving how we think they ought to behave. To the extent that anger is a signal that something is wrong, both ignoring the anger and nonconstructive outbursts protect us from trying to find out what needs to change and how to change it.

Understanding and adaptively acting on the information that our emotions provide is an incredibly complex task. Lerner’s model of anger management is based on the idea that the information in the anger is not just a raw feeling. As a signal, it needs to be conceptually analyzed with questions such as “what is the real issue here,” “what do I want to accomplish,” and “what options will and will I not pursue.” Because we can displace anger from one person to another, the answers to these questions are not always obvious.

Tavis (1989) shows that the consequences emotions such as anger have depends on our attributions about their initiating object. If I get angry with a colleague, the meaning of the emotion differs depending on whether I attribute the anger to something inappropriate my colleague did to me, whether this person “pushed one of my buttons” having to do with general life frustration and I just felt safe enough to get angry with him or her, or whether I am “really” angry about something at home and was reminded of it by my colleague’s behavior. The signal theory of emotions helps us see that personal development in terms of having less distorted self-understanding involves learning how to pay attention and conceptualize our anger, fear, and sadness.

Another important advantage to knowing about emotion is understanding the effect that emotional states or moods have on our cognitive processing. Emotional states serve as contexts for processing information. We think differently when we are in different emotional states and these differences are

10. Even though women may find it easier to express sadness, Tavis (1989) and the research she reviews shows that there are minimal differences between men and women with respect to being angry.

consistent from state to state. The experimental psychologist Gordon Bower (1981, 1992) has shown that, although a wide range of information is potentially available to us, we tend to select information that is mood congruent, that supports our current mood. For example, sad people will access memories that are consistent with feeling sad. Their interpretations of events are also mood congruent. Often, longer-lasting states such as pessimism, openness, and caution help construct people's world views. The obvious advantage to being aware of your general affect states is an increased ability to evaluate the nature of your reality testing. This advantage has received some experimental support (Forgas & Bower 1988).

A folk theory of emotions? Not only is awareness of emotional states fundamental to self-understanding, but philosophical opposition to emotions by scientific materialists ironically depends on an unwarranted folk psychological bias. This bias derives from the Cartesian idea of emotions as passions, as things that take us over against our will and reason (Averill 1980). Although Plato was an early believer in the base nature of emotions, Darwin's placement of emotional functions in the prehuman epoch of the species' evolutionary history has probably exerted the greatest influence on physiologically-oriented moderns. J. Hughlings-Jackson's model of the evolutionary levels of organization in the brain, with the reasoning/cortex at the top followed by the emotions and reflexes at "lower" levels, reflects this Darwinian view. In this model, emotional reactions are primitive, and rationality requires their minimization.

This folk model has been forcefully critiqued by the neurologist Damasio (1994), who claims that the cognitive structures to which scientists have traditionally attributed "rationality" are not built upon limbic system structures, but are built out of and integrated with them. According to Damasio, rationality requires the functional integration of emotion and cognition. For example, the important deficits in cases of frontal lobe brain injuries such as those of Phineas Gage involve an inability to experience the emotional reactions that we normally feel in response to imagined consequences of intended courses of action. Without emotional understanding, especially of negative consequences, our ability to engage in practical decision making is decimated.

The philosopher de Sousa (1987) proposes a similar view, claiming that there are so many possible goals for us to pursue and strategies by which we could pursue them, that we need emotional reactions to help us quickly eliminate from consideration goals and strategies with potential negative consequences.

In helping us frame problems, emotional reactions are an important cognitive asset because we can't afford to spend time rationally deliberating every possible course of action. Although Damasio provides experimental evidence to support the cognitive importance of emotional reactions, ever since Nietzsche and Freud, solid arguments for rejecting a Cartesian mind-body, thought-emotion dichotomy have been available. In this latter tradition, counseling and clinical psychologists have long worked on the basic premise that *we know ourselves and the world through our subjectivity, not in spite of it.*

6. Psychology and subjectivity II: Knowing others

In the previous section, I explored the advantages of having subjective information with respect to self-understanding. More important than the information subjectivity gives us about ourselves is how it influences our understanding of others. Jackson (1986) correctly states that the most important information that Mary would gain if she suddenly experienced seeing red would not be information about herself. The most important information would be the increased knowledge of how impoverished an understanding of other people's experiences she actually had in her black-and-white world. I think that this advantage would be expanded exponentially if Mary's deficit was affect blindness rather than visual blindness. In Oatley and Jenkins's (1996) phrase, for both humans and primates, our emotional lives are our social lives.

Feigl had his *Monday, Wednesday, Friday and Tuesday, Thursday, Saturday* problem because he realized that understanding another as a subject is extraordinarily important to what might be called our "humanity." His bedrock argument for the importance of subjectivity was that it would be impossible to understand the moral imperative "do no inflict pain on others," unless you had some knowledge of what pain is like. In Feigl's (1971) terminology, the surplus meaning that we get by knowing what pain feels like is indispensable.

To expand on the importance of suffering, one has to wonder how Hitler or Himmler might have responded if they could feel the loss and sorrow of their Jewish victims instead of the resentment, fear, and anger that they probably felt. Their moral failure followed from either a failure of feeling or a failure to attend to their feelings. Today, educating people about the horror of the Holocaust has to involve an attempt to evoke emotions that at least suggest what it must have felt like to be Jewish in a Nazi regime. If Oatley and Jenkins

(1996) are correct in defining the necessary criteria for an emotional state being “a readiness to act,” the Nazis’ failure to either have the appropriate feelings or attend to their feelings was the occasion for their moral failure.

Ultimately, the reason that we consider persons with psychopathy to be psychopathological relates to how they treat others. Without knowing about certain subjective states, they cannot imagine others as being in those states. Their inability to realistically imagine themselves suffering is the most common explanation for both the fact that they don’t bother to avoid punishment (Cleckley 1941, 1976; Fowles 1993), and for their pathological inhumanity. In lacking experiences of suffering themselves, persons with psychopathy do not empathize with other people’s pain and anxiety. They see others as means to ends, objective things to be manipulated rather than persons. The reason persons with psychopathy have limited empathy follows from their own muted subjectivity. In Dennett’s (1978) terms, they can adopt the intentional stance, but not the personal stance.

Empathy. Empathy is essential for sophisticated forms of psychological mindedness. Hoffman’s (1984) distinction between intuitive empathy, where feelings are contagious as laughter is contagious, and abstract empathy, which involves imagining what it would be like to be in another’s situation and paying attention to your subjective reactions, is a good one. Hoffman and his fellow developmental psychologists have shown that intuitive empathy is the kind of empathy had by young children. It is natural empathy. Abstract empathy is the kind of empathy used by psychotherapists and competent parents. It is practiced empathy and corresponds to what Gordon (1986) calls *simulated practical reasoning* or just simulation.

In this context, Jackson’s (1982) Fred argument is more useful than his Mary argument with respect to the problem of understanding others. It is more useful because it asks us to imagine someone who experiences something that we do not. Fred is a person who benefits from a genetic mutation that allows him to make a particular color discrimination that we lack. If given a batch of ripe tomatoes, Fred sorts them into two different groups, which he calls red₁ and red₂. For him red₁ and red₂ are separate hues, as different to him as yellow and blue are to us. According to Fred, the rest of the world is red₁, red₂ color blind. He has tried to teach his friends the difference between red₁ and red₂, but failed.

If some clever neuroscientists ran tests to see whether Fred’s visual system is objectively different, and they found out that Fred has an extra class

of retinal receptors that are excited at a particular point in the 700 nanometer range, and only at that point, we would have an objective reason for believing that Fred is not just lying to us. Objective knowledge about the state of Fred's brain would not, however, tell us what red₁ and red₂ look like. Although there is no immediate difference that makes a difference with respect to discriminating between red₁ and red₂ (except for Fred), discriminating between feeling something and feeling nothing, between feeling sad and angry, or between feeling for me and feeling for you is a difference that makes a difference for each of us.

Part of being a competent psychotherapist involves being able to process subjective information, both your own and the client's. Knowing what emotions feel like and to some extent feeling what clients feel can be a difference that makes a difference in therapy. Having feelings is not always useful to the therapy and may sometimes be rather trivial, but not always. It is therefore important that therapists learn to examine their feelings because feelings can both facilitate and hinder the therapeutic process. Training in psychotherapy actually involves helping students learn communication rules for achieving more accurate empathy. (Technically, if awareness of your own subjective reactions is first-person information, awareness of another's subjectivity could be called second-person information.)

I will briefly discuss three kinds of first-person information available to the therapist: (a) classic counter-transference, (b) object relations therapy counter-transference, and (c) ego-extraneous feelings.

In classical psychoanalysis, counter-transferential feelings involve the therapist's personal and neurotic responses to clients and the clients' experiences. These responses are subjective distortions which interfere with accurate understanding and client-centered interventions. In this case, first-person information interferes with obtaining accurate second-person information. An example of classical counter-transference would be seen in a female therapist who, in listening to a client tell about how his mother verbally abused him, remembers her own experience of being verbally abused by her mother. This therapist also blames herself for not standing up to her mother. Counter-transference would occur if she got angry with her client for not standing up to his mother, and wanted to focus on his timidity problem. She may distort her client's material and assume that (as for her) his self-esteem is low because of passivity.

Within the object relations and interpersonal frameworks, having counter-

transferential feelings is important to successful therapy because counter-transference refers to appropriate emotional responses. Upon becoming aware of feeling angry toward a male client who tends to dismiss opinions other than his own, an interpersonal therapist might engage in the following stream of thought" "Why am I angry? Is this how other people react to him? His presenting problem is feeling alone, but if others react as I do, I know why he is alone." Sharing the anger with the client could be an important therapeutic intervention. Knowing about others' subjective reactions to him might give this client new information about himself.

Sometimes the emotions therapists have are more appropriately attributed to the client rather than to the therapist. These could be called ego-extraneous feelings, even though they are probably just a variant of everyday empathy related to abstractly imagining yourself in the other person's situation. In this case, first-person information contributes to obtaining accurate second-person information. To illustrate, a therapist may listen to a female client repeatedly discuss her father's commitment to his career above all else. Listening to this story, an astute therapist may notice that they themselves are feeling sad. Upon examination, this emotion does not seem counter-transferential in the classic sense or a counter-transferential reaction to the client's story in the interpersonal sense. What often comes to mind is the idea, "this person feels sad." Asking the person if they feel sad will often result in a very surprised and wide-eyed expression on the client's face: "I do feel sad!" In the ensuing discussion, the client learns something about themselves. On many occasions, the only cue for this kind of intervention is the therapist's own emotional reactions. Young therapists even have to be taught not to do the client's work, i.e., to not feel things for them.

Paul Churchland (1992) exaggerates when he claims that first-person information and the ability to imagine people as subjects is not necessary for understanding the behavior of others. A world in which we did not view others as persons would be a grim place. If all knowledge is reducible to third-person information, we should be able to eliminate any reference to subjects and everything would be the same. Since we all live in a world where explanations based on first-person information and second-person speculation are readily available, even those who have a limited understanding of others as subjects can gain some benefit from these explanations. For example, persons with psychopathy have some understanding of the concepts of guilt and suffering, but social comprehension is not the same as empathy.

Given (a) the importance of emotional reactions and (b) the importance of subjectivity for understanding emotions, we can reasonably conclude that being in a brain/body state provides us with information that we cannot learn about by having only a physical description of that state. In materialist terms, empathy is taking my knowledge of being in certain brain/body states, including their consequences, and understanding the other person as being in similar states. Clinical and counseling psychologists claim that if you do this, you learn something important about the person. In terms of learning about ourselves and the world, neuroscience is limited and therefore, even if materialism is true and the brain is the substrate of psychological states, physicalism is false.

Feigl (1967) put it this way with respect to Sellars's eliminativism: physical theory can explain phenomenal experience, but it cannot explain it away. This view of subjectivity is similar to Dennett's (1991) view that consciousness can be explained in terms of non-conscious processes, but that kind of explanation does not explain it away. *Subjectivity-consciousness-phenomenal awareness is a fact of the material world.* Searle (1992) makes a similar point when he claims that mental phenomena are biological processes. In seeing subjectivity as a fact about certain parts of the material world, Materialism without Physicalism provides a different answer to the problem of subjective information than either dualism or unity of science physicalism. It allows psychologists to avoid dualism without having to be behaviorists or neurologists.

7. A brief note on the issue of ineffability (or privacy)

Mind-body dualists have traditionally claimed that ineffability is a property of the mental. Ineffable means that something can't be described or put into language. Jackson's argument is a kind of ineffability argument because he is claiming that being in or remembering the brain state that instantiates seeing red is a necessary condition for Mary's knowing what red looks like. You must have the capacity to be in that kind of state to really know the information it provides, even though you can know a lot about that brain state from a third-person perspective. I would agree that being in a brain state is ineffable in this sense, and even say that a materialistic understanding of subjective states removes some of the mystery surrounding ineffability. It is ineffable

because you have to be in the state to know what it is like. Gunderson (1970) has a similar solution to the mystery of the problem of other minds: The physical reason that we can't know about another's experience in the same way that we can know about our experience is that we can't be our brain and their brain at the same time.

What it is like to be in a brain/body state is also something we communicate about, so it is not ineffable in the sense of being "absolutely unspeakable," meaning that we can say nothing about it. We talk about what pain feels like or what love feels like all the time. If you don't know what the brain/body state is like, your understanding of those descriptions is limited. We can even communicate about brain/body states without words, e.g., painters such as Van Gogh or musicians such as John Coltrane communicate about emotions without words by putting us into those states. The subjective information is part of the meaning of their work and they use certain media, colors, and sounds, to evoke certain emotional states in us. Good novelists and film directors do the same thing.

Andrew Bailey (personal communication) has suggested that if a poet can use language to put us into a phenomenal state such as feeling sorrow, a complete mathematical description of a cube could probably conjure the image of a cube in a person who has never seen a cube. This assumes a great amount of mathematical knowledge on the part of the person, but it is possible. If this is so, then perhaps a very complete description of the functional or neural realization of the brain state "seeing red" could help the reader of that description see red. This assumes great neuroscientific knowledge on the part of the person, but it is also possible (claims Bailey).

In my view, the mathematical argument works if we assume that the mathematician has a normally functioning brain and, for some currently unimaginable reason, has never before seen a cube. With the complete description, he or she should be able to actually construct a cube and probably imagine one before actually constructing it. Stretching it a little, this could be called knowing what a cube is like. They can create that external physical state we call a cube (even though I bet they could not imagine what a five dimensional object would be like). In the same way, the blind neuroscientist should be able to imagine the brain state of seeing red and model it externally, just as the mathematician models the cube. Unlike the cube example, imagining an external (dynamic) model of the brain state of seeing red would not put the blind neuroscientist in that brain state, especially if, on the strong version,

they had a degenerated area V4 of their visual cortex. The description would fail in the same way that certain literary works or musical pieces fail to effect persons with alexithymia.

8. Conclusion: Subjectivity at Least Confers a Cognitive Edge, and Probably More

Feigl (1958, 1967) and Meehl (1967) provide the cleverest defense of physicalism by making scientific knowledge the paradigm of good knowledge, i.e., true *justified* belief. Although they both thought that our subjective access to our inner states does provide a unique kind of evidence, they held that this evidence was important only for the context of discovery. Knowledge of our inner states aids us in making certain discoveries, e. g., that I am depressed or anxious. It also has heuristic value and helps us hypothesize about the nature of the world, e.g., that depression is worse in the morning but gets better as the day goes on. Having what Churchland calls an intimate causal connection to our inner states confers what Meehl (1967) called a “cognitive edge.” This cognitive edge is clearly a difference that makes a difference.

Feigl thought that no type of evidence is special with respect to knowledge about the world. A Martian having an entirely different subjective makeup, with enough time spent learning human language and observing human behavior, could construct the same kinds of theories about emotions and psychological states that humans have constructed. He just triangulates them using different types of evidence. Once we have these concepts and hypotheses, we have to *justify* them in terms of reasons and rules. So subjectivity is not necessary for the context of justification. Not only do we not need a subjective perspective to justify these hypotheses, the process of justification is always going to be an intersubjective activity. In terms of scientific justification, Feigl thought that subjectivity is simply factored out.

Meehl (1967) wrote about knowing the subjective referent of a particular term as understanding a language entry rule. Part of knowing that “s-a-d means *sad*” involves knowing what it feels like to be sad. Subjective experiences also get us started talking about certain phenomena, particularly emotionally laden states. I would add that in addition to an entry rule, it is part of the reason we keep emotion terms around, so it is also a linguistic endurance condition. If we exiled, to another planet, a group of alexithymic people who

had no experience of guilt, they might continue to talk about guilt because it is already a part of their language, but if they had guiltless children, the concept of guilt would probably decay into non-use in a few generations. The term would have no more use in their psychological lexicon than phlogisten does in our chemical lexicon. Psychopathic cultural historians could write about how people on Earth use to talk about demons and guilt and altruism, but in their world, those things just would not exist. Many third-person statements require some reference to subjective states to be fully understood. Subjectivity cannot just be factored out. In Meehl's (1967) words, to say that they *do not fully understand it* means that they cannot intend, by their use of guilt, what we intend, in our use of guilt. Any statements involving depression, anxiety, confusion, or desire are richer if we know what those brain/body states are like. Subjectivity is not the foundation on which psychology is built, but it is part of the evidence. This provides an important role for the psychological level of analysis as partly distinct from both the sociological and neuroscientific levels of analysis.

PART III

THE PSYCHOLOGY IN PSYCHIATRY

CHAPTER 8

Diagnosis, Behavior, and First-Person Information

1. Introduction

As stated in Chapter 2, biomedical materialism is partly defined by the goal of making psychiatry a science. Biomedical materialists think that becoming an objective science will allow psychiatry to join other medical specialties as one of the great contributions modern learning has made to the good of humanity. The most committed proponents of biomedical materialism would prefer to eliminate the distinction between psychiatry and neurology, turning all psychiatric problems into problems of neuroscience.

Although a scientific approach is important to any legitimate medical specialty, it is also important to not simplify “science.” Scientism, the view that the more rigorously and exclusively we use the scientific approach in *any* endeavor, the more superior and utopian the product, leads to such simplification. One sure sign of scientific simplification is black-and-white thinking, exemplified by a tendency to rigidly divide disciplines into the scientific, the pseudoscientific, and the irrelevant. In addition, scientific thinkers take the mere suggestion that experimental methodology by itself might not solve all important problems to be an attack on science in favor of a “prescientific” view of the world. In medicine, assigning exclusive priority to rigorous experimental methodology also encourages a more exclusive focus on physiology and genetics because, as Guze (1992) says, “causal hypotheses that do not involve complex psychosocial or cultural factors are generally easier to test” (p. 91).

In Chapter 5 I argued that those biomedical and eliminative materialists who advocate scientism have adopted a Comtean view of history and they use this view of history to make decisions about what constitutes legitimate

evidence and legitimate theoretical models. Comtean history divides human thinking into three stages, the anthropomorphic, the metaphysical, and the positive (objective/physical). The earlier the stage, the less legitimate its concepts. The later the stage, the more legitimate its concepts. Since the biological is more physical and the psychological is thoroughly anthropomorphic, for Comteans, being more biological means being more objective and hence, more scientific. Therefore, the more psychological an analysis, the more primitive and superstitious it is. These Comtean assumptions form the basis of several persuasive but empty slogans favored by biomedical materialists.

Isn't it time to bring psychiatry out of the Dark Ages and face schizophrenia for exactly what it is—a brain disease in need of research, sick individuals in need of services, the most tragic disease in western civilization (Torrey 1988, p.14).

While Watson and Crick established the structure of DNA, psychiatrists agonized over the reality of involuntional melancholia as a separate entity, their arguments resembling medieval disputations rather than informed scientific arguments. (van Praag, Lader, Rafaelsen, & Sachar 1979, p.ix).

The equation, “more scientific means more biological” is one of the major fallacies of biomedical materialism. In this chapter I will explore several reasons for rejecting the view that the more exclusively genetic and physiological psychiatry is, the more legitimate it becomes as a medical specialty. It will become clear that the actual practice of diagnosis contradicts the biomedical materialists' assumptions about the possibilities of a purely objective psychiatry. The important role played by first-person information in diagnosis and treatment means that psychiatrists can be materialists, but not physicalists.

2. Adjoining Levels of Analysis Cross Fertilize Each Other

Like the eliminative materialists in philosophy, the biomedical materialists in psychiatry also reformulate the goals of their discipline in conformity with their nearly exclusive commitment to biological explanations. This commitment takes two forms. For one, they want to be as scientific as possible, with scientific meaning as biological as possible. Second, just as the eliminative materialists reformulate psychological problems into problems about how the brain works, biomedical materialists reduce the problem of understanding psychiatric pathological processes into the problem of understanding biological pathology.

Guze (1992) employs a subtle version of problem redefinition when he writes about the importance of understanding functional causes, such as Warblers flying south in the winter because they will otherwise starve, but qualifies the importance of this “cause” by noting that Warbler physiological mechanisms are the real cause of migration. In the same way, he states that whatever the evolutionary reasons for our having a liver, it is the anatomical and physiological understanding of the liver that allows medicine to treat it when it is not functioning properly. He suggests that since psychiatry is a branch of medicine, the same is true in all respects for psychiatric problems. In contrast to Guze’s claim, it is only true when the problem in question is understood solely with respect to broken brains. In this chapter I will show that there are scientifically-based reasons for believing that psychiatric problems cannot be understood solely with respect to broken brains.

Andreasen’s (1988) redefinitions are much less subtle. For example, studies have shown that the concordance rate of developing schizophrenia between monozygotic twins (who share the exact same genetic structure) is 28%-40%, whereas among siblings (including dizygotic twins) the concordance rate is 6%-15%. While demonstrating a genetic basis, these studies also demonstrate an environmental component to schizophrenia. This has long been considered a validation of the diathesis-stress model of psychopathology, which states that actually developing a disorder such as schizophrenia requires a biological predisposition (genetics) and stressful life events (environment). In her own listing of “environmental factors” Andreasen (1988) notes that the variables to target for further research include maternal nutrition, maternal substance abuse, labor and delivery, childhood illnesses that might produce some type of encephalitis, various viral illnesses including slow viruses, and head injury. This recommendation is an excellent example of reformulating all problems into problems of biology because environmental is defined in such a way that it is directly translatable into concrete physiological consequences. Traditional environmental variables such as expressed emotion are eliminated.

One example of practical competence requiring more than the physiological level of analysis is provided by Engel (1977, 1980). From the perspective of a scientist-practitioner, Engel addresses the deficiencies involved in narrowly applying a biological researcher’s model to practice. He claims that, because the biomedical model cannot sufficiently account for all the causes of a physical illness, it constitutes inadequate science.

Engel tells a story about a patient who experienced his second heart attack in six months. After being admitted to the emergency room this patient lost consciousness due to a cardiac arrest. Engel points out that a practitioner using the biomedical model would conclude that it was very lucky the patient came to the hospital in time. According to Engel, such a narrow reliance on the biomedical model produces a faulty understanding of cause.

For example, this particular heart attack patient had stubbornly denied the importance of his symptoms earlier in the morning and was persuaded to come to the hospital only through a sophisticated and empathic intervention on the part of his employer. Once in the hospital, the patient was feeling stabilized and his symptoms had disappeared. He reacted to an unsuccessful attempt at an arterial puncture with anger and frustration. When his attendants left him to get help, he lost confidence in his caretakers and became outraged. At this point he had the cardiac arrest. According to Engel, a narrowly oriented biomedical practitioner, unaware of these other causes, would not be able to make the kinds of interventions that would have prevented a cardiac arrest. For Engel, it would be false to think that this cardiac arrest was part of the inevitable progression of the earlier heart attack.

A more psychiatry-relevant example is given by Gabbard (1992), who shows that even if one defines the problem biologically and uses a biomedical treatment strategy, psychological approaches can contribute to a successful outcome. He gives the example of a 29-year old male with severe obsessive-compulsive problems. Although prescribed clomipramine in the past, the patient had not taken the drug consistently. His condition, although uncomfortable, was personally advantageous. It helped him maintain a dependent relationship with his mother, whose life revolved around her house-bound son, including being with him nearly every hour of the day. By uncovering the "secondary gain" that the illness had for him, he was helped to overcome his fear of getting better. Once separation from his mother became less threatening, getting better became more attractive. Both the biomedical theory of dysfunction in the serotonergic system and the psychodynamic theory of defenses contributed to this person's treatment.

3. Systematic Diagnosis is not Co-extensive with Biomedical Materialism

The implication of having practical problems in the context of multiple levels of analysis, even for committed biomedical materialists, is evident with respect to the problems of diagnosis. Although the biomedical materialists have invested in scientific diagnosis as the first step in the biomedical revolution, this revolution is not as purely physical or objective as they claim. In this section it will become evident that because of the practical demands of day-to-day commerce with individuals, differences between the biological and the natural sciences undermines the biomedical materialists' own views of what psychiatry should be.

Although in Chapter 2 I stressed the biomedical component of the neo-Kraepelinian movement, neo-Kraepelinianism also connotes concern with systematic diagnosis. In the field of abnormal psychology, Kraepelin was best known for his descriptive approach to psychiatry and his concern with improving diagnostic categorization. The neo-Kraepelinians became a dominant force in American psychiatry by having a major influence on 1980 revision of the *Diagnostic and Statistical Manual of the American Psychiatric Association* (DSM-III).

The purpose of this revision was to make psychiatric diagnosis more reliable.¹¹ Four reasons for improving classificatory reliability were advocated by proponents of the biomedical revolution.

- (a) When researchers began investigating the effectiveness of psychiatric drugs in the mid-1960s, it became clear to them that the assignment of persons to diagnostic categories was not as reliable as it could be. As a result of poor reliability, there was too much heterogeneity within independent variables to isolate treatment effects.
- (b) Epidemiological studies of incidence were inconclusive because of wide variations in diagnostic practices across cultures (Andreassen 1984).
- (c) The anti-psychiatrists' (Szasz 1961; Rosenhan 1973) critique of the reliability and validity of medical diagnosis in psychiatry called the scientific status of psychiatry into question. Low reliability and validity meant that psychiatry was unable to clearly demarcate the healthy from the sick.

11. In psychometric theory, reliability refers to the consistency and stability of measurement. The DSM-III revision was concerned with inter-rater reliability, which is the extent to which independent evaluators can agree on a diagnosis.

(d) Finally, the low reliability of psychiatric diagnoses was an embarrassment to the field when psychiatry's credibility was undermined in a number of dramatic court cases (Kirk & Kutchins 1992; Klerman 1986). Psychiatrists were especially embarrassed within the medical profession.

The publication of the Feighner criteria by the St. Louis group of biomedical materialists represents an early attempt to improve diagnostic reliability. (Feighner, Robins, Guze, Woodruff, Winokur, & Munoz 1972). The St. Louis group adopted an operationalist methodology so that their system could be utilized by researchers working from different theoretical orientations, and so that people could communicate with each other and compare findings. Being foreshadowed by the "tough-minded" approach of Guze (1970), Robins and Guze (1970), and to a lesser extent other behavioral ratings scales developed in the 1960s (e.g., Beck 1969; Hamilton 1960), these criteria were developed specifically for use in research. The 1972 Feighner et. al. article became the most cited paper in the psychiatric literature throughout the 1970's (Blashfield 1984).

Under the aegis of the National Institute of Mental Health, the Feighner criteria were modified to become the *Research Diagnostic Criteria* or RDC (Spitzer, Endicott and Robins 1978). In 1980, these operational criteria became the basis for the publication of the DSM-III. With the publication of DSM-III, a researcher's model became the field's clinical paradigm.

The biomedical materialists' historical involvement in the development of DSM-III is beyond question. In addition to providing the inspiration for the DSM-III revision, biomedical materialists in the persons of Andreasen, Klein, and Woodruff were vocal and influential members of the nine person DSM-III task force (Millon 1986). Wilson (1993) reports that half the psychiatrists on the task force had current or past affiliations with the St. Louis group. He further reports that an analyst added to the task force resigned after two years because he thought that his psychodynamically-informed suggestions were dismissed out of hand by the other members.

As stated, some biomedical thinkers identify systematic diagnoses with their movement. For example: "The Neo-Kraepelinian revival had become the DSM-III revolution" (Andreasen 1984, p. 154) and "The objective definitions of the DSM-III are simply the first step in the biological revolution that is now occurring" (Andreasen 1984, p. 161). Other writers have also defined systematic diagnosis in opposition to psychological and particularly psychodynamic approaches (e.g., Klerman 1986; Maxmen 1985). This identification is, however, mistaken.

4. Psychological approaches are not anti-diagnostic or anti-operational

Blashfield (1984) traces the anti-diagnostic sentiment in American psychiatry back to Adolf Meyer rather than to Freud. Meyer was an influential psychiatrist who originally proposed, in today's terminology, what would be called a biopsychosocial model as opposed to purely psychogenic or biogenic model of pathology. Although originally a Kraepelinian, Meyer came to distrust the crudeness of Kraepelin's classification system, thinking that it drew the clinician's attention away from significant facts found in individual presentations. This partly followed from his Darwinian preference to think of pathology as an adaptation failure rather than as a disease. He was much more interested in the process of pathogenesis than in the disease that was produced. Later in his career he came to believe that psychiatrists need to treat patients and not diseases, and as a result made several disparaging comments about medical model-oriented diagnosis.

The biomedical materialists' suggestion that psychiatry dominated by psychoanalysis was hostile to systematic classification is simply untrue. Although Karl Menninger, the United States' most famous post World War II analyst, did not favor of the proliferation of entity-like diagnostic categories¹² (Menninger, Mayman & Pruyser 1963), Freud's own writings made explicit diagnostic distinctions, such as those between neuroses and psychoses, or between mourning and melancholia. He also described hysterical, paranoid, and obsessive-compulsive personality dynamics. A member of his inner circle, Karl Abraham (1927), discussed psychopathology as fixations at specific developmental stages (oral, anal, etc.). In recent years, George Vaillant (1977, 1986) has hierarchically ordered defense mechanisms from primitive to mature, suggesting that some defenses are more pathological (denial, acting out) and others are more adaptive (humor, sublimation). Furthermore, most axis II personality disorders were originally described in the psychoanalytic literature.

Two psychiatric classics, Shapiro's (1965) *Neurotic Styles* and MacKinnon and Michels' (1971) *The Psychiatric Interview in Clinical Practice* were psychologically-oriented diagnostic manuals which generations of clinicians consulted for suggestions about differential diagnosis and specific

12. Menninger's resistance to diagnosis resulted from how he framed the problem. Rather than treating people's problems, psychoanalysts were interested in helping them achieve insight. Problem-focused diagnosis was less important to them. The initial diagnostic question was only whether one was analyzable (neurotic) or not analyzable (psychotic).

treatment approaches for particular disorders. In addition, at the Menninger Clinic itself, Rappaport, Gill, and Schafer (1945, 1946) wrote a book called *Diagnostic Psychological Testing*, which was revised in 1968 under the editorship of Robert Holt. Although much criticized, it describes diagnostic criteria for schizophrenia, paranoid conditions, clinical depression, and neurotic syndromes such as anxiety, hysteria, and obsessive-compulsiveness.

Millon (1986) reports that the major psychoanalytic criticism of the DSM-III task force was its rejection of neurosis as a diagnostic category. The vehemence over this point may explain the American Psychoanalytic Association's refusal to contribute to a proposed axis for defense mechanisms during DSM-III's development (Klerman 1986). Since the psychoanalysts thought that anxiety underlies most disorders, they considered it a mistake to make "anxiety disorder" a separate syndromal category. Although the task force members convincingly argued that neurosis is a vague concept, and the value of treating panic as a problem on its own has been demonstrated, the psychoanalytic commitment to "neurosis" cannot be equated with a general antithesis between psychological approaches and systematic diagnosis.¹³

Psychoanalytic thinkers have traditionally been critical of the behavioral approach taken by the DSM-III task force. For them behavioral criteria constitute counting symptoms, a practice which can potentially lead clinicians to ignore underlying pathological processes. In the old DSM-II, analysts placed what would become diverse categories in the DSM-III into two categories. These were the categories of *neurosis* (hysteria, phobia, neurasthenia, depression, and hypochondriasis) because the underlying pathological process supposedly involved anxiety and the *functional psychoses* (schizophrenia, mania, paranoia, and psychotic depression) because the underlying pathological process supposedly involved a loss of contact with reality.

Although many behavioral and cognitive-behavioral psychologists disagree with the analysts on this point, the analysts' criticism of behavioral criteria does not distinguish them from the biomedical materialists, who express the same complaint about DSM-III. They differ from the analysts only by defining underlying pathological process biologically rather than psychodynamically.

13. Shorter (1997) claims that anxiety disorders were pushed by psychiatrists who had allegiances to drug companies, referring to them as "Upjohn disorders." He catalogues a long history of companies and physicians profiting from everyday misery. If true, this represents the darkest side of biomedical materialism. My guess is the truth is more complicated, and the "villainy" more ambiguous.

Their complaints about descriptive diagnosis are exemplified in Andreasen's (1984) claim that one limitation of DSM-III is its tendency to focus on (a) symptoms instead of biological causes and (b) disorders instead of diseases.

Contradicting Andreasen's claims regarding the unique relationship between DSM-III and the biological revolution, the DSM-III operationalist criteria were developed to avoid classification based on etiology; therefore, no preference for biological, social, or psychological causes were specified. As a matter of fact, Shorter (1997) points out that Kraepelin himself, who studied under Wundt, was generally more interested in psychology than biology. As he gained a greater appreciation of promising biological explanations, his goal as department chairperson was to develop a comprehensive department of psychiatry, not a department of biological psychiatry. No true Kraepelinian would reduce the problems of psychiatry to broken brains.

5. Behavioral evidence and the psyche

Complimenting the biomedical materialists' confusion between the historical facts of the DSM-III's development and the conceptual fact of what was developed, is the confusion evident in the psychoanalytic and humanistic therapists' mistrust of behavioral criteria. One of the values of the realism versus instrumentalism debates in the philosophy of mind, whatever their solution, is they can help clinicians see that psychological states such as beliefs and desires are unknowable apart from behavior.

To illustrate, one of the important problems of philosophical psychology is trying to understand what it means to claim that psychological states such as beliefs can be causes of behavior. The early functionalists in psychology such as James and Dewey did not think that the Humean model of causation could adequately describe the belief-behavior relationship. In the Humean model, in order for something to be a cause, the cause must be separate from the effect and it must precede the effect. James and Dewey pointed out that beliefs and desires tend to be put into action as they occur. Rather than being separate from behavior, they accompany it. We usually just behave rather than thinking about what we will do beforehand. For example, as I write this chapter, I don't think "I want a sip of Coke" and then take a sip of Coke, I usually just take a sip of coke without thinking about it at all. Only when there are delays or when something unexpected occurs can we say that beliefs and desires precede behavior.

Philosophers have pointed out several problems with claiming that psychological states can cause behavior, one of them being the circularity problem. For example, I can answer the question “why does this person slash their wrists, engage in impulsive sex or drug use, and change their hair style and wardrobe every few months” with the response “because they have borderline personality disorder.” Borderline personality disorder, however, is defined by symptoms such as suicidal behavior, impulsive acting out, and unstable identity. Circularity occurs when we use the disposition (belief, desire) as an explanation of behavior, and then define the disposition in terms of the behavior.

What I call the circularity problem, Dennett (1991) calls the *virtus dormitiva* problem. Dennett’s phrase was inspired by physicians at the turn of the century who claimed that the reason sleeping pills cause us to fall asleep is because they have a *virtus dormitiva*, or sleep-causing power. Another example of *virtus dormitiva* that Dennett gives is to say that *Cheryl Tiegs photographs so well because she is “photogenic.”* Dennett thinks that claiming we laugh because we are amused is another version of the same problem. Amusement is just an alternative description of laughter, not an internal cause. Explaining laughter with reference to amusement is therefore empty. Earlier in his career he referred to this as the homunculous problem, for example explaining intentional behavior such as working hard by inferring an intentional agent in the head such as the ego, creating explanations such as *Those who work hard do so because they have ego-strength.*

Dennett is correct to point out these errors, but is mistaken if he identifies this species of psychological explanation with the whole category of psychological explanation. Circular explanations are immature forms of dispositional explanation; errors we are prone to make. Once we explain a behavior with respect to an internal state (Joe yelled because he is angry), any attempt to explain that state (why is he angry) has to be more than a description (because he yelled).

In the ego-strength example, to say that someone works hard because they have ego-strength is to say that they work hard because they have the capacity to delay gratification. Although only a partial explanation, it is not circular. In the borderline personality example, if I say this person slashed their wrists last night and you ask “why” and I say because she is borderline, and you ask “why,” I have several options, but referring back to wrist slashing is not one of them. To answer your question I need to have some theory of the

underlying pathological process. That theory may involve disturbances in the structure of the self (Kernberg 1975; Masterson 1976), an inability to tolerate negative affect (Linehan 1993), or a host of biological processes including genetically programmed emotional instability (Muller 1992; Torgersen 1984), but I have answer your question with respect to that theory.

Dennett's choice of laughter was a good one for making his point because no one has developed a convincing biological or psychological explanation for laughter. Not knowing why we laugh, redescribing the action using words like "because I was amused" or "because I thought it was funny" sounds like a good explanation, but those states cannot be considered causes of laughter in the Humean sense. Normally laughing just is amusement. It is not, however, always amusement. For example, sometimes people laugh because they are embarrassed, not because they are amused. They may also laugh out of spite in order to hurt someone, or from fear of not going along with the crowd. Given the heterogeneity of laughter, claiming you laughed because you were amused is not as empty as Dennett thinks it is.

Vulnerability to the error of circular explanations by redescribing the effect (photographs well) as a casual force (photogenic) does not make belief explanations meaningless. The purpose of the attributing beliefs is to give internal consistency to otherwise disparate behaviors, which is something that behavior alone cannot do. One important function of beliefs and desires in our theories of the self is to integrate related behaviors. They make them intelligible as a group.

Because we don't have phenomenological access to beliefs, observing behaviors are necessary for figuring out what we believe. In Meehl's (1967) apt observation, the problem of our own mind parallels the problem of other minds.

For example, Daryl Bem's (1967, 1972) self-perception theory states that we become aware of ourselves by watching what we do. To illustrate, Albert knows that he believes that the first amendment is crucial for a just society because he gets angry when judges think that they have a right to make Christianity the official dogma of their courtroom, because he argues with colleagues who want to censor the Internet and what is in video stores, and he because disagrees with friends who think it is acceptable to prohibit expression if the expression is "wrong." In his twenties, Albert engaged in these behaviors without specifically labeling his political beliefs as something more distinct than "liberal." Once he discriminated a more fine-grained belief with

respect to the first amendment he began engaging in new behaviors, such as writing checks to civil libertarian organizations once a year. These isolated behaviors are the best evidence both Albert and we have for his beliefs, but the behaviors do not explain why they are related together, the beliefs do.

As suggested in Chapter 6, beliefs are outcomes of making distinctions within the framework of the self. In this section I have claimed that beliefs also provide us with some self-consistency. The beliefs explain the consistency of Albert's behaviors (why they all go together), but the consistency is not in the behavior, it is in Albert (explanation with reference to what is in the head). Bennett (1991) makes a similar point when he claims that intentional descriptions unify mechanistically diverse events. Beliefs and behavior are intimately tied together, sometimes indistinguishably so, but explaining behavior with respect to belief (self-consistency) is not circular or empty. Although behavior is the evidence for beliefs, other beliefs are also used to explain beliefs. For example, if we ask the question, "Why does Albert believe in the importance of the first amendment?," we need to refer to other beliefs more than we would refer to other behaviors, for example, beliefs about the importance of an open society and the moral use of power.

This last part is important. To say that we learn about our beliefs by observing our behaviors, suggests that beliefs may be summaries of behaviors. In psychology, this would make them *intervening variables*, meaning both that they have no surplus meaning above and beyond the behaviors they summarize and that they can be defined completely in terms of those behaviors. The claim that beliefs also have to be defined in terms of other beliefs suggests that they have some surplus meaning above and beyond behavior. This makes them *hypothetical constructs*, similar to things such as electrons, depression, and intelligence.

Especially because they can be used to predict as yet unobserved events, hypothetical constructs have more theoretical force than intervening variables. As Dennett (1987) claims, the value of a belief attributions is that they allow us to predict behavior. In my view, predictiveness refers to the extent to which consistency in behavior will be maintained in the future. As a matter of fact, in psychodynamic psychology, the person's most central beliefs and desires are inferred from those behaviors that are consistently and rigidly exhibited. This is called the repetition-compulsion.

Even as hypothetical constructs, beliefs are still largely defined in terms of behavior. No psychology, including psychoanalysis and humanistic psychol-

ogy can escape the primacy of behavioral evidence. This makes a thoughtful use of operationalization not only non-threatening, but necessary for any observation-based psychological theory. To illustrate, Gabbard (1994) provides an excellent example of behavioral criteria in distinguishing borderline personality disorder from dependent personality disorder. According to Gabbard, persons with borderline personality react to abandonment with rage and manipulation, while persons with dependent personality react to abandonment by becoming submissive and clingy. Even with traditional psychological concepts such as neurosis, clinicians need to have some kind of visible evidence that justifies applying the label. Operationalization as systematizing and clarifying evidence is a good thing.

In conclusion to this subsection, when the American Psychopathological Association, in 1959, invited the philosopher of science Hempel (1965) to speak about the taxonomy of mental disorders, he outlined an operationalist program remarkably similar to that endorsed by the Feighner group, except he held that it was not for him to

speculate on the direction that theoretical developments in this field may take and especially on whether the major theories will be couched in biophysiological or biochemical or rather in psychodynamic terms that lack an over-all physiological or physiochemical interpretation. Theoretical systems of either kind can satisfy the basic requirements for scientific theories. (p. 150).

Descriptive diagnosis, what is currently called “scientific diagnosis,” does not favor biomedical or psychological theories. Biomedical materialists, psychoanalysts, and humanistic therapists all need to modify their opinions about the natural relationship between “scientific diagnosis” and psychology. The biomedical materialists may have been the people to develop the DSM-III, but the DSM-III model cannot be used to establish the superiority of biomedical over psychological conceptualization.

6. DSM-III and DSM-IV Utilize First-Person Information

Materialism without Physicalism (Chapter 7) also indicates that operationalization in terms of third-person description should not be the only source of evidence in psychiatry. As a matter of fact, although the DSM-III, and its successors the DSM-III-R and the DSM-IV all attempt to focus on more observable and less abstract criteria, they are not as objective or physical as

some of their proponents claim. Many criteria for specific disorders are intersubjective, others are not. For example, in diagnosing a panic attack, the criteria *trembling* and *sweating* are intersubjectively observable. Other criteria are physical but require self-report such as *nausea*, *tingling sensations*, and *chest pains*. Such quasi-objective physical symptoms are not unknown in medical diagnosis. One could probably formulate behavioral indicators of nausea or chest pains, but no one has seen a need to do so in order to make them “scientific.”

Panic attack criteria such as *depersonalization* and *derealization*, however, not only require self report, but the diagnostician may have to specifically inquire for them and even educate the patient/client about the potential existence of the criteria before they can be reported. The criteria for depersonalization are themselves non-behavioral, e.g., *feeling like one is in a dream*. Knowing what a dream is like is first-person information. We know what it is like because we have been in that state. Understanding these criteria requires a subjective sense of what the symptom feels like. Had psychiatrists limited themselves to purely objective data, depersonalization or derealization would not have been allowed as criteria.

Other examples of criteria that require subjective understanding to be defined would be *unstable sense of self* and *chronic feelings of emptiness* for someone with borderline personality disorder, or *feeling worthless* and *excessive guilt* for major depressive disorder. Like any psychological state, these clearly have public accompaniments. Because verbal reports are intersubjective, reports of depersonalization and feeling worthless are intersubjective. The verbal reports, however, are no more the same as the depersonalization experience than the words “I feel sick” are the same as nausea.

Materialism without physicalism is the view that subjective states are brain/body states and that although their mechanism can be explained physically, to the extent that they are information bearing states, they cannot be replaced by third-person description. Part of being a biological creature with a central nervous system is having a phenomenology. Only those who make the error of radical anti-anthropomorphism (Chapter 5) fail to accept that reports and inferences about subjectivity are central to biology, particularly primate biology. The subjective as belonging to both biology and psychology is natural in a levels of analysis analysis (Chapter 6), where the biological and psychological levels adjoin each other.

Manfred Spitzer (1994) claims that psychiatrists have failed to attend to

potentially important first-person information. He shows how hallucinations have been so vaguely defined that psychiatrists don't know whether complexity, modality, length, frequency, constancy over time, concomitant affect, relevance to overt behavior, and conviction of independent existence can tell us something about differential diagnosis or not. The same is true of delusions. He thinks that biological psychiatry's conservative commitment to third-person information is probably interfering with the discovery of useful diagnostic distinctions.

The importance of the subjective as a category of evidence is also a reason for classifying the natural sciences, the biological sciences, and the social sciences into separate categories. Dennett (1991) and Mayr (1988) both claim that scholars are beginning to see that the biological and the natural sciences are different kinds of sciences. Dennett thinks that the methods of biology are unique in that they use reverse engineering to solve problems. By reverse engineering he means we are supposed to develop models of how "biological machines" work. Mayr thinks what that separates biology from physics is the importance of genetic "programs" in living organisms. Echoing the same theme, Dawkins (1986) claims that the "stuff" of biology comprises complicated things that give the appearance of having been designed for a purpose. In my view, the "engineering," "program," and "apparent design," analyses of complicated organisms need to be supplemented with an acknowledgment of the *material facts of subjectivity*.

The DSM-IV specifically recognizes the importance of the subjective as a category of biological evidence. For example, it defines mental disorder as "a clinically significant behavioral or psychological syndrome or pattern that occurs in an individual and is associated with present *distress* or disability or with a significantly increased risk of *suffering*, death, *pain*, disability, or an important loss of freedom" (p. xxi) (*italics mine*). Although there was opposition to the continued use of the term "mental disorder" by some members of the DSM-IV task force, even if they had changed it to "psychiatric disorder" or "psychopathology," no one seems to have argued that phenomenological/subjective criteria be eliminated. The DSM-IV also recognizes subjective criteria in its symptom-sign distinction:

sign An objective manifestation of a pathological condition. Signs are observed by the examiner rather than reported by the individual (p.770).

symptom A subjective manifestation of a pathological condition. Symptoms

are reported by the affected individual rather than observed by the examiner (p. 771).

syndrome A grouping a signs and symptoms, based on their frequent co-occurrence that may suggest a common underlying pathogenesis, course, familial pattern or treatment selection. (p.771).

The DSM-IV is obviously not the final word on psychiatry. It is also possible that the more traditional forces in psychiatry (tender-minded psychiatry) contaminated the DSM-III and its successors by not letting the tough-minded neo-Kraepelinians be as rigorous as they wanted to be. The tough-minded biomedical materialists for political reasons, may have had to accept subjective criteria, or the rest of the field would have rejected DSM-III.

If we examine the Feighner criteria which are (a) more behavioral than DSM-III and (b) the undisputed intellectual property of the neo-Kraepelinians, however, this hypothesis loses plausibility. For example, their definition of affective disorder includes criteria such as *discouraged*, *feelings of self-reproach or guilt*, and *wishing to be dead*. Under anxiety neurosis, we find listed criteria such as *feelings of impending doom*; under obsessive compulsive neurosis, *a sense of subjective compulsion*, and *a desire to resist the event which is experienced as ego-alien*. Since these concepts refer to subjective states, the neo-Kraepelinians' own criteria do not meet their stated standards of objectivity.

Rather than claiming that the early neo-Kraepelinians were too much a part of the old mentalistic psychiatry to make a clean break, the nature of biological science itself makes it likely that psychiatry's concern with patient reactions is part of its uneliminatable psychological core, having both diagnostic and treatment implications. In his attempt to argue that psychiatry should be conceptualized as a branch of medicine, Guze (1992) even suggests that a focus on the patient's experience is a unique and important contribution made by psychiatry to medical science, although he tries to minimize the impact of this distinction by claiming that other physicians are also beginning to appreciate these factors.

Ironically, adopting an exclusive focus on biomedical factors has contributed to a weakening of psychiatry's influence because relationships with patients are reduced to diagnostic evaluations and prescription services. For example, one of my colleagues from graduate school has a private practice focusing on the treatment of eating disorders. She spent her first few post-doctoral years working in a hospital. When a group of psychiatrists decided to

leave the hospital to start a private practice, they invited her to come along, but not as a partner. She asked for partnership, but they refused. Mulling over her prospects, she decided to go into private practice for herself. After making sure the hospital administrator would still be willing to work with her, she returned to the psychiatrists and informed them that she was going into private practice, and she expected them to continue referring clients to her. If they did not, she would refer all her clients to psychiatrists who were more willing to work with her. Since she was therapist, the one who had developed relationships with the patients and their families, they were more likely to stay with her than with whoever gave them their pills. One prescription is as good as another. Realizing that they could lose 50 or 60 patients immediately, the psychiatrists agreed to work with my colleague.

7. Psychiatry's psychological core

Psychiatry's psychological core manifests in three ways. The first, *management issues*, involve treatment. The second two, *secondary reactions* and *pathogenic reactions*, involve diagnosis.

Management issues

Management issues involve deportment toward patients, specifically, a concern about their reactions. Rules about how to manage persons with particular diagnoses have a long history in psychiatry. For example, students are taught to not challenge persons who are actively paranoid in an initial meeting. Students are also taught to not ask paranoid patients to trust them. Neither agreeing nor disagreeing with the person's paranoid ideation, and empathizing with them rather than telling them they can trust you both help establish a working relationship. Knowledge of the likely reactions of the person who is suicidal, the dependent conflict-avoider, and the perpetrator of child abuse are important psychiatric management skills that are not a part of common sense. Engel's (1977) example of the man who had a heart attack in the hospital emergency room and Gabbard's (1992) example of the man with obsessive-compulsive disorder who refused to take medication both illustrate management issues. These kinds of practical problems require dealing with individuals, often in psychologically complex ways.

Even the most committed biomedical materialist has to be concerned with managing reactions, particularly with respect to getting the information needed for a diagnosis and for the problem of medication compliance. With respect to the latter, Strauss (1986) observes that instead of the passive term “compliance,” the term “collaboration” more accurately describes the problem facing psychiatrists, which includes getting patients to take an active role in their own treatment. The idea that patients are more than just passive entities tends to be ignored by the disease models used in the rest of medicine, but not in psychiatry, and especially not in clinical and counseling psychology where the term “client” is preferred to “patient.”

A good example of management as treatment was once described to me by a physician who worked as a child therapist. One of the ways she worked with her patients was to let them express themselves in play. Each child had a box of their own toys that they were allowed to play with during session. At the end of the session the toys were put back in the box, which was then placed on a shelf. Sometimes children would ask to play with the toys in other children’s boxes. The physician in question systematically refused these requests, telling the children that she does not let others play with their toys when they are not here, and they can’t play with the other children’s toys either. She created a situation that helped initiate the child’s reaction and took advantage of the opportunity to manage that reaction. Her intervention created a sense of trust, specialness, and stable boundaries, an important part of child therapy.

Secondary reactions

Secondary reactions involve the person’s reaction to a more primary disorder. According to Guze (1992), the early neo-Kraepelinians made distinctions between primary and secondary disorders. They believed that depression as a *response* to either having schizophrenia or a panic disorder is different than depression as a primary disorder. Guze notes that this distinction still makes sense because depression in people with preexisting anxiety disorders or schizophrenic disorders is not related to increased frequency of depression in first-degree relatives. Reactive depressions are legitimate depressions and need to be treated, but they also have to be understood in a psychological context. They are consequences of another disorder.

Even twenty years into the DSM revolution, partialing out a primary

disorder (symptoms and signs) from reactions to the disorder is an important diagnostic task. For example, Clark and Watson (1991) indicate that there is a 50% comorbidity between anxiety and depression. The appendix for the DSM-IV even has a criteria set for a new disorder, called mixed anxiety-depressive disorder. From one perspective, anxiety and depression share a predominance of what Tellegen (1985) calls negative affect, which may be an underlying predisposition for both disorders. If so, we should treat the negative affect. From another perspective, people with an anxiety disorder as the primary diagnosis are more likely to become depressed, but people with depression as primary diagnosis are less likely to develop an anxiety disorder. When depression is a reaction to the anxiety disorder, the goal should be to treat the anxiety.

In 1952's DSM-I, all "Psychoneurotic" and "Psychotic" disorders such as depression, dissociation, anxiety, mania, paranoia, and schizophrenia were called "reactions." Psychoneurotic disorders were supposed to be reactions to anxiety. The manual failed to specify exactly what persons with psychotic disorders were reacting to. All disorders could further be qualified by secondary reactions, which were either psychotic, neurotic, or behavioral (meaning personality disordered). These secondary reactions constituted symptoms superimposed on a primary disorder. For example, someone could be diagnosed with a neurotic depressive reaction, upon which a secondary psychotic reaction had been superimposed. If psychotic, neurotic, or behavioral qualifiers were used with respect to organic disorders, clinicians were urged to consider these secondary reactions as precipitated by the biological deficit, and to focus their diagnosis and treatment efforts on the biological problem. In an ironic sense, biomedical psychiatrists have not revolutionized psychiatry. They have only changed psychiatry's ideas about what counts as an organic disorder. Once a disorder has a "biological etiology," the same recommendations apply, then and now.

In 1968's DSM-II, the term reaction was eliminated from the title of most disorders. The committee wanted to leave the question of cause more open to alternative interpretations. They had limited success. For example, the affective psychoses were identified as *not* having a precipitating life experience. The neuroses were still considered reactions, defined with respect to underlying anxiety. So depressive neurosis was essentially a reactive depression without a psychotic process. The main exception to the elimination of the title "reaction" was in the functional psychoses section, where in the "Other

Psychoses” category the manual listed various “reactive psychoses.”

In 1980’s DSM-III, the category of Neurotic Disorders was eliminated. The committee claimed that there was too much confusion between *neurotic disorders* indicating a level of severity and *neurotic process*, indicating a specific etiology involving unconscious conflict. Instead, anxiety, depersonalization, and affective disorders became categories themselves. Again, etiology was not as absent as the manual’s introduction indicates. For example, depression now referred to a state that was not due to some other mental or physical disorder, suggesting that it just happens. This clearly conforms to the biological time bomb model. Confusingly, depressive reactions to other illnesses were still coded on axis I. Although no longer a specific category of disorder, the term “neurosis” was used in parentheses to specify the severity of certain disorders. These parenthesized titles included depressive neurosis, anxiety neurosis, phobic neurosis, obsessive-compulsive neurosis, hysterical neurosis, hypochondriacal neurosis, and depersonalization neurosis.

The DSM-III-R of 1987 retained the neurosis categories in parentheses, but by this time clinicians had gotten use to the new names. The introduction to the DSM-III-R barely mentions the term neurosis, except to say that there is no diagnostic class of “neurosis.” The definitions for neurotic disorder and neurotic process in the DSM-III glossary were also eliminated from the glossary in the DSM-III-R. In 1994’s DSM-IV, any hint of a reaction has been minimized in the description of the disorder, and the term neurosis has been almost completely removed from the manual. Having been fully eliminated, there is no listing for neurosis in the index.

Even though they have successfully eliminated the category of neurosis, biomedical psychiatry has not been able to completely eliminate the category of subjective reactions. The reason reactions have not been written out of the manual is because they are still a good idea. In the DSM-IV, Adjustment Disorders cover what the DSM-I and DSM-II called “transient situational disturbances/stress,” and Brief Psychotic Disorder with Marked Stressors covers what was called “reactive psychosis.” The idea behind Adjustment Disorders is that they are acute reactions to specific events that will probably remit by themselves. Unfortunately, the category of chronic adjustment disorder contradicts the idea of a temporary reaction. Many people formerly diagnosed with a depressive neurosis or an anxiety neurosis would probably be given a chronic adjustment disorder in today’s nomenclature.

Related to this issue is a thoughtful essay by the biomedical psychiatrist

Strauss (1994), who proposes that the time course of chronic psychiatric disorders are poorly understood. He notes that many patients with schizophrenia experience plateaus in recovery where things just seem to stop. He suggests that some of them go through a self-regulatory phase, where they are accumulating resiliency in the form of self-esteem and increased abilities to function socially. The growth is not obvious, even though it is partly conscious, and afterward people have some kind of awareness that they needed to do whatever they did. He suggests that control theory and a focus on self-monitoring, self regulation, interpretation (meaning attribution), and action need to be integrated into the understanding of syndromes. In other words, we need to consider reactions. He also believes that capacities such as goal-directness and will, which can effect the course of a syndrome, have been written out of the diagnostic manuals because they are difficult to operationalize. His point is that his biomedical colleagues are ignoring certain variables because they are ideologically inconvenient, not because they are unimportant.

Pathogenic reactions

Pathogenic reactions are more complicated, but also a logical part of the biomedical view of psychiatric disorders. These are reactions that cannot be separated from the development of the disorder as easily as secondary reactions can. For example, the neo-Kraepelinians considered syndromes to be endpoints of cascade effects where one problem (biological or psychological) leads to predictable coping sequences. The actual biological deficit may only be the starting point of what becomes the syndrome treated by psychiatry. In schizophrenia, the initiating physical deficit may functionally be unlike what we think of as "schizophrenia." To use an imaginary example, the initiating problem may be an attention deficit problem that prevents the affected person from filtering out irrelevant internal stimuli. That deficit creates problems in relating to others, which themselves lead to predictable reactions/adaptations to these problems. The end point of a long chain of adaptations may be something like one of the schizophrenias. The development of even biologically-loaded disorders can therefore include psychological variables in terms of reactions, counter-reactions, etc.

With respect to the cascade model of syndrome pathogenesis, Guze (1992) points out that our ability to test out psychosocial influences on behavior will be increased once we gain a more targeted understanding of the

genetic inputs to the major mental illnesses. This is because whatever the genetic inputs may be, they are probably only initiate the final syndromes. Teasing out primary functional deficits and the eventual syndrome, which involves the person's reactions will contribute to the strengthening of both psychological and biological explanation.

Pathogenic reactions not only involve the development of the identified syndrome after biological initiation, they also refer to the fact that reactions themselves can contribute to the worsening of the syndrome even after it has fully developed. For example, whatever causes depression, once developed, the disorder affects a person's life, sometimes leading to further problems and reactions, creating more symptoms. Someone may get depressed because he or she was demoted at work. Their depression leads them to perform poorly and results in their being fired. Being fired causes family tensions, and possible enhancement of the dysphoria. People's negative reaction to the ongoing depression eventually leads to consistent interpersonal rejection. All this feeds back into the depression, which from a certain perspective is now its own cause.

Alternatively, a biologically based depressive-personality style can create problems establishing a successful social life, with long term disappointment leading to a pessimistic view of the world and a concomitant worsening of the depression (biologically defined). To the extent that the depressive character state is functionally autonomous from the constitutional initiator, it becomes a barrier to effective pharmacological treatment. With circular causality where the conditions which initiated the depression can be influenced by the depression's consequences, the line between biology and psychology is extremely difficult to draw.

Related to pathogenic reactions, the psychiatric literature makes a distinction between personality structure as a *vulnerability for a disorder*, as an *interpersonal precipitate*, or as *pathoplastic*.

Vulnerabilities are not causes, they are occasions. For example, being in New Orleans was not the cause of Hugo's being mugged, but it was the occasion for his being mugged. Paul Meehl's (1962) idea of schizotypal personality as the phenotypic expression of a genetic vulnerability to disorganized schizophrenia is the best example of this view. Meehl-influenced psychologists think that the bizarreness and eccentricity of schizotypes are actually milder disorders on the schizophrenic spectrum. Schizotypal personality is the psychological context in which disorganized schizophrenia develops.

A looser kind of etiology takes the form of *interpersonal precipitates*, a corollary to my idea of reactions contributing to the worsening of syndromes. For example, the impulsivity and identity problems of the person with borderline personality precipitate the unstable relationships and crises they constantly experience. The suspiciousness of the paranoid personality, especially with respect to partner infidelity, leads to a breakdown of trust in the relationship. Often, their once-committed partner is no longer on their side and seriously thinks about leaving. Social psychologists refer to this as the self-fulfilling prophecy phenomenon. It increases paranoia.

The notion of personality as *pathoplastic* refers to the fact that a preexisting personality style can mold the expression of a clinical syndrome. The DSM-I referred to this as a premorbid personality factor. Someone with a dependent personality style who develops an anxiety disorder may look clingy and helpless. They will isolate themselves and expect others to do things for them. Someone who is more independent and extroverted can have the same anxiety disorder as far as the DSM is concerned, but they will appear more frantic, excitable, and frustrated. They will also engage in approach-avoidance behavior try to make contact as usual, but fail. As a result they become even more frustrated and anxious. Culture can also exert a pathoplastic influence, demonstrated in Wender and Klein's (1982) claim that guilt as a symptom of depression is limited to people raised in Western Judeo-Christian society since the sixteenth century. In alternative cultural contexts, guilt is rarely part of the presentation of a depressive disorder.

Not all biomedical materialists have grasped the practice-based justification for understanding the role that subjective reactions play in medical science. Paul Churchland's idea that an accurate neuroscientific description of cognitive activity will allow us to eliminate folk psychology in the lab, the clinic, and even in the marketplace finds a parallel in certain biomedical materialist research programs. For example, when neuroleptic medication was first applied to psychotic patients in the fifties, these patients demonstrated a drastic decrease in florid symptomatology such as hallucinations and delusions (positive symptoms). Other symptoms such as withdrawal, or affective blunting and bizarre behavior were left un-altered (negative symptoms). Contradicting medical psychiatry's claims to having produced miraculous remissions, some thinkers began to point out that removing florid symptomatology was only a partial solution (Bellak 1979).

Attempting to explain this problem, biomedical psychiatrists proposed

that positive symptoms and negative symptoms constitute two distinct illnesses, identified as type I and type II schizophrenia, or non-deficit and deficit schizophrenia. (Carpenter, Heinrichs, & Wagman 1988; Crow 1980, 1985). The two diseases were considered dichotomous, with most patients having one type or the other. The basic reasoning went as follows: because neuroleptic medication seemed to affect only certain symptoms, those symptoms left intact must represent another disease. My own introduction to the positive-negative model in a physiological psychology class took the form of the professor making a distinction between positive symptom schizophrenia and negative symptom schizophrenia as two different illnesses.

There was anatomical evidence (now discredited) to support this claim; and it was hypothesized that positive symptoms were related to a neurochemical disorder in the dopaminergic system, and negative symptoms were related to diffuse brain deterioration as measured by ventricular enlargement. Biomedical psychiatrists also proposed that more chronic schizophrenic patients can be distinguished by both a significant prodromal period and negative symptoms, whereas acute schizophrenic patients have fewer negative symptoms.

Categorizing symptoms as either negative or positive became a major task, with factor analytic procedures being used to extract symptom groupings. Based on an early factor analysis, Andreasen and Olsen (1982) concluded that there may actually be three types of schizophrenia: positive, negative, and mixed, because a large number of patients seemed to have both positive and negative symptoms.

Psychologically-oriented thinkers were more inclined to think about behavioral withdrawal as either the consequences of other problems or part of the premorbid personality of persons who have more chronic forms of schizophrenia. Either hypothesis naturally occurs to someone working from a theory that takes reactions into account. Supporting the more reaction-based hypotheses, Tandon and Greden (1991) observe that most researchers have found an orthogonal relationship (uncorrelated) between positive and negative symptoms instead of a bipolar relationship. De Leon, Simpson, and Peralta (1992) also claimed that negative symptoms have also been shown to respond to medication, undermining the putative justification for the distinction in the first place.

Although the idea of taking positive symptoms and negative symptoms to be two discrete and mutually exclusive kinds of schizophrenias seems silly in retrospect, it represents a concrete attempt to use only objective biological

concepts in the definition and treatment of disorders. Interactions between biological initiators and personal reactions, however, have been re-inserted back into the thinking of even the most committed biomedical materialists. For example, Andreasen, Flaum, Swayze et. al. (1990) claim that the underlying pathology in schizophrenia is structural (e.g., ventricular enlargement), and suggest that some phenomenology might be related to biopathological processes (e.g., negative symptoms equal frontal lobe abnormalities) and some phenomenology might be reactions to other symptoms (e.g., isolation equals a response to hallucinations).

Obviously, the old DSMs were mistaken. The psychotic versus neurotic distinction is about severity rather than etiology. Previous generations of psychiatrists failed to see that biological predispositions could play a casual role in the development of both psychotic and neurotic depressions. To some extent these mistakes followed from Adolf Meyer's (a) simplification (and criticism) of biological causes as necessary and sufficient time bombs that go off in the body and (b) definition of reactions as etiological factors in neurotic disorders that have no biological basis.

The mid-1960s notion of a mutually exclusive relationship between biology and subjective reactions naturally created hostility toward subjective reactions on the part of biomedical materialists. In the 21st century however, if we can accept the fact that all psychiatric disorders have biological substrates, we can also accept the fact that all psychiatric disorders involve subjective reactions to a greater or lesser extent (i.e., accept that the brain is an open system).

In an attempt to be comprehensive, the post-1980 DSMs have categorized problems on five dimensions or axes.

Axis I: Clinical Scales and V-codes.

Axis II: Personality Disorders, Mental Retardation, Personality Traits & Defense Mechanisms.

Axis III: General Medical Conditions.

Axis IV: Psychosocial and Environmental Problems.

Axis V: Global Assessment of Functioning.

Although "pathogenic reactions" and "secondary reactions" are what axis IV (Psychosocial and Environmental Problems) was originally supposed to alert the clinician to, it has never been explicitly conceptualized in that way. For example, although the manual states that these "events" can initiate problems,

exacerbate them, or be the consequence of pre-existing problems, in practice, clinicians just list isolated stressors rather than stating how they may influence the disorder in question.

The solution to the problem of ignoring pathogenesis is not to reinsert the old specifiers, but textbooks and training programs at least need to discuss the potential role of subjective reactions in the development of syndromes, especially since their role is marginalized in the current taxonomy. They are an important part of pathogenesis, and will continue to be so once contributing biological initiators have been isolated. Although recognizing the advantages of both operationalism and atheoretical description understandably leads professionals to resist a *clinical formulation* axis, where the professional speculates about how the first four axes combine to create the presenting problem, in theory, this should be done if professionals are using the multi-axial format to its fullest advantage. The DSM-III rejected any attempt to specify development in order to avoid etiological assumptions, but also it recommends that information from all axes be used in treatment planning. The disadvantage of not asking professionals to speculate on pathogenesis is that when they don't do it, unquestioned assumptions about pathogenesis are rarely on the table for evaluation.

8. Conclusion

Diagnostic practices based on actual experiences with the complex problems of real patients indicates that even biomedical psychiatry is not as purely objective and anti-anthropomorphic as some of its proponents claim. First-person awareness is an important part of the evidence in medical diagnosis. Nor are psychological states incompatible with operationalized criteria. They are unknowable apart from behavioral indicators.

Although biomedical materialists prefer to reformulate all their professional problems so that they can be described on the biological level of analysis, the practical problems of psychiatry require them to consider both the psychological and biological levels of analysis. Rather than contradicting each other, these two levels of analysis can be seen to productively cross-fertilize each other. This cross fertilization contributes to a richer understanding of both biology and psychology, and to a more comprehensive view of psychiatric disorders.

As a discipline lying in between psychology and biology, psychiatry should be less concerned with passively conforming to the rest of medicine and more assertive about what it has to teach other medical specialties. Psychiatry's sophisticated understanding of its patients is based on the active-mind model, rather than the classical empiricist's passive-mind model. The active mind model focuses psychiatric practitioners on the extent to which patient's reactions influence the course of their disorders. In many ways, the profession of medicine would be better if it was more in conformity with psychiatry.

CHAPTER 9

Evolution, Adaptation, and Psychiatry

1. Introduction

For most of this century ethologists have distinguished between evolutionary causes of behavior and physiological causes of behavior. Evolutionary causes of behavior involve describing how the behavior in question aids adaptation, i.e., describing the behavior's function. "*Running away*" and "*knowing the difference between friends and enemies*" are functional descriptions of behavior. The physiological mechanisms underlying these behaviors exist because of events best described at the functional or adaptive level. As noted in Chapter 5, the difference in scope between evolutionary explanations and physiological explanations is what led Skinner to claim that the behavioral level of analysis can do considerable explanatory work on its own. The evolutionary biologist Ernst Mayr (1988) also distinguishes between evolutionary explanations and physiological explanations, noting that both molar (evolutionary) and micro (physiological) analyses are needed to fully understand any biological phenomenon.

In this chapter I argue that biological psychiatry has to be concerned with the science of evolution as well as the science of physiology. The evaluative concepts *normal*, *pathological*, and *optimal* gain their meaning in the broad context of evolution. According to evolutionary theory, adaptation is a fluid process. It is not a fixed trait existing in organisms. Because understanding adaptation requires considering the interaction between traits and conditions, no behavior described as adaptive or maladaptive can be reduced without remainder to the physiological and genetic levels of analysis. I show that a more molar level of analysis is necessary for even identifying a particular pattern of behavior as "disordered." It is also important for developing a more accurate understanding of brain functioning.

Contrary to Paul Churchland's (1994) claim that the victims of mental illness are victims of sheer chemical circumstances whose origins are more metabolic than social or psychological, going outside the "chemical" levels of analysis is a precondition for there even being any psychiatric *disorders*.

2. Adaptation and Dysfunction Do Not "Belong" to the Physiological and Genetic Levels of Analysis

The concept of adaptation. According to evolutionary biologists, adaptation is a contingent and pragmatic phenomenon. Rather than there being inherent, monadic traits that define adaptativeness, adaptiveness is defined as whatever confers a competitive advantage. As the local ecology changes, what counts as adaptive changes, so that traits adaptive in some situations can be maladaptive in other situations.

For example, Ayala (1996) points out that although the sickle cell trait in heterozygous form produces a biological disadvantage in contemporary American society, it is quite beneficial in environments where malaria is endemic. Those who are heterozygous for the sickle-cell trait are protected against the most lethal form of malaria.

In the same spirit, Wakefield (1993) notes that being in a psychological state of hyperalertness through believing that the mafia is trying to kill you is adaptive if you are a government informant and the mafia actually is trying to kill you. That same psychological state is maladaptive if you are delusional, and the mafia is not trying to kill you. These kinds of states have what philosophers call narrow content. Their adaptiveness can't be evaluated independent of external conditions.

The aggressive sensation-seeker who is genuine hero in war may be an equally genuine villain as a peace-time father and bread winner. Being an obsessive workaholic may make someone a poor spouse but a great civil rights lawyer. In the anchors framework, dispositions (aggressiveness) interact with external demands (kill the enemy, raise the child) and that interaction is *evaluatively* placed somewhere on the continuum from adaptive to maladaptive.

The pragmatics of natural selection also require that there be very little necessity as to where the psychiatric states defined in diagnostic manuals lie on the continuum of adaptiveness. Although in our world they tend to be robustly maladaptive, one could imagine possible environments where the brain states

underlying generalized anxiety disorder, bipolar disorder, or psychopathy have adaptive virtues. What makes them maladaptive are the consequences they have at the behavioral level of analysis. Understanding adaptation therefore requires a molar level analysis that physiology and anatomy alone cannot subsume.

Adaptation in clinical psychology and psychiatry As might be expected, the importance of adaptation or “functionality” cuts across various schisms in contemporary clinical psychology and psychiatry. Everyone involved agrees on the importance of considering adaptiveness.

One example of the universal agreement about the importance of adaptiveness can be seen in the debate between proponents of categorical versus dimensional models of personality pathology. Those who support categorical models such as Millon (1994), Gunderson (1984, 1992), Cantor and Genero, (1986) and Gorenstein (1992) believe in internally consistent pathological states, such as psychopathy or borderline personality. They individuate these states with respect to a shared etiology, time course, family history, biological markers, and treatment response. They also think that the difference between normal and abnormal is a qualitative difference, so the psychopathic personality has a different personality *structure* than the “normal” personality.

Those who support dimensional models such as Costa and McCrae (1990), Trull (1992), and Widiger and Francis (1994) hold that a finite number of dimensions such as extroversion and neuroticism underlie everyone’s personality. They think that a personality disorder is best described as a unique combination of extreme positions on the various dimensions of personality. They also think that the difference between normal and abnormal is a quantitative difference, with more or less of the same fundamental traits being what is important.

Each group still defines psychopathology in terms of *distress* and *disability*. With respect to disability, either the category “borderline personality disorder” is maladaptive, or the person’s idiosyncratic combination of positions on the fundamental dimensions of personality creates problems in adaptation (extreme scores themselves are not pathological). Those favoring categorical models want to assess symptoms, whereas those favoring dimensional models are willing to assess normal personality. Widiger (1994) even states that, rather than the presence or absence of a certain number symptoms and traits, the primary criteria for a personality disorder should be maladaptiveness itself. For example, one could meet criteria for psychopathic personality and function

fairly well or not meet enough DSM criteria to be diagnosed with psychopathy, but still have problems in adaptation related to psychopathic traits. In Widiger's model, a mental health professional would assess personality in order to better understand already identified problems in adaptation.

Biological psychiatrists such as Wender and Klein (1982) and Andreasen (1984) also use adaptiveness to define why a disorder is a disorder. Wender and Klein claim that moods regulate our dispositions to act. Those with enduring mood disorders are continually predisposed to act in maladaptive ways. Andreasen claims that in evaluating mental illness in general, one of the most important questions to ask is how much it affects the afflicted person's ability to work, have a normal family and social life, to think clearly, and to have well-tuned emotional responses.

Paralleling my claim that determining maladaptiveness requires an evolutionary-molar (external) evaluation, Gorenstein (1992) claims that the medical model involves both a search for the causes of disorders, which entails a biological etiology, and an attempt to decide what constitutes a disorder, which requires a social evaluation. For example, the reason why mania is considered an illness is because persons with mania are socially dysfunctional. If their functioning was not maladaptive, we would not view mania as an illness. Gorenstein further claims that establishing a biological basis for schizophrenia is not a sufficient reason for claiming that schizophrenia is a disease any more than establishing a biological basis for extroversion is sufficient reason for claiming that extroversion is a disease. We need to demonstrate maladaptiveness as well. There is therefore more to psychiatry than internal physiological and genetic facts.

The biomedical materialists' focus on physiological and genetic factors as the only scientifically important factors is consistent with the wispy molarism of Paul Churchland that I critiqued in Chapter 6. It ignores *the ecology of neuroscience*. What happens in the head and in the body is important, but we can't evaluate those processes as good or bad without looking at the molar level concept of adaption, which involves organism-world interactions. As Chapter 6 indicates, the most appropriate level of analysis for describing internal-external interactions is the psychological level analysis. As long as psychiatrists define psychiatric disorders with respect to maladaptiveness, the science of psychiatry has to consider more than physiology and genetics. As was the case for Paul Churchland, extra-physiological factors, i.e., psychological factors, are more than mere elaborations.

Robert Spitzer, the primary author of the DSM-III model of psychiatric illness claimed that mental disorders are “medical disorders whose manifestations are primarily signs or symptoms of a psychological (behavioral) nature, or if physical, can be understood only using psychological concepts” (Spitzer & Endicott 1978, p. 18). Any psychiatry wanting to respect both physiology and evolution would be well-advised to take a second look at this carefully-considered definition, or one similar to it.

3. Bottom-up Neuroscience and Psychoanatomy Versus the Internal Ecology Model

Guze (1992) claims that evolutionary analyses are interesting, but often too vague to be of any use to medicine, unlike analyses in terms of physiology and anatomy. Whatever the evolutionary reason for a particular state of affairs, the real job of the psychiatrist is to understand the internal mechanisms responsible for the behavior in question. He thinks that psychiatrists need to be concerned primarily with treatment and prevention, and only secondarily with historical questions about how certain patterns evolved.

In the view I am proposing though, this bottom-up strategy is unworkable. For example, it doesn't give us any criteria for knowing why someone is a patient or not a patient. Abroms (1983) writes about helping people become “biologically normal,” but psychiatrists need some functional reason to believe that patients are sick before they seek to “normalize” their biology. Although Abroms suggests that having delusions, being depressed, experiencing panic attacks, and wanting to commit suicide are biologically abnormal, to label them abnormal we have to (a) consider the precipitating event (if there is one), (b) have some theory about what constitutes an appropriate coping response, and (c) have some idea about the amount of subjective pain which is typical of the event in question. For example, panic and depression are normal states under certain conditions, such as severe trauma or the death of a loved one.

Contra Guze, treatment success is functionally defined. If the functional deficits disappear, we consider the treatment successful. For example: *He is out of bed and back to work; She is not beating her children; He is not cutting himself; She is able to achieve orgasm.* Even if some kind of statistically defined neurological abnormality persists, as long as the functional sequelae of the brain state have minimal interference, we will be satisfied. Interestingly,

referring to the biomedical materialist's favorite exemplar, Gorenstein (1992) points out that psychiatrists know about the etiology of general paresis (the syphilitic spirochete) and they know about the treatment (antibiotics), but they still have a limited understanding of the actual physical mechanisms that produce the maladaptive psychological symptoms. Since we can eliminate the symptoms, there is no pressing need to understand their physiological mechanisms.

Wexler (1992a) holds that, because the same syndrome can be caused by many different biological states, using syndromes (identified by symptoms and signs) as independent variables is a poor strategy for isolating underlying causes. He proposes avoiding the problem of within-category biological heterogeneity by rejecting diagnostic categories as independent variables, and focusing directly on biological markers. Wexler proposes that we should make distinctions between patients by looking only at objective physiological markers such as REM-latency. Bottom-up proposals such as this one claim that we have to understand the brain on its own terms (i.e., inherently), not on terms dictated by an inadequate descriptive nosology which has been defined according to functional observations instead of brain states.

Although Wexler proposes studying persons with depression and schizophrenia, he provides no physiological or genetic or anatomical argument for using these two categories. One of his suggested "objective" variables is dexamethasone suppression of cortisol (a hypothesized marker for depression), but using the DST in the way Wexler recommends, in addition to studying depression and schizophrenia, we should include persons experiencing dementia, anorexia nervosa, bulimia, alcohol withdrawal, mania, and schizoaffective disorder (Braddock 1986; Kandel 1991a).

The biological marker approach also contradicts a basic assumption of the medical model, specifically that syndromes are the result of some underlying pathological process which manifests in signs and symptoms. If we begin with the category "depression," we can identify a group of depressed persons who suppress and a group who don't suppress, and then seek other differences between these groups. But beginning with dexamethasone suppression is like beginning with a fever. There is no reason to assume that the biological marker in different persons represents the same pathological process. For example, we wouldn't argue that lung cancer, gas poisoning, smoke inhalation, a cold, bronchitis, and viral pneumonia all involve coughing, so we should forget the syndrome and focus on the coughing. Coughing means something different in

various contexts. Presumably, dexamathosone suppression or increased REM latency mean something else in different contexts as well.

One of the embarrassments of biomedical psychiatrists is that their own research findings have made it clear that they have consistently exaggerated the expected benefits of their model. This exaggeration is usually the result of simplified theories of etiology. Biomedical psychiatrists have done a good job of pointing out that psychological symptoms can be so diverse from patient to patient, that they are best considered effects rather than causes. Whatever the pathological process is, for any unitary disease, it has to be the same for all affected individuals. No defense mechanism or childhood experience meets this criteria. The same however, is true for biological markers. There does not seem to be any genetic pattern, anatomical abnormality, prenatal event, or early childhood disease shared by *all* persons with schizophrenia and bipolar disorder. Once we get beyond genetics, many biological markers are best conceptualized as signs rather than causes.

Wexler himself should realize that a radical bottom up strategy is scientifically flawed. In a separate article he rejects as unworkable the idea that psychiatrists adopt single-focus anatomical models in seeking the biological substrates of psychiatric disorders (Wexler 1982b). He uses A. R. Luria's (1966, 1973) model of brain function to claim that, instead of anatomical deficits, physiological processes which link brain regions with one another should be the focus of biological psychiatry. Rather than Andreasen's (1984) "psychoanatomy," it is likely that psychological functions are systemic processes, not reducible to any one anatomical area. Philosophers such as Fodor (1983) think that subsystems called modules are autonomous from each other, but Luria-influenced neuropsychologists would not support the idea of closed-system modules. It seems more likely that functionally distinct subsystems are also open systems, responsive to what else is going on in the brain. The brain has its own internal ecology. A thinker such as Donald Hebb (1949) would also claim that these functional units, called cell assemblies, are environmentally induced brain structures.

Damasio (1994) provides a historically relevant example of thinking about the brain in terms of integrated systems. Persons such as Phineas Gage who have had injuries to their frontal lobes undergo drastic personality changes. They may have poor judgement and become uncharacteristically irresponsible or just be chronically unmotivated (but not depressed). Historically, the Gage case and others like it provided strong arguments that a circumscribed brain

lesion in a particular location is associated with a circumscribed functional deficit. Traditionally, neuropsychologists claimed that because executive decision making functions are located in the frontal lobes, damage to the frontal lobe impairs the rational functions performed by that area.

Through a series of creative neuropsychological experiments, however, Damasio and his colleagues have shown that the kind of deficits demonstrated by Phineas Gage and people with similar injuries result from a reduction in emotional reactivity that leaves them unable to choose the most advantageous course of action. In laboratory tests, people with frontal lobe injuries have intact abilities to logically analyze problems, including those involving social and moral rules, but they fail to use information about their emotional reactions regarding possible negative consequences. As a result, their practical decision making is decimated. They will debate endlessly over options without being able to evaluate any of them as good or bad, and therefore do not know what to do. Damage to the frontal lobe causes loss of executive functions, but not because those functions are contained in the frontal lobes. No one thinks that emotional states are contained in the frontal lobes, and one of the primary functional deficits in hypofrontal patients relates to their inability to use emotional information.

With respect to consciousness, Dennett (1991) offers a more philosophical version of the functional model of the brain that subsumes what has been called the localization and the equipotentiality (holistic) theories in neuroscience. Dennett critiques what he calls the Cartesian theater model of consciousness. In the Cartesian theater model, consciousness is the linear culmination of information processing in the brain. The “theater of consciousness” is where all the information is integrated in awareness. No doubt something like the Cartesian theater lies behind a common observation in introductory biological psychology lectures, where the professor states that “we know information is transduced in the retina, enters the brain, and is processed at several points including the thalamus and the occipital lobes, but we don’t know where it becomes the experience of color.” Nineteenth century notions about the threshold between conscious and unconscious also echo the Cartesian theater, as does Freud’s (1900) distinction between conscious, preconscious, and unconscious levels of awareness.

According to Dennett, the Cartesian theater model makes the mistake of taking the idea of an observational point of view and moving that analysis into the head. For example, if someone bangs a drum on the opposite bank of a

river on which you are standing, the auditory and visual information reaches you (the observer) at different times. The observer is where the information is integrated. In moving this analysis inward, we look for a place in the brain where it all gets integrated as well.

Instead of the Cartesian theater, Dennett offers the multiple drafts model. He thinks that there is no place where it all gets put together. Distributed activity in the brain becomes conscious, but it is not unified in space or time. There is no neat divide between conscious and unconscious processes, nor is there an unbroken stream of consciousness in the Jamesian sense. Our brain neither edits information and presents it to the theater of consciousness, nor presents it the theater and reedits it when new information arrives.

This is because there is no temporal point in a linear sequence of information processing where experience occurs. If you close your eyes and imagine the room you are in, we all understand that the “mental space” is a logical construction, there is no place in your brain where the room is literally spatially recreated. Dennett thinks that the same is true for time. Brain events that subsume a moment of consciousness do not necessarily occur at the same moment.

In the Cartesian theater model, consciousness is the final common information pathway. An event happens and is then presented to consciousness and then another event happens and it is presented so that linear time is recreated. Instead, Dennett claims that information exists in multiple places at multiple points in time. Rather than consciousness being a stage where the whole play is presented to us, the final draft of conscious content occurs during our reporting mental content, remembering mental content, and acting on mental content. Rather than being there for us to use, consciousness content comes into being when we use it.

Damasio and Dennett’s models both suggest that the bottom up approach which focuses on inherent biological markers and “psychoanatomy” may be bad neuroscience. If functional systems in the brain interact, they can’t be studied in isolation or put together inductively one piece at a time. There is probably no monadic neural structure or chemical deficiency which is by itself responsible for someone’s depression or schizophrenia. Given anatomical contextualism, having some pre-existing concept of function is necessary for piecing together how the brain works. Function always takes us into a more molar level of analysis. At the anatomical level, it forces us to look at how brain systems interact. The functions that result from integrated brain systems

themselves interact with the external world, requiring the kind of molar analysis subsumed by evolution, ecology, and psychology.

Also useful is the *Neural Darwinism* of Gerald Edelman (1987). What was important about Darwin's model of evolution is that it allowed scientists to reject the notion that the biological world is explainable *only* with reference to God's design. In this tradition, Edelman proposes a model of brain development showing that, not only is there no completed design in the mind of God, there is no completed design in the genes either. The genes initiate the process of brain development, but what actually happens during development is controlled by selection at different points in time — called Neural Darwinism. Giving up on 19th century essentialism (see Chapter 10) and adopting the perspective of contemporary evolutionary biology helps us see that brain development can't be reduced to some inherent program in the genes, and also see that behavior can't be reduced to some inherent program in the brain.

One of the most helpful concepts in the philosophy of mind for thinking through the complexities of a biologically-informed psychology is Patricia Churchland's (1986) idea of co-evolution. Her idea is that in order to find neural substrates for memory, attention, anger, and abstract reasoning, we need to have psychological models of memory, attention, anger, and abstraction. Substrates are substrates of something. According to Patricia Churchland, neuroscientific progress will most likely lead us to alter our psychological theories of memory and anger, which will make the integration of psychology and neuroscience more achievable.

Although primarily proposed in a eliminative context to argue that neuroscience matters for psychology, Patricia Churchland realizes that co-evolution works both ways. Psychology matters for neuroscience as well. In addition to the basic cognitive functions such as memory and attention admitted by Patricia Churchland, more molar psychological concepts such as projection, object splitting, dissociation, and self-esteem are potential candidates for co-evolutionary work. As is consciousness itself, including the problems of sentience and selfhood. This is especially true if we adopt the radical empiricism of William James, where any evidence that can contribute to the solution of a problem is good evidence. Just as Paul Churchland had to reinsert the social world back into his thinking about how the brain works, any scientifically complete psychiatric research program will have to use functional and ecological analyses to make sense of the physiological and genetic facts.

4. A Note on Psychology and The Internal Ecology of The Brain

As expected, a parallel to the internal ecology of the brain exists in psychology, even though it is normally considered a weakness of psychology. In addition to the circularity problem discussed in Chapter 8, another problem with folk psychological explanations involves the problem of what Fodor (1985) calls semantic implications, and Patricia Churchland (1986) calls tacit beliefs. If I explain my behavior such as buying a ticket for a movie because I believe I will enjoy the movie, the intelligibility of that account includes a host of other implicit beliefs: I believe that the theater will accept paper money, I believe that the movie is in English or has subtitles, I believe that roof of the theater is not going to fall in, etc.

In addition to beliefs, we need to explain behavior with respect to desires, such as going to a movie because I desire entertainment, because I desire to escape my everyday reality, and because I desire to meet the woman selling popcorn, etc, with the object of desire being some perceived good. Taking both beliefs and desires into account, the inferential content of any psychological state is potentially prodigious. As Fletcher (1995) puts it, the set of causally important beliefs and desires are enormous, making them impractical candidates for developing simple billiard ball type explanations of behavior. (Meaning bounded event A causes event B, such as the cue ball hitting the eight ball, causing the eight ball to go into the corner pocket)

An example of the fluidity of dispositional explanations involves the problem of using self-esteem as an explanation of behavior. Contradicting a truism of pop-psychology, there is no monadic entity such as low self esteem that is a reliable cause of behavior. The idea of generalized low self-esteem makes little sense, except perhaps in describing persons who are clinically depressed. People tend to have low self-esteem about specific activities, such as playing golf, getting a date, or singing a song, rather having low self-esteem in general. Furthermore, someone can have low self-esteem with respect to a certain activity, but still try and even do well. When they succeed they don't take any credit, but they can succeed irrespective of self-esteem. This is because any psychological state can be mediated by another psychological state, for example, "I'm no good at this, but I don't want to let my parents down." Because we can believe many things at once, there is no 1:1 correlation between believing and behaving.

The interconnected nature of belief and desire is a version of Quine and

Churchland's network thesis, which itself parallels the internal ecology of the brain. In psychology, beliefs entail other beliefs which in turn entail other beliefs and the whole set of beliefs is so large that there is no simple explanation of behavior. There is no such thing as a monadic belief *qua* thing or entity or in my head causing me to do something in the same way that there is an individual cue ball hitting an eight ball and causing it to go into the corner pocket.

If biomedical psychiatrists and eliminativist philosophers want to point to specific brain regions as the causes of abnormal behavior, they should be willing to say that beliefs can cause behavior as well. Those who choose to go holistic and deny that there are monadic belief systems that cause behavior should also deny that there are monadic neural systems that cause behavior.

5. From The Dualism of “Organic Versus Functional” to Biopsychosocial Pluralism

One of the important advances of the DSM-IV was psychiatry's institutional rejection of the once popular distinction between organic and functional disorders. Technically, organic disorders referred to disorders that had identifiable physiological causes such as delirium, general paresis, and dementia. Functional disorders supposedly had more complicated biopsychosocial causal explanations, in which personal reactions played a role. Sometimes functional meant that there was no identifiable biological basis at all (English & English 1958; White & Watt 1973). In practice, the organic-functional distinction became a synonym for biogenic versus psychogenic causality, and hence contributed to biology versus psychology dualism. The writers of the DSM I even classified all disorders into two kinds, those with a biological etiology, and those with a psychosocial etiology — where any concomitant brain dysfunction was secondary to the psychological problem.

For depression, the organic-functional distinction had an analogy in the endogenous-exogenous distinction. Endogenous depressions were biologically-based conditions and exogenous or neurotic depressions resulted from a person's reactions to events. The only difference was that there was not always a known physiological cause for an endogenous depression. Although these labels fell into misuse with the introduction of the DSM-III, until the publication of the DSM-IV in 1994, the term organic was still used in opposition to the “mental.”

The proposal that the term “organic disorder” be eliminated from the DSM-IV was agreed upon for multiple reasons. Apparently, biomedically-oriented professionals resented the categories “organic personality disorder,” “organic depression,” and “organic anxiety disorder,” because they suggested that the other personality disorders, depressions, and anxiety disorders were not organic. One advantage of thirty years worth of biomedical research is that empirical evidence for biological substrates of every psychiatric disorder has been uncovered. In what has to have been a major biomedical materialist coup, the manual also claims that the only difference between psychiatric disorders and general medical conditions is a nominal one, meaning that general medical conditions are those conditions and disorders listed outside the Mental and Behavioral Disorders chapter of the International Classification of Diseases. This is consistent with Allen Frances’s (1994) somewhat exaggerated claim that “There could arguably not be a worse term than *mental disorders* to describe the conditions classified in the DSM-IV” (p. vii)

Proponents of psychosocial analyses supported the rejection of the organic-mental distinction as well. As I pointed out in Chapter 8, one mistake made by the more committed biomedical materialists is seen in their attempt to redefine all psychiatric problems as problems of neuroscience, even though the primary criteria for psychopathology, distress and disability, belong to the psychosocial levels of analysis. According to Materialism without Physicalism, distress is a brain/body state, but it cannot be replaced by the kinds of third-person descriptions favored by objective psychiatry. According to evolutionary theory, maladjustment is a functional concept. It requires examining the environmental context and evaluating “success” according to some normative standard. Therefore, just as any disorder is organic at some level of analysis, it is functional (or dysfunctional) at some level of analysis.

The co-evolutionary biopsychosocial perspective holds that psychological analyses and biological analyses can compliment each other. The ecology of neuroscience and the systems approach to understanding brain function both indicate that multiple targets of intervention need to be considered in helping a person get better. In good pragmatic fashion, the best targets are going to be the ones that work. As a matter of fact, effective treatment sometimes requires adopting different targets of intervention for clients who have the *same* disorder.

A worthwhile strategy for integrating biological and psychosocial analyses involves trying to discover what factors are preventing the person from

getting better. Many people who are depressed have biologically-based problems in mood regulation. Those problems are what is preventing them from getting better. This is the heart of the biomedical materialist treatment philosophy. Wender and Klein (1982) even claim that application of the term “illness” requires believing that something has gone wrong with the person’s biologically-evolved regulatory processes. When the biological problems are the primary factors preventing people from getting better, medication is going to be most effective (and natural) treatment.

Self-regulation, however, can be conceptualized on multiple levels of analysis. In addition to a biologically-based problem with mood regulation, a depressed person may develop a depressive personality style. As they mature, they learn to evaluate themselves and the world in a negative way. This enduring evaluation style becomes functionally autonomous from the biologically-based mood disorder. We can even say that it becomes biological in the way that untreated PTSD becomes biological, but that experience-induced biological state is different than genetically regulated problems in mood regulation. Medication may ease severe depressive episodes, but the person is still going to have a depression creating world-view. In these cases, cognitive approaches can help them change how they evaluate themselves and the world.

Unfortunately, cognitive approaches may not always work either, especially when the person is acting on less conscious emotionally-laden beliefs, expectations, and wishes. When biological and cognitive approaches are not sufficient, psychodynamic and interpersonal approaches can be useful in uncovering what is preventing this person from getting better, such as the need to repeat certain relational configurations in order to maintain a sense of identity. Systems approaches, where family units and couples are seen together can also be useful, especially when the situational determinants are modifiable.¹⁴

14. I do not mean to imply that I am in favor theoretical eclecticism. I am generally opposed to it because in its typical form, eclecticism is a prescription for superficial conceptualization and creates a vulnerability for committing pop psychology. Although different kinds of evidence and different exemplars (techniques) have to be integrated in the name of explanatory pluralism, they need to be integrated into a coherent theoretical system. The connections between states and exemplars will be more evident in an integrated theory. A consistent object relations theorist or a consistent cognitive-behaviorist who adopts explanatory pluralism will have more comprehensive views than an eclectic therapist.

To return to our depressed person, perhaps they are rigid and not open to change. If so, they will resist altering their typical way of evaluating the world. Their disposition toward rigidity may need to be a target of intervention. The psychological question is: what about this person explains their rigidity? The answers can be diverse, including such things as coming from an authoritarian subculture, or having adopted an attitude of certainty to compensate for a lack of security as a child, etc. Obviously a biological-based rigid personality style should be considered as a possible independent contribution to the persistent depression as well.

The trial and error nature of treatment may lead us to suspect that the rigidity may not be the best target for our interventions. Some other trait or enduring condition may be more important in maintaining the depression. The person in question could be overly dependent on family members who continually reinforce their negative self-image; they could have a conflict between an emotionally comfortable social role (traditional man/woman) and an intellectually prescribed social role (modern man/woman) that prevents them from feeling happy in any role; or there may be some secondary gain to being depressed, etc. All these factors can have an influence even if there is an underlying biological basis to the depression. The factor we pick is going to be the one we have success in changing — we just have to look for something that works. Focusing exclusively on physiology and genetics is a mistake. The biopsychosocial perspective is the more responsible one for the scientifically-minded mental health profession to take, even if it is harder to master.

People are more complex than case studies. I don't think a cookbook linear model: try situational factors, then biology, followed by cognition, family systems, interpersonal style, and psychodynamics is a workable strategy, for several reasons. As was suggested with respect to pathogenesis in the last chapter, sometimes several factors interact, and the problems they create cannot be so neatly separated. They may all need to be worked on at once. It is also unrealistic to expect any single professional to be knowledgeable about everything.

It may even be the case that psychiatry should follow the example of rehabilitative neurology and adopt the model of multi-disciplinary treatment teams in order to provide comprehensive patient care. Even though it will offend some physician egos, those psychiatrists who adopt the role of physiological technicians by focusing exclusively on traditional biological factors will not be the most competent persons to lead these treatment teams.

6. Conclusion

In Chapters 8 and 9 I have claimed that the biomedical materialist view about what it means for psychiatry to be scientific underestimates the complexity of biological science. Chapter 8 focused on the notion of distress and Chapter 9 focused on the notion of disability. Together these chapters show that the dust bowl empiricism of the neo-Kraepelinians breaks down in several ways. Contrary to common sense assumptions, biological science is not just about physiology and genetics, for two reasons. As indicated in Chapter 8, diagnostic practice in psychiatry includes gathering first-person reports, knowing what symptoms feel like, and considering the contribution patient reactions make to the progression of disorders. As indicated in this chapter, “maladaptive” is an evaluative term, and it is best conceptualized at the social and behavioral levels of analysis. Internal states that are adaptive in one context can be maladaptive in another context. What makes a psychiatric disorder pathological therefore cannot be explained solely with reference to internal biological variables.

The more molar biopsychosocial model attempts to overcome the dualism of organic versus functional disorders so prominent in past editions of the DSM. It allows us to consider more sophisticated models of pathogenesis, where the line between biology and psychology is difficult to draw. It also provides for a multi-level treatment approach that is thoroughly pragmatic. Because so many factors interact to produce maladaptive behavior, no single factor is always going to be the best target of our treatment interventions. Sometimes biology will be the best place to intervene, but sometimes it will not. More likely, multiple interventions on several levels of analysis will produce the most robust outcomes.

CHAPTER 10

Psychiatry, Science, and Anti-essentialism

1. Introduction

A natural kind is an entity that is regular (non-random) and internally consistent from one instance to the next. Elements such as carbon, gold, or a species of animal such as tigers are common examples. Bechtel (1988) defines natural kinds as “sets of objects which figure in scientific laws and have defining conditions” (p. 57). Defining conditions refer to necessary and sufficient properties that are *inherent* to the thing in question. For example, any element that has an atomic number of 79 is gold. Having 79 protons is the essence of what it means to be gold, in all possible worlds. Any object that looks like gold, but is not made out of atoms having 79 protons, is not gold. Thinkers who believe in the widespread existence of natural kinds are called *essentialists*.

Although mental health professionals do not use the term natural kind, they have used related concepts to evaluate classification systems. As framed in psychiatry and psychology, the goal of any nosological system is to carve *nature at her joints*. According to Kendell (1975) “In terms of the familiar aphorism that classification is the art of carving nature at the joints, it should indeed imply that there is a joint there, that one is not sawing through bone” (p. 65).

Related to the concept of natural kinds, Blashfield (1986) applies the traditional philosophical distinction between intensional and extensional definitions to the analysis of psychiatric categories. An intensional definition of depression would be a list of necessary and sufficient conditions that define the inherent meaning of depression. The extensional definition of depression would be the set of all people who are depressed. This model is essentialistic.

Some of the arguments in this chapter with commentary and a response by me can be found in Zachar, P. (2000). Psychiatric Disorders are not Natural Kinds. *Philosophy, Psychiatry and Psychology*.

There is a specific kind of thing with an inherent meaning (intension) and all members of that set of things can be listed (extension).

In this chapter I argue that it is a mistake to think of psychiatric syndromes as natural kinds, meaning bounded categories that have necessary and sufficient internal conditions for both their description and their diagnosis. This is important because thinking about something as a natural kind suggests that there is a God's eye view of that thing, a single accurate description of what it really is — independent of any particular way that we may conceptualize it.

Because phenomena such as diseases and species (as currently conceptualized) are not natural kinds, mental health professionals should not think of psychiatric disorders as natural kinds either. This holds whether they are using the medical model favored by physicians, or the psychometric model favored by scientific psychologists. Like species and diseases, psychiatric syndromes are best considered as belonging on the continuum of practical kinds. Thinkers who prefer to think in terms of practical kinds are called pragmatists. Pragmatists are committed *anti-essentialists*.

Thinking of psychiatric disorders as practical kinds makes it possible to ask whether someone has generalized anxiety disorder or anxious personality disorder without believing that this is a fixed either/or question. There may be sound reasons for preferring one category over the other in terms of the consequences that the labels have, but choosing between these two categories is not a question of diagnosing the “real” disorder. We shouldn't expect, even in principle, that the clouds could open up and an authoritative voice proclaim: “It's Anxious Personality Disorder, Not Generalized Anxiety Disorder.”

2. Kinds of kinds

Psychometrically defined, since natural kinds have fixed internal properties that make them be what they are, they can potentially be identified with perfect reliability. Natural kinds can be identified as the same kind of thing every time. To illustrate, once you know what the essence of gold is, you can decide whether a particular element is or is not gold with perfect accuracy. A natural kind is a pure kind, and once you have defined its essence, errors of identification are eliminated.

From an essentialistic perspective, any category that cannot be defined with respect to fixed internal properties is an artificial (or arbitrary) category.

For example, phrenological kinds such as a large forehead signaling abstract reasoning, analogical abilities, and critical thinking have been conceptualized as artificial kinds. These kinds were developed by measuring the morphology of a specific person's skull, associating distinctions in skull shape with specific traits, and generalizing the results to everyone else.

In psychiatry, Thomas Szasz (1961) is famous for arguing that schizophrenia is an artificial kind. Artificial kinds are supposedly pseudo-kinds. They don't really exist. What limited reliability they have capitalizes on chance. Some psychologists, especially those who favor dimensional models such as Robert McCrae (1994), also claim that the personality disorder categories in the American diagnostic system are arbitrary. Because both schizophrenia and the personality disorders are not natural kinds in the way that Alzheimer's disease is supposedly a natural kind, an assumption is made that they must be artificial kinds. Both Szasz and McCrae fail to consider the continuum of practical kinds.

Gorenstein's (1992) description of the kind *drug* is a good example of a practical kind. According to Gorenstein, *drug* is a superordinate category that describes the role of a diverse set of chemical compounds used in medical practice. Drugs include "throat lozenges, cholesterol reducers, nasal sprays, muscle relaxants, antibiotics and diaper rash relievers" (p. 15). Many different kinds of compounds can be drugs. Being a drug is not an inherent property of any chemical. It is a relational property.

Practical kinds are fuzzier than natural kinds, but they are not arbitrary. Psychometrically defined, classification of practical kinds requires balancing criteria that change their values in different contexts. As a result practical kinds do not have perfect reliability. They can be thought of as existing on a continuum, with some of them having higher reliability than others. To illustrate, deciding whether or not a particular instance of behavior is altruistic requires considering several factors, and there are no fixed rules telling us which factors are most important. A decision about whether a mother lion's self-sacrifice for the sake of her cubs is altruistic cannot be made with perfect reliability.

Understanding more about the role of practical kinds is important, primarily, because of a tendency among some scientifically-minded psychiatrists and psychologists to think about psychiatric disorders as natural kinds. For example, thinking that schizophrenia or bipolar disorder can be fully understood as broken brains, or thinking that diagnostic overlap between categories

such as anxiety, depression, borderline personality, and histrionic personality disorder indicates that these categories are arbitrarily defined. Those who believe in natural kinds claim that the goal of the scientist is to isolate the “real” categories. In contrast I argue, similar to Paul Meehl, that this kind of essentialistic thinking is scientifically malignant.

3. The DSM does not assume natural kinds

The advantage of rejecting the notion of psychiatric natural kinds is reflected in the Diagnostic and Statistical Manual of Mental Disorders (DSM) conceptualization of categories, which is based on the prototype model of categories rather than the classical model of categories. The prototype model is an attempt to define how human beings actually categorize objects and concepts. It is based on the work of psychologist Eleanor Rosch and her colleagues, and it is robustly anti-essentialistic. (Rosch 1981; Rosch & Mervis 1975).

Classical categories are the kinds of categories an essentialist would believe in. They are natural kind categories. According to Lakoff (1987), classical categories have distinct boundaries, so someone either is or is not a member of the category. Mars either is or is not a *planet*, or a figure can be a *triangle* or a *square*, but not both. Classical categories also have a set of necessary and sufficient properties that define them. For example, Aristotle’s definition of human beings as “rational animals” is an example of classical categorization. Rational animal expresses the essence of what it means to be human. In this view, anyone who could read the book of nature, God’s cookbook, could know what something really is.

As opposed to classical categories, categories in the prototype model have *fuzzy boundaries*, so it is not always clear who is and who is not a member of the category. Some members are better examples of the category than others, for example a robin is more prototypical of the category “bird” than is an ostrich, and the King’s throne is more prototypical of the category “chair” than is a bean bag. There are prototypical (a robin is a bird), atypical (a whale is a mammal), and borderline (a bookend is a piece of furniture) examples of any particular category.

In addition, prototype categories do not have necessary and sufficient conditions that define membership. A picture of a horse without legs would still be correctly identified as a horse by most people. Members of a category

do not need to share all properties in common, instead they share a *family resemblance*. This means there can be alternative criteria for being placed in a category.

For example, although an Aristotelian would take “rational animal” to be a necessary and sufficient criterion for being human, it also works to define humans as “featherless bipeds.” Furthermore, those who are brain dead may not be rational, but we would still think of them as human beings. Nor do we consider a chimp who can both use tools and rationally generate sentences in sign language to be human. There are not always clear and distinct sets of properties that define category membership. As the DSM-IV states, classification is not a cookbook affair, it sometimes requires clinical judgement that takes context into account.

In the DSM, patients are diagnosed according to how well they match the criteria set, but no criterion or group of criteria is necessary and sufficient. Diagnosticians call this the “polythetic” criterion strategy, and it is a version of the *prototype* model. Polythetic criteria sets are organized so that the most prototypical criteria are listed first. As Widiger and Francis (1994) note, there are 93 different ways to meet criteria for being diagnosed with borderline personality disorder in the DSM-III-R, and 848 different ways to meet criteria for antisocial personality disorder. A syndrome such as antisocial personality disorder is a family of personality types rather than a single discrete type.

The manual also provides differential diagnostic suggestions to help with the issue of *fuzzy boundaries* between categories. For example, someone can be depressed or schizophrenic, or depressed *and* schizophrenic, or schizoaffective. To distinguish schizoaffective disorder from depression with psychotic features, the person must have experienced sustained hallucinations and delusions in the absence of a mood disturbance. To distinguish schizoaffective disorder from schizophrenia *and* depression, a mood disturbance must be present for a substantial portion of the active psychotic phase. The biggest fuzzy boundary categories are called V-codes, where the category “psychiatric disorder” itself has borderline cases. V-codes refer to conditions such as marital discord and spiritual crises, problems that may deserve clinical attention but are not labeled as mental illnesses.

Meehl-influenced scientific realists such as Grove and Tellegen (1991) claim that prototype models confuse clinicians’ cognitive processes with the nature of reality. In their view, we may naturally think in terms of prototypes, but that does not mean the world is really organized into prototype categories.

Unfortunately, Paul Meehl over-emphasizes the project of discovering reality, jumping from the legitimate goal of trying to find out how the world is to the questionable suggestion that we can find out how the world really is. As a good falsificationist, he knows that claims about finding out about *The Truth* are wrong. He is so good at pointing out what is false, that he sometimes talks as if robust statements that survive logical critique can be thought of as having been confirmed — a possibility that Popper rejects. His Minnesota colleagues are even looser with talk about “carving nature at the joints.”

To calm the fears of my “tough-minded” readers, the absence of absolute criteria does not leave professionals with “whatever goes.” There are criteria and those criteria are a function of more than personal whims. For example, even if the categories of schizophrenia and borderline personality are best considered prototypical in structure, where there are clear cases and borderline cases for each, there is still a difference between schizophrenia and borderline personality. Even though diagnosticians cannot provide a single set of conditions that are both necessary and sufficient for the diagnosis of schizophrenia, schizophrenia can still be distinguished from other disorders. Furthermore, we can give plenty of *reasons* for saying that “racist personality disorder” is not a legitimate mental illness, without having to think that we are carving nature at the joints. “Whatever goes” is not an option.

4. Two approaches to classification, both arguably anti-essentialistic

In the next two sections I argue that the medical model approach to identifying psychiatric categories and the psychometric approach to identifying psychiatric categories are both consistent with anti-essentialism.

Approach one: the medical model

Classification has been and continues to be one of the most important problems in psychiatry. It involves deciding what syndromes psychiatrists should diagnose and treat. Proponents of the biomedical model would define syndromes just as other syndromes are defined in medicine. The first step involves a clinical analysis where various signs and symptoms are seen to co-occur in a way that suggests that more than chance is operating. So the co-occurrence of a sore throat, runny nose, and head & chest congestion would

suggest an integrated syndrome, classified as “the common cold.” This is called the clinical presentation of the illness. The next step is to describe the course of the syndrome, so we find out the sore throat may come first and disappear, then sinus congestion, manifested in a yellow discharge followed by a clear discharge as the person becomes non-infectious. At some point in the process chest congestion develops, and it can linger on for weeks. Recovery is spontaneous. In this model, syndromes have a common etiology and therefore a common cure. Describing the physical mechanisms that produce the syndrome is the heart of the biomedical model. Once it is clear that there are such mechanisms, syndromes are called diseases, and traditionally conceptualized as natural kinds.

If psychiatric syndromes are biological natural kinds, biological variables such as genetic codes and drug response may help us isolate their underlying reality. Biomedical materialists assume that, because physical diseases are the bedrock reality of medical science, in order to be scientifically valid, histrionic personality disorder and schizophrenia have to be understood as physical diseases. As diseases they must have underlying biopathological processes. For example, in the one-time psychiatric disorder *general paresis*, the symptoms or the presentation can vary from a paranoid syndrome to a depressive syndrome to a grandiose syndrome, but the underlying biopathological process is the same (Blashfield 1984). The underlying pathological process is untreated syphilis. The presence of the spirochete is necessary and sufficient for a diagnosis of syphilis.

According to Staats (1991), one characteristic of a mature and unified science is being able to see how superficially diverse phenomena are really manifestations of the same phenomena—such as an underlying pathological process. Explaining histrionic personality disorder and schizophrenia just as general paresis was explained is an important goal for the biomedical model.

The strategy of using its biological basis to define the essence of a syndrome is exemplified in Wender and Klein’s (1981) concept of “masked depression.” According to Wender and Klein, some people have psychiatric disorders that are not associated with the symptoms we use to identify depression, but these disorders have the same causes as depression. Some examples of these atypical symptoms include muscle pain, anorexia, panic, sexual impulsiveness, and wanderlust. Wender and Klein claim that because these problems often respond to antidepressant medication, they could be considered forms of depression. The concept of masked depression indicates that

phenomenology in terms of symptoms such as sadness may only be superficial appearances, relatively unimportant to understanding what depression really is, i.e., a biopathological process with specific kinds of causes.

Gorenstein's (1992) spectrum disorder concept works in the same way. Spectrum disorders are behaviorally and phenomenologically dissimilar disorders that result from the same kind of defect in the central nervous system. As an example, Gorenstein suggests that alcoholism, antisocial personality, and childhood hyperactivity can all be traced to the same defects in the frontal lobes and the limbic system, specifically an inability to inhibit dominant response tendencies. He defines this problem functionally as an inability to delay gratification. Using family prevalence studies, Wender and Klein (1982), add somatization disorder to this list, labeling the group of illnesses the "unnamed quartet."

In a parallel fashion, materialistic philosophers generally deny natural kind status to entities that are defined only phenomenologically. For example, gold defined only in terms of properties such as density, hue, and reflectance or depression defined only in terms of dysphoria would not be natural kinds. These properties are only about superficial appearances, as opposed to something such as an atomic number or failed dexamethasone suppression of cortisol.

Although studying drug responses to learn more about shared pathogenic factors is important, making drug response to lithium or Prozac a necessary criterion for individuating and diagnosing a disorder would work only if the drug could be said to always cure the disorder. At present, no psychiatric drugs can be said to do this. Lack of lithium is not the cause of mania, therefore lithium response cannot be taken as a necessary criterion for defining mania. Lithium's efficacy with bipolar disorder varies from patient to patient, and some bipolar patients do not respond to lithium. If we make lithium response a necessary criterion for bipolar disorder, we might have to tell someone whose husband just mortgaged the house to buy one million plastic worms, that, although he looks manic, he is not because he has never responded to lithium. In individuating categories, we should not limit ourselves to one type of evidence. Drug response is only one type of evidence. Figuring out causes is more complicated than Wender and Klein acknowledge.

With respect to masked depression, Wender and Klein fail to note that psychotherapists developed similar ideas independent of medication response. Psychodynamic thinkers have long talked about atypical presentations of a

syndrome such as depression in terms of acting out the dysphoria rather than feeling it, a common psychological explanation of alcoholism (Gabbard 1994), exhibitionism (Mitchell 1988), and impulsiveness in borderline personality disorder (Masterson 1976). Rather than making the anti-anthropomorphic claim that atypical medication responses undercut the importance of traditional psychological analyses, it makes just as much sense to say that they sometimes cohere with them. Wender and Klein's speculations are more plausible when integrated with traditional psychological analyses. This co-evolutionary approach is likely to be more productive in the long run than the mind versus brain approach that anti-anthropomorphism prescribes.

As noted in Chapter 8, psychiatric syndromes are end products. They cannot be reduced to inherent necessary and sufficient criteria. For example, genetics may be necessary as process initiators, but they are not sufficient causes of most syndromes. The fact that impulsiveness is common to psychopathy and hyperactivity does not mean that these conditions are variations of the same disorder any more than the fact that both pneumonia and appendicitis share a fever means that they are variations of the same disorder. Like a fever, impulsiveness could be an effect rather than a cause.

Although the notion that syndromes conceptualized as diseases will help mental health professionals discover psychiatric natural kinds, diseases cannot be conceptualized only as discrete physical entities. Following an examination of this issue, I will examine the concept of the species, showing that evolutionary biologists reject the idea of absolute boundaries between species. If diseases and species are not considered to be natural kinds, psychiatric disorders should not be considered natural kinds either.

Diseases are not natural kinds

According to Blashfield (1984), disease literally once meant dis-ease; but advancing medical knowledge led to the discovery of conditions where people could have a disease without any subjective discomfort, for example high blood pressure. The meaning of the term disease evolved and continues to do so. Some diseases are fatal, others are inconveniences. Some diseases represent high or low ends on a normal continuum of biological processes, while others represent qualitative deviations in biological processes (Guze 1992).

Our inability to point to one thing and say this is the disease is summarized by Roth and Kroll (1986)

Thus for example, not everyone exposed to tuberculosis develops the disease in its complete form. The state of the immune system depends on the host's genetic constitution, nutritional status, viral infections that may produce an immunodeficiency syndrome, previous exposure to similar microbial pathogens, state of fatigue, state of anxiety, level or morale, presence of depression, recent major life changes and other 'psychological' factors (p. 63).

The bacillus is necessary but not sufficient for tuberculosis. Infections exist in hosts, and disease processes result from the interaction between infection and host. They are relational rather than inherent properties. Wallace (1994) shows that anti-essentialism pertains to treatment as well. He notes that although infectious diseases are the most physiologically-based diseases in medical science, (a) the same infection in two different patients may not respond to the same antibiotic or antiviral medication, and (b) different microorganismic infections may respond to the same medication. Wallace's observations should lead us to be skeptical of some biological psychiatrists' claims that every disorder that responds to anti-depressant medication must be a variation of the same disorder.

Infectious diseases are prototype diseases. If someone has the TB bacillus, he or she will be diagnosed as having tuberculosis and treated. The same is true for syphilis. If the spirochete is present, the person will be treated for syphilis before any symptoms are present. Such problems can be reliably diagnosed, and accurate diagnosis helps physicians make predictions about what is going to happen to the person if they are not treated. Once a category is defined, understanding its causal mechanisms becomes an important clue to deciding if it is present. The high reliability and predictive validity of infectious diseases makes them the most practical of practical kinds.

Because practical kinds cannot be fully defined with respect to inherent properties, external criteria play a role in their definition. As noted in the previous chapter, Gorenstein (1992) notes that we mistakenly confuse the issue of the biological basis of syndromes such as schizophrenia and histrionic personality disorder with the question of whether they are diseases. Like Fulford (1991), he thinks the disease concept also involves a social evaluation of maladaptiveness, which is a different problem from the problem of deciding if schizophrenia exists. As Kendell (1975) notes, in practice, claiming that a person has a disease really means that there is something wrong that needs to be treated. Like all practical kinds, diseases cannot be fully defined with respect to inherent properties.

An example of the role of external criteria in identifying practical kinds is the American Psychiatric Association's reclassification of homosexuality from a pathological sexual perversion to a normal variation in sexual orientation. We still think of homosexuality as a type of behavior with a biological basis, but we do not think of it as a maladaptive disease. The gay-lesbian community has even used theories about a biological basis for homosexuality to support the idea that it is a normal variation. Thirty years ago, an identified biological basis would have been considered confirmation of the real existence of a disease. If the community is unwilling to label homosexuality as an illness that needs to be treated, its biological basis will not be called a disease.

If we separate the question "what is schizophrenia" from the question "is schizophrenia a disorder," it may be possible to define schizophrenia essentially. If natural kinds are defined only with respect to causal mechanisms, the essence of schizophrenia would be its causal mechanisms. However, schizophrenia would still not be a classical category. Defining a disease with respect to its causal mechanisms without considering that disease to be a classical category might be called a soft natural kind. This raises the thorny question of what level is going to be considered the essential causal level, and the problem of overdetermination (or multiple causality). Also, it may not be possible to separate the question "what is histrionic personality disorder" from the question "is histrionic personality disorder a disorder."

One promising strategy for rescuing the classical category model has been to substitute the concept of disease for the concept of a disorder. Wakefield (1992a, 1992b, 1993) specifically defines mental disorder as "harmful dysfunction." *Harm* refers to the fact that the condition has negative consequences for the person. It involves a reduction in well being, defined by social values and meanings. *Dysfunction* refers to the fact that something has gone wrong with an internal mechanism. It is not operating the way it was designed to operate. The concept of dysfunction helps distinguish a mental disorder from normal responses that also have negative consequences, such as grief and trauma. So "disorder" refers to harm to the person because of the failure of some internal mechanism to operate in the way it was designed to operate.

Wakefield's definition conforms to the classical category model in that "design failure" and "harm" are in combination necessary and sufficient for labeling a particular state a disorder — with design failure being the underlying pathological process. Wakefield defines "dysfunction" like Thomistic philosophers defined "evil", it is a privation — an absence of something that

ought to be there. It is not an entity. The biggest challenge in using this model is in deciding what ought to be there.

I am not convinced that Wakefield's concept of harmful dysfunction is being proposed as a natural kind because the harm criteria does not refer to internal or inherent properties. For Wakefield, harm means maladaptive. As long as "maladaptive" is part of the meaning of psychiatric disorders, defining psychiatric disorders as *identical* with some fixed internal state will be insufficient. This is because rather than there being inherent, fixed traits that define adaptativeness, adaptiveness is defined as whatever confers a competitive advantage. As the local ecology changes, what counts as adaptive changes, so that traits adaptive in some situations can be maladaptive in other situations.

Even design failure cannot be understood only with reference to internal properties. According to Dennett (1987), we can't understand an organism's internal design without making reference to external conditions. Imagine, asks Dennett, finding a heretofore unknown life form that has been put into a kind of suspended animation. Our job is to study this organism's design and figure out how it would behave. We could not proceed without developing some theory about what kind of environments the organism is adapted for. Learning more about the internal design of the life form will always include more specific theories about what kind of environments it is adapted for. Natural selection acts on interactions between an organism and the environment. Realizing this, Wakefield (1999) indicates that identical internal mechanisms may constitute a design failure for one subspecies and adequate design for another subspecies — depending on their evolutionary history. Design failure is therefore not a natural kind — defined solely with respect to fixed inherent properties.

Contra Wakefield and similar to Lilienfeld and Marino (1999), and Richters and Hinshaw (1999), I would prefer to leave open the possibility that syndromes such as psychopathy and dysthymia are currently maladaptive variants rather than dysfunctions imposed on the brain. Evolutionary theory readily admits the existence of individual variations that are maladaptive, but consistent with an organism's "design."

For example, after watching enough Nature programs on PBS, it seems reasonable to hypothesize that a condition such as narcissistic personality disorder could be labeled "alpha male syndrome" — a normal variation within the range of male behavior. If we follow leading evolutionary theorists such as Hamilton (1964), Williams (1966), Trivers (1971), Dawkins (1976), and

Cosmides and Tooby (1999) in holding that the gene is the unit of selection, narcissistic personality disorder could be an effective strategy for maximizing gene replication and therefore consistent with an organism's design. All of Wakefield's examples of design use an individual-centered concept of evolution. It is possible that from a gene's-eye point of view, Wakefield would have to bite the bullet and say that narcissistic personality disorder is not really a disorder. If these kinds of counter-intuitive cases (false negatives) begin to pile up, we will begin to think that the harmful dysfunction model needs to at least be augmented with some auxiliary propositions.¹⁵

Rejecting the 19th century view of disease casts doubt on the tendency of the biomedical materialists to subsume all psychiatric disorders under the exemplars of either general paresis, pellagra, or mental retardation. It also casts doubt on Paul Churchland's use of traumatic brain injury as his ruling exemplar. Although important, only discussing what is broken in the brain is not necessary and sufficient for explaining and understanding psychiatric syndromes.

Species are not natural kinds

If the anti-essentialistic model of disease sounds too tender-minded, it may help to know that the existence of natural kinds is also doubtful in zoology and palentology. For example, Hull (1989) notes that species are statistical abstractions rather than essences.

At any one time, one can rarely discover a set of traits which is possessed by all members of a species and by no members of some other species. In addition, the members of successive generations of the same species are usually characterized by a slightly different set of traits. (p. 147).

Gould (1983) points out that alternative taxonomic systems provide scientists with different classifications of species. A major debate in zoology in the past thirty years has occurred between proponents of phenetic versus cladistic classification. According to the *numerical phenetic model*, which is based on a mathematical analysis of outward appearances, mountain zebras, Burchell's

15. As long as Wakefield is defining disorder by stipulation, I think his proposal has some merit. By stipulation I mean that he is only proposing a model that should be useful in helping diagnosticians distinguish between disorders and non-disorders, rather than making an authoritative statement about what a disorder really is. Furthermore, it is not unreasonable that harmful dysfunction in practice would become a radial category with prototype cases and borderline cases.

zebras, and Grevery's zebras are all part of the species zebra. According to the *cladistic* or phylogenetic model, where organisms are grouped according to common ancestors, mountain zebras are classified as a kind of horse. Gould (1983) also notes that orangutans are cladistically more distant from chimps and gorillas than humans are. Another example is given by Ridley (1989), who points out that lungfish are more similar to cows than to salmon according to cladistic criteria. Neither phenetic nor cladistic classifications are whimsical; for example, neither model would classify orangutans as a type of reptile. The phenetic and cladistic taxonomies also have significant overlap, but what counts as a group is partly model-dependent. Zoologists have not achieved what might be called a fixed God's eye view of species.

A compromise between the phenetic model and cladistic model is offered by one of the century's most prominent biologists, Ernst Mayr (1988, 1989, 1991), who believes in the reality of species more than Darwin did, but rejects the natural kind view of species favored by pre-Darwinian thinkers such as Linnaeus. Ruse (1988) and Mayr (1988) both claim that post-Darwinian biology has given up on Aristotle's idea of species as natural kinds for an understanding of species defined by polythetic criteria. Mayr notes that the very possibility of the evolution of species contradicts the idea of a fixed inherent structure that defines all members of a species. The facts of evolution suggest that any criteria set will eventually become outdated. This is why Mayr (1969) claimed that the traditional approach of classifying what species exist, i.e., classical taxonomy, must be augmented with an understanding how life in all its diversity fits together, i.e., systematics.

Biologists do not think of the individual-species relation as analogous to the member-class relation, where members belong to a class because they share common properties, rather they think of it as more analogous to the cell-organism relation, where individual cells make up a larger organism. Instead of organism, they follow Darwin in using the word "population." In contrast to Linnaeus, Darwin defined a species in terms of populations of unique individuals rather than individuals sharing a common essence. A population is a genetic, behavioral, and ecological system whose members compete with each other, and as a whole with the members of other species. Rather than what an essentialist would call "imperfections," "errors," or "accidents," individual variation is central for understanding the long-term fluid nature of species.

Mayr's own compromise between the phenetic and the cladistic models is called the *biological species* model. A species is defined as:

- (a) A reproductive community, usually mating only in their group.
- (b) An ecological unit; individuals in the group share an environmental niche and relate as a group to other species.
- (c) A genetic unit; any individual only holds a part of the species' gene pool.

Especially for the two behavioral criteria (a) and (b), species are defined in terms of relationship patterns rather than an internal essence. In contrast to the phenetic model, Mayr shows that groups with very different external appearances can interbreed, and groups with similar external appearances cannot interbreed. In contrast to the cladistic model, even though birds and crocodiles are closer together with respect to shared ancestors than crocodiles are to other reptiles, ecological and behavioral variables make crocodiles more like reptiles than like birds. Both models provide evidence that we can use in a comprehensive conceptualization.

As a confirmed anti-essentialist who explicitly rejects nominalism, Mayr believes that species are not just inventions. The distinctions accepted by taxonomists are far from arbitrary. For example, there are non-arbitrary gaps between many species. There is a clear gap between primates and reptiles, whose inability to interbreed is stable. Whether genetic manipulation could transform this "law" into an empirical generalization is an open question (and probably one that should never be answered).

In terms of populations that are less distinct, however, it can be difficult to clearly see what counts as a genetic or reproductive community, especially when you have incipient species — groups that have acquired some but not all characteristic of a species. With these borderline cases, a species as an absolutely fixed type evaporates away. With a change of habitat, single groups can divide into two or more groups (speciation), or two groups that were separate under certain conditions (e.g., wolves and dogs) can relate to each other as a species. As Dawkins (1986) notes, the primary reason that we can maintain a belief in discrete boundaries between species is that intermediaries tend to be extinct.¹⁶ A few intermediaries do, however, still exist. For ex-

16. Dawkins (1986) shows that genes don't have inherent effects either. The effect of a gene is not a simple property of the gene itself. It "is a property of the gene in interaction with the recent history of its local surroundings in the embryo." "A gene turned on in the cells at the base of the spinal chord in the third week of development will have a totally different effect from the same gene turned on in the cells of the shoulder in the sixteenth week of development." (p.296)

ample, Kendell (1975) notes the platypus is neither cold blooded like a reptile nor warm blooded like a mammal, but a mixture of the two.

The process of categorizing species can illuminate the process of categorizing psychiatric disorders. As the phenetic-cladistic debate shows, although external appearances do not define the essence of categories, they still have an important role to play in categorization. Appearances are always part of the evidential basis in any comprehensive system for defining categories. For example, it is impractical to exclusively classify lungfish with cows and not with salmon because lungfish behavior is so much more congruent with salmon behavior. Habitat matters.

In the same way, it would be impractical to exclusively classify anorexia as a biologically-based mood disorder, or to reduce anxiety and depression to serotonin dysfunction as some people in psychiatry have suggested. Internal biological properties, whatever they may be, are important, but they tend to be insufficient for understanding syndromes. Phenomenological appearances still have an important role to play in categorization — especially in categorizing *psychopathology*.

Psychologists such as Widiger and Trull (1991) use terms such as “arbitrary” to refer to psychiatric classification systems which are based on similarity of presentation. Arbitrary is a strong word, suggesting whims or preferences as opposed to the kind of sustained regularity one would expect in natural kinds. Unfortunately, a pejorative word such as “arbitrary” is merely a rhetorical device, used to demean an opposing model.

Let me illustrate this by analyzing Corning’s (1986) example of classification by similarity of presentation. It involves his six-year old son’s rearrangement of his office. Corning drew baby-sitting duty with his six-year old son on an afternoon that he had to chair a thesis defense. He arranged it so the boy would be occupied in his office while he was at the meeting. Upon returning to his office after the meeting, he found that his son had reorganized his filing system. All his brown folders were neatly stacked in a pile on the floor. All his manilla folders were stacked in their own pile. The documents in each file had been removed and placed in their appropriate piles. Legal size yellow notepaper was in one pile, legal size white notepaper was in another pile, notebook size yellow notepaper was in a third and so on. All white paper with typing on it was in its own pile. Upon his father’s return, the boy proudly proclaimed that he had “straightened the office out.” Corning suggests that his son’s classification was analogous to psychiatric classification, where a super-

ficial analysis based on outward appearances takes the place of an analysis of categories in terms of the information they contain.

This amusing story, however, fails to make the point that proponents of natural kind categorization think it does, i.e., that it is an example of arbitrary classification. The boy's re-classification of Corning's files into folder type and paper type was not arbitrary. It was concretely systematic in its organization. The important issue is not what rules we use to develop categories. The important issue is deciding how useful the proposed categories are. Organizing offices in terms of files, so that information on attention deficit disorder is in one file, information on lateralization is in another file, and information on cluster analysis is in another file is more useful to an academic than putting all the yellow paper in one pile. Categories are practical kinds. The categories developed by Corning's son were impractical, not arbitrary. Traditional psychiatric categories are not arbitrary either. Rejecting essentialism in favor of anti-essentialism does not require one to adopt nominalism. Practical kinds are more than names, but less than inherent essences.

Maximize taxonomic advantages, compensate for disadvantages

If philosophers of biology are correct, medical model theorists should be able to propose alternative taxonomic systems for defining syndromes, and each system may have different sets of syndromes. Classification schemes developed at different level of analysis, e.g., (including but not limited to) the genetic level, the neurochemical level, the anatomical level, the affective level, the cognitive level, the phenomenological level, and the sociocultural level may not be perfectly isomorphic with each other. Each taxonomy would have validity for certain purposes, but no one could be called the real taxonomy.

All but the most partisan thinkers agree that this is true with respect to the categorical versus the dimensional classification of psychiatric disorders. Under certain conditions, disorders considered unique can be seen to be a variants of the same disorder:

Alcoholism, attention deficit hyperactivity disorder, and psychopathy equal the same disorder (Wender and Klein 1981).

Comorbidity between personality disorders is a function of shared pathogenic factors, (McCrae 1994).

Or a single disorder can be more usefully studied by being fragmented into several different types:

Schizophrenia equals a positive symptom presentation and a negative symptom presentation (Andreasen and Olson 1982).

'Borderline schizophrenia' includes borderline personality disorder and schizotypal personality disorder; Schizoid personality in DSM-II equals schizoid and avoidant personality in DSM-III (Gabbard 1994).

Each taxonomy would have a different set of advantages and disadvantages, but syndromes would not just be inventions.

Another version of the same point is made by Widiger and Francis (1994). They note that one of the most important decisions in developing DSM diagnoses was to decide where to place cut off points. For example to be diagnosed as antisocial personality disorder, one has to meet at least five of the nine criteria for that disorder. Widiger and Francis note that any diagnostic system will be used for many purposes, including "hospitalization, medication, psychotherapy, insurance coverage, scientific research, criminal responsibility, disability, and so forth" (p. 23). The optimal cutoff point would be slightly different for each of these purposes. No diagnostic system can be considered The System.

An excellent example of a conceptual advantage being written into a taxonomy is the inclusion of bipolar II disorder in the DSM-IV. This is important because bipolar type disorders often respond to lithium. In the current diagnostic system, in addition to the traditional manic depressive (bipolar) and cyclothymic¹⁷ disorders, the DSM-IV describes bipolar II disorder as the combination of mild mania (hypomania) and major depression. Because people do not complain to physicians or psychologists about mild mania, they are often misdiagnosed with a major depressive disorder when they need to be on lithium. Bipolar II increases clinical sophistication by alerting diagnosticians to the subtleties of mood changes over time, helping them notice different presentations in the bipolar spectrum.

One disadvantage of the DSM as currently written is that the descriptions are so listy and the categories so inert, it encourages a kind of simplistic thinking about disorders. It is easy to reify the category into an entity and look for its biological basis, ignoring the ecology of syndromes with respect to

17. Cyclothymia involves alterations between hypomania and dysthymia. Hypomania is "mild mania" which includes the experience of high energy and euphoria. It tends to be experienced as very enjoyable. Dysthymia is chronic low grade depression or a "depressive personality."

reactions or *biopsychosocial pathogenesis*, which views disorders as processes. This simplification would have been less possible with the DSM-II and is currently less possible in The International Classification of Disease-10 (e.g., “enduring personality changes after a psychiatric illness” and “post-schizophrenic depression”). Just as physical appearances such as colorful plumes can have reproductive advantages, but be an adaptive disadvantage because it is harder to hide from predators, so the appearance of objectivity can be advantageous with respect to scientific respectability, but be disadvantageous because it discourages developing an integrated framework for understanding how disorders develop. In Chapter 8, I referred to this as the DSM ignoring the importance of the clinical formulation.

With respect to biomedical materialism, mental health professionals should agree that if we could resolve all psychiatric disorders like physicians resolved general paresis, it would be a wonderful achievement. Although the causal mechanisms underlying syndromes such as schizophrenia and bipolar disorders are clearly more complicated than paresis, if we could figure them out and stop the progression of the “disease,” we should. At the same time, at this stage of development, forced unification of psychiatric disorders under the umbrella of biomedical materialism could be more harmful than helpful. Those thinkers such as Churchland and Andreasen who would redefine all problems into problems of neuroscience and merge psychiatry with neurology are attempting to force unification on a field that still deals with complex multi-level problems.

Premature unification because of a guess about what future discoveries will reveal robs psychiatry of the vitality and diversity needed to develop a more comprehensive taxonomic system. Both the co-evolutionary stance and the ecology of neuroscience perspectives suggest that redefining all problems into problems of neuroscience constitutes an overly conservative model for understanding the brain.

Approach two: psychological measurement

One problem with clinical observation is that human judgement may not always be sensitive enough to detect subtle patterns. For example, AIDS as a syndrome existed in patient populations long before physicians noticed it. In psychiatry, ever since Kraepelin proposed the label dementia-*praecox*, students of what came to be called schizophrenia have believed that is not a

unitary syndrome, but a group of related disorders. Unfortunately clinicians have not been able to intuit the different syndromes that make up what we call “schizophrenia.”

The traditional medical model is not the only approach to classification. Clinical psychologists, who are trained in scientific methodology rather than applied medicine, claim that co-occurrence is a synonym for correlation. Correlation has an exact mathematical definition. It is an index of the proportion of total variance that is due to scores that covary in a predictable manner. The best way to decide what variables are correlated is by statistical analysis. Therefore multivariate methods such as factor analysis and cluster analysis, which can objectively determine the presence or absence of patterns in the data may be more scientifically sound than clinical observation. Because the observer-independent patterns found by statistical analysis are also by definition non-random, they could even be called natural kinds.

Statistics require assumptions

Unfortunately for psychological science, statistical methods are not so artless. Skinner (1981) and Blashfield (1980) have both criticized the naive empiricism adopted by some proponents of statistically-guided classification. For example, in *Monte Carlo* studies, where artificial data sets are generated to test statistical assumptions, even with a random pattern of data, factor analytic procedures will extract factors. They capitalize on chance variance in the data to find a pattern. The same can be true for cluster analysis. Even if no patterns exist, statistical analysis may find them. This means that there is a potential false positive problem with multivariate statistical methods. They may find patterns that are not really there. More seriously, if the appropriate variables are not entered into the analysis, multivariate methods can also fail to detect patterns, i.e., yield false negatives.

Another problem with multivariate statistical methods is that their calculations are not as objective as some of their proponents claim. As Skinner (1981), Blashfield (1981), and Tinsley and Tinsley (1987) note, various theoretical and mathematical decisions have to be made before analyses can be run, and these decisions can affect the results. These decisions include deciding whether variables or persons should be correlated, deciding whether factors should be correlated or uncorrelated (orthogonal), deciding whether all the variance to be partitioned is considered common variance, deciding whether items should load highly onto one and only one factor (simple

structure), deciding how distances between variables are to be computed, and deciding what strategy the investigator uses to form clusters. All these decisions can influence the results. Therefore, the derived patterns are partly method-dependent.

Mathematically structured folk constructs are not natural kinds

Widiger and Corbitt (1994), Widiger and Trull (1991), and McCrae (1994), who favor psychometrically discovered dimensional models, critique clinically discovered categorical models because they are “hypothetical constructs,” “arbitrary,” and “not naturally occurring categories.” They contend that the DSM’s comorbidity problem is unacceptable for a system that is supposed to be composed of discrete entities. This essentialistic critique of categorical models, which conforms to the 19th century view of disease, suggests that their own dimensions based on the five-factor model of personality will somehow carve nature at her joints. Their preferred measure of the five factor model is called the NEO-PI.

As noted, there are good reasons for claiming that psychometric methods for discovering psychological dimensions do not carve nature at the joints. It is true that once certain parameters are defined, relatively stable solutions emerge, but change the parameters and different solutions may appear. In carving, we should not find different joints if we switch knives. When the issue is considered under the rubric of scientific realism versus instrumentalism, dimensional models do not themselves meet the standards that their proponents use to reject categorical models.

Furthermore, considerable subjective judgement is required to name a factor. For example the Conscientious factor on the NEO-PI could also be called dependable, responsible, scrupulous, conformist, or, as a rebellious young client of mine once stated, “ass-kisser.” These are not mere synonyms. It is like describing someone as flexible versus describing them as flaccid or as rigid versus rigorous. Different terms have different connotations. These are not natural kinds.

The biggest misinterpretation of latent mathematical categories is to confuse a factor with a factor-derived scale. As Cattell (1978) notes, a factor is a latent entity that accounts for a proportion of variance in a correlation matrix. Each item in the correlation matrix usually loads onto the factor. Psychometricians often take the items on a factor with the highest loadings and put them on a single scale, but the scale is not the same thing as the factor. The factor

usually contains some of the variance from all the items, not just the items with the highest loadings. Furthermore, the items with high loadings still contain variance that is not associated with the factor.

During the development of the DSM-IV in the early 1990s, the NEO-PI dimensions were proposed as alternatives to the personality disorder categories. The NEO-PI measures five traits, Neuroticism, Extroversion, Openness, Conscientiousness, and Agreeableness. These are called the Big Five because they have emerged in several major factor analytic research programs over the years. In their review of its conceptual beginnings, McCrae and Costa (1990) claim that the NEO-PI is based on the descriptions of personality found in natural languages, which they refer to as “folk wisdom” (p. 30). They also claim that over the centuries, all important traits have been encoded in natural languages. In their view, because factor analysis can find latent dimensions underlying these person-in-the-street trait labels, we can, in principle, isolate real categories for the psychology of personality. McCrae and Costa (1997) use evidence for the cross-cultural consistency of the NEO-PI to claim that they have discovered the universal structure of personality.

Not all philosophers and psychologists would agree with Costa and McCrae’s assessment. For example, in what he calls the fallacy of the perfect dictionary, Alfred North Whitehead (1938) criticized the pernicious idea that humans have “consciously entertained all the fundamental ideas that are applicable to [their] experience” and encoded them in language (p. 173). With respect to the NEO-PI, Tellegen (1993) convincingly argues that Costa and McCrae’s exclusion criteria led to an item pool that failed to adequately sample the personality descriptions used in natural languages. For example, their exclusion criteria eliminated both evaluative terms such as *pretentious* or *charismatic*, and mood states such as *happy* and *fearful*. Almagor, Tellegen, and Waller (1995) claim that the initial narrowness of the Costa and McCrae item pool explains why John (1989) could not classify traits such as *independent*, *peculiar*, and *conservative* on the Big Five dimensions.

Tellegen also argues that the *a priori* elimination of evaluative and mood terms from what is supposed to be a comprehensive index of folk personality descriptions weakens the NEO’s application to be the framework for the DSM’s assessment of maladaptive personality styles. For example, removing evaluative terms eliminates any consideration of self-esteem as a source of individual differences. Ben-Porath and Waller (1992) astutely claim that NEO-PI still needs to demonstrate incremental validity above and beyond

standard clinical measures such as the Minnesota Multiphasic Personality Inventory to even be *included* in the assessment of psychopathology, let alone to define it.

Supporting Tellegen's claim is the fact that the Big Five use to be the Big Three! Costa and McCrae's (1985) first attempt to measure the structure of personality was called the NEO inventory. It had three factors, Neuroticism, Extroversion, and Openness. Later, Costa and McCrae decided that the structure of personality changed. They added two more factors, Conscientiousness and Agreeableness. It is not unreasonable to expect that, if an improvement is offered, the model will change again. Many clinical and counseling psychologists have ignored these issues. They have ignored them because Costa and McCrae have succeeded in making traits scientifically respectable again, and because the NEO-PI has had great heuristic value with respect to topics for theses, dissertations, and tenure-track publications. The popularity of Costa and McCrae's model has, unfortunately, circumvented needed criticism.

Ironically, dimensional classification is very similar to the anti-diagnostic model of Karl Menninger, who held that the discrete categories described in textbooks cannot help us truly understand people's problems. Instead, he thought we should think in terms of scales or yardsticks. On one end of the scale would be "maladjusted" and on the other end "adjusted." Once people get into a maladjusted range, the mental health professional is supposed to help them figure out how to achieve a greater sense of "normality" (Menninger, Mayman & Pruyser 1963). This recommendation compares favorably with Widiger's (1994) proposal that mental health professionals first assess degree of maladjustment, and then determine the person's position on the basic dimensions of personality to understand the nature of the maladjustment.

Rather than fixed states called disease entities, Menninger claimed that we should think in terms of shifting positions on various yardsticks of personality functioning. In an interesting parallel with *neo-Kraepelinianism*, Menninger calls this view *neo-Jacksonianism*, after J. Hughlings Jackson. By this he means a focus on quantitative (dimensional) rather than qualitative (categorical) distinctions between different kinds of mental illness. What modern day dimensional proponents propose to add to neo-Jacksonian psychiatry is a scientifically-based model of personality functioning.

Personality traits having a biological basis are not natural kinds

Like some physicians, psychometric thinkers may also be vulnerable to confusing the presence of a biological basis with having natural kind status. For example, according to Lykken and Tellegen, (1996), folk constructs such as negativism and happiness and altruism have a biological/genetic basis. Sandra Scarr (1987) also suggests that 24 to 40 percent of the variance in personality stems from heredity. Based on these findings, psychologists might conclude that some psychological traits really exist — as natural kinds.

First, the notion that traits which have a biological basis are traits that really exist has to be rejected by anyone claiming to be a materialist. For materialists, no cognitive or emotional states would exist without brains. According to the materialist's brain-as-substrate thesis, every psychological state has some kind of biological basis. For example, the reason a rock cannot get depressed or do calculus, is that it doesn't have a biological basis for either depression or calculus. Any trait, such as extroversion, or a cognitive-emotional state, such as depression, exists because of a biological predisposition. Every aspect of human psychology has a biological basis.

Second, to say that something has a genetic basis does not mean that it is a discrete entity at the level of DNA. For example, other traits such as *traditionalism, religiosity, well being, delinquency, emotional stability, ego-strength, and time watching television*, have been shown to have a genetic basis as well (Bouchard, Lykken, McGue, Segal & Tellegan 1990; Bouchard & McGue 1990; Cattell, Rao & Schuerger 1985; Prescott, Johnson & McArdle 1991). No one (I hope) suggests that our ancestors evolved a time watching television gene! Dispositions to react to possibilities presented by one's culture may not have evolved with those possibilities in mind.

With respect to watching television, there is probably a biological basis, a protein-synthesizing program creating a nervous system with a cognitive-affective predisposition, that initiates a process which ends up in a person watching more television if they are given the opportunity to do so, but the final point in the process is not the biological basis. The same is true for personality traits. They are end products, and cannot be reduced to inherent necessary and sufficient criteria. Genetics may be necessary as process initiators, but they are not sufficient causes of most traits.

The philosopher and geneticist Kelly Smith (1999, 2000) has done some good work on the notions of genetic disease and genetic trait. Deciding on these matters is more complicated than many psychologists and psychiatrists

acknowledge. Some psychologists and psychiatrists tend to think that if a disease or trait can be called genetic, then it really exists. According to Smith, however, one cannot claim that something is a genetic disease or a genetic trait unless having the relevant gene or polygenes makes it more likely than not, that the person will develop the disease or trait in question. With respect to the philosophy of medicine, most psychiatric disorders and personality traits cannot legitimately be called “genetic”.

When we look at a trait such as extroversion or a syndrome such as schizophrenia, psychological, behavioral, and biological covariation reflects some kind of coherent organization so consistently that we can say there is something there, we just cannot reduce it only to genetic or biological covariation. There are no inherent properties that make traits and syndromes be what they are. They are practical kinds.

5. Conclusion

There are no necessary and sufficient internal conditions of objects that makes them be something like a chair. Chairs are not natural kinds. There are many reasons for refusing to believe that syndromes, diseases, species, and personality traits are natural kinds as well. This is a property of any categorical system that can be considered to be continuous or dimensional. Neither the traditional medical model and its methods for isolating disease nor the psychologist’s mathematical approach to classification have succeeded in isolating what could be called natural kinds. Both have discovered stable patterns that are more than mere inventions, but the idea of an isolated inherent reality, definable by using only biological experimentation and sophisticated statistical analysis, is mistaken. We need too many other variables and kinds of evidence to individuate patterns, and adopting different methods or evidential priorities can alter the patterns we find.

Meehl (1986) discussed the advantages of thinking of diagnostic taxa as open concepts, and considered any other strategy to be “scientifically malignant” (p. 220). Reality is always going to be more complex than what is captured by our categories. No matter how specifically we define disorders such as schizophrenia, we will always have to admit exceptions — cases that do not fit the model. The more specific the criteria, the more exceptions we can expect. We can avoid the problem of exceptions by using broader defini-

tions, but that would lower reliability. Using the terms discussed in this chapter, thinking of our categories as natural kinds, as closed absolute concepts, is unwarranted. Scientific openness to evidence is supported better by considering psychiatric categories to be practical and not natural kinds.

CHAPTER 11

Psychiatry and Reality

1. Introduction

The problem of reality is one of the most difficult issues faced by clinicians and philosophers alike. In psychiatry and the allied mental health professions, reality is usually considered both in (a) evaluating the severity of maladjustment and (b) in debates about the value of psychological understanding and explanation. This latter problem is the one that is also of concern to philosophers.

(a) In the Freudian ego psychology model, psychopathology is defined as loss of contact with reality. Those persons more out of touch with reality are more pathological. A person in a psychotic state is supposedly more out of touch with reality than someone who simply engages in “neurotic” distortion. According to the ego psychologists, we distort our perception of reality by using defense mechanisms. Defensiveness involves the distortion of either internal reality (facts about the self) or external reality (facts about the world).

Humanistic thinkers also think of maladjustment in terms of reality *distortion*, identifying it using terms such as incongruence, *perceptual distortions*, and denial (Meador and Rogers 1984). Cognitive behaviorists conceptualize maladjustment in terms of logical errors, *cognitive distortions*, and dysfunctional schemas (Beck & Weishaar 1995).

The problem of reality distortion was much more prominent in the late 1960s than it is today. The DSM-II even offered an alternative to the traditional “loss of contact with reality” definition of psychosis. The alternative definition described psychosis as “the complete inability to adapt to the demands of life.” Because *complete inability* is as vague as *reality distortion*, the second definition did not help clarify matters. The shift to a more “objective” biologically-based psychiatry in the 1980s minimized the importance of the concept of reality distortion because it transformed the field’s working assumptions about

nature of the underlying pathological process. Distorted perceptual processes were replaced with broken brains. Behavioral approaches played a similar minimizing role with respect to reality distortion in psychology.

Even for biomedical materialists and especially for behaviorists, reality is still an issue with respect to asking what constitutes a real disorder and a real explanation. The reality or unreality of consciousness, beliefs, thoughts, desires, emotions, personalities, and mental illness itself has been continually debated throughout the history of both psychiatry and scientific psychology. Although complicated enough in the past, with the dominance of the brain-as-substrate thesis, it is even more complicated today. If all psychological states are brain states at some level of analysis, it seems logical to draw the conclusion that physical brain states are what is really real.

As argued in the last chapter, both dimensional models and prototype models of categorization suggest that maladaptive states such as depression and schizophrenia cannot be identified and conceptualized only with reference to necessary and sufficient biological conditions. For example, if genetics provides a vulnerability for developing disorganized schizophrenia, which we call schizotypal personality disorder, we also have to decide at what point schizotypal traits become schizophrenia proper. Information about adaptiveness and about phenomenology are therefore important aspects of answering questions about what schizophrenia is. A “levels of analysis” analysis as presented in Chapter 6 would also lead us to think that the biological level is only one level of reality. A working understanding of dimensional models, prototype theory, and a levels of analysis analysis is, unfortunately, not reflected in the language of many mental health professionals.

2. Having neurological effects does not make psychology neuroscience

As stated in Chapter 2, those thinkers inclined toward biomedical materialism are also inclined to believe that the biological level of analysis is the really real level of analysis. For example, in discussing masked depression, Wender and Klein (1981) write: “Obviously, in the absence of any independent way of ascertaining whether depression truly exists, such as a blood test or a measure of brain activity, labeling these disorders as masked depressions must remain speculative” (p. 64). The most robust proponents of biomedical materialism hold that if psychological states are attributes of the brain, then they can be

thought of as really being brain states. From their perspective, neuroscientific hypotheses about the brain are hypotheses about what actually exists and through neuroscience we will find out the truth about psychology.

The idea that the brain is what is really real often takes the form of a common sense identity theory, where psychology is collapsed into biology.

One's feelings and thoughts are as biological as one's blood pressure or gastric secretion: feelings and thoughts are manifestations of the brain's operations just as blood pressure reflects the operations of the cardiovascular system and gastric secretion the stomach's function (Guze 1992, p.130).

Psychotherapy can properly be thought of as a biological technique that fits neatly into the medical model of mental illness.... Whatever the details, effective psychotherapy has to be considered a method of changing synaptic transmission (Lickey and Gordon 1991, p. 362).

Medication, dream interpretation, and empathy simply become different ways to alter different neurotransmitters, presumably in different parts of the brain (Mohl 1987, p. 325).

Psychotherapy involves exploring and changing the connections between the interconnected neurons of the upper cortex that make up our minds (Vaughan 1997, p. 57).

From the supervenience perspective, this kind of analysis is unobjectionable. Psychological processes such as memory and attention are functions of the brain, therefore any change in memory or attention involves a change in the brain. These claims are partly congruent with my contention that subjectivity is a biological fact. What is objectionable is the attempt to justify the psychological *because* it expresses brain functions. Some thinkers mistakenly conclude that psychological interventions are not scientifically respectable by themselves, so in addition to doing what they are supposed to do, they gain more credence if they are described in terms of the brain, however vague the connection.

Common sense ideas about the ontological priority of the biological over the psychological are even held by thinkers putatively trained in defining problems psychologically.

A group of psychologists were discussing post-traumatic stress disorder (PTSD). A few months earlier these psychologists had responded as an agency to a community crisis and were well-informed on the course of PTSD. They were all aware that it begins with an event that is extraordinarily stressful; that a person has to be able to process the event in a timely manner or certain symptoms such as flashbacks, insomnia, and an exaggerated startle response can occur; and if these symptoms are not attended to rather quickly,

treatment may take a long time and be very difficult.

One of the psychologists, who had recently been to a conference on trauma and critical incident debriefing procedures, reported that researchers have shown that if trauma occurs and is not re-processed in some way, there are measurable structural changes in the brain, and these changes make the disorder very difficult to treat. "Well that makes a lot of sense" said another psychologist, "that really helps me understand it." Others around the room nodded their approval.

One reason I am attributing these beliefs to common sense is that, even for doctoral-level therapists, folk materialism (Chapter 4) suggests that any psychological process that can be identified with brain states has a greater claim to being a real psychological process than one which cannot. Of course, this particular biological explanation added nothing to the functional analysis that the psychologists had already mastered.

On a common sense level, these various biomedical theses appear to be philosophically sound, and even congruent with my "psychology is explanation with reference to what is in the head" anchor. A little reflection, however, indicates that they do not necessarily follow from the brain as substrate thesis, even for committed materialists.

It is a category mistake of the first order to refer to the ontology of the identity theory (psychological states are brain states) and claim that, in utilizing psychological concepts, mental health professionals are just talking about brain states under another description. It is a category mistake because psychology and neuroscience are not transitive, you cannot just replace one for other. If they were transitive, mind-brain identity would work both ways. If psychotherapy is a biological treatment, then pharmacotherapy is a psychological treatment. Yet no one claims that prescribing Prozac constitutes psychotherapy. Someone specializing only in pharmacological approaches to mental illness cannot responsibly claim to be a psychotherapist or vice versa. Mohl (1987) and Lickey and Gordon (1991) are therefore mistaken in defining psychotherapy as a "biological treatment."

The difference can be demonstrated through a *reductio ad absurdum*. For example, after reading Kandel's (1983) "metapsychology to molecular biology" article and concluding that successful behavior modification alters an organism's microbiology, few university deans would interview a behavioral analyst for a job in the microbiology department. Behavior modification is not about biology and neither is psychotherapy.

If biomedical psychiatrists can claim that, in attending to suicidal ideation

and panic attacks, they are focusing on brain states, then psychoanalytic psychiatrists can claim that in interpreting Oedipal conflicts and mapping defense structures, they too are focusing on brain states. Defense mechanisms could be considered as biological as deteriorated nigrastriatal pathways. They are, however, not the same, and one does not have to reject materialism to keep them separate.

Claiming that psychotherapy is a biological treatment makes the mistake of minimizing differences between the psychological and biological levels of analysis. Paradoxically, accepting that there is a difference between psychology and biology is a necessary condition for any materialistic devaluation of psychology. Biomedical materialists who refuse to acknowledge a difference between the psychological and the biological should abandon the anthropomorphic devaluation of psychology. Those who accept the difference, e.g., *we need more neuroscience and less psychology*, cannot equate knowing feelings and thoughts with knowing blood pressure and brain dopamine levels.

3. Literally changing the brain is not the only way to change psychology

Folk materialism accords well with biomedical materialism's claim that all valid psychiatric syndromes are biopathological processes. Psychological and social variables may have an influence on the development of the problem, but its etiology, with respect to a cause which has to be present in order for the disorder to occur, is biological. Andreasen and Olsen (1982) even claim that psychological content may be irrelevant to treating the actual problem. For example, it is not diagnostically important if someone suffering from general paresis thinks that their brother is plotting to take over the family business. A psychodynamically-informed exploration of the fantasies and self-object configurations being expressed in delusions will not cure general paresis; what is important is treating the underlying brain disease that is the cause of the delusional symptom.

A similar view is suggested by Dennett with respect when the intentional stance (explaining behavior with respect to beliefs and desires) fails. Dennett says that using the intentional stance requires assuming that the person is rational or reasonable. If no reasons for the behavior in question can be found, we have to forgo the intentional stance and adopt the design stance,

looking at what the system in question is supposed to do and determine where it has broken down.

The biomedical materialists' broken brain perspective claims that there are no psychological reasons for pathological syndromes, they are the result of a break down in the machinery of the brain, and any other kind of explanation is probably confabulatory. Especially with respect to general paresis, the biomedical materialists are probably correct. After examining a couple of more cases that cohere with the biomedical materialists' treatment philosophy, and explicitly defining that philosophy, I will review some cases that do not cohere with the biomedical materialist viewpoint.

A prominent example of a false psychological explanation is Charcot's belief that Parkinson's disease was the result of violent moral emotions originating from reactions to political unrest in eighteenth century France (Roth and Kroll 1986). Today, we can all see that it is better to believe that the pathological process underlying Parkinson's disease is a degeneration of dopamine producing neurons whose cell bodies begin in the substantia nigra and extend to the caudate nucleus.

Another important example of a false psychological explanation occurs with respect to denial of deficit in persons with a right-hemisphere brain injury. A person-with-a-brain-injury's refusal to believe that his impulsiveness might interfere with his ability to get back his old job as a diplomat, rather than being a function of his not wanting to admit a deficit, might be a function of a neurologically-based problem with self-monitoring which interferes with the normal updating of his self-image. In this case we would say that the best reasons we can give for this person's behavior follow from the logic of neuroscience, not the logic of psychology.

Biomedical materialism claims that because the brain is always a factor in psychological disorders, the more we focus on the brain, the greater the gain. This argument, *the brain is the final common pathway* argument, is used by both philosophers and psychiatrists. In their shared view, whatever "causes" we may hypothesize to be the determining factors of psychological states, these causes all exert their influence by affecting the brain. Gorenstein (1992) has a similar, quasi-biomedical view, claiming that explaining mental illness requires tracing it back to its origin in the *abiding properties* of the central nervous system.¹⁸

18. Gorenstein also uses Skinner's (1974) term *conceptual nervous system*, which is a promising approach for reminding professionals that brain activity can be conceptualized on multiple levels of analysis.

Another consequence of the final common pathway argument is that treatments which target the brain directly are considered to be direct routes to the pathological process. Biomedical treatment philosophies also provide the moral justification for biomedical research programs, specifically in the form of a claim that it is inhumane to not realize when problems are biological in nature. Guze (1992) claims that the insurance companies are able to discriminate against patients with psychiatric disorders primarily because the general public believes that psychiatric disorders are not real illnesses. In his view, it makes no sense to discriminate against patients whose hallucinations and delusions are the result of a brain disorder called schizophrenia while at the same time providing for patients whose hallucinations and delusions are the result of encephalitis. He argues that if psychiatric disorders were recognized as true illness, the insurance companies would have no justification for their policies of discrimination (see Chapter 12 for a criticism of this argument).

The pragmatics of treatment, however, are not as neat and tidy as biomedical case examples indicate. For example, certain disorders have relatively more biological determinants (e.g., manic-depression, mental retardation) and other disorders have relatively more psychosocial determinants (e.g., multiple personality disorder, PTSD). Although it is biologically instigated, severe mania is a problem because it interferes with adequate psychosocial adaptation. Although it is psychosocially instigated, if untreated, something such as PTSD can evolve into a biologically-based problem in regulating feelings of security. Keeping these complexities in mind, I am not going to offer general theoretical explanations for specific disorders. It is a mistake to assume that there is any tight uniformity between the different kinds of psychiatric disorders.

Biomedical materialists and eliminative materialists, however, both assume more uniformity between disorders than the evidence warrants. As stated in Chapter 2, the exemplar disorder for biomedical materialism is general paresis of the insane. At one time general paresis was a psychiatric disorder, but it has now been shown to be the result of untreated syphilis. Although its psychological consequences are part of the reason we want to treat syphilis, they are consequences of an underlying biopathological process which itself should be the actual focus of treatment.

Paul Churchland (1995) also has his preferred exemplar, specifically, traumatic brain injury. He prefaces his own discussion of psychiatric issues with examples of "nature's experiments" in the form of closed head injuries, strokes, genetically programmed deteriorations, and drug-induced brain dam-

age. In his discussion of the brain in trouble, he moves from traumatic brain injury to schizophrenia without taking a breath. In discussing the pharmacology of depression and the bipolar disorders, he claims that genetic vulnerabilities and stress are integrated in a neurochemical nexus (the final common pathway). He suggest that stress is partly a function of inferior social position, and reviews studies by Raleigh, McGuire, Brammer, and Yuwiler (1984) that indicate that social position is the direct result of serotonin levels in the brain. Just like Andreasen (1988), he identifies all important causal variables as physiological, genetic or anatomical, differing from the biomedical materialists primarily in using brain injury rather than general paresis as his primary exemplar.

The mistake, and it is a subtle one, is the uniformity myth that leads eliminativists to conceptualize all cases of psychiatric disorder using either the general paresis or the traumatic brain injury models (and hence reduce psychiatry to neurology). As stated, I do not intend to engage in disputes about the nature of any particular psychiatric disorder, nor is it necessary to do so to make my point. I only need show (a) that there are additional exemplars that lead us to question the common sense assumption that directly modifying the brain is always the most effective, elegant, and ethical form of treatment, and (b) that these exemplars do not conflict with the brain as substrate theses.

The first exemplar is the temper-tantrum exemplar and refers to behavior that is maladaptive, but also developmentally normal. Most temper-tantrums are not disorders. At the same time, they are psychological problems. The point of the temper-tantrum exemplar is that, although we can change behavior by modifying the brain directly, direct brain modification itself is not necessarily as elegant (or ethical) as is claimed.

A typical two-year old's temper-tantrum in the grocery store is the result of a brain state. One reason that toddlers may cry uncontrollably, get enraged, and roll around on the floor has to do with the states of their brains. Dealing with the temper-tantrum, however, by directly altering the child's brain physiology, although convenient, would be foolish. Children have to learn (a) to talk about feelings rather than act on them, (b) that they cannot get what they want all the time, (c) that they have to accept compromises, and (d) that there are more appropriate ways to receive attention (Bath 1994). In these cases, the concepts delay of gratification, maturity, and behavioral extinction are more useful than detailed hypotheses about hypothalamus, thalamus, anterior cingulum, hippocampus, and amygdala activation as discussed by Mandoki,

Sumner, & Matthews-Ferrari (1992). Not reinforcing the temper-tantrum would be a more elegant solution than directly intervening at the level of brain physiology.

The second exemplar is the self-concept and brain injury exemplar. As stated in Chapter 8, the concept of reactions is important in psychiatry. Many problems associated with traumatic brain injuries are best understood in terms of reactions rather than physiology. For example, a major task for the person with a brain injury is dealing with the loss of the pre-injury self. How the person valued their pre-injury self will be an important influence on their post-injury adaptation.

After an aneurism, Tavis experienced troubling physical deficits, including coordination problems. Before the injury he was very involved with music, which was also a valued family activity, especially for his mother. Being unable to perform as he use to perform was so upsetting to Tavis that experienced professionals feared he was a future suicide case. In discussing his feelings of loss, Tavis would reflect on his friend Sally who, although suffering a similar injury, had lost the ability to accurately perceive herself, and was thus unaware of how much she was changed by the injury. Tavis would alternate between thinking he was lucky for still having his awareness, and then thinking that Sally was lucky for not having to experience the pain of mourning the loss of her pre-injury abilities.

Helping Tavis cope with his brain injury required more than neuroscience. Having social support in terms of family and friends was important to his initial recovery, perhaps just as important in the long run as state-of-the-art rehabilitation. Also important were therapeutic interventions that educated him about the importance of dealing with loss of self. Although Tavis continued to be upset by his physical deficits, involved professionals soon ceased to worry about imminent suicide. In addition to having social support, being able to think about his anxiety as a loss of self issue and adopting the goal of forming new ideal “possible selves” helped him cope. Although it may have been useful to have a self-esteem pill or an anti-suicide pill for Tavis, the interventions used were much more elegant.

The third exemplar is the problems-in-living exemplar. It addresses the strategy of changing the environment in order to affect psychology. Laura was referred for a psychiatric consultation because some of her children were coming to school showing signs of being neglected. The psychiatrist checked for a history of depression in the family, and asked about vegetative states such as sleeping habits, etc. At the end of the session the psychiatrist wrote

Laura a prescription for medication. When social workers later interviewed Laura, they discovered that her husband had died in an accident in the past six months, learned that she had five children at home under the age of six, found out that she was unemployed and had no job or work related skills, and determined that she had no social support in the area. The social workers were able to postpone the medication and make alternative interventions such as child care, job training, peer support, and public assistance. The interventions were successful and medication was not prescribed.

Using similar examples drawn from everyday life, Wender and Klein (1981) claim that a good deal of unhappiness in people's lives is a realistic reaction to bizarre, unnatural, or inadequate social-cultural contexts. It is never a waste of time for the mental health professional to consider these external factors when evaluating individual reactions. We should not automatically medicalize psychiatric problems when alternative solutions may be available.

Sometimes, we are constituted to react in certain ways to specific situations, and "abnormal behavior" can better be attributed to an understandable reaction to an abnormal situation than attributed to some abnormality in the person. Humanistic approaches which minimize pathology and maximize normalization and positive regard rely heavily on these kinds of exemplars. Social psychologists have also pointed out that people tend to blame other's mistakes on factors inside them, but blame their own mistakes on situational factors. Those who label this the fundamental attribution error think we should look more at the situational factors of others. (Those who label this the self-serving bias think we need to look more at our own contributions to our own problems.)

The problems-in-living exemplar also challenges my claim that psychology involves explanation with reference to what is in the head (Chapter 6). As far as the DSM is considered, psychiatric disorders must refer to a dysfunction *in the person*. With his idea of mental illness as "harmful" dysfunction, Wakefield (1993) states that some internal mechanism must not be working properly. Humanistically-oriented mental health professionals have long found this disturbing. What is disturbing to them is that clinicians who fail to check for situational causes may incorrectly treat normal but disabling reactions as psychiatric *illnesses*.

The fourth exemplar is the interpersonal intervention exemplar. Paul came into therapy a harried man. He was high strung and jumpy. He com-

plained of depression, but medication had provided limited benefit. For most of the session he discussed his problems negotiating the competing demands of his wife and his mother, who barely spoke to each other. Both women wanted control of Paul's life. One day in therapy Paul was discussing a problem with his son when his therapist looked up and stated, "Right now I really feel like telling you what to do, but I don't think I will." This confused Paul who wanted to know what his therapist thought he should do. The therapist admitted that the inclination to tell Paul what to do was strong, and that he was not usually inclined to tell clients what to do. He wasn't sure what it was, but something about Paul made him want to take control. He wondered if this attribute of Paul's was also playing a role in his problems with his mother and his wife. This observation struck Paul. It instigated a period of careful self-observation and evaluation that led him to learn about his contribution to the problem of being domineered. In time, he developed strategies for changing his interpersonal style. His mother and wife became less domineering and as a result, Paul felt a greater sense of control over his life. His depression dissipated and therapy was terminated.

The fifth exemplar is the pharmacologically-initiated-insight exemplar. It involves concepts we use to understand the efficacy of particular biological treatments. In an intriguing marriage of psychological treatment and biological treatment, Kramer (1993) discusses a rejection-sensitive patient who was helped to become less rejection-sensitive with Prozac. After the Prozac was discontinued, she was able to make changes based partly on the insights she gained about her relationship dynamics from being less sensitive while on the drug. Although Prozac was an important part of the treatment, it was not a sufficient cause of this person's eventual improvement. Kramer also believes that the people who he classifies as "good Prozac responders" were psychologically ready to become "better than well" because they had undergone extensive psychotherapy. Those who have not gained the insight into themselves offered by therapy may not be able to have "better than well" responses to Prozac.

Rather than proposing these exemplars as competitors to general paresis and traumatic brain injury (TBI), I am suggesting that biomedical and eliminative materialists are mistaken in trying to fit all psychiatric problems into the Procrustean beds of syphilis and TBI. According to Kuhn (1970), fields do not just have one exemplar, they have many exemplars — which become a group of classic solutions. By being familiar with all the classic solutions, profession-

als gain the capacity to see similarities between old problems and new problems, leading to an equally successful solution of new problems. Laudan's (1977) idea of multiple research traditions in any single science is also consistent with this kind of pluralism. Contradicting biomedical materialism's own common sense, elegant solutions are best defined as maximally natural, relatively permanent, and time efficient. Both biological and psychological solutions to psychiatric problem can be elegant under the right conditions.

4. Psychiatry and practical problem solving

William James's (1907) distinction between theories (or exemplars) as "ultimate answers" to enigmas and theories as "instruments" helps clarify their role in psychiatry. As instruments, theories are cognitive tools we use to make a difference. In psychiatry and psychology the difference that makes a difference involves a change of behavior and consciousness in such a way that we can *evaluate* the person in question, in the long run, as better than before. Since Aristotle first addressed the issue, Western philosophers have known that bringing about a "better" state of affairs involves practical problem solving.

Practical problem solving is a function of experience, what Sternberg, Wagner, Williams, and Hovarth (1995) call action-oriented knowledge and what the DSM-IV calls *clinical judgement*. Psychological research also shows that, in general, as one gains more experience with practical problems, one's ability to solve them increases. One of the things that happens with practice is that the relevant facts and the strategies for juggling them become more available. What goes unnoticed or appears superfluous from the arm chair, gains cash value in practice. The fact that practical problem solving ability increases until late adulthood, whereas academic abilities begin to decline in early and middle-adulthood, also suggests that action-oriented knowledge constitutes a separate system of knowledge (Cornelius & Caspi 1987; Denny and Palmer 1981).

An example of practical reality confronting arm-chair theory is given by Paul Churchland (1995) himself, who writes that he was surprised to find that his own children were naturally inclined to understand complex psychological concepts such as "anger, promises, friendship, ownership and love" and that they did so before understanding something as physically simple as the basic

color categories. That children generate some psychological concepts on their own is predicted by the maturational-evolutionary view of psychological mindedness (Chapter 6). Similar realities confront the practicing psychiatrist, psychologist, and social worker every day.

5. So Where Is Reality?

In his definition of *basic realism*, Lakoff (1987) includes criteria such as (a) there is a world external to human beings, (b) the world is somehow the cause of our knowledge, and (c) a rejection of the view that any belief system is as good as any other belief system (i.e., whatever goes relativism). Basic realism is consistent with the claims I am making in this book. It is also consistent with scientific psychiatry, but represents a much weaker realism than that suggested by the biomedical materialists. The biomedical materialists claim that psychiatric disorders are really broken brains, indicating that the biological description of psychiatric disorders are the only kinds of explanations we need. Pathological processes are there to be discovered, just like buried artifacts are there to be discovered. In their view, once we discover those processes, we will know what psychiatric disorders really are.

This kind of realism is a function of accepting the *correspondence theory of truth*, which is the common sense view of truth. It is also a metaphysical and essentialistic view of truth. According to the correspondence theory, truth equals correspondence to reality. Our beliefs are true “if they represent the world as it really” is or if they “correspond to the facts.”

Philosophers have pointed out that the correspondence theory of truth does not account for everything that we call true. It is a fine model of truth, but it is not universally applicable. For example, Rorty (1982) notes that the truths of logical propositions such as “a thing cannot be X and not-X at the same time and in the same respect” or “either it is or is not raining” do not depend on simple correspondence. Nor does simple correspondence to the facts explain the truth of evaluative statements such as “people should be just in all their dealings” and “the end does not justify the means.”

The *coherence theory of truth* is a logical view of truth, looking at the rules we use to decide what can be true. It is a way to decide if something is a legitimate candidate for being true. According to the coherence theory, truth candidates have to be consistent with other things we believe in order to be

taken as true. Those that are not consistent with other beliefs are not taken to be acceptable candidates. If I tell you that I saw a live gargoyle on the roof of the church, that statement probably does not cohere with your other beliefs. In the name of coherence, your initial assumption is going to be that I was dreaming, hallucinating, or that the “live gargoyle” was a trick of light, but I didn’t really see it because live gargoyles don’t exist. Coherence is a species of correspondence where beliefs have to correspond with each other or with the evidence, but it occurs in the context of checking for consistency (fitting it all together) rather than checking reality.

One version of the coherence view of truth in psychology is called establishing construct validity. In establishing construct validity for a test, psychologists make predictions about what other constructs should logically cohere with the test in question, and then see if those predictions do in fact obtain (convergent validity). They also make predictions about what constructs should not cohere, and see if those predictions are borne out as well (discriminant validity). Those who favor the natural science model want coherence to be lawful.

When an anti-anthropomorphic thinker such as Andreasen (1984) defines psychological concepts such as “loss of ego-boundaries” and “lack of self esteem” as “metaphors used to describe biological processes” (p. 132), she is framing her inquiry within pre-existing beliefs about reality. For her, psychiatric reality is biological. What she takes to be truth candidates are going to have to cohere with that belief. Hence, the strategy of redefining all problem into problems of neuroscience. Although biomedical materialists often talk as if they have a correspondence view, of truth, in practice they rely heavily on the coherence theory.

The *pragmatic theory of truth* is a non-essentialistic, psychological view of truth. This version of Jamesian pragmatism is Darwinian as well. Beliefs and theories that are adaptive tend to survive, i.e., are taken to be true — or selected.

Once something becomes a truth candidate we have to justify it. The pragmatic theory states that, for us, knowing “the truth” is never separable from its justification. For a proposition or a belief to be taken as true, we have to be able to successfully act on it. To illustrate, the reason that I don’t believe that I can walk through walls is that I cannot successfully act on that belief. The reason I believe that if I flip the lamp switch when I get home at night, a light will go on, is because I can successfully act on that belief. Empirically

considered, what we take to be true depends on what we can justify, e.g., I can't justify a belief that I can walk through walls. We have to try beliefs out, experiment with them so to speak. If a successful application of a belief can be replicated over and over again, we take it to be robustly true. In the pragmatic theory, the relation between truth and reality is reversed. Rather than correspondence with reality being the only standard for deciding truth, taking a proposition as true (validated by experience) is a precondition for attributing reality to the entities it describes.

A pragmatic orientation is an important component of the scientific conception of truth, especially for scientists who take skepticism seriously. The goal of science is to find the best possible explanation by systematically eliminating alternative explanations. Scientists test their hypotheses by pitting alternatives against each other. Once they find alternatives that work, however, scientists always have to be open to the possibility of a better explanation. There is nothing to scientific truth beyond justification. Karl Popper even defined science as a process of conjectures and refutations.¹⁹ Eternal skepticism about the truths of science rather than dogmatically defending those truths is what is supposed to separate science from theology, and eternal scepticism is inconsistent with strict correspondence to reality.

6. Science and Realism

As argued in Chapter 10, like diseases and species, it is probably a mistake to think of psychiatric categories as natural kinds. Biomedical and eliminative materialists suggest that any legitimate psychiatric disorder is a natural kind, and that its necessary and sufficient conditions are biopathological processes. This is related to a version of scientific realism that states that the more biological or physical an explanation is, the more scientifically valid it is, and therefore the more it corresponds to reality, i.e., carves nature at the joints.

The difference between myself and Paul Churchland (1992) is that he suggests that psychiatrists can really “carve nature at her systematic joints” (p. 287). From a pragmatist's perspective, the metaphor *carving nature at the joints* or finding *the laws of nature*, is overly ambitious. Thinking of concepts as tools, we can definitely develop better tools, but “better” is a long way from

19. Popper was a scientific realist who thought that theories were refuted by nature.

“correspondence to reality.” Avoiding common sense realism in terms of “carving nature at the joints” is one advantage a term such as co-evolution has over a term such as “successive approximations.”

If pressed, Paul Churchland may agree with me. Like Daniel Dennett, he is a master at showing how concepts that seemed unassailable in the past have been abandoned because of new discoveries. He knows that the development of Newtonian physics depended on the availability of calculus as a tool for modeling physical phenomena. For hundreds of years, scientists assumed that Newton had discovered God’s own laws. Einstein’s alternative conception, based in part on the development of a non-Euclidean geometry that allowed him to model events in four dimensions, showed that Newton’s theory was not universally true. Quantum theory shows the same thing at the subatomic level. As was the case for Newton, it is a live possibility that some as yet to be developed mathematical system will provide us with another alternative view of nature, one that can’t yet be imagined. These facts of history are partly responsible for physicists abandoning the idea that they have discovered God’s own laws.

I propose that mental health professionals think of realism in science using the example of Newton-to-Einstein which coheres with a pragmatic theory of truth rather than the example of Ptolemy-to-Copernicus, which coheres with a correspondence theory of truth. Using the exemplar of Copernicus discovering the real organization of the solar system biases professionals to think about mind-independent facts. The exemplar of Einstein’s model solving problems that Newton’s model could not solve, and dissolving some other problems as non-problems helps professionals to think in terms of conceptual validity rather than correspondence to reality. Those who think about validity replace questions about *correspondence* with questions about *generalizability*. Classical mechanics had limited generalizability, and hence, can not be said to correspond to reality in an unqualified sense.

Expanding on the Copernicus example, it is natural for us to believe that Copernicus was wrong to think that planets travel around the sun in circular orbits. Critics of pragmatism claim that pragmatists *have* to believe that Copernicus was right in defining the motions of the planets as circular because that assumption worked for him. But he was really wrong, they are elliptical, so truth is more than justification.²⁰ This complaint has less force against

20. Copernicus was doubly wrong because he still posited epicycles. His theory was an improvement because he posited only 34 epicycles while the Ptolemaic theory posited 80.

pragmatism than it has been given. Pragmatists will define truth as what no one gives us any good reason to doubt. Because we all have good reasons to doubt Copernicus's idea of circular motion, we can't really see him as having true beliefs. The pragmatists' criteria still operate. We believe that the motions of the planets are elliptical because no one has given *us* any good reason to believe something different. If some future astronomy, for whatever reason, argues that the motions of the planets are not elliptical, what we now take to be objectively true will then be taken to be objectively false.

Not only can we think of mathematical models as conceptual tools, we can think of our brains as tools, called epistemic engines. Churchland offers the interesting suggestion that, at some point in the evolution of scientific knowledge, developing better models may require us to alter the very structure of our brains, building better epistemic engines. The belief that there are always ways to improve our knowledge is the heart of scientific skepticism and its healthy mistrust of tradition. This skeptical framework is incompatible with literally claiming to carve nature at the joints.

As the history of physics indicates, new models of classification based on tools that have not yet been proposed may radically alter what we take to be the basic categories of psychiatry. The "cash value" of any classification system and explanatory model will not be established because it literally "carves nature at the joints," but because it has more useful applications. The best system will have a cash value that generalizes to many different kinds of professional problems. With respect to the importance of cash value, the eliminativists and I are in agreement. In contradistinction to them, I think a broadly considered co-evolutionary perspective, which accepts multiple levels of analysis, explanatory pluralism, the ecology of neuroscience, and molar explanation (in the evolutionary sense of the term) are all important to psychiatry. The philosopher Abraham Edel (1974) succinctly described the situation over twenty years ago: "the mantle of 'reality' is the reward waiting for variables that will turn out to occupy strategic roles in explanation" (p.970).

7. Conclusion

It would be a mistake to reject the claim of the biomedical materialists that a disorder such as general paresis represents a useful exemplar for psychiatrists. At the same time, there is no evidence that all psychiatric disorders can be best

understood as variations on this single exemplar. It has limited generalizability. Rather than disputing the worth of the general paresis exemplar, I offered additional exemplars which showed that directly modifying the brain is not always the most elegant way to modify psychology.

The practical problems of psychiatry are incredibly complex, and like all practical problems involving achievement of the good, or bringing about a better state of affairs, extra-scientific evaluations are required. Any responsible person confronted with actual psychological problems, from the philosopher become parent to the general physician confronted with a case of depression, has to consider all the evidence available to her or him rather than attempting to reduce every problem to a problem of physiology. The difficulty of integrating multiple levels of analysis is no excuse for “nothing but” reductionism.

Folk materialism leads people to believe that biological explanations are the real explanations, but this ontological bias is not scientifically required. General paresis was not made more real by the discovery of the syphilis spirochete. The history of science tell us that the theories we develop in order to understand the world always have to be taken as provisional; it could turn out that the world is not like our theories say it is. What we are left with is assessing how well theories work.

As argued in Chapter 10, the reality of psychiatric disorders is more like the reality of species and diseases. Like species and diseases in general, psychiatric disorders are not natural kinds, but they highlight important regularities. They are not arbitrary and we just don't make them up. We pin down regularities by becoming involved with our subject matter and by trying out different ideas and seeing what works — or to use Francis Bacon's phrase, we have to twist the lion's tale.

This pragmatic view also supports the claim made in Chapter 8, that psychiatry's psychological core follows from the fact that psychiatrists are not mere spectators. They have to interact with people who have psychiatric disorders, and hence use the kind of “folk psychology” that arm-chair philosophers can more easily reject. Other concepts such as species recognition, ego-strength, self-organization, and narcissism also gain justification in practice.

Ian Hacking (1987) claims that an experiment has a life of its own. He thinks that too much of the history of science focuses on what scientists have thought, ignoring the importance of what they did. What we call scientific truths arise from interactions with the world, and they gain their justification

in those interactions. If our concepts and exemplars help us interact successfully with what Popper called medium dry sized goods such as pebbles, tables, and people, then they are as real as we need them to be. If you can spray an electron, then it is real. If you can strengthen an ego, then it is real. A more consistent Baconian science would be much less anti-anthropomorphic than what is offered us by biomedical and eliminative materialists.

CHAPTER 12

Psychiatry and the Rhetoric of Morality

1. Introduction

In addition to claiming that biomedical materialism is a theory about the nature of science and science's value to psychiatry, I claim that in making assertions about moral and immoral treatment approaches, it contains an ethics. Believing that the role of the psychiatrist is to reduce suffering caused by "mental illness" is the first guiding principle of biomedical materialist morality. Although complicated when applied to disorders such as psychopathy, this principle helps psychiatrists avoid the guru-attitude that is endemic in the mental health professions. Guru-ism begins when professionals start believing that they can teach people how to live optimally. The second guiding principle of biomedical materialist morality is the traditional Hippocratic dictum to do no harm.

From this well-anchored starting point, both biomedical and eliminative materialists contend that it is inhumane to ignore the fact that psychiatric problems are biologically-based. They claim that accurate, biologically-based conceptualizations support successful treatment while inaccurate, psychologically-based conceptualizations are iatrogenic. Mental health professionals who rely on radically false psychological concepts therefore harm their patients. Torrey (1992) goes so far as to claim that Freudian theory alone has had a "malignant effect" on American culture, being directly responsible for the narcissistic belief that personal happiness is the greatest good, the belief that we are not responsible for our actions, and our misogynistic denigration of women. Like Torrey, other biomedical materialists think that psychiatrists have a duty to educate the population about the biological basis of psychiatric disorders because in addition to its targeted iatrogenic effects, common sense psychology has a widespread negative influence on social policy. They be-

lieve that although demonstrably wrong, psychological analyses dominate the thinking of too many policy makers.

Also important to biomedical materialist morality is a belief in scientific progress, and the notion that accurate scientific knowledge can contribute to the creation of a more Utopian society. As indicated in Chapter 5, this view partly defines scientism and is not limited to biomedical materialists. It was important for Skinner as well. Progressive Utopianism is derived from the 18th century natural theologian's model of evolution as "development toward a state of perfection." This view of evolution grew out of Thomas Aquinas's emphasis on Aristotle's *scala naturae*. It is robustly non-Darwinian and paradoxically non-scientific.

Although persuasive, the biomedical materialists' case for the moral superiority of their model has several fatal flaws. The thesis of this chapter is that biomedical approaches do not have a unique claim to being the most humanitarian approach we can take toward psychiatric problems. Like psychological approaches, they can also be used to blame patients and support harmful social policies. Moral treatment is consistent with both psychological and biological approaches to problems, and immoral treatment can result from both approaches as well.

2. Stigmatization Is a Psychological Problem That Is Independent of Etiological Models

In making their claims for the benefits of biomedical approaches to psychiatric problems, biomedical materialists appeal to the sentiments of the crowd. Andreasen (1984) and Torrey (1983) provide heart rendering sketches of their patients, offering dramatic characterizations of how an inaccurate understanding of psychiatric illnesses creates fear and blame — ending with the non sequitur conclusion: therefore we should adopt a biological approach to these disorders.

In Andreasen's (1984) sketch about the depressed physician "Bill," Bill is described as a witty, sensitive, and cultured physician who experienced several major depressions. His first case of depression occurred during college. He recovered six months after beginning psychotherapy. Andreasen claims that psychotherapy didn't work, instead, Bill experienced spontaneous improvement. On the other hand, after being placed on medicine for depression during

medical school, he improved in four months. Future bouts of depression, including one following the death of his first wife, were usually removed with medication or hospitalization.

After being abandoned by his second wife and the ensuing chaos associated with that event, Bill became seriously depressed. When anti-depressant medication failed to ameliorate his depressed mood, he was hospitalized and given electro-convulsive therapy. At this time his license to practice was suspended. Although he recovered and was able to resume working, he killed himself two weeks after his license was re-instated. Even though suicidal ideation is a symptom of clinical depression, Andreasen claims that Bill “was killed not by his disease, but by the people around him who misunderstood his illness” (p. 2). In short, Bill’s psychological reaction to social stigmatization resulted in his death. In her opinion, if society would view depression as a disease like it views cancer and kidney failure, Bill would have not been stigmatized and still be alive.

Biomedical materialists such as Torrey (1983) and Winokur (1981) also claim that viewing psychiatric disorders as biological in origin would prevent us from blaming families for causing the illness. Torrey specifically blames psychoanalytic and family interaction theorists for willfully creating the guilt and shame that has led to depression, divorce, and suicide. He claims that these kinds of tragedies have been wholly generated by the psychiatric profession.

Stigmatization and blame are psychosocial processes

Especially among the middle class, having a physically or intellectually disabled child, on-going problems with finances, drug addiction, and serious marital problems can lead to both subtle and blatant rejection of individuals and families. A common form that this isolation takes is when old friends no longer call and exclude you from their social activities. Financial difficulties or having a physically disabled child can lead to rejection even when others specifically believe that the problems is no one’s fault. What they react to is “irregular behavior” experienced as unpleasant in some way.

For example, Prigatano (1986) notes that persons who have personality changes as a result of traumatic brain injuries often end up isolated and rejected. Traumatic brain injuries (TBI) occur as a result of motor vehicle accidents, falls, strokes, and shootings. Although deficits such as speech

problems and loss of coordination are apparent, less obvious psychological problems such as disturbed attention, memory, and practical problem solving, plus emotional lability, increased selfishness, and lack of impulse control are equally pervasive. That brain injuries are physical illnesses is common sense.

Following the injury, loss of former friendships, divorce, and eventual withdrawal from social interaction is common. Those family members who cannot understand the disturbance tend to avoid the person with the injury. Even for those who try to understand the reasons for the irregular behavior, their relationship with the person can be strained. As the frustration and embarrassment about the unusual behavior mounts, family and friends sometimes begin to blame the person for their problems, which takes the form of a claim that they just aren't trying hard enough. They also feel guilty about being so annoyed. Even professionals are sometimes reluctant to work with those head injured persons who are difficult to manage. According to Lezak (1992), the negative reactions (stigmatization and blame) of patients and their family members to the resulting emotional and personality disturbances constitute the most debilitating long-term consequence of traumatic brain injuries.

Contradicting biomedical materialist ideas about etiology, people's negative reactions to a psychiatric disorder in a family member or friend occur for the same reasons that negative reactions to traumatic brain injuries occur. Persons having a psychiatric disorder can be difficult to work and live with, leading to fear, frustration, disappointment, and avoidance (Farina, Thaw, Lovern, & Mangine 1974; Farina & Ring 1965). For both traumatic brain injuries and psychiatric disorders, etiological theories have a limited impact on people's social reactions to deviant behavior and its implications. Martin Willick (1994) a psychoanalytically trained psychiatrist who also accepts a biological model of major mental illness notes that even though he knew better, he still felt shame and humiliation when having to announce his own name when visiting his son who had been hospitalized for schizophrenia. These automatic reactions themselves lead to secondary guilt and shame, such as feeling shame about being ashamed.

Reducing blame, rejection, and isolation are psychological problems

Andreasen and Torrey accurately identify an important issue: rejection and the person's reaction to that rejection. Eliminating the rejection by reducing guilt and fear is a psychological problem, and their proposed solution is what

therapists call a psycho-educational intervention. They claim that educating people about the biological basis of their problems can reduce blame and may even provide hope. They also make a stronger claim, that psycho-educational interventions which focus on the biological nature of psychiatric problems will universally lead both to better treatment of the mentally ill by society, and more positive self-evaluations by patients themselves. This stronger claim, however, has already been empirically refuted.

Mehta and Farina (1997) investigated how people react to and evaluate others, looking at the difference between reactions to persons who self-disclose previous psychiatric problems and persons who do not mention having had psychiatric problems. They show that people react differently to “normals” than they do both to persons who have had psychosocially-caused psychiatric problems and persons who have had biologically-caused psychiatric problems. With respect to the psychiatric problem, confederates in the psychosocial etiology condition and biological etiology condition described themselves as having the same problem, differing only in whether their doctor attributed the problem to psychosocial or biological causes. The psychosocial cause was identified as childhood experiences and the biological cause was identified as a physical disease. After interacting with these confederates in a traditional learning study, the research participants evaluated their stability, self-understanding, and coping skills. There was no difference in their ratings of the psychosocial etiology group and the disease etiology group, and both patient groups were rated as more dysfunctional than normals, even though their performance was experimentally controlled so the two patient groups and the normal group performed exactly the same.

Furthermore, as measured by intensity and duration of administering electrical shock, participants treated Mehta and Farina’s disease group harsher than they treated those in both the normal group and the psychosocial group. The paradox of the study was that participants were less likely to blame those in the disease group for their failures, but they also treated them more harshly. They gave them stronger and longer shocks when errors were made, and their escalation of the shock with extended contact was greater toward confederates in the disease condition than it was for those in the psychosocial and normal conditions. The biomedical materialists are right in claiming that their model may lead to less blame, but it also leads to harsher treatment of the person with the problem.

Research by Farina, Fisher, Getter, and Fischer (1978) and Fisher and

Farina (1979) indicates that people who are taught to conceptualize their psychological problems in terms of “diseases” feel more helpless about what they can do to get better and are more likely to use alcohol and drugs to relieve their distress. They also ignore their problems more. They tend to see problems as being out of their control, with solutions depending on the development of effective medication. Those taught psychosocial conceptualizations are more active in trying to change things and are less likely to turn to alcohol and other kinds of drugs.

The biomedical materialist’s claim that educating people about the biological basis of their problems will provide them hope is therefore suspect. If psychiatrists such as Strauss (1994) are correct about the importance of self-regulation with respect to symptom management and the time-course of disorders, strict disease models may lead people to adopt an overly passive stance toward their problems and hence encourage less effective coping, making the course of the disorder worse than it need be.

Beahrs (1986) notes that mental health professionals need to consider clients’ frames of reference when educating them about the nature of their problems. Individual differences between people will affect their reaction to psychological and biological explanations of problems. Some people are likely to interpret psychological problems as indicating that there is something wrong with them, with their self, as opposed to a biological problem where there is something wrong with their body, not them. This latter reaction may be more likely when people can be given information about high prevalence rates of the “biological disorder” in the population. Alternatively, some people are likely to be upset by a biological problem, which may be more out of their control, whereas a psychological problem means that they can do something about it. Folk materialism may lead people to think that biological problems are more real, and therefore worse. We don’t want to lie to people or hedge etiological facts to suit their preferences, but being aware of people’s reactions and checking them out is a better strategy than just assuming that it is always preferable to tell them that they have a disease.

Biomedical materialists point out that when the problem is very severe, such as bipolar disorder, schizophrenia, or early-onset childhood hyperactivity, disease models are much more likely to reduce inappropriate guilt on the part of family members. They are surely right, but reducing guilt is still a psychological problem. Even for families, a psycho-educational intervention which focuses on the biological etiology can only be a part of the solution

because inappropriate guilt is, by definition, what psychologists call a boundary issue. People have to learn to accept their child or family member as a separate person who has their own reactions. Parents and children have to learn that they are not responsible for everything that happens to their child or sibling, and that what their child does is not always a reflection on them. They need to accept that they have to attend to their own needs first if they want to be able to continue to care for the afflicted person. In addition to not blaming the person for their illness, family members can benefit from learning to let go of unmet expectations regarding the afflicted person's functioning. Developing more realistic explanations can reduce disappointment when expectations are too high, and curb overprotectiveness when expectations are too low.

Although psycho-educational interventions about biological causes are excellent beginning points for addressing boundary issues, traditional therapeutic interventions can address them more directly and elegantly. Once again, psychological and biological analyses work best together. The biomedical materialists are right about guilt and blame being a problem, but they fail to see what a more complete solution involves.

Psychological understanding reduces blame and stigmatization

Kardiner (1977) reports that Freud was infuriated when he (Kardiner) told Freud that one could do no harm with psychoanalysis. Freud claimed that if Kardiner held that psychoanalysis can do no harm, that means he also held that it can do no good. If psychological interventions can create changes, those changes can be for better or for worse. Analogously, when biomedical materialists discuss how people are harmed because their self-esteem is lowered and they are made to feel guilty by therapists imposing blame-creating psychological analyses, they in effect claim that interpersonal events and psychological explanations can change people. As Freud's response to Kardiner suggests, if psychological events and interventions can make things worse, they can also make things better.

When centered in empathy and an attempt to get inside the other person's head, psychological analyses connect us to others rather than separate us from others. I referred to this in Chapter 8 under the rubric of the diagnostic relevance of first-person information. Torrey's (1983) own plea that we understand the workings of the schizophrenic brain in order to understand schizophrenia is a plea to imagine what it is like to have schizophrenia.

Colours seem to be brighter now, almost as if they are luminous painting. I'm not sure if things are solid until I touch them. (p. 19)

My concentration is very poor. I jump from one thing to another. If I am talking to somebody they only need to cross their legs or scratch their heads and I am distracted and forget what I was saying. (p. 23)

I have to put things together in my head. If I look at my watch I see the watchstrap, watch, face, hands and so on, then I have to put them together to get it into one piece. (p. 29)

Torrey's contention about the importance of understanding what it is like to have schizophrenia demonstrates the kind of sophisticated biopsychosocial understanding that other medical specialties sometimes lack.

In Chapter 7 I stated that empathy is correlated with psychological mindedness, so that mature forms of empathy represent a more advanced psychological understanding of self and others. In addition to observing others, empathy requires that you attend to your own first-person reactions and requires that you be able to imagine being in another's situation.

Accurate empathy is also an interpersonal process. It requires imagining what things are like for the other person and communicating that understanding to them. By communicating it, you let them know that you understand, and also give them a chance to correct any misunderstandings. Since people tend to pay less attention to their feelings and desires, the information gained from accurate empathy has considerable incremental validity for understanding others. It also increases the person's own self-understanding. It can free them to empathize with themselves, making it safer for them to acknowledge their own reactions. In therapy, both therapist and client gain a better understanding of the client's experience. The shared nature of this process helps reduce distance on both sides of the relationship. Engaging in this process is what helps family members stay engaged with the person who has psychiatric problems. Those who can't empathize don't stay engaged.

Carl Rogers' humanistic approach to therapy downplays pathology, considering most people's reactions as understandable responses to situations such as externally imposed conditions of worthiness (e.g., Do this and I'll like you). He even considered empathy to be one of the necessary and sufficient conditions of change in therapy. Although traditional humanists mistakenly minimize the importance of assessing psychopathology, they are right about personality dynamics. Understanding another and communicating that understanding to them enhances the relationship and reduces alienation on both

sides. They feel closer to you and you to them. Unless persons with major mental illness are secretly members of another species, and as long as they are not actively psychotic, the rules of empathy apply them as well. Understanding *what it is like* makes it harder to reduce the person to their problem.

Inaccurate biological explanations have also been iatrogenic

Although the treatment of general paresis is the chosen exemplar of biomedical materialists, in the early twentieth century, an equally prevalent treatment was neurosurgery. Livingston (1975) and Shorter (1997) report that in 1935 John Fulton and Carlyle Jacobsen demonstrated that experimentally-induced anxiety and frustration in chimpanzees could be eliminated by an ablation of their frontal lobes. The chimps appeared to have traded anxiety for peace of mind and cheerfulness. Based on these findings, the psychiatrist Egaz Moniz developed a procedure called a frontal leucotomy in which he transected the anterior part of the frontal lobe from the rest of the brain in human patients. His goal was to reduce anxiety states, replacing them with peace of mind. This procedure became more widely known, in its outpatient form, as the frontal lobotomy. It gained widespread popularity after the publication of a monograph by the Americans Walter Freeman and James Watts in 1942. Valenstein (1986) reports that psychiatrists' desire to be seen as respectable *medical* professionals alongside neurologists and neurosurgeons led to the widespread use of the lobotomy, even on an outpatient basis. This latter goal makes the lobotomists mid-century proponents of the biomedical materialist movement. The response of the scientific community to the new scientific psychiatry was so positive that Moniz won the Nobel prize in 1949.

It soon became apparent to many professionals, however, that the initial enthusiasm for the frontal lobotomy was exaggerated. "Side effects," in addition to death, included an inability to inhibit impulses, severe emotional blunting, loss of social skills, impaired judgement (executive function deficits), and epilepsy. In contemporary terms, the removal of positive symptoms came with a price: the addition of permanent negative symptoms. Dramatically put, loss of mind rather than peace of mind better describes the effect of many lobotomies.

Goodwin (1987) reports that one of the more famous treatment failures was that of Rosemary Kennedy, the mentally retarded sister of John, Robert, and Ted Kennedy. According to contemporary criteria, Rosemary Kennedy

would be diagnosed with either mild mental retardation or borderline intellectual functioning. The story begins with Ms. Kennedy having serious adjustment problems after returning to the United States from Great Britain, where her father Joseph Kennedy was the American Ambassador from 1938–1941. At this time, she was in a continuous state of rage and frustration, fighting with family members and wandering the streets alone. The family became concerned that the attractive Rosemary would become sexually active, so something had to be done.

Joseph Kennedy became aware of the lobotomy as a potential miracle cure during his stay in England and, in 1941, decided on his own to have Rosemary lobotomized. Because she was raised at home with the rest of the family, Rosemary had developed enough social skills to participate in family activities, including very formal occasions involving the British royalty. Her mother wanted her to be as normal as possible. After the operation, however, these hard-won abilities were lost. She was almost mute and did not even know who she was. Her deficits were so profound that she was institutionalized for the rest of her life, and her father refused to let the rest of the family even see her. From a professional standpoint, the sad thing about this case in particular is that what could have been treated psychologically as a developmental issue was treated biologically as a physical deficit.

Shorter (1997) reports that over eighteen thousand lobotomies were performed in the United States by 1951. Unfortunately, they were performed indiscretely, not just on patients who had been unmanageable for years, but on patients who might have recovered anyway. Many of them did not have what would be considered a severe psychiatric disorder. This is a clear case of iatrogenics.

In addition to the embarrassing consequences of psychosurgery, biological explanations have also had widespread negative influences on social policy. For example, the biological explanation of intelligence, deviant behavior, and moral depravity was very popular in the early third of the 20th century. To borrow a phrase from Paul Churchland, these problems were considered to be the outcome of sheer chemical and genetic circumstances whose origins were more metabolic than social or psychological. This kind of analysis offered the hope for biological solutions to social problems.

Stephen J. Gould (1981) and E. Fuller Torrey (1992) note that many scientific intellectuals, and especially psychologists, believed that racial and individual differences in intelligence were due to biological endowment, and

not to education and learning. As a result, a scientifically rationalized belief in the biologically-based inferiority of immigrants and blacks was widespread. These views were held by both liberals and conservatives, including American presidents such as Theodore Roosevelt, Calvin Coolidge, Woodrow Wilson, and Herbert Hoover.

Americans not only favored Francis Galton's program of positive eugenics, involving selective breeding where more "capable" people were encouraged to mate with each other, they also favored a program of negative eugenics, in the form of compulsory sterilization. Torrey (1992) reports that twenty states passed sterilization laws much like the one passed in Iowa which mandated the

prevention of procreation [by] criminals, rapists, idiots, [the] feeble-minded, imbeciles, lunatics, drunkards, drug friends, epileptics, syphilitics and moral and sexual perverts, and diseased and degenerate persons (p. 47)

By 1933, over 20,000 people have been sterilized in the United States (Torrey 1992). Breggin and Ross-Breggin (1988) note that the American eugenicist Paul Popenoe claimed that in addition to psychiatric inmates themselves, their families should also be sterilized. These laws exemplify Burleigh and Wippermann's (1991) phrase "the purification of the body of the nation."

Of course Burleigh and Wippermann are referring to Nazi Germany. Although the majority of eugenicists in the United States recanted by the 1930s, the American laws became the basis of the more infamous Nazi laws. When the Nazis came to power, they upheld the American laws as examples for Germany to follow. In Hitler's Third Reich, compulsory sterilization was applied to anyone having an alleged hereditary illness, particularly psychiatric patients suffering from schizophrenia and manic-depression. Before the implementation of the Nazis' infamous final solution, 400,000 people were sterilized. Ironically, the Allies could not prosecute those who performed German sterilizations as war criminals because similar activities had been so prevalent in the United States (Proctor 1988; Torrey 1992).

Furthermore, the first victims of the holocaust were the mentally retarded and the mentally ill. Under the auspices of medical science, the gas chambers at Auschwitz and Treblinka were originally built for psychiatric institutions. Aly (1994) also notes that psychiatric patients were not just killed. Doctors studied them carefully both before and after the euthanasia in order to learn more about the nature of psychiatric disorders.

Many of these abuses followed from the psychiatric theory of degeneration. As Shorter (1997) notes, beginning in the mid-eighteenth century biological psychiatrists came to believe that certain psychiatric illnesses were progressively degenerate, meaning they got worse from generation to generation. For example, acquired alcoholism could be passed on to one's children, and in them degenerate into impulse control problems of all sorts, spiraling downward generation after generation into even worse disorders. Quarantine of family groups (e.g., ghettos) and sterilization were common sense solutions to these progressive psychiatric diseases where the sins of the father were literally inflicted on sons and daughters.

Were I to imitate the rhetorical style used by Nancy Andreasen and Paul Churchland in their description of psychosocial explanations, I would now claim that biological psychiatry is hopelessly corrupted. That kind of rhetoric, however, is mistaken. These abuses are *not* inherent to either the biomedical model or to genetics. They are aberrations. Leahey (1992) points out in the 1920s and 1920s, the evolutionary biologists who synthesized Mendelian genetics with Darwin's theory of natural selection condemned eugenics as "biologically stupid" (p. 354). E. Fuller Torrey's own description of these abuses is as damning as any in the literature. They are, however, examples of inaccurate biological explanations that caused much harm. It is hard to imagine more misguided social policies than those that resulted from the biological explanations offered by the proponents of eugenics.

The biomedical materialists fail to consider history when evaluating the benefits and deficits of their treatment philosophy. They also fail to consider history when they evaluate Freud. Whatever else people may believe about psychoanalysis, Freud's initial ideas about treatment in the early 1900s helped psychiatrists abandon quite harmful biological therapies such as electroshock body therapy, insulin coma therapy, icy showers, beating with wet towels, and the destruction of female genitalia. He also provided moral guidance to mental health professionals, asking them to sacrifice a preferred theory when confronted with contradictory clinical data, to avoid imposing their own needs and values on clients, and insisting that they forgo intimate physical encounters with clients both inside and outside the professional relationship.

Paul Churchland (1995) states that there is always going to be a dark side to increased biological knowledge of how the brain works. He claims that it is certain that dangerous drugs will be prescribed, that promising treatments will be shown to have harmful long term effects, that psychosurgery will cause

irreversible brain damage, and that ignorant policy makers will try to solve social problems with biological interventions. However widespread these occurrences, he thinks these failures should be considered perversions. Meanwhile, biomedical and eliminative materialists point to perversions such as false memory syndrome as proof of the emptiness of psychotherapy in general. It is illogical to universalize psychotherapeutic failures while minimizing or ignoring biological failures. What goes for biological treatment goes for psychological treatment and vice versa. The dark side of any efficacious technology is of great concern, but particular mistakes don't justify the wholesale abandonment of either the biological or the psychological approach.

3. Biomedical Explanations Do not Guarantee Better Access to Care

Biomedical materialists such as Guze (1992) provide another reason for advocating biological explanations, claiming that when society realizes that psychiatric syndromes are real illnesses, government agencies and insurance companies will no longer be able to discriminate against psychiatric patients and underfund psychiatric services. He thinks that insurance companies assume that the public will tolerate discrimination against psychiatric patients because folk wisdom leads people to believe that psychiatric disorders are not real illnesses.

Contradicting Guze's claims, the factors affecting the support of psychiatric services are *not* related to a failure to view psychiatric disorders as real illnesses. A more plausible reason for underfunding psychiatric treatment is a purely economic one, specifically the fact that many psychiatric problems are chronic, and therefore costly. From the deinstitutionalization of psychiatric patients to welfare reform, fiscally conservative policy makers have long wanted to limit spending public money on chronic problems. Torrey (1997) notes that the introduction of Medicaid in 1965 gave the states a chance to shift the burden of care for the psychiatric patients to the federal government. By releasing psychiatric patients from state-supported hospitals and getting the federal government to pay for their housing, food, and emergency room services, the states came out ahead. Meanwhile, as Phares and Trull (1997) note, federal cutbacks in 1968 ended the Kennedy-Johnson goal of comprehensive community mental health treatment in the United States. When the states shifted the burden to the federal government and the Congress failed to

come through, psychiatric services were a big loser.

They lost even more the second time around during the Reagan administration, when the federal government adopted what economists call fiscal federalism. In the Reagan years, the federal government shifted social service responsibilities back to the states while cutting social service dollars. This pitted interest groups against one another and forced states to choose which programs to support. The influence of small interest groups such as psychiatric patients and their families were diffused because these groups now had to deal with fifty state legislatures rather than one centrally organized governmental bureaucracy. By the early 1990s, the states were in rebellion about what became called “unfunded mandates” and the budget slashing cycle continued.

Even when they are physically defined, since psychiatric problems do not directly lead to death, they are less “serious” than cancer or heart diseases. Mueller (1993) notes that the American government has given up on the idea that all citizens have a right to the same level of care for the idea that all citizens have a right to some minimal level of care. As long as people are not dying from psychiatric illnesses, they will get subsistence care rather than comprehensive care. This is true for both psychiatric illnesses and traumatic brain injuries, and the public surely believes that brain injuries are real medical illnesses.

Like governmental agencies, insurance companies are also reluctant to provide coverage for anything that can be considered a chronic disability such as traumatic brain injury, or chronic conditions that are expensive to treat, such as AIDS. These policies may not indicate that insurance companies are prejudiced against someone with TBI, AIDS, or schizophrenia as much as they indicate their realization that, from a business standpoint, covering these kinds of problems decreases profit margins astronomically.

It is easier for insurance companies to determine appropriate treatment for a condition such as appendicitis than it is for conditions such as schizophrenia and depression. Even if treating appendicitis costs more than long term prescriptions for Thorazine or Prozac, the insurance companies will prefer to cover appendicitis because they know what they have to pay to get out. People are only going to want their appendix removed once. With chronic type disorders there is a larger degree of uncertainty. Since insurance coverage is based on assessments of known risks, bottom line-oriented executives are reluctant to commit themselves to treating any problems that may have endless cycles of treatment.

The profit motive hypothesis may be especially accurate in the United States where, as Gray (1991) notes, investor-owned corporations are increasingly responsible for a majority of the country's health needs. The traditional private hospital's concern for the public good, such as accessible crisis care for the poor and uninsured, does not conform to the profit needs of private corporations. The outcome of the interaction between (a) fiscally-conservative lawmakers, (b) for-profit insurance companies who manage care and (c) investor-owned hospitals does not favor long-term psychiatric services.

In fact, in the current cost-conscious age, relatively cheap pharmacological treatment is going to receive more support than ongoing psychosocial rehabilitation and treatment. Treatment approaches that fall outside the domain of a strict biomedical model are the most likely candidates for underfunding. As physicians, biomedical materialists need to be careful because their favored treatment philosophy is going to be more acceptable in the prevailing social and economic environment, but the reason for this increased acceptability relates to cost-containment, not the physician's goal of reducing suffering. A more exclusive support of biomedical treatment would be a good thing if psychiatric diseases were really like general paresis, but they are not. Their chronic nature requires long term care.

The genetic basis of psychiatric diseases also challenges the biomedical materialist's claim that insurance coverage for psychiatric problems will increase if we adopt an exclusively biological perspective on the nature of the underlying pathological process. Since any psychological state is partly the actualization of genetic predispositions, any psychiatric problem can be taken as a sign of a genetic vulnerability. Actually developing a psychiatric disorder would prove that the person is going to be vulnerable to that illness for the rest of their life. As with individuals, biomedical analyses could even lead to claims of family based-genetic defects. Everyone in the family (born and unborn) would gain the label of potentially schizophrenic or depressed.

Interestingly, Shorter (1997) points out that the biological concept of degeneration had such unpleasant implications, that turn of the century psychiatrists came to be seen as potential enemies rather than as friends and helpers. Just as contemporary clinics may change their names from "Chittiville Mental Health Center" to "Chitiville Human Services," turn of the century clinics gave up being institutions for the *insane*, and became institutions for *nervous disorders*. Although psychiatrists do not have to be seen as enemies, it is unlikely that people are ever going to experience the need for psychiatric

consultation as “good news.”

Finally, until actual cures are available, psychiatric problems fall into an insurance company’s category of pre-existing condition. If they can deny a woman who changes jobs during a pregnancy medical care because of a pre-existing condition, they can do the same for anyone who has undergone or is currently undergoing psychiatric treatment. Once we call genetic endowment a pre-existing condition, actuarially-oriented bureaucrats working for insurance companies will see great opportunities for limiting services. Any “in the head” perspective always risks blaming the victim. This is as true for biomedical explanations as it is for psychoanalytic explanations.

4. Moral Understanding

Although morality is supposed to provide us with standards for how we ought to behave, what constitutes a standard is open for debate. Mark Johnson (1996) claims that it is a mistake to think that ethical theories provide *moral governance*, meaning rules specifying how we should act in concrete situations. He says that instead of governance, the purpose of ethical theory is the enrichment and cultivation of *moral understanding*. Moral understanding still provides us guidance, but not absolute decontextualized guidance.

One of the ways we achieve moral understanding is through stories. Being able to make analogies between our current situation and stories about what actions count as good and bad helps us decide what to do. We could investigate anyone’s moral understanding by finding out about the stories they use to guide their behavior. Stories about Moses and the prophets, Jesus, the Buddha, or Muhammad provide many people moral guidance. Even though we attribute moral authority to these great figures, that attribution is often contingent on already having gained moral guidance from their stories. The meaning is in the story.

Many different kinds of stories guide us, not just religious stories. Stories about moral figures such as Martin Luther King Jr., Mohandas Gahndi, Galileo, King Arthur, Huck Finn, Superman, and Jean Luc-Picard provide us with moral guidance if we can make analogies between their situation and ours. Stories about the Nazis and the Stalin regime, the McCarthy trials and Governor George Wallace of Alabama guide us as well, telling (some of us) us how not to act. Nor do we always agree on what counts as a good story.

Individuals as diverse as Sigmund Freud, B. F. Skinner, Madonna, Ronald Reagan, and Pat Robertson are proclaimed both heroes and villains.

This notion, that morality is transmitted by means of “morality plays,” is a folk psychologized version of Paul Churchland (1994) and Owen Flanagan’s (1996) moral network theory. Churchland and Flanagan both claim that knowledge, including moral knowledge, involves recognizing patterns. For example, Churchland’s prototype activation model of explanatory understanding is based on the idea that brains don’t work by manipulating sentence-like *rules*. Rather they recognize features, sequences, consequences, abstract configurations, and *plots*. Recognition depends on the activation of stored prototypes. Using this feature recognition model, Churchland (1995) believes that understanding is a species of perception. To understand something is to “see” it. He claims that we learn to see “cruelty, patience, meanness, and courage” (p. 145).

Similar to Churchland, Flanagan claims that prototype activation explains our understanding of the social world. We learn to recognize particular social situations as examples of prototypical situations, with the prototypes providing guidance about what counts as good and bad actions in those situations (or stories). Moral sensibility involves being able to enter a new situation and recognize its moral features, which involves activating stored moral prototypes. For example Clifford Durr, the white lawyer who helped black lawyers plan their defense of Rosa Parks for refusing to move to the back of the city bus in Montgomery, Alabama, interpreted the story that was being acted out before his eyes in a different way than Governor George Wallace or Birmingham’s Sheriff Bull Connor did. Durr’s story was about Alabama’s failure to fully accept the implications of the Constitution and the Bill of Rights; Wallace and Connor’s stories were about Northern aggression and its century-long attempt to destroy the Southern way of life.

Flanagan thinks that we need multiple prototypes or “moral competencies” that can be activated across many situations. In the folk psychology version of moral network theory, it makes sense to say that these social situations are represented in term of stories and plots about being a good boy/girl and about heroes and villains, and what is activated when we are confronted with moral problems are those plots.

Churchland and Flanagan also help us see the similarity between scientific problem solving and moral problem solving. Both types of problem solving use prototypes or exemplars as examples of what counts as a good

solution. Sometimes these prototypes can be solutions to both scientific and moral problems. This means that some scientific exemplars, in addition to being solutions to professional problems are also solutions to moral problems.

For example, the story of the late 18th century physician Ignaz Semmelweis is a both a scientific and a moral exemplar. According to Glasser (1976) Semmelweis was concerned about the problem of puerperal fever, a condition where previously healthy women who had just given normal birth were dying. They all had the same set of symptoms, including fever, chills, seizures, and heart failure. He disagreed with hypotheses that bad food, water or air were responsible for the deaths because women in the same hospital who had not given birth were still alive, and they had been exposed to the same food, water, and air. Although some people suggested that the women should be allowed to give birth at home, financial pressures to keep the wards full prevented doctors from agreeing to these requests.

One day a pathology assistant cut his hand during an autopsy of a woman who had died from puerperal fever, and the assistant died after experiencing the same symptoms as the woman. Semmelweis saw a connection and wondered if it had to do with the autopsy. He visited other hospitals and learned that the death rate was highest in the hospitals where physicians would give birth directly after being in the pathology lab where they might handle the diseased tissue of recently deceased women. He also found that in hospitals where midwives gave birth, the death rate was much lower. This led him to conclude that the physicians were contaminating their patients.

Semmelweis tested his theory that iatrogenic influences were responsible for these deaths by forcing physicians to wash their hands before both giving birth and performing pelvic examinations, and the *death rate dropped dramatically*. Although physicians were outraged at the suggestion that their actions were harming their patients, Semmelweis physically forced those under his charge to wash their hands. Even though women continued to die of puerperal fever, not a single case occurred on Semmelweis's wards after he insisted on these new procedures. Unfortunately his data was dismissed and he was ridiculed by his colleagues. Glasser reports that the pressure prevented him from continuing to practice medicine. Stories like this one provide us with an important example of how to act.

The biomedical materialists' general paresis exemplar and Paul Churchland's brain injury exemplars also provide them with moral guidance.

Once upon a time, general paresis was a psychiatric disorder, but now it has been shown to be the result of untreated syphilis. We now know that the psychological exploration of the content of the delusions people have in the advanced stages tells us nothing about the causes of the problem. The reasons for their behavior are biological, not psychological. The effects of this disease are tragic and the right thing to do is to treat the syphilis and prevent the brain from deteriorating.

Nature herself performs experiments on animal subjects with a blind cruelty beyond human imitation. For example, damage to the hippocampus leaves people unable to form new memories. They retain their old memories and can learn new procedural skills, but they can't learn any new verbal information. You could meet them every day for eighteen years straight, and for them, every time you meet will be as if it is the first time. In contrast to localized injuries, systematic degeneration of the brain produces the delusions, hallucinations and emotional blunting known as schizophrenia. If we could repair this damage, we could cure the person of the disorder. Compared to the frontal lobotomy and chronically feeble psychotherapeutic interventions, direct chemical modulation has created a wiser and more humane era in psychiatry where real reduction of pain and suffering is now possible.

Biomedical and eliminative materialists claim that we should learn to recognize all psychiatric problems as versions of these situations and act accordingly.

I've had some success in an ethics class helping students recognize their own moral sensibilities by asking them to think about the specific stories and actions they take to be strong examples of good and bad behavior. They are not allowed to use any religious stories. To illustrate what I'm looking for, I give them examples of stories I find powerful. After they have finished writing down their stories, I ask them to look at what principles are embodied in these stories. This activity helps them personalize their moral sensibility and helps them understand why they react to certain problems the way they do. Mental health professionals could also benefit from examining their preferred moral prototypes as well.

Paralleling my claims in Chapter 11 about professional exemplars, the general paresis and brain injury exemplars are perfectly good solutions to moral problems. But mental health professionals need to possess a repertoire of moral prototypes wider than just these two exemplars. Clinical reality is more complex than what is described in these two solutions.

The additional exemplars I gave in Chapter 11 are also worthy moral prototypes for guiding us in certain situations. For example, the *problems in*

living exemplar is an important moral prototype, especially for humanistic psychologists. Sometimes people are placed in intolerable situations and their reactions may only look like illnesses. The plot line of this story indicates that right thing to do is to help the person change their situation. There are many ways to change situations. In this story, the villains are those who blame the victim.

The *traumatic brain injury exemplar* provides another moral prototype. Paul Churchland (1995) states that one of the primary aims of medical science with respect to TBI is to promote whatever degree of cognitive recovery remains possible for the patient. Unfortunately he does not expand on this important point about *rehabilitation*.

Although recovery includes struggling with loss-of-self issues and dealing with personal reactions to the new deficits, the guiding principle of rehabilitation psychology is that we have to focus people on their strengths, on what they can still do to compensate for some of their losses. Just as society has begun to construct ramps for people in wheel chairs to help them get around, we have to help persons with brain injuries construct their own personal ramps. Doing so is a good thing because it helps them live as independently and productively as possible. Prigatano (1991) refers to this comprehensive approach as dealing with the person's disordered mind *and* their wounded soul.

Applying Churchland's arm-chair deficit-oriented model of TBI to these problems would be inadequate. According to Prigatano, research shows that failure to address broad psychological issues is correlated with long term deterioration of functioning. For rehabilitation psychologists, deficit-oriented approaches and pathologizing approaches are villainous. By ignoring personal factors, and we can easily fail to do what is right.

A third moral prototype is the *interpersonal intervention exemplar*. We are social creatures, and our greatest adaptive challenge is relating to other people. Sometimes we develop habitual patterns of reacting to others, repeatedly acting those patterns out, whether or not they are appropriate. One advantage of focusing on the therapeutic relationship is that it provides the opportunity to help people directly experience their dysfunctional patterns of behavior. Treatment in this case involves participation and engagement. The person is a client, not a patient. Instead of thinking that something has gone wrong with their brain, they can see that they continually recreate problems in their habitual style of relating to others. Since it is something they do, it is also

something that they can change. Fostering client responsibility in this way is the right thing to do. The villains are those who tell people that they are helpless and that their problems are out of their control.

5. Conclusion

In this chapter I argued that some of the biomedical materialists' appeals for a more exclusive reliance on neuroscience can be reduced to the demonstrably false claim that neuroscience is good and psychology is bad. I also argued that the problems of stigmatization and guilt which legitimately concern biomedical materialists, are by definition *psychological* problems. As automatic reactions to irregular behavior, stigmatization and guilt are independent of etiological theories about that behavior. The biomedical materialist solution, educating people about the biological basis of psychopathology, does reduce blame, but research indicates that it also undermines hope. For some reason, putting the problem in the body also increases mistreatment of the person with the problem.

As far as the "neuroscience is good, psychology is bad" claim, proponents of biomedical and eliminative materialism exaggerate the faults of psychological treatment and minimize the faults of biological treatment. To correct this minimization, I reviewed the iatrogenic tragedy called the prefrontal lobotomy. I also showed how the racial purity ideology of the eugenics movement, and chronic hatred of immigrants, the poor, and the disadvantaged have long been defended as being justified by the facts of biological science, including biological solutions such as sterilization and euthanasia. These harmful treatments and social policies are not necessary consequences of biological psychiatry, but neither is blaming the victim a necessary consequence of psychological psychiatry. As a matter of fact, a psychological understanding of the person with a psychiatric disorder helps professionals, families, and friends continue to engage them in meaningful relationships.

Iatrogenics with respect to psychology and biology are analogous to Social Darwinism with respect to natural selection. Social Darwinists think that those who are at the bottom of the social ladder are there because they can't compete, and they can't compete because they are inherent losers. They also think that those who are economically successful have achieved that position because of inherent superiority and need to be fully supported for the good of the social body. According to modern Darwinian thinking, this is

plain wrong. The whole idea of a unidimensional social ladder and higher/superior and lower/inferior is more theological than scientific. Higher and lower are grounded in a cosmological theory about God and angels at the top, followed by humans and animals — and the corresponding idea that some humans are closer to the angels than others. Social Darwinism is also wrong because adaptation is not an absolute quality that someone possesses. What counts as adaptive varies from time to time and situation to situation. The adaptive becomes evident over thousands of years, not over two hundred years. The world created by natural selection is much more complex than Social Darwinists give it credit for.

Psychology and biology are also more complex than their opponents give them credit for. The anti-psychiatrists who demonize biology and the biomedical and eliminative materialists who demonize psychology fail to see the incongruity between their straw man demons and the multifaceted explanatory approaches of psychology and neuroscience.

I ended this chapter by claiming that psychiatrists are correct to be concerned with morality, because deciding what counts as the right thing to do is a continual problem. I offered a folk psychologized version of moral network theory, claiming that we learn to recognize moral features in situations. Moral competency is not just given to us. Like scientific exemplars such as $F=ma$, they have to be taught, and knowing how to apply them to new situations is a matter of practice.

Many moral exemplars take the form of stories with clear heroes and villains. Because social reality is more complicated than what can be encoded in every day moral stories, we need multiple stories to guide us. The traditional general paresis and traumatic brain injury stories used by the biomedical and eliminative materialists are genuine moral prototypes, but they cannot provide universal moral guidance to mental health professionals. I offered some psychology-friendly prototypes that also need to be a part of our moral repertoire.

CHAPTER 13

Reflections

1. Introduction

One reason why philosophically-motivated devaluations of psychology are important is that our self-conceptualizations are partly shaped by ideas. We *learn* how to think about ourselves. For example, we learn that individualism is an achievement, or that individualism is being selfish at the expense of the group. We learn that it is healthy to be emotional, or we learn that it is better to be “rational.” We learn that it is good to study hard, or we learn that people who read books are eggheads. We learn that having sex is sinful, or that not having sex means you are incomplete as a person. These beliefs contribute to our identities. We do not choose them like we choose an evening movie; we are influenced to believe these things about ourselves by numerous cultural prescriptions, many of them contributed by contemporary intellectuals. The role that ideas play in molding our self-conceptualizations makes eliminativism more than just an academic debate.

In this chapter I explore some psychological reasons for biomedical and eliminative materialists adopting the narrow-scope models they do. I claim that, even if we have reasons for not believing the arguments supporting biomedical and eliminative materialism, they can be so persuasive that we are psychologically-loaded to believe them anyway. I also speculate about temperamental variables that may partly explain why certain kinds of arguments are so attractive to their proponents.

2. Procrustean Science?

The preceding twelve chapters critically examined the reasons given by the

biomedical materialists in psychiatry and the eliminative materialists in philosophy for their devaluation of psychological understanding and explanation. Some of these reasons appear quite unreasonable. For example, Chapter 4 highlighted the common sense roots of this devaluation, i.e., **folk materialism**, the view that the physical is what is really real, including the physical brain. Understanding materialism as common sense helps us see that, in making their “science versus common sense” attacks on psychology, eliminativist philosophers and psychiatrists ignore the fact the plausibility of their own position gains robustness from common sense assumptions about the world. Even three year old children know that we think with our brains. It is therefore hypocritical to attack psychological explanations because they accord so well with “common sense” assumptions.

Discussed in Chapter 5 was scientism, the view that the more rigorously we use the scientific method in any endeavor, the more superior and utopian the product. However, the idea of progress toward a more utopian state represents a pre-Darwinian view of the world, similar to the view espoused by the 18th century natural theologians. Although the 19th century Enlightenment thinkers took the side of science in the war between science and religion, their notion of progress still assumed a hierarchical cosmology in which evolution was conceptualized as “moving up the chain of being toward a state of perfection.” There is no comparable hierarchy in a truly Darwinian world.

The arguments against scientism were also anti-essentialistic. In highlighting the overlap between science and common sense, I claim that science and common sense are not black-and-white mutually exclusive classical categories. The anti-essentialistic attitude was also reflected in chapter 4 where I claimed that there is no such “thing” as folk psychology. The meaning of folk psychology is not fixed. As the term is used, folk psychology refers to much more than a prescientific conceptual system.

Chapters 6 and 11 discussed the arm-chair nature of many eliminativist claims, arguing that they cannot be practically sustained when we have to interact with real people on a day-to-day basis. Psychology as an interlevel context between internal and external worlds, as explanation with reference to what is in the head, and as understanding with reference to the self is not something we are just taught. It is part of our cognitive endowment. As with language, children *learn* basic psychological explanations, but they do so generatively. On the whole, eliminativist writings about human nature are split off from some of their own experiences, while clinical psychologists at

least attempt to formulate concepts in “experience near” terms.

Throughout the book, sometimes under the rubric of first-person information, at other times as phenomenological symptoms, elsewhere under the title of subjective reactions, I have also discussed consciousness. Eliminativist externalists such as B.F. Skinner would do away with consciousness along with beliefs and desires. Eliminativist internalists such as Paul Churchland are more inclined to accept that the concept of consciousness has a role to play in our self-understanding, and primarily dispute the validity of the theoretical network which currently forms the context for introspection. At the same time, eliminativist internalists are disinclined to admit that there is an important difference between first-person and third-person descriptions with respect to their ability to inform us about self and others. My general position has been that we should accept the notion of *levels of analysis*, and not be third-person (physiological-behavioral) reductionists. We should be *explanatory pluralists* and accept that beliefs, desires, first-person reports, and subject reactions are legitimate concepts as long they contribute to the solution of professional problems.

Chapters 10 and 11 cast doubt on the assumption that when we understand psychiatric disorders as bounded biological entities, we will know what they really are. These are among the most complex arguments in the book. For one, thinking of the biological world in terms of distributed continua means that identifying entities requires evaluation and not just accurate observation. The facts of evolution also suggest that once we identify biological entities, they have to be understood contextually. Finally the biological entity models proposed by psychiatrists and philosophers, although useful, also have limited generalizability. This means that explaining and understanding psychiatric disorders is a multi-level, temporally extended, and dynamic problem.

What has not been discussed since the first three chapters is how persuasive devaluations of psychological explanations can be, no matter how strongly one might believe in their validity. For example, a few hours after finishing Peter Kramer's (1993) *Listening to Prozac*, I had an appointment with a client who had struggled for years with obsessive-compulsive disorder. I had already referred him to the agency psychiatrist, so wondering if he should have his dosage of either Prozac or Anafranil increased was not my job. He had been reading books on cognitive-behavioral strategies for extinguishing unwanted behaviors, and in the previous couple of months we had uncovered some very tangible psychological correlates of his need to compuls-

sively perform rituals. All the same, it was twenty minutes into the session before I was able to shift my focus from “I wonder if further modifying his brain serotonin levels will help him,” to psychological questions such as “How can I help him understand that his rituals are associated with both feeling out of control, and a long standing fear of his own anger” or “How can I get him invested in trying the exposure-habituation paradigm.” Kramer’s case studies and riveting examples exerted such a strong influence on what I attended to, that I had to struggle to shift my attention and think about my client psychologically.

The mechanisms responsible for these kinds of cognitive constrictions have been studied by Tversky and Kahneman (1974), who investigated how we solve problems and make decisions. They found out that, oftentimes, we are influenced by an *availability heuristic* in which the most readily accessible examples (or facts) are used in decision making. Research by Hamill, Wilson, and Nisbett (1980) has also shown that if information is readily available and somewhat dramatic, we have a tendency to consider it to be more generally true than it actually is (statistically speaking), and we often ignore falsifying data in doing so. Nisbett and Ross (1980) also show that the causal hypotheses we develop are influenced by the limited range of potential casual factors available to us.

Obviously, Peter Kramer’s examples of his patients’ miraculous responses to Prozac exerted a strong influence on how I thought about my client. The availability heuristic influences biomedical psychiatrists as well, whose most accessible examples are overwhelmingly biological in nature. Many of these examples, such as split-brain phenomena (Gazzaniga 1967, 1983; Sperry 1968, 1982) and contralateral neglect syndrome (Kupfermann 1991), are very dramatic. The reduction of an episode of florid psychosis following the administration of a neuroleptic or the tempering of the mania-depression cycle with a course of lithium are equally dramatic, especially considered from the standpoint of the history of psychiatry. To the extent that *psychiatrists* and *philosophers* immerse themselves in biological examples, they are psychologically-loaded to have a difficult time seeing anything other than biological factors. Their attempts at explanation may therefore have systematic biases.

Perhaps the most flagrant example of the medicalization of everything mental is the concept of a shadow mental illness. The concept shadow mental illness amounts to the suggestion that, since psychological states are brain

states, any psychological state that is maladaptive could be considered an illness, or to put it more bluntly, any “quirk” potentially represents some form of brain damage. As touted in Sharon Begley’s January 26, 1998 *Newsweek* article, pathological states rest on a continuum, and all states on that continuum — even if they are in the normal range — can be considered a mental illness. The mistake of this folk biomedical materialism is not in the recognition that there may be genetic and physiological substrates of all psychological states, but in defining any maladaptive quirk as a “shadow syndrome.” The simple formula *maladaptive + biological basis = mental illness* is inadequate. Especially because adaptiveness is situation specific, almost every idiosyncrasy could be considered a shadow mental illness under the right conditions. That anyone would even make this kind of an argument indicates that biomedical psychiatrists and geneticists need to be more clear on the differences between the broad category of the biological and the narrow category of the medical.

Procrustean biases favoring physiological data are also found among research-oriented psychiatrists. In a January 13 article for *The New York Times*, Angier (1993) reports that psychiatrists experienced a “crushing blow” when they were unable to confirm a 1987 study linking manic depressive illness to a single gene. This retraction apparently dampened hopes that physicians would soon be able to offer genetic counseling. With respect to the importance of environmental variables, Angier quoted the psychiatrist David Pauls saying “We’ve tended not to pay that much attention lately to nongenetic influences” (p. B7). It is telling that this lack of attention to nongenetic factors persisted in spite of the well known scientific fact that genetics account for only a proportion of variance, in even the most biologically-loaded psychiatric disorders.

In the same *New York Times* article, responding to the puzzling fact that bipolar disorder used to begin at age 32 in the mid-1960s, but by the early 1990s generally began just under the age of 19, Frederick Goodwin, then director of National Institute of Mental Health (NIMH), suggested that this may be explained by a rise in the use of cocaine, even though he had no firm data to support this hypotheses. Goodwin realized that it is unlikely that the genetically programmed time course of manic-depressive illness mutated in the years from 1963 to 1993; therefore some kind of environmental influence is probably involved in the change. His concept of “environment” however, is extremely narrow.

Such well-entrenched “diagnostic styles” are analogous to compass needles. Compass needles always point in one direction. In the case of biomedical and eliminative materialists the diagnostic needle always points in the direction of: “Science says, the brain is the only really real thing in psychiatry.” Using all the persuasive stories and psychological research we can muster, we can shake that compass up, twist it around, turn it upside down and try to get that needle to move, but no matter what we do, it returns to the same point on the dial.

Frederick Goodwin’s hypothesis about the change in the time course of bipolar disorder and NIMH researchers’ obsessive focus on single gene theories demonstrates how narrow in scope some psychiatrists’ visions can be. I need not add that if dramatic biological exemplars constrict the perception of psychologically-informed psychiatrists and psychologists, they will affect the perception of general practitioners to an even greater extent.

Other physicians have training only in medical approaches, whereas even the most committed biomedical psychiatrists have had some exposure to psychological conceptualizations. Maxmen (1985) points out that instead of being treated by mental health personnel, 54.1 percent of the patients with psychiatric disorders are treated by primary care physicians. Cowley (1994) reports that a 1993 Rand Corporation study found that, of the general practitioners in their survey who treat depression, only half of them spend three minutes or more discussing their patient’s personal problems. From experience working in the community mental health system, I can attest to the prescriptive conservatism of psychiatrists relative to that of general practitioners, especially with respect to addictive anti-anxiety medications. The unlimited prescription privileges enjoyed by all physicians means that biomedical materialism’s greatest influence on our society may come not from psychiatrists, but from general practitioners.

In reflecting on the implications of good Prozac responses, Kramer (1993) notes that Prozac changes the sort of evidence to which psychiatrists attend. To illustrate this point, he describes the case of Lucy, a patient to whom he prescribed Prozac because she seemed to have problems with rejection-sensitivity, a condition for which he had found Prozac to be helpful. Although Lucy improved on both Prozac and Zoloft, Kramer discontinued the medications because they also made her overly agitated. He saw the agitation as an undesirable medication side effect.

Reflecting on her agitation later in therapy, Lucy hypothesized that it

resulted from the fact that the medication enhanced her conscious access to her emotions and memories — particularly a sense of longing for her own mother, whose murdered body she had been the first to find as a child. In comparing his side-effect hypothesis to Lucy's existential-longing hypothesis, Kramer suggests that his focus on medication could have blinded him to the obvious psychological meaning of Lucy's intensified sense of urgency. The scope of interpretations that were readily available to him had been narrowed by the drama of Prozac.

Although I am critical of biomedical materialists who adopt a philosophically-narrow focus on “the broken brain,” I do not envy the psychologically-minded psychiatrist who has the ability to write prescriptions, and therefore has an immediate press to transverse the neuroscience-psychology gap. Given the difficulty of making the shift from thinking about the physical brain to thinking about the dynamic mind, it is easier for physicians to focus on neuroscience and devalue psychology. The biomedical devaluation of psychology is psychologically convenient rather than logically necessary. Just as it is easier to study intellectual history by focusing only on great thinkers and ignoring social and economic forces, it is easier to study human behavior by focusing only on the brain, ignoring higher-order brain functions and the external environment. We can only attend to so much at any one time.

If it is true that the brain is the organ of adaptation to the world, and psychology is the level of analysis at which we can most cogently conceptualize brain-world interactions, then mental health professionals should strive to consult the perspectives of both biology and psychology in understanding their clients' concerns. However, the difficulty of this prescription should not be underestimated. It is an intellectually daunting task. Nor is it a question of adopting a facile “let a thousand flowers bloom” approach to pragmatism. It is important to be inclusive and open minded, but it is also important to be intellectually skeptical and sensitive to the dangers of quackery. There is such a thing as bad data. The four humors were bad data. Bumps on the skull were bad data. The events surrounding toilet training were bad data. On the whole, though, openness to data is a good thing in the long run — especially for self-proclaimed scientific realists. Patient reactions, personality styles, and developmental trajectories are good data.

Tversky and Kahneman's research indicates that the process of limiting the evidence to which we pay attention is psychologically understandable, but from a philosophy of science standpoint, being psychologically understand-

able does not make biomedical myopia logically correct any more than the understandable psychological reasons for experimenter bias makes it logically correct. One can empathize with how hard it is for mental health professionals to integrate multiple levels of analysis without having to condone their preferences for limiting their analysis of problems to data coming from the physiological-genetic spectrum.

3. The Psychology of Biomedical and Eliminative Materialism

Research also indicates that the cognitive devaluation of psychology cannot be attributed to medical training alone. Based on Engel's (1977, 1980) biopsychosocial model, Silverman, Gartell, Aronson, Steer, and Edbril (1983) hypothesized that the biomedical model constitutes a culture-wide folk model to which students are inculcated before they enter medical school. They demonstrate that medical students not only have a strong preference to seek primarily biological data at the very beginning of their education, they continue to exert a preference for biological data even after biopsychosocial analyses are integrated into their curriculum. In this study, even when patient cases were loaded to elicit the kinds of psychological hypotheses suggested by their coursework, over 90% of the student's requests were for biological data, and 60–70% of the students requested only biological data. This means that the compass needle effect may be a barrier to training physicians who take more comprehensive views of their professional problems.

The idea that people are predisposed to categorize the world in specific ways is also consistent with the genetics of personality, where DNA encodes predispositions to seek out types of experiences offered by particular environments (Scarr & McCartney 1983). Genetic endowment does not guarantee that someone will be attracted to specific kinds of worldviews, but it does make certain choices more probable than others. Using a more traditional psychological analysis, William James (1907) talked about temperamental differences between what he called tough-minded and tender-minded thinkers in an attempt to account for his own distaste of experimental work.

Lykken, Bouchard, McGue, and Tellegen (1993) and Waller, Lykken, and Tellegen (1995) claim that interests are one step removed from personality. Using the concept of *reactive gene environment correlations*, they conceptualize interests as learned traits. In this model, genetic endowment determines

both physiognomy and certain personality styles, which in turn predispose people to adopt interests in certain kinds of activities if those activities are environmentally available. If specific activities are available and socially approved, interests in those activities can become “well-traited.” Well-traited interests are stable over time and are more commonly shared by monozygotic twins than by dizygotic twins.

A priori preferences for intellectual experiences calls to mind C.P. Snow’s (1964) characterization of the rift between scientific and literary intellectuals in his *Two Cultures* essay. Snow argued that the intellectual values of mathematicians/physicists and novelists/poets differed so much, that many of them are not even able to converse professionally. Snow tells the story of a humanities scholar, who, at a faculty dinner attempted to make chitchat with the man on his left, but could get no more than a grunt out of him. He then tried to talk with the man on his right, and got another grunt. At this point the man on the right looked at the man on the left and asked “Do you know what he’s talking about?” “I haven’t the least idea” replied the other. The host tried to put his guest at ease by telling him not to worry. “Those are mathematicians! We never talk to them.”

Differences in intellectual temperament may also be reflected in what psychologists refer to as the scientist and practitioner split. The preferred work settings of some psychologists consist in complex arrays of equipment such as Skinner boxes, surgical tools, and computer printouts; other psychologists’ preferred work settings include an intimate therapy room containing two chairs and a tape recorder. Anyone who has spent time in a large psychology department knows that therapists often devalue scientific research as “trivial” and scientists dismiss clinical conceptualizations as “smoke-and-mirrors.” The research and therapy environments are so dissimilar, that two people with radically different jobs, can share the same job title. Empirical research clearly indicates that interest in scientist activities and interest in practitioner activities are negatively correlated with each other, and very stable over time (Leong & Zachar 1991; Zachar & Leong 1992; Zachar & Leong (in press)).

The scientist-practitioner split in psychology probably generalizes to a natural science-human science split in other social science and humanities disciplines — which is what James meant by tough-minded and tender-minded to begin with. Tough-minded thinkers are more comfortable with objective third-person events whereas tender-minded thinkers believe that the events of conscious experience such as emotions, desires, and hopes should

not be ignored. Obviously, biomedical and eliminative materialists' personal make-ups should emphasize natural scientist attributes.

Having done empirical research on scientist-practitioner differences myself, I've grown skeptical about being too dualistic about so-called temperamental differences. For example, it is hard to take an applied behavior analyst and label them as being just a scientist or just a practitioner. Any clinical or counseling psychologist who is interested in assessment has to be knowledgeable about science of psychometry. There are some people who are going to be primarily science-oriented and primarily therapy-oriented come hell or high water, but most people are more malleable.

In this light, it is important to know that Nancy Andreasen earned a Ph.D. in English literature before she became a physician, even though her humanities training is not always reflected in her writings as a psychiatrist. Although Patricia Churchland claims that traditional philosophy is fairly worthless compared to neuroscience, she did get a Ph.D. in philosophy before retraining — and is greatly respected for being someone who can do both. Finally, even though Paul Churchland's language gives the impression that he is a card-carrying member of the fraternal order of natural science nerds, his primary academic interest is still in the resolution of traditional philosophical problems.

Furthermore, the likelihood that there are temperamental influences on what positions someone adopts has no bearing on the accuracy or inaccuracy of those positions. A description of how a person comes to hold a certain set of beliefs has no role to play in the assessment of whether those beliefs are justified. The important point of the temperamental analysis is that disagreements about opinions that are "well-traited" are difficult to resolve when the problems in question have no black-and-white answer.

4. Conclusion

So have I made the world safer for psychology? That is a question readers can answer only for themselves. For readers who began this book as some kind of a sociotrope, refusing to believe in any biological determinants for psychiatric disorders, I've probably knocked psychology down off the pedestal you had it on (if you made it this far). I assume, however, that folk materialism makes most people more inclined to believe in the importance of the brain's role in

the study of human psychology.

For everyone else, I hope I have made it easier for you to believe in the importance of an increased understanding of the brain's role in our explanation and understanding of behavior, without having to dismiss the importance of psychological analyses. Believing in materialism does not require one to adopt physicalism (in the form of behaviorism or physiological reductionism), and believing in consciousness does not require one to adopt mind-body dualism.

This is important because the loose language in the popular and scientific literature leads me to suspect that people have not thought the issues through as clearly as they could. Thinking carefully about language is especially important for mental health professionals. Questions of the type: "If depression is a brain state, what must we do about it?" transect philosophy and psychiatry, theory and practice, economics and morality. It is therefore important for professionals to think about these questions in a comprehensive way. Non-professionals will be inclined to believe what mental health professionals tell them. With increased regularity, people are requesting answers to questions such as: *How much of it is due to my brain?*; *How much of it is due to the way my mother treated me?*; *How much of it is due to the way my family was organized?* Understanding the complexity involved in the brain-behavior relationship can help professionals be more comfortable with answers such as "We don't know for sure, but lets see what can be done about it." Those who don't know for sure have a greater range of potential solutions available to them.

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Name Index

A

Abraham, K. 189
Abroms, E. 215
Aly, G. 283
Amsterdam, B. 147
Andreasen, N. 15, 28, 29, 31, 37, 39, 41,
44-43, 70, 90, 123, 124, 185, 188, 206,
207, 214, 217, 244, 245, 257, 260, 266,
274-275, 284, 304
Angier, G. 299
Applebaum, S. 147
Aquinas, T. 83, 237
Aristotle 83, 95, 102-103, 114-115, 170,
230-231, 240
Astington, J. 6
Averill, J. 165, 172
Ayala, F. 212

B

Bacon, F. 30, 270
Bailey, A. 178
Baillarageon, R. 85-86
Behrs, J. 278
Bechtel, W. 14, 141, 142, 227
Beck, A. 71
Begley, S 299.
Bellarmine 74-75, 116, 117
Bem, D. 193
Ben-Porath, Y. 248
Bennett, J. 194
Bergman, A. 146, 150
Blashfield, R. 37,189, 227, 233, 235, 146
Boakes, R. 110, 111
Boring, E. G. 109, 112
Bouchard, T. 26, 250, 302
Bower, G. 13, 172

Bowlby, J. 146
Breggin, P. 48, 283
Brooks-Gunn, F. 146
Bruetsch, W. 29
Brunner, J. 12
Bunge, M. 116-117
Burleigh, M. 283

C

Cade, J. 35
Carnap, R. 67, 158-159
Cattell, R. 247
Ceci, S. 12
Chalmers, D. 160
Charcot, J. 258
Churchland, Patricia 62, 63-64, 74, 79, 80,
81, 84, 92, 119, 127, 144, 220, 221, 304
Churchland, Paul 14, 57-68, 69-75, 80,
81, 83-84, 92, 93, 94, 97, 98, 100, 102,
109, 114, 116, 131-134, 137, 140-143,
144, 154, 160-163, 176, 205, 214, 220,
245, 259-260, 264, 267-269, 284, 289,
297, 304
Clark, A. 128
Cobb, S. 36
Collingwood, F. 130
Comte, A. 71, 72, 82-83, 116
Conan-Doyle, A. 88
Conway, M. 7
Copernicus 111, 268-269
Corbitt, E. 247
Corning, W. 242-243
Cosmides, I. 238
Costa, P. 213, 248-249
Costal, A. 110, 111, 112, 114
Cowley, G. 300

D

Damasio, A. 170, 172-173, 217-218, 219
 Damon, W. 99
 Darden, L. 127
 Darwin, C. 71, 83, 111, 114, 115, 147,
 172, 240, 284
 Dawes, R. 4, 96
 Dawkins, R. 113, 197, 238, 241
 de Sousa, R. 172
 De Leon, J. 206
 DeCasper, A. 145
 Delay, J. 34-35
 Deniker, P. 35
 Dennett, D. 14, 96, 111, 115, 124, 143,
 145, 149, 150, 155, 162-163, 168, 174,
 177, 192-193, 194, 197, 218-219, 238,
 257, 268
 Descartes, R. 87, 89, 90, 110, 137, 140,
 170, 172, 218
 Dewey, J. 9, 96, 112, 142, 191
 Dickinson, E. 88-89
 Dixon, J. 146

E

Eagle, M. 102, 138
 Ebbinghaus, H. 66
 Edel, A. 269
 Edelman, G. 220
 Einstein, A. 54, 268
 Ellenbecker, H. 31
 Ellis, A. 71, 136, 166
 Ellis, R. 115
 Engel, G. 125-127, 185-186, 302

F

Fairbairn, W.R.D. 101, 138
 Farina, A. 277-278
 Feighner, J. 188, 198
 Feigl, H. 52, 144, 159, 161, 163, 173,
 177, 179
 Feyerabend, P. 53-54, 55, 63, 65-66, 93-
 94, 108, 119, 163
 Fivush, R. 10
 Flanagan, O. 289
 Flavel, J. 86, 149, 150

Fletcher, G. 81, 221
 Fodor, J. 3, 81, 221
 Foucault, M. 94
 Frances, A. 213, 223, 231, 244
 Freeman, W. 281
 Freud, A. 101
 Freud, S. 34, 39, 71, 95, 101, 104, 119,
 130, 131, 135, 137-138, 140, 143, 152,
 170, 173, 218, 279, 284
 Fulford, W. 236

G

Gabbard, G. 40, 186, 195, 198, 235, 244
 Galileo 54, 74, 116, 117
 Gall, F. J. 31
 Gallup, G. 147
 Galton, F. 283
 Gergen, K. 142
 Gill, M. 141-142, 190
 Goethals, G. 6
 Goodwin, F. 299, 300
 Gopnick, A. 6, 148, 150
 Gordon, B. 28, 255, 256
 Gorenstein, E. 213, 214, 216, 229, 234,
 236, 258
 Gottesman, I. 25-26
 Gould, S. J. 238, 282
 Graham, G. 117, 153-154
 Gray, B. 287
 Greenberg, J. A. 139
 Greenwood, J. D. 104
 Griesinger, W. 32
 Grove, W. 231
 Gunderson, J. 102, 213
 Guze, S. 22, 27, 30, 36, 37, 42, 47, 183,
 185, 188, 198, 200, 203, 215, 235, 255,
 259, 284-285

H

Hacking, I. 71, 95, 270
 Haeckel, E. 120
 Hamilton, W. 238
 Hanson, N. R. 57
 Harlow, H. 130

Hart, D. 99
 Harter, S. 99-100
 Hebb, D. 217
 Heider, F. 81
 Heil, J. 91
 Hempel, C. 104, 195
 Hinshaw, S. P. 238
 Hoffman, M. 174
 Holt, R. 190
 Horgan, T. 97, 117
 Hornick, R. 148
 Hull, D. 238
 Hume, D. 72, 117-118, 119, 191

J

Jackson, F. 159, 160, 161, 169, 174, 177
 Jackson, J. H. 172, 249
 Jacobson, E. 102
 James, W. 9, 66, 113, 139, 143, 165, 191,
 219, 220, 264, 302, 303
 Jenkins, J. 173, 174
 Johnson, C. N. 89, 146
 Johnson, M. 288

K

Kahneman, D. 298, 301
 Kalat, J. 3
 Kallman, F. 25.
 Kandel, E. 25, 216, 256
 Kardiner, A. 279
 Kelley, H. 81
 Kendell, R. 227, 236, 242
 Kennedy, J. S. 113, 133
 Kennedy, R. 281-282
 Kernberg, O. 102, 138, 167, 193
 Kety, S. 26
 Kierkegaard, S. 117
 Kim, J. 111
 Klein, D. 22, 39, 56, 188, 205, 214, 224,
 233, 234, 254, 262
 Klein, M. 101, 243
 Klerman, G. 36, 46-47, 188, 190
 Kraepelin, E. 33, 187, 189, 191, 245
 Kramer, P. 263, 298-299, 300-301
 Kroll, J. 47, 235-236, 258

Krystal, H. 166
 Kuhn, T. 20, 21, 36, 66, 119, 263

L

Laing, R. D. 46
 Lakatos, I. 64
 Lakoff, G. 230, 265
 Langs, R. 124
 Laudan, L. 65, 264
 Leahey, T. 9, 284
 Leong, F. T. L. 303
 Lerner, H. 170-171
 Leslie, A. M. 149
 Lewinson, P. 12
 Lezak, M. 90, 276
 Lickey, M. 28, 255, 256
 Lillienfeld, S. 238
 Lillard, A. 150
 Linehan, M. 167-168, 193
 Loeb, J. 109
 Loftus, E. 11, 96
 Lorenz, K. 135
 Luria, A. R. 217
 Lykken, D. 26, 168, 250, 302

M

Mahler, M. 102, 146, 150
 Maull, N. 127
 Maxmen, J. 35-36, 188, 300
 Mayr, E. 197, 211, 240-241
 McCauley, R. 125, 130
 McClelland, 132
 McCrae, R. 213, 229, 243, 247, 248-249
 Mead, G. H. 7, 9, 10, 139
 Meehl, P. 96, 159, 161, 163, 179-180,
 193, 204, 230, 231-232, 251
 Mehta, S. 277
 Meltzoff, A. 145, 146, 148, 150
 Menninger, K. 189, 249
 Meyer, A. 189, 207
 Millon, T. 123-124, 188, 213
 Mitchell, S. J. 139, 140, 235
 Mohl, P. 28, 255, 256
 Moore, M. 145
 Morgan, C. L. 109-114

Mueller, R. 286
Munsterberg, H. 9

N

Nagel, T. 54, 66, 159, 160, 161, 169
Neisser, U. 11, 12
Nemiah, J. C. 166
Newton, I. 120-121, 132, 158, 268
Nietzsche, F. 173
Nisbett, R. 6, 7, 8, 9, 14, 96, 153, 298

O

Oatley, K. 173, 174

P

Pauls, D. 299
Pavlov, I. 66
Perner, J. 150
Peterfreund, E. 102.
Phares, E. 285
Philipse, H. 58, 91
Piaget, J. 86, 146, 147, 148
Pine, F. , 146, 150
Popper, K. 64, 232, 267, 270
Povinelli, D. 147, 149
Premack, D. 146, 149
Prigatano, G. 90, 275-276, 292
Putnam, H. 115, 124

Q

Quine, W. 56-57, 119, 221

R

Ramsey, W. 132
Rappaport, D. 190
Reckman, R. 6
Repacholi, B. 148
Richters, J. 238
Ridley, M. 240
Roberts, W. 114
Robins, E. 36, 188
Rogers, C. 253
Romanes, G. 109-112
Rorty, R. 54-55, 63, 90-91, 93, 96, 108, 117, 122, 265

Rosch, E. 230
Rosenbaum, M. 12.
Rosenhan, D. 23, 46
Ross, L. 14, 298
Roth, M. 7, 47, 235-236, 258
Rumelhart, 132
Ruse, M. 240

S

Scarr, S. 250, 302
Schachter, D. 9
Scheff, T. 46
Searle, J. 177
Sejnowski, T. 127.
Seligman, M. 7, 23
Sellars, W. 52, 53, 65, 80, 93, 153, 163, 177
Simmelweis, I. 290
Shapiro, D. 189
Shorter, E. 33, 36, 313, 190, 191, 281, 282, 284, 287
Sifneos, P. 166
Silverman, D. 302
Skinner, B. F. 39, 51, 72, 107-109, 113, 114, 116, 119, 121, 127, 133-134, 141-143, 152-153, 154, 211, 246, 274, 297
Smart, J. J. C. 52, 158-159
Smith, K. 250-251
Snow, C. P. 303
Sorell, T. 116
Spelke, E. 85
Spitzer, M. 196-197, 215
Spitzer, R. 188
Staats, A. 233
Sternberg, R. 264
Stevens, S. S. 49
Stich, S. 3
Strauss, J. S. 203, 278
Sullivan, H. S. 9, 10, 139
Sulloway, F. 34
Suppe, F. 57
Szasz, T. 46, 229

T

Tavris, C. 170
Taylor, C. 140

Tellegen, A. 26, 210, 231, 248, 249, 250, 302
Thomas, R. 110, 111
Thorndike, E. L. 112
Tinbergen, N. 113, 135
Tinsley, H. 246
Tooby, J. 238
Torrey, E. F. 25-26, 41-42, 273, 274, 275, 279-280, 282, 283, 285
Trivers, R. 238
Trull, T. 213, 242, 247, 285
Tversky, A. 298, 301

V

Vaillant, G. 101, 189
Valenstein, E. 281
Vaughn, S. 255

W

Wakefield, J. 212, 236-237, 262
Wallace, E. R. 236
Waller, N. 248, 302
Watson, J. B. 66, 67, 72, 104, 107-108, 109, 114, 119, 142

Watts, J. 281
Wellman, H. 89, 149, 150
Wender, P. 39, 56, 205, 214, 224, 233, 234, 243, 254, 262
Westen, D. 102, 167
Wexler, B. 216-217
Whitehead, A. N. 248
Whitehorn, J. 30
Widiger, T. 213, 231, 242, 244, 247
Williams, G. 238
Willick, M. 276
Wilson, M. 188
Wilson, T. 6, 8, 9, 14, 96, 298
Wimmer, H. 150
Wimsatt, W. 127
Winnicott, D. 145-146
Winokur, G. 36, 38-39, 42, 188, 275
Wiperman, W. 283
Wolpe, J. 71
Wundt, W. 66, 96, 191

Z

Zachar, P. 303

Subject Index

A

ability argument 162, 164
adaptive-maladaptive 19,138, 189, 211-215, 223, 236, 238, 299
ad hoc hypothesis 39, 152-154
alexithymia 166-167, 168
anti-anthropomorphism 16, 18, 37-38, 71-73, 107-122, 143, 150
anti-psychiatry 45-48, 223, 229, 294
anxiety & panic 38-39, 129, 170, 196, 200-201, 300

B

biomedical materialism 16, 21-49, 68-74, 90, 183-184, 188, 198, 205-207, 214, 224, 233-235, 245, 254-255, 258-260, 265-266, 273-275, 277-279, 281-284, 285, 288, 291, 293, 300-310, 302-304
behaviorism 39, 51-52, 72, 73, 107-109, 133, 134, 142, 153, 154, 254
biopathological process 19, 29-30, 184, 186, 233, 257-260
biopsychosocial model 131, 185-186, 189, 222-225, 245
belief-desire framework (see folk psychology)
bipolar disorder (manic-depression) 26, 35, 130, 214, 259, 278, 298, 299
borderline personality 91-92, 101-102, 167-168, 192-193, 194, 205, 231
bottom-up model 105, 215-217, 219
brain injury (see TBI)
brain-as-substrate (see supervenience)
beliefs 95-96, 97-98, 104, 151, 154, 191-195, 221-222, 266-267

C

causality 14, 60-61, 129-130, 133-134, 158-159, 191-193, 204, 207, 211, 221, 235, 237, 260, 277
categorical model 213, 243-244, 247
categorization 230-232, 242-245
classification 227, 232-249, 242-245, 246
co-evolution 130, 151, 220, 223, 235, 239-242, 245
common sense 3-4, 17, 62, 71, 79, 84-91, 116, 123, 255-256, 296
Comtean positivism 71, 72, 82-84, 116, 183-184
computationalism (connectionsim) 62, 73, 104, 129, 132
consciousness 18, 80, 111, 112, 120, 177, 218-219, 297
confabulation 5-13, 92
Copernican theory 111, 268-269
counseling psychology 100, 173
counter-intuitive explanations 91-92
counter-transference 175-177

D

defense mechanisms 101, 186, 189-190, 253
degeneration 284, 287
depression 13, 38, 41, 56, 104, 129, 132-133, 136, 138, 179, 200-201, 202, 204, 216, 222-223, 224-225, 231, 233-234, 254, 262, 274-275, 300, 305
developmental issues 100, 260, 282
developmental psychology 6, 10, 85-86, 98-100, 145-152

diagnosis 39, 40, 184, 187-191, 228,
236, 244
dimensional model 214, 243-244, 247-
249
disease 19, 23, 46, 206, 233, 235-239,
270, 277-278
dispositions 154-155, 192, 221-222
DSM 40, 187-191, 195-199, 200-203,
207-208, 222-223, 230-232, 244-245,
247-249, 253-254, 264
dualism 40, 128, 160, 177, 222, 305

E

ecology of neuroscience 19, 133-134,
211, 219-220, 223, 241, 242, 245
eliminativism 16, 51-55, 61-68, 68-74
82-83, 91-93, 97-103, 104, 107-109,
113, 118-120, 132-140, 143, 153-154,
160-163, 183, 259-260, 273, 291,
302-304
emotions 10, 104, 136, 137, 140, 143,
148, 165-173
empathy 92, 174, 176, 279-281
essentialism (& anti-essentialism) 19,
73, 91, 227-228, 236, 239-243, 296
ethology (animal behavior) 109-111,
113-115, 133, 136
evolution of mind 111, 147
evolutionary endowment model 144,
145, 149, 152-155, 296
exemplars 20, 23, 259-264, 290-293,
300
explanatory pluralism 113, 130-132,
135-136, 143, 185-186, 222-225, 257-
264, 297

F

falsificationism 64, 232
five-factor model 247-251
first-person, third-person 18, 158, 159-
165, 175-177, 195-199, 297
folk materialism 85-87, 256, 257, 270,
278, 296, 304
folk psychology
in philosophy 16, 17, 61-66, 79-
106, 152-155, 270, 296

in social psychology 81
functionalism 69-70, 80

G

general paresis 23, 30, 129, 216, 233,
245, 257, 259, 270, 287, 290-291
genetics 25-27, 28, 115, 131, 185, 203-
204, 220, 249-251, 287, 299-300,
302-303
the Given 52-53, 55, 119

H

health insurance 42, 285-288
heuristics 298-302
humanitarianism 41-45, 73-74, 173-174,
259, 273
humanistic psychology 191, 194, 253,
262, 280, 292

I

iatrogenics 41-42, 273, 281-285, 290-
293
identity theory 52, 54, 72, 134, 165,
255, 256
implicit definition 55-56
incommensurability 66, 94-95
interlevel theory 127
interpersonal psychiatry 9-10, 138-140,
176, 189 & 207, 205, 263, 292-293
internal versus external 109, 126-128,
134-135, 136-140, 142-143, 146, 147,
154
intertheoretic reduction 65-66
introspection 67-68, 110, 153

L

levels of analysis 124-134, 216-217,
220, 224-225, 233-235, 243, 250-251,
255-257
lithium 35, 234, 244

M

Mary - blind neuroscientist 159-163
materialism 55, 129, 157-159, 160, 177-
178, 196, 250, 305

meaning 53, 54, 56-57, 94, 179-180,
227-228, 235, 296
 medical model 37, 44, 214, 216, 228,
232-239, 246
 memory construction 11-13
 Morgan's Canon 109-114
 moral evaluation 20, 273-274, 288-293

N

natural kind 19, 63, 104, 129, 227-232,
233, 234-245, 247-251, 267, 270
 natural science 65, 66-67, 104-105, 108,
197, 266
 naturalism 118-120, 157-159
 natural selection 115, 133, 143, 153,
154, 212, 238-239, 284
 neo-Kraepelinian 36-37, 187-188, 198,
200, 203, 249
 network theory 55, 56, 58, 94, 95, 221-
222, 289
 neurology 16, 32-24, 46
 Newtonian physics 23, 53-54, 64, 268
 nominalism 241, 243

O

obsessive compulsive disorder 44, 186
 operationalism 190-195, 203, 208
 organic versus functional 40, 222-223

P

paranoia 199, 205
 parsimony 72, 109-111
 physicalism 157-160, 177, 179, 184, 196
 personality 26, 82, 204-205, 213, 249-
251, 303-304
 personality disorders 101, 167, 204-205,
213-214, 223, 228, 229, 238-239,
247-249
 pharmacological therapy 34-36, 44-45,
186, 199, 205-206, 224, 234, 236,
256, 263, 287, 300-301
 phrenology 31
 plasticity of perception 55, 59, 93
 practical kinds 228-230, 236, 243
 practical reasoning 218, 264-265, 270

pragmatism 113, 139, 164-165, 223-225,
228, 266-269, 301
 privacy 53, 167, 177-179
 propositional attitude 73, 80, 114
 prototypes 23, 230-231, 236, 289
 Prozac 36, 234, 256, 263, 286, 297-298,
300-301
 pseudoscience 31, 63
 psychoanalytic/psychodynamic 33-34,
40, 70-71, 92, 95, 101-102, 119, 131,
137-140, 152-153, 188-190, 194, 224,
234-235, 253, 257, 273, 275, 279,
284, 288
object relations theory 130-140,
175-176
 psychological explanation 15-16, 54, 62,
79, 82, 92, 95-97, 99-100, 115, 130,
135-143, 152-155, 175-177, 191-195,
221-222, 256-257, 258, 262, 305
 psychological mindedness 95, 97, 147-
152, 153-155, 280
 psychometry 20, 187, 228-229, 245-249,
266
 psychopathy 101, 168-169, 174, 176,
180, 238
 psychotherapy 4, 39, 45, 70-71, 91-92,
99-100, 120, 175-176, 200, 223-225,
255-257, 262-263, 279-285, 291-292,
300-301
 Ptolemaic theory 22, 111, 152, 268

R

rationality 73, 87, 257-258
 reductionism 124-125, 133, 270
 reality-realism 27-30, 71, 228, 248, 250,
253-257, 265-269, 300
 representations 136, 137, 141, 150, 161

S

schizophrenia 25-27, 28, 29-30, 35, 41,
47, 105, 185, 203-204, 205-206, 214,
216, 229, 231, 236, 237, 245-246,
254, 278, 279-280
 science versus religion 72, 94, 116-118
 scientific realism 28, 55, 60-61, 62, 71,

340 PSYCHOLOGICAL CONCEPTS AND BIOLOGICAL PSYCHIATRY

231-232, 267
scientism 115-120, 128, 161, 183
self-evidence 52, 53, 63, 93-97, 153
self-concept 9-10, 98-99, 261, 295
self 144-152, 167
social constructionism 46-47
Social Darwinism 293-294
social policy re: psychiatry 42, 259, 284,
285-288
species 19, 113-114, 154, 239-243, 270
stigmatization-blame 42-43, 274-281
subjective reactions 148, 198, 199-208,
261, 278
subjectivity 80, 111, 147, 149, 154, 157,
161, 163, 164-177, 178, 179-180,
196-197, 255
supervenience (brain-as-substrate) 15-
17, 20, 22-27, 69, 125, 128-130, 220,
250, 254-257, 260

symptoms & signs 29, 196-198, 213,
216-217, 232-235
syndrome 29-30, 37, 216-217, 232-235

T

taxonomy 239-242, 243-245
theory of mind 147-152
theory 'theory' 60, 98, 153, 154
theory-observation distinction 55, 57-58
traits 82, 249-251
traumatic brain injury (TBI) 24-25, 42-
43, 90, 217-218, 258, 259, 261, 275-
276, 290-291, 292
truth (models of) 232, 256-267, 268-269

U

unity of science 65, 104, 115, 158, 179,
233, 245
utopian argument 73-74, 103, 115, 116,,
274

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