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On unity in psychology: A philosophical study

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ON UNITY IN PSYCHOLOGY: A PHILOSOPHICAL STUDY

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By

James Luke Peacock

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ON UNITY IN PSYCHOLOGY: A PHILOSOPHICAL STUDY

By

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ABSTRACT

ON UNITY IN PSYCHOLOGY: A PHILOSOPHICAL STUDY

By

James Luke Peacock

August 1994

For some time now psychologists have been troubled by the fact that their discipline shares no conceptual or methodological framework in which its practitioners work. In this thesis I analyze this problem of unification in psychology. The philosopher Thomas S. Kuhn argues that, absent such a unifying framework, a discipline cannot begin to be considered mature. Through a detailed review of current psychological literature I conclude that psychology's disunity is caused by incommensurable competing frameworks, and that the problem of unification is rooted in an ontological dispute, i.e., the classical mind-body problem. As a solution, I argue for an antirealist reading of psychology to obviate the ontological dispute. I conclude by proposing Paul C. L. Tang's Complementarity Model of MindBrain. I suggest that Tang's model might very well be the unifying framework that psychology needs.

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This thesis is dedicated to my love Joyce, my brothers, my sister and my parents. They each in their own way command my deep love, respect and admiration. This thesis is a small token of appreciation for all they mean to me individually and collectively.

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CHAPTER ONE

INTRODUCTION

There is a perplexing aspect of psychology that distinguishes it from the natural sciences. Despite the fact that psychology is one of the older sciences, its scientific development has been slow and has produced few significant achievements. Indeed, one prominent psychologist and historian has gone so far as to characterize 20th century psychology as a mere "footnote to the 19th century."[1] John Haugeland makes a similar point, concluding that psychology's "discoveries are conspicuously narrow, even small, compared to the depth and scope of psychology's pretheoretic purview."[2] According to Haugeland, all that is required to surmise clearly just how little psychology has enlightened us is a bit of brief reflection. Haugeland continues:

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[1]Daniel N. Robinson, An Intellectual History of Psychology (New York: Macmillan, 1976), 34. Sigmund Koch concurs with Robinson, arguing that for the most part psychology is guided by commitments originally conceived by philosophers. See Koch, "The Nature and Limits of Psychological Knowledge: Lessons of a Century qua Science," American Psychologist 36 (March 1981): 267.

[2]John Haugeland, "The Nature and Plausibility of Cognitivism," The Behavioral and Brain Sciences 1 (1978): 224.

> How is it, for example, that we recognize familiar faces, let alone the lives reflected in them, or the greatness of Rembrandt's portrayals? How do we understand conventional English, let alone metaphors, jokes, Aristotle, or Albee? What is common sense, creativity, wit, or good taste?

Haugeland's questions address phenomena that go beyond the ken of psychology. The point Haugeland makes is that psychology has not achieved a significant level of fecundity in important areas of its domain. Consequentially, the discipline has not developed to the same extend as the natural sciences.

My aim in this thesis is to say something illuminating about the nature of psychology, and the correlated problems in its development. Philosophers of science have, for some time now, written on scientific development in general, and many have written specifically on psychology's development. Likewise, many psychologists acknowledge past and present difficulties, and have documented specific troublesome areas in psychological inquiry. It makes sense, therefore, to seek an understanding of psychology's woes with the guidance of philosophical positions and psychological research.

A conclusion consistently surfacing from the philosophical and psychological literature is that psychology is a disunified discipline. By that I mean that there is little consensus among its practitioners over basic theoretical and methodological commitments. This results in there being no unified framework to guide psychology. Without a unified framework, psychologists lack consonant conceptual and methodological direction. No wonder, then, that psychology is disunified.

The central project of this thesis is to resolve what I call the "problem of unification" in psychology. I hope to accomplish this task in the following manner. First, I shall argue that psychology is a disunified discipline whose competing frameworks are incommensurable. Moreover, I shall argue that such incommensurability ensures that psychology will never become unified. Second, the incommensurability of psychological frameworks is rooted in the traditional mind-body problem. To overcome the mind-body problem, I shall suggest that psychology be treated in an instrumentalist, pragmatic fashion, i.e., antirealistically. Third, based upon the antirealism I previously developed, I shall attempt to resolve the problem of unification by developing Paul C. L. Tang's Complementarity Model of MindBrain.

This thesis will consist of five chapters. In chapter two I shall establish that there is a problem of unification in psychology by reviewing current psychological literature. Currently, there is considerable interest in psychology in the problem of unification. Next, I shall investigate the problem of unification within psychology's historical record. A brief synopsis of major movements in the history of modern psychology will be offered that highlights a disjointed pattern of development. Last, I shall provide a brief exposition of Thomas S. Kuhn's theory of scientific development. Included in this discussion will be an explanation of key terms, such as paradigm, normal science, anomaly, crisis, revolution. Moreover, I shall elucidate Kuhn's notion of a mature science since, according to Kuhn, unification is one of the defining characteristics of a mature science. For a science to be mature, its scientific community must employ a common paradigm which provides a conceptual and methodological framework in which practitioners of the field work. I conclude the chapter by noting that psychology is not a Kuhnian mature science.

Chapter three builds on the previous chapter. Here, I shall attempt to explain why psychology is disunified. To accomplish this, I shall introduce the incommensurability thesis. Further, I shall argue that two types of incommensurability, viz., topic incommensurability and meaning incommensurability, clearly capture the scope and breadth of the disunity. In so arguing, I shall make use of a review of current psychological literature in neuropsychology and cognitive psychology. This review will demonstrate that the subdisciplines of psychology are incommensurable. Last, I shall argue that the incommensurability thesis suggests that psychology's incommensurable frameworks can never become unified.

In chapter four I shall clarify the discussion of incommensurability of chapter three. I shall argue that the incommensurability indicates deep ontological debates, centered around the mind/body problem. Philosophers of science have long debated the ontological importance of science. Currently, the debate is between realism and antirealism. Realism argue that successful sciences are generally referential, and should be accepted as offering facts about the world. Realism, though, continues to be plagued with many difficulties. The most serious challenge to realism is the underdetermination thesis. This thesis, most strongly advanced by W. V. O. Quine, shows that for any set of empirical statements describing a phenomenon, there is an infinite number of theories that imply the set. Consequently, it is possible to construct alternative theories that are empirically indistinguishable yet logically compatible.

After considering the underdetermination thesis, I shall investigate the realist claim of the development of science as accumulation of knowledge. I offer an alternative account of the history of science. I conclude that realism can not give a coherent treatment of the historical record of science.

Next, I shall offer as an example of a antirealist reading of science the highly successful theory of Quantum Mechanics. The Copenhagen Interpretation of Quantum Mechanics, developed primarily by Niels Bohr, argues that one of the most surprising phenomena emergent from quantum studies is the particle-wave duality of light. That is, the double slit experiment demonstrates that light is both wave-like and particle-like. To explain this conceptual paradox, Bohr advanced the complementarity model of light. This allowed for incorporation within a single theory two mutually exclusive properties of the phenomenon of light. Depending upon the circumstances, light is either measured as a wave or as a particle.

A realist reading of light complementarity gives rise to philosophical problems, viz., that light cannot be considered both wave and particle in its fundamental character. Yet, that is exactly what quantum physicists say. To obviate this problem, quantum theory is treated anti-realistically. No longer is concern given to describe how the world really is; rather, quantum mechanics is treated in a pragmatic and instrumentalist fashion.

I shall end chapter four by concluding that the realist position does not make sense of science. I then shall argue that the proper stance to all scientific theories is anti-realistic. That is, the scientific theories do not produce literally true accounts of the world. In support of my antirealist position I shall employ Bas van Fraassen's antirealist position, which he calls constructive empiricism.

In chapter five I shall develop an anti-realist approach to mind-body, modeled after my discussion in chapter four. Specifically, I present Tang's antirealist Complementarity model of MindBrain. The Complementarity Model of MindBrain, I shall argue, will be in a unique position to elucidate and resolve current debates centered on the mind-body problem in psychology, including the problem of unification.

CHAPTER TWO

THE PROBLEM OF DISUNITY

In his 1991 Presidential Address to the Canadian Psychological Association, John B. Conway begins:

> I shall consider an old question in this address. The question is this: How is it that we psychologists come to hold such contrasting metatheoretical positions about the discipline? What leads some of us to believe that the mind is brain, that human behavior is completely determined, or that humans can be explained by the laws of a natural science of behaviour, while others of us reject such beliefs in favor of contrasting positions? How do we make up our minds about where we stand on such large issues?[1]

Psychologists have not made up their minds on the metatheoretical issues raised by Conway; they continue to debate the very questions Conway raises. Moreover, such metatheoretical debates are not new to the discipline.

In this chapter I shall provide a brief review of some current writings by psychologists on the lack of a unified metatheoretical view of the discipline. Next, I shall provide a limited and brief history of modern psychology. The historical record from Wilhelm Wundt's structuralism, through functionalism, to behaviorism

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[1]John B. Conway, "A World of Differences Among Psychologists," Canadian Psychology 33, no. 1 (1992): 1.

illustrates a disjointed pattern resulting from different answers to key metatheoretical questions. I aim to show that metatheoretical issues in psychology have been debated since the beginnings of modern scientific psychology. Next, I shall consider the work of Kuhn, whose socio-historical and philosophical analysis of scientific development will be presented. I shall suggest that Kuhn's historical and philosophical analysis is particularly helpful for discussing the metatheoretical issues facing psychology. Care will be taken to describe precisely Kuhn's notion of a mature science. I shall provide an example of Kuhnian mature science which will serve as a point of comparison and contrast to the disunified state of psychology. Mature science, I shall argue, ought to be the goal of psychology.

Disunified Discipline

One approach to understanding psychology's disunity is by explicating the position of psychology among the sciences. Customarily, psychology is placed between the natural sciences and the social sciences.[2] The interdisciplinary divisions of psychobiology, neuropsychology and psychopharmacology are areas closely akin to, and allied with, the natural sciences. Nevertheless, when consider-

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[2]I am borrowing this distinction from Herbert Feigl, "Philosophical Embarrassments of Psychology," American Psychologist 14 (1959): 115-128.

ing clinical psychology, personality psychology, and cognitive psychology, psychology appears remote from the natural sciences and more akin to the social sciences. As shall be shown, this splintering of psychology across different traditions of natural science and social science indicates deeper divisions.[3]

Just two years after the profession celebrated its centennial, psychologists Arthur Staats and Sigmund Koch echoed calls for significant interdisciplinary revisions. Staats summarizes his view of the state of psychology as follows:

> The concept of separatism describes our science as split into unorganized bits and pieces, along many dimensions. Divisions exist on the basis of theory, method, and the types of findings that are accepted, as well as on the basis of student training, organizational bodies such as divisions, journals, and individual strivings. Our field is constructed of small islands of knowledge organized in ways that make no connections with the many other existing islands of knowledge.[4]

Other psychologists have echoed Staats' assessment. For instance, Howard H. Kendler writes that the "unity of psy-

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[3]The psychologist Howard H. Kendler discusses at length the tension between a natural science psychology and a social science psychology in "A Good Divorce is Better than a Bad Marriage," in Annals of Theoretical Psychology, vol. 5, eds. Arthur W. Staats and L. P. Mos (New York: Plenum Press, 1987), 55-89.

[4]Arthur Staats, "Paradigmatic Behaviorism, Unified Theory, Unified Theory Construction Methods, and the Zeitgeist of Separatism," American Psychologist 36 (March 1981): 239.

chology has all but collapsed. Psychology is a multi-disciplined field with different segments employing irreconcilable orientations."[5] Hans J. Eysenck agrees, noting that

> after more than a century of official existence psychology still lacks a coherent set of values. There is little harmony among different groups of psychologists engaged in different specialized disciplines, and there is even debate over the definition of our subject matter.[6]

There is wide agreement that psychology consists of disparate frameworks, and that this has disunified the discipline.[7] However, what is to be done about the disunity is fiercely debated. Should psychologists strive for unification? Or, rather, should psychologists continue to foster fragmentation? Staats has been the most vocal of those psychologists pressing for unification of the frameworks that make up the discipline. While not

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[5]Howard H. Kendler, Psychology: A Science in Conflict (New York: Oxford Press, 1981), 371.

[6]Hans J. Eysenck, "The Growth of a Unified Scientific Psychology," in Annals of Theoretical Psychology, vol. 5, eds. Arthur W. Staats and L. P. Mos (New York: Plenum Press, 1987), 91.

[7]By "framework" I mean the collective conceptual and methodological commitments of a group of psychologists, e.g., behaviorism, cognitive psychology, neuropsychology, humanism, etc.

successful yet at achieving his ultimate goal, Staats has been attracting attention to his cause.[8]

Staats, though, has not been without his critics. Many psychologists see the goal of a unified psychology as unattainable. Koch, in the same edition of the journal, argues against Staats' goals.[9] Rather, Koch claims that the discipline has tried to handle too much, and that "psychologists must finally accept the circumstances that extensive and important sectors of psychological study require modes of inquiry rather more like those of the humanities than the sciences."[10] He suggests the replacement of the discipline of Psychology with a general "Psychological Studies." This, Koch informs us, would permit the disunity that is unresolvable.[11]

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[8]For instance, in 1985 Staats helped establish the "Society for Unity Issues in Psychology." See Arthur Staats, "Unified Positivism and Unification Psychology: Fad or New Field?," American Psychologist 46 (September 1991): 899-912.

[9]Koch, "The Nature and Limits of Psychological Knowledge," 257-269. As the remainder of this thesis will show, I am unconvinced of the need to purposefully disunify psychology. What Koch's proposal offers, at best, is institutional separation. There still would be, I believe, persistent questions concerning, say, cognition and its relation to the brain.

[10]Ibid., 257.

[11]For an interesting discussion of the possible consequences of Koch's view see Thomas R. Scott, "A Personal View of the Future of Psychology Departments," American Psychologist 46 (September 1991): 975-976.

Attempting to understand better the metatheoretical issues that divide psychology, Gregory A. Kimble, in a well known and interesting study of attitudes among psychologists, found evidence of a fragmented discipline.[12] Based on a survey of psychologists' attitudes, Kimble concludes that there is a dichotomy of "two cultures" in the discipline comprising the scientists and the humanists. Kimble's research is consistent with other works. For instance, D. W. Fiske and R. A. Shweder depicted the dichotomy as "related to different degrees of emphasis on subjectivity, diverse views about how subjective experience is to be used or avoided in social science, and opposed positions on the extent to which science is compatible with subjectivity."[13] Kimble, characterizing the disunity as "psychology's identity problem", later concludes from the Staats/Koch debate:

> Although I would prefer to side with Staats on this question, Koch's pessimistic assessment may be more realistic, if only because the disagreements have been around for so long, in such varied contexts, and expressed by so many different individuals with no indication that we are moving toward consensus.[14]

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[12]Gregory A. Kimble, "Psychology's Two Cultures," <u>American Psychologist</u> 39 (August 1984): 833-839.

[13]D. W. Fiske and R. A. Shweder, <u>Metatheory in Social Science: Pluralisms and Subjectivities</u> (Chicago: University Press, 1986), 367.

[14]Kimble, "Psychology's Two Cultures," 833. In chapter five of this thesis I shall propose what I believe is a plausible approach to the disunity problem.

In investigating why psychology has not been successful at achieving a unified conceptual and methodological framework, it is important to consider the historical record. As early as the turn of this century, the American psychologist William James was concerned with achieving unity of the discipline.[15] That there is a long-standing history of this debate is significant because it helps to put the problem in perspective. The fact that modern psychology has been confronted with the extremely problematical unity-disunity problem informs us that it may be a deep metatheoretical issue that conflicts with traditional conceptions of the discipline.

I shall turn my attention to briefly examining the modern history of psychology. There are, I shall show, definitive patterns in the history of modern psychology that are evidence for concluding that the unity-disunity problem is not new. Some thirty five years ago, the philosopher Herbert Feigl characterized the friction between psychology as competing frameworks arguing for a psychology that is nothing but a natural science (i.e., no talk of mental events), versus a psychology that is

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[15]See Wayne Viney, "The Cyclops and the Twelve-Eyed Toad: William James and the Unity-Disunity Problem in Psychology," American Psychologist 44 (October 1989): 1261-1265.

something more than a natural science (i.e., the inclusion of mental events).[16]

There is significance in Feigl's distinction for contemporary psychology. Competing frameworks are still arguing over the proper route for psychology, causing, in part, the unity-disunity problem. In order to address effectively this problem, it is important to understand how psychology in the past has developed.

Disunity in the History of Psychology

Not until advances in biology did psychology begin to establish its modern roots. Developments in neurophysiology by Johannes Muller (1801-1858), psychophysics by Ernst H. Weber (1795-1878) and Gustav T. Fechner (1801-1887), in addition to Darwin's theory of evolution all led researchers to emphasize a new route for psychology. The culmination of this new route occurred in the work of Wilhelm Wundt, the father of modern scientific psychology.[17]

In 1875 Wundt, a medical doctor, was appointed to the Chair of Philosophy at the University of Leipzig. Until then, Wundt's background consisted of research into physiology. However, his concerns quickly changed to psychol-

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[16] I am borrowing the terminology and distinction from Feigl, "Philosophical Embarrassments," 122.

[17] I am using Robert W. Lundin's summary of Wundt's psychology in Theories and Systems of Psychology, 3d ed. (Lexington, MA: D. C. Heath and Company, 1985), 79-92.

ogy, which was still part of the philosophy department. Nonetheless, his development of a laboratory at the University was influential in breaking the link between philosophy and psychology.

According to Wundt, the subject matter of psychology was distinct from the other sciences. Psychology, for Wundt, was the study of conscious experience, including sensations, feelings, emotions, and ideas.[18] All conscious experience has two aspects, the material and the subjective. The material aspect of consciousness constitutes mediate experience while our subjective experience of it is immediate experience. In the case of seeing an object, mediate experience would consist of light and sound waves, while immediate experience would consist of color and tone. Immediate experience was the concern of the psychologist. As Wundt wrote, concerning the relation between the two types of experience:

> As a result of this relation, it follows that there must be a relation between all facts that belong at the same time to both experiences of the natural sciences and to the immediate experiences of psychology, for they are nothing but components of a single experience which is mere-

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[18]Wundtian psychology is more commonly referred to as structuralism. Furthermore, Wundt's student Edward Bradford Titchener is credited with developing structuralism in America.

> ly regarded in the two cases from different points of view.[19]

Wundt's primary methodology was introspection. He employed this technique, also called inner perception, to observe conscious experience directly. He believed that introspection was analogous to traditional notions of observation used in other sciences. Subjects participating in Wundt's experiments were trained introspectionists. Training focused on building skills to (a) determine the introduction of a mental elements, (b) maintain attention, (c) repeatedly experience mental content, and (d) isolate mental structures.

In sum, Wundt's commitment to a psychology that studied mental phenomena is readily apparent.

> The assertion that the mental life lacks all casual connection, and that the real and primary object of psychology is, therefore, not the mental life itself but the physical substrate of that life-- this assertion stands self-condemned.[20]

In addition, his use of introspection, coupled with a focus on subjective conscious experience clearly distinguished psychology from the natural sciences. That is,

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[19]Wilhelm Wundt, Völkerpsychologie, vol. 1 (Leipzig: Englemann, 1900-1920); quoted in Lundin, Theories and Systems, 87.

[20]Wilhelm Wundt, Principles of Physiological Psychology, vol. 1 (New York: Macmillan, 1910); quoted in Howard H. Kendler, Historical Foundations of Modern Psychology (Philadelphia: Temple University Press, 1987), 42.

Wundtian psychology was something more than a natural science.

However, Wundt's influence on psychology began to wane. In its place developed a new psychology that focused on the functions of mental events, hence the name "functionalism." Contrary to Wundtian psychology, functionist psychology focused on the functions, or roles, of these mental operations. For instance, mental events such as perception, memory, feeling, judgement and will were considered adaptive behavior. Two early influences were John Dewey and James Angell. Dewey's 1896 paper, entitled "The Reflex Arc Concept in Psychology," and Angell's 1904 textbook, Psychology, are clear indications of a separation from the then dominant Wundtian psychology. Both works attacked the elementalist and reductionist psychology of Wundt.

The psychologist Harvey Carr represents functionalism at its height. According to Carr, the subject matter of psychology should be the study of mental activity. As he writes:

> The type of conduct that reflects mental activity may be termed adaptive or adjusted behavior . . . An adaptive act is a response on the part of an organism in reference to its physical or social environment of such a character as to satisfy its motivating conditions.[21]

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[21]Duane P. Schultz and Sydney Ellen Schultz, A History of Modern Psychology, 5th ed. (New York: Harcourt Brace Jovanovich, 1992), 195.

Carr considered psychology to be closely aligned to biology. Nonetheless, mental phenomena still separated the psychologist from the biologist. In this regard, Carr promoted a psycho-physicalism:

> We shall make no attempt to explain the nature of this psycho-physical relationship. We merely note the fact that these mental acts are psycho-physical events and insist that they must be studied as such.[22]

Functionalism, then, conceives of psychology, similarly to Wundt, as <u>something more</u> than a natural science. However, important trends were developing. Functionalism de-emphasized the role of introspection, while introducing a more Darwinian "adaptation" view of mental processes. Moreover, increased importance was placed on empirical research, and the emerging field of comparative psychology. These trends later led to a radical break from both Wundtian psychology and functionalism.

John B. Watson, in 1913, began the process of even more radically transforming psychology. His brand of psychology, behaviorism, was eventually to affect all areas of psychological inquiry. Watson's 1913 paper, "Psychology as the Behaviorist Views It," is known as the "behaviorist manifesto" because it was a bold, straightforward, definitive statement of a new brand of psychol-

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[22]Ibid., 198.

ogy.[23] Watson, in effect, redefined psychology as the study of behavior. No longer was it proper to study mental phenomena. For Watson, a psychology excluding mental phenomena was clearly a branch of the natural sciences.

Later in the Behaviorist movement came B. F. Skinner, possibly America's most influential psychologist ever. Skinner's psychology, called radical behaviorism, was committed to the study of behavior and its environmental controls. In Science and Human Behavior, Skinner argues that the environment acts upon the organism to produce behavior.[24] If psychology can describe accurately the relation of environment to the organism's behavior, then any reference to the mental is otiose.

> The objection to inner states is not that they do not exist, but they are not relevant in a functional analysis. We cannot account for the behavior of any system while staying wholly inside it; eventually we must turn to forces operating on the organism from without. [25]

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[23]John B. Watson, "Psychology as the Behaviorist Views It," Psychological Review 20 (1913): 158-177.

[24]B. F. Skinner, Science and Human Behavior (New York: Macmillan, 1953).

[25]Ibid., 35. The topic Skinner raises here was the subject of a well known essay by Carl G. Hempel entitled "The Theoreticians Dilemma," chapter in Aspects of Scientific Explanation and Other Essays in the Philosophy of Science (New York: The Free Press, 1965), 173-228.

Behaviorism was the first substantial modern psychological framework to argue for psychology as nothing but a branch of the natural sciences.

We see in this brief account of psychology's recent history a fragmented pattern of development. This pattern comports with the larger historical patterns displayed in psychology.

> Different schools of thought have developed during the course of the history of psychology, each one an effective protest against what had gone before . . . As the new system developed and gained supporters and influence, it inspired opposition and the whole combative process began anew.[26]

In this section I argued that a similar pattern can be seen regarding major transitions from Wundt to Skinner. First was structuralism, which placed central importance on mental phenomena. Next came functionalism, which also included mental phenomena, but in a more limited way. Lastly, behaviorism excluded all talk of mental phenomena. And the debate continues. Over the years behaviorism has been seriously challenged by cognitive psychology. With the rise of cognitivism came a return to the study of mental phenomena, and thus a return to a psychology as something more than a natural science. Presently, cognitive psychology is the dominant framework, so the discipline is for the most part held to be closely related to

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[26]Schultz and Schultz, History of Modern, 20.

the social sciences. Though, as with previous times, cognitive psychology is not the framework of psychology. In fact, challenges from neuropsychology, and the like, are forcing psychologists to continue to address their status within the sciences.

Kuhn on the History and Philosophy of Science

Kuhn's influential book, The Structure of Scientific Revolutions (SSR), gave rise to a new approach to understanding the sciences.[27] In SSR, Kuhn offers a theory of scientific development that characterizes the history of science as a series of discontinuous paradigm shifts.[28] Between these shifts are a series of developmental stages: normal science; anomaly; crises; adoption of a paradigm; eventual return to normal science. I shall present a

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[27]Thomas S. Kuhn, The Structure of Scientific Revolutions, 2d ed. (Chicago: University of Chicago Press, 1970).

[28]Kuhn developed his theory of scientific development by studying the history of physics and chemistry and generalizing his results to all of science. Other philosophers have explored Kuhn's theory in other disciplines. For instance, Michael Ruse applies Kuhn's theory to Biology, "Two Biological Revolutions," Dialectica 5 (1971): 17-38. For a discussion of Kuhn's theory within the history of psychology, anthropology and biology, see Paul C. L. Tang, "Paradigm Shifts, Scientific Revolutions, and the Unit of Scientific Change: Towards a Post-Kuhnian Theory of Types of Scientific Development," PSA 1984, Proceedings of the 1984 Biennial Meeting of Philosophy of Science Association, vol. I, eds. Peter D. Asquith and Philip Kitcher, by the Philosophy of Science Association (East Lansing, MI: Philosophy of Science Association, 1984), 125-136.

brief overview of Kuhn's theory, focusing on the above key concepts. Then, I shall discuss Kuhn's idea of mature science. Kuhnian mature science states that in order for a science to be mature, it must possess a dominant and unifying paradigm. I shall conclude by arguing that psychology is not a Kuhnian mature science.

SSR is both a philosophical and socio-historical analysis of scientific development. In the opening line of SSR, Kuhn writes: "History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed."[29] The "image of science" that Kuhn challenges was developed primarily by the logical positivists, and is considered the "received view". Ian Hacking summarizes the salient points of the "received view" that Kuhn challenges.[30] Traditionally, science is viewed as cumulative enterprise that aims to discover the real world. Moreover, science is sharply distinguished from other sources of belief, partly because scientific concepts are precise, having fixed meanings. Further,

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[29]Kuhn, Structure, 1.

[30]The "received view" is meant to include the work of Karl Popper, Rudolf Carnap and Hans Reichenbach. See Ian Hacking, Representing and Intervening (New York: Cambridge University Press, 1983), 1-5. For a useful brief account of the "received view" see Dudley Shapere "Meaning and Scientific Change," in Scientific Revolutions, ed. Ian Hacking (New York: Oxford University, 1981), 28-59.

there is a robust distinction between observational reports and theoretical statements, with the former providing the foundations for justifying theories.

Contrary to the received view, Kuhn introduces the term "paradigm" to discuss his theory of scientific development. Paradigms consist of the entire constellation of beliefs, values, techniques, etc., of a scientific community. In the postscript to SSR, Kuhn equates "paradigms" with a disciplinary matrix.[31] A disciplinary matrix is "'disciplinary' because it refers to the common possession of the practitioners of a particular discipline; 'matrix' because it is composed of ordered elements of various sorts, each requiring further specification."[32] There are four characteristic parts of a disciplinary matrix: symbolic generalizations, models, values and exemplars. Symbolic generalizations are expressions which can be cast in logical or mathematical form, supplying essential laws, formulas and definitions to the scientific community. Often they take symbolic form, but they can also be expressed in words. Models captures the shared commitments

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[31]Kuhn, Structure, 137. Kuhn introduced the idea of a disciplinary matrix after he received heavy criticism over his notion of a paradigm. Margaret Masterman offered the most poignant criticism, claiming that Kuhn uses the term 'paradigm' in twenty-one different senses. See her "The Nature of a Paradigm," in Criticisms and the Growth of Knowledge, eds. Imre Lakatos and Alan Musgrave (New York: Cambridge University Press, 1970), 59-90.

[32]Ibid., 138.

of scientists to such beliefs as: heat is the kinetic energy of the constituent parts of bodies. Models expressly refer to the metaphysical portion of a disciplinary matrix, that is, the analogies, metaphors, models and beliefs associated with them. The function of a model is to set the agenda of the discipline, determining the problems to solved. Values are basic commitments to the way the discipline is to be practiced and include standards of empirical practice and qualitative judgements. The final element of the disciplinary matrix, exemplars, are "the concrete problem-solutions that students encounter from the start of their scientific education, whether in laboratories, on examinations, or at the ends of chapters in science texts."[33]

Paradigms form the foundation upon which normal science proceeds. The aim of normal science is to actualize the promise(s) of the paradigm. Kuhn refers to this as "mopping-up operations", and writes:

> Mopping-up operations are what engage most scientists throughout their careers. They constitute what I am here calling normal science. Closely examined, whether historically or in the contemporary laboratory, that enterprise seems an attempt to force nature into the preformed and relatively inflexible box that the paradigm supplies.[34]

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[33] Ibid., 187.

[34] Ibid., 24.

Normal science, then, is an activity that aims to increase the scope and precision of the paradigm. At no time is the aim the production of novel facts or theories. Rather, normal science is bound by the paradigm it is articulating.

Occasionally, though, there are difficult cases that seem to resist satisfactory resolution within paradigm based expectations. If these anomalies persist, the paradigm itself may be called into question. If continued attempts to adjust the paradigm fail, a crisis occurs in the paradigm. If the anomaly continues to be resistant, the paradigm might be displaced, and a new paradigm might emerge. Kuhn calls such of paradigm shifts scientific revolutions. Scientific revolutions are "those non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one."[35] With a change in paradigms comes a change between incompatible modes of a scientific community. The paradigm shift occurs between paradigms that are incommensurable with each other.[36]

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[35]Ibid., 92. For a contrary view, see Karl Popper "The Rationality of Scientific Revolutions," in Scientific Revolutions, ed. Ian Hacking (New York: Oxford University Press, 1981), 80-106.

[36]The incommensurability thesis was jointly introduced to philosophers of science Paul Feyerabend and Kuhn. For discussion of this point, see Hacking, Representing and Intervening, 67. Some philosophers have rejected the incommensurability thesis because it portrays scientific

For Kuhn, a paradigm functions to unify a discipline through the sharing of the conceptual and methodological elements of the disciplinary matrix. Only when a paradigm is adopted as normal science is a science considered to be mature. Disciplines that achieve normal science are captured in Kuhn's idea of a mature science. The transition from immaturity to maturity occurs because the paradigm achieves a high degree of success in the domain in question, and this achievement is recognized by practitioners in the field. The result is a more directed and narrowly focused field. As Kuhn writes:

> The practitioners of a mature scientific specialty are deeply committed to some one paradigm-based way of regarding and investigating nature. Their paradigm tells them about the sorts of entities with which the universe is populated and about the way the members of that population behave.[37]

Standard example of a mature science are Newtonian Mechanics and Neo-Darwinism. However, mature sciences do not have to come in such large scales as these. For instance, Kuhn discusses electricity theory, and its subsequent transition to maturity:

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development as based upon nonrational theory acceptance. See Isreal Scheffler's Science and Subjectivity (Indianapolis: Bobbs-Merril, 1967), 147-149.

[37]Thomas S. Kuhn, "The Function of Dogma in Scientific Research," in Scientific Knowledge: Basic Issues in the Philosophy of Science, ed. Janet A. Kourany (Belmont, CA: Wadsworth Publishing Company, 1987), 260.

> What the fluid theory of electricity did for the subgroup that held it, the Franklinian paradigm later did for the entire group of electricians . . . Only the paradigm did the job far more effectively, partly because the end of interschool debate ended the constant reiteration of fundamentals and partly because the confidence that they were on the right track encouraged scientists to undertake more precise, esoteric, and consuming sorts of work. Freed from the concern with any and all electrical phenomena, the united group of electricians could pursue selected phenomena in far more detail, designing much special equipment for the task and employing it more stubbornly and systematically than electricians had ever done before. Both fact collection and theory articulation became highly directed activities.[38]

Kuhnian mature sciences, then, have highly focused paradigms. Newtonian Mechanics, Neo-Darwinism, and Franklinian electrical theory are all historical examples of paradigms that were unquestioned, entrenched, as well as deeply committed to by practitioners in the field. These paradigms successfully influenced entire scientific communities to confront their discipline in a unified manner.

Conclusion

Kuhn's analysis of science leads to the identification of a specific and defining problem of psychology, viz., the absence of a paradigm.[39] Kuhn argues that in

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[38]Ibid., 18.

[39]Kuhn does not directly address the development of psychology. As he writes: "The nature of the transition to maturity deserves fuller discussion than it has received in this book, particularly from those concerned with the development of the contemporary social sciences." Kuhn, Structure, 178-179.

the absence of a paradigm, a discipline cannot achieve a unified approach, either conceptually or methodologically, and hence cannot even begin to be considered mature. As I have shown, the development of a mature scientific psychology has eluded psychologists and philosophers for some time.

Presently, there is a sharp contrast between Kuhnian mature sciences and contemporary psychology. Consider Staats's summation of the present state of psychology:

> It is my thesis that psychology suffers from a crisis of disunity . . . Psychology has developed the prolific character of modern science, without the ability to articulate its knowledge. The result is a great and increasing diversity--many unrelated methods, findings, problems, theoretical languages, schismatic issues, and philosophical positions.[40]

As we saw earlier, the views expressed in the above passage are well rehearsed in both the philosophical and psychological literature. What is not found in the literature though, are discussions of the criteria for solving the problem of unification. Kuhn provides such criteria in SSR with his theory scientific development.

In this chapter I have shown the following: (1) psychologists consider their discipline to be highly disunified; (2) the history of psychology shows a pattern of disunity, i.e., lack of an overriding paradigm;

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[40]Staats, "Unified Positivism and Unification Psychology," 899.

(3) Kuhnian mature science, requiring an overriding paradigm, continues to be an elusive goal for psychology. In the next chapter, I shall explore just what it is that makes the goal of a mature psychology so elusive.

CHAPTER THREE

THE PROBLEM OF INCOMMENSURABILITY

In the last chapter, I argued that the disunity that psychologists have identified has stopped their discipline from achieving scientific maturity. In this chapter, I _explain_ why there is disunity in psychology. To accomplish this task, I shall expand upon the incommensurability thesis mentioned in the previous chapter. As I shall argue, the incommensurability thesis is able to explain the present disunity of subdisciplines of psychology by identifying important features of scientific language. In developing this account, I shall offer a few representative examples taken from psychological literature. Moreover, I shall argue that the incommensurability thesis blocks any hope of future unification of the subdisciplines of psychology.

The Incommensurability Thesis

Kuhn and Paul Feyerabend offer an insightful examination of science that emphasizes an analysis of the language of science. According to Kuhn and Feyerabend, all scientific terms are rooted in theory. This "theory-ladeness" entails that the meaning of all scientific terms depend on the theory or framework in which they are embedded. As Feyerabend writes, "the meaning of every term we use depends upon the theoretical context in which it occurs. Words do not 'mean' something in isolation; they obtain their meanings by being part of a theoretical system."[1] A "theoretical system" is a network of terms. Each network uniquely places its terms in a theoretical system. Similarly, each network uniquely determines, in part, the relationships between terms. The meaning of terms, then, is network dependent.

This was a radical break from the standard view advanced most strongly by the logical positivists.[2] According to this contrary view, all nonlogical vocabulary in science can be divided into two parts: observational terms and theoretical terms. The observational terms apply to publicly observable items and their qualities, e.g., "red" and "apple". Theoretical terms apply to items and their qualities that are not publicly observable, yet are indirectly referred to, e.g., "electron" and "charge." The importance of this distinction for the positivists is

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[1]Paul Feyerabend, "Problems of Empiricism," in Beyond the Edge of Certainty, ed. R. Colodny (Englewood Cliffs, NJ: Prentice Hall, 1965), 180.

[2]For a classic example, see Rudolf Carnap, "The Nature of Theories," in Introductory Readings in the Philosophy of Science, eds. E. D. Klemke, Robert Hollinger and A. David Kline (Buffalo, NY: Prometheus Books, 1988) 162-176.

that observable terms are thought to form the foundation of our scientific knowledge because they are fixed by our observations, independently of a theory. Thus, there is a degree of certainty that the positivist claims.

Kuhn adamantly rejects this view:

> But is sensory experience fixed and neutral? Are theories simply man-made interpretations of given data? The epistemological viewpoint that has most guided Western philosophy for three centuries dictates an immediate and unequivocal, Yes! . . . Yet [this viewpoint] no longer functions effectively, and the attempt to make it do so through the introduction of a neutral language of observations now seem to me hopeless.[3]

For Kuhn, all accounts of observations depend upon what we look at and the conceptual framework we bring to the visual experience. In support of this view Kuhn refers to the work of Norwood Russell Hanson. According to Hanson, all observational terms are theory dependent, or, as has since been coined, "theory-laden."[4] In a clever thought experiment, Hanson places the astronomers Tycho Brahe and Johannes Kepler together on a hill to observe the dawn. After observing the morning horizon, both men are asked what they saw. Surprisingly, each man uses very different observational terms in recounting what he observed.

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[3]Kuhn, Structure, 126.

[4]Norwood Russell Hanson originally introduced the notion of "theory-loaded", though "theory-laden" is more common in the literature. This thesis claims that a feature of all language is that it is always "loaded" or laden" with a theory. See his Patterns of Discovery (New York: Cambridge University Press, 1958).

Brahe, an Aristotelian astronomer, reported observing the sun rising above the earth's fixed horizon. Kepler, though, recounted a fixed sun, with the earth rising. Hanson concludes that something more than mere observation occurred. Each astronomer made observations, and those observations were placed within the conceptual framework each was committed to. Kuhn agrees:

> Surveying the rich experimental literature from which these examples are drawn makes one suspect that something like a paradigm is prerequisite to perception itself. What a man sees depends both upon what he looks at and also upon what his previous visual-conceptual experience has taught him to see. In the absence of such training there can only be, in William James's phrase, "a bloomin' buzzin' confusion."[5]

An important consequence of theory-ladeness is captured in the incommensurability thesis, jointly introduced by Kuhn and Feyerabend. "Incommensurability" originally appeared in Greek mathematics, meaning "no common measure." Kuhn and Feyerabend extend this notion to capture features of science. Incommensurability in science claims that because proponents of competing scientific conceptual frameworks do not have the same network of terms, and because the conceptual framework determines the meaning, in effect, scientists working in competing conceptual frameworks are not using the same terms, though they may be inscriptionally the same. Competing scientific frameworks

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[5]Kuhn, Structure, 113.

are said to be at cross-purposes because the initial disagreements over meanings of terms and concepts lead to corresponding disagreements over non-empirical assumptions, the list of problems to be solved, and experimental methodology and results. Kuhn writes:

> The reception of a new paradigm often necessitates a redefinition of the corresponding science . . . And as the problems change, so, often, does the standard that distinguishes a real scientific solution from a mere metaphysical speculation, word game, or mathematical play. The normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often incommensurable with that which has gone before.[6]

Ian Hacking further develops incommensurability in science, delineating three types: dissociation incommensurability, topic incommensurability, and meaning variance incommensurability.[7] The first type, dissociation, is not central to this thesis.[8] However, the latter two, topic incommensurability and meaning variance incommensurability, are central to the concerns of this thesis.

Topic incommensurability thesis claims that frameworks with incompatible topics also address different problems, use distinct concepts and employ unique applica-

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[6]Ibid., 103.

[7]Hacking, Representing and Intervening, 67.

[8]Ibid., 68. Briefly, Hacking argues that dissociation incommensurability occurs when a long period elapses between theories, causing an earlier theory to become unintelligible from the vantage of a newer theory.

tions. For instance, the French chemist, Antoine Lavoisier, in developing the modern conceptual framework of chemistry, changed the entire network of terms previously used, thereby changing the very topics of discussion. We can retrace the record to show that, where his contemporaries "saw" dephlogisticated air, Lavoisier "saw" oxygen.[9] In order to accomplish this shift in viewpoint, Lavoisier was also required to change other concepts as well. Fire and rust were no longer seen as releasing phlogiston. Chemists no longer accepted the claim that all combustible substances contained phlogiston. Indeed, the stage was set for a new conception of chemical elements. Lavoisier's initial lead resulted in original research directed at the new topic of conservation of mass in chemical reactions. In a fundamental way, this shift away from phlogiston theory to oxidation theory caused chemists "to see" the world in a new way. Phlogiston theory concerned itself with problems, employed concepts and guided research in ways that modern chemistry does not. Simply put, with the shift in chemical theory came a shift in topics of chemistry. Such a shift in topics to

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[9]Kuhn uses a vision metaphor, saying that a conceptual framework provides a "way of seeing" the world. This point is closely related to my previous discussion of "theory-ladeness." See Thomas S. Kuhn, "A Function For Thought Experiments," chap. in <u>The Essential Tension: Selected Studies in Scientific Tradition and Change</u> (Chicago: University of Chicago Press, 1977), 263.

the research program initiated by Lavoisier caused the earlier phlogiston theory to be incommensurable with the new oxidation theory.

Meaning variance incommensurability states that the meaning of individual terms in a network are stipulated by the larger conceptual framework in which the terms are embedded and that terms and concepts in different networks denote differently. When the relationships between terms changes, there is a change in meaning. As I argued earlier, incommensurability even applies when inscriptionally identical terms appear in competing conceptual frameworks.

Consider an example from the history of astronomy. Ptolemy's astronomical system, which placed the earth at the center of our celestial system, is said to be incommensurable with the later development of Copernicus' system, which placed the sun at the center.[10] Ptolemy developed a mechanistic model of the heavens that accurately predicted planetary movement. Under the Ptolemaic system, the "sun" refers to a planet embedded in the moving homocentric crystalline spheres, and "earth" refers to a stationary object at the center of the heavens. Copernicus, however, when writing his famous letter to the Pope in 1543, argued that the earth is a planet moving around the sun. Copernicus' claim did not simply revise

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[10]Kuhn, Structure, 149.

an earlier theory. Rather, Copernicus redefined "earth" and "sun," thereby creating a new network of terms. For instance, Ptolemy's sun meant a celestial body that obeyed the complex combination of planetary movements of deferents, epicycles, eccentrics and equants.[11] Moreover, the earth meant a stationary planet at the center of the sun's deferent. In contrast, Copernicus's sun meant the center of the universe. The earth meant a celestial body moving in three circular motions: a diurnal axial rotation, an annual orbital motion, and an annual conical motion of the axis.

Paul Hoyningen-Huene further elaborates incommensurability in astronomy.[12] For him, the meaning variance incommensurability captures similarity and dissimilarity relations between the same term located in different networks. For instance, in Ptolemaic astronomy the sun belonged to the same "similarity class" as Mars. That is, what was similar between them was that they both circled the earth. The earth then, being unlike the sun and Mars, was stationary and thus part of the "dissimilarity class"

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[11]Thomas S. Kuhn, The Copernican Revolution: Planetary Astronomy in the Development of Western Thought (Cambridge, MA: Harvard University Press, 1957), chap. 4 passim.

[12]Paul Hoyningen-Huene, "Incommensurability in Kuhn," Reports of the 13th International Wittgenstein Symposium. Philosophy and Natural Science: Borderline Questions (1989). (Vienna: HÖlder-Pichler-Tempsky, 1989), 142.

for the sun and Mars. The change to the Copernican astronomical system brought corresponding changes in the similarity/dissimilarity relations. What was formerly dissimilar, the earth and Mars, became similar--they were both considered planets orbiting around the sun. Likewise, what was formerly a similarity relation between the sun and Mars now changed to a dissimilarity relation--a stationary sun and the orbiting Mars.

For astronomers, changes in the meaning of celestial objects caused correlated changes in questions that gave rise to possible questions and acceptable answers.[13] The effect was that physics and astronomy were reconceived in fundamental ways. Few of the central features of "earth" and "sun" remained the same. That is, with the shift to Copernicus' astronomical system, the terms "sun" and "earth" became *incommensurable* with their pre-Copernican meanings.

In sum, then, the incommensurability thesis characterizes those aspects of science that disconnect scientific research programs from each other. A shift in topic causes corresponding shifts in problems to be solved, questions to be addressed, concepts to be employed, and methods for achieving these goals. Meaning variance

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[13]For instance, calculating the correct orbital path of the sun around the earth was no longer a legitimate question. In addition, answers that were previously given by Ptolemaic astronomers were now considered wrong.

incommensurability characterizes differences in the meaning of terms as rooted in incompatible frameworks. Now that incommensurability in general has been discussed, I shall direct my attention to displaying examples of both topic incommensurability and meaning variance incommensurability in subdisciplines of psychology.

Incommensurability in Psychology

Two important conceptual frameworks in psychology are neuropsychology and cognitive psychology. However, each conceptual framework studies distinct subject matter by using dissimilar methods, addressing diverse problems, and employing incompatible terms and concepts. In effect, the disparate research programs of neuropsychology and cognitive psychology can be described as topic incommensurable.[14] Moreover, similar terms and concepts used by these frameworks "share no common measure," and are thus meaning variance incommensurable in addition.

Neuropsychology is the conceptual framework that seeks to explain behavior through the study of the anatomical, physiological and biochemical properties of the

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[14]For an interesting discussion of this see Mind and Brain: Dialogues in Cognitive Neuroscience, eds. Joseph E. LeDoux and William Hirst (New York: Cambridge University Press, 1986). The book is organized as a series of dialogues by neuroscientists and psychologists who address some of the concerns raised here.

nervous system.[15] Motivating such a research program are questions about the role of the structure and function of the nervous system in behavior. For instance, neuropsychology has concerned itself with the role of the hippocampus in learning, of the amygdala and septum in emotion, of the hypothalamus in motivation, etc. The techniques of investigation used by neuropsychologists include the anatomical mapping of neural connections, electrical and chemical stimulation of the brain,
electrical and chemical recording of brain activity, and brain lesioning.

In contrast, the cognitive psychology research program seeks to explain the "nature of human intelligence and how people think".[16] Human intelligence and thinking are unpacked by the cognitive psychologist in terms of information processing. As such, cognitive psychologists are interested in the areas of pattern recognition, problem solving, decision making, recall, recognition, metacogntion, categorization, etc. Research focuses on

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[15]Because of the large degree of interdisciplinary activity, I am using "neuropsychology" to denote areas such as physiological psychology, psychobiology, behavioral neurology, and their equivalents. See Simon Green, Physiological Psychology: An Introduction (London: Routledge and Kegan Paul, 1987), xv.

[16]John Anderson, Cognitive Psychology and Its Implications (San Francisco: Freeman, 1980), 3.

explaining how information is "transformed, reduced, elaborated, stored, recovered, and used".[17] Important techniques for studying these cognitive processes include protocol analysis, content analysis and computer simulation.[18] Protocol analysis is of particular importance because it is widely used. Briefly, protocol analysis treats the experimental subject's verbal reports as data, which are interpreted in terms of such variables as reaction time, accuracy, or memory performance.

The above analysis, albeit very brief, highlights important disagreements between neuropsychology and cognitive psychology. The subject matter, problems, and techniques of neuropsychology are dissimilar to those of cognitive psychology. It appears that the two disciplines are proceeding independently.[19] Motivating such independent frameworks are important philosophical views. On the one hand, neuropsychology assumes reductionism, which

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[17]Ulric Neisser, Cognitive Psychology (New York: Appleton-Century-Crofts, 1967); quoted in Stephen K. Reed, Cognition: Theory and Applications (Pacific Grove, CA: Brooks/Cole, 1988), 3.

[18]Herbert A. Simon and Craig A. Kaplan, "Foundations of Cognitive Science," in Foundations of Cognitive Science, ed. Michael I. Posner (Cambridge, MA: MIT Press, 1989), 20-37.

[19]This is precisely the conclusion of "Cognitive Neuroscience: An Overview," in Mind and Brain: Dialogues in Cognitive Neuroscience, eds. Joseph E. LeDoux and William Hirst (New York: Cambridge University Press, 1986), 1-6.

holds that psychological research should be directed at explaining behavior at the most elementary level. Moreover, "just as physics moves toward explanation at the subatomic level, and biology searches for explanation at the genetic and molecular levels, so the neurosciences should become oriented increasingly toward the nerve cell and the chemical and electrical events that occur within."[20]

On the other hand, cognitive psychologists see their research based, not upon physics and biology, but rather upon computer science.[21] Computer science offers cognitive psychology a model approach to science that separates research into two different levels of acceptable explanation, viz., the level of hardware, describing the electrical and mechanical properties of the computer, and the level of software, which consists of the rules of the program. In computer science, each level is sufficient for certain tasks, and each is accepted as legitimate. For the cognitive psychologist, the same type of distinction should be drawn in psychological inquiry. The cognitive psychologist views neuropsychology, while not wrong or uninformative, as simply incomplete. For the cognitive

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[20]Howard Gardner, The Mind's New Science (New York: Easic Books, 1985), 286.

[21]Owen Flanagan, The Science of the Mind (Cambridge, MA: MIT Press, 1991) 180.

psychologist, over and above research into the physical subject matter of psychology, there is research into the cognitive operations of the mind. Cognitive psychology is interested in an analysis of cognitive processes in functional terms. Consider the following:

> If a psychologist wants to describe how information is represented in a person's head, isn't it necessary to talk about how the brain and nervous system work . . . ? After all, psychologists generally agree that all information is ultimately represented by electrical and chemical activity in the nervous system. However, cognitive psychologists claim that it is possible--indeed, necessary--to study mental representation without investigating the nervous system directly.[22]

From the foregoing analysis, we can conclude that neuropsychology and cognitive psychology are topic incommensurable.

Now I shall demonstrate meaning variance incommensurability. Neuropsychology and cognitive psychology are the two dominant frameworks in depression research. For cognitive psychology, depression means a mental state that is a response to stressful life events such as bereavement, natural disasters, and personal crisis.[23] The man whose wife dies, the women who has had a hysterectomy and feels she has lost her femininity, the agoraphobic who is

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[22]Arnold Lewis Glass and Keith James Holyoak, Cognition, 2d ed., (New York: Random House, 1986), 8.

[23]George Winokur, Mania and Depression: A Classification of Syndrome and Disease (Baltimore: The Johns Hopkins University Press, 1991), 167-177.

despondent because of the inability to leave the house are all vulnerable to situational depression. For this type of depression, maladaptive thought patterns are to blame. Aaron T. Beck's negative self-schema model of depression is an example of this conceptual strategy.[24] For Beck, we are all guided in part by a schema, which is an organized representation of prior knowledge that guides the processing of current information. Beck informs us that some individuals are prone to depressive illness because their schemata contain negative content, i.e., negative cognitive distortions about themselves, the world, or the future. The depressive schema distorts incoming information in a negative way.

A very different meaning of depression occurs when we consider the neuropsychological framework.[25] Under this conception, depression is an organic dysfunction. Genetic, hormonal, and neurotransmitter studies are undertaken to identify the organic factors involved in depression. An example of this research program is the

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[24]I am borrowing this analysis from Richard R. Bootzin, Joan Ross Acocella and Lauren B. Alloy, Abnormal Psychology: Current Perspectives, 6th ed., (New York: McGraw-Hill, 1993), 254-258.

[25]Floyd E. Bloom and Arlyne Lazerson, Brain, Mind and Behavior, 2d ed. (New York: W. H. Freeman, 1988), 327.

"Hypersensitive Serotonergic Receptor Theory of Depression".[26] This model claims that neurotransmitter release is reduced in people predisposed to depression. Evidence for this model comes from comparative studies on rats, as well as from the current understanding of the role of specific cerebral areas in mood and emotion, namely the cortex, hippocampus, and midbrain.

We see here a noticeable gap between cognitive and neuropsychological definitions of depression. According to Beck's negative cognitions theory, "depression" <u>means</u> negative self-schema. According to the theory of Hypersensitive Serotonergic Receptor Theory, depression <u>means</u> a neurotransmitter imbalance. There is, in effect, no common measure, or thread between the two definitions of depression. That is, cognitive psychology's term "depression" is meaning variance incommensurable with neuropsychology's term "depression."

Conclusion

In this chapter, I have discussed topic incommensurability and meaning variance incommensurability in general. Then I offered concrete examples, from the

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[26]Morris H. Aprison and Joseph N. Hingten, "A Neurochemist's Perspective on Human Depression and Stress," in <u>Affect Disorders: Perspectives on Basic Research and Clinical Practice</u>, eds. Tetsuhiko Kariya and Michio Nakagawara (New York: Brunner/Mazel, 1993), 3-25.

psychological literature, that demonstrate the incommensurable subdisciplines in psychology. I argued that the incommensurability noted explains the present disunity in psychology. Neuropsychology and cognitive psychology each advance standards governing acceptable concepts, problems, and explanations that are incompatible with the other. Consider the following passage by a psychologist comparing his own neuropsychological framework with a cognitive psychology framework:

> Do the two disciplines use the same language? As a neuroscientist, my task in preparing this chapter is a little like that of a traveler in a foreign country. With an incomplete knowledge of grammar and syntax, the traveler attempts to understand the fine points of the country's culture. The problem is both idiomatic (familiar terms, such as features and attention, are used in unique ways) and neologistic (for example, textons and perceptrons). In addition, there are words, such as transparency, emergent features, primal sketches, and integral dimensions, not used in the traveler's native language.[27]

The above passage emphasizes a crucial point of this thesis: The present disunity of psychology is rooted in the competing frameworks that employ their own incommensurable languages. Moreover, this incommensurability ensures that the competing frameworks, or subdisciplines, that make up psychology will never be unified.

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[27] Richard T. Marocco, "A Neurobiological View of the Psychology of Perception," in Mind and Brain: Dialogues in Cognitive Neuroscience, eds. Joseph E. Deloux and William Hirst (New York: Cambridge University Press, 1986), 82.

CHAPTER FOUR

ONTOLOGICAL IMPORT OF SCIENTIFIC KNOWLEDGE

In chapter two I introduced the problem of disunity in psychology. As evidence for the disunity, I cited psychologists and briefly retraced the historical record. At the end of the chapter, I discussed Kuhn's work concerning the history and philosophy of science. Influenced by Kuhn's fecund approach, I concluded that psychology was not a Kuhnian mature science. Attempting to understand more fully the nature of the disunity, and the blockage to maturity, in chapter three I introduced the incommensurability thesis. In reviewing the psychological literature, I found significant evidence for incommensurability of frameworks in psychology. This incommensurability, I argued, barred progress toward unification, and eventual scientific maturity.

This summary reminds the reader of the thesis' arguments leading up to this chapter. In this chapter I shall claim that the incommensurability, and hence disunity, in psychology has its origins in ontology. More specifically, the disunity in psychology is rooted in the classic mind-body problem. Any successful attempt to build a unified psychology, I shall argue, must come to terms with this difficult philosophical problem.

Psychology's Mind-Body Problem

The mind-body problem has quite a long history in philosophy. In arguing that the problem of unification is actually an instance of the mind-body problem, I do not mean to suggest, in addition, that we need to return to traditional Cartesian-like debates. To identify clearly what the mind-body problem means for contemporary psychology, I shall borrow an important, and insightful distinction, made by Patricia S. Churchland and Terrence J. Sejnowski, between the "traditional mind-body problem" and its contemporary formulation.[1]

> In its traditional guise, the mind-body problem can be stated thus: Are mental phenomena (experiences, beliefs, desires, etc.) actually phenomena of the physical brain? Dualists have answered No to this question. On the dualist's view, mental phenomena inhere in a special, nonphysical substance: the mind (also referred to as the soul or the spirit).[2]

A little further on, they continue:

> Materialism answers the mind-body question (Are mental states actually states of the physical brain?) in the affirmative. The predominant arguments for materialism draw upon the

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[1]Patricia S. Churchland and Terrence J. Sejnowski, "Neural Representation and Neural Computation," in Mind and Cognition: A Reader, ed. William G. Lycan (Cambridge, MA: Basil Blackwell Press, 1990), 226.

[2]Ibid., 227.

> spectacular failure of dualism to cohere with the rest of ongoing science.

The classical formulation of the mind-body debate is between substance dualists, like Descartes, and materialists. Churchland and Sejnowski argue that this formulation is no longer significant for modern psychology. Cognitive psychologists, who talk in terms of a mind and its operations, do so without postulating "a special nonphysical substance." Rather, the contemporary version of the mind-body problem is a debate between materialists.

On the one hand, there are those who, like modern cognitive psychologists, hold what Stephen P. Stich calls the "belief-desire thesis."

> A venerable view, still very much alive, holds that human action is to be explained at least in part in terms of beliefs and desires. Those who advocate the view accept that the psychological theory which explains human behavior will invoke the concepts of belief and desire in a substantive way. I will call this expectation the belief-desire thesis [his emphasis].[3]

Earlier in chapter three, I argued that cognitive psychology seeks to explain the "nature of human intelligence and how people think".[4] Further, according to Beck's model of depression, negative beliefs about oneself (e.g., being too fat, being unlikable, etc.), coupled with negative

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[3]Stephen P. Stich, "Autonomous Psychology and the Belief-Desire Thesis," in Mind and Cognition: A Reader, ed. William G. Lycan (Cambridge, MA: Basil Blackwell Press, 1990), 346.

[4]Anderson, Cognitive Psychology, 3.

desires (e.g., to isolate oneself from others), are important variables which may possibly lead to the development of clinical depression.[5] From examples like these, it becomes obvious that the belief-desire thesis is central to the study of depression in cognitive psychology.

On the other hand, neuropsychology aims to study depression without ever referring to beliefs and desires. Rather, this position attempts to explain depression in terms of neuronal activity.[6] This is an explicitly reductionist approach. Briefly, reductionism argues that psychological phenomena will, in the final analysis, be explained by and, indeed, amount to nothing more than reference to neuronal activity.

The modern debate, then, is between those who argue for a belief-desire psychology and those who argue to exclude beliefs and desires, favoring a reductive psychology to the level of neurons. Churchland and Sejnowski refer to this as the "contemporary" version of the mind-body problem. Moreover, they argue that the debate is actually over theory dualism and theory monism.

> Thus, the mind-body problem in its contemporary guise is this: Can we get a unified science of

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[5]See my earlier discussion of Beck's model of depression, 43-44.

[6]See my earlier discussion of neuropsychology and depression, 44.

> the mind-brain? Will psychological theory reduce to neuroscience [their emphasis].[7]

If there is no successful reduction, and the belief-desire thesis guides psychological theories, then there will be what Churchland and Sejnowski call "theory dualism." That is, there will be a belief-desire based psychology and a neuropsychology. Moreover, there will be no claim to a unified psychology. If, however, beliefs and desires do not play a necessary role in psychology, then there will be theory monism, i.e., a neuropsychology only.

Before a decision is made to accept belief-desire psychology, and hence "theory dualism," a crucial question needs to be addressed: What is it to accept a scientific theory? The most troubling philosophical problem in accepting a scientific theory is the ontological implications scientific theories bring with them.[8] To sharpen the discussion, I will need to say more about theory acceptance, and its relation to ontology.

Philosophers of science have debated for some time the issue of the ontological import of science in general. Standard discussions of ontological import of scientific

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[7]Churchland and Sejnowski, "Neural Representation," 228.

[8]Bas van Fraassen writes that "theories with some degree of sophistication always carry some 'metaphysical baggage'. Sophistication lies in the introduction of detours via theoretical variables to arrive at useful, adequate, manageable descriptions of the phenomena." The Scientific Image (Oxford: Claredon Press, 1980), 68.

theories cluster around two general positions, viz. realism and antirealism. Realism asserts that successful scientific theories give us a literally true description of our world. Moreover, scientific theories tell us what is really there. That is, for the realist, there is "a desk-thumping, foot-stamping shout of 'Really!'"[9] Antirealism asserts that such a interpretation is not warranted. There is, for the antirealist, no privileged ontological vantage point that permits the exclamatory "Really!".

I shall direct this chapter to expounding the realism-antirealism debate. In order to accomplish this, I shall perform the following tasks. I shall give an account of scientific realism. Realism is currently the "received view" of science. However, realists themselves increasingly have become aware of important difficulties with their position. One of the strongest arguments against realism is the underdetermination thesis. I shall carefully explain this thesis, and point out the negative consequences of it for realism. I shall conclude that the underdetermination thesis makes a realist reading of science incoherent. I shall then inquire into the history of science. Criticisms have been made of the realist

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[9]Arthur Fine, "The Natural Ontological Attitude," in Scientific Realism, ed. Jarrett Leplin (Berkeley: University of California Press, 1984), 97.

assertion that scientific development is a progressive process of cumulating past successes in new theories. In particular, I shall explore Larry Laudan's arguments that claim that most instances in the history of science show a discontinuous, noncumulative development. Next, I shall offer a detailed example of an antirealist reading of science from physics. Quantum mechanics has been particularly resistant to realist readings. I shall end this chapter by proposing an antirealist position, founded upon Bas van Fraassen's constructive empiricism. Briefly, van Fraassen argues for a pragmatic and instrumentalist stance toward science.

Scientific Realism

Scientific realism is not an easy position to define. According to Jarrett Leplin, "scientific realism is a majority position whose advocates are so divided as to appear a minority."[10] Leplin goes on to list ten routine realist claims that no majority of realists would adhere to: (a) the best current scientific theories are at least approximately true; (b) the central terms of the best current theories are genuinely referential; (c) the approximate truth of a scientific theory is sufficient explanation of its predictive success; (d) the (approximate) truth of a scientific theory is the only possible

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[10]Leplin, ed., Scientific Realism, 1-2.

explanation of its predictive success; (e) a scientific theory may be approximately true, even if referentially unsuccessful; (f) the history of at least the mature sciences shows progressive approximation to a true account of the physical world; (g) theoretical claims of scientific theories are to be read literally and, so read, are definitively true or false; (h) scientific theories make genuine, existential claims; (i) the predictive success of a theory is evidence for the referential success of its central terms; and (j) science aims at a literally true account of the physical world, and its success is to be reckoned by its progress toward achieving this aim.

In order to pin down a concise definition of scientific realism, it is important to identify what scientific realism is about. After surveying realist writings, van Fraassen supplies us with a clue: "In the philosophy of science, the term 'scientific realism' denotes a precise position on the question of how a scientific theory is to be understood, and what scientific activity is."[11] Following van Fraassen's analysis, there are, then, two central questions scientific realism must address: (a) how is a scientific theory to be understood, and (b) what does a scientific theory do? Answers to these questions will give us, at least, the minimum requirements for scientific

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[11]van Fraassen, Scientific Image, 6.

realism. To answer these questions, I shall turn to the realist philosopher Richard N. Boyd, who characterizes scientific realism as embodying the following four central theses:

> 1. Theoretical terms in scientific theories (i.e., nonobservational terms) should be thought of as putatively referring expressions; that is, scientific theories should be interpreted "realistically."
> 2. Scientific theories, interpreted realistically, are confirmable and in fact are often confirmed as approximately true by ordinary scientific evidence interpreted in accordance with ordinary methodological standards.
> 3. The historical progress of mature sciences is largely a matter of successively more accurate approximations of the truth about both observable and unobservable phenomena. Later theories typically build upon the (observable and theoretical) knowledge in previous theories.
> 4. The reality which scientific theories describe is largely independent of our thoughts or theoretical commitments.[12]

We are now in a position to answer van Fraassen's two questions. Boyd, in the first thesis, identifies scientific theories as referential. In the second thesis, Boyd informs us that the references these theories make are often confirmed, and thus believed to be true. So, an answer to van Fraassen's first question, we conclude that science is understood to be genuinely referential and true.

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[12]Richard N. Boyd, "The Current Status of Scientific Realism," in Scientific Realism, ed. Jarrett Leplin (Berkeley: University of California Press, 1984), 41.

Van Fraassen's additional question, concerning what scientific theories do, is answered by Boyd's last two theses. Boyd's third thesis informs us that science, through time, builds on past success, accumulating more and more knowledge of the world. In addition, the fourth thesis informs us that our scientific knowledge is not wholly theory dependent. That is, our theories discover the world.[13]

So, according to Boyd, what scientific theories do is discover facts. Furthermore, over time, these facts accumulate in a progressive manner. Leplin agrees, "What realists do share in common are the convictions that scientific change is, on balance, progressive and that science makes possible knowledge of the world beyond its accessible, empirical manifestations."[14] Van Fraassen also agrees, and offers a helpful concise statement of scientific realism:

> "Science aims to give us, in its theories, a literally true story of what the world is like:

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[13]Some philosophers argue that science does not discover fact, rather it constructs facts. Supporting this view is the historical record which indicates a discontinuous development pattern. See Kuhn, Structure, 206. In addition, Larry Laudan offers persuasive evidence denying science's cumulative character. See Laudan, "A Confutation of Convergent Realism," in Scientific Realism, ed. Jarrett Leplin (Berkeley: University of California Press, 1984), 218-249.

[14]Leplin, ed., Scientific Realism, 2.

> and acceptance of a scientific theory involves the belief that it is true."[15]

For the realist, then, to accept a theory is to believe that it is literally true.

Scientific Realism and Quine's Underdetermination Thesis

Many philosophers of science, unconvinced by realist arguments, cite a single, simple, and very powerful epistemological argument, i.e., underdetermination. The underdetermination thesis has been most strongly and clearly advanced by W. V. O. Quine. Later, we shall see van Fraassen build upon the consequences of the underdetermination thesis in constructing his alternative to scientific realism. For now, consider the following. As Quine writes:

> Naturally it [our theory about the world] is underdetermined by past evidence; a future observation can conflict with it. Naturally it is underdetermined by past and future evidence combined, since some observable event that conflicts with it can happen to go unobserved.[16]

Quine here is arguing that it may be _a fact_ that our theory of the world is underdetermined. Simply put, Quine points out that we do not now have all the possible

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[15]van Fraassen, _Scientific Image_, 8.

[16]W. V. O. Quine, "On the Reasons for Indeterminacy of Translation," _The Journal of Philosophy_ 67 (1970): 178-179; quoted in Roger F. Gibson Jr., _The Philosophy of W. V. Quine: An Expository Essay_ (Tampa, FL: University of Florida Press, 1982), 84.

evidence in. As such, the evidence we do have does not decide, or does not determine, that our scientific theories are true.

However, there is a stronger version of the underdetermination thesis that argues that our scientific theories are, in principle, underdetermined. Continuing from the quote directly above, Quine writes:

> Moreover, many people will agree, far beyond all this, that physical theory is underdetermined even by all possible observations . . . Theory can still vary though all possible observations be fixed. Physical theories can be at odds with each other and yet incompatible with all possible data even in the broadest sense. In a word, they can be logically incompatible and empirically equivalent. This is a point on which I expect wide agreement, if only because the observational criteria of theoretical terms are commonly so flexible and fragmentary.

This second version states that scientific theories, or frameworks, are, in principle, underdetermined by their evidence.[17] By this is meant that there always can be constructed alternative theories to accord with the same empirical data because theories go beyond the evidence. As Roger Gibson writes:

> The doctrine of underdetermination of theory claims that theories about the world transcend all possible observations of the world, and, further, that different, competing theories can

[17]Quine eventually modifies his views on underdetermination. For a discussion of Quine's development of underdetermination thesis see Gibson, The Philosophy of W. V. Quine, 84-94. I shall, later, explore philosophers of science who take seriously this initial reading of underdetermination thesis.

> be developed on the same observational basis. In a word, theories can be shown to be logically incompatible with one another, yet empirically equivalent.[18]

Suppose there is a theory T regarding a set of observable and unobservable phenomena.[19] Now, a theory is empirically equivalent to T if it makes the same predictions about observable phenomena that T does. We can always produce new theories simply by adding any sentence about unobservables to T. For instance, assume 's' is a statement about unobservables. Now, I could add 's' and '~s' to T and establish two new theories, say T' and T'' respectively. T' and T'' both imply the same set of evidential statements and are empirically equivalent. However, because T' contains 's' and T'' contains '~s' they are logically incompatible. And any attempt to test which theory is to be preferred runs into difficulties. As Boyd writes:

> Since scientific evidence for or against a theory consists in the confirmation or disconfirmation of one of its observational predictions, T and each of the theories empirically equivalent to it [e.g., T' and T''] will be equally confirmed or disconfirmed by any possible observation evidence.[20]

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[18]Roger F. Gibson Jr., Enlightened Empiricism: An Examination of W. V. Quine's Theory of Knowledge (Tampa, FL: University of Florida Press, 1988), 9-10.

[19]I am borrowing this example from Gibson, Philosophy of Quine, 88.

[20]Boyd, "Current Status," 44.

The underdetermination thesis, then, states that only the empirical consequences of a scientific theory can be known. A practical consequence of this thesis is that, in situations where two theories are empirically equivalent, and both theories go beyond the evidence, one cannot decide which theory gives a true account of unobservables. Only empirical evidence can be compared between scientific theories. Boyd concludes "that scientific evidence can never decide the question between theories of unobservable phenomena and, therefore, knowledge of unobservable phenomena is impossible."[21]

The unsettling conclusion of the underdetermination thesis for the realist is that two independent and incompatible theories can equally account (i.e., be empirically equivalent) for scientific observations, yet logically incompatible. That is, there are "various defensible ways of conceiving the world."[22]

Quine's "Indeterminacy of Translation" thesis further explains the underdetermination thesis.[23] The indeterminacy of translation thesis claims that there are a variety of ways to translate linguistic utterances that go beyond

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[21]Ibid., 44.

[22]W. V. O. Quine, Pursuit of Truth (Cambridge, MA: Harvard University Press, 1992), 102.

[23]W. V. O. Quine, Word and Object (Cambridge, MA: MIT Press, 1960), 27.

the evidence. Assume there are three anthropologists who are assigned to translate a radically different language of some alien being. The three anthropologists together note that every time a rabbit runs across their path, the alien utters "Gavagi." However, the three anthropologists cannot come to an agreement on the proper translation of "Gavagi." One anthropologist translates it as the universal "rabbithood"; another translates the utterance as the full sentence "lo, a rabbit"; the last anthropologist translates "Gavagi" simply as "rabbit."

We see that each of the anthropologists translation of "Gavagi" is consistent with the given empirical data, i.e., whenever a rabbit is seen by the alien he utters "Gavagi." However, the evidence underdetermines all three translations. As Quine writes:

> All the objective data [the anthropologist] has to go on are the forces that he sees impinging on the native's surfaces and the observable behavior, vocal and otherwise, of the native.[24]

The point Quine makes is that the empirical data cannot decide the correct translation because the data is not implied by any one translation. There is no evidence that determines a translation, so reference to empirical data is inscrutable. For Quine, there is no fact of the matter.

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[24]Ibid., 28.

Scientific Realism and the History of Science

The history of science has played an important part in the realism-antirealism debate. Recently, realists have been challenged by those who argue that the historical record can only make sense under an antirealist view. To see the force of this challenge, consider Hilary Putnam's worrisome metainduction:

> What if all the theoretical entities postulated by one generation (molecules, genes, etc. as well as electrons) invariably "don't exist" from the standpoint of later science? . . . One reason this is a serious worry is that eventually the following metainduction becomes compelling: just as no term used in the science of more than 50 (or whatever) years ago referred, so it will turn out that no term used now (except maybe observational terms if there are such) refers.[25]

As we saw earlier with Boyd, realist philosophers argue that scientific development is progressive, building on past successes. Ernan McMullin has argued that natural science has over the last two centuries constructed theories that are best explained as the cumulative development of knowledge.[26] Scientific theories, McMullin claims, give us the ability to explain the phenomena of the natural world. McMullin provides specific examples from geol-

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[25]Hilary Putnam, "What is Realism?," in Scientific Realism, ed. Jarrett Leplin (Berkeley: University of California Press, 1984), 145.

[26]Ernan McMullin, "A Case for Scientific Realism," in Scientific Realism, ed. Jarrett Leplin (Berkeley: university of California Press, 1984), 26-30.

ogy, cell biology, and chemistry. I shall consider the first two.

Concerning geology, McMullin stresses the convergence of geological, physical, and biological theories that has given us knowledge about such theoretical entities as geological time periods (e.g., the Carboniferous, the Permian, the Devonian and the Cretaceous periods) and extinct species (e.g., the dinosaurs). McMullin makes two points regarding this convergence: (1) This process has continued despite lasting and important controversies, such as the sudden extinction of species at the end of the Cretaceous period, and a precise evolutionary account of intra-species relationships; (2) The historical record indicates that our knowledge of visible strata, their fossil records, and the sorts of life forms associated has been "pretty cumulative," despite considerable theory change since Hutton's day.

McMullin also considers a case from cytogenetics. Our knowledge of what goes on inside the cell is provided by a convergence in theories of cytology, genetics, and biochemistry. The growth of microscopy has allowed for even deeper penetrations, producing fecund structural models. Originally, biologists saw only the relatively macroscopic chromosomes. Only gradually, McMullin informs us, were the beadlike units within the chromosomes, viz., the genes, identified as the theoretical unit of hereditary transmission. Then appeared the Nobel Prize winning work of James Watson and Francis Crick, which made it possible to unravel the structure of the gene as molecules of DNA. Each molecule, we are now told, is constructed of intertwining, double helices of nitrogenous base pairs, viz., adenine and thymine; cytosine and quanine. McMullin concludes from these examples that there is an identifiable pattern of steady, progressive discovery.

Many philosophers are unconvinced by such cumulist accounts as offered by McMullin. As I previously mentioned in chapter two, Kuhn argues that science goes through revolutionary periods. In these periods, science is discontinuous because the scientific community accepts a new paradigm only by the rejection of another. With this change, old theories, laws and results are rejected by the new paradigm, or they are modified in substantial ways so as to be incommensurable with their previous status. Scientific development, Kuhn concludes, is discontinuous.

Laudan, concurring with Kuhn, supplies an impressive list of examples of the noncumulative development of science. For instance, Laudan, surveys the transition from Ptolemaic astronomy to Copernican astronomy, from Cartesian mechanics to Newtonian mechanics, from classical Darwinism to modern genetics, as well as other examples and concludes:

> loss occurs at virtually every level: confirmed predictions of earlier theories are sometimes not explained by later ones; even the 'observable' laws explained by earlier theories are not always retained, not even as limiting cases; theoretical processes and mechanisms of earlier theories are, as frequently as not, treated as flotsam.[27]

Laudan argues that this "loss" occurs primarily in ontology. He considers the transition from classical ether theory to relativistic and quantum mechanics. Fundamental laws and general assertions made by the ether theory are not contained in the newer theories because the central term in ether theory, "ether," no longer was considered to denote in the newer theories. As such, laws and general assertions made about the density and structure of ether, in addition to laws regarding the interaction between ether and matter were simply lost. From this and other examples Laudan concludes that development in science often causes wholesale changes in frameworks and their concomitant ontologies which make it impossible to capture many of the central theoretical laws and mechanisms postulated by the earlier theory.

Moreover, Laudan argues that science's noncumulative nature is actually a source of much of sciences success. Important and substantial achievements in science have been made precisely because scientists have not followed cumulative constraints. As Laudan writes:

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[27]Laudan, "Confutation," 236.

> In spite of his commitment to the growth of knowledge, the realist would unwittingly freeze science in its present state by forcing all future theories to accommodate the ontology of contemporary (mature) science and by foreclosing the possibility that some future generation may come to the conclusion that some (or even most) of the central terms in our best theories are no more referential than was 'natural place', 'phlogiston', 'ether', or 'caloric'.[28]

In sum, then, both Kuhn and Laudan have argued that science's development is not cumulative as the realist argues. Laudan has supplied insightful examples where science is not cumulative. The realist, then, must seriously consider the metainduction that Putnam believed was "compelling."[29]

Scientific Realism and Quantum Theory

Antirealists have argued that only their position can make sense of much of science. Quantum mechanics is an example of an explicitly antirealist theory that is extremely successful.[30] Quantum Mechanics originated out of anomalous problems in classical mechanics. Max

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[28]Ibid., 239.

[29]Putnam, "What is Realism?," 145. McMullin refers to this as a "disastrous metainduction" that must be blocked, or "realism simply would be false." McMullin, "Case for Realism," 22.

[30]Quantum theory's success is well noted, "it correctly predicts all the quantum facts we can measure plus plenty that we can't (such as the temperature of the sun's interior) or do not care to (the electron's piano attribute), for instance." Nick Herbert, Quantum Reality: Beyond the New Physics (New York: Anchor Books, 1985), 157.

Planck's work on blackbody radiation, Albert Einstein's explanation of the photoelectric effect, and Niels Bohr's theory of the atom all began the process of changing the classical architecture of physics. The culmination of these developments was Bohr's and Werner Heisenberg's Copenhagen Interpretation of Quantum Mechanics, currently the "received view." The Copenhagen Interpretation has radical implications for philosophy of science. For instance, Heisenberg's uncertainty principle places limits on what is knowable by blurring the distinction between the experimenter and the measurement, and Bell's Theorem challenged the idea of locality. But it is the Copenhagenist claim of a bipartite quantum world that has most severely challenged classical conceptions of the world. As Hanson writes: "All the philosophical problems and the conceptual difficulties of quantum theory, including the Copenhagen interpretation, arise from the practical necessity of describing micronature in terms of both waves and particles."[31]

Consider the "two slit experiment".[32] Assume there is a stream of photons traveling towards a screen, called

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[31]Norwood Russell Hanson, "Quantum Mechanics, Philosophical Implications of," in Encyclopedia of Philosophy, ed. Paul Edwards (New York: Collier MacMillan, 1967).

[32]Peter Gibbins, Particles and Paradoxes: The Limits of Quantum Logic (New York: Cambridge University Press, 1987), 36-40.

a diaphragm. The diaphragm acts as a barrier to the photons. However, in this experiment two small slits have been made in the diaphragm. The slits, which can be opened or closed, are just large enough to allow only a small amount of photons through. The photons that successfully pass through either slit strike another screen placed behind the diaphragm. This second screen is a photographic plate, recording the pattern of photon hits.

There are two important experimental conditions. In the first, only one of the slits is open. What is observed is that each photon that passes through the open slit strikes the photographic plate at a definite location. That is, light is behaving as if it were particles. However, when both slits are opened, something astonishing happens. First of all, areas that had a high frequency of hits when only one slit was open now had fewer hits. Secondly, the overall pattern of hits displayed a diffraction pattern. These two events forced physicists to conclude that when both slits were open, light acts as if it is a wave.

The two slit experiment demonstrates that light is both wave-like and particle-like. This dual nature of light is called the "wave-particle duality."[33] Attempts have been made to solve this duality. Max Born suggested

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[33]Ibid., 36.

that we collapse the duality in favor of the particle. On the other hand, Schrodinger argued for collapsing the duality in favor of the wave. Such attempt have not been successful. As Gibbins writes:

> The trouble with Born's particle ontology is exactly the trouble with all particle pictures, the fact that they handle only one half of wave-particle duality, leaving the other half a mystery. The same can of course be said of Schrodinger's wave ontology.[34]

To explain this conceptual paradox, Bohr advanced the complementarity model of light. That is, waves and particles are complementary descriptions of the same phenomena. Moreover, both are required for a complete account. This allowed for incorporation within one theory of two mutually exclusive properties of the phenomenon of light. Depending upon the circumstances, light is either measured as a wave or as a particle.

The "two slits experiment" presents realists with a difficult philosophical problem. The experiment tells us that light is both fundamentally waves and fundamentally particles. But this does not make sense, because waves are not particles, and vice versa.

> Ideally, a wave extends to infinity; ideally, a particle collapses onto a dimensionless point. Ideally, a coordinate intersection can locate an infinite number of waves but only one particle.

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[34]Ibid., 36. Note also that some have argued for a "wavicle" model. A "wavicle" is an entity retaining fundamental qualities of both wave and particle. However, this model is not generally received.

> Ideally, a particle is indivisible; a wave must be divisible if its essential periodicity (in space) is ever to be in evidence.[35]

To obviate this problem, quantum theory is treated in a anti-realist, pragmatic fashion.

In concluding this section, I shall briefly summarize. The underdetermination thesis gives us a strong argument against realism. Insofar as realism is committed to knowledge of unobservable phenomena, there are deep difficulties with a realist reading of science. Moreover, the underdetermination thesis is not the only argument advanced by philosophers unswayed by realist claims. Antirealists have in their favor the historical record of science, which shows that there have been many successful theories that have been overturned, abandoned or replaced. In addition, antirealists have been successful at arguing that the success of science is not an argument for a realist reading of science. The philosopher Leplin, a realist, agrees that antirealist arguments have legitimate and lasting points. He writes: "I believe it is fair to say that neither the problem posed by the historical record of theory change nor the problem about the connection between truth and success has been solved even to the satisfaction of realists."[36] Furthermore, antirealists

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[35]Hanson, "Quantum Mechanics, Philosophical Implications of."

[36]Leplin, ed. Scientific Realism, 4.

have argued strongly that quantum theory is a counter-example to realism. Later in this chapter I shall argue that van Fraassen's antirealism is able to account for quantum theory.

The underdetermination thesis, coupled with these other arguments, show that realism is an untenable position. As Fine concludes (quite boldly):

> Realism is dead. Its death was announced by the neopositivists who realized that they could accept all the results of science, including all the members of the scientific zoo, and still declare that the questions raised by the existence claims of realism were mere pseudo-questions. Its death was hastened by the debates over the interpretation of quantum theory, where Bohr's nonrealist philosophy was seen to win out over Einstein's passionate realism. Its death was certified, finally, as the last two generations of physical scientists turned their backs on realism and have managed, nevertheless, to do science successfully without it.[37]

I shall now turn my attention to developing an antirealist alternative.

The Antirealist Alternative

Given the problem of the underdetermination thesis, criticisms of the realist claim of the cumulative nature of scientific development, and the actual practice of Quantum Mechanics, I believe a realist interpretation of science is unpromising. When van Fraassen wrote The Scientific Image, he was well aware of the aforementioned

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[37]Fine, "Natural Ontological Attitude," 83.

problems realism encountered.[38] As an alternative, van Fraassen proposed an antirealist view that he calls constructive empiricism. This position offers an instrumentalist, pragmatic appraisal of science that, correctly I believe, "makes better sense of science, and scientific activity, than realism does and does so without inflationary metaphysics."[39] I shall now argue for van Fraassen's antirealism.

Constructive empiricism claims the following: "Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief that it is empirically adequate."[40] A theory is empirically adequate if it has at least one model of all observable phenomena, or, as he says, if it 'saves the phenomena'. To accept a theory, then, involves only the belief that the theory is empirically adequate. What acceptance of a theory does not involve is any existential claims beyond observable phenomena. This is not done naively, for van Fraassen knows that unobservable entities have had an indispensable role in scientific development, and that in all likelihood they will continue to do so. Rather, van Fraassen remains agnostic regarding the ontological status

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[38]van Fraassen, Scientific Image, 73.

[39]Ibid., 73.

[40]Ibid., 12.

of such unobservables. For van Fraassen, we are not warranted to make ontological claims that go beyond the evidence.

While van Fraassen identifies theory acceptance as belief in empirical adequacy, more than belief is involved in accepting a theory. Theories also have pragmatic virtues, including: internal consistency; scope; simplicity; fecundity; mathematical elegance; and unifying power. These extra-empirical virtues play an important role in the acceptance of a scientific theory. However, because there are a variety of ways to judge the pragmatic virtues of a theory, theory appraisal is influenced by the context in which it occurs. For example, two theories that are empirically adequate, may differ only in that one is of a larger scope, while the other is more fecund. In cases such as these, the decision to accept a theory is guided by reasons offered for one pragmatic virtue over another.

> In so far as [pragmatic virtues] go beyond consistency, empirical adequacy, and empirical strength, they do not concern the relation between the theory and the world, but rather the use and usefulness of the theory; they provide reasons to prefer the theory independently of questions of truth [my emphasis].[41]

Theory choice, then, is a process that is determined by the empirical adequacy and the pragmatic promises of each theory. Once a choice is made, we make a commitment to

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[41]van Fraassen, 88.

that theory, and confront nature within the conceptual scheme of that theory.

CHAPTER FIVE

AN ANTIREALIST TREATMENT OF PSYCHOLOGY

I shall begin this chapter with a brief review of the thesis so far. In chapter two I introduced the problem of disunity in psychology. In support of this claim, I cited several psychologists who see their discipline marked by disparate "islands of knowledge."[1] Further claim to disunity was seen in my brief synopsis of the historical record, which is marked by disjointed development, e.g., structuralist and functionalist inclusion of the mind, versus the behaviorist exclusion of the mind. At the end of the chapter, I discussed Kuhn's theory of scientific development. According to Kuhn's theory, for a scientific discipline to be considered mature, there needs to be in place a paradigm, an overarching conceptual and methodological framework, that directs the discipline, especially by providing model problems and solutions. Moreover, the paradigm must be significantly entrenched and form the foundation of "normal" scientific research. Influenced by Kuhn's fecund approach, I concluded that psychology was not a Kuhnian mature science.

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[1]Staats, "Paradigmatic Behaviorism," 239.

Attempting to understand more fully the nature of the disunity, and the consequent blockage to maturity, in chapter three I explored the incommensurability thesis. I made the distinction between topic and meaning variance incommensurability, and discussed both types. Turning to the psychological literature, I found significant evidence of topic and meaning variance incommensurability between the cognitive and neuropsychological frameworks, taken as sample subdisciplines of psychology. This incommensurability, I argued, barred progress toward unification of these subdisciplines, and eventual scientific maturity of the discipline.

In chapter four, I argued that psychology's disunity and the incommensurability of its subdisciplines were even more deeply rooted in the mind-body problem. In addition, I explained how the contemporary version of the mind-body problem has become a debate over the inclusion of beliefs and desires in psychological theories. This led me to investigate the ontology of science. I noted that the two dominant, general philosophical positions on the ontology of science are realism and antirealism. I reviewed the realist position, and found it to be significantly wanting for the following reasons: (1) the underdetermination thesis effectively argues against any knowledge claims beyond the observable; (2) the history of science displays a pattern of discontinuous development; (3) quantum theory is an example of an explicitly antirealist science that has been markedly successful. Consequently, I sketched an antirealist interpretation of science, modeled after van Fraassen's constructive empiricism.

The final aim of this thesis is to present a creative solution to the persistent problem of disunity in psychology. In this chapter, I shall first extend my discussion of constructive empiricism. Next, I shall introduce Tang's Complementarity Model of MindBrain.[2] Tang's model, very much a work in progress, creatively avoids the metaphysical debate over the mind-body problem by allowing for a synthesis of what appears to be two opposing, irreconcilable points of view. This antirealist model, Tang suggests, will unify the aforementioned disparate subdisciplines of psychology. Finally, I shall suggest that the Complementarity Model of MindBrain may constitute one way to solve the problem of unification for psychology.

The Complementarity Model of MindBrain

The reader will recall that constructive empiricism informs us that to accept a theory is to believe it to be

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[2]See Paul C. L. Tang and James L. Peacock, "Complementarity Model of MindBrain and the Unification of Psychology," Paper presented as part of the thirty-fourth annual meeting of the Western Social Science Association, Denver, CO., 22-25 April 1992. See also, Paul C. L. Tang and Ralph W. Brown, "Antirealism and the Complementarity Model of MindBrain," Boston Studies in the Philosophy of Science, ed. Robert S. Cohen (Dordrecht, The Netherlands: Kluwer Academic Publishers, forthcoming).

empirically adequate, and to have certain pragmatic virtues. Moreover, once we accept a theory, we make a commitment to confront phenomena within the conceptual framework that the theory provides. As van Fraassen writes:

> A main way in which [our scientific commitments] shows itself is that the language we talk has its structure determined by the major theories we accept. That is why, to some extent, adherents of a theory must talk just as if they believe it to be true [my emphasis].[3]

We see here another important feature of scientific commitments, that is, scientists talking "as if" a theory is true.[4] For a scientist to commit to a theory, he commits to a certain ontology which implies a way of talking about the world. That is, scientists talk "as if" they believe their commitments are true. So, when a cognitive psychologist talks as if there exist mental representations, mental categorizations, as well as more homely mental entities as beliefs and desires, he is displaying his commitment to a theory that postulates these entities, and not displaying his belief that they indicate a final

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[3]van Fraassen, Scientific Image, 202.

[4]The philosophy of the "as if" has its roots in Kant's philosophy of regulative ideas and regulative principles, and is generally recognized as the foundation of pragmatism. See also John R. Searle's use of the "as if" in the philosophy of mind, The Rediscovery of the Mind (Cambridge, MA: MIT Press, 1992), 78-79. Briefly, Searle discusses the notion of "as if" as it relates to assigning intentionality in debatable cases.

truth. Contrary commitments are unveiled when the neuropsychologist talks as if he can explain human activity based solely upon an investigation of the physical body, and more importantly for them, the brain.

It becomes readily apparent, however, that constructive empiricism does not, by itself, engender a solution to the problem of unification in psychology. Even if all psychologists became card carrying constructive empiricists, this philosophical move by itself would not necessarily unify the commitments that psychologists make. However, what constructive empiricism does provide is a reorientation to the problem of unification.

To understand the reorientation, recall that earlier I made the important claim that the problem of unification is rooted in the mind-body problem; therefore, in order to solve the unity problem, we need to confront the mind-body problem. But how?[5] Tang suggests that what is needed is a metalevel model that makes sense of the relation between the mind and brain; specifically, we need a conceptual model which informs us how we should think of mind-brain dualism. Tang further suggests that we look to science itself for a clue to the solution.

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[5]Feigl calls the mind-body problem "the riddle of the universe." This seems very appropriate given the persistence of the problem. See Herbert Feigl, The "Mental" and the "Physical" (Minneapolis: University of Minnesota Press, 1967), 29.

There is important precedent in science for embracing duality. Earlier, I discussed the Copenhagen Interpretation of quantum mechanics. A central feature of this interpretation is Bohr's Complementarity Model of light which treats two mutually exclusive, fundamental characteristics of light as complementary descriptions. Moreover, the word "complementarity" was introduced by Bohr "to denote the relation of mutual exclusion characteristic of the quantum theory with regard to the application of the various classical concepts and ideas [my emphasis]."[6] For the Copenhagen Interpretation, quantum theory is treated in an antirealistic fashion, as simply a tool or instrument for explanation and prediction. On this antirealistic foundation, it is no longer problematical to claim a duality of light, because Bohr's model informs us that both descriptions are required. Physicists, then, are committed to the view that light sometimes behaves as if it were composed of particles and at other times as if it were composed of waves.[7] The only criterion of ade-

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[6]Niels Bohr, Atomic Theory and the Description of Nature (Cambridge, England: Cambridge University Press, 1961), 19.

[7]The "as if" indicates pragmatism, and takes us back to van Fraassen's constructive empiricism. But "complementarity" extends beyond van Frassen's position, as indeed it must, to relate waves to particles. Similarly, I earlier noted that the solution to the disunity problem in psychology must likewise go beyond just the philosophical position of constructive empiricism.

quacy made is that, under this model, the Copenhagen Interpretation is highly functional and pragmatic. Indeed, the Copenhagen Interpretation has enjoyed tremendous empirical success in terms of explanation and prediction by embracing the wave-particle duality.

Tang argues that instead of attempting to solve the mind-body problem, we should take note of the fact that physics has a conceptual model that guides commitments concerning the duality relationship between waves and particles. However, psychology itself does not have a comparable conceptual model for mind-brain duality. Noting the success physics has had by embracing the Complementarity model of light, Tang suggests that psychology should borrow from Bohr's model of light the general concept of "complementarity," as a springboard or starting point, and apply it to mind-brain dualism, even though the analogy of complementarity will not be exact. With this concept, psychology will be able to talk of the mind and talk of the brain as complementary descriptions of the same event. Tang calls this new approach the Complementarity Model of Mind/Brain and claims this model may unify psychology by capturing a similar "relation of mutual exclusion characteristic" that Bohr referred to in physics.[8] One of the potential promises of the Comple-

[8]Bohr, Atomic Theory, 19.

mentarity Model of MindBrain is that it may very well provide a <u>conceptual</u> framework, a metalevel paradigm, that would guide psychological thinking and theorizing on questions concerning the relation of the mind to the brain.

Importantly, Bohr himself argued that complementarity is to be found in psychology. Bohr writes:

> We must, in general, be prepared to accept the fact that a complete elucidation of one and the same object may require diverse points of view which defy a unique description . . . The necessity of taking recourse to a complementarity, or reciprocal, mode of description is perhaps most familiar to us from psychological problems.[9]

Later, in the same article, Bohr writes:

> It might still be permitted here briefly to refer to the relation which exists between the regularities in the domain of psychology and the problems of causality of physical phenomena. When considering the contrast between the feeling of free will, which governs the psychic life, and the apparently uninterrupted causal chain of the accompanying physiological processes, the thought has, indeed, not eluded philosophers that we may be concerned here with an unvisualizable relation of complementarity. Thus, the opinion has often been expressed that a detailed investigation of the processes of the brain, which, although not practicable, is, nevertheless, thinkable, would reveal a causal chain that formed a unique representation of emotional mental experience.[10]

And, finally, he writes:

> According to the above-mentioned view on the relation between the processes in the brain and

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[9]Ibid., 96.

[10]Ibid., 100.

> the psychical experiences, we must, therefore, be prepared to accept the fact that an attempt to observe the former will bring about an essential alteration in the awareness of volition. Although, in the present case, we can be concerned only with more or less fitting analogies, yet we can hardly escape the conviction that in the facts which are revealed to us by the quantum theory and lie outside the domain of our ordinary forms of perception we have acquired a means of elucidating general philosophical problems.[11]

Bohr, then, claims that "complementarity" can be found in psychology. Seizing upon Bohr's realization, Tang claims that the Complementarity Model of MindBrain develops more fully Bohr's insight regarding complementarity in psychology. That is, Tang's Complementarity Model of MindBrain may provide a conceptual account of the relationship between mind and brain descriptions. Importantly, the Complementarity Model of MindBrain is conceived as a metalevel paradigm. The difficulties that psychology has experienced have resulted in frustrated attempts at establishing an object level paradigm. However, Tang suggests that we may need to go to a metalevel paradigm, such as the Complementarity Model of MindBrain, because of the underlying mind-body problem.[12] Psychology may be the only science that will have such a metalevel paradigm--

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[11]Ibid., 101.

[12]As a metalevel paradigm, the Complementarity Model of MindBrain would stand in a unique position between philosophy and psychology. That is, the model would be guided by philosophical concerns and actual psychological research.

again, because of the mind-body problem. This point has been consistently overlooked by psychologists and philosophers alike. If Tang's argument is correct, this would be the first time such an observation (the need for a metalevel paradigm to unify psychology) has ever been made.

Let us pause for a moment and provide a very practical realization of the philosophical points just made. Assume patient X goes to a psychotherapist because he is depressed. If the therapist is a cognitive psychologist, he may very well treat X with talk therapy.[13] Here, the therapist is acting as if patient X has both a mind and a brain, and that the mind is the cause of the depression. Assume further that X does not respond to the therapist's talk therapy. The therapist then decides that the proper treatment will consist of drug therapy, and prescribes X an antidepressant drug, such as prozac. Here, the therapist is treating X as if he had only a brain, and that the drug action on the brain will effect a cure.[14] In this common case, the psychotherapist uses methods of treatment

[13]The phrase "talking cure" was initially introduced by Freud and his contemporaries.

[14]For additional discussion of depression studies see Paul C. L. Tang and James L. Peacock, "The Monoamine Hypothesis, Placebos and Problems of Theory Construction in Psychology, Medicine and Psychiatry," Paper presented as part of the sixty-eighth annual meeting of the American Philosophical Association Pacific Division, Los Angeles, CA., 30 March, 1-2 April 1994.

that have a theoretical base grounded in the philosophical position of dualism, on the one hand, and materialism on the other.

Another practical example can be seen in aggression research. Presently, there are a number of competing view on the cause of aggression, including physiological, genetic, psychoanalytic, behaviorist, and cognitive.[15] Each framework either explicitly or implicitly takes a stance on the mind-body problem. I shall consider Albert Bandura's social learning analysis of aggression and Kenneth E. Moyer's biological model of aggression.

Bandura's observational learning hypothesis holds that the vast majority of human behavior is acquired by copying what others do.[16] Bandura and others concluded that "subjects given an opportunity to observe aggressive models later reproduced a good deal of physical and verbal aggression (as well as nonaggressive responses) substantially identical with that of the model."[17] Bandura's conclusion implies that observational learning occurs via

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[15]E. Jerry Phares, Introduction to Personality, 2d. ed., (Boston: Scott, Foresman and Company, 1988), 37-45.

[16]The classic study supporting Bandura's social learning analysis, known as the "bobo doll study" in the literature, is by Albert Bandura, D. Ross and S. A. Ross, "Transmission of Aggression Through Imitation of Aggressive Models," Journal of Abnormal and Social Psychology 63 (1961): 575-582.

[17]Albert Bandura, Aggression: A Social Learning Analysis (Englewood Cliffs, NJ: Prentice Hall, 1973), 102.

a cognitive capacity to actively pick and choose behaviors that are observed.

Contrary to Bandura's view, Moyer has proposed a "Bio-Experimental Model" of aggression, arguing that the cause of aggression is the physiological underpinnings and the corresponding neural systems.[18] As Moyer writes:

> The basic premise of the model proposed here is that there are in the brains of animals and humans neural systems that, when activated by the presence of a relevant target, result in aggression or destructive behavior towards that target.[19]

Moyer later implements androgens, endocrine factors, allergens, drops in blood sugar levels, and various drugs all as examples of biological/biochemical determinants for the excitation of the neural system and thus the increase potential for aggressive behavior.[20]

On the one hand, Bandura's model emphasizes the mental capacity of humans to actively pick and choose behavior. Moyer's model forgoes any discussion of mental activities, instead indicating that aggression is caused by neurophysiological events.

Currently, there is no <u>one</u> model of depression. Psychologists recognize that both Bandura's model and

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[18]Kenneth E. Moyer, <u>Violence and Aggression</u> (New York: Paragon House, 1987), 41.

[19]Ibid., 22.

[20]Ibid., 72.

Moyer's model have explanatory and predictive virtues. Recognizing this, the Complementarity model of mind-brain may be able to effect a reconciliation between these opposing frameworks. There is no longer a need to make any claim that aggression is "really a function of the mind" or that it is "really a function of the body." Rather, the only requirement is to develop frameworks of aggression that are empirically adequate and succeed at predicting and explaining. Under this view, observational learning and physiological events are considered complementary descriptions of the phenomenon of aggression.

Three important points stand out. First, the Complementarity Model of MindBrain displays the complementarity in psychological research, and it comports with the actual practice of psychologists. Second, Tang's Complementarity Model only works on an antirealistic and constructive empiricist view of science. Third, absent the antirealistic Complementarity Model of MindBrain, the problem of unity in psychology is misunderstood and clouded from view. This is why the psychologists and philosophers have not heretofore solved the problem. The problem if unity in psychology (and hence maturity) has been framed too closely, too literally in Kuhn's terms with the resultant judgement that "Psychology is a young a science."[21] But 2this is not the answer, because the argument to date shows that no unifying paradigm in a strictly Kuhnian sense is possible. And this is because of the underlying mind-body problem. Taking the mind-body problem seriously and contemporarily manner, i.e., creatively, antirealis-tic-ally, complementarily, the Complementarity Model of MindBrain may be thought of as a metalevel paradigm that unifies subdisciplines along the mind-body divide. This model would stand in a unique position between psychology and philosophy. That is, this model is not an object level paradigm. Rather, the model is a metalevel paradigm that serves to effect a unification of the disparate frameworks of psychology.[22]

Tang's Complementarity Model of MindBrain is very much a work in progress. However, given the discussion of psychology's disunity, as well as arguments concerning incommensurability, as well as the ontological problem of mind-body, I believe I have demonstrated that the Model is quite fecund. Moreover, the Complementarity Model of

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[21]Paul Fraisse, "Unity and Diversity in the Behavioral and Natural Sciences," in Annals of Theoretical Psychology, vol. 5, eds. Arthur W. Staats and L. P. Mos (New York: Plenum Press, 1987), 213.

[22]As a Kuhnian paradigm, the Complementarity Model of MindBrain would be subject to modification or even replacement. That is, over and above this model is the need for empirical adequate and pragmatically useful paradigms.

MindBrain just may become the unifying paradigm psychology needs.

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