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BEHAVIORISM AND LOGICAL POSITIVISM:
A REVISED ACCOUNT OF THE ALLIANCE
(VOLUMES I AND II)

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BEHAVIORISM AND LOGICAL POSITIVISM:
A REVISED ACCOUNT OF THE ALLIANCE

VOLUME 1
CHAPTERS 1 - 5

By

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B.A., Indiana University, 1972
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DISSERTATION

Submitted to the University of New Hampshire
in Partial Fulfillment of
the Requirements for the Degree of

Doctor of Philosophy
in
Psychology

May, 1983

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ACKNOWLEDGMENTS

This dissertation was researched and written with the guidance and support of an expert doctoral committee. David Leary, who chaired the committee, gave generously of his time and knowledge over a period of several years. In his enthusiasm for scholarly work and his immense skills as a historian and writer, he has provided a model for my own aspirations and has raised my own standards in many ways. I have been fortunate, too, in having the opportunity to study the history of psychology with Bill Woodward. It was Bill who introduced me to the systematic study of psychology's past, and he has continued to support and contribute to my research in many important respects. To Tony Nevin, I owe most of my knowledge of behavioral psychology. His remarkable effectiveness at conveying the excitement and richness of behavioral approaches to psychology is known and appreciated by all who have had the pleasure of studying with him. I am indebted to the two philosophers who served on my committee--Val Dusek and Bob Sharff--for their many astute comments and for helping me steer clear of some of the pitfalls that arise in the course of an undertaking such as the present one. Finally, I have acquired many insights about the philosophy of behaviorism from Jerry Zuriff, both in conversations with him and from his published works in the area.

My sincere thanks go to all of these individuals for their patience, support, and the knowledge which they so generously shared.

The University of New Hampshire proved to be an excellent environment for pursuing my studies. Among the faculty members who were not members of my committee, I have especially enjoyed and benefitted from numerous conversations with John Limber. I have also enjoyed stimulating interactions with my fellow graduate students, particularly Debbie Johnson and Doug Lea. To Doug go my special thanks for writing the word-processing software with which the dissertation was composed and for responding to my frequent pleas for assistance. I also wish to express my gratitude to the library staff at U.N.H. for aid in locating many obscure resources for my research.

My years as a student at Indiana University laid the groundwork for my interest in behaviorism and logical positivism. Among the psychologists there, George Heise and James Allison deserve mention for having exposed me to the varied fascinations of psychological theory and research. Among the philosophers of science, Ron Giere and Alberto Coffa were especially instrumental in giving me a sense of the importance of philosophical issues in science.

In conducting the research for the present work, I sent letters of inquiry to scores of philosophers and

psychologists who were involved, directly or indirectly, in the developments described herein. Their great generosity in replying to my inquiries has lent to the resulting work a richness that would have been absent without their help. I am very much indebted to all who contributed in this way.

Among those who contributed in varying ways to the present work, there will of course be many who would disagree with much of what I have had to say, both in matters of detail and in the conclusions drawn. Accordingly, it should not be assumed that the individuals mentioned here or in the reference notes of the work endorse any or all of its contents.

During the earliest stages of this research, I was supported by a Graduate Fellowship from the National Science Foundation. More recently, the work has been supported by a Summer Teaching Fellowship and a Dissertation Fellowship from the University of New Hampshire, and by a grant from the Rockefeller Archive Center in North Tarrytown, New York. I am very thankful to all of these institutions for their help. I am also grateful to the staffs of the following archives for their assistance in exploring their resources: Archives of the History of American Psychology (Akron, Ohio), Harvard University, Rockefeller Archive Center, University of Chicago, and Yale University.

The most important support throughout this project has been that of my family. To my parents goes my gratitude for supporting me in so many ways through my extended career as a student. More importantly, they long ago gave me the gift of intellectual curiosity and instilled in me the skills to pursue my interests. Finally, my deepest appreciation goes to Linda Silka, whose unflagging support throughout this project made it all possible and worthwhile. She has stimulated and inspired me in all my endeavors and continues to do so every day.

TABLE OF CONTENTS

| | |
|--|------|
| ACKNOWLEDGMENTS..... | iv |
| ABSTRACT..... | xii |
| VOLUME 1 | PAGE |
| CHAPTER 1: INTRODUCTION..... | 1 |
| The Standard Account of the Behaviorist- Logical Positivist Alliance..... | 9 |
| Koch: The Importation and Evaporation of Behaviorism's Methodology..... | 9 |
| Mackenzie: The Subordination of Sub- stance to Method..... | 13 |
| Leahey: The Empirical Refutation of Logical Positivism..... | 15 |
| History Versus Reconstruction..... | 18 |
| The Revised Account of the Behaviorist- Logical Positivist Alliance..... | 25 |
| The Nature of the Present Volume..... | 35 |
| CHAPTER 2: THE LOGICAL POSITIVIST VIEW OF SCIENCE.. | 48 |
| Frege and the New Logic..... | 51 |
| Mach and the Empiricist Tradition..... | 57 |
| Realignment of the Formal and Empirical..... | 64 |
| Logical Positivism..... | 74 |
| The Vienna Circle and the "Wissenschaftliche Weltauffassung"..... | 74 |
| The Context of Discovery and the Context of Justification..... | 78 |
| The Rational Reconstruction of Science..... | 84 |
| Metaphysics as Linguistic Violations..... | 87 |
| Verifiability and Its Variants..... | 89 |
| The Structure of Theories: Empirical and Formal Components..... | 101 |
| Physicalism and the Unity of Science..... | 103 |
| The Physicalist Doctrine..... | 103 |
| The Logical Positivists and Scientific Behaviorism..... | 109 |
| Unity of Science..... | 112 |

| | |
|---|-----|
| Conclusion..... | 116 |
| CHAPTER 3: PURPOSIVE BEHAVIORISM AND ITS PHILOSOPHICAL BACKGROUND..... | 127 |
| Tolman's Background and Career: An Overview.... | 129 |
| The Neorealist Background..... | 137 |
| Behavior as Purposive and Cognitive..... | 139 |
| Neorealist Epistemology..... | 144 |
| Tolman's Early Behaviorism..... | 148 |
| Concept of Behavior..... | 148 |
| Tolman's Early Epistemology and Proto-Operationism..... | 156 |
| Pitfalls of Neorealism: The Challenge to Tolman..... | 162 |
| CHAPTER 4: PURPOSIVE BEHAVIORISM AND LOGICAL POSITIVISM..... | 176 |
| Tolman's Thought: Pre-Vienna..... | 177 |
| The Shift From Immediate to Mediate Cognition..... | 177 |
| Contextualism, Pragmatism, and the Ineffability Doctrine..... | 184 |
| Schlick at Berkeley..... | 193 |
| Tolman's Thought: Vienna and After..... | 200 |
| Brunswik's Probabilistic Functionalism..... | 200 |
| Logical Positivism and Ontological Neutrality..... | 209 |
| Methodological Physicalism and the Ideal of Measurement..... | 218 |
| Intervening Variables and Sophisticated Operationism..... | 222 |
| Tolman, Brunswik, and the Unity of Science Movement..... | 242 |
| Conclusion..... | 246 |
| CHAPTER 5: TOLMAN'S PSYCHOLOGY OF SCIENCE..... | 264 |
| Science as Behavior: Mazes, Hypotheses, and Maps..... | 265 |
| Logic and the "Pragmatic Behavior-Attitude".... | 273 |

| | PAGE |
|---|------|
| VOLUME 2 | |
| CHAPTER 6: CLARK L. HULL: HIS BACKGROUND AND VIEWS OF SCIENCE..... | 288 |
| Hull's Background..... | 288 |
| Education and Early Career..... | 288 |
| Hull's Turn to Behaviorism..... | 294 |
| Hull's Anticipation of Logical Empiricism (1916-1937).. | 299 |
| The Materialist World-View..... | 300 |
| Materialism..... | 300 |
| Mechanism..... | 307 |
| Deductive Methods..... | 315 |
| Early Interest..... | 316 |
| The Changing Styles of Deductive Theorizing... | 321 |
| Deductive Method versus Metaphysics..... | 330 |
| Method as a Guarantor of Positive Progress..... | 334 |
| Method and Advance..... | 334 |
| Objectivity versus Emotionalism..... | 340 |
| Integration of Science..... | 343 |
| Unity of Method..... | 344 |
| Integration of Laws..... | 346 |
| Scientific Cooperation..... | 348 |
| Conclusion..... | 352 |
| CHAPTER 7: HULL AND LOGICAL POSITIVISM..... | 368 |
| Hull and the Unity of Science Movement..... | 370 |
| Woodger and Formalized Theory..... | 383 |
| Shared Views of Science..... | 383 |
| The Problem of Definition in Formal Theory.... | 388 |
| The Definition of Intervening Variables..... | 393 |
| The Logic of Theory Construction..... | 404 |
| The Logic Boom in Psychology..... | 404 |
| Logical Empiricism: The Iowa Connection..... | 409 |
| Hull's System: The Logical Empiricist Perspective..... | 412 |
| Hull and the New Methodologists..... | 424 |
| Operationism, Positivism, and Quantification..... | 429 |
| Operationism and Positivism..... | 429 |
| The Quantification of Behavior..... | 434 |
| Conclusion..... | 442 |

| | |
|---|-----|
| CHAPTER 8: HULL'S BEHAVIORAL PSYCHOLOGY OF SCIENCE.... | 458 |
| Knowledge as a Habit Mechanism..... | 460 |
| Behaviorist Theory of Theory..... | 467 |
| Theory as a Habit Mechanism..... | 467 |
| A Behaviorist Theory of Truth..... | 475 |
| Theory and Organism as Parallel Machines..... | 482 |
| An Empirical Interpretation of Logic..... | 488 |
| The Psychology of Logic..... | 488 |
| The Confirmatory Status of Logical Principles..... | 490 |
| The Hullian View of Logic..... | 494 |
| Conclusion: Hullian Logic versus Vienna Circle Logic..... | 498 |
| CHAPTER 9: B. F. SKINNER: RADICAL BEHAVIORIST PSYCHOLOGY OF SCIENCE..... | 513 |
| Skinner's Background and Turn to Behaviorism..... | 515 |
| Background..... | 515 |
| Turn to Behaviorism..... | 518 |
| Intellectual Roots: Biology and Positivism..... | 522 |
| Machian Positivism and Biological Economy..... | 522 |
| Positivist Biology of Behavior: Loeb and Crozier..... | 542 |
| Skinner's Relation to Logical Positivism..... | 547 |
| Early Interest..... | 547 |
| Reaffirmation of Mach: Skinner's "Case History"..... | 552 |
| Skinner's Behavioral Epistemology..... | 559 |
| The Concept of the Operant..... | 559 |
| Operant Psychology of Science..... | 563 |
| Conclusion: The "Bootstrap" Nature of the Epistemological Enterprise..... | 575 |
| CHAPTER 10: CONCLUSION..... | 590 |
| Neobehaviorism and Logical Positivism: The Alliance Reconsidered..... | 592 |
| Metaphysics, Metaphor, and Method in Neobehaviorism..... | 592 |
| A Reassessment of the Standard Account..... | 606 |
| Remarks on the Lore of Behaviorism and Logical Positivism..... | 618 |
| Neobehaviorist Epistemologies and the New Psychologism..... | 623 |
| Psychological Epistemology and the New Image of Science..... | 623 |
| Psychologism and the Foundations of Knowledge..... | 631 |
| ANNOTATED BIBLIOGRAPHY..... | 648 |
| BIBLIOGRAPHY..... | 657 |

ABSTRACT

BEHAVIORISM AND LOGICAL POSITIVISM:
A REVISED ACCOUNT OF THE ALLIANCE

by

LAURENCE D. SMITH

University of New Hampshire, May, 1983

The primary aim of this work is to show that the widespread belief that the major behaviorists drew importantly upon logical positivist philosophy of science in formulating their approach to psychology is ill-founded. Detailed historical analysis of the work of the neobehaviorists Edward C. Tolman, Clark L. Hull, and B. F. Skinner leads to the following conclusions: 1) each did have significant contact with proponents of logical positivism; but 2) their sympathies with logical positivism were quite limited and were restricted to those aspects of logical positivism which they had already arrived at independently; 3) the methods which they are alleged to have imported from logical positivism were actually derived from their own indigenous conceptions of knowledge; and 4) each major neobehaviorist developed and embraced a behavioral epistemology which, far from resting on logical positivist assumptions, actually conflicted squarely with the anti-psychologism that was a

cornerstone of logical positivism. It is suggested that the myth of an alliance between behaviorism and logical positivism arose from the incautious interpretations of philosophical reconstructions as historical conclusions. This and other historiographical issues are discussed in the concluding chapter, where it is argued that the anti-psychologism of the logical positivists is an unnecessary impediment to a fuller understanding of the phenomenon of knowledge.

CHAPTER 1

INTRODUCTION: BEHAVIORISM AND LOGICAL POSITIVISM

Like the sciences which developed before it, psychology grew largely out of philosophy, both intellectually and institutionally. The gradual separation of psychology from philosophy, which became a matter of open debate around the turn of this century, was reinforced by the rise of behaviorism in America during the second and third decades of this century. Yet by the late thirties, there was widespread talk among American psychologists of a rapprochement between psychology and philosophy.¹ This reconciliation was by no means a general return to the pre-divorce status, but rather a specific convergence of new movements within the two disciplines. These new schools of thought were neobehaviorism and logical positivism.

Logical positivism arose in the German-speaking world during the 1930s as an affirmation of the natural scientific world-view and a polemic against the powerful tradition of German idealism. Most of its proponents were trained as scientists, mathematicians, and logicians rather than as philosophers. Their fundamental principle was the claim that all statements are either analytic

(e.g., All bachelors are unmarried men; $2 + 3 = 5$), verifiable by observation (e.g., This meter reads 43 volts), or meaningless (e.g., The world of sense experience is unreal). In this trichotomous system, the assertions of traditional philosophy could purportedly be shown to belong to logic (if analytic), to science (if empirically verifiable), or else to be not an assertion at all (if meaningless). With the help of new developments in logic and the famous verifiability principle, the logical positivists assigned metaphysics to the category of nonsense. All that remained of philosophy, in their estimation, was the logical analysis of science and its concepts.

A similar sort of anti-metaphysical bent characterized behaviorism even before it came into contact with logical positivism.² In his 1913 proclamation of behaviorism,³ John B. Watson clearly expressed the urge to avoid the speculative and metaphysical and to focus instead on the concrete and pragmatic. According to Watson and most of the behaviorists who were to follow, the goal of psychology was not to explore the realm of subjective experience or mental phenomena, but to predict and control behavior. The standard arguments for behaviorism were mainly methodological: unlike the phenomena revealed by introspection, observations of behavior were assumed to be reliable and intersubjective. Claims about behavior were thus empirically testable. In both behaviorism and logical positivism,

the anti-metaphysical attitude was tied to empiricism in the form of an explicit or implicit principle of verifiability.

Within the framework of these broad similarities between behaviorism and logical positivism, there were additional strong parallels between the two movements. For one, the thrust of both movements was more methodological than substantive. Logical positivism made no claims about the nature of the world, as did traditional philosophy, but rather recommended linguistic and logical analysis as the method of philosophy. Similarly, behaviorism did not advance any particular psychological theory, but rather recommended the experimental investigation of behavior as the method of psychology. Secondly, the proponents of behaviorism and logical positivism often depicted their movements as turning points in the histories of their respective disciplines. For instance, the logical positivist Moritz Schlick argued that the age-old conflicts between the various systems of philosophy would evaporate once logical analysis showed those systems to have asserted nothing meaningful.⁴ Likewise, many behaviorists believed that behaviorism would lead psychology to a new and fruitful path, one from which traditional psychological issues (e.g., imageless thought, the number of sensation qualities) could safely be ignored. In effect, both behaviorism and logical positivism rejected

the historical problems of their disciplines as pseudo-problems.⁵ A third and related parallel was the common conviction of behaviorists and logical positivists that eliminating historical problems from consideration would clear the way for the achievement of piecemeal progress. In many cases, this progress was viewed as all but guaranteed by the consistent application of the methods of logical analysis and behavioral research.

In addition to these general parallels in intellectual orientation, behaviorism and logical positivism shared a common style as movements. Both were scientific and "tough-minded," in William James's well-known sense of the term.⁶ In light of the rejection of their disciplines' historical problems, both movements were viewed from within and without as radical developments. As a result, both were often promulgated with radical rhetoric and propaganda. Because both arose in somewhat hostile intellectual environments, they were defended in aggressive and polemical fashion. Finally, viewing their movements as the keys to progress in their respective disciplines led behaviorists and logical positivists alike to express wildly optimistic claims about the future benefits of acting on their presuppositions. The writings of both groups frequently showed a sort of missionary zeal, a zeal which was reflected in their occasional references to winning "converts."

One further dimension along which the behaviorist and logical positivist movements showed strong parallels is the historical one. Behaviorism was formally proclaimed about fifteen years before the official founding of logical positivism,⁷ but the historical parallels between them were much closer than this fact would suggest. Both movements arose from ideas which began to coalesce in the 1910s out of various strains of late nineteenth century thought. In both cases, these ideas took on more definite form in the 1920s, and then flowered in the 1930s. By the late thirties, both were clearly the dominant orientations within their disciplines (in the English-speaking world, at least), and their ascendancies continued through the 1940s. In the mid-thirties, both movements began to undergo liberalizations of their formerly more strict formulations. By the fifties, there was a growing recognition of how seriously these liberalizations compromised the original founding principles of each movement. Coupled with continuing criticism from outside, this recognition contributed to a decline of influence through the fifties. The sixties saw both movements lose their domination over their respective fields. In the philosophy of science, logical positivism was replaced by philosophies with historical, sociological, and psychological--rather than logical--orientations. In psychology, behaviorism was succeeded as a dominant per-

spective by the information-processing, computer-simulation brand of cognitive psychology.⁸

With their common intellectual background and orientation, behaviorism and logical positivism were naturally disposed to form some sort of alliance. But only after both movements were well under way was there any significant interaction between them. Prior to the surge of mutual interest in the 1930s, behaviorism was mostly confined to America while logical positivism was little known outside certain parts of central Europe. This geographical isolation ended during the thirties when two sorts of events began to transpire. First, certain prominent behaviorists traveled to Europe, where they encountered logical positivism, and subsequently became involved in the logical positivists' Unity of Science movement. Second, and more importantly, the thirties saw the arrival of leading positivists in America during the pre-war migration of European intellectuals. After the intellectual migration, there were personal interactions, and occasionally professional collaborations, between behaviorists and logical positivists. Eventually, American psychology as a whole came to be dominated by a view of science which coincided in broad outline with the shared view of the behaviorists and logical positivists. In the American psychology journals of the 1940s and 1950s, this fact was reflected in the considerable amount of

discussion devoted to such matters as theory construction and operationism, theoretical postulates and physicalistic data language. This period of active concern on the part of American psychologists with the philosophy and methodology of science has been dubbed the "Age of Theory."⁹

The kinship of behaviorism and logical positivism and the general influence of them on American psychology are topics of considerable interest to historians and philosophers of psychology. In light of the strong historical and substantive parallels between the two movements, it is perhaps not surprising that they are commonly assumed to have been associated with one another in a close-knit intellectual alliance. It would be no exaggeration to say that the presumed alliance of behaviorism and logical positivism has become a central story in both the written histories of twentieth century American psychology and in the lore or "folk history" that is informally propagated among American psychologists. The extent to which the presumption of such an alliance is ingrained in historiographical habits is indicated by the fact that Edwin G. Boring, America's most eminent historian of psychology, treated behaviorism, behavioristics, operational psychology, and logical positivism as though they were but aspects of a single movement.¹⁰ Standard historical texts and various papers and volumes on the philosophy of psychology routinely link behaviorism, or

at least neobehaviorism, with logical positivism. The presumed association has also been commented on by philosophers, often in the context of arguments about psychology's reliance on outmoded models of science.

It is thus widely recognized that there was some sort of alliance between the behaviorist and logical positivist movements and that they jointly held sway over much of American psychology in the second quarter of this century. At the same time, however, there has been little detailed analysis of the alliance, its historical preconditions, the actual interactions in which it was manifested, and the type and degree of intellectual confluence which underlay it. Those who comment on the alliance usually take it as a historical given and then proceed to make historical or philosophical points either from that perspective or in rejection of it. This tendency is no doubt partly due to the recency of the historical episode in question: commentators are still very much engaged in the active defense or criticism of the intellectual framework under scrutiny. Such circumstances have produced some interesting analyses of the relationship between behaviorism and logical positivism, but these analyses have not tended to include serious detailed attention to the historical dimensions of that relationship. The most detailed treatments of this topic are provided in the semi-historical works of Sigmund Koch and

Brian D. Mackenzie. The following section describes 1) the standard account of behaviorism and logical positivism that has emerged from the analyses of Koch and Mackenzie and 2) the incorporation of this account into a recent textbook on the history of psychology.

The Standard Account of the Behaviorist-
Logical Positivist Alliance

Koch: The Importation and Evaporation of
Behaviorism's Methodology

Sigmund Koch has written a number of essays analyzing and evaluating behaviorism and its philosophy of science. These essays focus on the deductive methods of the prominent neobehaviorist Clark L. Hull and their relationship to the logical positivist philosophy of science, but they include treatments of other major behaviorists as well and the conclusions Koch draws are by no means restricted to the Hullian brand of behaviorism. According to Koch, "neobehaviorism may be seen as a marriage between the orienting attitudes of classical behaviorism and one or another interpretation of the 'new' model of science." This new model or "new view" of science is depicted by Koch as a rather haphazard amalgamation of logical positivism, neopragmatism, and operationism. Koch states that the "dominant contours" of this view of science were provided by logical positivism and that psychologists, and neobehaviorists in particular, regularly drew upon this model throughout the 1930s.¹¹

As Koch points out, the logical positivists based their philosophy of science almost entirely on their logical analyses of selected theories in physics. Accordingly, he views psychology's turn to logical positivism as another chapter in the traditional emulation of physics by psychology. In this particular episode, Koch has written,

psychology did not go directly to physics but turned instead for its directives to middlemen. These were, for the most part, philosophers of science (especially logical positivists) and a number of physical science methodologists who had been codifying a synoptic view of the nature of science and who, by the early thirties, were actively exporting that view from their specialties to the scholarly community at large.¹²

Koch often speaks of the psychologists of the thirties, especially the neobehaviorists, as having "imported" their methodology from logical positivism. Furthermore, in his view, they did so inappropriately--what psychology needed then (and still needs), he says, is an indigenous epistemology, not an imported methodology. As we shall see, the conclusion that neobehaviorism imported its methodology is an important one that has influenced much thinking on the topic of behaviorism and logical positivism.

Koch's other major conclusion is closely related to the first one but is more directly historical in character. It begins with the assumption--one widely held by philosophers of science--that logical positivism is defunct both as a movement and as a viable account of the nature of science.¹³ Coupled with the first conclusion, this

assumption is taken by Koch to entail the demise of behaviorism as a viable approach to psychology. That is, having rested on logical positivist presuppositions, neobehaviorism is left with justification for its premises by the demise of logical positivism. Thus, behaviorism, which was all along primarily a methodological position, suffers seriously--indeed fatally--as a consequence of its "evaporating methodological support."¹⁴

Koch qualifies this claim only with the admission that, as is generally the case in science, there can be no "final and crushing refutation of behaviorist epistemology." Behaviorism he assigns to that "class of positions that are wrong but not refutable," and he proceeds to argue against the viability of behaviorism on several grounds. He states that his recounting of the relationship between behaviorism and the "new view" of science

suggests the story of the gradual attenuation of a position that was never seriously tenable, never consistent, based on thin and shifting rationales, and adopted more to serve needs for comfort and security than a passion for knowledge. . . . I think that our story begins to suggest the unfruitfulness of the position, its restrictive effects on problematic curiosity, its scholastic character, perhaps most of all, its basic ludicrousness. . . . When the ludicrousness of the position is made sufficiently plain, perhaps it will be laughed out of existence.¹⁵

For Koch, then, the behaviorist outlook is certainly discredited by its affiliation with the obsolete philosophy of logical positivism, but behaviorism has all along been

an implausible and self-discrediting position.

According to Koch, the joint failure of behaviorism and logical positivism leaves psychology in a peculiar position, indeed in an ironic and paradoxical position. In the wake of logical positivism's collapse as a tenable account of knowledge, new views of the nature of knowledge, and of science in particular, have assigned to psychology an important role in explicating epistemological processes.¹⁶ These new views, in Koch's words, are working toward "a redefinition of knowledge based on an empirical analysis of inquiry of a sort which must largely depend on psychological modes of analysis." In particular, says Koch, "philosophy and, more generally, the methodology of science are beginning to stand on foundations that only psychology can render secure." Yet--and here lies the paradox--"psychology seems hardly cognizant of the challenge implicit in these circumstances." Psychology's failure to take up (or even perceive) the challenge of contributing to the new conception of knowledge is attributed by Koch to the fact that "almost alone in the scholarly community, it remains in the grip of the old conception."¹⁷ Thus, for Koch, the major lesson to be drawn from the failure of the behaviorist-logical positivist alliance is a two-sided one: first, it was inappropriate for psychology to have imported an alien conception of knowledge and science; and second, the

importation of and adherence to this ill-fated conception has prevented psychology from developing its own indigenous epistemology, an epistemology now demanded and badly needed by scholarly culture at large. This conclusion is an extremely important one which will be discussed at some length in the conclusion of the present volume.

Interestingly enough, Koch is not only a major commentator on the behaviorist-logical positivist alliance but was also an important figure in the formation and development of that alliance. As will be described in Chapter 7, Koch studied under the logical positivist Herbert Feigl during the late thirties. He was among the first to explicitly connect Hullian behaviorism with logical positivism and was the very first to give an extended analysis of psychological theory from the perspective of logical positivism. As will become evident in subsequent chapters, Koch's interpretations of the behaviorist-logical positivist alliance have (not surprisingly) been strongly colored by his participation in the historical developments under consideration.

Mackenzie: The Subordination of Substance to Method

Another prominent critic of the behaviorist-positivist alliance is Brian D. Mackenzie. In his Behaviorism and the Limits of Scientific Method (1977), Mackenzie has followed Koch in identifying neobehaviorist

presuppositions with logical positivism and in judging the alliance to have been a clear and significant failure. Mackenzie's reconstruction of the alliance relies heavily on Koch's account, but differs from it in important respects. First, Mackenzie does not view neobehaviorism or its attempted integration with logical positivism as patently implausible or deserving of ridicule: "The failure of the attempt, both in practice and in principle, may quite properly stimulate a reappraisal of the limits of scientific technique; but it seems odd to take it as grounds for our scorn."¹⁸ Second, with respect to Koch's claims about the demise of logical positivism discrediting behaviorism, Mackenzie tends to put the shoe on the other foot. From his perspective, it is the failure of behaviorism that reflects back on and discredits the positivist dictates on which it is said to rest. As Mackenzie puts the conclusion of his analysis, "the main systematic contribution of behaviorism to psychology is its practical demonstration of the untenability of the methodological principles on which it was founded."¹⁹

Mackenzie's account of behaviorism and positivism is lengthier and broader than Koch's and is not as focused on neobehaviorism and logical positivism. Unlike Koch, Mackenzie explicitly acknowledges that behaviorism's positivism grew out of what he calls "the indigenous positivism of comparative psychology."²⁰ On the other

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hand, he agrees with Koch that 1) the neobehaviorists imported their methodology from the logical positivists and 2) that it was inappropriate to do so. Thus, according to Mackenzie, behaviorists took on "external standards of objectivity" and adopted "their methodological formalisms" from logical positivism.²¹ But, he argues further, the methodological techniques of logical positivism were inappropriate for neobehaviorist purposes because they constituted a set of procedures or rules by which theories and concepts could (perhaps) be reconstructed and assessed for metaphysical content, but which were incapable of guiding the construction of new theories. Mackenzie writes:

The behaviorists, for their part, had effected their own elimination of metaphysics from psychology already. Their problem was how to develop theories, given their established commitment to an objective observation base. Thus, they adopted the formal positivist measures, not so much to keep their theories free of metaphysics, as to enable them to develop theories at all.²²

Mackenzie places repeated emphasis on the alleged reliance of behaviorism on rules. Indeed, he asserts at the outset of his book that behaviorism

is important because it was the most sustained attempt ever made to construct a science of psychology through the use of detailed and explicit rules of procedure, because these rules were the outcome of the most sophisticated and rigorous analysis of the logic of science ever made, and because the attempt and the movement were ultimate failures.²³

Likewise, Mackenzie's concluding remarks contain the claim that

behaviorism was the only--or at least the most detailed, uncompromising, and sophisticated--serious attempt ever made to develop a science on methodological principles alone.²⁴

Not surprisingly, then, Mackenzie views the story of the behaviorist-positivist alliance as one that has "profound consequences for our understanding of the complementary roles of method and substance in science."²⁵ He proceeds directly to spell out the major lesson of this revealing episode:

. . . while both substantive and methodological principles are necessary, the substantive ones are more important. . . . Any tendency of methodological considerations to direct research needs . . . to be subordinated to the particular substantive issues present in individual cases in science.²⁶

In sum, Mackenzie's diagnosis is that behaviorism suffered (and died) from having subordinated subject matter to method; his prescription for psychology's recovery from this blow is to reverse the situation by subordinating method to subject matter.²⁷

Leahey: The Empirical Refutation of Logical Positivism

The account developed by Koch and Mackenzie appears to be a rather widely accepted version of the history of behaviorism and logical positivism, and it has been incorporated into a recent textbook on the history of psychology. In his A History of Psychology (1980),

Thomas H. Leahey follows Koch and Mackenzie in attributing to logical positivism a strong and formative influence on behaviorism. He states that the logical positivists "had a considerable influence on behaviorism," and especially on the neobehaviorists Clark Hull and Edward C. Tolman. Referring to these two behaviorists, Leahey claims that "their belief in an objective theory they learned from the logical positivists,"²⁸ He thus seems to accept the notion that the neobehaviorists, in some sense, imported their views of science from logical positivism.

Like Koch and Mackenzie, Leahey further accepts that both behaviorism and logical positivism were failures and then proceeds to analyze these failures in light of the presumed association between these two movements. After reviewing what he takes to be the unsuccessful attempts of Hull and Tolman to establish general theories of behavior, Leahey draws the following conclusions:

Their failure may lie with them, or with positivism. Given the intelligence and diligence of Tolman, Hull, and their students, it is unlikely that they failed through want of effort or intelligence. The failure, then, is probably traceable to logical positivism. . . . Logical positivism and operationism seemed to hold hope that a set of procedures existed that, if followed faithfully, would produce psychological science. Hull and Tolman made every effort to follow these procedures and by 1950 they had come to a dead end. Logical positivism was tried and found wanting.²⁹

Being based mainly on Mackenzie's work, Leahey's account leads to much the same conclusion: the demise of behaviorism reflects back upon and discredits the logical positivist view of science on which it was grounded.

Near the end of his text, this lesson from the history of the behaviorist-positivist alliance is stated even more forcefully. Leahey writes:

Behaviorism to a large degree proceeded on the premise that the positivist analysis of science was correct, and adopted methods and theories consistent with positivism's precepts. If the analysis is wrong, can the science be right? The crisis of behaviorism, especially the learning theory crisis of 1950, can be seen as an empirical refutation of logical positivism. The formal behaviorists, especially Hull, seriously tried to practice positivist psychology. As we have seen, they failed. Hull's theory, despite his best efforts, was never able to conform to positivist precepts, and in the end it became a sterile exercise in quantification. . . . Formal behaviorism showed that positivism's recipe for science is impossible to follow.³⁰

Leahey's textbook account thus contains the essential features of the Koch and Mackenzie accounts of the relation between behaviorism and logical positivism. Those features are the claims that 1) behaviorism and logical positivism were closely associated, 2) the former imported its view of science from the latter, and 3) the fates of the two movements were therefore linked, i.e., that the failure of one reflected on the viability of the other. The codification and inclusion of this interpretation of the behaviorist-positivist alliance in Leahey's textbook

history of psychology suggests that it has indeed become the standard account of the episode.

History versus Reconstruction

It is a significant fact that none of the writers whose views comprise the standard account of the behaviorist-logical positivist alliance has given serious consideration to the historical details or dimensions of that alliance. Koch admits to presenting "a shamelessly abstract historical rundown of the chief phases of behaviorism" in order that "a broad historical picture may be suggested, if only dimly."³¹ Similarly, Mackenzie acknowledges that his book "is not . . . a history of behaviorism," but rather a work in which "the alternation of historical and philosophical analysis is presented without apology."³² The historical analysis that Mackenzie does offer is largely on nineteenth century comparative psychology and in no case does it involve archival research. His historical account of logical positivism's influence on neobehaviorism is almost entirely based on Koch's admittedly sketchy account. Leahey's textbook version of the story, in turn, relies heavily on Mackenzie's version. None of these works utilizes any archival materials or detailed analyses (although Koch's account is informed by some anecdotes and first-hand experiences.)³³

Much the same may be said of two additional works

which give extended treatments of the behaviorist-positivist alliance. The first is Robert E. A. Shanab's unpublished dissertation "Logical Positivism, Operationalism, and Behaviorism" (1969). Shanab introduces his work with the statement that the "primary aim of this study is to exhibit the influence of both logical positivism and operationalism on neo-behaviorism." Focusing his analysis on Tolman's behaviorism, Shanab provides a rational reconstruction of the Tolmanian system, showing how closely it matched the logical positivist model of scientific theory, especially in its emphasis on the verifiability of scientific claims. Shanab writes: "Although Toman has not specifically asserted which of the various meaning criteria he was appealing to, one can still detect the general tenets of logical positivism in his writings. . . ." In concluding his chapter on Tolman, Shanab states that the "preceding discussion clearly indicates that Tolman, a leading exponent of neo-behaviorism, has been influenced directly or indirectly by the scientific movement of operationalism and the philosophic movement of logical positivism."³⁴ These rather weak conclusions are based entirely on an examination of published works. Shanab's work was not intended as a historical analysis, but his conclusions are largely historical in character, and (as will be shown in due course) they misleadingly contribute to the impression

of a close association between logical positivism and neobehaviorism.

A second work which contributes to the same impression is Cornelis Sanders's Die behavioristische Revolution in der Psychologie (1972/1978). Sanders follows the standard account in assigning to logical positivism an important role in the development of neobehaviorism. Asserting that neobehaviorism cannot be understood except in light of its grounding in the logical positivist philosophy of science, Sanders devotes a long chapter to logical positivism and operationism and then proceeds to reconstruct from that perspective the theories of the major neobehaviorists. Again, this is done with scant attention being given to the historical details. Sanders does, for instance, point out that the names of Hull and Toman appear among the advisory committee of the logical positivists' International Encyclopedia of Unified Science³⁵; but rather than pursuing the historical circumstances of this fact, Sanders uses it merely to suggest the necessity of understanding their work in the context of logical positivism.

It is important at this point to recall that logical positivism was the dominant philosophy in America during behaviorism's heyday. Indeed, it was widely regarded as the philosophy of science, rather than as one approach among others.³⁶ In light of this hegemony, it was quite

natural that neobehaviorist theories were reconstructed from the perspective of logical positivism and even that individual neobehaviorists were interpreted as having been logical positivists. The first explicit analysis of a neobehaviorist theory from the vantage point of logical positivism was performed by Koch in 1941.³⁷ This and subsequent analyses by him played no small role in promoting the widespread identification of behaviorist philosophy of science with logical positivism. In combination with Koch's personal experiences, these analyses have also (as will be argued in the sequel) strongly colored his own rendering of the history of the behaviorist-logical positivist alliance--a rendering on which many other writers have relied in their discussions of the alliance.

The various reconstructions of the behaviorist-logical positivist alliance have served several useful functions. They have, for instance, drawn attention to the very striking, and very real, parallels between the logical positivist view of science and the views of various neobehaviorists. They have served the vital critical function of evaluating neobehaviorist theory. In particular, Koch's detailed and masterful assessment of Hullian theory³⁸ was instrumental in curtailing Hull's once-dominant influence over learning theory. At the same time, these retrospective reconstructions have often

presented a limited, unbalanced, and sometimes misleading characterization of the relationship between behaviorism and logical positivism. Recent research in the history and philosophy of science has clearly shown that rational reconstruction is an importantly different enterprise from the genuinely historical study of science.³⁹ Most pertinent to the topic here under discussion is the realization that misunderstanding is apt to arise when the results of rational reconstruction are confused with those of historical analysis. With respect to the alliance of behaviorism and logical positivism, the problem is not so much that the reconstructions are incorrect or inappropriate as that they are by their very nature too limited to support the historical (or semi-historical) conclusions that have sometimes been drawn from them.

Even though the greatest danger of reconstruction lies in the confusion of philosophical conclusions with historical ones, there are also numerous specific hazards involved in reconstructing scientific views (or even views of science) from a current or past perspective other than the historical context in which those views arose. For present purposes, two examples of such hazards will suffice. First, there seems to be a widespread impression that the neobehaviorists, and Hull in particular, believed in the value of crucial experiments (i.e., individual experiments which could conclusively decide between com-

peting theories). This is not an unreasonable belief, especially in light of Hull's well-known advocacy of deductive methods in psychology; but a careful examination of the historical evidence shows that neither Tolman nor Skinner, and not even Hull, believed in crucial experiments. Yet, for example, Mackenzie speaks rather indiscriminately of the behaviorists' "dependence on the notion of 'crucial experiments'" and states that "each theorist or school of theorists . . . tended to expect that the results of 'crucial experiments' cited by them would be impossible to explain by a (rigorously construed) competing theory."⁴⁰ Now the neobehaviorists may have acted at times as if they believed in "crucial experiments,"⁴¹ but their views of science did not countenance such experiments in anything like their traditional sense. The neobehaviorists themselves did not use the term,⁴² and a faith in crucial experiments can be attributed to them only by removing their scientific views and activities from the appropriate historical context.⁴³

A second example of the capacity of reconstruction to distort (or at least be confused with) history is the collapsing of historically important distinctions which are not respected by the temporal or philosophical perspective from which the reconstruction is made. In regard to the case at hand, this difficulty is exemplified by the tendency of commentators on the behaviorist-positivist

alliance to downplay or neglect deep differences between various forms of behaviorism and between the various positivisms with which those behaviorisms were associated. As will be shown throughout much of the present work, the behaviorisms of Tolman, Hull, and Skinner differed importantly from one another, as did their positivisms. Tolman's operational positivism was similar in many (but not all) respects to logical positivism, but Hull's positivism was closer to that of Auguste Comte than to that of the Vienna Circle, and Skinner's is a distinctly Machian version of positivism. The reconstruction of these positions from a single perspective, rather than in their own several historical contexts, has led to a false impression of uniformity. It has been claimed, for instance, that "Hull's conception of a hypothetico-deductive science . . . owes a great deal to the work of Moritz Schlick and other members of the Vienna circle" and even that Skinner's system was "developed under the aegis of logical positivism."⁴⁴ A major aim of the present volume is to exhibit not only the distinctions between the major types of behaviorism but also between these behaviorisms' various forms of positivism, on the one hand, and logical positivism, on the other.

The Revised Account of the Behaviorist-
Logical Positivist Alliance

The present volume can be viewed above all else as an attempt to bring to our understanding of the behaviorist-logical positivist alliance the historical dimension that has never received adequate attention and that has been obscured by philosophical analysis. This undertaking has proven to be no mere matter of fact-mongering: the picture of behaviorism and its relation to logical positivism changes in important and intriguing ways when the topic is approached from a distinctly historical perspective. In the context of their own intellectual developments, the major neobehaviorists--Tolman, Hull, and B. F. Skinner--can be seen to have developed views of science which evolved out of and alongside their respective presuppositions about the nature of organisms' behavior. That is, their "philosophies" of science stemmed by and large from their behavioral psychologies, not vice versa as is commonly supposed.⁴⁵ To neglect this deep dependency of their views of science on their psychological thought is to pluck those views of science from their historical context and, eventually, to misinterpret them.

The historical analysis presented in this volume is based on an extensive, detailed investigation of the historical record. This record includes published and

unpublished writings, archival materials including institutional documents and personal correspondence, and first-hand reports obtained through correspondence and interviews with numerous philosophers and psychologists who played a role in the historical developments under scrutiny. As we shall see, the available evidence suggests that there was both more and less to the behaviorist-logical positivist alliance than the existing accounts would indicate. For example, the record presented here offers more than has been previously known about the activities of Hull and Tolman in the logical positivists' Unity of Science movement. There is even evidence that Tolman actually attended meetings of the Vienna Circle in 1933-34. On the other hand, this record also clearly suggests that the association of behaviorists with logical positivists was not a close one. In general, the association was based on relatively superficial convergences of opinion on broad issues and on matters of rhetoric and propaganda. In matters of substance, however, there lay beneath the surface agreements some deep divergences between behaviorists and logical positivists on the nature of science and its methods.

As the research for the present volume progressed, three lines of evidence emerged which, taken together, began to cast doubt on the notion that there was in general a close association between behaviorism and logical

positivism. First, the logical positivists' interests in psychology were not at all restricted to behaviorist approaches, nor were the neobehaviorists' interests in theories of science and methodology limited to those of logical positivism. Thus, the logical positivists took an interest in Gestalt theory, psychophysics, and psychoanalytic theory, while the behaviorists read and cited the metatheories of science advanced by John Dewey, Ernst Mach, Henri Poincaré and other philosophers who were not logical positivists.⁴⁶ The second, and more directly relevant, line of evidence is that there is remarkably little in the writings (published or unpublished) of the major neobehaviorists to indicate that logical positivism was an important influence on them. In this respect, a simple citation count from the published works of the major neobehaviorists is revealing: scattered throughout the voluminous writings of Tolman are but three references to the works of Rudolf Carnap, a central figure in logical positivism; Tolman's other citations of logical positivists are confined to a single paragraph of a 1935 paper. Similarly, Hull cited Carnap only once, and his works are otherwise devoid of references to major logical positivists. Skinner's works contain only three incidental citations of Carnap and occasional passing mention (without citation) of him and other logical positivist figures. In most of these latter cases, the logical positivists are being criticized by Skinner.⁴⁷

The third type of evidence discrediting the notion of a close intellectual alliance between behaviorism and logical positivism comes from an extensive correspondence with former students and colleagues of the major neo-behaviorists. When queried about links with logical positivism, only a few among the nearly one hundred respondents could recall any discussions of logical positivism. Not a single respondent could recall having been aware of Hull's or Tolman's involvement in the Unity of Science movement.⁴⁸ Although many respondents acknowledged that logical positivism was congenial with the approaches of Hull and Tolman, several explicitly denied that logical positivism had influenced either of them. Those who had more than a passing familiarity with logical positivism pointed out that they had not acquired their knowledge of it from Hull or Tolman.

Of course, none of these three lines of evidence is in itself unambiguously damaging to the standard account of behaviorism and logical positivism; but taken together they begin to reveal the sorts of problems which a careful examination of the historical record poses for the standard account. At the very least, they suggest that any general claim to the effect that behaviorism and logical positivism were closely associated needs a great deal more substantiation than has been given in the past.

The evidence just discussed points to the need for

a revised account of behaviorism and logical positivism, and the sum of evidence which will be presented in the following chapters justifies major revisions of the standard account. A careful investigation of the views of the major neobehaviorists shows that their relationships to logical positivism were generally much more restricted than is commonly supposed. The present work attempts not only to document the extent and limitations of these relationships but also to account for their restricted scope. From the historical perspective, the lines of evidence cited above can be understood as surface manifestations of the underlying intellectual limits on the behaviorist-logical positivist alliance. The importance and severity of these limits can best be appreciated in light of the following claim (the documentation of which is a central undertaking of this work): In their separate ways, Tolman, Hull, and Skinner all believed that science is at root a psychological phenomenon and that their respective theories of learning could (at least eventually) account for scientific knowledge. In effect, they were striving to develop empirical epistemologies that would extend even to science itself. Their efforts in this direction began early in their careers and continued to shape their thinking about their own scientific endeavors and science in general. In fact, Tolman, Hull, and Skinner all embarked on careers in psychology with strong and formative interests in epistemology. In sum,

the limits of the behaviorist-logical positivist alliance can be understood as a consequence of the fact that the "philosophies" of science of the major neobehaviorists were in reality psychologies of science derived from their deeply held theoretical and pre-theoretical views of psychology. If this is the case, then one of the major claims of the standard account--that the neobehaviorists imported extraneous methodologies into psychology--is in serious need of revision. Indeed, as will be argued in Chapter 10, the neobehaviorists' psychologies of science actually anticipated, in important respects, the new approach to the philosophy of science that has recently displaced the logical positivist approach as the dominant interpretation of science.

If in fact behaviorist views of science were crucially dependent on behaviorist views of psychology, then it becomes imperative to closely examine each behaviorist's psychological theory in order to understand his interpretation of science. The present volume provides just such examinations, in turn, for the major neobehaviorists Tolman, Hull, and Skinner. In the course of these examinations, the presentation of the revised account will emerge along four broad lines of argument. The resulting four themes can be briefly stated as follows:

1. Limited Sympathies. Each of the major neobehaviorists had limited sympathy with logical positivism. For

Tolman the area of sympathy was operationism, for Hull deductive methods, and for Skinner positivism itself.

2. Priority. In each case, the major neobehaviorists arrived at the area of agreement with logical positivism prior to contact with logical positivism. Thus, Tolman had worked out the essentials of his operationism before he encountered logical positivism (and even before he encountered P. W. Bridgman's operationism⁴⁹); Hull's interests in deductive methods were well-formed by 1927, approximately a decade prior to his encounter with logical positivism; Skinner's positivism was drawn from a reading (around 1930) of Mach, before he had developed any interest in logical positivism.
3. Indigenous Methods. Each neobehaviorist's (limited) convergence with logical positivism was developed in close connection with a deep-seated conception of organismic behavior. Tolman's operationism stemmed from his neorealist view of the manner in which an organism's purposes and cognitions are expressed in behavior. Hull's deductive methods grew out of his belief that behavior possesses a determinate and hierarchial causal structure, like that of a complex, logically designed machine. Skinner's positivism arose from his Machian view that behavior,

including scientific activity, is a biological process of adjustment to an environment. Thus, the methods of these behaviorists were not so much imported from outside of psychology as they were developed indigenously from their conceptions of psychology.

4. Behavioral Epistemology.⁵⁰ For each of the major neobehaviorists, the area of shared interest with the logical positivists was part of a behavioral epistemology in which the same conception of learning was applied equally to the scientist and the subject. Each believed (though in ways differing from each other) that knowledge is ultimately a manner of responding to an environment; each was consistent in applying this principle to rats and humans alike.

These four themes suggest immediately why the actual intellectual overlap between behaviorism and logical positivism was so limited. In the final analysis, the behaviorists saw knowledge in general and science in particular as psychological phenomena, whereas the logical positivists viewed knowledge and science as relatively abstract matters of language and logic. Beneath the surface agreements, naturalistic epistemologies were being pitted against formal epistemologies.

In the historical account comprising the bulk of this work, the deep difference between behaviorist and

logical positivist construals of science will be manifested in many different forms. It will become evident, for instance, that the three behaviorists considered here were closer philosophically to pragmatism than to logical positivism. It will also become apparent that their views of science placed greater emphasis on the context of discovery than did those of the logical positivists, who stressed the context of justification.⁵¹ Another manifestation--indeed, a decisive one--of the deep differences between the major neobehaviorists and the logical positivists is the fact that each of the former gave logic itself a psychological (and more precisely, a behavioral) interpretation. In doing so, they set themselves in direct opposition to what was perhaps the most fundamental of logical positivism's tenets--namely, that logic is tautologous, devoid of empirical content, and never properly construed in psychological terms.

Plainly, much of the present account of behaviorism and logical positivism is at odds with the standard account. But there is also much in the present account that corroborates and extends previous accounts. To be sure, logical positivism did exert an influence on neobehaviorism. At one time or another and in different ways, Tolman, Hull, and Skinner were all actively interested in and (in differing degrees) influenced by logical positivism. However, from a richly historical perspective this

influence can be seen to have been more restricted than is commonly supposed and also more varied in nature. Sometimes, logical positivism was a reinforcing--although not formative--influence on neobehaviorism. It shaped the mode of expression and the details of neobehaviorism's methodologies, but without having instigated them. At other times, it altered and diverted developments within neobehaviorism, sometimes suppressing novel and significant ideas. And, as suggested above, it occasionally stood in direct opposition to certain neobehaviorist views, thereby obscuring unique behavioral theses.

The revised account of the behaviorist-logical positivist alliance calls attention to certain fundamental epistemological issues. If in fact the neobehaviorists' "philosophy" of science was really a set of psychologies of science, what becomes of the epistemological status of their methodologies, which presumably rest on their views of science? Is it not circular to pursue a psychological science with methods that depend in the final analysis on the outcomes of that pursuit? If so, is this a vicious sort of circularity? If psychological methodology is not only relativized to psychological theory but to different theories at that, can the methodology provide an antecedently defensible means of evaluating and deciding between psychological theories? Is a set of theory-neutral methodological principles therefore a chimera? This

cluster of important issues, along with others of a historical, philosophical, and historiographical nature, will be discussed in the concluding chapter of this work.

The Nature of the Present Volume

As has already been described, the main works comprising the standard account of behaviorism and logical positivism have largely been carried out as rational reconstructions, with at most a semi-historical emphasis. As such, these works have tended to focus on the "Age of Theory," which began in the late thirties and ended in the mid-fifties. By way of contrast, the present account focuses on the period from roughly 1925 to 1938 in order to give a genetic account of those theories and the views of science with which they were intimately connected. For an understanding of behaviorism in its relation to logical positivism, this early focus is necessary for at least two reasons: because once logical positivism had assumed its position of hegemony in the philosophy of science, it became difficult to separate neobehaviorist theories of learning from that context; and because the indigenous psychological views of science developed by the major neobehaviorists were well-formed by the late thirties, when logical positivism had become an important influence on American psychology.

Just as the present work focuses on a relatively restricted period in the history of behaviorism, so too it focuses on a subset of the neobehaviorists, namely, Tolman, Hull, and Skinner. This restriction of range can be justified on two counts. First, these three figures are the most important neobehaviorists, both in the (intellectual) sense that they initiated and developed eminent theories of behavior and in the (sociological) sense that they established influential traditions of research that have recognizably endured to the present time.⁵² Second, all three of these figures have been explicitly linked with logical positivism in the literature and lore of the recent history of psychology. Since allegations of associations with logical positivism have been much more frequent for Tolman and Hull than for Skinner, greater emphasis has been placed on the accounts of Tolman and Hull. Nonetheless, Skinner is also treated, though much more briefly, because 1) he is sometimes linked with logical positivism⁵³, 2) he has since 1960 become the most important behaviorist, and 3) his relative aloofness to logical positivism is due to significant and characteristic reasons, the elaboration of which will help illuminate central themes of this work.

Although the present work contains a chapter on the development of logical positivism, it is primarily a work in the history of neobehaviorism. However, this is not

to say that it is purely a work in the history of science per se. While it does describe the development of certain theories in science, it concomitantly describes certain theories of science (as should be clear from the foregoing précis of the revised account). Accordingly, this work aims to provide, from a historical standpoint, a consideration of developments in three areas: behavioral psychology, philosophy of science, and psychology of science.

Historiographically speaking, the developments treated herein are approached from an internalist perspective. The emphasis throughout is on the internal evolution of ideas rather than on their sociological, cultural, and political context. Although a coherent picture of these developments emerges from the internalist perspective employed herein, the adoption of that perspective remains of course partly arbitrary. Indeed, external factors in the story being told here occasionally loom prominently in the background. Two examples may be briefly noted. First, the Unity of Science movement through which behaviorists and logical positivists came into contact with each other was explicitly designed in part to spread logical positivism outside of the hostile cultural and political climate in which it arose. Second, Hull's prominence among psychologists is at least partially attributable to the fact that his research was being liberally funded by the Rockefeller Foundation during the Depression, a time

when funding was extremely scarce and unemployment among psychologists was high. Clearly, these and other external factors deserve a more careful treatment than the present account has undertaken, and the entire story could no doubt profitably be retold from an externalist orientation.⁵⁴

As a related historiographical matter, it is the strategy of the present work to include biographical material only as it impinges on themes important to the book. Although details of personal lives have been kept to a minimum, they are often quite significant. Hull's early fascination with machinery and his background in engineering, for example, turn out to be crucial factors in his intellectual development. Likewise, Tolman's upbringing in the context of Unitarian liberalism seems to have engendered in him an openness of attitude and intellect which influenced his eclectic approach to science. Factors such as these are discussed in the treatments of each behaviorist, but in general they are deemphasized relative to the internal conceptual developments.

Finally, the reader should be alerted to the overall structure of this work. Following an introductory overview of logical positivism, the main body of the work consists of detailed historical treatments of the major neobehaviorists. For each of these figures, the

narrative is laid out sequentially in three parts. First, the ideas that each behaviorist held (roughly) in common with logical positivism are presented in an account that emphasizes their elaboration prior to and independent of the influence of logical positivism on psychology. Second, each behaviorist's actual interactions with logical positivism are described. Third, the divergence of each behaviorist from the logical positivist views of science is discussed, and in each case this divergence is shown to be the result of each behaviorist's psychologistic approach to science. In sum, the pattern is as follows: anticipation of logical positivist ideas, interaction with logical positivism, and divergence from logical positivism.

Notes for Introduction

1. I have described this rapprochement in "Philosophy and Psychology: Toward a Realignment, 1905-1935," Journal of the History of the Behavioral Sciences 17 (1981): 28-37.

2. It may be noted here that an anti-metaphysical positivism of a variety that predated logical positivism was already fashionable among many younger psychologists, some of whom were being reacted against by behaviorism. See Kurt Danziger, "The Positivist Repudiation of Wundt," Journal of the History of the Behavioral Sciences 15 (1979): 205-230. This earlier positivistic psychology was derogated by one observer with the remark that if the old psychology was "metaphysics" then the new psychology should be called "hypo-psychics." See J. MacBride Sterrett, "The Proper Affiliation of Psychology--With Philosophy or with the Natural Sciences?," Psychological Review 16 (1909): 85-106, on p. 102.

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

4. Moritz Schlick, "The Turning Point in Philosophy," tran. David Rynin, in Logical Positivism, ed. A. J. Ayer (New York: Free Press, 1959), pp. 53-59. This article originally appeared in the 1930-31 volume of the logical positivist journal Erkenntnis.

5. Rudolf Carnap, Scheinprobleme in der Philosophie, das Fremdpsychische und der Realismustreit (Berlin: Weltkreis-Verlag, 1928).

6. The distinction between tough- and tender-minded was elaborated in James's Pragmatism (New York: Longmans, Green, 1907). The distinction was later used to differentiate logical positivism from inferior "arm-chair" philosophy by the psychologist S. S. Stevens in his "Psychology and the Science of Science," Psychological Bulletin 36 (1939): 221-263.

7. Behaviorism was proclaimed in Watson's 1913 manifesto (see note 3 above). Logical positivism was officially proclaimed as a movement by Otto Neurath, Hans Hahn, and Rudolf Carnap in their Wissenschaftliche Weltauffassung: Der Wiener Kreis (Vienna: Artur Wolf Verlag, 1929). The actual expression "logical positivism" did not appear until two years later, in Herbert Feigl and Albert E. Blumberg, "Logical Positivism," Journal of Philosophy 28 (1931): 281-296.

8. The extent to which contemporary cognitive psychology actually differs from behaviorism has been questioned by Thomas H. Leahey, "The Revolution Never Happened: Information Processing Is Behaviorism," paper presented at the annual meeting of the Eastern Psychological Association, New York, 23 April 1981. The conceptual continuity between behaviorism and cognitive psychology has been discussed from a different perspective by Daniel C. Dennett, "Why the Law of Effect Will Not Go Away," in Brainstorms: Philosophical Essays on Mind and Psychology (Montgomery, Vermont: Bradford Books, 1978), pp. 71-89.

9. Sigmund Koch, "Psychology and Emerging Conceptions of Knowledge as Unitary," in Behaviorism and Phenomenology: Contrasting Bases for Modern Psychology, ed. T. W. Wann (Chicago: University of Chicago Press, 1964), pp. 1-41, on p. 11; "Epilogue" in Psychology: A Study of a Science, vol. 3, ed. Sigmund Koch (New York: McGraw-Hill, 1959), pp. 729-788, on pp. 731-733.

10. Edwin G. Boring, A History of Experimental Psychology, 2nd rev. ed. (Englewood Cliffs, N.J.: Prentice-Hall, 1950), pp. 620-663; Sensation and Perception in the History of Experimental Psychology (New York: Appleton, 1942), pp. 12-13.

11. Koch, "Emerging Conceptions," pp. 12, 10; "Epilogue," pp. 731-733; Encyclopedia Britannica (1961), s.v. "Behaviorism."

12. Koch, "Emerging Conceptions," p. 10.

13. See, for example, John Passmore's claim that logical positivism "is dead, or as dead as a philosophical movement ever becomes" in The Encyclopedia of Philosophy, ed. Paul Edwards (New York: Macmillan, 1967), s.v. "Logical Positivism." See also the section entitled "Swan Song for Positivism" in Frederick Suppe, "Afterword," in The Structure of Scientific Theories, 2nd rev. ed., ed. Frederick Suppe (Urbana, Ill.: University of Illinois Press, 1977), pp. 615-730.

14. This phrase (Koch, "Emerging Conceptions," p. 21) appeared in 1964, well before sustained criticisms of logical positivism had produced widespread belief in its untenability.

15. Koch, "Emerging Conceptions," pp. 6, 20. The italics in this passage are Koch's. In the present work, I have avoided adding italics to quoted material whenever possible. Therefore, I will not in subsequent notes

15. (cont'd.) specify that italics are in the original material. The reader can assume that, unless otherwise specified, all emphasized words in quotations were stressed by their original author.

16. Prominent among those who have called attention to the role of psychological factors in scientific epistemology are Michael Polanyi, Personal Knowledge (Chicago: University of Chicago Press, 1958); Norwood Russell Hanson, Patterns of Discovery (Cambridge: Cambridge University Press, 1958); and Thomas S. Kuhn, The Structure of Scientific Revolutions (Chicago: University of Chicago Press, 1962).

17. Koch, "Emerging Conceptions," p. 5. Since this was written in 1964, psychology has become somewhat less dominated by logical positivism, although how much less is debatable.

18. Brian D. Mackenzie, Behaviourism and the Limits of Scientific Method (Atlantic Highlands, N.J.: Humanities Press, 1977), p. 183.

19. Ibid., p. xiii.

20. Ibid., p. 149.

21. Ibid., pp. 100, 151.

22. Ibid., p. 116.

23. Ibid., p. 1.

24. Ibid., p. 154.

25. Ibid., p. 154.

26. Ibid., pp. 154-155.

27. Since early in its history, behaviorism has frequently been pronounced dead, despite the fact that theory and research in the behaviorist mold remain quite active even today. See G. E. Zuriff, "The Demise of Behaviorism--Exaggerated Rumor?: A Review of Mackenzie's Behaviourism and the Limits of Scientific Method," Journal of the Experimental Analysis of Behavior 32 (1979): 129-136.

28. Thomas H. Leahey, A History of Psychology (Englewood Cliffs, N.J.: Prentice-Hall, 1980), pp. 148, 312.

29. Ibid., p. 313.
30. Ibid., p. 356.
31. Koch, "Emerging Conceptions," p. 7.
32. Mackenzie, Behaviourism and Scientific Method, pp. xi, xii.
33. E.g., Koch, "Emerging Conceptions," pp. 7-8.
34. Robert Elias Abu Shanab, "Logical Positivism, Operationalism, and Behaviorism" (Ph.D. dissertation, Ohio State University, 1969), pp. 1, 187-188, 205. See also by the same author, "Tolman: Operationalism and Logical Positivism," The Pakistan Philosophical Journal 11 (1972): 1-15.
35. Cornelis Sanders, Die behavioristische Revolution in der Psychologie, trans. from the Dutch by Werner O. Vogel (Salzburg: Otto Müller Verlag, 1978), p. 79. Original Dutch edition published in 1972.
36. In this regard, it is noteworthy that the philosophy of science achieved the status of a discipline in America through the efforts of the logical positivists. The logical positivist Herbert Feigl has written that when he arrived at Harvard in 1930, there were only two philosophers of science in America (Morris Cohen and A. C. Benjamin). See Feigl, "The Wiener Kreis in America," in The Intellectual Migration 1930-1960, ed. Donald Fleming and Bernard Bailyn (Cambridge: Harvard University Press, Belknap Press, 1969), p. 660.
37. Sigmund Koch, "The Logical Character of the Motivation Concept. I," Psychological Review 48 (1941): 15-38.
38. Sigmund Koch, "Clark L. Hull," in Modern Learning Theory, by William K. Estes, Sigmund Koch, Kenneth MacCorquodale, Paul E. Meehl, Conrad G. Mueller, Jr., William N. Schoenfeld, and William S. Verplanck (New York: Appleton-Century-Crofts, 1954), pp. 1-176.
39. See, for example, I. Bernard Cohen, "History and the Philosopher of Science," In Suppe, Scientific Theories, pp. 308-349.

40. Mackenzie, Behaviorism and Scientific Method, pp. 152, 147. In general the repeated claims by Mackenzie that neobehaviorist science was strongly governed by rules of procedure are overstated and not well substantiated. Likewise, Koch's assertions to the effect that behaviorists have relied on "decision procedures" and "differential tests" ("Emerging Conceptions," pp. 9, 12) are ambiguous and misleading.

41. In this connection, Mackenzie mentions only the controversy over the phenomenon of transposition, but he might just as well have referred to the controversy over latent learning (Behaviorism and Scientific Method, p. 148). Careful historical analysis of these controversies would be required to determine whether or not 1) the relevant experiments were intended as "crucial experiments" and 2) they were in fact crucial experiments.

42. Hull did speak of "crucial tests" of a theory, but for Hull these were simply experiments designed to test previously untested implications of a theory, not tests which would arbitrate between competing theories. See Hull, "The Conflicting Psychologies of Learning--A Way Out," Psychological Review 42 (1935): 491-516, on p. 495.

43. Mackenzie's use of single quotation marks around the expression "crucial experiments" is perhaps intended as an acknowledgement that the phrase was not used by the neobehaviorists. In any case, this ambiguity exemplifies the dangers of applying concepts to historical situations in which the principal figures did not themselves use those concepts.

44. Encyclopedia of Philosophy, s. v. "Psychological Behaviorism," by Charles Taylor; Benjamin B. Wolman, Historical Roots of Contemporary Psychology (New York: Harper & Row, 1968), p. 8.

45. "Behavioral psychologies" is intended here in a broad sense which includes deep (essentially metaphysical or pre-theoretical) conceptions of the nature of organismic behavior.

46. The logical positivists' interests in Gestalt psychology and psychophysics are evidenced, for instance, by the publication in the logical positivist journal Erkenntnis of such articles as Karl Duncker, "Behaviorismus und Gestaltpsychologie," Erkenntnis 3 (1932/33): 162-176 and J. F. Brown's "A Methodical Consideration of the Problem of Psychometrics," Erkenntnis 4 (1934): 46-63.

46. (cont'd.) Regarding the logical positivists' interest in psychoanalytic theory, see Rudolf Carnap, "Intellectual Autobiography," in The Philosophy of Rudolf Carnap, ed. Paul A. Schilpp (LaSalle, Ill.: Open Court, 1963), pp. 1-84, on p. 58. Citations of Dewey by Hull are contained in Clark L. Hull, Carl I. Hovland, Robert T. Ross, Marshall Hall, Donald T. Perkins, and Frederic B. Fitch, Mathematico-Deductive Theory of Rote Learning: A Study in Scientific Methodology (New Haven, Conn.: Yale University Press, 1940), while B. F. Skinner has cited Mach and Poincaré in his "The Concept of the Reflex in the Description of Behavior," Journal of General Psychology 5 (1931): 427-458.

47. Edward C. Tolman, "Psychology versus Immediate Experience," Philosophy of Science 2 (1935): 356-380, on p. 364; "An Operational Analysis of 'Demands'," Erkenntnis 6 (1937): 383-392, on p. 390; E. C. Tolman, B. F. Ritchie, and D. Kalish, "Studies in Spatial Learning. I. Orientation and the Short-Cut," Journal of Experimental Psychology 36 (1946): 13-25, on p. 14. Hull's sole citation of Carnap is given in his "The Problem of Intervening Variables in Molar Behavior Theory," Psychological Review 50 (1943): 273-291, on p. 277. Skinner's citations of Carnap appear in his Behavior of Organisms: An Experimental Analysis (New York: Appleton-Century-Crofts, 1938), p. 429 and Verbal Behavior (Englewood Cliffs, N.J.: Prentice-Hall, 1957), pp. 110, 319. The significance, or lack thereof, of these citations will be discussed in the appropriate chapters below.

48. In regard to Hull's attendance at the Third International Congress for the Unity of Science in Paris (1937), only one respondent (out of approximately twenty who knew Hull either as students or colleagues) reported even having been aware that Hull had gone to Paris: "All I remember about Hull's trip to Paris is that he came back with a beret. I have no idea why he went" (Robert R. Sears to Laurence D. Smith, 20 February 1981).

49. Bridgman's first pronouncement of operationism was in Percy W. Bridgman, The Logic of Modern Physics (New York: Macmillan, 1927). As we shall see in Chapter 3 below, Tolman was already practicing an early brand of operationism by 1927.

50. This expression is preferable to "behaviorist epistemology" because it emphasizes that the epistemologies in question inherently involved behavioral processes, not merely that they were epistemologies held by behaviorists. For an example of a behaviorist epistemology which is not a behavioral epistemology, see Kenneth W. Spence, Behavior Theory and Conditioning (New Haven, Conn.: Yale University Press, 1956), pp. 11-15. Spence, who was the major disciple of Hull, subscribed to a revised version of logical positivism, or at least held it as an ideal epistemology which psychologists could strive to follow.

51. According to this important and influential distinction, the context of discovery includes the social and psychological factors involved in arriving at a scientific idea or theory, whereas the context of justification concerns the validation of an idea or a theory once it has arisen. The distinction was first drawn by Hans Reichenbach, Experience and Prediction (Chicago: University of Chicago Press, 1938).

52. The latter claim is perhaps difficult to defend in the case of Tolman, whose eclecticism and open-mindedness in matters of science make the tradition he engendered difficult to delineate (see David L. Krantz and L. Wiggins, "Personal and Impersonal Channels of Recruitment in the Growth of Theory," Human Development 16 [1973]: 133-156). Nevertheless, the Tolmanian tradition can be discerned in the work of the former Tolman student Donald T. Campbell, who shares Tolman's views that 1) all knowledge is tentative, 2) trial and error is a fundamental means of gaining knowledge, 3) psychological phenomena are typically multiply determined, and 4) science is itself a psychological phenomenon. See, for example, Campbell's "Methodological Suggestions from a Comparative Psychology of Knowledge Processes," Inquiry (Oslo) 2 (1959): 152-182; "Evolutionary Epistemology" in The Philosophy of Karl Popper ed. Paul A. Schilpp (LaSalle, Ill.: Open Court, 1974), pp. 413-463. For an example of contemporary work in the Hullian tradition, see Robert Rescorla and Allan R. Wagner, "A Theory of Pavlovian Conditioning: Variations in the Effectiveness of Reinforcement and Non-Reinforcement" in Classical Conditioning II: Current Theory and Research, ed. Abraham F. Black and William F. Prokasy (New York: Appleton-Century-Crofts, 1972).

53. See Wolman, Historical Roots, p. 8 (cited in note 45 above), and C. H. Waddington's remarks in "General Discussion," in Beyond Reductionism: New Perspectives in the Life Sciences, ed. Arthur Koestler and J. R. Smythies (Boston: Beacon Press, 1969), p. 418.

54. Indeed, the general fashion in which the American political and economic climate fostered behaviorism is a topic worthy of detailed treatment. For a brief account, see David Bakan, "Behaviorism and American Urbanization," Journal of the History of the Behavioral Sciences 2 (1966): 5-28. This topic is also treated in John M. O'Donnell, "The Origins of Behaviorism: American Psychology, 1870-1920," (Ph.D. Dissertation, University of Pennsylvania, 1979).

CHAPTER 2

THE LOGICAL POSITIVIST VIEW OF SCIENCE

In 1922, Moritz Schlick arrived at the University of Vienna to assume a chair in the history and theory of inductive science, a position which had once been held by Ernst Mach. Like Mach, Schlick was a physicist-philosopher interested in the epistemology of the natural sciences. Schlick's arrival at Vienna would prove to be a decisive event in the history of Western philosophy, for, in the years following his arrival, there grew up around him a discussion group which came to be known as the "Wiener Kreis" or Vienna Circle. The ideas developed in this group exerted a powerful influence on philosophical and scientific thought in the Western world, especially in English-speaking countries, during the subsequent three or four decades.

Even though the Vienna Circle was known as a school of philosophical thought, its members were trained mostly in science, mathematics, and logic rather than in philosophy. Among its members trained in physics were Rudolf Carnap, Philipp Frank, Schlick, and Schlick's student Herbert Feigl. The mathematicians in the Circle included Kurt Gödel, Hans Hahn, and Gustav Bergmann.

Several of the members were trained in logic, the more expert among them being Carnap and Hahn. Although physics and the formal sciences were the predominant fields in the Circle, other disciplines were represented as well. There was the economist-sociologist Otto Neurath, the historian Victor Kraft, and the lawyer Hans Kelsen.¹ The guiding theme which held together these thinkers of diverse backgrounds was the idea that all knowledge could be accounted for, without resort to metaphysics, from the perspective of the scientific world-view. In their general aim of promoting the scientific outlook, they saw themselves as followers of Mach, but they believed that Mach had seriously underestimated the role of mathematics and logic in science. For the members of the Vienna Circle--and especially for those who, like Carnap, were both scientists and logicians--a major aim was to depict knowledge in a way that did full justice to its empirical and logical components.

It was this dual emphasis in the Vienna Circle that led its members to refer to their philosophical position as "logical positivism" or, equivalently, "logical empiricism."² In either form the designation emphasized their belief that knowledge is grounded in experience. But whereas earlier forms of positivism had stressed the biological and sociological aspects of knowledge and earlier versions of empiricism had emphasized the importance

of perception, the logical positivists concerned themselves primarily with knowledge in its linguistic and logical aspects. They gave empiricist philosophy what has been fittingly described as a "linguistic turn."³ The logical thrust, they believed, complemented the empiricist view of knowledge and provided a resolution of the age-old opposition of rationalism and empiricism. The complementarity of logic and empiricism in the logical positivists' thought was manifested in their endorsement of a strong distinction between analytic and synthetic propositions. It was also revealed in their three-way classification of sentences into logical claims, empirical claims, and nonsensical utterances. According to them, there could be no meaningful discourse outside the realm of logic and science.

As it turned out, the logical positivist aim of clarifying the roles of logic and mathematics, on the one hand, and the role of empiricism, on the other, was never realized in a completely satisfactory way. The strong distinction between logical and empirical propositions, as well as a host of related sharp distinctions, proved difficult to maintain when subjected to close scrutiny or when applied to science in any detailed way. It was as if, having once dichotomized the logical and the empirical, the logical positivists were never quite able to reunite them into a plausible picture of scientific

knowledge. Their attempts to do so constitute much of the story of their philosophical movement.

The following section provides a brief review of the historical background of logical positivism. It focuses on the intellectual heritages of both the logical and empiricist-positivist sides of the movement and on the intellectual tensions that resulted from this dual heritage.

Historical Precursors: The Roots of Intellectual Tensions

Frege and the New Logic

After a long period of little or no development in the field of logic, the nineteenth century saw revolutionary changes in which logic became a more exact and powerful discipline. These changes, which were inspired by the need for clarification of the foundations of mathematics, began to appear with the publication in mid-century of important works by Augustus de Morgan and George Boole. But the most important advances were made later in the century by Gottlob Frege.⁴ Frege's overriding aim was to formalize mathematics in a manner sufficiently rigorous as to eliminate the need to appeal to intuition in mathematical proofs. Mathematics had grown rapidly since the scientific revolution, but without corresponding increases in the understanding of its foundations. Mathematicians generally relied on intuition

rather on explicit statements of their assumptions. Frege stressed that such a reliance on intuition was risky--as had been shown by the discovery of non-Euclidean geometries--and he set out to remedy that situation.

In his attempt to clarify the foundation of mathematics, Frege's efforts were focused on the reduction of arithmetic to logic. He was only partially successful in carrying out this "logicist" program, but his general approach to the problem and the system of logic he developed for the undertaking were to have a profound influence on twentieth century philosophy. In terms of his general strategy, Frege had employed the Cantorian theory of sets in defining the concept of natural number. This was an approach later followed not only by Russell and Whitehead in their treatment of mathematics but also by Russell and Carnap in their logical constructions of the empirical concepts of science. In terms of his system of logic, Frege's precise formalization of the logic of relations, with its introduction of quantified predicates, superseded the older Aristotelian logic of subject and predicate--an accomplishment that made Frege the founder of modern mathematical logic.

The new power and flexibility of the Fregean logic raised hopes that traditional philosophical problems would succumb to careful logical analysis. Many who followed Frege believed that the new logic provided philosophy

with a solidly grounded method which would ensure philosophical progress, just as they believed scientific progress was guaranteed by the application of scientific methods. For Russell, who believed that Fregean logic had made possible the resolution of age-old perplexities concerning the nature of numbers, the methods of logic meant that philosophy could be "piecemeal and provisional like science."⁵ For Frege's student' Rudolf Carnap, the new logic was at once the key to epistemology and to the elimination of metaphysics. He wrote that "the theory of knowledge, which is after all nothing but applied logic, can no more dispense with symbolic logic than physics can dispense with mathematics." As for the traditional systems of philosophy, whatever could not be adequately formulated in symbolic logic would have to be rejected as metaphysical. "[I]n the new logic . . . ," said Carnap, "lies the point at which the old philosophy is to be removed from its hinges."⁶

Both Frege and the logical positivists after him recognized that in seeking a precise yet general language for the expression of ideas they were revitalizing a tradition begun by Leibniz. In his works on logic, Leibniz had enunciated the goal of developing an ideal language, one which was capable of expressing facts and inferences with such clarity and accuracy that all of human reasoning could be carried out by straightforward calculations within

that language. Such a language would not only form a basis for the unity of science but it would permit the settlement of all disputes by mere calculation. Frege, who was not as sanguine about the possibility of a completely universal language, aspired to Leibniz's goal only for the realm of mathematics. But the successes of Russell and Whitehead in Principia Mathematica (1910-13) in revising and extending Frege's logicist program and in developing a convenient system of notation inspired in the logical positivists a somewhat greater optimism over the prospects for an ideal language that would encompass the empirical as well as formal sciences. Although this optimism faded with time, the logical positivists continued to adhere to the more general notion that traditional philosophical puzzles and metaphysical disputes arise from the unfortunate formulations and grammatical traps of imprecise language. In Leibnizian fashion, Frege once characterized his work in logic as "a battle against the logical blemishes of language," and the logical positivists were also guided by this conception of the role of logic.⁷

Related to this distinction between ideal and ordinary languages was another position espoused by Frege and the logical positivists--their anti-psychologism. Psychologism was the doctrine, commonly held by naturalists and empiricists, that the laws of logic are laws of psychology or, more generally, that epistemology is a

branch of psychology. John Stuart Mill's works on logic epitomized nineteenth century psychologism with the claim that logic rests on empirically induced rules of thought and therefore belongs to the field of psychology. Later in the century, naturalism spawned other psychologistic interpretations of logic as well, and Frege vehemently denounced psychologism in all of its guises.⁸ For Frege and others of a formalist bent, it was difficult to see how the laws of logic could have their character as universal and necessary truths if they were based on the inexact and ephemeral psychological processes of individual human psychological activity. Certainly, any psychologistic view of logic would appear to render it incapable of serving as a foundation for mathematics and thus would undercut Frege's major intellectual aim of reducing mathematics to logic. Accordingly, Frege wrote the following admonition against psychologism:

Never let us take a description of the origin of an idea for a definition, or an account of the mental and physical conditions on which we become conscious of a proposition for a proof of it. A proposition may be thought, and again it may be true; let us never confuse these two things. We must remind ourselves, it seems, that a proposition no more ceases to be true when I cease to think of it than the sun ceases to exist when I shut my eyes.⁹

Frege's attacks on psychologism were relentless. He spoke of "the devastations which have been brought about by the incursion of psychology into logic" and denounced

psychologism as "a widespread philosophical disease."¹⁰ The image of disease was a recurring feature in Frege's harshly worded critiques of psychologism. W. C. Kneale has written of Frege that

he insisted always on the need for a sharp distinction between logic and psychology, and condemned the logic teaching of his day as psychologisch verseucht, that is, infected with psychology, or perhaps even more strongly, rotten with psychology.¹¹

Frege's distinction between a proposition and its psychological use or occurrence would be difficult to uphold for most naturalists and empiricists. For them, a proposition is of interest only in its natural occurrence. For Frege, an avowed rationalist, this was no problem. His interest was only in demonstrating the a priori basis of mathematics and logic. But for Russell and the logical positivists, the situation was not so simple. They were attempting a synthesis of logic and empiricism, and their aspirations were to extend the application of the new logic to the natural, and even social, sciences. In accord with his background in the British epistemological tradition, Russell admitted that propositions are mental events; but the logical positivists rejected Russell's psychologistic tendencies and, with respect to logic itself, held an anti-psychologistic position akin to Frege's. With respect to the broader issue of psychologism in epistemology as a whole, the logical positivists had

to reformulate their anti-psychologism in such a way as to acknowledge a certain role for psychology. Carnap's early claim that epistemology is merely applied logic required a slight modification to accommodate the empiricist claim--one difficult to dispute--that knowledge in fact has psychological origins. The logical positivists' attempted reconciliation of Fregean anti-psychologism and empiricism eventually took the form of a distinction between the context of discovery and justification, an important distinction that will be discussed below.

Mach and the Empiricist Tradition

In their empiricist aspect, the logical positivists claimed to be descendents of the intellectual tradition which began in earnest with the British empiricism of the eighteenth century. An important early representative of this tradition, and one who anticipated major themes of logical positivism, was David Hume. Hume distinguished between matters of fact and relations of ideas. Whereas statements about matters of fact were to be tested by direct reference to experience, statements about relations of ideas could be evaluated without reference to experience by mere introspective examination of the relevant ideas. What later became known as "necessary" truth could be found only among relations of ideas. Any statements that could not be verified either empirically or

through the analysis of ideas were rejected by Hume as meaningless discourse. In his Enquiry Concerning Human Understanding (1748), Hume gave colorful expression to these ideas:

When we run over libraries, persuaded of these principles, what havoc must we make? If we take in our hand any volume; of divinity or school metaphysics, for instance; let us ask, Does it contain any abstract reasoning concerning quantity or number? No. Does it contain any experimental reasoning concerning matter of fact and existence? No. Commit it then to the flames: for it can contain nothing but sophistry and illusion.¹²

Hume's distinction of the empirical and the necessary and his rejection of all other candidates for knowledge clearly foreshadowed the logical positivists' trichotomy of empirical, logical, and nonsensical expressions. Furthermore, his vitriolic rejection of metaphysics presaged the anti-metaphysical temper of logical positivism.

However, despite these close parallels between Hume's views and logical positivism, Hume's epistemology differed deeply from that of the logical positivists in being a psychologistic theory of knowledge. Empirical knowledge, for Hume, consisted of habits in which sequences of sense impressions were strung together by the psychological laws of association. In Hume's account, even the knowledge embodied in necessary truths was given a psychologistic interpretation. In the first place, ideas were said to be sense impressions which endure in memory or reflection or else combinations of such sense impressions conjoined by psychological laws of association. In the

second place, the processes by which knowledge of necessary truth was said to be gained were the psychological processes of introspection and analysis. Thus, even the necessary knowledge which Hume admitted to exist was psychologized by him in a way that would have been unacceptable to Frege and the logicians of the Vienna Circle.

Among the major figures in nineteenth century empiricism, two who were frequently claimed by the logical positivists as intellectual forbearers were Auguste Comte and John Stuart Mill. Comte, who coined the term "positivism," insisted that all genuine knowledge is based on experience. The laws of science, in his view, are statements of succession and similarity among observed phenomena, and the claims of theology and metaphysics which transcend direct experience are to be rejected. Comte's rejection of metaphysics was backed by a theory of history according to which the theological and metaphysical stages of human development are superseded by a positive stage dominated by science. In this last stage, he believed, the crowning development would be a positive sociology which would permit a rational and harmonious social order to be established along scientific lines. The atavistic religious needs of people would be met in the positive stage by the église de raison--the church of reason--of which Comte was the self-appointed high priest. Although

the quasi-religious aspects of Comtean positivism were rather an embarrassment to the logical positivists, they endorsed his scientific view of positive knowledge, and those who, like Neurath, gave serious attention to the social implications of science drew upon his view of the relation between social unity and the scientific outlook.

Mill sympathized with many of Comte's ideas and integrated some of them with the British empiricist tradition. Like Hume and Comte, he held that all knowledge is founded on experience and that transcendent claims to knowledge are unnecessary and impossible. He accepted Comte's doctrine of the three stages of human progress and acknowledged the need to organize society on a scientific basis. But whereas Comte had emphasized the sociological ramifications of science, Mill's writings on science stressed its methodological aspects. According to him, all sciences--including psychology--investigate causal regularities through the use of a set of inductive methods. Mill's codification of these methods in his Logic was a major contribution which paved the way for the logical positivists' views on the unity of scientific methods. However, Mill's epistemological views, methodology included, were like Hume's in being psychologistic and highly empiricistic. For him, mathematics was a very abstract, general description of empirical regularities,

and the rules of inference in logic and methodology were psychologically derived laws of thought. As was the case with Hume, Mill's psychologism was at odds with the later views of both Frege and the logical positivists.

The empiricist who is usually credited with having had the greatest influence on the logical positivists is Ernst Mach.¹³ Mach was identified by the members of the Vienna Circle as their immediate predecessor in the empiricist tradition, and the group which they organized in the twenties to spread the ideals of logical positivism was named the "Verein Ernst Mach" in his honor. Like Schlick, who succeeded him in his chair at the University of Vienna, Mach was a philosophizing physicist rather than a pure philosopher. The logical positivists were especially drawn to his views on the unity of science and its relation to the rejection of metaphysics. According to Mach's radical empiricism and experiential positivism, the aim of all science is to provide concise descriptions of the functional dependencies among phenomena. In Mach's neutral monism, the elements which are related in the descriptive laws of science are pure experiences which are neither mental nor physical but neutral givens. As such, they enter into all science equally, whether the science be physics, physiology, or psychology. The special sciences thus differ from one another only in their modes of organizing experience. Which mode is chosen will

depend on its practical convenience, but at root all sciences are alike in yielding descriptions of experience. Moreover, for Mach, complete description is all one can ask of science; any attempt at explanation which goes beyond the bounds of description is to be eschewed as metaphysical. Whereas many philosophers had attempted to unify knowledge by invoking all-embracing metaphysical systems, Mach thus held that science could be unified only by the elimination of metaphysics in favor of a strict empiricism. Mach's explicit connection of the unity of science with the rejection of metaphysics was a great source of inspiration to the logical positivists, although their subsequent elaborations of this notion depended more on logical analysis than on radical empiricism.

In his complete rejection of transcendental and a priori claims to knowledge, Mach construed epistemology as nothing more than the "psychology of knowledge."¹⁴ In his view, all knowledge, including science, consists of efficient adaptation to an environment. Consequently, knowledge must be studied in terms of the concrete psychological processes of the knower and, ultimately, in terms of biological behavior. All thought--even logic and mathematics as well as the less exalted forms of thinking--is experiential in origin and subject to what Mach called the principle of biological economy. Mach's positivism and psychologism were little more than out-

growths of his submission of epistemology to the demands of expedient biological adaptation. Mach was not only uninterested in formal logic but was even hostile to it, at least to the substantial extent that it was irrelevant to furthering the aims of survival. At most, logic would be for him something like a "universal economy of thought."¹⁵ Mach's psychologistic interpretations of epistemology in general and logic and mathematics in particular meant that his views diverged deeply from those of the logical positivists. As Hans Sluga has recently commented: "In spite of their appeal to the name of Mach, it is therefore open to doubt whether the logical empiricists can really be considered his successors."¹⁶

All of the major empiricists recognized by the Vienna Circle as part of its intellectual heritage held positions that were more thoroughly empiricistic than that of the logical positivists themselves. Whether from the perspective of psychology, sociology, or biology, they all attempted naturalistic accounts of knowledge and these accounts generally included psychologistic views of logic. On the other hand, the truly novel impetus for logical positivism came from the development of the new logic; and this development, in turn, was accompanied by an emphatic rejection of psychologism. Before logical positivism even got under way, then, there were already intellectual tensions that needed to be resolved in order

for it to become a viable philosophical movement. By the time the movement was recognized as a movement in the twenties, a number of its immediate predecessors had attempted reformulations of the relation between the empirical and the formal that would reconcile the empiricist and logical trends that led up to it. These formulations are briefly considered in the following section.

Realignment of the Formal and Empirical

Henri Poincaré was primarily known as a brilliant mathematician, but he also contributed major works in the philosophy of science. He was a follower of Mach, but like many others who worked in the formal sciences he was unsympathetic to psychologistic interpretations of mathematics and logic. In two books published shortly after the turn of the century, he developed the important notion of conventions. The discovery of non-Euclidean geometries, said Poincaré, cast serious doubt on the empirical view that geometry describes the properties of observable space. When confronted with rival geometries each of which is logically coherent, one cannot test between them empirically but rather one simply chooses between them: the chosen axiom system is adopted as a convention.¹⁷ But the choice is not therefore arbitrary. On the contrary, it is guided by concerns such as coherence, convenience, and economy of expression with respect

to observations. Conventions, which have the character of disguised definitions, are also found in science. The principle of conservation of energy is one such convention. It is neither an empirical generalization nor a synthetic a priori truth but rather an agreed upon stipulation concerning the use of the concept of energy. Poincaré's formulation of conventions thus assigned to the formal sciences a role in empirical science. A priori claims were to serve a regulative function--not as transcendental conditions of knowledge, as Kant would have it--but as stipulations chosen to govern the linguistic usage of scientists.

A somewhat similar account of the relation between the formal and the empirical was inspired by the axiomatic method of the mathematician David Hilbert. Around the turn of the century, Hilbert depicted geometry as a system of axioms which are abstracted from their putative subject matter and treated as a purely formal system. These axioms, taken together with the theorems which can be derived from them without reference to intuitions of geometrical objects, form a relational structure which in itself has no content. Such a system can, however, be applied to an empirical domain by demonstrating that certain objects and relations in the world satisfy the axiom system. This process of empirical interpretation amounts to establishing an isomorphic relation between

the formal structure and a system of empirical entities plus the lawful regularities by which they are related. In the case of Poincaré's conventions and Hilbert's formal axioms, there is no question of truth other than coherence or logical "truth." Even in their applications to natural science, they can (strictly speaking) only be said to be appropriate or inappropriate, convenient or inconvenient, or satisfied or unsatisfied. In this way, conventions and axiom systems retain their formal status while finding a place in natural science.

The approaches of Poincaré and Hilbert were integrated by Moritz Schlick in his Allgemeine Erkenntnislehre (1918). First, he characterized Hilbert's axioms as implicit definitions, a strategy later employed in the logical positivist account of scientific theories.¹⁸ Because axioms are unprovable, their validity had often been thought to be demonstrable only through appeal to intuition. But intuition was not only imprecise and unreliable (as the abandonment of the parallel postulate had shown), but reliance on it was a form of psychologism. Hilbert had shown that axioms require no validation, and Schlick took this to mean that the primitive concepts involved in an axiom system are defined only by their implicit role in the system and not by their capacity to be imagined in the intuition. Then, Schlick argued that the purely formal concepts can be connected to empirical

concepts by the appropriate choice of coordinating definitions, which he took to be conventions in Poincaré's sense.¹⁹ To be sure, the choice of these conventions would be governed by their usefulness for a given empirical domain, but they would remain stipulations governing linguistic usage and thus would be devoid of empirical content. Once the implicit and coordinating definitions were set, whatever remained--for example, the statements of laws, the empirical or "concrete" definitions--would constitute the empirical content of the overall conceptual system.

For obvious reasons, Schlick's approach was best suited to the analysis of highly developed physical theories. But he also undertook to analyze various claims in biology, psychology, and phenomenology. In all cases, he attempted to show that the sentences in question were either analytic or empirical, depending entirely on the conventions chosen for their interpretation, but in no case were they synthetic a priori. Those which had appeared to be synthetic a priori gained that appearance only through being expressed in logically faulty or unclear fashion. In other words, all meaningful expressions were either analytic or synthetic and none could be both at once.

Around the time of Schlick's work, Bertrand Russell was developing applications of the Principia Mathematica

logic to traditional problems of empiricism. He suggested that the entities of the empirical world could be logically constructed as sets of sense-data or appearances in a manner analogous to Frege's construction of numbers as sets. This approach, he thought, would prove especially valuable in giving an account of the abstract unobservable entities (e.g., electrons, matter, mental processes in others) which science had found it useful to postulate but which resisted straightforward analysis in empiricist terms. At the root of Russell's strategy was his dictum to substitute logical constructions for inferences whenever possible. Epistemology would thus be given a firm foundation by removing psychological (hence unreliable) inferences and replacing them with purely logical relations, which were precise and now well understood because of the new logic.

Like Schlick, Russell believed that the logical analysis of science would reveal a distinct separation between its formal and empirical components. As long as all knowledge was held to be derived by inference from experience, one was led like Mill to impute empirical content to even the most abstract of expressions--those of logic and mathematics. But according to Russell the replacement of inferences by constructions shows otherwise, not only for mathematics and logic but also for some of the principles of physics. The principle of the

impenetrability of matter was one such case in Russell's view. He wrote:

One cannot help feeling that impenetrability is not an empirical fact, derived from observation of billiard balls, but is something logically necessary. This feeling is wholly justified, but it could not be so if matter were not a logical construction. An immense number of occurrences coexist in any little region of space-time; when we are speaking of what is not logical construction, we find no such property as impenetrability, but, on the contrary, endless overlapping of the events in a part of space-time, however small. The reason that matter is impenetrable is because our definitions make it so. . . . Impenetrability is a logically necessary result of definition, though the fact that such a definition is convenient is empirical.²⁰

Russell went on to argue that such analyses could profitably be applied to psychological phenomena, that cognition for example "must be preserved as a construction, not as inferred entity."²¹ The only reason to hesitate in doing so, he felt, was that psychological theory was perhaps not sufficiently advanced to indicate just what concepts would be useful to reconstruct.

In the celebrated Tractatus Logico-Philosophicus (1921/22)²², Russell's pupil Ludwig Wittgenstein gave what was to become the canonical formulation of the distinction between analytic and synthetic propositions. Therein he developed the propositional calculus and the method of truth tables for ascertaining the truth value of propositions compounded out of atomic propositions by means of logical connectives. Those propositions

which are true for any combination of truth values of its constituent propositions were defined as tautologies--the analytic truths of logic. And here was the crucial point: because their truth is independent of the truth of the atomic propositions, they have no content, empirical or otherwise. These empty tautologies could be used as syntactical rules which, when applied to empirical propositions, would yield other empirical propositions without any change in their truth value; but, in themselves, logical truths have no content. Furthermore, since mathematics was believed to be reducible to logic, its propositions were also empty tautologies. Wittgenstein's formulation was hailed by the logical empiricists as a major breakthrough in the history of empiricism. No longer, they felt, was there any justification for the psychologistic view that logic and mathematics are general descriptions of either the world or the working of thought. Nor was there any need for the belief--which even Frege, the rationalist, held--that logic and mathematics describe a subject matter in the rationalist realm of transcendent entities. Without either an empirical or rational subject matter, logic and mathematics were believed to be free of metaphysics and could therefore serve as neutral instruments for the excision of metaphysics from all discourse. The previously metaphysical opposition of rationalism and empiricism had been

transformed into the ostensibly innocuous linguistic distinction between analytic and synthetic truths.

The system elaborated by Wittgenstein in the Tractatus came to be known, appropriately, as logical atomism. In some respects, it was akin to Hume's psychological atomism, but differed from it in two important respects.²³ First, Hume's epistemology stressed concepts over judgments; atomic ideas and impressions were compounded into ideas not propositions. Second, the process of compounding simples into complexes was for Hume a psychological process governed by the natural laws of association. For Wittgenstein, the process was a purely logical relation involving only the logical connectives and their definitions. According to Wittgenstein's scheme of logical atomism, the world is composed of possible states of affairs (arrangements of objects). Each possible state of affairs is pictured, in an ideal language, by an atomic proposition the structure of which perfectly mirrors that of the corresponding state of affairs. If the state of affairs actually obtains, the atomic proposition which pictures it is said to be true; otherwise it is false. The molecular propositions compounded out of the atomic propositions depict complex states of affairs. Unlike tautological molecular propositions, which are true for all possible states of affairs, these molecular propositions have a truth value which depends on the truth

value of their component propositions and on the specific logical connectives which occur in them. Along these lines, Wittgenstein construed a general statement such as "Something is F" as a disjunction of atomic statements: "a is F or b is F or c is F, etc." Likewise, the statement "Everything is F" was construed as the conjunction "a is F and b is F and c is F, etc." The similarity of this latter example to the structure of scientific laws was not lost on the logical positivists, who drew heavily on Wittgenstein's formulation in their account of the verifiability of scientific claims (see below).

All of the figures discussed in the present section strove to distinguish sharply between the analytic and the synthetic and to clarify their respective roles in science. And all of these figures were important sources of the philosophy of science which was subsequently developed in the Vienna Circle. As logical positivism evolved, the analytic-synthetic distinction and the family of dichotomies which rested on it became gradually more difficult to maintain plausibly, especially when applied in detail to actual cases of scientific knowledge. Over time, the original strict formulations of logical positivism underwent liberalizations, and these revisions were in important respects concessions to empiricism. As a consequence, the naturalism of the empiricist tradition gradually crept back into the analytic philosophy of the

Vienna Circle. In this respect, Wittgenstein's story is illuminating. No sooner had he exerted his decisive influence on logical positivism than he began to work out a new, more naturalistic philosophy--one which repudiated the formalist approach to language and science that his earlier work had helped to spawn. Hans Sluga has characterized these developments as follows:

Analytic philosophy arose in reaction to a dominant naturalism. From the very beginning it opposed radical empiricism, psychologism, historicism, evolutionism, and subjectivism. In contrast, it concerned itself with logical, formal, or a priori questions. As the tradition developed from Frege through Russell to Carnap and finally the later Wittgenstein it was forced to make greater concessions to the claims of empiricism. In Wittgenstein's later philosophy the tradition has reached a point at which it reconnects with the naturalism that emerged in the first half of the nineteenth century. . . . Frege thought he had banished radical empiricism, but in Wittgenstein it has returned to haunt the analytic tradition.²⁴

The significance of these developments for the present work is that the behaviorists discussed herein represented many of those very traditions--radical empiricism, psychologism, and evolutionism--which analytic philosophy was opposed to. By the time they came into contact with logical positivism, they had all begun to develop naturalistic epistemologies on the basis of their psychological views. In each case, these epistemologies were largely incompatible with the epistemological views of analytic philosophy and even anticipated some of the

philosophical developments which subsequently overthrew the logical positivist view.

Logical Positivism

The Vienna Circle and the "Wissenschaftliche Weltauffassung"

In 1929, Moritz Schlick was offered a lucrative position in Bonn. Because he was the organizer and personal center of the Vienna Circle, his departure would no doubt have had serious consequences for the fate of the Circle. But in a momentous decision, Schlick chose to remain in Vienna. He then spent part of a year as a visiting professor at Stanford University. Upon returning from America, he was presented with a pamphlet celebrating his return and his decision to stay in Vienna. This short monograph--authored by Neurath, Carnap, and Hahn--reviewed the development of ideas that had led up to the Vienna Circle and proclaimed that the Circle's scientific approach to philosophizing constituted a new movement in philosophy. The basic orientation of the movement was reflected in the title of its manifesto: "Wissenschaftliche Weltauffassung--Der Wiener Kreis" or "The Scientific World-Conception: The Vienna Circle."²⁵ If the pamphlet was a manifesto of a movement, it was also a programmatic statement of an intellectual position. Pointing out the dual heritage of logical positivism, its authors emphasized that the integration of the new

logic into the empiricist framework constituted a break with the traditional forms of empiricism and positivism. Echoing the anti-psychologism of Frege, they wrote: "It is the method of logical analysis that essentially distinguishes recent empiricism and positivism from the earlier version that was more biological-psychological in its orientation."²⁶ As for the logical side of this program, which was described as "[l]ogistic and its application to reality," they cited the work of Leibniz, Frege, Russell, Whitehead, Wittgenstein, and Hilbert.

In proclaiming a new movement, the authors of the "Wissenschaftlich Weltauffassung" were expressing their confidence that Schlick's decision to stay in Vienna was an auspicious sign of future productivity and growth for the movement. In fact, it proved to be just so. The period between 1929 and 1936, when Schlick's assassination brought the heyday of the Vienna Circle to a tragic close, saw not only a systematic elaboration of the Circle's philosophy but also a considerable expansion of its influence. Even in 1929, a conference on the epistemology of the exact sciences held in Prague brought the members of the Vienna Circle into contact with like-minded thinkers from other parts of Europe and resulted in the dissemination of its views. In 1930, the logical positivists took over the journal Annalen der Philosophie, renaming it Erkenntnis and transforming it into an effective instrument

for the articulation and dissemination of logical positivism. At the same time, logical positivism was being spread through personal channels. In 1930, Herbert Feigl went to Harvard University as a visiting scholar, and, after a brief return to Vienna, emigrated permanently to the United States. Schlick's visit to Stanford was followed by a second visit to America, this time at Berkeley, in 1931. In 1932, Schlick lectured on the Vienna Circle philosophy at the University of London, as did Carnap two years later. Just as important were the philosophers who were drawn to the Vienna Circle from their native countries and returned to them with word of the new philosophy. These figures included Charles Morris and W. V. O. Quine from the United States, Arne Naess from Norway, and A. J. Ayer from England--all of whom visited the Circle in the years 1932-34. Ayer returned to England to publish his classic polemic Language, Truth and Logic (1936), a book which instantly established logical positivism as a philosophy to be contended with in the English-speaking world.

The most important of the Vienna Circle's philosophical allies on the Continent were the scientifically-minded philosophers and philosophically-minded scientists in the Berlin Society for Empirical Philosophy. Among the participants in this group were Hans Reichenbach, Walter Dubislav, Kurt Grelling, Carl Hempel, and

the psychologists Kurt Lewin and Wolfgang Köhler. The Berlin Society, headed by Reichenbach, was contemporaneous with the Vienna Circle and its members shared the same general philosophical orientation as that of the Circle. Their writings, however, lacked the strident denunciations of metaphysics that were characteristic of Vienna Circle writings, and it has been said that the members of the Berlin Society "had some reservations about the tendency of the Vienna Circle to form systems and set up prescriptions and prohibitions."²⁷

One reflection of the Berlin Society's more tolerant attitude toward scientific epistemology was Hans Reichenbach's strong emphasis on the roles of probability and induction in science. To be sure, the Berlin group included logicians--notably Carl Hempel--who followed the Vienna Circle strategy of relying on deductive logic in giving rational reconstructions of science. But Reichenbach viewed all empirical knowledge as hypothetical and probabilistic and therefore preferred to formulate his reconstructions in terms of probability and confirmation rather than logic and verification. Thus, for example, whereas the Vienna Circle members had claimed that two general propositions were identical in meaning when the same set of verifiable basic sentences could be deduced from them, Reichenbach held them to be identical when all possible observation sentences conferred the

same degree of probability on them.²⁸ In general, logic was the appropriate tool of analysis when scientific laws were construed as deterministic and nomothetic, and probability was the analytic tool of choice when laws were viewed as only probabilistic. As we shall see in Chapter 7, this difference between dominant approaches in Vienna and Berlin later surfaced in the debates in psychology over the proper formulation of psychological laws.

The Context of Discovery and the Context of Justification

As was discussed above, one of the basic assumptions of those who developed the new logic and advocated its application to science was that psychology was irrelevant to logic. In their view, the truths of logic and mathematics could achieve their necessary and universal status only by virtue of their independence from the psychological processes of those who devise and use them. To accept psychologism in any form was to relinquish the claim that logic and mathematics rest on a solid epistemological foundation and that they could in turn provide a firm footing for the sciences in which they are used.

But now in applying the techniques of the new logic to empirical science--something which Frege himself did not do--the logical positivists faced the problem of psychologism in a new form. Even granting that the formal

sciences could do without psychology by virtue of their explicitly stated axioms and rules of inference, the epistemology of the empirical sciences could hardly seem to avoid psychologism. After all, the empirical sciences are a product of empirical discoveries (some of them accidental), the psychological activities of observation and inference, the belief systems of individual scientists, and the collective and individual use of images, metaphors, and analogies. The logical positivists could not simply deny that such psychological processes do in fact enter into science. But, wanting to give empirical science a firm foundation, they attempted to neutralize the psychological dimensions of science by first isolating them from the realm of logic and validity and then by downplaying their importance.

The classic statement of the strategy used by the logical positivists was given in Reichenbach's Experience and Prediction (1938). Reichenbach cited the example of mathematicians and theoretical physicists. They may arrive at their claims as well as the proofs for them by a variety of relatively haphazard psychological processes, but in publishing their results they communicate them in a reconstructed form that makes their rational justification evident to the reader. Reichenbach wrote:

There is a great difference between the system of logical interconnections of thought and the actual way in which thinking processes are performed. The psychological operations of thinking are rather vague and fluctuating processes; they almost never keep to the ways prescribed by logic and may even skip whole groups of operations which would be needed for a complete exposition of the subject in question.²⁹

The activities of science, then, are divided into two contexts which Reichenbach called the context of discovery and the context of justification. The former includes the psychological factors and processes which enter into the discovery of scientific ideas. The latter includes all the factors involved in justifying a claim once it has been arrived at--the checking of inferences leading to and from it, the testing of the claim by empirical procedures, ascertainment of coherence between the claim and previously validated knowledge, and so on. Only the products of the context of justification can qualify as scientific knowledge. Implicit knowledge, intuitions, and any heuristic guides to discovery are ruled out of the realm not only of science but of epistemology in general. In this way, the logical positivists believed, all knowledge would necessarily be justified knowledge. Just as the elimination of psychologism from the formal sciences was to have ensured the objectivity and universality of logical and mathematical claims, so too would the laws of science be ensured an objective and universal status by the elimination of their

dependence on the psychological processes of scientists.

Although the important distinction between discovery and justification did not receive its canonical formulation until 1938, it had already for some time been a crucial distinction in logical positivist thought. In his celebrated tract Der logische Aufbau der Welt (1928), Carnap discussed the distinction as follows:

It must be possible to give a rational foundation for each scientific thesis, but this does not mean that such a thesis must always be discovered rationally, that is, through an exercise of the understanding alone. After all, the basic orientation and the direction of interests are not the results of deliberation, but are determined by emotions, drives, dispositions, and general living conditions. This does not only hold for philosophy but also for the most rational of sciences, namely physics and mathematics. The decisive factor is, however, that for the justification of a thesis the physicist does not cite irrational factors, but gives a purely empirical--rational justification.

Carnap went on to apply the distinction to philosophy itself:

We demand the same from ourselves in our philosophical work. The practical handling of philosophical problems and the discovery of their solutions does not have to be purely intellectual, but will always contain emotional elements and intuitive methods. The justification, however, has to take place before the forum of the understanding; here we must not refer to our intuition or emotional needs. We too, have "emotional needs" in philosophy, but they are filled by clarity of concepts, precision of methods, responsible theses, achievement through cooperation in which each individual plays his part.³⁰

In philosophy, as in science, the irrational and the merely psychological are screened out of knowledge when claims to knowledge are subjected to the demands of justification.

The place assigned to psychology in the logical positivist scheme was expressed even more clearly and forcefully in Carnap's later writings. Traditional philosophy was viewed as an admixture of metaphysical, logical, and psychological claims--a confusing hodge-podge which could be straightened out only by careful logical analysis. The metaphysical claims were, of course, removed from philosophy on the grounds of being non-sensical and placed with other purely emotive expressions, such as those of poetry. Turning to the consideration of psychology, Carnap wrote:

When we have eliminated metaphysical problems and doctrines from the region of knowledge or theory, there remain still two kinds of philosophical questions: psychological and logical. Now we shall eliminate the psychological questions also, not from the region of knowledge, but from philosophy. Then, finally, philosophy will be reduced to logic alone (in a wide sense of this word).³¹

This separation of psychology from philosophy was viewed as an especially important distinction to maintain in the realm of epistemology. As Reichenbach put it, psychology and epistemology have two different tasks, and "[m]any false objections and misunderstandings of modern epistemology have their source in not separating

these two tasks."³² The logical positivists' exclusion of psychology from epistemology was described by Carnap as follows:

Epistemology or theory of knowledge in its usual form contains both psychological and logical questions. The psychological questions here concern the procedure of knowledge, that is, the mental events by which we come to know something. If we surrender these questions to the psychologist for his empirical investigation, there remains the logical analysis of knowledge, or more precisely, the logical analysis of the examination and verification of assertions, because knowledge consists of positively verified assertions.³³

In other words, psychology addresses genuine questions but only in the context of discovery, i.e., "how we come to know something." But on the logical positivist view, the context of discovery is of no philosophical concern. Epistemology deals only with matters of justification and validity.

From the logical positivist perspective, this separation of psychology and epistemology was a natural distinction to draw. From the outset, they approached science first and foremost as a linguistic phenomenon--a domain to be analyzed by means of the "logical syntax of language." Validity was thereofre analyzed from a logical perspective. The behaviorists, on the other hand, viewed science from a psychological--and ultimately a Darwinian functionalist--perspective. As we shall see, their psychologies of science were concerned not only with scientific discovery but also with validity

in their own special Darwinian sense. For them, the "validity" of knowledge was a matter of the biological value of an outcome produced by some behavior executed under the guidance of that knowledge.

The Rational Reconstruction of Science

Once the logical positivists had purged epistemology of metaphysical and psychological questions, all that remained was the logical analysis of science. At this point, the logical positivists turned to Russell's replacement program: the fallible and subjective psychological processes involved in the actual attaining of knowledge were to be replaced by logical constructions by means of which the logical relations between the various concepts and claims of science would be exhibited. When in 1921 Carnap read Russell's appeal urging philosophers to adopt such an approach, he responded with enthusiasm. "I felt as if this appeal had been directed to me personally," he wrote. "To work in this spirit would be my task from now on!"³⁴ In 1928, Carnap published his Aufbau, a work which despite its flaws remains a monument in the logical empiricist tradition. In that work, Carnap attempted what he called a "rational reconstruction" (rationale Nachkonstruktion) of science, that is, a rigorous logical reconstruction of science in its logical and epistemological relationships. Although the rational

reconstruction was not meant to be a literal description of the psychological processes that enter into science, Carnap desired that it should nonetheless reflect actual epistemological processes. Accordingly, he chose to construct the system of concepts on a phenomenalist basis, somewhat along the lines of Mach's radical empiricist analysis of sensations.³⁵ As Carnap later described the enterprise:

The main motivation for my choice of a phenomenalist basis was the intention to represent not only the logical relations among the concepts but also the equally important epistemological relations. The system was intended to give, though not a description, still a rational reconstruction of the actual process of the formation of concepts.³⁶

Carnap's decision to have rational reconstructions reflect actual epistemological processes rather than to capture purely logical relations was a momentous one. As critics of the Aufbau have subsequently argued, a purely logical reconstruction of science is governed by different criteria of success than a partly epistemological reconstruction, and indeed the criteria may be at odds.³⁷ Carnap's decision was already a major concession to empiricism-- a concession which threatened to blur the sharp distinction between the empirical and the formal and which opened the door to considerations which would have earlier been condemned as psychologistic. As it turned out, Carnap never abandoned the analytic-synthetic distinction, but all of the major revisions of his views since the Aufbau

were justified by him, at least in part, by the claim that they provide a more accurate reflection of scientific practice.

If the general spirit of the logical positivists' rational reconstructions was taken from Russell, their basic distinctions and conceptions regarding science were derived from their reading of Wittgenstein's Tractatus. Twice during the 1920s, the Vienna Circle devoted extended periods to reading and discussing the Tractatus at its meetings.³⁸ Although the Circle members were sometimes put off by the obscurity of Wittgenstein's formulations and his tendency toward mysticism, they gradually worked out an interpretation of his views which appealed to them and greatly influenced their thinking in the late twenties and early thirties. In addition to the treatment of logical truths as empty tautologies (as described above), three other closely related notions were extracted from Wittgenstein's Tractatus and made into tenets of early logical positivism. These were: 1) the rejection of metaphysics on purely linguistic grounds; 2) the notion of a pure observation language consisting of basic propositions; and 3) the idea that non-analytic general propositions are verifiable in terms of the basic statements of the observation language.³⁹ Together with Wittgenstein's view of logical truth, these interpretations of his work were transformed into the familiar

three-fold classification of analytic, empirical, and non-sensical claims. The aim of rational reconstruction was to first purge science of any metaphysical elements, then to distinguish its analytic and empirical components, and to show the latter to be reducible to the elementary statements of the observation language. In what follows, each of these three tasks will briefly be considered.

Metaphysics as Linguistic Violations

In the famous final statement of the Tractatus, Wittgenstein wrote, "Whereof one cannot speak thereof one must be silent."⁴⁰ For him, this was an assertion about the limits of language and an affirmation of the existence of a mystical realm incapable of being described linguistically. But among the logical positivists, it was taken to be a denial of the intelligibility of metaphysics, rather than an acknowledgement of a metaphysical realm. That is, having insisted that all genuine knowledge is expressible in language (or at least in an ideal, logically purified language), they naturally viewed claims concerning the inexpressible as meaningless nonsense having no importance or cognitive significance. In their "linguistic turn," the logical positivists were giving positivism a novel twist. As A. J. Ayer stated it: "The originality of the logical positivists lay in their making the impossibility of metaphysics depend not upon

the nature of what could be known but upon the nature of what could be said."⁴¹

In the early stages of logical positivism, logical analysis was conceived to be a matter only of logical syntax, that is, of the forms of language, not of their meanings or semantic interpretations.⁴² This meant that all meaningful discourse fulfilled the rules of logical syntax and, conversely, that meaningless expressions violated those rules. In certain difficult cases, metaphysical claims appeared to be meaningful because they took on the superficial form of declarative sentences, but in each instance careful analysis could show them to be ill-formed formulations which violated syntactical constraints. Carnap cited the example of Martin Heidegger's claim that "The Nothing itself nothings."⁴³ Although this statement is superficially of a subject-predicate form, argued Carnap, it is countersyntactical because of its use of the word "nothing" as a noun. In its proper logical form, "nothing" is not a name but rather an abbreviation of a logical form, specifically a negative existential statement. Other metaphysical claims considered by Carnap included "Pure Being and pure Nothing are one and the same," "The principle of the world is pure water," and "God is." But logical positivist analyses were not limited only to the obviously problematic assertions of traditional metaphysics and theology. They equally

rejected some claims that were often associated with the scientific world-view. Thus, assertions that the external world is real, that the world consists of material entities and their interactions, and that experience consists of neutral sensations were all eschewed as meaningless, despite their general congeniality with the scientific temper.

The most important class of linguistic violations involved the non-analytic statements which could not be reduced to basic statements of the observation language. These were the claims which were eschewed on the grounds of their failure to meet the requirements of verifiability.

Verifiability and Its Variants

In the Tractatus, Wittgenstein spoke of atomic propositions which express or "picture" the most elemental facts of the world. Any empirical proposition that was not an atomic sentence was said by him to be a molecular proposition logically compounded from atomic ones. But the truth value of atomic sentences is immediately evident, and genuine molecular propositions--i.e., those which are syntactically well-formed--are truth functions of their constituent atomic sentences. This meant, at least to the logical positivists, that all genuine empirical claims are verifiable either directly or else indirectly by means of logical derivation of their

component elementary propositions. Wittgenstein himself did not advance a verifiability criterion, saying only that "[t]o understand a proposition means to know what is the case if it is true."⁴⁴ But in Wittgenstein's formulations, the members of the Vienna Circle saw a way to make explicit the traditional empiricist idea that genuine knowledge is grounded on experience.

In their empiricist rendering of Wittgenstein's views, the logical positivists embraced the notion of a basic observation language or pure data language. It would consist of sentences analogous to Wittgenstein's atomic propositions and would serve as the foundation of all empirical knowledge. The exact nature of these basic sentences was a topic of much debate in the Vienna Circle, and in their various interpretations they appeared as Schlick's "confirmations" (Konstatierungen), Neurath's "protocol statements" (Protokolsätze), and Ayer's "basic propositions."⁴⁵ Regardless of their precise characterization, they were assumed to stand in some clear logical relation to the more general statements which they supported.

Wittgenstein had claimed that for each genuine molecular proposition there would be one and only one "complete analysis" into its component atomic sentences.⁴⁶ Schlick and Friedrich Waismann--the two members of the Vienna Circle most strongly influenced by Wittgenstein--

gave early formulations of the verifiability principle which closely followed Wittgenstein's uniqueness claim. Thus, Waismann wrote in 1930, "A statement which cannot be verified conclusively is not verifiable at all; it is just devoid of any meaning."⁴⁷ In stating this strong criterion of verifiability, Waismann was modelling the general statements of science after Wittgenstein's construal of the molecular proposition "Everything is F" as a conjunction of atomic propositions about individuals being F. But Wittgenstein had advanced this notion only for an idealized language, and the extension of it to general scientific claims was clearly based on faulty analogizing. This was readily recognized by other logical positivists, who noted that the universal laws of science apply to an unlimited number of cases and cannot be considered logically equivalent to a finite number of observation sentences. Furthermore, a universal law can have as logical consequences an unlimited number of basic statements about the past and future, and certainly not all of these can be verified. The strong principle of verifiability thus ruled out scientific laws as meaningless--a highly unacceptable outcome for a philosophical movement which upheld scientific knowledge as the paradigmatic form of all knowledge.

Schlick attempted to evade the untoward consequences of the verifiability principle by giving a different inter-

pretation to scientific laws. On this new view, laws were taken to be, not descriptions of states of affairs, but inference rules for passing from certain basic statements to others. As such, they would presumably not need to be reducible to some one set of basic statements. However, as Carnap and others pointed out, the laws of science are routinely treated as descriptive statements which are subject to falsification; but it would make little sense to attempt to falsify a rule. For this and other reasons, Schlick's instrumentalistic interpretation of laws as inference rules was rejected.

By the mid-thirties, it was apparent that the early formulations of the verifiability principle were far too strict. The relationship between a general scientific claim and the empirical basis that assures it of a meaningful status was obviously more complicated and less direct than the relation of logical equivalence between the claim and a determinate set of basic statements. In what became a vast literature in the philosophy journals, the principle of verifiability was subjected to heated debate. Numerous revisions of it were advanced in the hope that each new formulation would answer the criticisms of previous versions. But each newly proposed criterion of meaningfulness seemed either to exclude scientific claims that would ordinarily appear to be acceptable or else to admit as meaningful claims that appeared to

be metaphysical. As the debates went on, two general changes took place: 1) there were more concessions to empiricism, both in the sense of a reduced role for logical deduction in the formulation of the relation between observation and knowledge claim and in the sense of that relation being tailored to reflect actual scientific practice; 2) the unit of analysis for cognitive significance shifted from the proposition to the concept. Both of these changes were revealed in Carnap's writings.

The first concession to empiricism was to acknowledge that, even though a general statement was still to be regarded as strictly equivalent to a set of observation statements, the equivalence between them need not be analytic. That is, the interderivability between them could be achieved by means of valid scientific laws (independent of the law in question) rather than by means of only the rules of logical syntax, as the Wittgenstein truth-functional model would have it. In his Philosophy and Logical Syntax (1935), Carnap remarked that the possibility of empirical, as opposed to formal, equivalences had not been taken sufficiently into account in previous explications of the criterion of meaningfulness.⁴⁸

In 1936-37, Carnap published his lengthy article on "Testability and Meaning"--a landmark in the liberalization of logical positivism.⁴⁹ Therein, he admitted that no general proposition of science can ever be con-

clusively verified. Such claims could, however, be tested and gradually confirmed in increasing degrees, and the meaningfulness of them would depend on this property. Carnap distinguished between testability and confirmability. A sentence was said to be testable if known procedures (e.g., particular experiments) were available which would confirm or disconfirm it in some degree. A sentence was said to be confirmable if it were possible to state what sort of evidence would confirm or disconfirm it, regardless of whether that evidence were obtainable through available procedures. Carnap advocated the weaker of these requirements, partial confirmability, as a new criterion of meaningfulness. This approach had the advantage of admitting scientific laws and hypotheses as meaningful assertions and it reflected scientific practice in which laws are held in varying degrees of belief; but it proved to be difficult to give a sound technical explication of the notion of confirmation. Carnap devoted much of the rest of his career to studies of the "logic" of confirmation. He also continued to revise the criterion of meaningfulness, giving his last, highly liberalized version of it in 1956.⁵⁰ Despite his continued efforts, and those of Carl Hempel and others, it is widely agreed that no fully satisfactory formulation of the empiricist criterion of meaningfulness was ever devised.⁵¹

Carnap's "Testability and Meaning" also contained important developments in regard to the problem of empirical definition. In a break from the Fregean tradition of giving judgments priority over concepts, Carnap began to consider the "observable predicate" as fundamental and to define the "confirmable sentence" in terms of the predicate or predicates occurring in it.⁵² Accordingly, the definability of terms began to take precedence over the confirmability of statements. Here, too, Carnap greatly liberalized the earlier formulations. In previous characterizations of empirical definitions, Carnap and others had emphasized the explicit definability of concepts. A concept was explicitly defined in terms of observations when the observations were asserted to be logically equivalent to the attribution of that concept to an entity. Thus, to say that person a is angry is to say that the person's body shows certain observable characteristics which are strictly equivalent to the anger. In symbolic notation (where F stands for "angry" and P for certain bodily states):

$$Fa \equiv Pa$$

But explicit definitions were open to two sorts of criticism. First, explicit definitions were rigid in the sense that they strictly identified the concept in question with a fixed set of manifestations and therefore could not be extended to new manifestations without

redefining the concept. But, it was argued, the flexibility of concepts was not only characteristic of their actual use in science but even a source of their utility.⁵³

Second, many concepts of science are dispositional in character and have no observable manifestations except under particular conditions. A concept of this sort, such as "solubility," cannot plausibly be identified with a set of previously determined observations.

In response to these points, Carnap proposed the notion of a "reduction sentence" or "conditional definition" which would in many cases take the place of an explicit definition. A dispositional predicate such as "soluble" (S) would be defined as follows: If an object (a) is placed in water (W), then it is soluble if and only if it dissolves (D). In notation:

$$Wa \Rightarrow (Sa \equiv Da)$$

Such a definition is conditional in the sense that it leaves the predicate S undefined unless the antecedent (Wa) is fulfilled. Therefore the predicate cannot in general be eliminated by replacing it with equivalent observation terms--a fact that has led some to question whether the reduction sentence can properly be considered as a definition at all.⁵⁴ In any case, reduction sentences were acknowledged to capture the open-ended character of scientific concepts since they do not pretend to exhaust the meaning of the concept. When new applications of the concept are discovered, new reduction sentences

could simply be added to the old. Each one is then said to provide a "partial interpretation" of the concept.

The logical positivists recognized that their views of verifiability and empirical definition had a certain affinity to the views of the pragmatists and operationists in America.⁵⁵ Pragmatism was founded by the American philosopher Charles S. Peirce, who anticipated its central theme in his 1878 paper "How to Make Our Ideas Clear" when he stated that "there is no distinction of meaning so fine as to consist in anything but a possible difference in practice."⁵⁶ Since Peirce tended to mean by "practice" the activities of scientists in their experimental investigations of nature, his statement can easily be read as saying that ideas or distinctions which have no counterpart in the manipulations or outcomes of scientific experimentation play no important role in knowledge. William James took up this general line of reasoning and assimilated it to his functionalist theory of thinking, according to which the value of ideas lies in the practical outcome of their use in satisfying the needs and interests of the individual. In his view, the techniques of scientific verification fulfill these same functions by providing laws which permit the prediction of the future and adjustment to the environment. Along similar lines, John Dewey characterized scientific theories as "leading principles" which have the practical consequences of guiding future inquiry and mediating activity in the world. Although

James actually used the pragmatic view of truth to justify religious belief (on the grounds that it had favorable outcomes), the general pragmatist notion that "a difference must make a difference to be a difference" was broadly congenial to the anti-metaphysical spirit of logical positivism.⁵⁷

Nevertheless, there were important differences between pragmatism and logical positivism, and these differences are of considerable relevance to the eventual relationship between behaviorism and logical positivism. Whereas logical positivism approached the problem of knowledge from a linguistic perspective, pragmatism tended to approach it from a biological perspective, especially once it had become closely associated with James's functionalism. In James's hands, pragmatism showed a special concern for the particulars of concrete experience and the demands placed on the individual in coping with the environment. Logical positivism, on the other hand, focused on the universal characteristics of knowledge and attempted to use the power and comprehensiveness of modern logic to formulate those characteristics in terms of the forms of language. But those differences which "make a difference" will not necessarily be the same in the contexts of pragmatism and logical positivism. In the logical reconstruction of science, the distinctions of traditional metaphysics made no difference, but in the struggle of daily living, James

could say, religious beliefs might well make a difference. Conversely, in the concrete activity of science the fine logical distinctions drawn by the logical positivists might make no difference. The scientist of a pragmatist bent would be likely to view such distinctions as mere "logic-chopping" which would only get in the way of the quotidian conduct of inquiry and the solution of specific problems. Indeed, as we shall see in the chapters that follow, some of the behaviorists who stood in the pragmatist tradition held just such views about the uses of logic.

A variation of pragmatism which was closer in spirit to the logical positivist notion of verifiability was the operationism of P. W. Bridgman. As articulated in his The Logic of Modern Physics (1927), Bridgman's principle of operationism stated that "We mean by any concept nothing more than a set of operations; the concept is synonymous with the corresponding set of operations."⁵⁸ Thus, the concept of length would be defined in terms of the specific operations used in arriving at it (e.g., the repeated placing of a measuring rod along some body). Concepts for which there were no corresponding operations were to be rejected as incompatible with empiricism. In a passage which closely resembled Carnap's treatment of "pseudo-problems," Bridgman went on to discuss "meaningless questions," that is, questions for which there were no opera-

tions for arriving at answers.⁵⁹ In much the same way that the logical positivists were denouncing metaphysics, Bridgman remarked that meaningless questions "poison" one's thought, not only in the realm of physics but also in social and philosophical matters.

The logical positivists quickly recognized Bridgman's operationism as a view closely allied to their own.⁶⁰ His statements seemed especially close to Shlick's early claim that "the meaning of a proposition is the method of its verification."⁶¹ But the similarities should not be overstated. For Schlick, "method" meant the logical technique of performing a Wittgensteinian truth-functional analysis. For Bridgman, an operation was a concrete activity performed by a practicing scientist. Operationism also differed from verificationism in generally concerning itself with the empirical sense of concepts rather than propositions. This feature of operationism, along with its emphasis on the availability of actual operations for assessing the applicability of a concept, led Carnap to view the "principle of operationism" as approximately equivalent to the requirement of testability.⁶² But even this interpretation distorts the fundamental character of operationism by forcing it into the mold of analytic philosophy. Testability was nominally a thesis--in the context of the rational reconstruction of science--about the linguistic form of the logical relationship between

basic statements and meaningful concepts. In Bridgman's view, operationism was not a linguistic thesis nor was it a "principle"--rather it was an "attitude toward concepts" and a "point of view."⁶³ That is, it was a practice engaged in by scientists in the course of dealing with their subject matter.

The Structure of Theories: Empirical and Formal Components

One of the recognized aims of the rational reconstruction of science was to clarify and distinguish between the empirical and formal components of science. Having drawn a sharp distinction between analytic and synthetic claims, the logical positivists were forced to trace that distinction into the complex logical structure of theories. Giving a plausible formal account of theories which maintained all the desired distinctions proved to be no easy task. In parallel with the repeated revisions of the logical positivist criterion of meaningfulness, the logical positivist view of the structure of theory underwent many reformulations as it evolved into what has become known as the "received view" of theories.⁶⁴

On the received view, a theory was characterized as a linguistic structure having a kind of hierarchical nature. At the top of the structure was a purely formal set of axioms. In the manner of Hilbert and Schlick, the axioms themselves were taken to be an "uninterpreted system" or a "formal calculus" which provided implicit

definitions of the high-level theoretical concepts. As in Poincaré's conventionalism, these axioms are simply to be chosen on some basis and are not derived from experience. Just below the axioms in the hierarchy are the various theorems--also part of the formal calculus--which are derivable from the axioms on the basis of logic alone. At the base of the theoretical structure lie the basic statements expressing pure observations. From these, the empirical concepts and low-level laws of science are constructed by means of empirical definitions, whether they be in the form of explicit definitions or some form of reduction sentence. At this point, the formal and empirical components are, at least in principle, distinct. With the appropriate selections of correspondence rules (or coordinating definitions), the empirical and formal levels can be joined together, making the theoretical structure a continuous hierarchy. This permits the postulate system, which "'floats' or 'hovers' freely above the plane of empirical facts," to partake of empirical content. The whole system is said to be infused with significance by virtue of an "upward seepage" of empirical meaning from the data base to the theoretical terms.⁶⁵

In this scheme, a considerable burden is placed on the correspondence rules since they provide the link between what are, at least putatively, the purely formal and purely empirical components of the structure. Not

surprisingly, the epistemological status of the correspondence rules has been the subject of considerable controversy. They have most commonly been construed as analytic propositions or as conventions regarding the use of terminology. But they have also been alleged to have empirical content, especially in light of the historical observation that they sometimes change as a consequence of empirical research.⁶⁶ All of this means that the traditional logical empiricist distinction--one necessitated by the analytic-synthetic distinction--between the observation language and the theoretical language is called into question. It has been acknowledged that actual scientific theories seem not to embody any such sharp distinction. Recognizing this difficulty, Carnap has attempted to evade the issue by suggesting that the theory-observation distinction be regarded as a matter of convention rather than a question of empirical fact.⁶⁷

Physicalism and the Unity of Science

The Physicalist Doctrine

In the Aufbau, Carnap had adopted a phenomenalist data base from which to erect his logical constructions. In doing so, he was following the tradition of Mach, who believed that all science is based on the experience of neutral sensations. Since the special sciences differ

only in how they organize those sensations, according to Mach, experience could provide the basis for the unification of science. All members of the Vienna Circle shared Mach's basic goal of unifying science, but the Marxist sociologist Otto Neurath came to dispute the desirability of a phenomenistic data base. He felt that it was dangerously close to the idealistic metaphysics which predominated in Germany at the time and which in his view tended to counteract social progress. He further feared the phenomenist language would reinforce the popular distinction in German thought between natural sciences and Geisteswissenschaften (social sciences), a distinction which he regarded as a barrier to the extension of logical positivist methods to the social sciences. Around this time, Karl Popper, who was not himself a member of the Vienna Circle but frequently interacted with its members, was emphasizing that no sentence of science can be regarded as irrevocably true. If even observation sentences are falsifiable, they must be expressed in a language that admits of intersubjectivity rather than a phenomenistic language.

Under the influence of Neurath and Popper most members of the Vienna Circle had by 1930 adopted physicalist language as the universal language of science. Physicalism was widely regarded by members of the Circle as a recommendation for a language in which to formulate a data base

rather than as an assertion about reality. Because of their intersubjective nature, observation statements such as "this thing is black and heavy" were to be recommended over statements such as "there is now a red triangle in my visual field." Neurath even went so far as to argue that the observation statements (Protokolsätze) should be formulated in the third person. An observation reported by Neurath himself would thus be expressed in sentences such as "Otto now sees a red circle" or "Otto now joy."⁶⁸ The behavioristic character of such basic sentences was a reflection of Neurath's philosophical motives and did not in itself constitute an endorsement of psychological versions of behaviorism. Neurath was, in fact, sympathetic to psychological behaviorism, but as we shall see below his insistence on a physicalist data language was independent of psychological concerns.

In regard to the purely linguistic character of the physicalist doctrine, Carnap in particular was quick to emphasize that physicalism was not a version of materialism--physicalism, like his earlier phenomenism, carried no ontological commitment. As always, sentences making ontological claims were to be eschewed as devoid of cognitive meaning. The choice of one data language over another, in Carnap's view, could be made at will or according to pragmatic concerns. In other words, the choice was a matter of adopting a convention, a notion

that Carnap later made explicit in his "principle of tolerance."⁶⁹ The sole requirement was that different languages not be carelessly mixed, for to do so was to invite the metaphysical perplexities associated with such pseudo-problems as the mind-body issue. The linguistic neutrality licensed by the principle of tolerance was reminiscent of Mach's neutral monism. But neutral monism was itself rejected as meaningless metaphysics by the logical positivists. As Neurath put it:

It would be misleading to express the physicalist thesis by saying that the distinction of "psychical" and "corporeal" no longer existed, but had been replaced by "something neutral." It is not at all a question of "something," but simply of correlations of a physical character.⁷⁰

The adoption of physicalism as the basis of unified science turned the attention of logical positivists toward psychology because it necessitated the problematic requirement that every sentence of psychology be capable of formulation in physical language. Carnap's "Psychology in Physical Language" appeared in Erkenntnis in 1932, just a year after his first published proclamation of physicalism.⁷¹ The essay self-consciously called for openness of mind on the part of the reader to help overcome "emotional resistance" to the advent of physicalism. Carnap's thesis is captured in the following passage:

We are not demanding that psychology formulate each of its sentences in physical terminology. For its own purposes, psychology may, as heretofore, utilize its own terminology. All that we are demanding is the production of the definitions through which psychological language is linked with physical language. We maintain that these definitions can be produced, since, implicitly, they already underlie psychological practice.⁷²

Carnap went on in the paper to specify the form of these definitions as follows: for every sentence P in psychological language there must be a sentence Q in physical language such that P and Q can be logically deduced from each other. This requirement of explicit definability was, as we have seen, soon thereafter dropped by Carnap in favor of the weaker relations of confirmability and reducibility. He later viewed reduction sentences as eminently suited to the definition of psychological disposition predicates.

In 1935, the physicalist thesis involving Carnap's original strict formulation was dubbed "logical behaviorism" by Carl Hempel. Like Carnap, Hempel emphasized that physicalism would place no restrictions on the subject matter of psychology. "Logical behaviorism claims neither that minds, feelings, inferiority complexes, voluntary actions, etc., do not exist, nor that their existence is in the least doubtful."⁷³ The linguistic or logical, rather than empirical, character of physicalism was exhibited most strikingly by Hempel's assertion that

physicalism is a "logical theory about the propositions of scientific psychology" which "seeks to show that if in psychology only physicalistic statements are made, this is not a limitation because it is logically impossible to do otherwise."⁷⁴ In a similar vein, Neurath stated that "it is a matter of indifference for the position here maintained whether certain individual theses of Watson's, Pavlov's or others are upheld or rejected."⁷⁵ The intersubjective nature of the physicalist data base thus made its adoption a desideratum, or even a necessity, for psychology quite aside from the success of specific behaviorist programs.

In marked contrast to the views of Carnap, Hempel, and Neurath, Moritz Schlick claimed that physicalism was indeed an empirical thesis. According to him, the universal and intersubjective qualities of the physicalist language arise from contingent features of the world; the fact that psychological or phenomenistic language seems to lack these properties is likewise contingent. Schlick advocated that physicalism be treated "as a paradigm, as one possibility among others" rather than as a philosophical movement.⁷⁶ In his realist tendencies Schlick diverged from the general stance of the Circle, despite being recognized as its personal center.

The Logical Positivists and Scientific Behaviorism

The fact that the logical positivists adopted logical behaviorism leaves entirely open the question of their relationship with scientific behaviorism of the sort that was being practiced by American behaviorists. Being nothing more than the extension of the physicalist doctrine to psychology, logical behaviorism was a linguistic thesis or theory of meaning, not an approach to scientific psychology. Although practicing behaviorists sometimes offered behavioral definitions of mentalistic terms, their doing so was by no means an essential part of scientific behaviorism. Logical and scientific behaviorism were thus rather different enterprises with differing aims and differing methods.

Nevertheless, the logical positivists showed an interest in behaviorism even during the 1920s. In reading Russell's Analysis of Mind (1921), the members of the Vienna Circle first became acquainted with Watsonian behaviorism, and several of them subsequently read Watson's works.⁷⁷ Scattered references to Watson and behaviorism soon began to appear in their writings.⁷⁸ Watson's polemicizing for science and against metaphysics no doubt struck a sympathetic chord in the Circle, and once physicalism had been proclaimed, the names of Watson and Pavlov could be invoked in support of the

physicalist thesis. This was so despite the fact that, strictly speaking, the achievements of Watson and Pavlov were irrelevant to the legitimacy of adopting the physicalist language. The logical positivists perceived that the apparent implausibility of a physicalist treatment of psychology would be an obstacle--a source of "emotional resistance"--to the acceptance of the doctrine, and they were prepared to make propagandistic use of Watson's and Pavlov's names.

There was, however, one area of genuine common ground shared by the logical positivists and the early behaviorists. Both groups held a primitive epistemological assumption to the effect that certainty of knowledge is to be sought by reducing the objects of knowledge to their atomic forms. In philosophy, this meant reducing laws to basic statements and in psychology it meant the reduction of observable behavior to physiology. Donald Campbell has written:

Both psychology and philosophy are emerging from an epoch in which the quest for punctiform certainty seemed the optimal approach to knowledge. To both Pavlov and Watson, single retinal cell activations and single muscle activations seemed more certainly reidentifiable and specifiable than perceptions of objects or adaptive acts. The effort in epistemology to remove equivocality by founding knowledge on particulate sense data and the spirit of logical atomism point to the same search for certainty in particulars.⁷⁹

By the 1930s, when the extension of physicalism to psychology was being actively pursued, the logical positivists were relying on physiological reduction in their definitions of psychological concepts in terms of physicalist language. They routinely appealed to states of the nervous system, for example, in their definitions of "anger."⁸⁰ To be sure, reference was sometimes made to overt behavior, but the translations of psychological predicates into physicalist language usually resorted eventually to physiology--especially in those cases in which the predicates appeared to have no reliable manifestations in overt behavior.

Interestingly enough, this reductionistic strategy was already outmoded by the early thirties. Although the logical positivists were unaware of it until near the end of the decade,⁸¹ behaviorism had undergone a major transformation. As will be described in the following chapters, the "molecular" or physiological behaviorism of the classical behaviorists had been superseded by the "molar" behaviorism of the neobehaviorists. This development had many far-reaching implications. The most significant of them in the present context is that the phenomena of molar behavior were shown to be just as law-like as the phenomena of molecular behavior, and sometimes even more so. As a result, an appeal to the laws of molar behavior in defining psychological predicates would have

been more justifiable than an appeal to laws relating those predicates to physiology--laws which neither Watson nor Pavlov had even come close to discovering.

Sophisticated versions of molar behaviorism were already being formulated in 1932 when Carnap wrote "Psychology in Physical Language." Yet among the examples he gave of psychological definitions in terms of gross behavior was an extended example of character analysis through handwriting. The only supporting reference he gave was to a book on graphology that had been published in 1920. If the logical positivists were not exactly abreast of relevant developments in psychology, it is only fair to point out that the behaviorists equally failed to keep up with developments in logical positivism. As will be argued in what follows, the relationship between behaviorists and logical positivists rested more on mutual support in matters of polemic and rhetoric than it did on genuine intellectual understanding.

Unity of Science

With the advent of physicalism, the logical positivists believed they had finally found the last essential ingredient for realizing the long-held dream of the unification of science. Leibniz's dream of unification had been thwarted by the lack of an adequate symbolic logic; but now the new logic was available. On the

empiricist side, Mach's version of that dream had been flawed by the solipsism inherent in his phenomenism; but now the physicalist language was believed to provide an intersubjective empirical foundation of knowledge. On the twin foundations of the new logic and a refined data language, the erection of an integrated system of the sciences was thought to be not only possible but perhaps even inevitable given sufficient time.

In the final system, the unity of science would be effected on three distinct but related fronts. The first would be the unity of all scientific concepts. This would amount to no more than showing that all concepts are reducible, by one means or another, to the physicalist observation language. All concepts in unified science would thereby be assured of empirical significance. The adequacy of the physicalist language for this purpose was taken to be established. As Carnap wrote, "there is a unity of language in science, viz., a common reduction basis for the terms of all branches of science. . . ." ⁸²

The second kind of unity was to be the unity of scientific laws. On this conception, Carnap wrote, "The construction of one homogeneous system of laws for the whole of science is one aim for the future development of science." ⁸³ The scenario was as follows: Since all the concepts of the various sciences belong to a common physicalist language, they can be compared and connected

by means of logical analysis. Certain laws can then be shown to be logically derivable from others, and gradually there would emerge a deductive hierarchy in which the laws of psychology and social science are reduced to those of biology, and the laws of biology are in turn reduced to those of physics and chemistry. The unity of laws was acknowledged not to be an accomplished fact, but Carnap noted that the achievement of partial reductions of biology to chemistry was a hopeful sign of future prospects.

The third type of unity of science was unity of method. Given the received view of the logical structure of theories, the unity of method was quite naturally conceived as based on hypothetico-deductive method. Hempel has characterized this type of unity:

The thesis of the methodological unity of sciences states, first of all, that notwithstanding many differences in their techniques of investigation, all branches of empirical science test and support their statements in basically the same manner, namely by deriving from them implications that can be checked intersubjectively and by performing for those implications the appropriate experimental or observational tests. This, the unity of method thesis holds, is true also of psychology and the social and historical disciplines.⁸⁴

Although logical positivists disclaimed any intention of accounting for the genesis of knowledge in the context of discovery, many of them did not hesitate to offer one or another version of the hypothetico-deductive procedure

as the appropriate method for assessing all knowledge within the context of justification. The claim of methodological unity has been justly attacked as both an inadequate description of actual scientific practice and as an overly restrictive prescription for practice--especially when applied to the less advanced sciences. Of the behaviorists considered in the present work, only Hull was receptive to hypothetico-deductive method, and even so he interpreted it in an unorthodox way.

In addition to the unities of concepts, laws, and method, the logical positivists advocated a unity of science at the social level. Following Comte, they believed that the elimination of metaphysics and the acceptance of the scientific frame of mind would lead to social cooperation in the furthering of scientific aims. Neurath's slogan "Einheitswissenschaft--ohne Emotion" (unity of science, without emotion) captured the spirit of their thinking. Without the emotion engendered by needless metaphysics, the world would supposedly be set to accept the scientific world view and to work to promote it. With Neurath as its primary organizer, a Unity of Science movement was formed to advance these ideals. The movement held a series of International Congresses for the Unity of Science and eventually published a series of monographs under the general title of the International Encyclopedia of Unified Science. The Congresses were

held at Paris (1935), Cambridge, England (1938), Cambridge, Massachusetts (1939), Chicago (1943), and Berkeley (1953). As we shall see, efforts were made to recruit objectivistic psychologists into the movement--and among those recruited were Tolman and Hull.

Conclusion

Logical positivism arose as the joint product of two intellectual traditions which conflicted deeply with one another. In attempting to unite these traditions, its adherents created an extremely influential approach to philosophy but one that embodied serious intellectual tensions from its dual ancestry. The blending of Fregean logicism and Machian empiricism was an unstable philosophical position. The sharp dichotomies inherited from the rationalist side began to blur as concessions were made to the empiricist side. Some of the proponents of logical positivism gradually surrendered the analytic-synthetic distinction as well as the family of distinctions that went with it. Carnap was not among those willing to relinquish the analytic-synthetic distinction, and as early as 1940 he found himself lamenting its abandonment with the query: "are we now back with John Stuart Mill?"⁸⁵

In his famous critique of logical positivism, "Two Dogmas of Empiricism" (1951), Quine argued from a pragmatist perspective that the analytic or synthetic character

of a proposition is only a matter of degree. But, in fact, he had already argued in 1936 that the distinction could not be absolute because in the actual world one acts on beliefs only with degrees of certitude.⁸⁶ Here was a Fregean distinction being relativized on naturalistic--or worse yet, psychologistic--grounds. Since then, epistemology in general and the philosophy of science in particular have moved steadily away from a reliance on logic and toward a reliance on empirical analysis of science, whether of a historical, sociological, or psychological nature. Philosophers of science are seriously entertaining the possibility that their discipline will actually become the psychology of science. Even Hempel has recently remarked that he is "now putting psychology and logic closer together."⁸⁷ The pendulum of philosophical opinion seems to be swinging back toward psychologism for the first time in this century.

As will be shown in the following chapters, the major neobehaviorists developed their own psychologistic accounts of science and, in doing so, anticipated some aspects of current epistemological trends. Preferring to subordinate logic to psychology, they were all along more empiricistic than the logical positivists. In this regard, they were not unlike nineteenth century proponents of psychologism. But what made this psychologism unique was that it was a behavioristic psychologism. If

psychology could be made an objective science, there would no longer be any reason to reject psychologism on the grounds of subjectivism. And certainly behaviorists have believed above all else that behaviorism could make psychology objective.

Notes for Chapter 2

1. Although all the figures mentioned in this paragraph had significant roles in the activities of the Vienna Circle, it should be borne in mind that they were connected with the Circle in various degrees and during differing years. In particular, Kelson's affiliation with the Circle was a rather loose one. For a listing of figures associated with the Vienna Circle, see Herbert Feigl, "Logical Empiricism," in Twentieth Century Philosophy, ed. D. D. Runes (New York: Philosophical Library, 1943), pp. 371-416, on pp. 406-408.

2. The phrase "logical positivism" was coined in 1931 by Herbert Feigl and Albert E. Blumberg, "Logical Positivism, a New Movement in European Philosophy," Journal of Philosophy 28 (1931): 281-296. By the mid-thirties, some adherents of logical positivism were expressing preference for the designation "logical empiricism." Currently, the two phrases are widely regarded as equivalent, although some philosophers reserve the former to refer to the earlier strict version of the Vienna Circle philosophy and the latter to refer to later liberalized versions. One has even dated the transition between logical positivism and logical empiricism as having taken place with the publication of Carnap's 1936-37 paper "Testability and Meaning" (see Harold I. Brown, Perception, Theory and Commitment: The New Philosophy of Science Chicago: University of Chicago Press, Phoenix Books, 1979, p. 23). In the present work, the two expressions will simply be used interchangeably.

3. Richard Rorty (ed.), The Linguistic Turn: Recent Essays in Philosophical Method (Chicago: University of Chicago Press, 1967).

4. The Logical innovations of Frege and his immediate predecessors have been described by John Passmore, A Hundred Years of Philosophy (Harmondsworth, England: Penguin Books, 1966), pp. 120-155, and in greater detail by Hans Sluga, Gottlob Frege (London: Routledge & Kegan Paul, 1980).

5. Bertrand Russell, Outline of Philosophy (New York: World Publishing, 1960), p. 3. Originally published as Philosophy (New York: Norton, 1927).

6. Rudolf Carnap, "The Old and the New Logic," in Logical Positivism, ed. A. J. Ayer (New York: Free Press, 1959), pp. 133-146, on pp. 137, 134. Originally published as "Die alte und die neue Logik," Erkenntnis 1 (1930): 12-26.

7. See Sluga, Gottlob Frege, p. 64. Well before logical positivism was born as a philosophical movement, Bertrand Russell was working under this conception of logic. In fact, Russell wrote his doctoral dissertation on Leibniz. See Bertrand Russell, A Critical Exposition of the Philosophy of Leibniz (London: G. Allen & Unwin, 1958). Originally published in 1900.

8. Sluga, Gottlob Frege, pp. 35-41.

9. Gottlob Frege, Foundations of Arithmetic: A Logic-Mathematical Enquiry into the Concept of Number, tran. J. L. Austin (Oxford: Basil Blackwell, 1959), p. vi. Original German edition published in 1884.

10. Sluga, Gottlob Frege, pp. 39-40. Sluga notes that these remarks were made in response to Edmund Husserl's psychologistic views of the early 1890s and that they may have been instrumental in leading Husserl to abandon his psychologism in favor of the anti-psychologism for which he is currently better known.

11. W. C. Kneale, "Gottlob Frege and Mathematical Logic," in The Revolution in Philosophy, ed. A. J. Ayer (London: Macmillan, 1960), p. 31.

12. David Hume, An Enquiry Concerning Human Understanding (La Salle, Ill.: Open Court, 1963), p. 184. Originally published in 1748.

13. See, for example, Philipp Frank's statement that Mach "may be considered one of the spiritual ancestors of the Unity of Science movement and, particularly, the real master of the Vienna Circle" (Modern Science and Its Philosophy [Cambridge: Harvard University Press, 1949], p. 79). See also Julius R. Weinberg, An Examination of Logical Positivism (Paterson, N.J.: Littlefield, Adams, 1960), p. 9 and The Encyclopedia of Philosophy, s.v. "Logical Positivism," by John Passmore.

14. In Chapter 9, Mach's psychologistic epistemology will be discussed in considerable detail in connection with Skinner's behavioral epistemology.

15. This characterization of Mach's view of logic was given by one of Mach's associates, Wilhelm Jerusalem, in his Introduction to Philosophy, tran. Charles F. Sanders (New York: Macmillan, 1910), p. 51.

16. Sluga, Gottlob Frege, p. 39.

17. For a brief account of Poincaré's conventionalism, see The Encyclopedia of Philosophy, s.v. "Henri Poincaré," by Peter Alexander.

18. Moritz Schlick, General Theory of Knowledge, tran. Albert E. Blumberg (New York: Springer-Verlag, 1974), p. 33. Originally published as Allgemeine Erkenntnislehre in 1918.

19. *Ibid.*, p. 71.

20. Bertrand Russell, "Logical Atomism," in Logical Positivism, ed. A. J. Ayer (New York: Free Press, 1959), pp. 31-50, on pp. 36-37. Russell's paper was originally published in 1924.

21. *Ibid.*, p. 37. This general notion was developed at length in Russell's Analysis of Mind (London: George Allen & Unwin, 1921).

22. Ludwig Wittgenstein, Tractatus Logico-Philosophicus, tran. D. F. Pears and B. F. McGuinness (London: Routledge & Kegan Paul, 1961). This work first appeared in German in 1921 and in English in 1922; it was written in 1918.

23. The similarities and differences between the atomisms of Hume and Wittgenstein are concisely spelled out in Brown, Perception, Theory and Commitment, Chapter 1.

24. Sluga, Gottlob Frege, p. 186.

25. Schlick, who was an individualist in matters of philosophy, is reported to have been somewhat dismayed at the prospect of being cast as the leader of a philosophical movement. See Herbert Feigl, Inquiries and Provocations: Selected Writings 1929-1974, ed. Robert S. Cohen (Dordrecht, Holland: D. Reidel, 1981), p. 22.

26. "Wissenschaftliche Weltauffassung," reprinted in English translation in Otto Neurath's Empiricism and Sociology, ed. Marie Neurath and Robert S. Cohen (Dordrecht, Holland: D. Reidel, 1973), pp. 299-318 on p. 306.

27. Joergen Joergensen, "The Development of Logical Empiricism," in Foundations of the Unity of Science, ed. Otto Neurath, Rudolf Carnap, and Charles Morris (Chicago: University of Chicago Press, 1970), pp. 845-946, on p. 895.

28. Hans Reichenbach, Experience and Prediction (Chicago: University of Chicago Press, 1938, Phoenix Books, 1961), p. 54.
29. Ibid., p. 5.
30. Rudolf Carnap, The Logical Structure of the World, tran. Rolf A. George (Berkeley: University of California Press, 1969), p. xvii.
31. Rudolf Carnap, Philosophy and Logical Syntax (London: Kegan Paul, Trench, Trubner, 1935), p. 31.
32. Reichenbach, Experience and Prediction, p. 6.
33. Carnap, Philosophy and Logical Syntax, p. 83.
34. Rudolf Carnap, "Intellectual Autobiography," In The Philosophy of Rudolf Carnap, ed. Paul A. Schilpp (La Salle, Ill.: Open Court, 1963), pp. 1-84, on p. 13. The work of Russell's that Carnap was reacting to was Bertrand Russell, Our Knowledge of the External World as a Field for Scientific Method in Philosophy (New York: Norton, 1929). Originally published in 1914.
35. Actually the elements chosen by Carnap were the "total instantaneous experiences" suggested by Gestalt psychology. See Carnap, "Intellectual Autobiography," pp. 16-17.
36. Ibid., p. 18.
37. See, for example, Nelson Goodman, "The Significance of Der logische Aufbau der Welt," in The Philosophy of Rudolf Carnap, ed. Paul A. Schilpp (La Salle, Ill.: Open Court, 1963), pp. 545-558.
38. Herbert Feigl, Inquiries, pp. 60-63.
39. The fact that these notions were attributed to Wittgenstein by the logical positivists does not necessarily mean that Wittgenstein himself held them. Indeed, efforts to interpret Wittgenstein's writings have always been controversial, in large part because of the enigmatic quality of his thought. See The Encyclopedia of Philosophy, s.v. "Ludwig Wittgenstein," by Norman Malcolm.
40. Wittgenstein, Tractatus, p. 150 (the English translation given on the subsequent page differs slightly from the one given here, which is the more traditional rendering).

41. A. J. Ayer, "Editor's Introduction," in Logical Positivism (New York: Free Press, 1959), pp. 3-28, on p. 11.
42. Carnap's later admission of semantics into philosophical discourse was largely due to the influence of the Polish logician Alfred Tarski. See Carnap, "Intellectual Autobiography," pp. 60-62.
43. Rudolf Carnap, "The Elimination of Metaphysics Through Logical Analysis of Language," in Logical Positivism, ed. A. J. Ayer (New York: Free Press, 1959), pp. 60-81. Carnap's article was originally published in 1932.
44. Wittgenstein, Tractatus, p. 41.
45. For a useful review of the various interpretations of basic sentences, see The Encyclopedia of Philosophy, s.v. "Basic Statements," by R. W. Ashby.
46. Wittgenstein, Tractatus, p. 25.
47. Friedrich Waismann, "Logische Analyse des Wahrscheinlichkeitsbegriffs," Erkenntnis 1 (1930). Quoted in English translation in The Encyclopedia of Philosophy, s.v. "Basic Statements," by R. W. Ashby.
48. Carnap, Philosophy and Logical Syntax, pp. 90-91.
49. Rudolf Carnap, "Testability and Meaning," Philosophy of Science 3 (1936): 419-471; 4 (1937): 1-40. Also reprinted with omissions in Readings in the Philosophy of Science, ed. Herbert Feigl and May Brodbeck (New York: Appleton-Century-Crofts, 1953), pp. 47-92. Subsequent references are to this reprinted version.
50. Rudolf Carnap, "The Methodological Character of Theoretical Concepts," in The Foundations of Science and the Concepts of Psychology and Psychoanalysis, ed. Herbert Feigl and Michael Scriven, Minnesota Studies in the Philosophy of Science, vol. 1 (Minneapolis: University of Minnesota Press, 1956), pp. 38-76.
51. See, for example, Carl Hempel's admission to this effect in "Empiricist Criterion of Cognitive Significance: Problems and Changes," in Aspects of Scientific Explanation and Other Essays in the Philosophy of Science (New York: Free Press, 1965), pp. 101-133.

52. Carnap, "Testability and Meaning," pp. 63-65.
53. This argument, which has been advanced by F. P. Ramsey and R. B. Braithwaite, is discussed in Brown, Perception, Theory and Commitment, pp. 38-39.
54. See Ibid., p. 42; Goodman, "Significance of Aufbau," p. 556.
55. See, for example, Feigl, Inquiries, pp. 69, 83.
56. Quoted in The Encyclopedia of Philosophy, s.v. "Pragmatic Theory of Truth," by Gertrude Ezorsky.
57. Feigl, Inquiries, p. 13.
58. P. W. Bridgman, The Logical of Modern Physics (New York: Macmillan, 1927), p. 5.
59. Ibid., pp. 28-31. Bridgman's notion of "meaningless questions" was apparently developed independently of Carnap's "pseudoproblems." On the latter notion, see Rudolf Carnap, Scheinprobleme in der Philosophie: Das Fremdpsychische und der Realismusstreit (Berlin-Schlachtensee: Weltkreis-Verlag, 1928). Reprinted in English translation in Carnap, Logical Structure of the World (1969).
60. In fact, Feigl went to Harvard in 1930 in order to study with Bridgman.
61. Moritz Schlick, "Form and Content, an Introduction to Philosophical Thinking," in Gesammelte Aufsätze, 1926-1936 (Vienna: Gerold, 1938), pp. 151-336, on p. 181. This long essay was delivered as three lectures at the University of London in 1932.
62. Carnap, "Intellectual Autobiography," p. 59.
63. Bridgman, Logic of Modern Physics, pp. 3, 31.
64. For a detailed statement of the "received view," see Frederick Suppe, "The Search for Philosophic Understanding of Scientific Theories," in The Structure of Scientific Theories, ed. Frederick Suppe (Urbana, Ill.: University of Illinois Press, 1977), pp. 1-232.
65. See Brown, Perception, Theory and Commitment, pp. 46-49.
66. Ibid., p. 48.

67. See Suppe, "Scientific Theories," p. 47.

68. Otto Neurath, "Protocol Sentences," in Logical Positivism, ed. A. J. Ayer (New York: Free Press, 1959), pp. 199-208, on p. 202. Protocol sentences, in Neurath's view, are actually somewhat more complicated than these examples, because they must also include information about time and place of their utterance.

69. Rudolf Carnap, The Logical Syntax of Language (London: Routledge and Kegan Paul, 1937), pp. 51-52.

70. Otto Neurath, "Sociology and Physicalism," in Logical Positivism, ed. A. J. Ayer (New York: Free Press, 1959), pp. 282-317, on p. 299. This article was originally published as "Soziologie im Physikalismus," Erkenntnis 2 (1931): 393-431.

71. The proclamation was his "Die Physikalische Sprache als Universalsprache der Wissenschaft," Erkenntnis 2 (1931): 432-465.

72. Rudolf Carnap, "Psychology in Physical Language," in Logical Positivism, ed. A. J. Ayer (New York: Free Press, 1959), pp. 165-198, on p. 167. The paper was originally published as "Psychologie in physikalischer Sprache," Erkenntnis 3 (1932): 107-142.

73. Carl G. Hempel, "The Logical Analysis of Psychology," in Readings in Philosophical Analysis, ed. Herbert Feigl and Wilfred Sellars (New York: Appleton-Century-Crofts, 1949), pp. 373-384, on p. 381. This paper was originally published as "Analyse Logique de la Psychologie," Revue de Synthèse 10 (1935): 27-42.

74. Ibid., p. 381.

75. Neurath, "Sociology and Physicalism," p. 298.

76. Moritz Schlick, "On the Relation Between Psychological and Physical Concepts," in Readings in Philosophical Analysis, ed. Herbert Feigl and Wilfred Sellars (New York: Appleton-Century-Crofts, 1949), pp. 393-407, on p. 399. This article originally appeared as "De la Relation Entre les Notions Psychologiques et les Notions Physicques," Revue de Synthèse 10 (1935): 5-26.

77. Herbert Feigl to Laurence D. Smith, 26 July 1977.

78. See, for example, Carnap, Logical Structure of the World, p. 96; Neurath, "Sociology and Physicalism," p. 298.

79. Donald T. Campbell, "Pattern Matching as an Essential in Distal Knowing," in The Psychology of Egon Brunswik, ed. Kenneth R. Hammond (New York: Holt, Rinehart and Winston, 1966), pp. 81-106, on p. 82.

80. See, for example, Carnap, "Psychology in Physical Language," pp. 172-173.

81. The first published indication of any awareness of developments in neobehaviorism on the part of a logical positivist is in Rudolf Carnap, "Logical Foundations of the Unity of Science," in International Encyclopedia of Unified Science, vol. 1., no. 1, ed. Otto Neurath, Rudolf Carnap, and Charles Morris (Chicago: University of Chicago Press, 1938), pp. 42-62, on pp. 48, 56.

82. Ibid., p. 61.

83. Ibid., p. 61.

84. Carl G. Hempel, "Logical Positivism and the Social Sciences," in The Legacy of Logical Positivism: Studies in the Philosophy of Science, ed. Peter Achinstein and Stephen F. Barker (Baltimore: Johns Hopkins Press, 1969), pp. 163-194, on p. 191.

85. Carnap, "Intellectual Autobiography," p. 65.

86. W. V. Quine, "Truth by Convention," in Readings in Philosophical Analysis, ed. Herbert Feigl and Wilfred Sellars (New York: Appleton-Century-Crofts, 1949), pp. 250-273, on p. 270. This article originally appeared in Philosophical Essays for A. N. Whitehead, ed. Otis H. Lee (New York: Longmans, Green, 1936).

87. Quoted in "Discussion," in The Structure of Scientific Theories, ed. Frederick Suppe (Urbana, Ill.: University of Illinois Press, 1977), p. 511.

CHAPTER 3

PURPOSIVE BEHAVIORISM AND ITS PHILOSOPHICAL BACKGROUND

In terms of its immediate intellectual antecedents, behaviorism is usually considered, and rightly so, to have been a product of Pavlov's research on conditioned reflexes, Thorndike's work on trial-and-error learning and the law of effect, and Watson's vigorous polemics for objectivism in psychology. It is thus a measure of Edward C. Tolman's originality and independence of thought that the neobehaviorism which he began developing in the 1920s embodied a self-conscious rejection of all three of these traditions. For those who expect all behaviorists to come out of the same mold, Tolman is indeed a puzzling figure. He downplayed Pavlov's reflexology, outright repudiated Thorndike's connectionism, and criticized Watson's equivocal conception of the learned response. He openly embraced Gestalt psychology at a time when it had become the target of many a behaviorist diatribe, and he designated Kurt Lewin and Sigmund Freud as the great psychologists of his era.¹ With characteristic iconoclasm, he disputed the mechanistic picture of behavior presented by many versions of behaviorism and insisted instead that behavior is inherently purposive and cognitive. Such unusual features of Tolman's behaviorism

have led some to question whether his system was really a behaviorism at all², but Tolman certainly viewed himself as a behaviorist and tried to justify that self-ascription by consistently defining his elaborate system of concepts in terms of observable behavior.

In fact, it was Tolman's unique combination of behaviorism and cognitivism, his simultaneous emphases on observability and conceptual complexity, that forced him to pay careful attention to the constellation of issues surrounding the problem of empirical definition. Had he been less of a behaviorist, he could have let his theorizing range further from his data base of experiments in animal behavior. Had he been less of a cognitivist, he might have sacrificed some of the complexity of his concepts by simply basing them on the relatively standardized models of Pavlovian and instrumental conditioning. As it turned out, his unwillingness to compromise in either direction left him with the substantial task of ensuring the empirical content of his higher-order, quasi-mentalistic concepts. This was no mean task, but Tolman's behaviorism possessed a degree of epistemological sophistication not evident in the classical behaviorism of Watson. The solution to the task, as proposed by Tolman in the thirties, was an operational behaviorism in which cognitive concepts were represented as intervening variables--that is, variables which intervene between the

independent variables of antecedent conditions and the dependent variables of consequent behavior. The operational intervening variable paradigm quickly became the fashion of the day in psychology, and it still stands as Tolman's major methodological contribution.³ As we shall see, Tolman's proclivity for operationalizing his concepts derived from his background in neorealist philosophy, but his operationism of the thirties was worked out under the additional influence of logical positivism. His interest in empirical definition was his major intellectual link with the logical positivists.

Tolman's Background and Career: An Overview

Tolman was born in Newton, Massachusetts, in 1886 to a well-to-do family of manufacturers.⁴ With the expectation of entering the family business, he and his older brother Richard both attended the Massachusetts Institute of Technology. Richard found the academic life agreeable, however, and went on to become a prominent theoretical physicist.⁵ Having identified more closely with his brother than with his father, Edward followed Richard into scholarly pursuits. A reading of William James in his senior year led Edward to consider philosophy as a career, but after taking a single philosophy course at Harvard he decided that psychology was closer to his interests. Psychology, he felt, offered "a nice com-

promise between philosophy and science."⁶

But Tolman had other reasons for entering a career in psychology. These were the specifically humanitarian concerns instilled in him by the social milieu in which he was raised.⁷ Tolman wrote that there

persisted in our family and in those of some of the neighbors the legacy of reformism, equal rights for Negroes, women's rights, Unitarianism and humanitarianism from the early days of the "Flowering of New England." These social tendencies were combined with the special Bostonian emphasis on "culture" together with, in our family,⁸ a special dose of moral uplift and pacifism.

Tolman added that he and his brother were "set to increase the sum of human knowledge and presumably were to apply such an increase to the betterment of mankind."⁹ To implement these aims, he considered becoming a Unitarian minister but decided on psychology in the belief that discovering "what made people tick" would be "much more successful than preaching at them."¹⁰ Tolman's pacifism, liberalism, and humanitarianism were evident throughout his career. For example, he addressed his major work on motivation to the causes of war, he was an active member and one-time president of the Society for the Psychological Study of Social Issues, and he led the protest against the University of California's infamous loyalty oath during the McCarthy era.¹¹ Tolman's concern with human problems had important implications for his theoretical work, for it meant that theory had to extend beyond the

confines of the laboratory to address the complex issues of human life. Before his death, several of his colleagues wrote:

He believes that a theorist's job is to try to describe and account for the entire field that lies within his discipline, and not to restrict it arbitrarily to the more amenable areas. Tolman has never been interested in writing a "small scientific theoretical system"--he has always sought the complete formulation. This has meant the witting rejection of attempting finalistic formulations at this stage of the science, and the characteristically cheerful acceptance of a programmatic role.¹²

The implied contrast in this passage between Tolman and Hull is a revealing one. The rigor of formulation achieved by Hull was gained largely at the expense of assuming all psychological phenomena to be conditioned habits. Tolman, with his broader and more applied focus, placed comprehensiveness above rigor, or at least formal rigor, and remained skeptical about the prospects for achieving both.

Tolman began his graduate studies at Harvard in 1911 and received his doctorate in 1915.¹³ There he took courses in philosophy and psychology and was exposed to the works of William McDougall, Edward B. Titchener, and John B. Watson. Most importantly, he studied with the neorealists Ralph Barton Perry and Edwin B. Holt, whose philosophically inspired behaviorism laid the foundation for Tolman's later neobehavioristic rejection of Watsonian behaviorism. But this development did not come right

away. Tolman spent the first three years after graduate school as an instructor at Northwestern, where he was "still thinking largely in terms of classical introspectionism and associationistic problems" and conducting research on "such pre-behavioristic problems as retro-active inhibition, imageless thought, and association times for pleasant, unpleasant, and neutral words."¹⁴ It was not until after his arrival at Berkeley in 1918 that he began to view himself as a behaviorist. In that year he introduced a course on comparative psychology and shortly thereafter initiated the studies of rats in mazes which became the hallmark of his behaviorism.

In the course of his long career at Berkeley, the maze became for Tolman what the conditioned-reflex machine was for Hull--a centerpiece of laboratory activity, a heuristic source of concepts and hypotheses, and eventually a general metaphor for psychological phenomena. As a means of constraining learned behavior into spatial patterns, the maze was an apparatus highly suited to Tolman's penchant for thinking in spatial terms. Tolman perceived himself as having weak verbal imagery but good spatial abilities. As he put it:

. . . I feel comfortable only when I have translated my explanatory arguments into diagrams. I always did like curves better than equations. Analytic geometry was a lot more fun than advanced algebra. . . . I am very unhappy whenever I do not have a blackboard in my office.¹⁵

With the help of his diagrams, Tolman's spatial images and metaphors were developed into not only his concepts of learned behavior but also, as we shall see, the concepts which underlay his interpretation of science itself.

Tolman combined the conception of learned behavior which he had absorbed from his neorealist teachers with his experimental research of the twenties into his magnum opus of 1932, Purposive Behavior in Animals and Men.¹⁶ This tome is rightfully regarded as the first book-length work of neobehaviorism. In it, Tolman addressed the issue of the definition of the learned response, arguing that learning is not normally a matter of acquiring isolated, punctate movements in response to punctate stimuli but rather one of acquiring entire "emergent" patterns of action in response to environmental objects or even global environmental situations. Watson had spoken of both conceptions, the narrow reflexological one and the more integrated wholistic one, but he usually fell back on the former conception in his polemics for objectivism. Tolman caricatured the former conception as "Watsonian Muscle Twitchism" and took up the challenge of showing the latter conception to be equally objective and more fruitful. In drawing the important distinction between these two conceptions of learned behavior, Tolman adopted the terms "molecular" and "molar" from the philosopher Donald C. Williams.¹⁷

Purposive Behavior represented the culmination of Tolman's early career, a period in which he tended to view the purposive and cognitive features of behavior as immediately presented in that behavior. But it was also a transitional work, for therein Tolman adumbrated the notion of what would later be called the intervening variable. Upon completion of the book, Tolman took a sabbatical and went to Europe, where he spent seven months in Vienna. Having polished his German during trips to Germany in 1912 and 1923,¹⁸ Tolman was equipped to make the most of his stay in Vienna. The influences he came under there, specifically Egon Brunswik's probabilistic functionalism and the Vienna Circle's brand of empiricism, served to reinforce trends that were already evident in his thought when he wrote Purposive Behavior. By the late thirties, he had incorporated the influence of logical positivism and was prepared to move beyond it in the latter part of his career.

For Tolman, moving beyond logical positivism meant assimilating what he found worthwhile in it to his own conception of knowledge as a psychological phenomenon, but it meant no more than that. Although Tolman's pragmatic empiricism made him generally sympathetic to the methodological prescriptions of logical empiricism, fixed prescriptions of any kind were basically incompatible with his view of knowledge acquisition as a highly flexible,

tentative, and exploratory activity. In Tolman's view, the scientist, like any other organism, needs to maintain an adaptive flexibility in the face of a complex and changing environment. All ideas, all perspectives on psychology, were seen by Tolman as deserving of exploration, and any methodological pronouncements which would constrain the range of investigation were to be regarded with a skeptical eye.

In philosophy and psychology alike, Tolman was skeptical of restrictive pronouncements, especially those advanced with an air of authority. He remarked in his autobiography that his family background seemed to be the type "conducive to the developing of ambitious, but non-authoritarian personalities."¹⁹ The non- and even anti-authoritarian aspect of Tolman's character was revealed in his political views, in his style of teaching, in his psychological views, and in his conception of science. As mentioned above, Tolman did not bow to the authority of Pavlov or Thorndike, and he showed an equal degree of independence in regard to his respected contemporaries. While Hull was deriving Pavlovian conditioning as a special case of instrumental conditioning and Skinner was codifying the two into the operant-responder distinction, Tolman was arguing for the existence of at least six kinds of learning.²⁰ Proposals to limit investigations of learning to one or two paradigms

were accordingly rejected by Tolman.

Tolman's open-ended style of theorizing was not modelled after any other style, and his theoretical statements were advanced tentatively and often with self-deprecating humor.²¹ As his colleagues put it:

System building for him is not a grim business. It is a happy, gay, creative activity, and his papers express all of this to the full. No matter what the subject, how abstract the treatment, his wit, humor, magnanimity, and tolerance are written into each analysis. He is constitutionally incapable of writing dogmatically or of publishing a polemic.²²

Tolman's students testified to the fact that his non-authoritarian character and his exploratory style of thinking were carried into the classroom.

His classes (which frequently evolve into loud free-for-alls in which student and teacher cannot be differentiated) reveal the searchings and fumbings of the creative scientific mind rather than a digest of conclusions already reached, organized, and neatly filed away. Nothing has ever been authoritarian, static or finished either in his systematic psychology or in his personal relations.²³

Tolman's skepticism about authority went hand-in-hand with his free-wheeling scientific style. And, as we shall see, these characteristics led him to oppose any narrow or dogmatic version of logical positivism.

The extraordinary openness shown by Tolman to various ideas and approaches to psychology gave his theorizing a high degree of eclecticism. His psychological views incorporated strains of neorealist epistemology,

Gestalt psychology, Lewinian theory, neutral monism, probabilistic functionalism, Stephen C. Pepper's contextualism, and Freudian theory. In contrast to the relative single-mindedness with which Hull and Skinner pursued their respective brands of behaviorism, Tolman developed his purposive behaviorism in a diffuse, varied, and shifting fashion. For the historian of psychology, the task of sifting out and making sense of the strains of influence is a difficult one; Tolman's work resists description and interpretation in any fixed or univocal way. Nevertheless, there are general patterns in the development of his thought which, when drawn out, reveal much about his relationship with logical positivism. Any proper understanding of Tolman's views must begin with an examination of the neorealism of his Harvard teachers Holt and Perry.

The Neorealist Background

The adherents of neorealism²⁴ viewed the oscillations of nineteenth century philosophy as ample demonstration of the inadequacies of the idealist, dualist, and materialist traditions. Their primary aim was to steer a course through these traditions leading directly to a thoroughgoing naturalism, one which would sacrifice inclusiveness for a more compatible relationship with the special sciences. The movement initially arose as a critique of idealism, first expressed in America in the

attacks of Perry and William Montague on the Harvard idealist Josiah Royce in 1901 and 1902.²⁵ Voicing a theme that would often be repeated, these authors assaulted the idealist notion that the world is constituted by the pre-eminent act of cognition. Rather than decisively conditioning the world, cognition was to take its place within the natural world and provide the basis for the more complex derivative phenomena of value and ethics.

Although the neorealist program was first and foremost a criticism of idealism, it quickly broadened into a further critique of dualism and materialism. Perry in particular argued against the dualist view that "one's own mind, or the mind at home, must be preferred as more genuine than the mind abroad" and that mind is a private entity "encased in a non-mental and impenetrable shell."²⁶ The New Realism held that the world is presented to an observer rather than being represented by "invisible pawns" in the perceiver's private sphere, a dualist fiction regarded as having adverse implications for both psychology and philosophy. To the neorealists, materialism was an equally fallacious doctrine primarily because it erred in "denying the facts, as well as the theory, of consciousness."²⁷ Materialism's assignment of mental events to the realm of epiphenomena collided with the New Realism's insistence that the things of thought be given the same ontological status as physical entities.

The American neorealist movement was formally proclaimed in a statement coauthored by Holt, Perry, and four collaborators just a year before Tolman's arrival at Harvard in 1911. An elaboration of the neorealist position appeared two years later in a manifesto entitled The New Realism.²⁸ During this heyday of the movement, Tolman was introduced to and "excited by" the New Realism through a seminar taught by Holt.²⁹ This proved to be an important intellectual discovery for Tolman. As we shall see, it provided him with 1) relatively complex but workable conceptions of stimulus and response, 2) an enduring predisposition to view the psychologist and the subject as operating on the same epistemological level, and 3) a realist epistemology which formed the basis of his early research on animal behavior and which established his propensity for operationalizing psychological concepts.

Behavior as Purposive and Cognitive

At about the time that Tolman was being exposed to the New Realism, Perry and Holt began extending the neorealist program to psychology by formulating a behaviorism which gave the concepts of purpose and cognition an objective status in the natural world. In doing so, they characterized organismic behavior in a way that adumbrated the later concept of "molar" behavior.

Holt's seminal paper on "Response and Cognition" and his book The Freudian Wish both appeared in 1915. These works criticized the adoption of the materialist's "bead theory" of causality in the realm of behavior. As opposed to the materialist view of behavior as a chain reaction of isolated reflexes touched off by isolated stimuli, Holt contended that true behavior is an integrated and novel synthesis which exhibits "objective reference" to environing objects. The immediate stimulus (e.g., light on the retina) governing the isolated reflex "recedes" in importance as behavior becomes more highly organized until the environing object itself controls the response. Holt's important notion of the recession of the stimulus enabled him to define the act of cognition as simply the "objective reference" of an integral response to an object. The synthetic response, the object responded to, and the relation of objective reference between them were thus all "out there" waiting to be recognized and investigated by the psychologist.

According to Holt, the putative and suspect relation, much discussed by philosophers, between the subject and object of consciousness is nothing more than the relation of objective reference between behavior and object. To ignore the functional reference of behavior is to succumb to the dualist superstition that "ideas" in the "sensorium" somehow represent the environment. Anticipating

later attempts by psychologists to evade the philosophical puzzles of dualism by operationalizing mentalistic concepts, Holt identified consciousness with a manner of responding. He wrote:

When one is conscious of a thing, one's movements are adjusted to it, and to precisely those features of it of which one is conscious. The two domains are coterminous.³⁰

Consciousness, long considered to reside exclusively in a private mental world, was thus externalized; the genuineness of the "mind abroad" was affirmed.

At about this time, Perry was pursuing a similar analysis of the concept of purpose. In "Purpose as a Systematic Unity" (1917), he argued against the idealist notion that purpose is a global characteristic underlying the universe and advocated a behavioral interpretation of concrete instances of purposiveness. In "Purpose as Tendency and Adaptation" (1917), he examined mechanistic biological treatments of purpose as tendencies and homeostatic adjustments and found them to be too limited to account for the purposive nature of highly organized plastic behavior. In its most general case, said Perry, purpose must be identified with adaptability rather than mere adaptation, a crucial insight which he developed in his 1918 essay "Docility and Purposiveness."

In that paper, Perry identified purpose with docility or "teachable-ness." He wrote:

Docility thus construed requires that the behavior of the organism shall be variable in all three of the aspects into which it can be analyzed: namely, feature of the environment attended and responded to, physical movement, and effect. Purposiveness thus appears in life pari passu with variability or modifiability of behavior.³¹

As Perry explained it, each combination of environmental feature (e) and response (r) produced a particular consequence (e.g., a, b, or c). The three elements would combine in varying fashion (e.g., $e_3 + r_1 = a$, $e_1 + r_2 = b$) until one produced the desired outcome m (e.g., $e_2 + r_3 = m$). This successful combination was said to instantiate the general set or governing propensity ($E + R = M$), and r_3 could then be said to be performed for the sake of the goal M. The observed variability and modifiability of responding with respect to the goal were for Perry the crucial features of purpose.

In identifying purpose with that "margin of modifiability" which characterizes the activity of a docile organism, Perry was denying the internal locus and private status of purpose. Like Holt, he was affirming the genuineness of the "mind abroad" by emphasizing the functional relatedness of the response to its setting, in particular to the object being responded to and for. Cognition for Holt and purpose for Perry could be identified with manners of responding because responding was for them a cohesive pattern of behavior rather than a mere discrete movement.

By 1918, Holt and Perry had thus arrived at their shared conception of behavior as a highly integrated, synthetic, and modifiable pattern of responses. This conception was motivated philosophically by their desire to objectify and naturalize the metaphysically charged concepts of purpose and cognition rather than consign them to oblivion in a materialistic world-view. In a sense, the neorealists' conception of behavior was a natural extension of the doctrine of external relations to the realm of behavior. Purpose and cognition involved the observable actions of an organism in relation to gross objective features of the environment. Any narrowly physiological definition of behavior would necessarily fail to acknowledge this crucial relatedness of response and environment.

Although Tolman was exposed to this orientation during his Harvard years, it was not until after his arrival in Berkeley in 1918 that he worked it into the full-blown research program which he came to designate "molar behaviorism." Neither Holt nor Perry had directed their reconceptualizations of behavior against Watson, but their viewing the response as more like an act than a muscle movement or a glandular activity laid the foundation for Tolman's later critique of Watson. Tolman's early theoretical papers leading up to the publication in 1932 of his Purposive Behavior in Animals and Men often cited

Holt and Perry as important early proponents of a non-physiological behaviorism. Tolman also repeatedly acknowledged having borrowed from Perry the idea that a molar behaviorism must recognize the purposive or docile character of behavior.³²

Neorealist Epistemology

While the influence of Holt and Perry on Tolman's concept of the response is relatively direct and well-known, the influence of their general epistemological views on Tolman's psychology has gone unrecognized by historians of psychology. The neorealist epistemology involved some unusual, perhaps even extreme, philosophical positions. These were implicitly adopted by Tolman early in his career but were later gradually abandoned as the epistemological base of his molar behaviorism shifted from a problematic direct realism to a more workable probabilistic functionalism. The epistemological views of the New Realism which underlay Tolman's early theorizing are the topic of the present section.

In their eagerness to reject the idealist claim that the cognitive act constitutes the world, the New Realists were unwilling to admit that anything experienced depends for its existence on the fact of being experienced. The English neorealist T. P. Nunn had already in 1909 pursued this stance to the point of asserting that even pain

is external to the mind--that it is an objective experience to be reckoned with just as any material object. Holt followed suit by arguing that such allegedly non-veridical perceptions as illusions, hallucinations, and mirror images likewise reside outside the mind and that one's behavior can exhibit objective reference to them. Since the contents of experience do not come tagged as "real" or "unreal," Holt simply assigned all experiences to the ontological realm of neutral "subsistents."

Holt and Perry were devoted students and admirers of William James, and they followed him--as did Bertrand Russell somewhat later--in subscribing to a neutral monism.³³ In the neorealists' neutral monism, subsistents were neutral in the sense of having "being" without any connotation of reality or unreality. According to Holt, "every content . . . subsists of its own right in the all-inclusive universe of being."³⁴ A content could thus occur in consciousness without depending on it. Whereas the idealists had claimed that direct knowledge is possible only if the known depends on the knower, the neorealists asserted as their cardinal tenet "the independence of the immanent." Subsistents were both immanent, i.e., directly presented in experience, and independent of that experience.³⁵

In denying that the act of knowing involves any constructive activity on the part of the knower, the neo-realists were led to the further important conclusion of denying the distinction between primary and secondary qualities. Holt was especially willing to educe the bolder implications of this rather extreme view. Secondary qualities such as color subsist, according to him, out there in the thing which consciousness selects and are therefore just as objective as primary qualities.³⁶ The neorealists had maintained all along that "the knower . . . is homogeneous with the environment . . . and may itself be known as are the the things it knows."³⁷ In conjunction with the contention that qualities are presented, not represented, in perception, this claim meant that the psychologist who studies cognition in another organism has rather direct access to another mind. Taken literally in its strongest form, the New Realism implied that the mental qualities of another organism are just as objective as its bodily qualities and are therefore directly presented to, rather than being constructed or inferred by, the observer. Being of equal stature on the objective plane of subsistence, mind and body were viewed as equally manifest to the observer of behavior.

By following James in his refusal to draw distinctions between the real and the unreal, the neorealists felt that they could liberate psychology from the

conceptual shackles of idealism and dualism, and thereby enhance its claim to equal membership in the natural sciences. Idealism had given mind too central a role in nature; dualism had given it too isolated a role. Only a direct realism could restore the harmony of mind and nature, and it did so by making nature's contribution to the contents of experience indifferent to the distinction of mind and body. With respect to the psychological experiment, both the subject and experimenter were conceived as directly receiving the complex presentations of the world. To think of the subject as receiving only isolated sensations while the experimenter perceived objects and relations in the environment was to fall prey to one of the many "concretely intolerable" implications which dualism held for psychology.³⁸ If knowing was an objective relation occurring in the natural world, then knowing about knowing was also just such a relation.

In its application to psychology, the epistemology of neorealism entailed two separable themes. First, there was the claim that a direct realism governs the perception and cognition of organisms. Second was the more general claim that the subject and the experimenter, the known and the knower, share an equivalent epistemological standing. As we shall see, Tolman initially accepted both theses but later abandoned the first while retaining the second.

Tolman's Early Behaviorism

Concept of Behavior

Foremost among what Tolman acquired from his neo-realist mentors was the idea that behavior stands in a patterned relation to the environment in which it occurs. The "bead theory" of causation which Holt had ridiculed was viewed by Tolman as the Achilles' heel of Watsonian behaviorism. In his first theoretical paper on behaviorism, "A New Formula for Behaviorism" (1922), Tolman called Watsonianism into question. According to Watson, behaviorism aims to predict a response from knowledge of a given stimulus. Tolman replied:

Very good! But how does he define stimulus and response? He defines them, he says, in the terms in which physiology defines them; that is, stimuli are such things as 'rays of light of different wave lengths, sound waves differing in amplitude, length, phase and combination, gaseous particles given off in such diameters that they affect the membranes of the nose,' etc., and responses are such things as 'muscle contractions and gland secretions.'³⁹

But in Tolman's reckoning, Watson was unable to see the behavior for the reflexes: by defining causal stimuli in narrow physical terms he was led to view their behavioral effects as equivalently bead-like. On the stimulus side, he had ignored the recession of the stimulus; on the response side, he had failed to notice the objective reference of behavior. Taken together, Watson's physio-

logical leanings rendered him unable to recognize that behavior inherently displays purpose and cognition. In his papers of the twenties, Tolman's aim was to counter Watsonian behaviorism by arguing that the more complex and higher-order neorealist notion of behavior was just as objective and even more workable than the Watsonian version. As Tolman later put it, he had learned from his Harvard teachers "to be complicated but to remain naturalistic."⁴⁰

Tolman's positive program for redefining the learned response can be viewed as a working and reworking of the idea that behavior is inherently related to and directed toward objects in the environment; that is, behavior is not mere movement elicited by stimuli but rather a pattern of adjustment involving functional reference to the world. Tolman recognized that he was not alone in this quest and cited the formulations of behavior given by J.R. Kantor and Grace de Laguna in addition to those of Holt and Perry. For instance, de Laguna had already written in 1919 that:

In order to understand behavior we must resolve it into a system of interrelated functions. . . . Now just as there is a physiological economy, so there is a larger vital economy in closest union with, yet distinguishable from it. This is the system of behavior, by means of which the being, animal or human, maintains his relations with the environment. . . . The science of behavior has the task of tracing the lineaments of this larger economy.⁴¹

In this passage, which Tolman quoted, were the ideas congenial to his own that behavior is functionally related to the environment and can be distinguished from mere physiology.

In developing the relational definition of the response, Tolman took issue with Watson's treatment of the emotions. Watson had tried to define emotions such as fear by simply enumerating stimuli and responses; but because the enumerated responses were often unobserved visceral responses, Tolman argued, Watson had to resort to his intuitive knowledge of fear-producing situations in order to identify relevant stimuli. Watson's difficulty, according to Tolman, was due to his failure to recognize that emotions are necessarily "total behavior situations."⁴² Thus, wrote Tolman, "It is not a response, as such, nor a stimulus situation, as such, that constitutes the behavior definition of an emotion but rather the response as affecting . . . the stimulus situation."⁴³ In his causal analysis of stimuli and responses, Watson had neglected the objective reference of the response to the stimulus. Emotional responses normally act back on the stimuli which produce them in such a way as to eliminate them or mitigate their effects. As such, they exhibit cognitive reference to the stimuli as well as purpose with respect to them. In accounting for behavior, Watson had downplayed any sort of "back action" or effect,

a notion he felt was tinged with subjectivity, in favor of the more automatic processes of frequency and recency. Any adaptive value, or seeming purpose, in a response had to be attributed to the biological value of the original innate response on which it was based, i.e., from which it was conditioned.⁴⁴ Without denying an important adaptive role for innate behavior, Tolman objected to the narrowness and mechanical character of Watson's notion of behavior. As always, he found inadequate any account of behavior limited to a few principles.

The relatedness of environment and response, qua purposive and cognitive, became a recurring theme in Tolman's early papers. This motif took on added prominence after Tolman's 1923 visit to Giessen where he absorbed the doctrines of Gestalt psychology from Kurt Koffka. In 1926, Tolman distinguished two kinds of features of the environmental stimulus or situation to which behavior has objective reference. The manipulation features were those environmental properties capable of supporting given sorts of behavior with respect to the environing object. Thus, a chair might present a "to-be-sat-on-ness" feature to a human and perhaps a "to-hide-behind-ness" feature to a rat.⁴⁵ The discrimination features of an object or situation were those cues given off by it which could serve as signs for its manipulation features. They were thus not neutral givens but rather

always bound up with behavior possibilities. Having distinguished between discrimination features and manipulation features, Tolman then asserted that the two kinds of features could unite, given the appropriate experiences, into what he called "the ultimate units of behavior."⁴⁶

Learning was thus a matter of forming relations between the discriminations and manipulations or, in other words, of learning what can be done with what.

It was in the elucidation of his concept of the ultimate unit of behavior that Tolman revealed the influence of Gestalt psychology on him. He wrote:

These units of behavior . . . can be of very different degrees of generality or extensiveness. For not only "entering an alley," "clawing at a loop," and the like, are to be described as such units of behavior, that is, of discrimination-manipulation wholes, but also such more extended operations as "setting out of the box," "running through the total maze," "buying a house," "going to Europe," or even "embarking on a career." But in these more extended units it is obvious that the defining discriminations must be less specifically detailed than in the cases of "clawing at a loop," and "entering an alley." A wider variety of different particular stimuli and of different particular supports can be substituted in them and have them still preserve their defining characters. All that is necessary is that the general pattern of discriminations and manipulations remain the same. And here, it may be noted, is where the Gestalt psychology comes in. For the Gestalten, as I see it, are just such discrimination-manipulation units, which as the Gestalt psychologists themselves have emphasized, do retain their specific defining outlines in spite of wide changes and variations among their constituent elements.⁴⁷

Here was the neorealist relation of objective reference transformed into the Gestalt of a discrimination-manipulation whole. By placing added emphasis on the inherent functional relatedness of discrimination and manipulation, Tolman was able to subordinate the elements of stimulus and response to the more fundamental patterned relation between them. The relation became the defining property of the unit of learning, and the particular stimuli and responses were reduced to the status of something like accidental properties. The intersubstitutability of particulars was the key to molar behaviorism, for it permitted a definition of the learned response without reference to the mere movements and physiological activities invoked by Watson in defining behavior.

As of 1926, then, Tolman's molar conception of behavior involved a two-term relation between a set of discriminations and a set of manipulations. As was the case with Holt's notion of objective reference, Tolman's Gestalt concept of the response omitted any explicit distinction between the responded-to object as a source of cues and the object as a goal to be achieved or avoided. In 1927, however, Tolman did draw this distinction and incorporated it into his revised concept of learning as a "grand total Gestalt." Discussing a discrimination situation in which a rat learns that an alley marked with a white cue leads to food whereas an alley marked with

a black cue leads to no food, Tolman characterized the acquired Gestalt as follows:

This total Gestalt would contain the differentiation of white from black, of food from nonfood, and of the sign relationship of white as leading to food from that of black as leading to nonfood.⁴⁸

Under the revised concept, learning thus involved three components fused into a single Gestalt: the cues, the goal object, and the relation of "leading-on-ness" between them. The neorealists had held that relations could be directly perceived, and Tolman had found that rats in mazes could learn spatial and causal relations. The concept of Gestalt offered itself as a natural way for Tolman to express these psychological facts.

By the time Purposive Behavior had appeared in 1932, Tolman's excogitations on the redefinition of behavior had culminated in his fundamental concept of the sign-Gestalt. Like its forerunner, the sign-Gestalt consisted of three parts: the sign-object (or cue), the signified object (or goal), and the means-end-relation. The latter was a kind of behavioral route for getting from the sign-object to the signified object. But Tolman recognized that in the typical environment there is a multiplicity of mutually equivalent, intersubstitutable routes leading from a sign to its object. This "multiple trackness" meant that

means-end-relations . . . are . . . probably never adequately represented by single lines, but always rather by some degree of spreading, fanning, or networking of the lines. Means-end-relations are, to put it another way, essentially field-relations.⁴⁹

Once again the intersubstitutability of particulars was a crucial feature of the molar response definition. Responding was rendered independent of physiology by conceiving it as an equivalence class of movements, routes, or means. The response concept foreshadowed by Perry's three-term relation of stimulus, response, and effect--each of which was conceived as variable--found its ultimate expression in the sign-Gestalt. So conceived, the learned response was close to the notion of a psychological act and remote from the notion of a colorless movement.

Tolman's formulation of the sign-Gestalt capped a decade of refinements of the molar concept of behavior. This development was important not only for understanding Tolman's thought and the history of behaviorism, but also for understanding the relationship between behaviorism and logical positivism. First, in separating the science of behavior from that of physiology, the molar concept of the response rendered naive and dated the logical positivists' repeated appeals to physiology for justifying their own versions of behaviorism (see Chapter 2). Second, when Tolman complicated his system by asserting that organisms could form expectations of sign-Gestalts,

he was led to seek an operationism sophisticated enough to handle such complexity--and in doing so, he was brought into contact with the Vienna Circle (see Chapter 4). Third, the sign-Gestalt was the basic concept underlying Tolman's psychological view of science, a view largely incompatible with the logical positivist picture of science. As we shall see (in Chapter 10), Tolman's purposive behaviorism was proposed to the logical positivists as a framework highly suited to the study of science as an empirical phenomenon; but not surprisingly, given the anti-psychologism of the logical positivists, the proposal fell on deaf ears.

Tolman's Early Epistemology and Proto-Operationism

Tolman's derivation of molar behaviorism from the early formulations of Holt and Perry was one important legacy of neorealism. Equally important in terms of the story to be told here was Tolman's adoption of the neorealist stance on epistemology. Implicit in Tolman's conception of behavior was the notion that organisms directly perceive and cognize the environing objects and relations in which they are interested. And like the neorealists, Tolman saw no need to invoke one theory of knowledge for the observed organism and another for the observing organism; in psychology, the subject and experimenter were, epistemologically speaking, on a par.

In his early theoretical papers of the twenties, Tolman often spoke of mental characteristics--as would be consistent neorealist--as if they were presented directly to the observer of behavior, that is, as if they were primary qualities. Purpose, for example, was viewed simply as a "descriptive phenomenon" which when adequately conceived "is itself but an objective aspect of behavior." As such, purpose was considered to be identical with a set of behaviors which exhibit a certain "persistence until" character, but was not to be inferred from behavior. To infer purpose from behavior was, according to Tolman, to eschew behaviorism in favor of a mentalism of the sort advocated by William McDougall. Tolman wrote that "the fundamental difference between him and us arises in that he, being a 'mentalist,' merely infers purpose from these aspects of behavior; whereas we, being behaviorists, identify purpose with such aspects."⁵⁰

Purpose, according to Tolman, did not need to be inferred or "read into" behavior because it was there to be directly "read off" the behavior. Purpose, he said, could be "pointed to" and "discovered by looking at another organism." In Tolman's words,

It is a descriptive feature immanent in the character of behavior qua behavior. It is not a mentalistic entity supposed to exist parallel to, and to run along side of, the behavior. It is out there in the behavior; of its descriptive warp and woof.⁵¹

This is a strong statement, one that not only reveals Tolman's embracement of the neorealist epistemology but also reflects the neorealists' bold style of assertion. In the statement we see a recapitulation of the realist doctrine of the "independence of the immanent": the purpose of another organism is directly presented in the observer's experience and yet is independent of it.

Much the same can be said of Tolman's early treatment of cognition. Cognition, qua the objective reference of behavior to an object or relation in the environment, was an immanent descriptive feature of behavior. Consisting of an organism's adjusting to and commerce with an object, cognition was not something to be inferred from behavior, much less to be introspected, but rather a point-at-able feature out there in the behavior. Knowing about cognition was therefore no more problematic, in principle, than ordinary cognition. As with any other subsistent, the cognition of one organism (say, a rat) could itself be cognized, i.e., responded and adjusted to, by another organism (such as a scientist). Likewise for purpose: to point to, describe, and investigate another's purpose was to exhibit objective reference with respect to that purpose. The epistemological views of the New Realism were bold and perhaps not wholly plausible, but they supplied just the epistemological leverage that was needed for a behaviorism that claimed to be purposive,

cognitive, and objective.

Tolman's early epistemological biases thus arose from his philosophical background, but they did not operate in isolation from his experimental research. As has already been mentioned, Tolman's research from 1918 on was conducted with mazes. Of all the types of apparatus in use by psychologists at that time, the maze was probably the type most appropriate for a neorealist because it made the objective reference of the investigator toward the rat's purposes and cognitions a matter of perceiving spatial relations.⁵² The rat's cognitions could be directly observed as its sequence of turns toward the goal box. Its purposes were presented as gettings-away-from the start box and gettings-toward the goal. Any investigator who merely observed these patterned behaviors was immediately privy to cognitions and purposes qua relations in physical space. Tolman's philosophical views and his chosen experimental method were thus mutually reinforcing aspects of his research program.

As is probably already evident from the foregoing account of Tolman's background and early behaviorism, his approach to psychology during this period constituted a type of what would later be called operationism. From his neorealist forbears, he had learned that mind was not an internal, private entity but rather something public and scientifically tractable. By virtue of its

inherent functional relatedness to its environment, the "mind abroad" could be directly perceived and therefore identified with events and relations in the outer world. In effect, Tolman's papers of the early twenties represented a series of efforts to operationalize psychological concepts previously thought of as inescapably "mentalistic." For each such concept, Tolman sought a criterion for its application, and in each case, he said, "this criterion, whatever it may be, is to be found somewhere in the externals of the situation."⁵³ As Tolman later put it, "when I began to try to develop a behavioristic system of my own, what I really was doing was trying to rewrite a common-sense mentalistic psychology . . . in operational behavioristic terms."⁵⁴

Tolman's use of the maze both as an experimental apparatus and as a conceptual device fit in neatly with his proto-operational viewpoint. Indeed, for defining mentalistic notions in terms of the "externals of the situation," it was unsurpassed because, unlike the typical Pavlovian conditioning apparatus, the maze environment by its very nature imparted an observable structure to the behavior which took place in it. The role of the maze in Tolman's proto-operational approach is revealed in the concluding statement of an address given by him in 1926 to a group of philosophers:

Human beings . . . can not convey per se private mental contents to their fellows. If their fellows do not understand what they are talking about, then all they can do is to point and grossly to behave. Or, better yet, they can take their fellows over to a really good laboratory and show them a really good rat in a really good maze, and say, 'See, this is what I mean, this is the sort of thing that is going on in my mind.'⁵⁵

If Tolman's operationism was born of his philosophical background, it was nonetheless his maze experiments that transformed it into a working program of research.

Explicit references to operationism did not appear in psychology until 1930, and operationism as a movement did not begin to make itself felt until around 1935.⁵⁶

But Tolman had certainly worked out the essentials of it during the twenties. Recognizing this fact, the eminent historian of psychology E. G. Boring wrote that:

Some--Holt and Tolman first--were clear that behaviorism does not exorcise consciousness, but absorbs it, reducing it to the behavioral observations by which it is observed. One can not say that operationism began at any point. It was there all along, to be understood and used by the astute who were not blinded by their own impetuosity.⁵⁷

In the second half of the 1930s, the operationist movement came to be explicitly identified with the logical positivist movement. Tolman's early operationism anticipated not only that development but also came before any influence of logical positivism had reached American shores. For our present purposes, the important conclusion is that operationism--the very point of intellectual

contact between Tolman and the logical positivists-- was developed by Tolman prior to his knowledge of logical positivism and in a fashion that was indigenous to his evolving behavioristic thought. As such, it was not so much a methodological device for combatting metaphysical influences in psychology or for neutralizing mind; rather, it was a natural outgrowth of his neorealist metaphysical presuppositions and the scientific research based on those premises.

Pitfalls of Neorealism: The Challenge to Tolman

With its conception of behavior and its epistemological views, the New Realism launched a novel brand of behaviorism and an early version of operationism. Its philosophical stance lent support and credence to those new developments during their crucial infancy. But the philosophical claims of neorealism were peculiar, if not outright extreme, and not endowed with great initial plausibility. The idea that mind is not exclusively private was not difficult to accept, but the implication that another mind could be directly presented to an observer strained one's credulity. Surely most knowledge of other minds is inferred from, rather than read off, other's behavior. The neorealists had advanced their arguments with audacity and originality, but as one historian of philosophy put it, "there was something

suspect in the very ingenuity which Perry and Holt brought to bear on their epistemology."⁵⁸

The mental qualities of another organism were said by the neorealists to "subsist," like any other qualities, as givens in experience. They were thus neither more nor less real than other subsistent qualities. But in the absence of compelling arguments to the effect that mental qualities need not be inferred, the invoking of a realm of subsistence proved to be a philosophically awkward maneuver and lent little support to the program of objectifying mental phenomena. As a consequence both Holt and Perry frequently fell back on neurophysiological interpretations in their pursuit of objectivity, especially in those cases in which mental events were not so clearly revealed in behavior. They spoke, for instance, of expectations as nascent physiological adjustments and of ideas as centrally stimulated signs. But in resorting to such interpretations, they not only compromised their anti-materialist position, but also undercut their claims that mind is directly presented in behavior. In the early twenties, Tolman was aware that physiologizing was a pitfall for a molar behaviorist, and he was criticizing Perry for not being "wholly self-conscious of this essential difference between such a true behaviorism and a mere physiology."⁵⁹

As a movement, the New Realism had begun as a reaction against the excesses of idealism and ended as a victim of its own excesses. Although its radical implications were drawn out with considerable ingenuity, they were soon recognized as untenable overreactions. By 1920, the Critical Realism of Arthur O. Lovejoy, Roy Wood Sellars, and others had reasserted the activity of the observer in conditioning the known, the representational character of knowledge, the philosophical import of nonveridical experience, and the need for an ontology of greater complexity than simple subsistence.⁶⁰ Holt and Perry themselves abandoned neorealism, the former pursuing instead a materialistically inclined philosophical behaviorism and the latter developing a theory of value.⁶¹

After his theoretical papers of the early 1920s, Tolman rarely cited the works of Holt and Perry, but he was no doubt aware of the fate of neorealism. To the substantial extent that his early behaviorism rested on a neorealist foundation, it was left in something of a philosophical vacuum by the demise of neorealism. The neorealist legacy placed Tolman in an awkward trilemma where he wavered for a time between three equally to-be-avoided alternatives. The first alternative was to continue in neorealist fashion to claim that the mental life of another organism, its purposes and cognitions, could be read directly off its behavior. Faced with the essential

implausibility of this approach, he could have, secondly, resorted to physiological interpretations of mental events. As a third alternative, he might have abandoned his attempts to objectify mental phenomena by defining them in terms of observables. This last possibility was, for Tolman the empiricist, out of the question all along. The second, he entertained briefly early in his career but then quickly rejected. But the first alternative proved difficult for Tolman to give up. Although he began to move away from it as early as 1926, he did not completely renounce it until 1935, the year after his stay in Vienna.

Tolman's short-lived flirtation with physiological interpretations of mental events is revealed in a paper of 1918 entitled "Nerve Process and Cognition." In this, his very first theoretical paper, he attempted a neuro-physiological rendering of cognition in much the fashion of his neorealist teachers. Seeking "a definition of cognition which naturally and of itself provides its own neurological explanation,"⁶² he defined cognition as an internal neural sorting of qualities resulting in a system of interconnected neural paths; a path for an idea like "finance" would have connections with paths for the ideas of "commerce" and "industry" as well as with paths for sensory qualities such as "dollar signs." These speculations conflicted with Tolman's empiricism and were deeply incompatible with the molar behaviorism which

he was just then beginning to develop. By 1922, he had clearly recognized the futility of such speculation and was even, as remarked above, criticizing Perry for his incomplete eschewal of physiologizing.

Tolman's move away from direct realism and his gradual acknowledgement of the inferred status of mental properties was a much slower development. In 1925, he began referring to purpose and cognition as both "descriptive aspects" and "determiners" of behavior.⁶³ This equivocal treatment of the concepts as having both manifest-descriptive and underlying-explanatory roles persisted in Tolman's thought for a period of ten years. Thus, in 1926, he was claiming that purpose was "out there in the behavior" and could be pointed to."⁶⁴ But in 1928, Tolman was forced by the criticisms of the Chinese behaviorist Z. Y. Kuo to admit that purpose "has to be inferred from its effects and cannot be directly sensed." Even so, Tolman followed this concession with the qualifying assertion that purpose is a "perfectly objective feature which appears in behavior" and is therefore neither mentalistic nor introspectionistic.⁶⁵ Tolman's vacillation on this issue suggests that he realized the philosophical difficulties involved in applying direct realism to purpose and cognition, but that he found it difficult to forgo the conceptual advantage of construing purpose and cognition as immediate features

of behavior.

As to whether mental characteristics are given in behavior or inferred from it, Tolman's thinking continued to vacillate through the appearance in 1932 of his classic Purposive Behavior. E. G. Boring has remarked that the concepts employed by Tolman in that work "show how necessarily indeterminate is the line between the direct observation of datum, on the one hand, and the not-so-fully conscious inference of function, on the other."⁶⁶ Nowhere was the fuzziness of this line more apparent than in the case of Tolman's most fundamental concepts of purpose and cognition. Within a single paragraph of Purposive Behavior, Tolman described purposes and cognitions on the one hand as "immanent" in behavior, "in-lying," "immediate," and "discovered" by observers, and on the other hand as "determinants" and "causes" of behavior which are "invented" or "inferred" by observers.⁶⁷

Tolman's lack of resolution on this crucial matter, even through the time of his magnum opus, reflected the demise of the philosophy which had spawned and supported his molar behaviorism and his proto-operational epistemology. He was unwilling to give up behaviorism, but he could no longer view mental qualities as primary-like qualities of observable behavior. Likewise, he could not physiologize them away or assign them to some contrived ontological realm such as "subsistence." Those strategies

had been tried and found wanting; they were the pitfalls of neorealism. The collapse of the New Realism presented Tolman with a challenge: how could he retain purpose and cognition as objective, naturalized, and in some sense "operationalized" concepts while avoiding the untoward implications of the neorealist epistemology? That is, could purpose and cognition be "read into" or inferred from behavior and still be objective?

What was needed was some sophisticated version of operationism by means of which mental properties could be admitted to have an inferred status and yet at the same time be objectified by being given empirical definitions. Purposive Behavior, in which Tolman's equivocation on the observed-versus-inferred issue had been so blatant, appeared the year before his trip to Vienna. Tolman later wrote that he was struggling during this period toward definitions of purpose and cognition as intervening variables.⁶⁸ If so, it was significant that he chose Vienna for his sabbatical in 1933-34, for it was there that he worked out a resolution of the conceptual difficulties that had beset his budding purposive behaviorism. Under the twin Viennese influences of logical positivism and probabilistic functionalism, Tolman formulated his influential response to the challenge presented by the downfall of neorealism. These developments are the topic of the following chapter.

Notes for Chapter 3

1. See G. W. H. Leytham, "In Memory of Edward Chace Tolman (1886-1959)," Bulletin of the British Psychological Society 49 (1962): 21-28, on p. 26.
2. See, for example, Benjamin B. Wolman, Contemporary Theories and Systems of Psychology (New York: Harper & Row, 1960), pp. 140-141. Wolman titled this brief section "Is Tolman a Behaviorist?"
3. As will be discussed in Chapter 7, Clark Hull was among the psychologists who followed Tolman in adopting intervening variables. For a general discussion of the intervening variable paradigm and its role in social science, see Paul Lazarsfeld, "Concept Formation and Measurement in the Behavioral Sciences: Some Historical Observations," in Concepts, Theory, and Explanation in the Behavioral Sciences, ed. Gordon DiRenzo (New York: Random House, 1966), pp. 144-202.
4. Edward C. Tolman, "Edward Chace Tolman," in A History of Psychology in Autobiography, vol. 4, ed. Edwin G. Boring, Heinz Werner, Herbert S. Langfeld, and Robert M. Yerkes (Worcester, Mass.: Clark University Press, 1952), pp. 323-339, on p. 323. The present section is based largely on this autobiographical sketch.
5. Like Edward, Richard was to spend his career teaching in California (in Richard's case, at Cal Tech) and eventually became involved in the Unity of Science movement.
6. Tolman, "Edward Chace Tolman," p. 323.
7. For an astute discussion of the moral concerns that have motivated psychologists since early in psychology's history, see David E. Leary, "The Intentions and Heritage of Descartes and Locke: Toward a Recognition of the Moral Basis of Modern Psychology," Journal of General Psychology 102 (1980): 283-310.
8. Tolman, "Edward Chace Tolman," p. 324.
9. Ibid.
10. Ibid., p. 325.

11. The loyalty oath episode has been briefly described by Leytham, "In Memory of Tolman," pp. 27-28.

12. "Foreword," in Edward C. Tolman, Behavior and Psychological Man: Essays in Motivation and Learning (Berkeley: University of California Press, 1966), pp. i-xiv, on p. vi. (Although the authorship of this foreword is not specified, an accompanying note states that it was "prepared from suggestions and comments of several of Professor Tolman's colleagues and students.") The mention in this passage of a "small scientific theoretical system" is a thinly veiled reference to Clark Hull's "miniature systems" (see Chapter 6 below).

13. Tolman, "Edward Chace Tolman," pp. 325, 327. Philosophy and psychology did not become separate departments at Harvard until the 1930s. Tolman's exposure to the philosophers there was a decisive influence on his psychological thought, even though his dissertation was on rather narrow topics in experimental psychology.

14. Tolman, "Edward Chace Tolman," p. 329.

15. *Ibid.*, pp. 326-327.

16. Edward C. Tolman, Purposive Behavior in Animals and Men (New York: Century, 1932).

17. Tolman, "Edward Chace Tolman," p. 331. Although Tolman adopted the terms from Williams, the molecular-molar distinction was originally drawn, in a somewhat different fashion, by the British philosopher C. D. Broad in The Mind and Its Place in Nature (London: Routledge & Kegan Paul, 1925).

18. Tolman, "Edward Chace Tolman," p. 327.

19. *Ibid.*, p. 324.

20. See, for example, Edward C. Tolman, "There Is More Than One Kind of Learning," Psychological Review 56 (1949): 144-155.

21. Tolman's eclecticism and self-deprecating humor are discussed as possible causes of his failure as a "tribal leader" by his former student Donald T. Campbell, in "A Tribal Model of the Social System Vehicle Carrying Scientific Knowledge," Knowledge: Creation, Diffusion, Utilization 1 (1979): 181-201.

22. "Foreword," in Tolman, Behavior and Psychological Man, p. vi.

23. Ibid., p. xiii.

24. Neorealism (or equivalently New Realism) was inspired by the works of Franz Brentano and Alexius Meinong on the Continent, and William James in America. It was developed in England by G. E. Moore, T. P. Nunn, and Bertrand Russell. Holt and Perry, who were students and devoted followers of James, were two of its leading exponents in America. For brief expositions of neorealism, see John Passmore, A Hundred Years of Philosophy, rev. ed. (New York: Basic Books, 1966), pp. 259-280, and The Encyclopedia of Philosophy, s. v. "New Realism," by Thomas Robischon. A critical account of New Realism may be found in Authur O. Lovejoy, The Revolt Against Dualism (Chicago: Open Court, 1930).

25. Ralph B. Perry, "Professor Royce's Refutation of Realism and Pluralism" Monist 12 (1901-1902): 446-458; William P. Montague, "Professor Royce's Refutation of Realism," Philosophical Review 11 (1902): 43-44. Although Royce was the specific target of these assaults, the New Realists were generally critical of idealism, which was still a powerful force in American academic philosophy during this decade.

26. Ralph B. Perry, Present Philosophical Tendencies (New York: George Braziller, 1955), pp. 273-274. This book was originally published in 1912.

27. Edwin B. Holt, "Response and Cognition," Journal of Philosophy, Psychology and Scientific Method 12 (1915): 265-272, 393-409. Reprinted in Holt, The Freudian Wish and Its Place in Ethics (New York: Henry Holt, 1915).

28. Edwin B. Holt, Walter T. Marvin, William P. Montague, Ralph B. Perry, Walter B. Pitkin, and Edward G. Spaulding, "The Program and First Platform of Six Realists," Journal of Philosophy 7 (1910): 393-401; The New Realism (New York: Macmillan, 1912).

29. Tolman, "Edward Chace Tolman," p. 325.

30. Holt, Freudian Wish, p. 172.

31. Ralph B. Perry, "Docility and Purposiveness," Psychological Review 25 (1918): 1-20, on p. 20. Perry's works referred to in the preceding paragraph are "Purpose as a Systematic Unity," Monist 27 (1917): 352-375 and "Purpose as Tendency and Adaptation," Philosophical Review 26 (1917): 477-495.

32. Tolman's concept of purpose and its relationship to the concepts of Perry and Holt have been discussed, but without reference to neorealism, by Margaret Boden, in Purposive Explanation in Psychology (Cambridge: Harvard University Press, 1972), pp. 73-85. The extent to which Tolman ever actually clarified his concepts of purpose and cognition has been questioned by Richard F. Kitchener, "Behavior and Behaviorism," Behaviorism 5 (1977): 11-71, on pp. 32-41. On Tolman's relationship with realist philosophy, see also W. M. O'Neil, "Realism and Behaviorism," Journal of the History of the Behavioral Sciences 4 (1969): 152-160.

33. Russell's neutral monism was espoused, for example, in his Philosophy (New York: Norton, 1927). An important statement of neutral monism is contained in Edwin B. Holt, The Concept of Consciousness (New York: Macmillan, 1914), a work completed in 1908. The neorealists' various psychological views rested heavily on James's analysis of consciousness. See William James, "Does 'Consciousness' Exist?" Journal of Philosophy, Psychology and Scientific Method 1 (1904): 477-491.

34. Edwin B. Holt, "The Place of Illusory Experience in a Realistic World," in Holt et al., The New Realism, pp. 303-373, on p. 366.

35. In adopting subsistence rather than existence or reality as a fundamental mode of being, the New Realists divorced themselves decisively from naive realism and what Holt referred to as the "singularly crude brickbat notion of physical object." The ambiguity of the term "realism" was admitted by Holt: "for realism by no means everything is real; and I grant that the name realism tends to confuse persons who have not followed the history of the term." See Holt, "Illusory Experience," pp. 371, 359.

36. Holt, "Illusory Experience," passim.

37. Edwin B. Holt, Water T. Marvin William P. Montague, Ralph B. Perry, Walter B. Pitkin, and Edward G. Spaulding, "Introduction," in Holt et al., The New Realism, pp. 1-42, on p. 35.

38. Ibid., pp. 38, 39.

39. Edward C. Tolman, "A New Formula for Behaviorism," in Behavior and Psychological Man, pp. 1-8, on pp. 1-2. This essay appeared originally in the Psychological Review 29 (1922): 44-53. Watson's definitions of stimulus and response were given in John B. Watson, Psychology from the Standpoint of a Behaviorist (Philadelphia: Lippincott, 1919), p. 10.

40. Tolman, "Edward Chace Tolman," p. 339.
41. Grace A. de Laguna, "Emotion and Perception from the Behaviorist Standpoint," Psychological Review 26 (1919): 409-427, on pp. 410-411.
42. Edward C. Tolman, "A Behavioristic Account of the Emotions," in Behavior and Psychological Man, pp. 23-31, on p. 28. This paper first appeared in the Psychological Review 30 (1923): 217-227.
43. Ibid., p. 27.
44. See Richard J. Herrnstein, "Introduction to John B. Watson's Comparative Psychology," in Historical Conceptions of Psychology, ed. Mary Henle, Julian Jaynes, and John J. Sullivan (New York: Springer, 1973), pp. 98-115. This essay was originally written as the introduction to the 1967 reissue of John B. Watson, Behavior: An Introduction to Comparative Psychology (New York: Holt, Rinehart & Winston, 1967).
45. Edward C. Tolman, "A Behavioristic Theory of Ideas," in Behavior and Psychological Man, pp. 48-62, on p. 56. This article originally appeared in the Psychological Review 33 (1926): 352-369. Hyphenated expressions were highly characteristic of Tolman's inventive psychological language. Tolman later commented on this fact when he wrote: "I am amused that I can't seem to get away from neologisms and hyphens even today. One reason is, I suspect, the tremendous impact of the German language on me in the summer of 1912 when I went to Geissen in preparation for my language examinations. Good psychology and compounded nouns, as used in German, somehow became synonymous for me" (Tolman, "Edward Chace Tolman," p. 332).
46. Tolman, "Theory of Ideas," p. 57.
47. Ibid. Tolman's mention of "clawing at a loop" is in reference to well-known experiments by the American psychologist Edward L. Thorndike in which cats gained release from a puzzle-box by pulling a loop of cord that was connected to a latch.
48. Edward C. Tolman, "A Behaviorist's Definition of Consciousness," in Behavior and Psychological Man, pp. 63-68, on p. 68. This essay was originally published in the Psychological Review 34 (1927): 433-439.

49. Tolman, Purposive Behavior, pp. 170, 171.

50. Edward C. Tolman, "Behaviorism and Purpose," in Behavior and Psychological Man, pp. 32-37, on p. 33. This paper appeared originally in the Journal of Philosophy 22 (1925): 36-41.

51. Tolman, "Theory of Ideas," p. 51. The neorealist view of perception and cognition that underlies this passage finds perhaps its closest contemporary counterpart in the perceptual theory of James J. Gibson. Gibson was in fact a student of Holt at Princeton after Holt left Harvard. As he recorded in his autobiographical sketch, Gibson took to Holt's ideas "with enthusiasm." See "James J. Gibson" in A History of Psychology in Autobiography, vol. 5, ed. Edwin G. Boring and Gardner Lindzey (New York: Appleton-Century-Crofts, 1967), pp. 125-143, on p. 129.

52. Although he makes no reference to Tolman's neorealist heritage, B. F. Skinner has pointed out the spatial character of purpose in Tolman's maze studies. Skinner writes: "His experiments were designed to make purpose visible in spatial terms, in the movement of an organism toward or away from a goal object." See Skinner's "Preface to the Seventh Printing," in The Behavior of Organisms (New York: Appleton-Century-Crofts, 1938), pp. ix-xiv, on p. x. This preface was written in 1966.

53. Tolman, "Behavioristic Account of the Emotions," p. 23.

54. Edward C. Tolman, "Principles of Purposive Behavior," in Psychology: A Study of a Science, vol. 2, ed. Sigmund Koch (New York: McGraw-Hill, 1959), pp. 92-157, on p. 94.

55. Tolman, "Theory of Ideas," p. 62. The group of philosophers to whom these remarks were addressed was the Philosophical Union of the University of California.

56. B. F. Skinner credits Harry M. Johnson with having been the first psychologist to mention Bridgman's operationism in print. Skinner himself seems to have been the second. See B. F. Skinner, "The Concept of the Reflex in the Description of Behavior," Journal of General Psychology 5 (1931): 427-458. However, psychologists showed very little explicit interest in operationism until the appearance in 1935 of a number of influential papers. The most important of these were S. S. Stevens, "The Operational Basis of Psychology," American Journal of Psychology 47 (1935): 323-330; "The Operational Definition

56. (cont'd.) of Psychological Concepts," Psychological Review 42 (1935): 517-527.
57. Edwin G. Boring, A History of Experimental Psychology, 2nd rev. ed. (Englewood Cliffs, N.J.: Prentice-Hall, 1950), p. 656.
58. Passmore, A Hundred Years of Philosophy, p. 264.
59. Tolman, Purposive Behavior, p. 3.
60. See Durant Drake, Arthur O. Lovejoy, James B. Pratt, Arthur K. Rogers, George Santayana, Roy W. Sellars, and C. A. Strong, Essays in Critical Realism (London: Gordian Press, 1920). For an incisive overview of the infighting at Harvard which hastened the decline of neo-realism, see Bruce Kuklick, The Rise of American Philosophy (New Haven: Yale University Press, 1977), pp. 347-350.
61. See Edwin B. Holt, Animal Drive and the Learning Process (New York: Henry Holt, 1931); Ralph B. Perry, General Theory of Value (New York: Longmans, Green, 1926).
62. Edward C. Tolman, "Nerve Process and Cognition," Psychological Review 25 (1918): 423-442, on p. 423.
63. Edward C. Tolman, "Purpose and Cognition: The Determiners of Animal Learning," in Behavior and Psychological Man, pp. 38-47, on pp. 38, 47. This paper appeared originally in the Psychological Review 32 (1925): 285-297.
64. Tolman, "Theory of Ideas," p. 51.
65. Edward C. Tolman, "Purposive Behavior," Psychological Review 35 (1928): 524-530, on pp. 524, 525. Kuo's criticisms were expressed in Z. Y. Kuo, "The Fundamental Error of the Concept of Purpose and the Trial and Error Fallacy," Psychological Review 35 (1928): 414-433.
66. Boring, History of Experimental Psychology, p. 721.
67. Tolman, Purposive Behavior, p. 19.
68. Tolman, "Edward Chace Tolman," p. 333.

CHAPTER 4

PURPOSIVE BEHAVIORISM AND LOGICAL POSITIVISM

The preceding chapter has described Tolman's background, his early behaviorism, and the epistemological premises which underlay that behaviorism. According to the neorealist epistemology, an organism could directly cognize objects and relations in its environment, and the psychologist studying such an organism could directly cognize its cognitions. Since the "mind abroad" was taken to be mind itself and not merely outward manifestations from which mind might be inferred, Tolman saw that mental phenomena could be defined in terms of, in fact identified with, the "externals of the situation." This strategy of definition, frequently used by Tolman during the twenties, amounted to an early version of operationism. This was an operationism which unlike the "ametaphysical" operationism of the mid-thirties had the metaphysical support of a philosophy of direct realism. With the demise of neorealism, however, Tolman's proto-operationism lost its philosophical support and he was confronted with the challenge of revising his epistemological views; but he needed to do so without abandoning the program of objectifying and naturalizing mentalistic concepts.

The aim of this chapter is to characterize Tolman's relationship to logical positivism, particularly in the context of the role played by logical positivism in Tolman's response to the challenge facing his behaviorism at the turn of the decade. To do so, it will be necessary also to examine the probabilistic functionalism of Egon Brunswik, for the influence of logical positivism on Tolman is intimately bound up (both historically and conceptually) with the influence of Brunswik on him. But the twin influences of probabilistic functionalism and logical positivism were ones which had already been anticipated in the development of Tolman's own thought; that is, they were influences that reinforced Tolman's ideas, not ones that instigated or formed them. Accordingly, the present chapter begins with accounts of Tolman's psychological and philosophical views as they stood just prior to his trip to Vienna in 1933-34.

Tolman's Thought: Pre-Vienna

The Shift from Immediate to Mediate Cognition

As was described in the preceding chapter, Tolman vacillated during the period 1925-35 over whether purposes and cognitions could be directly known by an observer of behavior or whether they had to be inferred from that behavior. Over this ten year span there was in Tolman's thinking a gradual transition from the former alternative

to the latter. Part of this change can no doubt be attributed to Tolman's awareness of the fate of neo-realism: By 1920, the critical realists had laid bare the implausibility of much of the New Realism and had shown that it could not adequately account for error in the knowledge process without making some concessions to dualism. But by the mid-twenties, Tolman was focusing more on his experimental research than on purely philosophical matters. It is thus appropriate that a major impetus for his shift toward viewing cognition as a mediate process came from his studies of rats in mazes.

In a paper of 1925, Tolman reported a set of experiments in which rats were placed in a simple T-shaped maze and reinforced indifferently for selecting the right or left arm of the maze. Under such conditions, the rats showed stable strategies of responding, e.g., always choosing the left or consistently alternating left and right. Tolman referred to these persistent patterns of responding as "cognitive hunches," and said that such behavior "can be observed quite definitely to impute (whether correctly or incorrectly)" a structure to a particular part of the maze.¹ In 1926, Tolman was claiming that behavior "postulates, expects, makes a claim for" whatever feature of the environment is being responded to. In 1927, he was speaking not only of "postulations" of objects by behavior but also of "representations" of goal

objects. He wrote, "To make an adjustment to an act is to achieve a representation (based, of course, upon what has happened upon previous occasions when this act or similar ones have actually been performed) of the probable stimulus results to be expected from the act."²

In these reformulations, Tolman was moving toward the general notion that knowledge of an environment is mediated by claims or postulations about, or representations of, features of that environment. In other words, the rat was beginning to be conceived as playing an active role in constructing knowledge of the world and not as simply receiving direct presentations of the world. It is a symptom of Tolman's vacillation on the issue of direct versus indirect cognition that he was still at the same time claiming that the rat's purposes and cognitions, and even its postulations, could be directly read off its behavior. But there was a degree of inconsistency in holding that the psychologist's objective reference to the rat's cognitions could be direct and immediate while the functional reference of the rat's behavior to its environment was mediated by postulations. Apparently, Tolman recognized this difficulty, for it was around this time that he began to admit, although not without equivocation, that the purposes and cognitions of another organism have to be inferred by the investigator from its behavior. Although Tolman was moving away from the direct realism

of the neorealists, he was still striving to observe their maxim that the observing organism and the observed organism operate at the same epistemological level.

This attempt to maintain a consistent epistemology was even more pronounced after the turn of the decade. In the early thirties, Tolman's student Ivan Krechevsky (later to be known as David Krech) undertook a series of studies³ on the persistent response patterns which Tolman had earlier called "cognitive hunches." In the course of analyzing these patterns and their modifiability under different conditions of reward, Krechevsky began to refer to them as "hypotheses" in much the same way that Tolman had viewed behavior as "making a claim" or "postulating" about the environment. But Krechevsky's work went further because it emphasized the control of the behavior patterns by the conditions of differential reinforcement. A good hypothesis could be confirmed by reinforcement and a bad one could be disconfirmed by nonreinforcement. A hypothesis could be shown to be in error and thus had to be regarded as always tentative and fallible.

The tentative and fallible character of hypotheses pushed Tolman further toward the recognition that the purposes and cognitions of another organism must be imputed to that organism. Such imputations, like those of the observed organism itself, had to be regarded as tentative and subject to error. This suggested for purposes and

cognitions a hypothetical rather than observed status. In a joint paper which appeared in 1933, Tolman and Krechevsky referred to cognitions as variables which intervene in functions relating the observable variables of stimulus and response. But even this formulation was subject to equivocation, for they also spoke of cognitions as immanent in the functions relating observables, or more specifically as the forms of those functions.⁴

Although Tolman accepted Krechevsky's notion of hypotheses, he also developed in his own system the analogous concept of the sign-gestalt-expectation. This was, in fact, the fundamental concept of Purposive Behavior. The sign-gestalt, it will be recalled was the unit of learning in which the sign-object, means-end-relation, and signified object were fused into a single pattern. But, Tolman now reasoned, this pattern itself may be immediately cognized or postulated; that is, the organism may form a sign-gestalt-expectation. Like a hypothesis, this expectation (which is an organic event) may be confirmed or disconfirmed depending on the nature of the actual sign-gestalt (which is any "objective environmental complex").⁵ Like a hypothesis, a sign-gestalt-expectation had to be inferred from behavior, although Tolman was reluctant to admit it, and such an inference required a fallible imputation, hypothesis, or expectation on the part of the observer. The sign-gestalt-expectation

was clearly an "immanent determiner" of behavior, one of the cognitive variables which was said to intervene in functions relating behavior to its antecedent conditions. But, as we have seen, Tolman had not in 1932 completely settled the issue of whether such variables had a hypothetical or observable status.

Tolman later reflected on the development of the intervening variable paradigm as it stood in Purposive Behavior. The "immanent determiners" of which he had spoken in that book

were my first step toward what I later conceived as 'intervening variables.' I felt vaguely at that time that the cognitive and purposive features of behavior, which I was postulating, were somehow statements about the shapes of the functions connecting the final dependent behavior to its various independent determiners of environmental stimuli and physiological drive states. Therefore I said the cognitive and purposive features were "immanent" in these connections or functions. It was only later that I hit upon the notion of breaking up the total functions into two or more successive steps and inserting 'intervening variables' . . . between such successive steps or functions.⁶

The revised notion of intervening variables was first expressed by Tolman early in 1935, the year following his return from Vienna. As we shall see, it thus seems likely that he "hit upon" the intervening variable paradigm under the influence of logical positivism.

The present section has described the transition in Tolman's views from a direct realism to a mediated cognitivism. In the concluding remarks of Purposive

Behavior, Tolman confessed to propounding an epistemological dualism. He wrote that

in terms of our system, idea and object are two. It is clear that discriminanda-, manipulanda-, and means-end-readinesses and -expectations are logically, and usually also temporally, prior to the realities which would verify them, or in cases of error, fail to verify them. . . . Our doctrine is, therefore, in this degree, an epistemological dualism.⁷

In this important respect, Tolman had abandoned neorealism. This abandonment had begun with the attribution of hunches and postulations, and later hypotheses and expectations, to his experimental subjects. While attributing indirect cognitions to his rats, he still clung for a brief time to the neorealist belief that the rat's purposes and cognitions could be directly cognized by the investigator. But his even deeper conviction that the observer and the observed must be granted an equal epistemological status gradually took precedence. The demand for parity meant that the scientist, too, had to hypothesize and infer in order to know the world. In large measure, Tolman was being led by his rats to rethink and (ironically) to complicate his own epistemology.

As has been pointed out, neorealism provided just the epistemological leverage needed for a behaviorism to objectify complex mental phenomena. Under appropriate conditions, it was believed, such phenomena could be directly and unambiguously known. But this proved to be an unstable position, because it led to findings which

reflected back unfavorably on the presuppositions. Tolman had observed that his rats could be in error, that their postulations could be disconfirmed and altered by experience. To account for these facts, he had to attribute underlying determinants to their behavior. But since such determinants were, strictly speaking, unobservable, his attributions of them became likewise subject to ambiguity and error. The neorealist assumptions, which had never been able to adequately account for error in the knowledge process, could no longer be relied upon to guarantee a direct knowledge of another's cognitions. For scientists as for rats, the seeking of knowledge therefore became an ambiguous, error-prone, and risky enterprise. What Tolman needed, then, was some means of reducing the risk of error, perhaps a sophisticated operational method to govern the imputation of unobservable mental properties.

Contextualism, Pragmatism, and the Ineffability Doctrine

Tolman's adoption of an epistemological dualism was a development with deep implications, but it by no means constituted a shift to any sort of ontological dualism. As Tolman put it in Purposive Behavior, "Our doctrine is not . . . a transcendentalism or a metaphysical dualism."⁸ Tolman continued to view himself, as he had all along, as an adherent of a pragmatic naturalism, or in the

terminology of Stephen C. Pepper, of a "contextualism." Pepper's contextualism was a version of pragmatism updated with strains of Gestalt psychology and Tolman's own purposive behaviorism. The mutual influence of Tolman and Pepper on each other throughout their long and parallel careers was substantial. They had both been students of Ralph Barton Perry at Harvard, where Pepper finished his Ph.D. in 1916, just a year behind Tolman. Pepper became a professor at Berkeley in 1919, again just a year behind Tolman, and the two transplanted Easterners remained there for the duration of their careers. During this time they were close friends who found in each other an ongoing source of ideas.

Pepper developed contextualism primarily as a basis for his esthetic and value theory, but these theories in turn rested largely on Perry's and Tolman's notion of the purposive act. As a consequence, contextualism showed a strong kinship with Tolman's purposive behaviorism. As Pepper himself noted, "what is owing to Tolman is so interwoven with my own ideas that there can be no unravelling of it."⁹ In contextualism, an environment was said to be composed of textures, each of which was a sort of complex Gestalt of patterned relations of events. Each texture, despite its wholistic character, could be analyzed into strands which "extend into environmental textures."¹⁰ A strand could be followed up, analyzed,

or operated on in the context of any one of the textures to which it belonged, but it could never be analyzed in isolation from any texture. Contextualism, then, was the generalized, explicitly philosophical formulation of the basic notion which Tolman was already operating under: the elements of learning--stimuli, responses, and goals--cannot be understood except as they are functionally bound together in a single complex whole.

As a species of epistemological dualism, contextualism distinguished between a sign, cue, or idea and its referent; but the relation of reference between them was far from the usual dualistic notion of reference. In his lengthy commentary, written in 1933, on Purposive Behavior, Pepper described the contextualist notion of reference.

Analysis is an operational affair, a matter of following references from one texture to another. There is no assumption of similarity between the analyzed texture and the analyzing texture. If you call the latter texture the idea of an object, and the former texture the object, you have the type of all knowledge for a contextualist. The idea of an object is an instrument that will guide you to the object. The idea symbolizes the object in that sense. It does not need to copy the object in any manner whatever. To know an object does not mean that you have a picture of it. It simply means that you have an instrument by means of which you can obtain the object.¹¹

Although Tolman never stated it so baldly, this was essentially his view also. Whether the relation was between percept and object, sign-object and signified object, or sign-gestalt-expectation and sign-gestalt, it

was not one of representation in the usual sense but rather one of leading-on-ness. A strand could be followed from one point to another, but the first point was a guide to, not a reproduction of, the other. Pepper wrote that "perception is not an inner 'idea' corresponding in some puzzling way with an 'outer' object; it is a traceable relation of strands in a texture."¹² Similarly, Tolman held that in perception "the smallest unit of experience is not just a free sensory-perceptual pattern but such a pattern suffused with instrumental meaning."¹³ Perception is thus bound to a context of leading-to and -from and is necessarily relative to the goals and needs of the organism as well as its past.

If this principle was believed to hold for perception, then it applied all the more clearly to more complex forms of cognition. A rat's hypothesis concerning the locus of a goal and the means to it was not a passive description or representation of a goal in a maze but an instrument for getting to it. As such, the hypothesis was perforce conditioned by the drives and history of the organism. Tolman wrote in 1926 that

we may liken the environment to a multidimensional spider's web radiating out from the behaving organism in many directions. The far ends of the threads terminate in the to-be-sought-for quiescences of final to-be-avoided disturbances. Environmental objects and situations are responded to and cognized only in their character of providing bridges or routes along these threads.¹⁴

In operating on such a complexly textured environment, an organism would need to employ hypotheses and expectations about the various strands or routes because the final ends of the strands could simply not be known directly. Cognition in a contextualist environment became a guided movement along a strand of a texture, an operation mediated with varying degrees of success by the cognitive instruments of hypotheses and expectations.

But as we have seen, Tolman viewed the activities of the scientist as like those of the rat. Consequently, he extended the contextualist notion of cognition to scientific knowledge. This was done near the end of Purposive Behavior in a section titled "Methodology and Status of Science." Therein, Tolman addressed the issue of "the ultimate methodology and status of science which we have been adopting,"¹⁵ and developed his answer in the context of Pepper's philosophy. Just as the rat is guided through its textured environment by expectations, so the scientist explores the world with the help of a theory, or in Pepper's terminology, a "map."

All science necessarily presents, it seems to us, but a map and picture of reality. If it were to present reality in its whole concreteness, science would not be a map but a complete replica of reality. And then it would lose its usefulness. . . . One of the first requisites of a science is, in short, that it be a map, i.e., a short-hand for finding one's way about from moment of reality to the next--that it be a symbolic compendium by means of which to predict and control.¹⁶

Having come to acknowledge the mediate character of perception and cognition, Tolman was led to interpret theories in an instrumentalistic fashion. Without the possibility of direct knowledge of the world, the best a scientist could hope for was to construct a useful map. Like any other map, a theory could be constructed only relative to certain purposes and would necessarily leave out certain features of the environment being mapped.

Tolman agrees with Pepper that scientific naturalism was but one among many viable metaphysical systems for what Pepper termed "root metaphors." Naturalism itself was a kind of general map which leaves something out and is therefore necessarily incomplete and relative to certain ends. Tolman wrote:

Naturalism is the type of metaphysics which takes the features of prediction and control as all-important. And we are adopting the naturalistic position, but we are going further and admitting with Pepper that it is only a map. We tend, however, it may be noted, to differ from Pepper in that we believe the naturalistic map to be the only map. The other maps, i.e., mysticism, idealism, etc., seem to us to be not maps but poems. . . . They are momentary attitudes, not expandable into complete maps. Prediction and control are the very essence of 'mappishness.'¹⁷

For Tolman, naturalism was one viable root metaphor among others, but it alone could be said to provide maps in the proper sense of the term.

What was left out of the naturalistic map, according to Tolman, were the "raw feels" which make up the "concrete,

but ineffable, richness of real experience, as it comes."¹⁸ In scientific maps, the "raw feels" have no place and can simply be ignored. "They are mere scientific will-of-the-wisps," wrote Tolman. "They are subject matter for poetry and esthetics and religion, not for science."¹⁹ The objection to "raw feels" was one that Tolman had voiced as early as 1922 and then frequently thereafter: any such pure experiences could not be differentially responded to or talked about and thus had no place in science.²⁰ In Purposive Behavior, Tolman simply quoted the philosopher C. I. Lewis on this matter:

In the end, the supposition of a difference in immediate experience [i.e., our raw feels] which is not to be detected through divergence in discrimination and relation, is a notion very difficult to handle. Because such a difference would, ex hypothesi, be ineffable, we can have no language for discussing what no language or behavior could discriminate. And a difference which no language or behavior could convey is, for purposes of communication, as good as non-existent.²¹

This doctrine of the ineffability of immediate experience was a recurrent theme in Tolman's writings as well as a major point of Lewis's Mind and the World-Order (1929).

Like Tolman and Pepper, Lewis had been a student of Perry at Harvard, and like them he had gone to Berkeley to teach after graduating. He and Tolman were colleagues there in 1918-20 and continued to stay in touch after Lewis's return to Harvard in 1920,²² Tolman read and admired Lewis's Mind and the World-Order, a work which

went to press in 1928 and which independently developed many of the positions concurrently being worked out in the Vienna Circle. Lewis's philosophy is, in fact, often regarded as a version of logical empiricism.

But Lewis identified his philosophy as a "conceptualistic pragmatism," and if it was a version of logical empiricism, it was one with important differences arising from his pragmatism. For present purposes, there are two such differences to be noted. First, Lewis did not accept the logical positivist notion of logic as an empty tautology. He attributed to logic an a priori status but emphasized its pragmatic character. Logic, he said, is a useful instrument for acting in the world because it can be used to analyze the consequences of alternative actions regardless of which alternative might actually be chosen. In fact, the utility of logic is grounded in its capacity for illuminating possible but nonexistent states of affairs. Logic is a tool of thought, and thinking is for Lewis "an activity by which we adjust ourselves to those aspects of the environment which are not immediately apprehended in experience."²³

The second important difference between the standard versions of logical positivism and Lewis's philosophy concerns the empirical foundation of knowledge. The logical positivists had spoken of single atomic propositions or Protokolsätze (formulated in either physicalistic or

phenomenalistic language) as the basis of empirical knowledge. For Lewis, such unitary expressions of experience were not a form of knowledge at all. Empirical knowledge was rooted, rather, in a type of observation statement that was conditional in form. Such a statement which later became known as a "terminating judgment," was of the form: given some sensory cue, if some action is taken, then some expected result will occur. For example, a terminating judgment might be "given an impression of red directly before me, if I turn my head to the right, then the red will move to the left in my visual field." The most basic kind of knowledge is thus relational and active rather than atomic and passive. As with the pragmatistic views of knowledge found in Perry, Pepper, and Tolman, Lewis maintained that knowledge involves a cue, an action, and an actual or expected outcome.²⁴ If there were any such thing as a "dead given" in experience, it could not count as knowledge without having entered into a relation with action and outcome. Lewis's philosophy was thus a logical empiricism with a pragmatic twist in both its logical and empirical aspects.

Tolman's familiarity and sympathy with the philosophical views of Pepper and Lewis are important clues for understanding his relationship with logical positivism. Pepper's contextualist metaphysics, which was essentially the same as Tolman's own metaphysics, provided the

philosophical basis for Tolman's convergence with Brunswik's probabilistic functionalism and thus the framework to which Tolman eventually assimilated logical positivism. Lewis's conceptualistic pragmatism was the brand of logical empiricism to which Tolman was exposed and predisposed before his contact with the European version. Significantly both contextualism and conceptualistic pragmatism were far more pragmatic, and less linguistic, in thrust than European positivism. In their similarities to logical empiricism, they foreshadowed Tolman's positive response to it, and in their differences from it, they adumbrated the limits of that positive response.

Schlick at Berkeley

In 1931, the Vienna Circle leader Moritz Schlick went to Berkeley as a visiting professor. Along with Herbert Feigl, who went to Harvard in 1930, Schlick was part of the vanguard that brought logical positivism to America. Having been under the influence of Wittgenstein since 1927, Schlick was avidly propounding a strongly Wittgensteinian form of positivism at the time of his stay in California. It was during this visit that he delivered his influential lecture "The Future of Philosophy," in which he declared that the task of future philosophers would be to work with scientists in clarifying the meanings of scientific claims. It was also

during this visit that he became personally acquainted with Pepper and Tolman.²⁵

By 1931, Schlick and Tolman had already developed similar ideas on a number of issues and had even advanced similar arguments for them. As early as 1925, Schlick had analyzed the nature of purposive activity and argued against vitalists such as Hans Driesch that the "concept of purposiveness which is perfectly sufficient for the description of the facts of biology contains nothing that transcends, in principle, the processes and laws characteristic of inorganic matter."²⁶ Like Tolman, he had sought to "list the characteristics" of organic behavior which lead one to speak of purposiveness. And like Tolman, he associated purposiveness with a pattern of responding with respect to some outcome. Schlick wrote that

a group of processes or organs is called purposive with respect to a definite effect, if this effect is the normal effect in the cooperation of the processes or organs. The accent here is on co-operation; in a scientific case, these processes, depending upon the circumstances, may occur in a variety of ways, but they are dependent upon one another and linked together in such a way that on the whole the same sort of effect always ensues. . . .

Purposiveness, therefore, is tantamount to a certain type of relation, interaction, or concatenation.²⁷

Schlick went on to give purposiveness, qua the cooperation of processes with respect to some end, a functionalist interpretation in terms of the preservation and development of the organism.

At the time of his visit to Berkeley, Schlick was also advocating a version of the ineffability doctrine, which as we have seen was subscribed to by Tolman. Following Wittgenstein, Schlick drew a sharp distinction between the form and the content of experience. Language was said to be capable of expressing only the form or structure of experience; the content of experience, the "raw feels" discussed by Tolman, were viewed as inherently inexpressible or ineffable. Consequently, communication could involve, at most, the transmission of the structure of an experience. The recipient of the communication had to provide the content from his own immediate experience.

As Schlick put it:

What you call the 'understanding of the true meaning' is an act of interpretation which might be described as the filling in of an empty frame: the communicated structure is filled with content by the understanding individual.²⁸

As Tolman had done, Schlick emphasized this point with reference to the experience of colors.²⁹ The experience of greenness, the pure enjoyment of the quality, was taken to be inexpressible for purposes of communication. One could never know, or even meaningfully ask, whether one organism's experience of greenness was the same as another's. Only the structure of the experience could be communicated, and in the case of colors this meant that two observers could agree at most on the standing of

green relative to other colors. This relative standing would necessarily be revealed as the structure of their expressions or the pattern of their responses in the presence of a green stimulus.³⁰

For both Schlick and Tolman, the doctrine of ineffability thus led in the final analysis to a sort of behaviorism. The structural character of all knowledge meant that it was ultimately to be expressed as a set of responses, usually linguistic ones. In 1932, Schlick stated that psychological facts, like physical facts, were expressible only by repeating their structure in propositional form. He continued:

Old-fashioned psychologists used to think that we can "know" more about our own minds than about other people's, because only our own mind can be investigated by introspection. But this view rests again on a confusion of intuition and knowledge in the legitimate sense of the word. What we really know by introspection, can be expressed in our propositions and if this is the case we can learn just as much from the propositions in which other persons describe their own mental life, and from other manifestations in which that life expresses itself. As all bodily manifestations, including speech, form part of a person's behaviour, we may maintain that all psychological truths rest on behaviour as their only and absolutely sufficient basis.

If it is this, and nothing else that is implied by the doctrine of 'behaviourism' (of which I am not sure) the behaviouristic view seems to be absolutely unassailable.³¹

Schlick's behaviorism, like Tolman's, thus admitted introspective reports as valid evidence for a psychological science but not as evidence having a special status.

Schlick among logical positivists, and Tolman, among behaviorists, were both relatively undogmatic in their views. Schlick arrived at his behaviorism from mainly linguistic considerations and Tolman his largely from considerations of comparative psychology, but both arrived at a relatively catholic version of behaviorism. Both had been realists in the twenties and had moved toward positivism in the thirties, but even then their positivism and instrumentalism were tempered with realist leanings. All told, they had a lot in common in terms of their intellectual views and their open and flexible style of defending them.

Perhaps their greatest common ground was in their views of how empirical significance is to be achieved for the claims of science. According to Schlick, all proper knowledge is communicable structure, but the language which expresses this structure must end at some point. To be sure, the terms of a language can be defined with reference to other terms, but this process will eventually come up against the limits of language. As Wittgenstein had insisted, the process of definition must always terminate in the act of pointing. In "The Future of Philosophy," Schlick stated:

All of our definitions must end by some demonstration, by some activity. . . . The discovery of the meaning of any proposition must ultimately be achieved by some act, some immediate procedure, for instance, as the showing of yellow; it cannot be given in a proposition.³²

The notion that the contents of experience can be shown but not expressed had already been clearly formulated by Tolman. As we have seen, he had stated in 1926 that "human beings . . . can not convey per se private mental contents to their fellows. . . . all they can do is to point and grossly to behave."³³ Schlick and Tolman had arrived at the ineffability doctrine from rather different directions, but the outcome was much the same: empirical significance boiled down to the act of ostension.³⁴ Schlick and Tolman alike were thus led to a type of operationism. With his linguistic emphasis, Schlick formulated the operationalist conclusion in terms of the verification of propositions: "The Meaning of a Proposition is the Method of its Verification." But he also formulated it in terms more amenable to the pragmatist climate of America when he said that "a proposition has meaning for us only if it makes some kind of difference to us whether it is true or false."³⁵ It was this formulation, with its echo of Peirce and James, that was clearly applicable to Tolman's conception of scientific claims as constituting a map for the guidance of organismic activity.

It is unclear just how well Tolman knew Schlick and his ideas in 1931. Tolman's assertion in Purposive Behavior that mysticism and idealism "seem to us to be not maps but poems" appears to be one reflection of

Schlick's influence.³⁶ Most important for Tolman was the fact that Schlick, a prominent physicist and philosopher, was offering a fairly detailed account of how theories acquire their empirical content. Even the great "hypothetical-deductive" systems of physics, according to Schlick, make their final contact with reality by a complicated act of pointing.³⁷ In 1931, as we have seen, Tolman was still clinging to the hope that, in some way, purposes and cognitions could still be defensibly construed as "point-at-able" features of behavior. Perhaps the new empiricism of the Vienna Circle, where theoretical systems were being treated with the tools of modern logic, would provide the key to resolve Tolman's difficulties with empirical definition.

Once Purposive Behavior was published, Tolman took a full sabbatical during which he went to Vienna, where he stayed for a period of seven months.³⁸ There was obviously much in Vienna to draw Tolman's interest. His trips to Giessen in 1912 and 1923 had awakened him to the richness of the European psychological tradition. Vienna was the center of psychoanalytic theory, a topic of increasing interest to him. It was also the home of the Pedagogical Institute headed by the influential psychologists Karl and Charlotte Bühler. The Vienna of this period has been described as a "mecca," and it has been pointed out that "after the First World War Vienna

began to rival the German universities as an attraction to American psychologists."³⁹ But for Tolman, it appears to have been the Vienna Circle that provided the major impetus for going there. Edna Heidbreder has written that

Tolman's interest in logical positivism antedated his year in Vienna in 1933-34. He chose to spend his sabbatical there because of his interest in logical positivism and because he wanted to become familiar with it at its source.⁴⁰

The problems facing Tolman in his psychological theorizing were for the most part psychological ones, but they involved issues in the methodology and philosophy of science. The system of psychology presented in Purposive Behavior was a complex one involving relatively high-level cognitive concepts, and he began to look for outside help in ensuring their empirical definability. Such were the issues being dealt with in the Vienna Circle, and Tolman's personal acquaintance with its acknowledged leader provided his entree into the group.

Tolman's Thought: Vienna and After

Brunswik's Probabilistic Functionalism

Like Tolman, Egon Brunswik (1903-1955) turned to psychology after studying engineering. As a graduate student at the University of Vienna, he studied under Karl Bühler and came under the influence of Moritz Schlick.⁴¹ Upon receiving his doctorate under Bühler

in 1927, he became a research assistant at Bühler's Psychological Institute and in 1934 was made a Privatdozent at the University of Vienna. He also attended meetings of the Vienna Circle during these years.

As we have seen, Tolman had by the time of his visit abandoned his earlier direct realism in favor of an epistemological dualism. When he met Brunswik during his sabbatical there, Brunswik was also propounding an epistemological dualism, especially in the context of perception. As a student of Bühler, Brunswik stood in the European functionalist tradition which stemmed from the work of Franz Brentano. Just as Brentano and Bühler distinguished between the knower and the known, and yet held them to be functionally related, so Brunswik distinguished between a perceiver's "intending" (or focusing upon) an environmental object or situation and the perceiver's "attaining" of the object or situation.⁴² When the perceiver's cues (or "proximal" stimuli) and the to-be-perceived object ("distal" stimulus) stand in a strong functional relationship, the perception is relatively successful and exhibits its achievement character. But as Brunswik's own research showed, perception occurs with various degrees of success depending on the relative validity of the cues afforded by the environment; intendings become attainings only when the organism is able to exploit reliable cue-object correlations in the world. Just as Tolman's newfound

epistemological dualism made room for error in the knowledge process, so did Brunswik's functionalist dualism allow for the fallibility of perception.

Tolman and Brunswik recognized the strong similarities between their conceptual systems. Tolman had addressed the problem of knowledge from the response side and Brunswik had addressed it from the stimulus side, but they saw their respective approaches as complementary and mutually supportive. Brunswik's distinction between the intended and the attained was paralleled in Tolman's distinction between expectation (or hypothesis) and confirmation. In each case the former was a kind of psychological act and the latter was the (by no means inevitable) consequence of that act. Brunswik's perceptual intention-
alism was the complement of Tolman's behavioral purposivism.⁴³

But the similarities in their approaches went even further. Where Tolman emphasized that molar behavior achieves stability through the intersubstitutability of particular responses in reaching goals, Brunswik stressed the intersubstitutability of perceptual cues in the achievement of perceptual stability. Brunswik's extensive research on object constancy had demonstrated how perceivers could switch to the use of alternative cues or signs when previously used ones became unreliable or unavailable. Brunswik came to refer to this process,

which was equivalent to Tolman's "multiple trackness" of means-end relations, as "vicarious mediation."⁴⁴ Vicarious mediation is necessary, according to Brunswik, because any single cue-object relation in the environment is, at least to some degree, ambiguous. Likewise, for Tolman any particular means-end relation (such as a path in a maze) is likely to be unreliable (as when a path is blocked). For both Brunswik and Tolman, then, efficient adaptation to an environment requires the learning and utilization of a set of alternatives by which a perceptual object or goal may be achieved.

The major common features of Brunswik's and Tolman's approaches--their epistemological dualism, the inter-substitutability of means-end relations, and the ambiguity of individual means-end relations--were features that were integrated into their respective functionalist metaphysics. Again, they recognized the strong kinship of their functionalist world-views. For both, psychology is the study of the adaptation of organism to environment. The organism-environment relation is not direct but rather mediated through the positing, and subsequent correction, of intendings and hypotheses. The relation of knower to known is an activity, a functional relation, not a static logical relation; and because of the vagaries and ambiguities of the environment, this relation can be defined but not perfected.

For Tolman, these views were all part of his contextualist metaphysics. On the analogy of the multi-dimensional spider's web, knowing became a matter of choosing and following up strands or routes leading to goal states. Because of the multiplicity of these routes and the distances they often involve, hypotheses are needed to guide the organism's passage through the web. In a similar vein, Brunswik embraced a metaphysical view that fit with his functionalist psychology. He was drawn to the methodological objectivism of the Vienna Circle, but the Circle's insistence on the univocality of reference in scientific language and the determinate logical structure of scientific laws conflicted seriously with his own psychological research and theory. The one version of logical positivism that was consistent with Brunswik's psychological view of knowledge was Hans Reichenbach's probabilistic epistemology, and it was Reichenbach's approach that Brunswik integrated into his functionalist framework.⁴⁵ Following Reichenbach's notion that multiple causes can statistically produce a single effect, Brunswik came to picture the environment as composed of relations of "partial causes and partial effects"--relations not unlike Tolman's strands of texture. The organism was portrayed by Brunswik as an "intuitive statistician" whose behavior is based on "the probabilities, or past relative frequencies, of relevant interrelation-

ships lumped together." Analogous to Tolman's hypotheses and Brunswik's intendings were Reichenbach's posits. As Brunswik wrote: "All a finite, sub-divine individual can do when acting is--to use a term of Reichenbach--to make a posit, or wager."⁴⁶ For Brunswik and Tolman, the knower and the known are connected by functional relations, relations which their own respective research had shown to be fallible, and they accordingly saw Reichenbach's probabilistic epistemology as amenable to their own psychological formulations of knowledge.

During Tolman's stay in Vienna, he and Brunswik met regularly at the local coffeehouses to work out a joint statement of their theoretical position.⁴⁷ The result was a major article which appeared in the Psychological Review of 1935 under the title "The Organism and the Causal Texture of the Environment." For Tolman and Brunswik, the molar phenomena of psychology were regarded as dependent on the environment, so they devoted much of their paper to a characterization of the environment. They wrote that the "whole uncertainty of knowledge and behavior arises . . . out of . . . equivocality (Mehrdeutigkeit) in the causal surroundings."⁴⁸ In a passage which revealed their functionalist metaphysics, they depicted the environment as follows:

Consider for a moment the nature of the causal connections in the physical world independent of organisms. We observe that, whenever any individual event occurs, a more or less extended complex of many independent part causes must have been existentially operative. Further, any specific type of event will on different occasions and in different places have different causes, or more exactly speaking, different total complexes of part causes. And also, vice versa, any given type of an event will itself operate as a part cause on different occasions and in different places for the production of different final total events. The causal interweavings of unit events among one another are thus in both directions, equivocal. But some of these connections will be more probable than others.⁴⁹

This passage ended with a reference to Reichenbach's earlier work on "the nature of the causal structure of the world in general."⁵⁰ But in such phrases as "total complexes of part causes" and "causal interweavings," the passage also shows the influence of Pepper's contextualism. Indeed, Tolman and Brunswik acknowledged the contextualist influence at the outset of the paper: "For the term 'texture' as well as for advice on various other English terms we wish to express special indebtedness to Professor S. C. Pepper."⁵¹ The views of Reichenbach and Pepper were clearly compatible and were readily brought together as an underpinning for Tolman and Brunswik's psychologically derived functionalist world-view. In the remainder of their joint paper, Tolman and Brunswik elaborated the parallels between their molar psychologies, laboriously matching one by one Tolman's neologisms with their German equivalents (e.g.,

"means-objects" with "Mittel-gegenstände").⁵²

Although at the time of their meeting Tolman and Brunswik had already developed very similar positions, they clearly had different emphases and their encounter thus involved discernible influences as well as the mutual reinforcement of their general positions. As mentioned above, Brunswik came to molar psychology from the perceptual side and Tolman from the behavioral side. Their interaction had the effect of broadening Brunswik's perspective to include action and Tolman's to include more consideration of perceptual issues. Brunswik's stress on the equivocality of perception and knowledge led to a deepening of Tolman's epistemological dualism, as reflected in his increasingly sharp distinctions between secondary and primary qualities, hypotheses and confirmation, and even values (subjective) and valences (objective).⁵³ Tolman's emphasis on objective behavioral methods led Brunswik, who was already sympathetic to logical positivism, to a more thoroughgoing advocacy of methodological (but not thematic) physicalism and to the endorsement of operational criteria (although he continued to insist that such criteria can be applied only probabilistically).⁵⁴

The relationship between Tolman and Brunswik proved to be as congenial personally as it was intellectually. At Tolman's instigation, Brunswik spent the year 1935-1936

as a Rockefeller Foundation Fellow at Berkeley, where he lectured and conducted research. He then returned to Vienna for a year only to be invited back to Berkeley in 1937 to take a position as assistant professor. There he remained active both as a psychologist and, as will be described below, a contributor to the Unity of Science movement until his death in 1955.⁵⁵

Of all the views that Brunswik and Tolman held in common, perhaps the most important in the present context is that both of them were sensitive to the demands placed on methodology by psychological theory. Scientific methods need in general to be tailored to the subject matter at hand, but in psychology there is the further consideration that every finding and theoretical adjustment carries implications for the nature of knowing and hence, at least potentially, for the methods of the scientist. Brunswik has been hailed for his originality in deriving his method from his theory;⁵⁶ but, although he was unusually explicit in this respect, he was by no means unique. As was shown previously, Tolman's early operational methods came out of his theoretical views on the psychology of cognition. Under the impetus of his experimnts with Krechevsky on hypotheses, those views were undergoing revision at the time of his sabbatical in Vienna. In what follows, it will be argued that Tolman's new version of operationalism--a sophisticated

operationalism that he elaborated in a series of papers shortly after his trip to Vienna--was likewise based on psychological concerns. To be sure, the new operationism bore clear earmarks of logical positivist influence, but logical positivism was being accommodated to the overall framework of Tolman's cognitive behaviorism rather than vice versa.

Logical Positivism and Ontological Neutrality

As we have seen, there is good reason to believe that Tolman attended meetings of the Vienna Circle during his sabbatical in Vienna. Unfortunately, however, Tolman (unlike Hull and Skinner) did not keep personal records concerning his career, and there is little evidence from other sources about his experiences in Vienna.⁵⁷ Such details as the frequency of his attendance at meetings of the Vienna Circle, the degree of his participation in them, and the nature of his interactions with its members remain unknown. It is possible, nonetheless, to reconstruct on the basis of his published works the sorts of positivistic influences he came under during his stay in Vienna. These influences are clearly reflected in his articles of the mid-thirties, even though, as with the many other influences Tolman absorbed in the course of his intellectual career, the strains of Viennese positivism were assimilated to his own conceptual framework

for psychology. In this section and the next one, the impact of logical positivism on Tolman's thought will be discussed. The nature of that impact, it will be seen, was more to corroborate, extend, and refine already existing aspects of his thought than to instigate new ones. The final section of this chapter will be devoted to Tolman's involvement in the Unity of Science movement.

Tolman's sympathies with positivism in general were conditioned by his earlier abandonment of direct realism and his growing emphasis on the relativity of perception. Especially after his contact with Brunswik, he was acutely aware of the gap between what is presented in experience and the object of perception. The imperfection and ambiguity of the relation between the two meant that the objects of study in the world could not be directly grasped in the process of scientific observation. Under favorable conditions, certain proximal cues may serve as relatively reliable signs for objects, but immediate experience consists of a confusing welter of sensory qualities. To find one's way about in such a sensorily rich flux of experience, Tolman argued, one must seize upon the most reliable of the available cues and use them to construct a set of rules, maps, or equations by means of which experience can be predicted and controlled. The objects of science--in particular the psychological processes studied in psychology--which can not be reached

through direct observation are thus assigned a status as logical constructs.

This line of argument was presented by Tolman in his paper "Psychology versus Immediate Experience," which was published in the journal Philosophy of Science in the year following his return from Vienna. Much in the manner that Hermann von Helmholtz had been led to reject the idea of a Kantian noumenon on the basis of scientific findings concerning the indirectness of perception, Tolman was led to a positivistic construal of psychological processes on the basis of Brunswik's research on perception. Tolman's paper began with a discussion of the relativity of perception and ended with a positivistic account of intervening variables in terms of "logical constructs" and the "pointer-readings" by which they are related to observation.⁵⁸

In his discussion of the relativity of perception, Tolman noted that facts of perception had often in the history of philosophy been taken as support for a dualism of sense data and objects behind the sense data, and that this dualism was supported by a distinction between the mental and the physical as discrete domains for psychology and physics. Tolman was willing to admit a sort of primary quality-secondary quality distinction. In perceptual experience, he said, there are characters "which are relatively independent and which may be said

to inhere in the things or bodies themselves" and characters "which are dependent and determined not only by the character of the thing in question but also by the special relations of this thing to [the] percipient"; the former Tolman called "independents" and the latter "perspectives."⁵⁹ But he was eager to deny that such a distinction involved any ontological dualism of the mental and physical or any important difference between the sciences of psychology and physics. Brunswik had shown that when given appropriate instructions, experimental subjects could make the proximal stimuli the objects of study--that is, they could "intend" (more or less successfully) the proximal stimuli as well as the usual objects of study, the distal stimuli. Indeed, as Tolman argued, this is what introspective psychologists do when they examine their sensations and what artists do when they attempt to reproduce their visual experience.⁶⁰ But, Tolman insisted, when formulated in this way the distinction between independents and perspectives, i.e., between primary and secondary qualities "no longer seems fundamental." "Immediate experience," he wrote, "is a matrix which contains both perspectives and independents perceptually . . . intended."⁶¹ In this way, Tolman was able to maintain the epistemological distinctions that the demise of neorealism and Brunswik's research had shown to be necessary, and yet to divorce those distinctions from the traditional ontological

dualism he wished to avoid. Tolman states his position outright:

I shall abandon [the] conception of two sets of metaphysical stuffs--a physical world really "out there" to be studied by physics and a series of private mental worlds each in our own respective heads or minds to be studied by psychology.⁶²

Like Carnap, Tolman embraced a position of ontological neutrality, but, unlike Carnap, he was willing to speak of reality, which he identified with immediate experience. Following the Berkeley philosopher Jacob Loewenberg, he characterized immediate experience as "the immediately given preanalytical complex . . . as this appears to the naive man and before the subtleties of philosophical and scientific analysis have been applied to it."⁶³ Tolman elaborated:

Immediate experience as thus composed is rich, qualified, but perhaps in large part ineffable, that is, logically incommunicable from one sentient being to another. It is, however, real--the most real reality that we can have or desire. Its lack is that it does not provide in itself, or only in minor degree, its own rationale.⁶⁴

By saying that immediate experience does not provide its own rationale, Tolman meant that it does not come structured in the form of laws, that it does not, as he put it, "contain written on its face" the rules by which one is guided through it.⁶⁵ To extract such rules from immediate experience is the goal of all science, and in this regard neither psychology nor physics enjoys a

privileged relation to reality. Tolman wrote:

Immediate experience, as initially given, is not my private world or your private world. It is not something to be studied primarily by psychology. It is, rather, an initial, common matrix out of which both physics and psychology are evolved. It is the only tangible real that we have. Physics does not present another real behind that of immediate experience. Nor does psychology, as such, study this real of immediate experience in a more firsthand way than does physics.

Psychology is like physics in being "a set of logical constructs--a set of rules and equations whereby we are aided in finding our way about from one moment of immediate experience to another." Tolman continued:

The purpose of science, of psychology as well as of physics, is not to describe and relive experience but merely to explain it,--to help in predicting and controlling it,--or to use Professor Pepper's term, to give map accounts of it . . . my dichotomy will be between reality and its maps and not between two types of reality.⁶⁶

Thus, in a manner somewhat reminiscent of Mach, Tolman spoke of physics and psychology alike as growing out of the same soil of immediate experience.

But this is not to say that physics and psychology are in all respects alike, rather they are for Tolman complementary in their aims. In elaborating the difference between physics and psychology, Tolman returned to the Brunswik-inspired notions of perspectives and independents. It was also in this context that Tolman cited the work of the logical positivists in the only extended reference to logical positivism that was to appear in his published

writings. Tolman wrote:

As to the difference between physics and psychology my further thesis will now be that whereas physics is the system--the rationale--which attempts to explain all the possible 'independents' and 'perspectives' any organism could conceivably intend and attain in given environmental set-ups, psychology is the system--the rationale--which attempts to explain what perspectives and independents the particular organism or type of organism, on the given occasion will actually . . . intend; and why these intentions will have the specific degrees of success or failure of attainment that they do. In the rest of this paper I shall attempt to elaborate this thesis for psychology. The 'logical positivists,' that is such men as Wittgenstein, Schlick, Carnap in Europe and Bridgman, C. I. Lewis, Feigl and Blumberg in this country, have already done the task for physics. But no one as yet, so it seems to me, has satisfactorily done it for psychology.⁶⁷

This passage is a revealing one in several respects. First, it suggests that Tolman was situating his thoughts about logical positivism in the context of his psychology rather than vice versa. As we shall see in the next chapter, it was generally the case that Tolman subordinated logical, methodological, and philosophical concerns to psychological concerns. Second, Tolman's characterization of the respective roles of physics and psychology in regard to intendings and attainments, following his claim that the logical positivists had already elaborated such a thesis for the case of physics, suggests that his acquaintance with their works was neither particularly broad nor deep--or perhaps simply that he was speaking very loosely of their approach to physics.⁶⁸ As will be dis-

cussed shortly, Tolman selectively drew upon logical positivism for some of the details of his new version of operationism, but he was not sympathetic to other aspects of it and appears not to have ever studied it in any detail. Third, Tolman's list of "logical positivists" was rather inclusive, including as it did Bridgman and Lewis (or, for that matter, Wittgenstein who, despite Schlick's unsparing admiration, was scarcely a logical positivist by 1935). Again, this fact suggests that Tolman understood logical positivism only in a very general way. Lastly, it is significant that Tolman claimed that none of the logical positivists had given a satisfactory treatment of psychology. The passage ended with a footnote citation of Carnap's "Psychology in Physical Language" (1932/33), and Feigl's "Logical Analysis of the Psychophysical Problem: A Contribution to the New Positivism" (1934). But in the footnote, Tolman stated that "Carnap and Feigl have, to be sure, prepared the way, but they have done it only for 'molecular behaviorism' and not for 'molar behaviorism'."⁶⁹ As discussed in Chapter 2, the logical positivists had indeed relied heavily on physiological interpretations in their explications of behaviorism. Tolman had already found such a reductionistic approach unfruitful and was in the process of reformulating his molar behaviorism.

If Tolman's 1935 paper was not a whole-hearted

endorsement of logical positivism, still it evinced the influence of logical positivism. The neutral monism he adopted therein was not the ametaphysical neutrality attempted by Carnap--Tolman was, after all, speaking in frankly metaphysical terms--but it was a form of ontological neutrality, and, as such, it was probably inspired by his experiences in Vienna. In depicting both psychology and physics as being evolved out of immediate experience, Tolman seemed to be following Ernst Mach although it is likely that his neutral monism was drawn more from Holt, whose brilliant exposition of the doctrine was published in 1914 during Tolman's years in graduate school.⁷⁰ Most importantly, Tolman's neutral monism gave him the benefits of avoiding a mind-body dualism while permitting him the epistemological distinctions he needed. The major distinction that remained in Tolman's system was between immediate experience and its maps--a distinction which, although rendered in his own terminology, closely paralleled the logical positivist dichotomy of observation and theory. Just as the logical positivists encountered difficulties in accounting for the relation between theory and observation once they had made the distinction, so too Tolman faced the problem of bringing logical constructs into contact with experience. In the years immediately following his trip to Vienna, Tolman proposed for this problem a solution which constituted a sort of methodo-

logical physicalism.

Methodological Physicalism and the Ideal of Measurement

In his neutral monism, Tolman distinguished, as we have seen, between the richly qualified matrix of immediate experience and the systems of logical constructs which serve as guides to experience. He was left with the problem of relating the two epistemological levels. From the Brunswikian perspective, the desideratum was to determine which of the qualities or proximal stimuli found in immediate experience would provide the most reliable means of attaining the partially accessible events which have been logically constructed by science. From Tolman's own perspective, the desideratum was to continue, as he had been doing for more than a decade, to render observable the concepts of purpose and cognition. At this point, Tolman turned to Carnap's articulation of the doctrine of physicalism. Of course, Tolman's earlier proto-operationism had amounted to a version of physicalism, but in Carnap's writings he found a statement of physicalism which was explicit and which addressed the problem of relating a realm suffused with qualities to an unqualified realm of constructs.

In reading Carnap's 1934 monograph Unity of Science, Tolman focused on the section titled "The Physical Language as an Intersubjective Language."⁷¹ In that section,

Carnap developed an argument to the effect that the physical language is not only intersubjective but also "inter-sensory." A peculiar feature of physical concepts according to Carnap, is "their abstractness and the absence of qualities from their enunciation." Carnap explained this claim as follows:

The rules of translation from the physical language into protocol language are of such a kind that no word in the physical language is ever correlated in the protocol language with words referring only to a single sense field (e.g., never correlated with determinations of colour only or sound only). It follows that a physical determination permits the inference of protocol statements in every sensory field.⁷²

As Schlick and Tolman himself had done, Carnap turned to the example of colors to clarify his argument. A particular green color, for example, could be represented as a line of a certain position in a spectroscopic chart, or the color's wavelength could be represented (with the aid of appropriate devices attached to a spectroscope) as a certain sound from a loudspeaker, or alternatively, as the position of a pointer which could be felt.⁷³ In this way, the occurrence of a certain color could be detected even by a blind person, who could not experience the quality or "raw feel," as Tolman would have put it, of greenness. In a passage which Tolman underlined in his copy of the monograph, Carnap noted that such a physicalization of color was possible "only in virtue of the fact that the frequencies [i.e., wavelengths] in

question can be recognized by signs other than their respective colors. . . . "74 Carnap concluded his discussion by stating the principle--again underlined by Tolman--that "physical determinations are valid inter-sensorily."75

Tolman had long since discussed the possibility of externalizing the experience of colors, but here was a statement that considerably enriched the argument. Color experiences could be rendered in a scientific account as a set of signs or "pointer readings" which not only served as reliable cues, as in Brunswik's scheme, for the distal event of another's color experience but also eliminated the necessity of identifying that experience with its felt greenness. Raw feels, as Tolman had been urging, were thereby divested of any crucial status in a scientific account and relegated to their proper place in the ineffable flux of immediate experience. Tolman was already attuned to the implications of this stance for psychology in general. When Carnap extended his line of reasoning to the case of ascribing thirst to another person, stating that in doing so one recognizes the behavioral signs rather than the immediate experience of thirst, Tolman noted in the margin: "N.B. It is only the wrong view which leads to the doctrine of 'raw feels'."76

As Carnap noted at the outset of his argument, the physicalization of sensory experience relies on the

availability of precise physical concepts and the natural laws of physics and psychophysics. The strategy of correlating sensory experiences with pointer readings thus presupposes a system of measurement that is licensed by such laws. Tolman was surely aware that no such laws were available in the case of the more complex psychological phenomena, but he nonetheless adopted the rhetoric of measurement and precision. In the period following his trip to Vienna, he began to emphasize the prediction and control of behavior as the goals of psychology. He spoke of pointer readings in connection with the logical positivists and began to use the notation of functional equations as a shorthand for the laws of behavior.⁷⁷ In the ideal, the quality-less variables and constructs of a scientific system of psychology were to be related to pure experience through the process of measurement. Experience would be reduced to pointer readings from which a system could be constructed. Tolman's theoretical papers through the latter half of the thirties reflected this positivistic ideal, but it was to remain no more than an ideal. In practice, he never got much further than identifying the basic dependent variable as the proportion of left turns at the choice point of a maze.⁷⁸ When he spoke of pointer readings in his own system, he usually referred not to actual measurements but to qualitative observations of behavior. But he was always quick to

acknowledge that his system was only a program for a future theory, a framework in which one could roughly situate the variables that could be expected to figure in a more advanced account.

Intervening Variables and Sophisticated Operationism

In his masterwork Purposive Behavior, it will be recalled, Tolman had struggled with the problem of how to acknowledge the inferred status of the concepts of purpose and cognition while retaining their objectivity in the sense of intersubjective observability. At that time, he had blatantly equivocated on whether such concepts were of a merely descriptive or an underlying explanatory nature, but he had also just begun to work out a formulation in which they were construed as intervening variables along the lines of the unobservable constructs of physics. Toward the end of the book, he wrote that the system developed therein

conceives mental processes as functional variables intervening between stimuli, initiating physiological states, and the general heredity and past training of the organism, on the one hand, and final resulting responses, on the other. These intervening variables it defines as behavior-determinants. . . . which are discovered, in the last analysis, by behavior experiments. They have to be inferred "back" from behavior. . . . They are to behavior as electrons, waves, or whatever it may be, are to the happenings in inorganic matter. There is nothing private or "mentalistic" about them. They are pragmatically conceived, objective variables the concepts of which can be altered and changed as proves most useful.⁷⁹

Tolman's thinking in 1932 contained, at least in rudimentary form, all the ingredients of his post-Vienna resolution of the problem that faced him. Mental processes would be, in some sense, intervening variables; they would be discovered by and inferred from behavioral experiments; and they would have a status like that of physics' unobservables and be subject to pragmatic constraints. In Tolman's formulation of purpose and cognition in the mid-thirties, these features were expressed in the positivistic framework of functional equations and prediction and control.

Shortly before Tolman's trip to Vienna, Carnap had been grappling with a problem very similar to Tolman's. Giving a plausible account of psychological concepts in physicalistic terms was recognized as a crucial task for advancing the physicalist thesis as a universal account of science. Like Tolman, Carnap had originally attempted simply to identify psychological concepts with physical observations. He aimed to do this by means of strict explicit definitions. But by 1933, he had come to accept that certain abstract psychological concepts could not be related so directly to observations. When Neurath and some of his colleagues undertook to physicalize Freudian theory by translating one of his works sentence by sentence into physicalist language, Carnap objected that a more appropriate strategy would be to

analyze Freudian theory systematically in terms of its concepts. He later described the approach he was recommending as follows:

For some of the concepts, I thought, it would be possible to find behavioristic and thus physicalistic definitions. But the more fundamental concepts of Freud's theory should be treated as hypothetical concepts, that is, introduced with the help of hypothetical laws in which they occur and of co-ordinate rules, which would permit the derivation of sentences about observable behavior from sentences involving the fundamental concepts of the theory. I pointed out the analogy between concepts like 'ego,' 'id,' 'complex' and the field concepts in physics.⁸⁰

Carnap was thus noting at the time, as was Tolman, that certain psychological concepts had a status somewhat like the unobservables of physics, and he was further suggesting that such concepts could be related to observations by means of psychological laws.

Whether or not Tolman was in direct contact with Carnap during his stay in Vienna⁸¹, his pronouncements about intervening variables in the period immediately after his trip had much the flavor of Carnap's approach. Instead of psychological laws in general, he spoke of functional relations between antecedent conditions and dependent behavior. As we saw earlier, Tolman stated that during this period he "hit upon the notion of breaking up the total functions into two or more successive steps and inserting 'intervening variables' . . . between such successive steps or functions." He first presented this

scheme in his Philosophy of Science paper of 1935 and further elaborated it in a second article of the following year. In the 1935 paper, behavior was said to be a function of stimulus conditions (S), the organism's hereditary make-up (H), its past training (T), and its physiological condition of appetite or aversion (P). In Tolman's quasi-mathematical notation:

$$B = f_1(S, H, T, P)$$

But, Tolman continued, the form of this function is too difficult to determine all at once, so it is necessary to divide it into sets of "subordinate equations":

$$\begin{aligned} \text{and } B &= f_2(I_a, I_b, \dots, I_n) \\ I_a &= f_3^a(S, H, T, P) \\ I_b &= f_3^b(S, H, T, P) \\ I_c &= f_3^c(S, H, T, P) \end{aligned}$$

and so forth.⁸² Once the appropriate I's have been identified and the f_2 's and f_3 's determined, the original overall equation can be solved. But Tolman acknowledged that this would not be an easy task. All one could do would be to assert a set of intervening variables and seek the functions--analogous to Carnap's "hypothetical laws"--by which the intervening variables could be related to the observable antecedent and dependent variables.

Although Tolman set up his scheme in the form of equations to suggest the possibility of getting at intervening variables through the process of measuring observable

variables, he actually discussed the intervening variables which he asserted for his own system in terms of dispositions or "readinesses" to respond. He wrote:

I define the I's as behavior readinesses. And I would divide them into two main groups which I shall designate, respectively as demands and cognitions.

The demands were what he had previously called purposes; thus purposes and cognitions became logical constructs expressing behavior readinesses. These were to be inferred, to the extent that it was pragmatically useful to do so, from behavioral facts or pointer readings. Despite their inferred status, they could in principle at least be tied to the observable realm via the f_2 's and f_3 's, thereby retaining their objectivity. In this way, the purposes and cognitions that had earlier been given a neorealist ontological standing were now assigned a methodological status as intervening variables in equations for the prediction and control of behavior. In their epistemological standing, they were rather like Carnap's hypothetical concepts, but with a greater stress on their instrumental value.⁸⁴ As Tolman recognized and openly acknowledged, all of this remained a mere scheme, a program for the future development of psychology, rather than a statement of psychological achievements.

The instrumentalistic character of purposes and cognitions in Tolman's new formulation was phrased in an

especially clear manner in his paper of 1936. In that year, he wrote:

The particular map, the particular subset of predictions, in which psychology is interested concerns the to-be-expected behavior of organisms--the behavior to be expected from other organisms, and the behavior to be expected from ourselves. And in these predictions, mental processes, whether they be those of another or of ourselves, will figure only in the guise of objectively definable intervening variables. Or (to borrow a phrase from William James) the sole 'cash-value' of mental processes lies, I shall assert, in this their character as a set of intermediating functional processes which interconnect between the initiating causes of behavior, on the one hand, and the final resulting behavior itself, on the other.⁸⁵

It is significant that Tolman here availed himself of James's language of pragmatism. As we have seen, Tolman's receptiveness to logical positivist ideas was conditioned by his association with Pepper and Lewis, both of whom were operating intellectually within the context of pragmatism. Indeed, Tolman was also part of the Jamesian tradition. His functionalist psychology, his neutral monism, and now his views on science were all shaped by James's thought, albeit through the mediation of Holt and Perry, and later Pepper and Lewis. Tolman's generally pragmatistic orientation also made him, as will be discussed below, impatient with the niceties of formal logic, especially in its applications to science.

It has already been noted that Tolman's interest in logical positivism was limited to its empirical side--that is, to its operational, rather than logical aspect.

Bridgman's Logic of Modern Physics, his original statement of operationism, had been published in 1927; but despite the similarity of Bridgman's approach to Tolman's early definitions of purposes and cognitions, Tolman had not read Bridgman at the time Purposive Behavior was published, and he did not cite Bridgman until 1935.⁸⁶ Not until 1936 did Tolman give Bridgman more than a passing mention. In his paper of that year, Tolman referred to his own brand of psychology as "Operational Behaviorism." Therein, he cited not only Bridgman but also S. S. Stevens's two papers of 1935, the two papers which proved to be primarily responsible for sparking the interest of psychologists in operationism. Conspicuously absent from Tolman's 1936 article as any mention of logical positivism or any citations of logical positivist works. His preference seemed to be for Bridgman's less formalized approach to the problem of empirical definition, and indeed it would remain his preference.

In the paper on Operational Behaviorism, Tolman gave his most complete and explicit account of the intervening variable paradigm. Following his previously stated scheme of breaking up functional equations and inserting intervening variables into the component equations, Tolman specified that the goals of operational behaviorism were to assert a list of intervening variables (I's), to ascertain the laws or functions by which the I's depend

on the independent variables, and to ascertain the laws by which behavior depends on the I's. The intervening variables, said Tolman "are all that my operational behaviorism finds in the way of mental processes."⁸⁷ He proceeded to sketch out the various categories and subclasses of intervening variables that he thought would be necessary in an adequate psychology of molar behavior.

The problem remained, of course, of finding a way to relate the intervening variables to the observable independent and dependent variables. Only the latter, Tolman admitted, can be directly operated on and controlled by psychologists. How, then can knowledge of the intervening variables be attained? Tolman wrote:

My answer is as follows: In certain carefully chosen, controlled and 'standard' experimental setups, one tries to hold all but one, or one small group, of the independent variables constant and studies the functional connection between the variations in this one independent variable, or this one limited group of independent variables, on the one hand, and the correlated variations in some quantifiable feature of the final behavior on the other.⁸⁸

With the other independent variables held constant at some appropriately chosen "standard" value, Tolman continued, one can assume that the curve expressing the behavioral measure as a function of the selected independent directly reflects the functional relation between the selected variable and values of the relevant intervening variable. "In other words," said Tolman, "we must assume that we have chosen a setup such that the

variations in the selected aspect of the behavior mirror directly those of the desired intervening variable. . . . "89 Thus, for example, one might vary hours of food deprivation (P_1), record behavior as a function of deprivation, and take the resulting curve as a measure of the intervening variable "demand for food" (D_1). The vertical axis may then be rescaled and relabeled as strength of the demand rather than the specific measure of behavior. In sum, intervening variables are to be "operationally defined" by means of standard experiments. Tolman described the rationale for this procedure as follows:

Of course, the obtained behavior does actually depend upon a whole welter of other variables--general stimulus setup, number of previous presentations, other physiological drives, and the specific heredity factors, training factors, and maturity factors, as well as upon P_1 . So that we are assuming that the 'standard' values which we chose in this experiment for these other variables were such that they did not distort the picture. That is, we are assuming that we have obtained this functional relationship between P_1 and D_1 under standard conditions so that this same relationship will also hold between P_1 and D_1 under all conditions, even though under many of these other conditions it will no longer appear simply and directly in some single aspect of the behavior. 90

Defined in this way, the intervening variables could then play the role of independent variables in the functions relating the final behavior to the I's. Furthermore, when there was the need--as Tolman thought there was--for additional levels of intervening variables, the

component equations could be divided further so that "second-line" and "third-line" intervening variables could be inserted into the equations. In the resulting chains of equations, by which the investigator could in principle gain access to the further removed psychological processes, the intervening variables at each level would serve as independent variables for the next level closer to the dependent behavior.

Tolman recognized that his operational approach required a set of rather strong assumptions. First, he noted, it must be assumed that the form of the curve taken to reflect the intervening variable in its original defining experiment does not change when the variables held constant in that experiment are allowed to vary. In other words, the intervening variable must not be subject to interactions between the independent variables. Second, the particular "standard" values assigned to the incidental variables in the defining experiment must be assumed to have been chosen in such a way that the resulting behavioral curve provides a pure reflection of the intervening variable in question. Finally, if intervening variables defined this way are to be generally useful, the general form of their defining functions must be assumed to hold across other members of the same species (if not other species as well) and across a variety of situations.⁹¹ These were indeed strong

assumptions and Tolman remained appropriately cautious about presuming them to be sound in any unproblematic way. He wrote that "all such assumptions are ticklish and of very uncertain justification" and added that they are "dangerous" and "open to pitfalls."⁹² And problems raised by the failure of any of these assumptions would of course be compounded for the case of the second- and third-line intervening variables.

Tolman's 1936 statement of his operational behaviorism was the fullest and most sanguine expression of his positivistic scheme for psychology. It constituted a bold attempt to retain the purposive and cognitive features of his behaviorism, to acknowledge their necessarily inferred status, and yet to keep them objective by operationalizing them. Tolman's earlier neorealist-inspired operationism had somewhat naively identified purposed and cognitions with observable behavior. His new operationism was more sophisticated in the sense of recognizing the indirectness and fallibility of the relation between his concepts and their observational basis. It is instructive to compare these two states in the development of Tolman's operationism with the parallel developments that physicalism was undergoing at the hand of the logical positivists. Herbert Feigl has described the two phases of physicalism and alluded to their parallels in behaviorism:

The first phase was rather rash in its claim of the translatability of the statements of physics and those of psychology into those of the thing-language. . . . This radical and crude form of physicalism may be said to amount to an identification of mental states with overt behavior. Early behaviorism (especially that of J. B. Watson) has been rightly accused of just this fallacious reduction. This view was essentially revised and corrected in the later formulations. Strict translatability depends of course on explicit definitions. But no explicit definitions that would serve the purpose could plausibly be constructed. The concepts of physics and psychology could perhaps be introduced by means of test-condition-test-result-conditionals but not in any way be regarded as synonymous with concepts of the thing-language.⁹³

For the logical positivists, this translation was a logical matter, that is, it was a question of reformulating the logical character of the linguistic links presumed to obtain between concepts and the observations on which they rested. But for Tolman it was a psychological matter. The transition in his own thinking between the naive and sophisticated forms of operationism was motivated by psychological concerns, namely his and Krechevsky's research on hypotheses and Brunswik's research on perception. But more importantly, Tolman's reformulation of operationism was framed in psychological terms. That is, the sophisticated view of empirical definition contained in his Operational Behaviorism was advanced as a descriptive account of the behavior of scientists. Furthermore, that descriptive account was along the lines of a psychological, not logical, analysis. To document these points, we return briefly to Tolman's paper of 1936.

In presenting the intervening variable paradigm, Tolman asserted that the paradigm closely approximated the way in which psychologists in fact pursue their investigations. His three-fold scheme of postulating intervening variables, determining the laws which relate them to independent variables, and finding the laws which relate them to behavior, he said, was "a pretty fair summary of what psychology today is actually, operationally, doing."⁹⁴ Or, again, he stated that

a psychological operationism does no more than give a list of, and attempt to indicate the true functional interrelationship between, the actual types of experiment being done today in psychology.⁹⁵

Indeed, through the paper he illustrated the various points of the scheme with examples drawn from his own work and the research of others. He attempted to show how the major lines of then-current research--sensory, Gestalt, and dynamic psychology as well as research on problem solving, individual differences, and so on--fit into his general scheme at various levels of the first-, second-, and third-line intervening variables. As for the admittedly treacherous assumptions which he described as required by the scheme, he said that they were "the sort which in psychology we actually do employ today."⁹⁶ In sum, Tolman presented the intervening variable paradigm as a framework to be filled in by subsequent psychological research but also as a scheme which represented the activities of research as they stood in the 1930s. In

concluding his article, he wrote:

So much for my attempt to indicate the general operational meaning of the schema. You may, if you will, cavil at its details. But I doubt if you can get away from the general proposition which the schema embodies; namely that we do in psychology assume intervening variables more or less like the ones I have suggested, and that we do attempt to define these intervening variables by going at them experimentally, i.e., operationally, from the two ends.⁹⁷

Tolman's intervening variable paradigm was thus in itself intended as a rough descriptive account of scientific activity, but it was, more precisely, a psychological account. Not surprisingly, Tolman drew upon his own psychological views and his contextualist metaphysics in framing that account. This is revealed in a key passage at the beginning of his 1936 paper. Because the passage is crucial for understanding Tolman's indigenous epistemology, it bears quoting at length. Tolman wrote that

[t]he term 'operational' has been chosen with two different meanings in mind. In the first place, I have chosen it to indicate a certain general positivistic attitude now being taken by many modern physicists and philosophers and for which Professor Bridgman . . . has selected this word 'operational.' In this sense, an operational psychology will be one which seeks to define its concepts in such a manner that they can be stated and tested in terms of concrete repeatable operations by independent observers. In this sense, to quote from S. S. Stevens, 'a term or proposition has meaning (denoted something) if, and only if, the criteria of its applicability or truth consists of concrete operations which can be performed' . . . The behaviorism which I am going to present seeks, then, to use only concepts which are capable of such concrete operational verification.

But, in the second place, I have also chosen this designation, 'operational,' because of what seems to me a second connotation which in connection with the word 'behavior' it tends to have. For behavior as the thing observed also turns out to be essentially an activity whereby the organism in question 'operates.' In behaving, an organism, as Brunswik . . . puts it, 'intends' and more or less successfully 'conquers' its environment. It operates on its environment by such intendings and conquerings. . . .

To sum up, then, I will call mine an operational behaviorism because (a) my type of psychology would self-consciously seek to discover the concrete operations which an experimenter, or any observer, has to carry out to test the applicability or nonapplicability in any given instance of a specific psychological concept or proposition; and because (b) the observed behavior itself turns out to be a set of operations performed by the observed organism relative to its own environment. In a word, the activities of both of us, the observing and conceptualizing organisms, and of them, the observed and behaving organisms, are all ultimately to be characterized as operations of organisms upon environments.⁹⁸

As we have seen, Tolman's earlier neorealist epistemology had been a consistent epistemology in the sense that it applied equally to animal subjects and humans. But, as the preceding passage makes clear, Tolman's revised "operational" epistemology was likewise consistent in that sense. Knowing, for animals and humans alike, is characterized as a kind of operating on an environment. Because of the distal nature of the objects to be known, the operation called knowing requires the use of intendings or hypotheses, which will only in some degree attain their objects. For Tolman, intervening variables are the objects of knowledge for psychology, and the laws which

connect them with observables constitute the hypotheses through which psychologists seek to attain them in the course of their operations on the environment. . Such knowledge is fallible, not only because the necessary laws are not known with certainty, but also because of the questionable status of the assumptions involved in using the laws to get at the different levels of intervening variables. In Brunswikian terminology, the manipulable variables and observable outcomes serve as signs or proximal stimuli for the to-be-attained distal intervening variables. In the terms of contextualist metaphysics, the knowledge-seeking organism, whether animal or human, operates by following up strands of texture. And in Tolman's own spider-web metaphor, the organism must choose among the various strands of the multi-dimensional web that constitutes the world and follow the chosen ones to goals or final states of quiescence. In every case, knowledge is tentative and fallible because its objects are at a remove from the organism's position. In sum, Tolman's later operationism was drawn from his underlying presuppositions about organismic behavior, just as his earlier proto-operationism had been. In other words, he was coordinating his method with his subject matter rather than subordinating subject matter to method. As before, the method was indigenous to the psychology.

Tolman's intervening variable paradigm was presented once again in his presidential address of 1937 to the American Psychological Association. At that time, psychology's "Age of Theory" was rapidly approaching its peak, and Tolman responded by addressing the issue of what constitutes theory in psychology. In the APA presidential address of the preceding year, Clark Hull had given a formalized account of theory in terms of postulates and theorems (as will be discussed in Chapter 7 below), and Tolman reviewed Hull's statement as follows:

According to Professor Hull . . . , a theory is a set of definitions and postulates proposed by the theorists (on the basis presumably of some already found facts) from which other empirically testable facts, or as he calls them, theorems, can be logically deduced.

Continuing, Tolman presented his own view of theories:

For my own nefarious purposes, however, I wish to phrase this matter of the relationship of a theory to the empirical facts out of which it arises and to which it leads in somewhat other terms. A theory, as I shall conceive it, is a set of 'intervening variables.' These to-be-inserted intervening variables are 'constructs' which we, the theorists, evolve as a useful way of breaking down into more manageable form the original complete f_1 function.⁹⁹

Tolman's construal of theories was a humbler one than Hull's and he seemed eager to emphasize the differences between them. (In fact, Tolman had always avoided referring to his own system as a "theory.")

Scattered throughout his address were remarks which cast some doubt on the feasibility of not only Hull's

ambitious approach to theory but even his own relatively circumspect intervening variable scheme. For instance, he noted that certain basic features of the most fundamental behavioral law, the learning curve, were still unknown or in dispute. He added, "I doubt that the supposed laws of conditioning are as simple and as well-known as Hull assumes."¹⁰⁰ In considering the laws which would relate intervening variables to the resultant behavior, he expressed doubt that they could ever be portrayed adequately by mere algebraic addition of all the intervening factors; rather, he thought, they would have to be stated in terms of complex vectorial combinations.¹⁰¹ Moreover, in thinking about such laws, Tolman stated, "I am at present being openly and consciously just as anthropomorphic about it as I please," and added that anthropomorphism is "a perfectly proper heuristic procedure."¹⁰² In all of these respects, Tolman was not only setting himself apart from Hull but also moving away from any position involving simplistic assumptions about the pursuit of science by formula or the inevitability of progress in the face of complex problems.¹⁰³ Even with all of its explicit cautions, Tolman's paper of 1936 remained his most optimistic statement of the intervening variable scheme. In formulating it, he had been influenced by logical positivism, but even in the following year he was beginning to retreat from a positivist per-

spective on psychology. He had found some value in the formulations of logical positivism and had assimilated some of it to his own approach to psychology, but in the later part of the 1930s and thereafter he simply forged ahead with research on the psychological topics that interested him.

After the thirties, there was only one other time that a paper of Tolman's contained even a single mention of or reference to any of the logical positivists. That reference occurred in a brief experimental report of 1946 coauthored by Tolman and two graduate students, Benbow F. Ritchie and Donald Kalish. The experiment dealt with expectancies in rats, and in their report the authors attempted to give a logically precise formulation of the concept of "expectation." Referring in a footnote to Carnap's "Testability and Meaning" (1936-37), they adopted a definition of expectation which had the form of Carnap's bilateral reduction sentences.¹⁰⁴ But even though Tolman was the first author on the paper, the effort to formalize the definition of expectation was not his idea. Ritchie had come to Berkeley after studying philosophy at Chicago with Carnap, Bertrand Russell, and Morris Cohen; Kalish had begun working with Tolman while studying philosophy and logic at Berkeley.¹⁰⁵ According to Ritchie and Kalish, the concern in the paper with precise definitions was entirely at their own instigation. The two had

proposed the formal explication of expectation in a report to Tolman's seminar, and Tolman had agreed to participate in the relevant experiments. But according to Ritchie, Tolman "never discussed logical positivism either in the classroom or in [the] seminars or in the long coffee house discussions that took place almost every afternoon between Tolman and the graduate students that were running animal experiments under his guidance."¹⁰⁶ Likewise, Kalish has remarked, "I do not recall Edward ever commenting on, or discussing with us, logical positivism, before or after that seminar report."¹⁰⁷ The Tolman, Ritchie, and Kalish paper has recently been cited as an example of the use of logical positivist formulations by behavioral scientists.¹⁰⁸ But it would be misleading to infer from this that Tolman was actively pursuing the logical positivist ideal of science at that time. As is argued in the foregoing, the limited influence that logical positivism did have on Tolman was exerted during the mid-thirties and was declining in the 1940s.

Tolman's interaction with logical positivism took place in one further context, namely his involvement with the Unity of Science movement. That involvement is discussed in the following section.

Tolman, Brunswik, and the Unity of Science Movement

At the 1934 planning conference for the Unity of Science congresses, the Chicago pragmatist Charles Morris reported on the status of scientific philosophy in America. After being asked to write up his report for the 1935 volume of Erkenntnis, Morris conducted an informal survey of scientific philosophers in America in order to broaden the scope of the report. The Berkeley physicist Victor F. Lenzen replied to Morris's inquiries with the suggestion that Tolman be included in the report. Lenzen noted that Tolman's Purposive Behavior presented "a systematic psychology of macroscopic behavior" and that the final chapter was "quite in the spirit of logical positivism."¹⁰⁹ In the first mention of any neobehaviorist in Erkenntnis, Morris did include reference to Tolman's work in the published report.¹¹⁰ In the year 1935, the organizers of the Unity of Science movement were just beginning to actively recruit proponents of objective psychology for the movement (see Chapter 7), and whether because of Morris's report or through Tolman's contacts in Vienna, they invited Tolman to present a paper on psychology at the 1936 congress in Copenhagen.

Tolman accepted the invitation and prepared a paper entitled "An Operational Analysis of 'Demands'," which was to be presented at a session along with a paper by Brunswik.¹¹¹ As it turned out, Tolman was unable to

attend the congress, but his paper was read and discussed there, and it was published in Erkenntnis in 1937.¹¹² The paper was for the most part simply a statement of the intervening variable paradigm and a description of various psychological experiments on demands (i.e., motives or purposes). In it, Tolman expressed the usual cautions about the hazardous assumptions involved in defining the I's by means of standard experiments, and he emphasized the imprecision of the laws connecting the I's with observable variables. He made no explicit mention of logical positivism, although he did assert near the end of the paper that "'Demands' and 'hypotheses' are grosser wholes but they are just as 'physicalistic' as are reflex arcs and nerve currents."¹¹³ He also referred to Carnap's "Psychology in Physical Language" in connection with his claim that introspection was acceptable only in the form of a set of Protokolsätze.¹¹⁴ Otherwise, the paper was rather narrowly concerned with psychological issues.

Shortly before the Copenhagen congress, Neurath invited Tolman to contribute a pamphlet on psychology to the recently created monograph series, the International Encyclopedia of Unified Science.¹¹⁵ Tolman apparently agreed to write the piece, and soon thereafter Brunswik was added as coauthor.¹¹⁶ But there was concern on the part of Neurath and others that neither Tolman nor Brunswik would address the application of logic to

psychology, and the Norwegian philosopher-psychologist Arne Naess--who had attended meetings of the Vienna Circle in 1934-35--was taken under consideration as a potential author for the monograph. Late in 1936, Neurath wrote to Morris and Carnap:

I know the difficulties if we use Tolman alone. Perhaps in connection with Arne Naess? I appreciate Brunswik and I agree with you that he shall give also an addition, but I do not know in which extension he is willing to discuss our special logical aspect. (That is the same as in the case Tolman--I think so.)¹¹⁷

As will be documented in the following chapter, Tolman was not sympathetic to logical analyses of science, and it appears that he was not able to reach an agreement with the Encyclopedia's organizers as to how the pamphlet on psychology would be handled. By 1938, Neurath was listing the prospective monograph on psychology, under the title "Theory of Behavior," with Brunswik and Naess as its authors.¹¹⁸

Tolman had been dropped from authorship of the Encyclopedia piece, but this did not mark a complete end to his involvement with the Unity of Science movement. His name appeared on the membership lists Encyclopedia's Advisory Committee and the American Organizing Committee for the International Congress which was held at Harvard in 1939.¹¹⁹ However, it is doubtful whether Tolman's serving on these committees involved anything more than his lending his name to the movement. Tolman's student

Ritchie, who later became his colleague at Berkeley, has reported that he never heard Tolman mention the Unity of Science movement or his participation in it. Concerning Tolman's committee memberships, Ritchie comments:

I rather imagine that his appointment to the organizing committee of that movement represented his prestige at that time as a behaviorist, and his acceptance of the appointment represented his delight in finding a new audience for his work.¹²⁰

Tolman seems to have retained some degree of interest in the actual congresses. He declined an invitation to speak at the 1939 congress, but expressed an interest in attending the 1941 congress held at the University of Chicago, and chaired a session at the last Unity of Science conference, which was held at Berkeley in 1953.¹²¹

However, it was Brunswik who continued to be the psychologist most active in the Unity of Science movement. He prepared papers for Unity of Science congresses in 1935 (Paris), 1936 (Copenhagen), 1937 (Paris), 1938 (Cambridge, England), 1941 (Chicago), and 1953 (Berkeley).¹²² He was also a major organizer of the Berkeley conference, and was eventually the sole author of the Encyclopedia monograph on psychology.¹²³ Brunswik's enthusiastic participation in the Unity of Science movement made him the acknowledged liaison between psychology and the movement, and Tolman was evidently content to cede that role to him.

Conclusion

As part of his neorealist heritage, Tolman practiced an early brand of operationism throughout the 1920s. This proto-operational tendency was reinforced and set in a philosophical context through Tolman's contact with the Berkeley philosophers Pepper and Lewis. It also predisposed Tolman to respond sympathetically to logical positivism, or at least its empiricist side, when he became aware of the movement in the early thirties. At that time, Tolman's views on cognition had recently shifted from a direct realism to a view in which cognition was held to be mediated by hypotheses and other intervening processes. The latter view accorded well with the results of his research but raised difficulties with any attempt to identify the intervening processes in some simple way with observable behavior. Tolman was moving toward a resolution of these difficulties prior to his decision to spend a sabbatical in Vienna--a decision apparently motivated in part by the hope that logical positivism would help to clarify those difficulties.

Once in Vienna, Tolman met Brunswik, whose psychological views, emphasis on objective methods, and interest in logical positivism closely paralleled Tolman's own views. Like Brunswik, Tolman appears to have attended meetings of the Vienna Circle. The exact nature of his interactions with the logical positivists there is not

known, but his papers of the years immediately following his stay in Vienna reflected some of their influence. First, he adopted a neutral monism which was close in some respects to the logical positivists' metaphysical neutrality. Like Schlick, he drew an epistemological distinction between the constructs of science and immediate experience, the latter of which was said to be ineffable. Second, his post-Vienna papers showed a reinforced adherence to the operational approach, which was a kind of methodological physicalism, and the adoption of corresponding terminology--"functional relations," "pointer readings," "constructed variables," and the like. Third, he began to treat intervening variables somewhat along the lines of Carnap's hypothetical constructs and devised a sophisticated operationism in which the intervening variables were related to observables by means of defining experiments. But if these developments reflect the influence of logical positivism, they do no more than that. With the exception of terminological matters, all of the above developments were already evident, in more or less refined form, in Tolman's thinking prior to his trip to Vienna. He had been exposed to neutral monism through Holt, and his epistemological dualism and adherence to the ineffability doctrine were established by the early thirties. He had been practicing a sort of methodological physicalism for more

than a decade and had already begun to construe intervening variables as having a status like that of the unobservable constructs of physics. Thus, whatever influence logical positivism had on Tolman's thought, it was more of a corroborative than formative nature. That Tolman viewed it this way is suggested by the fact that he rarely mentioned logical positivism or cited its adherents.

Moreover, as has been argued in the present chapter, the developments in Tolman's thought which paralleled developments in logical positivism were motivated by, and framed in terms of, psychological concerns rather than logical concerns. Just as his earlier version of operationism had been indigenous to his neorealist-based psychology, so was his sophisticated operationism of the thirties indigenous to his later psychology of mediated cognition and to the pragmatist-contextualist world-view which underlay his psychology. For Tolman, operationism was a psychological process in which an organism--whether human or rat--operates on an environment by following up strands of causal texture in an effort to achieve distal objects or goals. The process is guided by pragmatic concerns and mediated by hypotheses, but it remains a fallible enterprise. In this operational epistemology, knowledge is achieved through activity in an environment and in this activity there is little use for narrow logical distinctions. Tolman did have sympathies with logical

positivism but those sympathies were limited by the fact that he viewed science from the perspective of his own psychology. Tolman's psychological view of science and his critical attitudes toward the use of logic in science will be discussed in the following chapter.

Notes for Chapter 4

1. Edward C. Tolman, "Purpose and Cognition: The Determiners of Animal Learning," in Behavior and Psychological Man: Essays in Motivation and Learning (Berkeley: University of California Press, 1966), pp. 38-47, on pp. 38, 44. This article appeared originally in the Psychological Review 32 (1925): 285-297.
2. Edward C. Tolman, "A Behavioristic Theory of Ideas," in Behavior and Psychological Man, pp. 48-62, on p. 52; "A Behaviorist's Definition of Consciousness," in Behavior and Psychological Man, pp. 63-68, on p. 65. The former article appeared originally in the Psychological Review 33 (1926): 352-369, and the latter in the Psychological Review 34 (1927): 433-439.
3. Ivan Krechevsky, "'Hypotheses' versus 'Chance' in the Pre-Solution Period in Sensory Discrimination-Learning," University of California Publications in Psychology 6 (1932): 27-44; "The Docile Nature of 'Hypotheses' in Rats," University of California Publications in Psychology 6 (1932): 45-64.
4. Edward C. Tolman and Ivan Krechevsky, "Means-End-Readiness and Hypothesis--A Contribution to Comparative Psychology," Psychological Review 40 (1933): 60-70, on pp. 62-63.
5. Edward C. Tolman and Egon Brunswik, "The Organism and the Causal Texture of the Environment," in The Psychology of Egon Brunswik, ed. Kenneth R. Hammond (New York: Holt, Rinehart & Winston, 1966), pp. 457-486, on p. 462. This article appeared originally in the Psychological Review 42 (1935): 43-77.
6. Edward C. Tolman, "Edward Chace Tolman," in A History of Psychology in Autobiography, vol. 4, ed. Edwin G. Boring, Heinz Werner, Herbert S. Langfeld, and Robert M. Yerkes (Worcester, Mass.: Clark University Press, 1952), pp. 323-339, on p. 333.
7. Edward C. Tolman, Purposive Behavior in Animals and Men (New York: Century, 1932), p. 428. Tolman's expressions "discriminanda" (cues to be discriminated) and "manipulanda" (objects whose manipulation leads to reinforcement) are two of his neologisms that have been widely adopted by psychologists. As for his epistemological

dualism, it continued to underlie his later work--for example, in the distinction between "behavior-spaces" (states of reality) and "belief-value matrices" (beliefs and expectations about reality). See Tolman, "Edward Chace Tolman," p. 332.

8. Tolman, Purposive Behavior, p. 428.

9. Stephen C. Pepper, The Sources of Value (Berkeley: University of California Press, 1958), p. 4. Pepper's indebtedness to Tolman was also indicated in the inscriptions he wrote in books he presented to Tolman as gifts. For example, Tolman's copy of Pepper's Aesthetic Quality (1937) bears the inscription: "Edward Tolman, friend, psychologist, and philosopher from his much indebted philosopher friend, Stephen, Christmas 1937" (Warner Brown Reading Room, Tolman Hall, University of California, Berkeley, Cal.).

10. Stephen C. Pepper, "The Conceptual Framework of Tolman's Purposive Behaviorism," Psychological Review 41 (1934): 108-133, on p. 111.

11. Ibid., p. 113.

12. Ibid., p. 114.

13. Tolman, "Edward Chace Tolman," p. 332.

14. Tolman, "Theory of Ideas," pp. 52-53.

15. Tolman, Purposive Behavior, p. 424.

16. Ibid., pp. 424-425.

17. Ibid., pp. 425-426.

18. Ibid., pp. 426, 424.

19. Ibid., p. 426.

20. Edward C. Tolman, "Concerning the Sensation Quality: A Behavioristic Account," Psychological Review 29 (1922): 140-145. This point was reiterated in several of Tolman's articles, including "A New Formula for Behaviorism," Psychological Review 29 (1922): 44-53; "A Behavioristic Account of the Emotions," Psychological Review 30 (1923): 217-227; and "A Behavioristic Theory of Ideas," Psychological Review 33 (1926): 352-369.

21. Tolman, Purposive Behavior, p. 426. This passage was quoted from C. I. Lewis, Mind and the World-Order (New York: Scribner's, 1929).

22. This claim is based on the fact that Tolman's copy of Mind and the World-Order (1929) was inscribed "To Edward C. Tolman, remembering an auto trip to Palo Alto, Clarence I. Lewis" (Warner Brown Reading Room, Tolman Hall, University of California, Berkeley, Cal.). It remains uncertain how close their relationship was. It is tempting to speculate that their joint trip to Palo Alto was to see Moritz Schlick, who was a visiting professor at Stanford in 1929.

23. C. I. Lewis, "The Pragmatic Element in Knowledge," University of California Publications in Philosophy 6 (1926): 205-227, on p. 217. (The ideas developed in this essay were first presented in 1926 in Lewis's Howison Lecture at Berkeley, an event quite likely attended by Tolman.) It is perhaps significant that Lewis's intensional system of logic became a major alternative to the reigning Principia Mathematica logic of the logical positivists. See C. I. Lewis, A Survey of Symbolic Logic (Berkeley: University of California Press, 1918) and C. I. Lewis and C. H. Langford, Symbolic Logic (New York: Century, 1932).

24. The striking similarity of Lewis's basic form of knowledge, the three-term terminating judgment, and Tolman's basic form of knowledge, the three-term sign-Gestalt-expectation, suggests the possibility of a close intellectual interchange between them. Or, alternately, the similarity may be due simply to their shared intellectual background in Perry's thought and the pragmatist tradition in general. On the topic of terminating judgments, see The Encyclopedia of Philosophy, s.v. "Clarence Irving Lewis," by E. M. Adams, and Andrew J. Reck, The New American Philosophers: An Exploration of Thought Since World War II (Baton Rouge, La.: Louisiana State University Press, 1968), pp. 19-20.

25. Although the evidence that Tolman was personally acquainted with Schlick is not entirely conclusive, those who were around the Philosophy Department at Berkeley at the time of Schlick's visit think it almost certain that he was. For example, David Rynin has written: "I was still a graduate student when Schlick was in Berkeley but got to know him pretty well as I was assigned to be his assistant (and taught him to drive a car). I was not included in social gatherings or in departmental meetings so I have no personal knowledge that Tolman knew Schlick

25. (cont'd.) but I have not the slightest doubt that he did. Tolman was, of course, well acquainted with members of the Philosophy Department . . . (David Rynin to Laurence D. Smith, 6 January 1981). In this regard it is also significant that Tolman's personal copy of Schlick's posthumously published Natur und Kultur (Vienna: Humboldt Stuttgart, 1952) was inscribed by Schlick's wife: "With compliments of Blanche Schlick, December 1952" (Warner Brown Reading Room, Tolman Hall, University of California, Berkeley, Cal.).

26. Moritz Schlick, "Philosophy of Organic Life," in Readings in the Philosophy of Science, ed. Herbert Feigl and May Brodbeck (New York: Appleton-Century-Crofts, 1953), pp. 523-536, on p. 529. This essay originally appeared as "Naturphilosophie," Die Philosophie in ihren Einzelgebieten, ed. Max Dessoir (Berlin: Ullstein, 1925).

27. Ibid.

28. Moritz Schlick, "Form and Content, an Introduction to Philosophical Thinking," in Gesammelte Aufsätze, 1926-1936 (Vienna: Gerold, 1938), pp. 151-336, on p. 164. This essay consists of three lectures given by Schlick at the University of London in 1932.

29. Ibid., pp. 161-168, 179, 209, 213.

30. Ibid., pp. 167, 179.

31. Ibid., pp. 213-214.

32. Moritz Schlick, "The Future of Philosophy," in Gesammelte Aufsätze, pp. 117-133, on pp. 129-130. A bibliographical note (p. 117) accompanying this essay states that it first appeared in "'Publications in philosophy' ed. by the College of the Pacific." [sic]

33. Tolman, "Theory of Ideas," p. 62.

34. For example, see Schlick, "Future of Philosophy," p. 129, where Schlick relates ostension to language acquisition in very much the same manner as can be found in Wittgenstein's writings.

35. Schlick, "Form and Content," p. 182.

36. Tolman, Purposive Behavior, p. 426. Compare this remark with Schlick's comment that:

The merit of poetry does not lie in its wonderful capacity of expression, it is to be found in the great effects it produces in our souls by that which it expresses. While the ultimate purpose of science is knowledge, . . . the purpose of art is to evoke in us certain emotions, and expression is but a means to this end. Emotions are content (possessing, of course, a certain structure), they are not communicated by poetry, but produced by it.

See Schlick, "Form and Content," p. 212.

37. Schlick, "Form and Content," pp. 204-209.

38. See G. W. H. Leytham, "In Memory of Edward Chace Tolman (1886-1959)," Bulletin of the British Psychological Association 49 (1962): 21-28, on p. 26.

39. W. W. Bartley, III, "Theory of Language and Philosophy of Science as Instruments of Educational Reform: Wittgenstein and Popper as Austrian Schoolteachers," in Method and Metaphysics: Methodological and Historical Essays in the Natural and Social Sciences (Dordrecht, Holland: D. Reidel, 1974), pp. 307-337, on p. 313; Marie Jahoda, "The Migration of Psychoanalysis: Its Impact on American Psychology," Perspectives in American History 2 (1968): 420-445, on p. 422.

40. Edna Heidbreder to Laurence D. Smith, 10 July 1981. According to Heidbreder, she got to know Tolman and Egon Brunswik "rather well" during her sabbatical in Berkeley in 1940-41, and is thus able to "speak with some confidence about Tolman's connection with L. P. [logical positivism]."

41. See Edward C. Tolman, "Egon Brunswik: 1903-1955," in Psychology of Brunswik, pp. 1-12, on p. 1. This obituary first appeared in the American Journal of Psychology 69 (1956): 315-324. A second obituary by Tolman appeared as Edward C. Tolman, "Egon Brunswik, Psychologist and Philosopher of Science," Science 122 (1955): 910.

42. Egon Brunswik, Wahrnehmung und Gegenstandswelt: Grundlegung einer Psychologie vom Gegenstand her (Leipzig and Vienna: Deuticke, 1934). The relation of Brunswik's psychology to the act psychology of Franz Brentano and the functional theory of mind developed by Karl Bühler is discussed by David E. Leary, "From Act Psychology to

Probabilistic Functionalism: The Case of Egon Brunswik," paper presented at Cheiron (The International Society for the History of the Behavioral and Social Sciences), River Falls, Wis., June 1981.

43. Although Tolman and Brunswik arrived at their highly similar positions independently, they shared some common intellectual heritage. The European tradition from which Brunswik's thought derived was also an acknowledged background of the approach taken by the neorealists (who, for example, traced their tradition to Alexius Meinong).

44. The notion of vicarious mediation is identified with that of intersubstitutability of cues, for example, in Egon Brunswik, "Historical and Thematic Relations of Psychology to the Other Sciences," in Psychology of Brunswik, pp. 495-513, on p. 512. This paper was first published in the Scientific Monthly 83 (1956): 151-161. Tolman recognized that Brunswik's perceptual constancies were essentially the same phenomena that Holt had referred to as the "recession of the stimulus"--that is, perception is of environing objects rather than of punctate cues (see Tolman's lecture notes of 11 February 1938, Box M133, Edward Chace Tolman Papers, Archives of the History of American Psychology, Akron, Ohio).

45. See Leary, "The Case of Egon Brunswik."

46. Egon Brunswik, "Organismic Achievement and Environmental Probability," Psychological Review 50 (1943): 255-272, on p. 258. On the notion of the organism as an intuitive statistician, see Egon Brunswik, Perception and the Representative Design of Experiments (Berkeley: University of California Press, 1956), p. 80.

47. Leytham, "In Memory of Tolman," p. 26.

48. Tolman and Brunswik, "Organism and Causal Texture," p. 468.

49. Ibid.

50. Ibid. The work referred to was Hans Reichenbach, "Die Kausalstruktur der Welt und der Unterschied von Vergangenheit und Zukunft," Bayerischer Akademiebericht (1925).

51. Tolman and Brunswik, "Organism and Causal Texture," p. 457.

52. Tolman also undertook this task in his review of Brunswik's book. See Edward C. Tolman, "Review of Wahrnehmung und Gegenstandswelt: Grundlegung einer Psychologie vom Gegenstand her, by Egon Brunswik," Psychological Bulletin 32 (1935): 608-613.

53. Such distinctions are especially salient in Edward C. Tolman, "Motivation, Learning, and Adjustment," Proceedings of the American Philosophical Society 84 (1941): 543-563. For Tolman's pre-Vienna functionalist distinction between primary and secondary qualities (one that shows a remarkable parallel with Brunswik's thought), see Edward C. Tolman, "Gestalt and Sign-Gestalt," in Behavior and Psychological Man, pp. 77-93, on p. 85. This article appeared originally in the Psychological Review 40 (1933): 391-411.

54. Thus, Brunswik wrote that "the essential operational aspects of the unity of science are rapidly becoming a matter of course in psychology" ("Historical and Thematic Relations," p. 512). He also endorsed "the probabilistic application of operational criteria of 'purposiveness' such as those of Tolman" (Egon Brunswik, "In Defense of Probabilistic Functionalism: A Reply," Psychological Review 62 [1955]: 236-242, on p. 238. Brunswik apparently took quickly to Tolman's proto-operational approach. A transcript of the discussion following a talk given by Tolman to Bühler's research group in Vienna (24 January 1934) reveals Brunswik's enthusiastic reaction to Tolman's studies on purposes, cognitions, and hypotheses. The transcript reads: "Dr. Brunswik stated that these studies show how far one can go with objective methods . . . Knowledge, hypothesis, etc. can translate into behaviorism, because they are solvable in terms of theoretical relations" (my translation). See "Diskussion" appended to "Lernprobleme bei Ratten," Box M133, Edward Chace Tolman Papers, Archives of the History of American Psychology, Akron, Ohio. Brunswik's sympathy with the behaviorist approach to psychology was such that he actually performed maze studies with rats after he came to Berkeley. See Egon Brunswik, "Probability as a Determiner of Rat Behavior," Journal of Experimental Psychology 25 (1939): 175-197. All the same, Brunswik remained sceptical of extreme claims about the benefits of operational definitions and of attempts to provide strict definitions (such as Watson's "dogmatically periperhalistic translations") instead of merely probabilistic ones. See Egon Brunswik, The Conceptual Framework of Psychology (Chicago: University of

54. (cont'd.) Chicago Press, 1952), pp. 44-45 (this monograph was part of the International Encyclopedia of Unified Science). In this monograph and elsewhere, Brunswik also criticized "thematic" physicalism, that is, an overly literal emulation of the physical sciences by practitioners of the behavioral sciences (pp. 36-44).

55. See Tolman, "Egon Brunswik: 1903-1955," p. 2, and Leary, "The Case of Egon Brunswik."

56. Kenneth R. Hammond, "Probabilistic Functionalism: Egon Brunswik's Integration of the History, Theory, and Method of Psychology," in Psychology of Egon Brunswik, pp. 15-80, on pp. 23, 54.

57. Except for the "Lernprobleme" paper cited above (note 54) and a draft in German of Tolman and Brunswik's "Causal Texture" paper, the Tolman papers in the Archives of the History of American Psychology contain no information regarding Tolman's trip to Vienna. My extensive correspondence with various colleagues and former students of Tolman has revealed nothing of his activities there--most correspondents were, in fact, completely unaware of how he spent his sabbatical of 1933-34. Similarly, my correspondence with various figures associated with the Vienna Circle failed to turn up any information on Tolman's possible participation in the Circle. However, none of these correspondents--who include A. J. Ayer, Gustav Bergmann, Herbert Feigl, Karl Menger, and Arne Naess--were actively participating in the meetings of the Circle during the months Tolman spent in Vienna.

58. Edward C. Tolman, "Psychology versus Immediate Experience," in Behavior and Psychological Man, pp. 94-114, on p. 103. This article appeared originally in Philosophy of Science 2 (1935): 356-380.

59. Ibid., p. 98.

60. Ibid., p. 99.

61. Ibid., pp. 99, 100.

62. Ibid., p. 96.

63. Ibid., p. 94.

64. Ibid., p. 100.

65. Ibid.

66. Ibid., pp. 96-97.

67. Ibid., p. 100. In footnotes accompanying this passage, Tolman referred to the following works of the "logical positivists" whom he listed: Wittgenstein's Tractatus Logico-Philosophicus (which Tolman dated as 1927); Schlick's "Positivismus und Realismus," Erkenntnis (1932); Carnap's Logische Aufbau der Welt (1928); Bridgman's Logic of Modern Physics (1927); Lewis's Mind and the World-Order (1929); and Feigl and Blumberg's "Logical Positivism," Journal of Philosophy (1931).

68. Thus, for example, Tolman referred to Carnap's Logische Aufbau, but he actually had little interest in or patience with logical approaches to science (see Chapter 5 below).

69. Tolman, "Psychology versus Immediate Experience," p. 100. The Carnap paper appeared originally in Erkenntnis and the Feigl paper in Philosophy of Science.

70. Interestingly enough, Tolman cited neither Mach nor Holt on this score. Holt's statement of neutral monism was published as The Concept of Consciousness (New York: Macmillan, 1914), although the writing of it was completed in 1908. Even though Tolman did not cite this work, it seems clear that Tolman absorbed his propensity for neutral monism from Holt. The conjecture that Holt mediated the transmission of neutral monism from James to Tolman has been raised by Paul Tibbetts, "The Doctrine of 'Pure Experience': The Evolution of a Concept from Mach to James to Tolman," Journal of the History of the Behavioral Sciences 11 (1975): 56-66, on p. 62.

71. Tolman's copy of this monograph contains very little marginalia or underlining outside of this particular section (Warner Brown Reading Room, Tolman Hall, University of California, Berkeley, Cal.). The section in question is pp. 52-67 of Rudolf Carnap, Unity of Science, tran. Max Black (London: Kegan Paul, Trench, Trubner, 1934). Tolman may very well have met Carnap during his visit to Vienna. Carnap reports that he often visited Vienna during that year even though he was by then living and teaching in Prague. See Rudolf Carnap, "Intellectual Autobiography," in the Philosophy of Rudolf Carnap, ed. Paul A. Schilpp (La Salle, Ill.: Open Court, 1963), pp. 1-84, on p. 58.

72. Carnap, Unity of Science, p. 57.

73. Ibid., p. 60.

74. Ibid., p. 59.

75. Ibid., p. 62. (This statement was italicized by Carnap as well as underlined by Tolman.)

76. This remark is on p. 79 of Tolman's copy.

77. Tolman, "Psychology versus Immediate Experience," p. 103. Tolman may have picked up the notion of "pointer readings"--a term that later came into common use in discussions of psychological method--from his reading of Eddington's The Nature of the Physical World (1929). This book contains a chapter titled "Pointer Readings," some passages of which Tolman marked in his own copy (Warner Brown Reading Room, Tolman Hall, University of California, Berkeley, Cal.). The source of Tolman's use of functional notation for expressing laws of behavior has become something of a point of controversy. B. F. Skinner has claimed that Tolman adopted the practice from him, and without an adequate acknowledgement of having done so. See B. F. Skinner, The Shaping of a Behaviorist: Part Two of an Autobiography (New York: Alfred A. Knopf, 1979), p. 206. One reviewer of Skinner's book has commented that: "All we learn about Tolman was that he [supposedly] stole an idea (a trivial notation, actually) without acknowledgment. The really important ideas that Tolman and Skinner shared are lost in the petulant account of the theft." See Robert C. Bolles, "Scholar's Progress," Science 204 (1979): 1073-1074, on p. 1073.

78. See Edward C. Tolman, "The Determiners of Behavior at a Choice Point," in Behavior and Psychological Man, pp. 144-178, on pp. 144-145. This paper, which is the published version of Tolman's presidential address to the American Psychological Association in 1937, first appeared in the Psychological Review 45 (1938): 1-41.

79. Tolman, Purposive Behavior, p. 414.

80. Carnap, "Intellectual Autobiography," p. 58.

81. See note 71 above.

82. Tolman, "Psychology versus Immediate Experience," p. 102.

83. Ibid., p. 103.

84. Intervening variables and hypothetical constructs eventually became important topics of methodological discussion in psychology. The classic work on the subject is Paul Meehl and Kenneth MacCorquodale, "Hypothetical Constructs and Intervening Variables," Psychological Review 55 (1948): 95-107.

85. Edward C. Tolman, "Operational Behaviorism and Current Trends in Psychology," in Behavior and Psychological Man, pp. 115-129, on pp. 116-117. This paper appeared originally in the Proceedings of the Twenty-fifth Anniversary Celebration of the Inauguration of Graduate Studies at the University of Southern California (Los Angeles: University of Southern California Press, 1936), pp. 89-103.

86. See Tolman, "Edward Chace Tolman," p. 331. Tolman's first citation of Bridgman was in "Psychology versus Immediate Experience."

87. Tolman, "Operational Behaviorism," p. 118.

88. Ibid., p. 122.

89. Ibid., p. 123.

90. Ibid., pp. 123-124.

91. Ibid., pp. 122-125.

92. Ibid., p. 125.

93. Herbert Feigl, Inquiries and Provocations: Selected Writings 1929-1974, ed. Robert S. Cohen (Dordrecht, Holland: D. Reidel, 1981), pp. 289-290.

94. Tolman, "Operational Behaviorism," p. 119.

95. Ibid., p. 118.

96. Ibid., p. 125.

97. Ibid., p. 127.

98. Ibid., pp. 115-116.

99. Tolman, "Behavior at a Choice Point," pp. 150-151.

100. Ibid., pp. 145-146, 155.

101. Ibid., pp. 161-162. The notion of vectorial combination was drawn from Kurt Lewin, whose concepts were described by Tolman as "the best lead that I have at present" for conceiving of how the various intervening variables might combine to produce overt behavior (ibid., p. 162).

102. Ibid., p. 163.

103. Thus, before describing his own intervening-variable theory of behavior, Tolman warned that "however complicated what I am actually going to present may appear, it will be in reality an oversimplified and incomplete version" ("Behavior at a Choice Point," p. 156). Tolman's unyielding recognition of the complexity of behavioral phenomena is reflected in the fact that he and a colleague were the first to apply the (now widely used) methods of multivariate statistics to psychological experiments. Unlike the statistical techniques used in psychology up to that point, these methods permitted the analysis of interactions between the various factors governing behavior, a fact of obvious relevance to Tolman in his struggle to disentangle the complex interactions between intervening variables. See Richard S. Crutchfield and Edward C. Tolman, "Multiple-Variable Design for Experiments Involving Interaction of Behavior," Psychological Review 47 (1940): 38-42.

104. Edward C. Tolman, Benbow F. Ritchie, and Donald Kalish, "Studies in Spatial Learning. I. Orientation and the Short-Cut," Journal of Experimental Psychology 36 (1946): 13-25, on p. 14. The paper referred to was Rudolf Carnap, "Testability and Meaning," Philosophy of Science 3 (1936): 419-471; 4 (1937): 1-40.

105. Benbow F. Ritchie to Laurence D. Smith, 5 February 1981; Donald Kalish to Laurence D. Smith, 23 February 1981.

106. Ritchie to Smith, 5 February 1981.

107. Kalish to Smith, 23 February 1981.

108. See the section titled "Remarks on the Lore of Behaviorism and Logical Positivism" in Chapter 10 below.

109. Victor F. Lenzen to Charles W. Morris, 18 December 1934, Unity of Science Collection, Regenstein Library, University of Chicago, Chicago, Ill. Subsequent references to this collection will give the title of the collection only.

110. Charles W. Morris, "Some Aspects of Recent American Scientific Philosophy," Erkenntnis 5 (1935): 142-151, on p. 147.

111. The program for the congress was announced in Erkenntnis 5 (1935): 428, where Tolman's paper is listed as "An Operational Analysis of Motives."

112. Edward C. Tolman, "An Operational Analysis of 'Demands'," Erkenntnis 6 (1937): 383-392. As it turned out, neither Tolman nor Brunswik attended the congress. Brunswik was unable to attend because the Rockefeller Foundation, under whose auspices Brunswik was at Berkeley at the time, was unwilling to grant permission for Brunswik to leave the United States before the end of his fellowship period ("Egon Brunswik," Fellowship Cards, Rockefeller Archive Center, North Tarrytown, N.Y.). Tolman's reasons for not attending are unclear. Brunswik's failure to attend may have been the major reason. Another possible reason is that Tolman had had a "very strenuous year," in part because his two daughters were badly injured in an explosion the previous December (an accident which killed Stephen Pepper's son). See Edward C. Tolman to A. A. Roback, 22 August 1936, Leytham Files, Archives of the History of American Psychology, Akron, Ohio. Alternatively, Tolman might simply have been losing interest in logical positivism by this time.

113. Tolman, "Operational Analysis," p. 389.

114. *Ibid.*, p. 390.

115. Otto Neurath to Charles W. Morris, 26 May 1936, Unity of Science Collection.

116. Otto Neurath to Rudolf Carnap and Charles W. Morris, 5 November 1936, Unity of Science Collection.

117. Otto Neurath to Rudolf Carnap and Charles W. Morris, 12 November 1936, Unity of Science Collection. Significantly, Neurath's remarks suggest that the organizers of the Unity of Science movement were encountering some difficulty in finding a psychologist who was interested in applying logic to psychology. (As an aside, it may be noted that Neurath was just learning English at this time--hence his awkward writing style.)

118. Neurath's correspondence indicates that Tolman had been dropped from participation by early 1937, although the final authorship was not announced in print until 1938. See Otto Neurath, "Einleitung," Erkenntnis 7 (1937-38): 135-137, on p. 137.

119. See *ibid.*, p. 136 for Tolman's membership on the latter committee. He is listed as a member of the Encyclopedia Advisory Committee on the copyright pages of all volumes and numbers of the Encyclopedia.

120. Ritchie to Smith, 5 February 1981. Significantly other of Tolman's former students and colleagues were wholly unaware of his involvement with the Unity of Science movement.

121. According to Brunswik, he and Tolman missed the 1939 congress because of their attendance at psychology meetings being held at the same time (Egon Brunswik to Charles W. Morris, 31 July 1940, Unity of Science Collection). It is uncertain whether Tolman actually attended the Chicago congress in 1941; in any case, his name does not appear on a short (but probably incomplete) list of persons attending the meetings. At the 1953 congress, Tolman chaired the "Symposium on the Probability Approach to Psychology" (see program for the "Berkeley Conference for the Unity of Science," Unity of Science Collection).

122. The titles of these papers were: "Psychologie als objektive Beziehungswissenschaft" (1935), "Gestalt und Zeichen in kausaler Wissenschaften" (1937), "The Conceptual Focus of Some Psychological Systems (1938), "Organismic Achievement and Environmental Probability" (1941), and "Probabilistic Psychological Theory and the Representative Design of Experiments" (1953). (The 1936 and 1938 papers were submitted for the respective conferences, but eventually not presented at them.)

123. Egon Brunswik, "The Conceptual Framework of Psychology," in Foundations of the Unity of Science, vol. 1, no. 10, ed. Otto Neurath, Rudolf Carnap, and Charles W. Morris (Chicago: University of Chicago Press, 1970). Also published as a separate monograph in 1952, as cited in note 54 above. Brunswik had planned to call the monograph "The Methodological Foundations of Psychology," but changed the title after Tolman had teasingly referred to the work as "The Mythological Foundations of Psychology" (Egon Brunswik to Charles W. Morris, 3 September 1950, Unity of Science Collection). Tolman's teasing was undoubtedly more than a mere attempt at humor: as will become evident in the next chapter, his pragmatic-contextualist-functionalism epistemology was incompatible with foundational approaches to knowledge.

CHAPTER 5

TOLMAN'S PSYCHOLOGY OF SCIENCE

As we shall see in the chapters that follow, Hull and Skinner developed psychological accounts of science by explicitly building them up on the basis of their fundamental epistemological concepts. For Hull the basic epistemological unit was the serially conditioned habit, and for Skinner it is the operant. Having initially analyzed behavior to arrive at these basic units, they were left with the task of working backwards to synthesize accounts of the higher-level phenomena of cognition and, eventually, of science. But for Tolman there was no such problem because his basic concepts were already explicitly cognitive. The sign-Gestalt-expectation, for instance, was from the outset characterized in terms suited to the phenomena of cognition and science. That is, Tolman spoke of it as a kind of postulation, propositionalizing, or hypothesizing--in other words, as an activity which is subject to confirmation or disconfirmation, revision, and refinement. Because Tolman's concepts were already formulated at the cognitive level, his application of them to science itself was not as salient in his writings as were the more explicit applications of behavioral psychology to science that are found in the writings of Hull and Skinner.

But Tolman did include science itself in the account of his systematic psychology, and the evidence of his having done so is scattered throughout his works.

Science as Behavior: Mazes, Hypotheses, and Maps

In Chapter 3, it was noted that Tolman relied heavily in his scientific thinking on spatial diagrams, analogies, and metaphors. This was true also of his thinking about science. The mazes that he used in his animal studies were well suited to his proclivity for spatial analogies, and in time the maze became a fundamental metaphor for him. He wrote, for example, that:

The world for philosophers, as for rats, is, in the last analysis, nothing but a maze for discrimination-manipulation possibilities, extended or narrow, complex or simple, universal or particular.¹

In one of his later classic papers, he spoke of "that great God-given maze which is our human world" and asserted that one of the causes of aggression and war is the narrowness of people's maps of the world.² Clearly, Tolman took his metaphor seriously. It served him not only as a world-view in which psychology and science in general took their places but also as a heuristic for his research. In its contextualist version of a multidimensional spider's web, for instance, it was translated into experiments on "string-pulling," in which rats, literally and figuratively, followed up strands of texture (strings) in order to

achieve goal objects (trays of food).³ And we have already seen how his maze apparatuses transformed his conception of purpose and cognition into spatial characteristics. The importance of the maze in Tolman's thought is reflected in a poem with which he concluded his presidential address to the APA:

To my ratiocinations
 I hope you will be kind
 As you follow up the wanderings
 Of my amazed mind.⁴

The verse, with its pun on "amazed," neatly captured Tolman's attitude toward science. His mind was indeed "amazed," i.e., suffused with maze-thinking, but it was also amazed in the sense that Tolman approached science with a sense of wonderment and a respect for the complexity of the natural world. Moreover, as will be discussed below, science for Tolman was a series of wanderings, that is, an exploratory activity. Such activity was to be guided by hypotheses, but it was not to be constrained or routinized by preconceived formulas for inquiry. In sum, Tolman held the world to be a complex, richly articulated maze which comes to be known in varying degrees by rats, ordinary humans, and scientists alike through their exploratory activity. Such activity was represented as strand-following or movement along a path.

This knowledge-seeking activity of movement through the world-maze was mediated and guided, according to Tolman,

by hypotheses. In Tolman's equivalent of what Brunswik called "intendings" and "conquerings," the organism was said to "hypothesize" and "confirm" (or perhaps disconfirm) its hypotheses through action in the environment. Tolman was speaking of the "confirmation" of hypotheses as early as 1933, and the concept played the role occupied by the notion of reinforcement in other learning theories.⁵ In describing Tolman's cognitive behaviorism, the authors of a classic text on learning theory have written that "[t]he goal-object, by its presence or absence, verifies or refutes hypotheses." They go on to characterize the role of confirmation in Tolman's theory:

The principle in Tolman's system that most nearly replaces reinforcement is the principle of confirmation. If an expectancy is confirmed, its probability value is increased; if an expectancy is not confirmed, its probability value is decreased (i.e., it undergoes extinction).⁶

Thus, Tolman was already construing learning in terms that made his account of it immediately applicable to science. Indeed, his definition of learning as "essentially the correction of old hypotheses and the formation of new ones" was in itself virtually a definition of science.⁷ For Tolman, as for his neorealist forbears, all knowledge, whether that of the scientist or the subject, is on the same epistemological plane. From the naturalistic perspective, the rat's knowledge is in principle no different from the scientist's.

Tolman's treatment of rats and humans in equivalent epistemological terms was rounded out in his well-known paper "Cognitive Maps in Rats and Men" (1948). In that article, he took the final step of attributing maps to rats. Ever since Purposive Behavior sixteen years earlier, Tolman had construed theories as maps, and now he was, in effect, claiming that rats and humans are alike in having theories. Hypotheses, once confirmed, were representations of mean-end relations, i.e., of what leads to what, in the world-maze. A set of well-confirmed hypotheses could, just as in science, be joined together into a composite map or theory. A cognitive map, then, was a sort of global representation of the means-end field. But Tolman's attribution of cognitive maps to rats was not something he undertook lightly or merely for the sake of giving rats epistemological parity with humans. Rather, it was suggested by a series of experiments in which rats trained in highly complex mazes continued to respond efficiently when the maze was altered so that previously learned routes were blocked. The rats appeared to have learned, not specific responses, but something about the general layout. The results illustrated one of the features of maps which would make them useful instruments for rats or humans: They could serve as effective guides for action in an ambiguous and changing environment. All in all, for Tolman, science was to be understood not in logical terms but in

psychological terms--or, to be more exact, in the spatial terms of his cognitive behaviorism.

But, as has often been argued, to construe science in a psychologistic fashion is to endanger its claim to provide genuinely universal laws and theories. If the products of science are dependent on and constrained by the psychological make-up and history of the scientists who produce them, then those products can no longer be held up as absolute truths. Tolman did not deny it. All science is ultimately behavioral and therefore, he admitted, constrained by the psychological nature and needs of human beings. Moreover, such a view necessarily leads to a pragmatistic approach to science. In a passage of Purposive Behavior that expressed all these points, Tolman wrote:

. . . it is to be emphasized that in the case of physics human knowledge of the external object is still limited and conditioned by a sort of distillation from all human behavioral needs and capacities. Even physics' account of the external world is, in the last analysis, an ultimately, though very abstracted, behavioral account. For all knowledge of the universe is always strained through the behavior-needs and the behavior-possibilities of the particular organisms who are gathering that knowledge. That 'map' knowledge is 'true' which 'works,' given the particular behavior-needs and the particular behavior-capacities of the type of organism gathering such knowledge. Physics and purposive behaviorism are both, therefore, but humanly conditioned, 'behavioral' maps.

In conclusion, it seems--we ask the philosopher--that we are asserting, are we not, a pragmatism? For we are asserting that all human knowledge, including physics, purposive behavior-

ism and our own present remarks, are but a result-
ant of, and limited by, human behavioral needs
and human behavioral capacities.⁸

A map or theory could be said to be "true" only in the sense that it "worked," and what would work in any instance was entirely relative to the specific needs and capacities at hand.

If science in general was viewed as a constrained product of human psychological activity Tolman was acutely aware that his own purposive behaviorism was in no way exempt from such limitations. In fact, he was a consistent champion of what one of his students has called "epistemic humility,"⁹ and he was not merely engaging in personal modesty when he pointed out the limitations of his own system or its potential ill-effects on those who would take it too seriously. Near the end of Purposive Behavior, he wrote:

It is obvious that the preceding pages have attempted to offer a new 'system' of psychology. But system-making is very properly open to suspicion. It is the resort of arm-chair hidlers from reality. And, once set up, a system probably does as much harm as good. It serves as a sort of sacred grating behind which each novice is commanded to kneel in order that he may never see the real world, save through its interstices. And each system is so obviously bound to be wrong. It is twisted out of plumb by the special cultural lack of building materials inherent in the time and place of its origin, as well as by the lack of skill of its individual architect or architects.

An apology, therefore, is in order. We can, in short, merely hope that the propositions summarized in the succeeding pages, when set up in front of you as a pattern of mullions through which to observe the psychological landscape, will

serve (but only temporarily) to limn into prominence for you new areas for the gathering of data.

But may neither you nor we ever seek to hold up these propositions, save in a somewhat amused, a somewhat skeptical, and a wholly adventure-seeking and pragmatic behavior attitude.¹⁰

As a sort of rough cognitive map, a system could serve as a guide to the "psychological landscape" (again, a spatial image) and suggest certain areas for investigation; but a system could also blind and restrict one's investigations, and it would turn out to be wrong in the long run anyway. Cognition, whether in rats or scientists, is fallible, and the best one can do in the face of this fallibility is to keep exploring, remain open-minded, and maintain a "pragmatic behavior-attitude."

Tolman continued to hold to this pragmatic orientation throughout his career. His refusal to systematize his views in precise terms became especially noticeable in the late 1930s when Clark Hull was vigorously urging all psychologists to formalize their systems. But Tolman remained skeptical of the value of formalizations, emphasizing instead the heuristic capacity of theories--especially loosely formulated ones--to suggest fruitful research.

In 1952, he wrote:

Theory is viable and to be justified only in so far as it stimulates, or is stimulated by, research. My theoretical pronouncements have, to be sure, usually been phrased merely loosely and programmatically. And so they have seldom made possible any precise theoretical deductions which could then be specifically subjected to experimental test. Nevertheless, these

theoretical meanderings have conditioned me and my students to be interested in certain kinds of experiment. The theory though loose, has been fertile; perhaps fertile primarily because loose.¹¹

Just as Tolman had observed his rats to learn about their environments by merely wandering and exploring,¹² he felt that his own knowledge of the world-maze came through his "theoretical meanderings." The means of learning and the circumstances in which it takes place were, for Tolman, the same for the scientist as for the rat. Earlier, he had written that "[h]ow fast and in what manner a rat 'learns' will be conditioned among other things by the range, methodicalness, and flexibility of his exploratory impulses."¹³ And it was just so for the scientist: one needed, for sure, to be methodical, but not at the expense of the range and flexibility of one's explorations.

In short, Tolman held a thoroughly psychological view of science. He freely acknowledged the constraints on science which that view entailed, and he embraced the pragmatic view of theories and truth which supported his view. Furthermore, he followed through with his pragmatic-instrumentalist view of theories by emphasizing the function they serve in the context of discovery. All of these facts about Tolman's views of science have a significant bearing on the nature of his relationship to logical positivism. If labels must be used, Tolman was not a logical positivist but rather a pragmatist. The differences between the two

positions are not always easy to perceive, but they are not necessarily minor differences. Writing on the problem of knowledge, one philosopher has characterized the difference as follows:

Whereas most logical positivists and empiricists start with an analysis of scientific knowledge, pragmatic naturalists usually begin with the natural agent coping with a practical environment and struggling for survival and advance. This setting completely transforms the nature of the problem.¹⁴

For the logical positivists, knowledge was to be accounted for primarily in terms of its linguistic products, and logic therefore became the major tool of analysis. But for the pragmatist, as Tolman's friend C. I. Lewis put it, "[t]he primary and pervasive significance of knowledge lies in its guidance of action; knowledge is for the sake of doing."¹⁵ Within the pragmatist framework, the narrow distinctions of formal logic were more otiose and burdensome than they were a source of insight about knowledge. That Tolman tended to view logical analysis in this light will be shown in the following section.

Logic and the "Pragmatic Behavior-Attitude"

For Tolman, science is a type of learning, and learning in the face of an ambiguous and oftentimes changing environment calls for flexibility on the part of the organism. In other words, the organism--rat or scientist--needs to maintain what Tolman called a "pragmatic behavior-

attitude." For the pragmatic scientist, this attitude entails not letting one's concepts restrain one's exploratory impulses and not letting one's systematic thinking become overly rigid. As we have seen, Tolman even tended to regard his own theorizing as fruitful because it remained loosely formulated.

Tolman's general attitude toward science dictated his views on the use of logic in science, and he was, in fact, outspokenly skeptical and even critical of attempts to make science logically rigorous. This was the case both before and after his contact with logical positivism. As early as 1923, he was expressing his sentiments on the issue. In that year he wrote:

Perhaps fortunately . . . , men (including psychologists) are not over nice in their logic so that, as a matter of actual practice, they have proceeded gaily with their experiments, leaving the purely methodological analysis of their procedure for post facto dissection.¹⁶

But it was especially after Hull began making a methodological issue out of the use of logic in psychology that Tolman gave voice to his views. In 1944, Tolman wrote to Spence that he was deliberately keeping his system in a programmatic rather than rigorous form because he did not want "to get hardened and rigid too soon."¹⁷ Or again, as he later put it, "To attempt to build psychology on the analogy of a closed mathematical or logical system seems to me a 'bad error'.¹⁸ Arne Naess, who spent a

year (1938-39) studying the research activities of both Tolman and Hull, has commented that "[t]he philosophical background of Tolman, and his anti-pedantic inclinations made him impatient with anything masquerading as logic."¹⁹ Similarly, after trying to interest Tolman and other psychologists in a project devoted to the logical explication of psychological concepts, Benbow Ritchie concluded that "psychologists are not to be distracted by logic-chopping no matter how carefully it is chopped."²⁰

Shortly before his death, Tolman gave one last, lengthy exposition of his systematic psychology. The account contained frequent expressions of his misgivings over logical analyses of scientific method. Near the beginning of the account, Tolman expressed himself in the following words:

. . . I suppose I am personally antipathetic to the notion that science progresses through intense, self-conscious analysis of where one has got and where one is going. Such analyses are obviously a proper function for the philosopher of science and they may be valuable for many individual scientists. But I myself become frightened and restricted when I begin to worry too much as to what particular logical and methodological canons I should or should not obey. It seems to me that very often major new scientific insights have come when the scientist . . . has been shaken out of his up-until-then approved scientific rules. . . .²¹

Later in the essay, Tolman commented on the distinction--one popularized by the logical positivists--between data language and construct language, saying ". . . I myself

can neither get very interested nor completely understand much more refined logical distinctions."²² And, again eschewing such distinctions, he wrote, "I shall not say anything about 'implicit' definition, 'explicit' definition, 'empirical or operational' definition, nor 'coordinating' definition, since my knowledge of these logical distinctions is too slight."²³ In short, Tolman was refusing to adopt and work with the conceptual framework that had evolved under the auspices of logical positivism. In his view, psychology would best remain an exploratory undertaking not to be saddled with logical and methodological refinements that were of little or no relevance to ongoing investigation. In concluding his essay, Tolman penned the following oft-quoted remarks:

The system may well not stand up to any final canons of scientific procedure. But I do not much care. I have liked to think about psychology in ways that have proved congenial to me. Since all the sciences, and especially psychology, are still immersed in such tremendous realms of the uncertain and the unknown, the best that any individual scientist, especially any psychologist, can do seems to be to follow his own gleam, and his own bent, however inadequate they may be. In fact, I suppose that actually this is what we all do. In the end, the only sure criterion is to have fun. And I have had fun.²⁴

When Tolman had earlier written approvingly of psychologists proceeding "gaily" with their experiments, he was not merely using a figure of speech. For him, science was properly a joyful and often spontaneous process of discovery, not a

slavish following of rules, and certainly not what Hull called "the long and grinding labor of the logical derivation of a truly scientific system."²⁵

In his final statement of his system, Tolman commented on the nature of intervening variables and, in doing so, shed new light on the role they were to play in his psychology. Given the great number and complexity of functions relating the different levels of intervening variables (and potential unforeseen interactions between them), Tolman expressed serious doubt as to the feasibility of working out an entire exact system of psychology. There was simply not enough known about all the possible relationships between variables. As Tolman put it:

Psychology, given all its many parts, is today still such a vast continent of unknowns that it has always seemed to me rather silly to try to be too precise, too quantitative, too deductive and axiomatic, save in very experimentally overcontrolled and overlimited areas.²⁶

Tolman had adumbrated his concern with the heuristic value of introspection in his APA presidential address, and now he was emphasizing the heuristic value of the intervening variables in conjunction with phenomenology. In effect, Tolman was suggesting that because of the unavoidable complexity involved in the use of defining experiments the intervening variables could not plausibly figure into the context of justification, but they could at least play a valuable role in the context of discovery.²⁷ In this conception, he wrote, intervening variables

are merely an aid to thinking ('my thinking,' if you will). All anyone really sees are the empirically stipulated independent and dependent variables. In developing notions of what happens in between--such as beliefs, expectancies, representations, and valences and finally what I call performance vectors and their interactions--all I am really doing is setting up a tentative logic (or psychologic) of my own, for predicting what the dependent behavior should be and how it should be affected by variations in such and such sets of independent variables.²⁸

Tolman's assignment of intervening variables to a role in the investigator's "psychologic" constituted a retreat from the stronger claim that they could be experimentally defined, but it was a move he had foreshadowed and one which was consistent with his generally pragmatistic outlook. It was pragmatism, after all, that emphasized the leading-on character of theories and concepts, that is, their fruitfulness in suggesting new routes in the process of discovery.

All told, Tolman's view of science as an uninhibited operation of exploration and discovery made little room for the careful logical distinctions that were characteristic of the logical positivist view. As a result, Tolman was by and large unsympathetic to applications of logic to science, either in the form of reconstructions of its products or of codifications of its canons of procedure. Furthermore, Tolman appears to have held a view of the nature of logic itself that was at odds with the logical positivist view of logic. Only once in his written

works did Tolman address the character of logic per se, as opposed to its applications. In Purposive Behavior, he argued that an environment (means-end field) has certain abstract properties such as leading-to-ness, distance, direction, multiple trackness, mutual alternativeness, and hierarchicalness. In any given means-end-field, these features would be manifested in concrete particulars, but they would also have a status as what he called "formal" principles or relations. Then, in a brief section titled "Means-End-Relations and Logic," Tolman proceeded to make the following remarks:

We shall now suggest that these 'formal' (field-, means-end-) relations are fundamentally the sort of thing with which the logician qua logician is concerned. That is, as we see it, the especial task of the logician is to discover just how many such independent means-end-relations have to be assumed--or just in how far they may be reduced one to another. For we are asserting that logic does (or should) concern itself with all the different kinds and complications and correlations of the fundamental means-end-facts of leading-on-ness and direction-distance correlations. It is the task of logic to build up a set of abstract rules with regard to the types and kinds of mutual interdependence of the facts of leading-on-ness and direction and distance. But we are not logicians and we must not try to usurp their function. We wish merely to suggest that this, as we see it, is the empirical stuff of logic. Logic, we assert, does naught but deal with the 'forms' to be found in means-end-field--as these 'forms' obtain for man, for apes, for cats, or for rats. For, if the logician be truly open-minded and catholic, he will be as much interested in the logic, i.e., the character of the means-end-relations, which obtain for the cat, or the rat, as he is in those which obtain for the man or the ape.²⁹

In other words, Tolman was claiming that, in some sense, logic expresses the abstract structure of the environment--at a level so abstract, in fact, that it is independent of which species might be operating in the environment. From a logical point of view, it is not entirely clear just what Tolman meant by these remarks, and he never elaborated on them; however, it is clear that in speaking of the "empirical stuff" of logic, he was interpreting logic in a way that departed significantly from the logical positivist view that logic is empirically empty. As will be shown in the following chapters, Hull and Skinner developed psychologistic accounts of logic that were more explicitly formulated than Tolman's but, like Tolman's, were incompatible with the logical positivist view.

It is worth re-emphasizing, in conclusion, that Tolman was from the outset of his intellectual career a member of the pragmatist tradition. Holt and Perry were pragmatists, as was Tolman's fellow student and lifelong friend Pepper. Significantly, Pepper was a major critic of logical positivism. In 1936, he wrote an influential rebuttal of Herbert Feigl's theory of mind,³⁰ and later recorded his negative reaction to the logical positivists:

I felt from their attitude and the tone of their statements, even before critically studying them, that they were not meeting the problem that needed to be met. I doubted if many of them had ever fully felt the problem. . . . Here was a method running away with issues, evidence, and value itself. It was, as Loewenberg once remarked, methodolatry.³¹

Pepper was the philosopher with whom Tolman was closest personally and probably also intellectually. It seems likely, then, that Tolman was well aware of Pepper's views and that they tempered his own response to logical positivism.

To be sure, Tolman was--especially in the thirties--sympathetic to and influenced by logical positivism, but the influence of logical positivism must be assessed in the context of Tolman's overall pragmatist orientation. His receptiveness of logical positivism was both engendered and limited by that pragmatist outlook. As we shall see, much the same conclusion may be drawn about Hull and Skinner.

Notes for Chapter 5

1. Edward C. Tolman, "A Behavioristic Theory of Ideas," in Behavior and Psychological Man (Berkeley: University of California Press, 1966), pp. 48-62, on p. 61. This article originally appeared in the Psychological Review 33 (1926): 352-369.

2. Edward C. Tolman, "Cognitive Maps in Rats and Men," in Behavior and Psychological Man (Berkeley: University of California Press, 1966), pp. 241-264, on p. 264. Originally published in the Psychological Review 55 (1948): 189-208.

3. Edward C. Tolman, "The Acquisition of String-Pulling by Rats--Conditioned Response or Sign-Gestalt?" in Behavior and Psychological Man (Berkeley: University of California Press, 1966), pp. 130-143. Originally published in the Psychological Review 44 (1937): 195-211.

4. Edward C. Tolman, "The Determiners of Behavior at a Choice Point," in Behavior and Psychological Man (Berkeley: University of California Press, 1966), pp. 144-178, on p. 172. This article appeared originally in the Psychological Review 45 (1938): 1-41. The poem, which also contains a pun on "rat," was actually penned by Tolman's friend the noted philosopher of education, Alexander Meiklejohn, as an inscription in a book given to Tolman.

5. Edward C. Tolman, "Gestalt and Sign-Gestalt," in Behavior and Psychological Man (Berkeley: University of California Press, 1966), pp. 77-93, on pp. 81ff. This paper originally appeared in the Psychological Review 40 (1933): 391-411.

6. Ernest R. Hilgard and Gordon H. Bower, Theories of Learning, 3d ed. (New York: Appleton-Century-Crofts, 1966), p. 207.

7. Edward C. Tolman and Egon Brunswik, "The Organism and the Causal Texture of the Environment," in The Psychology of Egon Brunswik, ed. Kenneth R. Hammond (New York: Holt, Rinehart and Winston, 1966), pp. 457-486, on p. 473. This paper was originally published in the Psychological Review 42 (1935): 43-77.

8. Edward C. Tolman, Purposive Behavior in Animals and Men (New York: Century, 1932), p. 430.

9. The phrase is that of Donald T. Campbell, quoted in Howard E. Gruber, "Darwin on Psychology and Its Relation to Evolutionary Thought," in Psychology: Theoretical-Historical Perspectives, ed. R. W. Rieber and Kurt Salzinger (New York: Academic Press, 1980), pp. 145-174, on p. 150.
10. Tolman, Purposive Behavior, p. 394.
11. Edward C. Tolman, "Edward Chace Tolman," in A History of Psychology in Autobiography, vol. 4, ed. Edwin G. Boring, Heinz Werner, Robert M. Yerkes, and Herbert S. Langfeld (Worcester, Mass.: Clark University Press, 1952), pp. 323-339, on p. 335.
12. In Tolman's famous "latent learning" experiments, unrewarded rats learned as much about a maze by wandering through it as did rats which were explicitly rewarded for reaching certain goals in the maze. The classic study on latent learning was reported in Edward C. Tolman and C. H. Honzik, "Introduction and Removal of Reward, and Maze Performance in Rats," University of California Publications in Psychology 4 (1930): 257-275.
13. Edward C. Tolman, "Purpose and Cognition: The Determiners of Animal Learning," in Behavior and Psychological Man (Berkeley: University of California Press, 1966), pp. 38-47, on p. 42. This article was originally published in the Psychological Review 32 (1925): 285-297.
14. Andrew J. Reck, The New American Philosophers: An Exploration of Thought Since World War II (Baton Rouge, La.: Louisiana State University Press, 1968), p. 29.
15. C. I. Lewis, Analysis of Knowledge and Valuation (La Salle, Ill.: Open Court, 1946), p. 3.
16. Edward C. Tolman, "A Behavioristic Account of the Emotions," in Behavior and Psychological Man (Berkeley: University of California Press, 1966), pp. 23-31, on p. 23. This article appeared originally in the Psychological Review 30 (1923): 217-227.
17. Edward C. Tolman to Kenneth W. Spence, 28 June 1944, Spence Papers, Archives of the History of American Psychology, Akron, Ohio.

18. Edward C. Tolman, "Principles of Purposive Behavior," in Psychology: A Study of a Science, ed. Sigmund Koch, 7 vols., vol. 2, (New York: McGraw-Hill, 1959), pp. 92-157, on p. 150.
19. Arne Naess to Laurence D. Smith, 7 April 1981.
20. Benbow F. Ritchie to Laurence D. Smith, 5 February 1981.
21. Tolman, "Principles of Purposive Behavior," p. 93.
22. Ibid., p. 149.
23. Ibid., p. 150.
24. Ibid., p. 152.
25. Clark L. Hull, "Mind, Mechanism, and Adaptive Behavior," Psychological Review 44 (1937): 1-32, on p. 31.
26. Tolman, "Principles of Purposive Behavior," p. 98.
27. Tolman's use of intervening variables in the context of discovery has been discussed by Robert E. A. Shanab, "A Defense of Tolman's Position Concerning Intervening Variables," Philosophical Review (Taiwan) (January, 1972): 139-145, on pp. 143-144.
28. Tolman, "Principles of Purposive Behavior," p. 148.
29. Tolman, Purposive Behavior, pp. 190-191.
30. Stephen C. Pepper, "A Criticism of a Positivistic Theory of Mind," University of California Publications in Philosophy 19 (1936): 211-232. Feigl has written of Pepper that he "repudiated my entire approach, shrewdly spotting some inconsistencies of which I was then unaware," and that Pepper's "sharp criticism of my first essay on the mind-body problem, though painful at the time, had stimulated my re-thinking of that baffling and perennial problem." See Herbert Feigl, Inquiries and Provocations: Selected Writings 1929-1974, ed. Robert S. Cohen (Dordrecht, Holland: D. Reidel, 1981), pp. 71, 84-85.
31. Stephen C. Pepper, World Hypotheses: A Study in Evidence (Berkeley: University of California Press, 1948), pp. viii-ix.

BEHAVIORISM AND LOGICAL POSITIVISM:
A REVISED ACCOUNT OF THE ALLIANCE

VOLUME 2
CHAPTERS 6 - 10

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DISSERTATION

Submitted to the University of New Hampshire
in Partial Fulfillment of
the Requirements for the Degree of

Doctor of Philosophy
in
Psychology

May, 1983

TABLE OF CONTENTS

| VOLUME 2 | PAGE |
|---|------|
| CHAPTER 6: CLARK L. HULL: HIS BACKGROUND AND VIEWS OF SCIENCE..... | 288 |
| Hull's Background..... | 288 |
| Education and Early Career..... | 288 |
| Hull's Turn to Behaviorism..... | 294 |
| Hull's Anticipation of Logical Empiricism (1916-1937).. | 299 |
| The Materialist World-View..... | 300 |
| Materialism..... | 300 |
| Mechanism..... | 307 |
| Deductive Methods..... | 316 |
| Early Interest..... | 316 |
| The Changing Styles of Deductive Theorizing... | 321 |
| Deductive Method versus Metaphysics..... | 330 |
| Method as Guarantor of Progress..... | 334 |
| Method and Advance..... | 334 |
| Objectivity versus Emotionalism..... | 340 |
| Integration of Science..... | 343 |
| Unity of Method..... | 344 |
| Integration of Law..... | 346 |
| Scientific Cooperation..... | 348 |
| Conclusion..... | 352 |
| CHAPTER 7: HULL AND LOGICAL POSITIVISM..... | 368 |
| Hull and the Unity of Science Movement..... | 370 |
| Woodger and Formalized Theory..... | 383 |
| Shared Views of Science..... | 383 |
| The Problem of Definition in Formal Theory.... | 388 |
| The Definition of Intervening Variables..... | 393 |
| The Logic of Theory Construction..... | 404 |
| The Logic Boom in Psychology..... | 404 |
| Logical Empiricism: The Iowa Connection..... | 409 |
| Hull's System: The Logical Empiricist Perspective..... | 412 |
| Hull and the New Methodologists..... | 424 |
| Operationism, Positivism, and Quantification..... | 429 |
| Operationism and Positivism..... | 429 |
| The Quantification of Behavior..... | 434 |
| Conclusion..... | 442 |

| | |
|---|-----|
| CHAPTER 8: HULL'S BEHAVIORAL PSYCHOLOGY OF SCIENCE..... | 458 |
| Knowledge as a Habit Mechanism..... | 460 |
| Behaviorist Theory of Theory..... | 467 |
| Theory as a Habit Mechanism..... | 467 |
| A Behaviorist Theory of Truth..... | 475 |
| Theory and Organism as Parallel Machines..... | 482 |
| An Empirical Interpretation of Logic..... | 488 |
| The Psychology of Logic..... | 488 |
| The Confirmatory Status of | |
| Logical Principles..... | 490 |
| The Hullian View of Logic..... | 494 |
| Conclusion: Hullian Logic versus | |
| Vienna Circle Logic..... | 498 |
| CHAPTER 9: B. F. SKINNER: RADICAL BEHAVIORIST | |
| PSYCHOLOGY OF SCIENCE..... | 513 |
| Skinner's Background and Turn to Behaviorism..... | 515 |
| Background..... | 515 |
| Turn to Behaviorism..... | 518 |
| Intellectual Roots: Biology and Positivism..... | 522 |
| Machian Positivism and Biological Economy..... | 522 |
| Positivist Biology of Behavior: | |
| Loeb and Crozier..... | 542 |
| Skinner's Relation to Logical Positivism..... | 547 |
| Early Interest..... | 547 |
| Reaffirmation of Mach: | |
| Skinner's "Case History"..... | 552 |
| Skinner's Behavioral Epistemology..... | 559 |
| The Concept of the Operant..... | 559 |
| Operant Psychology of Science..... | 563 |
| Conclusion: The "Bootstrap" Nature of the | |
| Epistemological Enterprise..... | 575 |
| CHAPTER 10: CONCLUSION..... | 590 |
| Neobehaviorism and Logical Positivism: | |
| The Alliance Reconsidered..... | 592 |
| Metaphysics, Metaphor, and Method | |
| in Neobehaviorism..... | 592 |
| A Reassessment of the Standard Account..... | 606 |
| Remarks on the Lore of Behaviorism | |
| and Logical Positivism..... | 618 |
| Neobehaviorist Epistemologies and the | |
| New Psychologism..... | 623 |
| Psychological Epistemology and the | |
| New Image of Science..... | 623 |
| Psychologism and the Foundations of Knowledge, | 631 |
| ANNOTATED BIBLIOGRAPHY..... | 648 |
| BIBLIOGRAPHY..... | 657 |

CHAPTER 6

CLARK L. HULL: HIS BACKGROUND AND VIEWS OF SCIENCE

Hull's Background

Education and Early Career

At the peak of his career, Clark Leonard Hull was the most influential and widely known behaviorist in the world.¹ His rise to pre-eminence during the thirties was fueled by a self-conscious desire for fame, a genius for the mechanical, and a record of successes in his psychological research. By the time of his arrival at Yale in 1929, Hull had already invented a sophisticated machine for computing correlation coefficients, performed pioneering studies in concept formation, and become known for his work in the areas of aptitude testing and hypnosis. Also in that year he declared that he was "deliberately making a bid for a certain place in the history of science."² The source of that scientific recognition would be found in the development through the thirties of an elaborate deductive account of adaptive behavior. His would become the grandest of the grand learning theories in what has been called psychology's "Age of Theory."³

Hull was born of an agrarian family in 1884, and found himself at an early age torn with conflict over the

religious values of his Midwestern surroundings. His religious crisis concluded with a rejection of "the whole religious hypothesis," a rejection that left in him a strong urge to be educated. At prep school, Hull discovered geometry, a subject which held an enduring fascination for him. Hull later described this discovery as "the most important event of my intellectual life." Shortly thereafter, he attempted to apply the geometric method to the deduction of "some negative propositions concerning theology" and came to study and admire the deductive technique of Spinoza's Ethics.⁴ Such details in the story of Hull's early life are revealing because they foreshadowed important characteristics of his later thought. Like other behaviorists who were hostile to religious and other nonscientific sources of knowledge and values, Hull was eager throughout his career to attack what he saw as the pernicious remnants of religion and idealist philosophy: intuitive claims, subjective principles, entelechies, and such. Unlike the other behaviorists, however, Hull's faith in the deductive methods of science was such that he believed values as well as knowledge could be deduced from the laws of behavior.⁵

Hull's graduation from prep school marked the beginning of a period of ill health which had important consequences for his life's work. The period began with a serious case of typhoid fever which left him with a memory deficit. Because of this amnesic tendency, Hull

began while in graduate school a series of "idea books" in which he recorded questions and insights that he feared might otherwise escape his recollection. Convinced as he was that creativity is lost with age, Hull also felt that these records would provide him with a store of research ideas to be pursued later in his career. The books, which number twenty-seven in all, contain a wealth of detailed information on the development of his psychological thought as well as important clues to his character.

Hull went on to study mathematics, physics, and chemistry for two years at Alma College in preparation for a career in engineering. At this point, however, he was struck by a second blow to his health: a case of polio which required three years of convalescence and left him able to walk only with the help of a leg brace which he designed for himself. During the period of recovery, Hull decided that he was too feeble to pursue engineering as an occupation and chose psychology instead. Of this decision, Hull wrote:

What I really wanted was an occupation in a field allied to philosophy in the sense of involving theory: one which was new enough to permit rapid growth so that a young man would not need to wait for his predecessors to die before his work could find recognition, and one which would provide an opportunity to design and work with automatic apparatus.⁶

In Hull's view, psychology fulfilled these demands by providing an involvement with theory, a promising avenue

for his ambition, and an opportunity to exercise his mechanical aptitude. In fact, much of Hull's subsequent success would lie in his ability to capitalize on these characteristics of American psychology in the early twentieth century.

After his period of convalescence, Hull went to the University of Michigan to finish his undergraduate degree and then to the University of Wisconsin to pursue graduate work. At the outset of his graduate career, Hull wrote that his future would unfold "in the free atmosphere of a great university" and would involve the creation of an experimental science of the higher mental processes.⁷ Despite his behavioristic turn in the late 1920s, Hull adhered to this goal throughout the course of his life. Indeed, he viewed his work on the theory of conditioned habits as serving this goal, because for him habit provided the raw material--or, more precisely, the computational unit--of mental action. Hull's first investigation of mental processes consisted of a series of experiments on abstraction and concept formation. These studies, which employed the experimental technique of Hermann Ebbinghaus's memory studies, were accepted as his doctoral dissertation and published as a monography in 1920.⁸

For the next nine years, Hull taught at Wisconsin, gradually taking over the teaching duties of older professors. Two of these courses, those in aptitude testing and medical psychology, led to Hull's interests in testing and hypnosis. Research in these areas absorbed most of his

energies during the twenties and culminated in the publication of his volumes Aptitude Testing (1928) and Hypnosis and Suggestibility (1933)⁹. Both books exhibited the methodological rigor of controlled experimentation and statistical treatment which were characteristic of Hull's work.

In Aptitude Testing, he argued that aptitude predictions could be improved by refining the methods of test validation and combining the tests into batteries. Since implementing these suggestions would involve laborious calculations of correlation coefficients, Hull set out to devise a machine that would perform them. His idea books of the mid-twenties are filled with sketches of various machine parts. Armed with university and federal grants, he completed the project in 1925. The machine, which is now on display at the Smithsonian Institution, won Hull renown among statisticians and psychologists and even made money for him. Yet for Hull the invention held deeper significance. He saw in it a demonstration that a purely physical mechanism with the right organization of material components could perform operations characteristic of higher mental processes. Furthermore, such operations could take place without the intrusion of any intrinsically mentalistic powers. As we shall see, Hull's mechanistic bias came to play a crucial role in the elaboration of his behaviorism.

Like his work in aptitude testing, Hull's hypnosis research largely preceded his explicit theorizing about learned behavior but was not wholly unrelated to it.¹⁰ His quantitative studies of post-hypnotic suggestion revealed the functional similarity between this phenomenon and various forms of learning and forgetting. These findings suggested to Hull the interpretation that hypnotic effects are habits--in effect, conditioned suppressions of internally generated stimuli which permit the hypnotist's suggestions to assume control over the subject's responses. To explain such suggestions, Hull invoked a form of ideomotor action in which the hypnotist's idea was presumed to elicit the subject's action. In a paper of 1931, Hull translated the notion of ideomotor action into that of the "pure stimulus act," a concept which was to figure centrally in his evolving theory of behavior.¹¹

The conceptual continuity between Hull's earlier and later work was, for various reasons, downplayed by him. His idea books reveal that once he had become the standard-bearer of an objective behavioral approach to psychology, he looked upon vocational forecasting and hypnosis as areas having rather disreputable scientific standing (despite his efforts to objectify them). He also eventually regarded his research on these topics as diversions which had cost him the precious resources of time and money in his rush to establish a general theory of learning.

Hull's Turn to Behaviorism

Hull's shift of interest from his early work on hypnosis and testing to his behavior theory began in 1925 when he taught a seminar in behaviorism. The period of transition ended in 1933 with the appearance of the book on hypnotism. After 1933, his efforts were completely focused on the theory of behavior. Hull's seminar of 1925 has been identified as "the beginning of his behavioristic theorizing,"¹² and this claim is well borne out by Hull's idea books of the period. The stated aim of the course was to provide "a critical constructive examination of the various behavioristic hypotheses." To this end, Hull chose as texts Watson's Psychology from the Standpoint of a Behaviorist (1924), which was a vigorous defense of behaviorism, and A. A. Roback's Behaviorism and Psychology (1923), an early detailed critique of the movement. Following the early behaviorists, Hull saw the course as "an attempt to translate the older psychology into behavioristic terms, in detail to see how well it works." Thus, he gave as examples the claims that being hungry is food-seeking and that being afraid is the action of flight and nothing more. But like other neobehaviorists who were to follow, Hull viewed consciousness as a phenomenon in need of explanation rather than as something to be ignored or denied.¹³

In his behaviorism seminars of 1925 and 1927, Hull stressed the philosophical background of behaviorism. In identifying the forerunners of behaviorism, he cited Thomas Hobbes, Auguste Comte, the realists F. J. E. Woodbridge and George Santayana, the neorealists Holt and Perry, and the pragmatists John Dewey and William James. But although the influence of these thinkers can be discerned in Hull's thought, the greater influence, at least according to Hull's own reckoning, was that of the British associationists. In fact, he viewed behaviorism as a "direct descendant" of traditional associationism.¹⁴

Hull especially revered Hume's Enquiry concerning Human Understanding (1748), which he referred to as "perhaps the most successful treatise in philosophy or psychology ever written." Hull even went so far as to assert that "a good deal of behaviorism is implicit in Hume and a considerable amount of it is explicit also."¹⁵ Like his associationist ancestors, Hull believed himself to be pursuing a theory of knowledge firmly based on a sound empirical psychology. His admiration of Hume was such that he considered modeling his magnum opus after Hume's Enquiry, treating the same topics and even adopting Hume's headings and chapter divisions. Hull of course abandoned this plan, but as we shall see he did develop a behaviorist theory of knowledge which included an empiricist interpretation of logic. Both Hull and the logical positivists claimed Hume

as an intellectual ancestor, but they drew upon his work in importantly different ways.

Hull's behaviorism seminars were clearly an important factor in the transition from his earlier work to his interest in behavior theory. Three additional factors deserve mention in this regard. The first was Hull's association with the famous Gestalt psychologist Kurt Koffka. Out of a genuine interest in Gestalt psychology, Hull had arranged for Koffka to spend a year (1926-27) at Wisconsin. As recounted in his autobiography, Hull found Koffka's lectures interesting, but he was struck by the amount of time Koffka spent attacking Watson.

While I found myself in general agreement with his criticisms of behaviorism, I came to the conclusion not that the Gestalt view was sound but rather that Watson had not made out as clear a case for behaviorism as the facts warranted. Instead of converting me to Gestalttheorie, the result was a belated conversion to a kind of neo-behaviorism. . . .¹⁶

As was the case with Tolman, Hull's behavioristic thinking was motivated from the beginning by the perception, one shared with the Gestaltists, of inadequacies in Watsonian behaviorism. Even in the twenties, Hull was criticizing Watsonian behaviorism as "crude," naive," and "simple."¹⁷ After reading Tolman's Purposive Behavior in the thirties, Hull noted approvingly that good behaviorist theory would obviate the need for Watson's "half-dishonest advertising" for the purpose of "putting over" behaviorism.¹⁸ Hull's

aversion to behavioristic propaganda was partly responsible for his efforts to ground behaviorism on an objective methodology, although he would later admit to using propaganda to advocate his own system.

A second factor which contributed to Hull's transition to behavior theory from testing and hypnosis work was the publication in 1927-28 of Anrep's translation of Pavlov's Conditioned Reflexes.¹⁹ Hull's thorough study of Pavlov's work led him to theorize about the relationship between the conditioned reflex and trial-and-error learning. This duality of learning phenomena was viewed by Hull, in characteristic fashion, as a challenge to determine the fundamental behavioral laws from which these two classes of learning could be derived as special cases. This challenge, as well as the objectivity of Pavlov's methods, spurred Hull's interest in behavior theory.

The third factor which contributed to Hull's abandonment of his early research was an event that took place shortly after his arrival at Yale. In 1930, after receiving a complaint from the parents of one of Hull's hypnosis subjects, the authorities at Yale conducted an investigation of Hull's research program. The resulting restrictions on Hull's use of subjects were so severe that he soon gave up his studies of hypnosis. The completion of the research which culminated in Hypnosis and Suggestibility was left to Hull's former co-workers in the Midwest, where, as he put

it, "the superstitious fear of hypnosis was not so great."²⁰ Nevertheless, this episode bothered Hull for years and produced in him a sense of caution that extended to his research on intelligent machines.²¹

These events which were instrumental in Hull's turn to behaviorism began to take place in the mid-1920s, but it was not until 1929 that Hull publicly entered the arena of behavior theory with his first paper on learning.²² By that time, Hull was well into his forties. The delays involving his poor health and his excursions into hypnosis and testing caused his slow start on the work he regarded as his most important.²³ Along with this late start, Hull's failing health and his belief that creativity falters with age left him engaged in what he gravely viewed as a race against time. These concerns were often expressed in his idea books during the 1930s and 1940s. For example, when Hull turned fifty in 1934, he wrote the following reaction to a medical report of weakness in his circulatory system:

It is difficult to say how serious this is, but it looks very much like the premature onset of senility. Accordingly it may very well turn out that my expectation that fifty years marks the decline of creative ability will be truer in my case than many. . . . From what evidence I have been able to gather, I have something like even chances of two more fairly effective years.²⁴

Hull typically concluded remarks such as these by drawing up a set of plans for his professional activities in hopes of making optimal use of remaining time. Such

plans usually involved a grueling schedule of publishing and research; significantly, they often called for sacrificing experiment to theory and detail to generality. These tendencies were reinforced by Hull's position in Yale's newly founded Institute of Human Relations, the mandated purpose of which was to coordinate a theoretical integration of social science research. In the race to establish his system of behavior, Hull increasingly took on the role of overseer, organizer, and theoretician, while those around him carried out the empirical investigations.²⁵ This division of labor contributed to Hull's inclination to extend his theoretical framework beyond reasonable warrant in his effort to unify the social sciences.

As we shall see, it was this interest in the integration of the sciences that eventually brought Hull into contact with the logical positivists in the Unity of Science movement. But by the time of this contact in 1937, Hull had independently developed a philosophy of science which bore a strong kinship to that of the logical positivists.

Hull's Anticipation of Logical Empiricism (1916-1937)

The primary aim of the remainder of this chapter is to describe the development of Hull's philosophy of science, especially as it was articulated prior to his encounter with the logical empiricist movement. His philosophy will be

laid out in terms of four related themes which figured prominently both in Hull's thought and at least some versions of logical empiricism. The general parallels between these two independently developed philosophical stances will be sufficiently patent as to not require more than passing comment. The emphasis will instead be placed on the interrelations between the themes in Hull's system. Against this background, the subsequent chapter will describe specific interactions between Hull and representatives of logical empiricism. These episodes took place within a context of intellectual confluence--as would be expected--over broad issues, but they also reveal divergences over significant details. As a way of describing how significant these details could be, the third and final chapter on Hull will show that his epistemological views included naturalistic interpretations of knowledge, including logic, which amounted to a behavioristic psychologism. Such a psychologism was sharply incompatible with the standard view of logic which stood at the very core of Vienna Circle philosophy.

The Materialist World-View

Materialism

As we have seen, E. C. Tolman followed the lead of his neorealist mentors in embracing a position of metaphysical neutrality. This neutrality, which corresponded

in spirit if not detail to Carnap's ametaphysical stance, stemmed in part from Tolman's distaste for the materialistic and molecularistic implications of Watsonian behaviorism. Clark Hull, on the other hand, criticized Watson not for his materialism but rather for his failure to give it an adequate defense. Hull was an out and out materialist. In his idea book of 1927-28, Hull stated, "I feel quite sure that all kinds of action, including the highest forms of intelligent and reflective action and thought, can be handled from the purely materialistic and mechanistic standpoints."²⁶ Significantly, this remark was recorded in reaction to anti-materialistic statements made by Holt in his Concept of Consciousness (1914). Hull would have no part of the Jamesian legacy of neutral monism that was an important element in the ametaphysical attitudes of the 1930s. His avowal of materialism came early in his career and remained uncompromising throughout it.

Hull's undisguised enthusiasm for materialism did not, however, indicate any general sympathy with metaphysical commitments. On the contrary, in his published works, materialism was invoked most often as a basis for his polemics against idealist metaphysics or--to avoid what was for Hull a redundancy--simply metaphysics. So seductive and pernicious were the temptations of idealism, in Hull's view, that even the most objective-minded scientist needed to remain alert to its intrusions. Hull's writings

are peppered with warnings, sometimes addressed to himself, against the "spirits and ghosts" which may "infest" the theory of the unwary scientist.²⁷ (Social scientists were considered especially susceptible to infestations of this sort but had no monopoly on them.) Even in the earliest stages of his theoretical development, Hull was expressing this concern. "If I am not careful," he wrote in the twenties, "I run the risk of putting an entelechy into my symbolic system. . . . I must watch myself. If I should slip here it would spoil the whole system."²⁸ The key to avoiding such a risk was firm adherence to a consistent materialism.

Although materialism provided a defense against the "paralyzing influence of metaphysical idealism,"²⁹ it more commonly served Hull in his aggressive attacks on the proponents of idealism. In one typical discussion of "the old idealistic philosophy and its various modern attenuations," he remarked that a "few well placed bombs should bring down the whole structure tumbling."³⁰ He roundly assaulted the "scholastic and theological perversions of intellect," and argued that the possibility of nonmaterial ideas producing physical action could be ruled out as a violation of the principle of conservation of energy.³¹

Hull's offensives generally lacked philosophical subtlety, and they usually left his opponents unidentified. As a result, they had the flavor more of vague polemic

than of reasoned argument. All the same, he was decidedly not tilting at apparitions or straw men. His targets included James Jeans, Sir Arthur Eddington, Alfred North Whitehead, and Hans Driesch (the latter two of whom he had personally encountered during the twenties³²). The eminent physicists Eddington and Jeans had written, in 1928 and 1930 respectively, popular philosophies of science in which the material world was subordinated to the mental world, which was taken to be the ultimate reality.³³ The depth of Hull's disagreement with them can best be appreciated by noting that Hull's plans for developing a materialistic theory of thought called for the explicit emulation of physics. Inevitably, idealist interpretations of physics were anathema to Hull. A similar point can be registered about the famed vitalist Driesch. Any theory of behavior which would emphasize, as did Hull's, both the mechanistic and biologically adaptive characteristics of behavior would find its worst threat in an influential biologist's insistence that adaptation is mediated by a nonmaterial force. Thus, Hull's diatribes against idealist metaphysics can be seen as a natural consequence of his intellectual aims and not as mere rhetorical forays.

Despite the very real differences between physicalism and materialism, they served similar roles for their proponents. Like the physicalism of the Vienna Circle, the materialism espoused by Hull provided a metaphysical stance

from which to issue broadsides against idealist metaphysics and, more generally, unscientific attitudes. Nevertheless, while explicit metaphysics determined the general thrust of such criticism, the details were often framed in the narrower context of methodological issues. After the popularity of Hull's deductive methods had been achieved in the mid-thirties, objective methodology began to replace materialism as the basis of his published assaults on his "idealist" opponents. The immediate targets of these attacks were the Gestaltists. Hull criticized them, first, for their failure to state postulates and deductions therefrom, and only secondarily for their emphasis on subjective experience. After having an argument with the Gestalt psychologist Max Wertheimer in 1939, Hull complained to Spence that Wertheimer had been "unable to give either the number of postulates or the number of theorems in his system."³⁴ By this time, such an observation sufficed, in Hull's view, to discredit an opposing position.

Even though Hull's materialism was the underlying basis for his polemics, he was not as dogmatic in his materialistic beliefs as might be supposed. In many respects, he was open-minded. He recognized, for example, that the final analysis of matter was an empirical issue not yet settled.³⁵ Furthermore, although he was eager to deny the ontological priority of the mental, he did not deny the reality or import of mental events. The phenomena of

insight and purpose were in fact the focus of his explanatory efforts during the thirties.³⁶ In concluding his detailed study of Hull's work, Sigmund Koch lauded this openness on Hull's part by calling him "the first S-R theorist who made a dedicated attempt to avoid cutting down problems to the size of his concepts."³⁷ For Hull, the mental was a subcategory of the physical, but it was an important subcategory which could not be dismissed or ignored.

Here was a point at which Hull felt burdened by Watson's negative tradition. He wrote in 1930 that: "Despite some half-hearted suggestions within recent years, no one has dared to challenge the dogma that an organism made up of consciousnessless particles may not possibly manifest consciousness."³⁸ Hull inclined to the view that organisms, unlike their component particles, really do exhibit consciousness. Along with purpose and insight, consciousness was regarded as something to be explained, not as an explanatory device in its own right. Such phenomena were emergent in the sense of being novel characteristics of whole organisms, but not in the sense of being inherently inexplicable or of existing apart from the material world. Hull firmly rejected the notion of an emergent as "an impassable gap" between physical science and mental phenomena, but was willing to apply the concept of "emergent" to a "significant novel phenomenon" such as

the habit-family hierarchy,³⁹ (which will be discussed in Chapter 8 below).

Hull's willingness to invoke the concept of emergence rested on his important and controversial contention that habits in their various combinations and interactions could eventuate in genuine novelty. Just as a machine could generate higher-order "mental work" on the basis of a small set of mechanical principles, an organism could generate novel adaptive responses, including higher mental actions, from various applications of habit principles. In 1930, Hull voiced his regret that habit had the reputation of being a "merely repetitive" and "rather stupid" process. But if behaviorism's detractors failed to appreciate what Hull called "the real flexibility of habit interactions"⁴⁰, it was in part because he had not yet published his papers of the 1930s in which that theme was artfully developed.

At the time of his move to Yale, Hull gauged the time to be ripe for the full-blown pursuit of a materialistic psychology. Referring to the dialectical materialism of Russia as well as to the popularity of behaviorism and naturalism in America, Hull professed a "deep suspicion that the world is just now pausing for a leap into a profound materialism." Hull's proven skill in mechanical matters would, he felt, enable him to demonstrate the plausibility of a materialistic psychology by designing intelligent automata or, as he called them, "psychic

machines." As Hull recognized, the general idea was not new--Hobbes had attempted a mechanistic psychology--but there had never before been sufficient knowledge of machinery to support the attempt. As of 1930, all of this had changed in Hull's view. He wrote: "The tide of civilization is running in my direction. An epidemic of the psychic machines has sprung up within a year--a truly remarkable phenomenon."⁴¹ Such machines were to assume an important role in Hull's thought and in his efforts to "dissolve the age-old problem of the opposition of mind to matter."⁴²

Mechanism

It will be recalled that Hull's interest in machines antedated his career in psychology and constituted one reason for choosing it. In fact, his fascination with machinery was a recurrent theme in his life. Like Tolman, he had studied engineering in college. In an undergraduate logic course under the critical realist Roy Wood Sellars,⁴³ Hull constructed a logic machine which employed a system of concentric metal plates to generate the implications of various syllogisms and fallacies. During and after his graduate training, his mechanical skills played no small role in his achievements as an experimenter. His automatic devices figure prominently in his autobiography, and his idea books of the mid-twenties brim with sketches of myriad machine parts.

The hard-won success of Hull's correlation machine evidently inspired in him the conviction that machines could play a central role in theoretical endeavors. "It is rather striking," he wrote, "as revealing the characteristic nature of my talent, that the two major scientific projects of my life [testing and adaptive behavior] should be associated with the design of an exceedingly complex automatic machine." The rationale for designing intelligent mechanisms was formulated by Hull in 1926:

It has struck me many times of late that the human organism is one of the most extraordinary machines--and yet a machine. And it has struck me more than once that so far as the thinking processes go, a machine could be built which would do every essential thing that the body does. . . . [T]o think through the essentials of such a mechanism would probably be the best way of analyzing out the essential requirements of thinking, responding to abstract relations among things, and so on. . . . In fact the whole thing can probably be reduced to a mathematical formula. . . .⁴⁴

In this passage, Hull went on to identify some of the features he thought would be required for a machine to exhibit adaptive behavior. Among these were a stock of random movements to be selected upon and a hierarchical system of control to govern the parts of the machine.

The recognition of the significance of hierarchical design is particularly noteworthy. Already in 1926, Hull had arrived at the notion of stimulus-response or habit hierarchies and had attributed them to organisms as a means of generating the flexibility and variety of

adaptive behavior. Mechanical hierarchies were now seen as a way of elucidating organic hierarchies, making their existence plausible, and exploring their capacities. There is a further important implication. If in fact the whole mechanical design can be "reduced to a mathematical formula," that formula will itself most naturally have a hierarchical form, which for Hull meant the form of deductive logic. In effect, Hull gave an early and clear expression of what would become the rationale of cybernetics, thereby anticipating the founding of that field by a decade.⁴⁵ But as we shall see in the following section, Hull's skills for expressing the notion of hierarchical control were restricted to a limited knowledge of logic and simple geometrical reasoning. His mechanical genius seems to have exceeded his ability to give it formal expression.

Given Hull's interest in conditioned habits as the computational basis for higher psychological processes, it is not surprising that the machines actually built by him and his associates were designed to demonstrate conditioning phenomena. Although the mechanical, electrical, and chemical details differed from case to case, all embodied a similar sort of strategy. There was devised some simple analogue of Pavlovian or trial-and-error conditioning which, when subjected to combinations of excitatory and inhibitory procedures, would exhibit

an array of known conditioning phenomena. These included such relatively complex phenomena as generalization, differentiation, higher-order conditioning, summation to a compound, and variability and persistence of responding until the attainment of a goal object.

The first of these machines was built at Wisconsin and was described in a brief article in Science in 1929,⁴⁶ the year in which Hull's first article on conditioning appeared. From that time on, but especially until the mid-thirties, Hull's thinking about behavior was carried out largely in terms of machine design. In his idea book of 1930, he expressed the hope that his planned theoretical work would be "a sufficiently original performance in what really amounts to mechanical design to be fairly impressive."⁴⁷ This remark reveals the intimate relationship between Hull's mechanical interests and his behaviorism, but the relationship is not so evident in his published works. In fact, it is only hinted at in those theoretical papers of the 1930s which laid the foundation for his magnum opus of 1943, Principles of Behavior. For reasons to be discussed shortly, the issue was usually relegated to footnotes and asides. It was, however, raised in significant if inconspicuous ways. In one case, after deducing a type of what might be called purposive behavior, Hull added:

if the type of explanation put forward above be really a sound deduction, it should be a matter of no great difficulty to construct parallel inanimate mechanisms, even from organic materials, which will genuinely manifest the qualities of intelligence, insight, and purpose, and which will insofar be truly psychic.⁴⁸

In another case, Hull deduced the outcome of an experiment which purported to show insightful problem-solving in rats. He then wrote:

To say the same thing in other words, we appear to have before us here a deduction of insight in terms such that it might conceivably be constructed by a clever engineer as a non-living--even an inorganic--mechanism.⁴⁹

The notion of the equivalence of machine design and theory received a boost in 1935 when a close associate of Hull, Douglas G. Ellson, published an account of an electromechanical device which operated on principles exactly analogous to those appearing in Hull's 1930 derivation of simple trial-and-error learning.⁵⁰ Devices of this sort which exhibited intelligent behavior were referred to by Ellson as "mechanical hypotheses." The expression was an apt one, for it suggested both the theoretical character of psychic machines and the mechanical character of hypotheses (the latter aspect will be developed below). Given the assumed correspondence between the design of theory and machine, it was natural for Hull to speak, as he did, of "translating" theoretical postulates into literal mechanisms.⁵¹

One of the parallels between theories and machines was that both could generate novelties. Just as a fruitful set of postulates could combine, with the help of clever deductions, to yield novel predictions of empirical phenomena, a well-conceived mechanical design could produce correlation coefficients, novel adaptive responses, or even "insightful" solutions to problems. The exhibition of genuine intelligence by machines was quite naturally seized upon by the Hullians as dramatic confirmation of their belief in the sufficiency of a scientific, materialistic monism. As Ellson put it, "The gap between organismic behavior and the physical laws of nature, which has generally been accepted as incapable of being crossed, is being slowly but surely bridged."⁵²

In the article in which they described their psychic machine, Krueger and Hull drew conclusions which were more directly anti-metaphysical than Ellson's. They wrote:

It is believed that the construction and study of models of the type described above will aid in freeing the science of complex adaptive mammalian behavior from the mysticism which ever haunts it. The belief is very widespread and persistent that certain complex forms of adaptation cannot take place by any imaginable concatenation of materials without the mediation of some nous, entelechy, soul, spirit, ego, mind, consciousness, or Einsicht. There is, on the other hand, the opposed belief that the above explanatory concepts are but the names of disembodied functions (ghosts) which, insofar as they have any objective existence, are themselves in need of explanation and may conceivably be duplicated by adroit concatenations of materials. . . . As progress is

made by the second group it may be anticipated that the first will retreat to the more and more inaccessible parts of the psychological terrain.⁵³

The exercise of designing intelligent machines was later explicitly advocated by Hull as an "effective prophylaxis" against the sundry forms of "anthropomorphic subjectivism."⁵⁴

In sum, Hull's commitment to mechanism was a central feature of his early research program. It underlay both his conception of adaptive organismic behavior and his conception of theories about that behavior. It served him well in his materialism and in his anti-idealist polemics. Both chronologically and conceptually, Hull's mechanism was prior to his behaviorism. A telling detail may help to crystallize this conclusion. Hull's idea books show in 1928, as a possible title for his magnum opus, "Psychology from the Standpoint of a Mechanist"-- for a mechanist, a most fitting paraphrase of Watson's 1919 title.⁵⁵

Hull's enthusiasm for the mechanistic outlook was such that at one time (1930), he planned to establish at Yale a museum of psychic machines. He listed several extant machines that might be included and discussed how more complex mechanisms could be developed out of these earlier ones. Significantly, the museum was seen as a way to attract physicists, chemists, physiologists, and engineers into his seminars.⁵⁶ In light of the centrality

of mechanistic themes in Hull's psychology, it was natural that a museum of intelligent machines would have been contemplated as a means of integrating research efforts from the various sciences. Although formal plans for a museum were dropped, the concern with integrating the sciences was to become not only the focus of Hull's work at the Institute of Human Relations but also the point of initial contact between Hull and the agents of logical positivism.

It was suggested above that Hull's mechanistic biases lay at the very kernel of his research program. But despite the prominence of mechanistic formulations in his unpublished writings, the extent of their role is far from apparent in his published works. Hull's idea books reveal the probable source of this curious discrepancy. It appears that Hull, with considerable reason, publicly downplayed his mechanistic views out of a fear of suppression. Even before authorities at Yale curbed his hypnosis research, he recorded his concern about the matter. "No doubt," he wrote in 1929, "this connection of extremely complex automatic machines with ambitious psychological projects and programs is a trifle grotesque, though no one seems to notice this very much except myself." He added that he was "pretty certain" to be criticized and called insane.⁵⁷ Following the episode of controversy over his hypnosis research, Hull remarked that the

proposed museum "must be handled discreetly with an avoidance of newspaper publicity" and that he "should not take the models too seriously, at least in the eyes of the public." In conclusion, he noted to himself, "Let it be, rather, a hobby."⁵⁸ Subsequent idea books contain comments which suggest that Hull's research in "psychotechnology" did later come under the threat of suppression.⁵⁹ The outcome of this threat is not revealed, but we can surmise that Hull was not unaffected by it. The last published account of a psychic machine by any of Hull's group appeared in 1935.

For a variety of reasons, the emphasis of the Hullian program shifted during the thirties from the attempt to capture adaptive behavior in the design of literal machines to the attempt to capture it through the construction of theoretical systems. The shift was not so great as it might seem *prima facie*, since machine design and theory construction were considered to be largely equivalent. Despite being an important key to understanding Hull's work, this equivalence has gone almost entirely unrecognized by historians of psychology. One who has recognized this point, but without elaborating on it, is Robert S. Woodworth. He wrote: "Always the inventor, Hull was evidently fascinated by the problem of designing a well-gearred conceptual machine, a theoretical system from which definite laws of behavior could be logically deduced for

submission to the test of experiment,"⁶⁰ It is to Hull's theoretical machinery, its purported power in methodological matters, and its role in the integration of science that the following sections are addressed.

Deductive Methods

Early Interest

As was the case with Hull's mechanistic leanings, his interest in deductive reasoning arose prior to his entry into a career in psychology. His discovery of geometry, an event to which he attached great significance, came while he was in prep school. The ensuing interest in deductive reasoning combined with his mechanical bent in the production of the logic machine during his undergraduate studies at Michigan. Hull's idea book of 1916 shows him to have had an early interest in hierarchies in general as well as in the particulars of logical processes.⁶¹ He viewed his dissertation research on concept formation as an empirical investigation of the reasoning process. And of course the empirical study of thinking from a materialistic viewpoint was the original stated aim of his scientific endeavors.

As Hull's program for the study of thinking was gradually diverted into the analysis of conditioned habits, deductive processes receded from the forefront as

a subject of investigation and came into prominence instead as a method of investigation. When his plan to derive thought and logic from learned habits became mired in the considerable complexities of conditioned behavior, the employment of logic in psychological theorizing emerged as the distinguishing feature of Hullian behaviorism. It remains today the best-known feature of Hull's work, the major source of whatever fame or infamy is attached to his name.

Although Hull's methodological application of logic became obvious only in the 1930s, it had been a concern to him for some time. In his very first idea book (1915-1916), Hull addressed the issue in a short dialogue:

Quest: Just what is the criterion for deciding where logic must be rigorous in the science of psychology and where it must not be applied at all?

Ans: Perhaps in determining matter of fact it may be applied so far as objective evidence goes. But clearly we must not deduce much or perhaps any from preconceived notions. Possibly this means that there can be little theorizing or prophecy--that there is little uniformity in mental things or at least that the uniformity is only approximate, appearing only in averages and central tendencies of other kinds.

X Work this out more X⁶²

Work it out he did. Sometime around 1930, Hull became convinced that psychology was susceptible of deductive theorizing and could therefore become a legitimate natural science.⁶³ The dearth of uniformity in psychological phenomena which concerned him in the above passage

would later be attributed to unavoidable variability in initial conditions and was thus not taken as impugning the deterministic nature of the scientific laws of behavior (see following chapter).

As we have seen, Hull's mechanistic views gave impetus and form to his views on theory. Machine design was seen as translatable into theoretical structure. In the 1926 idea book which contains Hull's first formulation of the rationale for his proto-cybernetics, we find him recognizing the need for a means of expressing that structure. "If I work out a coherent system of S-R psychology," he wrote, "it must be done by means of some coherent symbolism and it must be quantified."⁶⁴ Shortly thereafter, Hull arrived at one characteristic of the required symbolism. While reading Edwin B. Holt's Concept of Consciousness (1914), he was struck by Holt's notion of the neutrality of logic, a notion which Hull decided to give serious consideration in his own system. According to Holt, the proposition and rules of logic subsist in a neutral realm, and they operate whether experienced or not. In this respect, they operate just as they might in a calculating device or a logic machine. Hull's conclusion was an important one: "Possibly this means that logic is not so much neutral as indifferent,"⁶⁵

Of course, Hull's materialism prevented him from accepting Holt's idea of logic's ontological neutrality.

For Hull, logic had to be in some sense material (see Chapter 8). But the possibility which Hull read into Holt--that logic might be epistemologically indifferent--was just what Hull had been looking for. The hierarchical and deterministic characteristics of organismic behavior called for theories of behavior to be couched in hierarchical and deductive logic. But now Hull could go one better. The epistemological indifference of logic meant that those, and only those, theories of behavior which were framed in rigorous logic could be tested and decided between on an objective basis. Furthermore, Hull recognized that this assessment of theories was in principle capable of being carried out by a machine (see below). In the scientific process of deduce-and-test, the closer the scientist is to behaving like a machine, the more objective is the resulting science.

Whether all these implications were fully evident to Hull at the time of his reinterpretation of Holt's neutrality of logic is uncertain. In any case, it was not long before he was spelling them out. Speaking before a faculty group at Columbia University in the summer of 1929, Hull extolled the virtues of deductive reasoning in scientific theorizing. In what was to become a common refrain among psychologists of the Hullian persuasion, Hull argued that deductive methodology could be relied on to decide between competing systems of psychology, and

that such decisions were a straightforward matter of comparing the number of empirically confirmed deductions from each system.⁶⁶

Around the same time, Hull spent a summer teaching at Harvard. His experiences there furthered his ambitions for deductive theorizing in psychology. The summer at Harvard has been described by Frank Beach in his biographical sketch of Hull:

Discussions of various scientific concepts with C. I. Lewis and A. N. Whitehead strengthened Hull's interest in theory-building. At this time he purchased and became thoroughly familiar with Newton's Principia, a work which strongly influenced his thinking from that time on. He also found stimulating the Principia Mathematica of Whitehead and Russell.⁶⁷

As we shall see, Hull's emulation of Newton became a well-known feature of his program. As for the Principia Mathematica, his attraction to it was most likely not due to any working interest in logic--there is no evidence that Hull ever seriously delved into symbolic logic--but rather to its promise as a highly systematic and powerful system, yet one which evaded unnecessary philosophical attachments. Russell and Whitehead had stated in their preface that they had deliberately "avoided both controversy and general philosophy."⁶⁸ There was no need for Hull to work through the theorems to see the point: here was a symbolic machinery which was comprehensive and rigorous, authoritative yet uncontroversial, perhaps

even neutral in the required sense. That Hull never learned the system is probably the only reason that its application to his theoretical efforts waited until the 1940 publication of the Mathemático-Deductive Theory of Rote Learning. Even then, the application was achieved only by enlisting the assistance of the Yale logician Frederic B. Fitch.

By 1930, Hull was firmly convinced of the value of deductive methodology and was committed to its propagation wherever he could find a receptive audience. This central characteristic of his philosophy of science was thus arrived at well in advance of his contact with the philosophical movement of logical empiricism. The logical emphasis to which he resonated in that movement was clearly indigenous to his own thought and in particular to his mechanistic conception of organismic behavior.

The Changing Styles of Deductive Theorizing

In his detailed analysis of Hullian learning theory, Sigmund Koch has delineated three phases of Hull's theoretical activity.⁶⁹ The earliest phase, which covers the articles appearing from 1929 to 1935, is characterized by small sets of qualitative hypotheses designed to range over a restricted domain (e.g., simple trial-and-error learning) and informal derivations of their consequences. In the second phase, which includes the famous "miniature

systems" of 1935 and 1937⁷⁰, Hull's hypotheses were still essentially qualitative, but they were expressed in imitation of formal geometry in terms of definitions and axioms, and their consequences (called "theorems") were derived in a more explicit and detailed deductive manner. After the appearance of the rote learning theory in 1940 came the third phase, in which the overriding concern was quantification. Hull's publications in the period 1940-1952 showed a generally increasing interest in techniques for metricizing the variables of the system and relating them through equations. At the same time, they showed a decreasing interest in explicit formalization; the 1940 theory represented the peak in that regard.

The trend of these three stages in Hull's theorizing and the transitions between them are matters of considerable historical complexity. Without going into a detailed account, the present section will briefly sketch the historical circumstances surrounding these developments, especially the shift from the first phase to the second. Treatment of the final phase itself will be reserved for the following chapter.

The first phase, in which hypotheses were qualitative and deductions informal, reflected most directly Hull's interest in machine design. It was these restricted formulations that could actually serve as schemata for simple learning mechanisms, as Ellson was able to show.

Like the machine programs of cybernetics, Hull's early formulations were stated at a sufficiently abstract and functional level as to remain independent of the specific physical means and empirical parameters by which they might be realized. Had Hull remained true to his early mechanistic theoretical impulse, his proto-cybernetic animus, he might well have founded a branch of cognitive psychology strongly akin to the information processing approach that arose in the 1960s.⁷¹ But several factors operated to subdue and deflect this impulse. Two of these factors we have already touched upon: the threat of suppression from without and the unavailability in the 1930s of a highly appropriate means of expressing simulations. In the absence of modern programming languages, Hull was limited to a passing familiarity with geometrical method, and his enthusiasm for the method was never quite able to compensate for its meager utility in the early stages of psychological theorizing.

Hull apparently felt dissatisfied with his early papers in that each developed one or another mechanism of adaptive behavior, but there was no indication of how the various specific mechanisms might contribute to the overall functioning of the organism. In accordance with his early insights about hierarchical control, the solution to this problem was sought in the expressions of mechanisms as principles which could then be arranged in a hierarchy.

Each mechanism would exert its action from its place in the hierarchy, and the resulting interactions would determine the output of molar behavior. This approach to achieving theoretical integration and generality called for a more explicitly hierarchical form of expression to mirror the hierarchical structure of behavior. Hull's adoption of explicit geometrical method, which led to the second phase of theorizing, can thus be understood not only as part of his quest for objective method, but also as a highly appropriate strategy for the subject matter of psychology as it was construed by Hull. This conclusion suggests the need to reconsider those criticisms of Hull which claim that he imported an alien methodology ill-suited to the realm of psychology. He was, without question, influenced by the methods of mathematical and physical sciences, but given his particular conception of behaving organisms, such methods were far from alien.⁷²

Around 1934, Hull was propelled into the second phase of theorizing by two sorts of events. First, as was described above, a medical report on his failing health convinced him that he was nearing the end of his intellectual effectiveness. His gloomy estimate that perhaps two, but no more than five, years remained for him to contribute to the advancement of psychology added an acute sense of urgency to his already robust theoretical ambitions.

Second, Hull's ambitions began to be realized when his deductive approach started to gain favor among his fellow psychologists. This new favor came into evidence most notably at the 1934 annual meeting of the American Psychological Association. For example, Hull wrote that the discussion there

made it rather clearly evident that Tolman has never even considered making his work a theory in the sense that I use the term. This came out very clearly when he stated both publicly and to me privately that he would try to see what he could do in the way of deducing the results of his experiments by the rigorous processes which the Yale group aspires.⁷³

Having just returned from Vienna, Tolman was temporarily disposed to favor deductive methods, but as we have seen he shortly thereafter became skeptical of the value of such methods. What was significant in this episode was that Tolman lent to Hull's approach both public endorsement and private encouragement. Tolman's favorable reaction would have been all the more influential because of the acclaim generated by his recently published Purposive Behavior.

The favorable response was by no means limited to Tolman. Hull recorded that the "evident appeal" of his "rigorous approach" was widespread. During the meeting, Richard M. Elliott, editor of the prestigious Century Psychology Series, requested the rights to publish Hull's book on theory whenever it was ready. To Hull's delight,

Elliott referred to the book as Hull's "magnum opus." Another "influential factor," according to Hull, was the "evident enthusiasm" for his deductive approach shown by Edna Heidbreder, whose Seven Psychologies of 1933 had recently established her as an authority in systematic psychology.⁷⁴

In this sort of encouragement, Hull saw a possible solution to the personal crisis precipitated by his poor health. The most direct route to the goal of an integrated, comprehensive theory would be for Hull to focus on the basic structure of the theory rather than on experimental investigations. The selection of research problems would be subordinated to and determined by the elaboration of theory. This strategy seemed, in Hull's words, "extremely fortunate since as a result I shall be able to avoid the further scattering of my energies."⁷⁵ Hull's new priority was starkly revealed in a statement made just after the APA meeting: "I have definitely decided to do the work on theory first and leave the matter of the conditioned reflex to be treated incidentally, though possibly very effectively nevertheless."⁷⁶ The fact that theory was able to usurp from Hull's attention a phenomenon as central as the conditioned reflex may have appeared fortuitous to Hull at the time; but it was characteristic of Hull's growing tendency to let his speculations run injudiciously far ahead of their empirical base. With the benefit of hind-

sight, even psychologists within the Hullian tradition came to lament this tendency. As one put it: "Hull was evidently in a hurry. He wanted to try to get away with doing the 100 experiments in a given area instead of 100,000."⁷⁷

Hull presented his "miniature system" of rote learning at two other conferences in 1934, once in informal terms and once in the method of formal geometry.⁷⁸ He was surprised to find that the latter presentation was received much more favorably than the former. The impact of this observation on Hull was great, driving him further from close contact with his subject matter and with his original theoretical impulse to simulate mechanisms. He penned the following reaction to the difference between the responses to the two presentations:

. . . the apparent reason for this change is the use which I have recently made of the formal geometrical method in deriving my theorems. People apparently are impressed by the mere external appearance of rigor. This is a factor of considerable importance in the matter of propaganda. I shall certainly heed the evident moral in emphasizing this aspect when I write up the system as a whole.⁷⁹

As a part of his "bid for a place in the history of science," Hull had since 1930 been making waves as a proponent of rigorous methods in psychology. Now in 1934, he felt that the wave of psychological opinion had sufficiently coalesced to carry him on toward his ambitious goals. Expediently ignoring his aversion to propaganda in the hands of Watson, he was himself ready and willing to propagandize in order to

maintain his position at the crest of that wave. Hull's eminence was recognized officially by his colleagues when in 1936 he was elected president of the APA. In his widely cited presidential address,⁸⁰ Hull responded by presenting a second formalized miniature system, this time on adaptive behavior, but again in the geometrical style.

As early as 1936, the limitations of the geometrical method in psychology were coming to be acknowledged by Hull. In that year, he decided to seek Fitch's assistance in applying the symbolic logic of Principia Mathematica to the rote learning system.⁸¹ The reasons given by Hull for this change were that the geometrical method was limited in its versatility "for mediating quantitative deductions in the field of behavior," and that it had "proved to be clumsy in practice and limited in the logical rigor attainable."⁸² The decision to go more explicitly formal was reinforced by the opinion of the British biologist and logician J. H. Woodger, whom Hull met in 1937 and who had just completed an axiomatization of biological theory in PM notation. As we shall see in the next chapter, Woodger briefly worked with Hull on a formalized and expanded version of the 1935 miniature system, after which Fitch took over full responsibility for the symbolic translation. The resulting monograph of 1940 on rote learning theory was noteworthy both for its attempted logical rigor and for its use of equations. Although it seemed

no less "clumsy in practice" than earlier versions, it certainly gave the appearance of having overcome the other limitations of the geometrical method.

To serve his growing reliance on equations, Hull had hired mathematicians to work out the quantitative implications of the 1940 theory. With this step, the third phase of Hull's theorizing began in earnest. The emphasis had shifted from the mechanistic to the axiomatic and now to the mathematical. Impressed with the deductive power which equations brought to the system, Hull noted with due seriousness that "the change from qualitative to quantitative postulates with known constants increases the fertility of the postulate set between ten and fifty times."⁸³ At the same time, the new mathematical focus opened up two closely related methodological problem areas: intervening variables and the quantification of behavior. These topics, which comprised the bulk of Hull's later writings on methodological issues, will be treated in the following chapter.

This section has briefly reviewed the passage of Hull's deductive theorizing through its proto-cybernetic and axiomatic phases and up to its mathematical phase. These phases were by no means discontinuous; the transitions between them were gradual and incomplete. We find, for instance, polemical uses of deductive methodology in his early work and references to the correspondence between hierarchical theory and hierarchical subject matter in his later work. Nonetheless, we can discern a general

shift from deductive theory as mirroring psychological phenomena, as Hull saw them, to deductive theory as a propagandistic means of exhibiting formal rigor. In other words, there was a trend from indigenous method to methodolatry. This shift was driven in part by the peculiar combination of Hull's failing health and inflated ambitions, and in part by the rising clamor among psychologists for rigor and objectivity. Hull was only partly responsible for this clamor, and once caught up in it, he was as much its pawn as its perpetrator.

Deductive Method versus Metaphysics

Hull's emphasis on deduction naturally predisposed him to formulate views on the philosophy of science in close agreement with those of the logical empiricists. For example, even in 1930 he was asserting the symmetry of explanation and prediction, a claim which became a standard feature of the new positivist philosophy.⁸⁴ In a related vein, Hull appears to have adopted a version of the distinction between the contexts of discovery and justification. In 1935, he wrote:

The history of scientific practice so far shows that, in the main, the credentials of scientific postulates have consisted in what the postulates can do, rather than in some metaphysical quibble about where they came from. If a set of postulates is really bad it will sooner or later get its user into trouble with experimental results. . . . In a word, a complete laissez-faire policy should obtain in regard to postulates.⁸⁵

It is clear that Hull shared with the logical empiricists not only a core attitude toward deductive method, but also various ramifications of that attitude.

One of these ramifications, a particularly salient one in Hull's case, was the notion that deductive method could serve to combat metaphysics by keeping it distinct from science proper. Hull wrote in 1930 that:

if an hypothesis be so vague and indefinite, or so lacking in relevancy to the phenomena which it seeks to explain that the results neither of previous experiments nor those of experiments subsequently to be performed may be deduced from it, it will be difficult indeed to prove it false. . . . Unfortunately, because of its very sterility and barrenness in the above deductive sense, such an hypothesis should have no status in science. It savors more of metaphysics, religion, or theology.⁸⁶

In a way strongly reminiscent of Karl Popper, Hull thus linked unfalsifiability with metaphysics and non-science. This sort of contention was of course commonly advanced by scientific empiricists, but Hull's expression of it had its own emphasis. In contrast to many of the European positivists and American operationists who focused on observability per se, Hull emphasized the deductive capacity of a theory as the mark of its scientific status. Early in his career, Hull assumed the final link between deduced consequence and actual observation to be wholly unproblematic. The burden of establishing empirical contact was placed more on the system of deductively interrelated propositions than on its concepts and their empirical definitions.⁸⁷

Since deduced observable consequences were the sine qua non of scientific theory, there was for Hull no place in science for theory of any other kind. He conceded the possibility that, unlike physics, psychology was not amenable to scientific theorizing, but added, "if so we ought not to pretend to have theories at all." It was with this sort of attitude that Hull summarized his stance on theory in 1935:

If the deductions are lacking or logically invalid, there is no theory; if the deductions involve conditions of observations which are impossible of attainment, the theory is metaphysical rather than scientific; and if the deduced phenomenon is not observed when the conditions are fulfilled, the theory is false.⁸⁸

As close as Hull's views came to logical positivism on this point, there was in them no suggestion that metaphysics was strictly meaningless or nonsensical. In this respect, he was more like Popper, attempting to distinguish science from non-science rather than meaningfulness from meaninglessness. Not until after S. S. Stevens's groundbreaking papers of 1935 had triggered an interest in operationism and logical positivism did Hull's writings contain any mention of meaninglessness. This occurred in the 1937 published version of his APA presidential address, which also contained Hull's first reference to operationism. Only then did he invoke the trichotomy, familiar to European positivists, of truth, falsehood, and meaninglessness.⁸⁹ Even so, operational definition and meaninglessness were

each mentioned but once. They appeared to have been tossed in as an afterthought, as an offhand concession to a new brand of objectivism, and certainly not as an integral aspect of Hull's own views. The important respects in which Hull's deductivism stood in antithesis to an operational empiricism will be discussed in the following chapter.

In addition to its use in combatting metaphysics, deductive rigor was used by Hull as a weapon against forms of implicit knowledge such as anthropomorphism, at least as they might be used in science. Anthropomorphically generated predictions about behavior could have practical value according to Hull. He felt, however, that anthropomorphic accounts of behavior

are of no value as scientific theory because a truly scientific theory seeks to deduce what anthropomorphism reaches by intuition or by naive assumption. Prophecies as to the outcome of untried experiments based merely on such anthropomorphic intuitions should be credited to the intuitional genius of the prophet rather than to the theoretical system to which the prophet may adhere. Predictions, however successful, can have no evidential value as to the prophet's system until he is willing and able to exhibit the logic by which his predictions flow from the postulates of that system. . . .⁹⁰

Hull's idea books clearly reveal that the above comments were written in response to Tolman's Purposive Behavior.⁹¹

In fact, Tolman's refusal to formally systematize his ideas proved to be a long-term source of frustration for Hull.

If only psychology could adhere to the ideal of

deductive theorizing, metaphysical and other subjective influences could be kept outside its scientific sphere. The way would thus be cleared for the progress which is expected--or even inevitable--in genuine science. The assurance of progress which was integral to Hull's conception of scientific psychology is the subject of the following section.

Method as a Guarantor of Positive Progress

Method and Advance

When at the end of the 1920s Hull became convinced that psychology could attain the status of a true natural science, his optimism rested largely on the notion that appropriate methods would bring to psychology the sort of advance that appeared to be characteristic of physics. Hull surveyed the psychological scene at the turn of the decade and was appalled by the diversity of seemingly incompatible systems. Of the dozen or so systems described in Carl Murchison's Psychologies of 1930, Hull wrote:

To put the matter in an extreme form: if all of these twelve psychologies should be in specific disagreement on a specific point, then at least eleven of them must be wrong, and in such a welter of error the twelfth may very well be wrong also. . . .⁹²

The rectification of this situation, as anticipated in his talk at Columbia in 1929, called for the derivation of consequences from each system and the tallying of their

observed adequacy. In this way the logjam might be broken and the natural flow of progress set in motion.

With the zeal of a crusader, Hull promulgated his notion of progress through method during the thirties. The classic statement of this line is found in his 1935 "The Conflicting Psychologies of Learning--A Way Out." Therein he invoked the familiar distinction between fact and theory. Whereas the logical empiricists had drawn this distinction in terms of observation and theoretical language, Hull simply asserted that, in actual scientific practice, disagreement is almost entirely confined to theory. Evidence itself is neutral, objective, and unproblematic; all that is needed is a sure way to bring theories under the "impartial arbitration of the facts."⁹³ "If the theories of a science really agree with the experimental evidence," Hull wrote, "and if there is general agreement as to this evidence, there should be a corresponding agreement regarding theory."⁹⁴

Consensus on fact would force consensus on theory provided that there was a determinate and effective way to relate the two. The way, of course, was through deductive logic. As we have seen, Hull was convinced of logic's objectivity, its epistemological neutrality. He had known since his undergraduate days that it could be performed by a machine, and surely machines could show no bias or emotionalism. Although he never literally used machines

to derive theorems from his postulates, Hull did occasionally employ such machines as conceptual devices in advancing his methodological arguments.⁹⁵ Viewing both fact and logic as theoretically neutral meant that the crucial steps in the deduce-and-observe method were objective in a quite unproblematic sense. In light of the power of arbitration inherent in these two steps, the initial step of arriving at postulates could be downplayed, submitted to a policy of laissez-faire, or even presumably left to chance.

Systems of psychology which failed to pass muster under the deduce-and-test regimen would fall by the wayside. Those which did reasonably well could undergo alterations and adjustments followed by further tests. Through a gradual trial-and-error process, a system in the hands of responsible theorists would progress by successive approximations toward an ideal of perfection. If the proponents of a theory were willing to revise it each time it faced "a collision with a stubborn experimental fact," the proportion of erroneous deductions from it would inevitably decrease.⁹⁶ Depending on its relationship to other surviving systems, a theory had to meet one of two fates. If it attempted explanations at a level different from other systems (e.g., perceptual versus neurophysiological), it would hierarchically subsume the others or be subsumed by them. If the competing systems operated on the same level, they would gradually converge toward agreement,

differing finally, if at all, only in the vocabularies in which they were expressed. The certain result of the consistent application of objective method was thus convergence toward agreement, either through subsumption or identity.⁹⁷

The sort of guaranteed progress which Hull envisioned could be objectively monitored and recorded. To record this progress, Hull devised a kind of a scoresheet which was as objective as the methods for producing the progress. These so-called "validity tables" appeared frequently in his unpublished writings but rarely surfaced in print.⁹⁸ They typically consisted of a column listing the theorems and corollaries of the system under scrutiny. In a second column, a plus or minus sign indicated the availability or absence of empirical evidence for each proposition. Whenever there was a plus in the second column, a third column held a plus or a minus signifying empirical validity or invalidity. The validity tables provided at a glance information on which experiments needed to be performed and which postulates needed revision. They could easily provide summary statistics of a system's current status, and did so in a way that could conveniently be reported to colleagues, granting agencies, and the like. Progress became an observable matter of converting minuses and blank spaces to plusses.

All told, the Hullian version of scientific method was a highly mechanized procedure. The derivation of consequences, their empirical check, the recording of their adequacy, and even the resulting progress were conceived of as automatic as if science were a great calculating machine. These aspects of Hullian methodology have led one observer to view neobehaviorism as "the only systematic, extensive, and detailed attempt ever made to fulfill Leibniz's dream of a universal calculus."⁹⁹ Indeed, logical methods were something like a universal calculus for Hull. Although he specifically viewed them as the salvation of psychology (or any other scientific effort), the significance he attached to them spread far beyond the confines of academic psychology. The tenets of scientific method, he believed, would enhance social progress by establishing the basis of an effective moral education and generally provide a means for the conversion of long-standing philosophical issues into tractable scientific problems.

It is by now widely agreed that Hullian methodology rather seriously failed to live up to its promise. The depth of its misconception of the scientific enterprise has become especially clear in light of recent developments in the history and philosophy of science. From the strictly historical standpoint, it is pertinent to note that cogent objections to the Hullian view were quickly raised after its

pronouncement. They came from within the psychological community, were voiced in prominent channels, and often concerned its most basic features, namely the ease of consensus on observational evidence and the unproblematic deductive contact between fact and theory.

As regards the former feature, for instance, Tolman insisted in his presidential address of 1937 that not even the simplest of conditioning phenomena were capable of unequivocal interpretation.¹⁰⁰ With his background in neorealism, Tolman would naturally have been aware of the tenuousness, in principle, of any distinction between pure observation and inference; with his knowledge of the literature on learning in the 1930s, he was certainly aware of the practical difficulties involved in trying to forge any consensus on the "facts" of learning. As regards the deductive contact between theory and observation, D. K. Adams was quick to point out in the Psychological Review that even when a prediction could be derived via a chain of deductions, its confirmation was incapable of being conveyed back up the chain to the theory.¹⁰¹ Hull's determinate linkage was unidirectional; to speak of a fulfilled prediction confirming a theory was to commit the fallacy of affirming the consequent.

Hull believed so strongly in the essential soundness of his methods that he never even bothered to give systematic replies to critics of his methodological strictures. The

replies he did give were usually relegated to footnotes and asides.¹⁰² Ever conscious of the imminent failure of his health, he devoted little time to what were for him unimportant details of a promising methodology. The preferred course was to proceed full tilt with the development of his system and let the results speak for the method. This strategy left one problem: the progress and convergence of theories promised by his methods would be reached only by those who subscribed to the methods in the first place. The proof would be in the pudding, but not everyone agreed to follow Hull's recipe. Those who would not were regarded by Hull as failing to be scientific, and were thus legitimate targets for persuasion and propaganda.

Objectivity versus Emotionalism

Hull was eager to draw a sharp line between the objectivity of scientific procedure and the subjectivity common to metaphysical disputes. Commenting on the conflict between systems of psychology in the early 1930s, he wrote:

This emotionalism and this inability to progress materially toward agreement obviously do not square with the ideals of objectivity and certainty which we associate with scientific investigation; they are, on the other hand, more than a little characteristic of metaphysical and theological controversy.

The situation in physics was, of course, quite different thanks to the adherence of physicists to the deductive method.

Had Newton's system not been firmly anchored to observable facts, its overthrow would not have been possible and we would presumably be having at the present time emotionally warring camps of Newtonians and Einsteinians. Fortunately, we are spared this spectacle.¹⁰³

Emotionalism was for Hull just an unpleasant reminder that unexorcized metaphysical influences were still preventing the acceptance of scientific method.

Given the mechanical character of the deduce-and-observe procedure, Hull believed there was never any need for emotional dispute in science. Matters of logic and fact, being possessed of objective naturalness, remained all that was worthy of scientific attention. The tenacious maintenance of one's objectivity became a test of one's scientific character. The extremes to which Hull pursued these beliefs are revealed in the account he sent to Spence of an encounter in 1941 with the Gestalt psychologist Wolfgang Köhler. Hull wrote:

. . . appropos of the general argument I was putting forward to the effect that scientific matters should be settled on a scientific and logical basis rather than by some kind of warfare, he came out with this remark: he said that he was willing to discuss most things in a logical and scientific manner, but when people try to make man out to be a kind of slot machine, then he would fight! And when he said the word "fight," he brought his fist down on the table with a resounding smack, and he did not smile when he said it either. . . . I pointed out to him that even though a person did feel like fighting about such a matter, the fighting wouldn't settle it and was really futile so far as the scientific status of the thing was concerned. At that point he began telling me about the trouble he had had with the Nazis in Germany and commented on how stupid the

English had been not to prepare for war while the Germans were preparing for war, and so on. This seemed, and still seems to me to be utterly irrelevant to the logical question involved, though as a psychological proposition I can understand how a man's scientific wires might get crossed through emotional upsets in his personal life. Actually one would hardly expect a thoroughgoing scientist to do a thing like that, but Köhler clearly did it on that occasion. Upon the whole, it was not a very impressive demonstration of either scientific or philosophical poise.¹⁰⁴

Hull laid much of the blame for human misery and conflict on the "prevalent subjectivity" of the world.¹⁰⁵ Psychologists could not begin to solve those problems until their own subjectivity was cured. The Gestaltists' refusal to state their postulates, to submit their views to the machinery of science, was greeted by Hull with dismay and scorn, for it indicated an unscientific attitude. The well-known feuds between the Gestaltists and the behaviorists posed a special problem for the Hullian hope of progress through method. With a touch of sad resignation, Hull wrote in Principles of Behavior that

optimism in this connection is seriously dampened by the conviction that the differences involved arise largely from a conflict of cultures . . . which, unfortunately, are extra-scientific and are not ordinarily resolvable by either logical or empirical procedures.¹⁰⁶

Naturally, there was still no need for dispute in science; to engage in science was to subscribe to Hull's notion of method. Those who disputed were simply not in the arena of science as it was defined by Hull. Within the confines of science, Hull tried to respond to challenges in the

usual manner of contriving elaborate derivations of phenomena. In extra-scientific matters, he could not-- and did not--avoid explicit polemic and propaganda. Hull's claim to have had "an intense aversion to controversies, seems odd in light of the vigor and relish with which he engaged in controversy;¹⁰⁷ but his claim amounts to little more than another expression of regret that his particular view never achieved unanimous support.

Integration of Science

In Hull's vision of science, the absence of metaphysical dispute and emotionalism led naturally to the integration of science. With a single method accepted for all branches of science, the various laws of those branches would eventually take their place in a deductive hierarchy of knowledge. A single method of obviating dispute also removed any barrier to social cooperation among scientists and even provided the rationale for a division of scientific labor. In what follows, the integration of science at the levels of method, law, and social cooperation is briefly discussed.

Unity of Method

One of the most salient and widely known features of Hull's philosophy of science is the conviction that the methods appropriate for the physical sciences are equally appropriate for psychology. As has often been pointed out, the notion that psychology might be united with the older and more advanced sciences by a common method was one which held great appeal for psychologists in the 1930s. Faced with a bewildering proliferation of theoretical systems on one hand and the practical demands of a society in turmoil on the other, psychologists no doubt longed to share the prestige of the "hard" sciences. Among them, Hull was the most audacious advocate of the unity of method.

Not surprisingly, Hull turned early and often to physics as a model. In 1930, he wrote that in developing his system "the method will be the same as that which has proven so successful with the mathematical physicists."¹⁰⁸ Hull's paper of that year on trial-and-error learning began with a reference to Einstein's gedanken experiments,¹⁰⁹ which were construed by Hull as deductive processes like those he was himself employing. Hull also occasionally invoked quantum mechanics, especially when he wished to highlight the tentative nature of his own theoretical ventures (see Chapter 8 below). But by far the scientific theory most emulated by Hull was Newtonian mechanics.

Hull's discovery of Newton's Principia around 1930 had a strong impact on him. Here was a powerful scientific theory expressed in the mode of the Euclidean geometry he so admired. With its eight explicit definitions and three postulates, the theory showed a degree of formal rigor and implied a hypothetico-deductive method for its assessment. Principles were deductively related to phenomena in a way that compelled the theory's abandonment when confronted with its Einsteinian successor.

The Principia became a kind of a bible for Hull. In the 1930s he began the practice of assigning portions of it to be studied by members of his seminar. "Several hours," he admonished, "devoted to a perusal of this great work would scarcely be wasted, even by a social scientist." The purpose of reading Newton was "not to understand the details of the mathematics but to observe his procedure."¹¹⁰ The importance of the Newtonian model in Hull's advocacy of postulational technique would be difficult to overestimate. One visitor to Hull's lab in the late thirties characterized the scene as follows: "On his large table Newton's Principia was placed demonstratively between himself and any visitors. No nonsense: admit your postulates!"¹¹¹ Hull's exhortation to psychologists was often repeated in his published works as well. As Hull was emboldened by the favorable reaction to his views, his statements of the unity of method gradually sounded less

like recommendations for scientific praxis and more like logical imperatives. In his presidential address to the APA, he stated, "Surely the same logic which demands strict deduction from explicitly stated postulates in physical theory demands it for the theory of adaptive behavior."¹¹²

Integration of Laws

As has already been described, Hull believed that the consistent application of rigorous methods would result in a system of deductively interlocked scientific laws. The search for basic behavioral laws from which subsidiary laws and phenomena could be derived was an early concern for Hull. Even in the late twenties, he was attempting to integrate the facts of Pavlovian and instrumental conditioning under a single rubric.¹¹³ In 1931, a student suggested to Hull that the remote excitatory tendencies of Ebbinghaus might be governed by the same principles as the trace conditioned reflexes of Pavlov.¹¹⁴ This suggestion, later known as Lepley's hypothesis, was the basis of Hull's subsequent rote-learning system, in which he tried to assimilate verbal learning to Pavlovian conditioning. As a third example of theoretical integration, Hull speculated on the relations between learning theory and psychoanalytic theory, suggesting for instance that Freudian sublimation might be a form of response alternation.¹¹⁵

When in the mid-thirties the trappings of postulational technique began to figure prominently in Hull's writings, the issue of integration was phrased in terms of postulate systems. He wrote in 1935 that "each lower level should be able to deduce the relevant basic postulates of the system above it in the hierarchy of systems."¹¹⁶ Hull's view of the hierarchy of the sciences was elaborated in his APA presidential address.

According to this view the theoretical physicists will ultimately deduce as theorems from electrons, protons, etc., the six postulates which we have employed as the basis for the deduction of adaptive behavior. If this deduction were accomplished we should have an unbroken logical chain extending from the primitive electron all the way up to complex purposive behavior. Further developments may conceivably extend the system to include the highest rational and moral behavior. Such is the natural goal of science. This is the picture which a complete scientific monism would present. Unfortunately, theoretical physics is very far from this achievement, and judgment regarding its ultimate accomplishment must be indefinitely suspended. At most such a view, attractive as it is, can be regarded only as a working hypothesis.¹¹⁷

Like the logical positivists' second thesis of physicalism, Hull's view placed physics at the foundation of a hierarchy of deductive explanation. The attainability of such an integration was admitted to be an open question, but it was a hypothesis in which great faith was invested.

As in matters of methodology, Hull looked to physics for a model of how to achieve integration of laws. To this end, he brought the Yale physicist Leigh Page into his seminar in 1935 to lecture on theoretical integration in

physics. Page reviewed the development of physics with emphasis on the subsumption of earlier laws (e.g., Kepler's) under later more general laws (e.g., Newton's). "It was an impressive story," wrote one psychologist in attendance, "one we wanted to emulate in our own field."¹¹⁸ Page was apparently as sanguine as Hull about the possibilities for future integration. His diagram of the branches of physical theory showed, arching over relativity theory and quantum mechanics, an as yet undiscovered theory representing the "assumed point of contact between the two."¹¹⁹

Faith in the hierarchical coordination of theories took on more and more importance through the late thirties as its implications for the strategy of research became clear. The following section describes how Hull and his co-workers responded to a push for integration at the Institute of Human Relations by adopting integrated theory as the basis for a coordinated attack on the problems of the social sciences.

Scientific Cooperation

The Institute of Human Relations was founded in 1929 with a ten-year grant from the Rockefeller Foundation. Several million dollars of support was granted on the premise that the Institute would develop a coherent research program for the unification of the social and biological sciences. The original plan of attack, based

on the model of medical research, was to bring together experts from different areas to work on specific problems, just as chemists and physicians might work together on curing a certain disease. The various experts who were actually enlisted in the IHR, however, tended to carry on their own research in isolation from one another, and by the mid-thirties the Rockefeller Foundation's agents were conveying their dissatisfaction to officials of the IHR. The prospects for renewal of funding were dim unless demonstrable scientific integrations could be effected.¹²⁰

In response to this crisis, the IHR took several steps. The psychologist Mark May took over as director of the Institute, a group of younger researchers with broader training was recruited, and Clark Hull in 1936 devoted his Wednesday evening seminar to the development of a concerted research program. These were auspicious ingredients for a reorganization. Unlike his predecessors, May was willing to exert pressure on the Institute's various researchers to collaborate. Hull was a natural to assume leadership in the efforts at integration given his views on science and its methods. The cast of talented participants in Hull's seminar included John Dollard, Neal Miller, Robert Sears, Leonard Doob, Donald Marquis, O. Hobart Mowrer, F. S. C. Northrop, and Warren S. McCulloch. Hull evidently had little difficulty advancing his views on integration in the seminar. He recorded in 1936 that "all seem to

be wholly sold on the desirability of having a clearly explicit theory (in the strict deductive sense) as the basis for the systematic attack on the problems of any Institute program."¹²¹

The move toward integration would begin at the simplest level of cooperation. Each member of a research group might be assigned one value of an independent variable so that each would contribute a point in the overall behavioral function.¹²² Haphazard investigations were to be replaced by systematic studies in the hope that results would add up to a new principle rather than a mere collection of facts. Postulated relationships would guide the selection of research problems, and experiments would lead to revisions and discoveries of postulates. Efforts at systematizing within disciplines would be followed up by integrations across disciplines.

The assumed hierarchy of the sciences provided the basic structure of the enterprise. In his seminar notes, Hull listed "in order of logical development" the various problem areas to be addressed. The list began with the physiology of motivation and passed through conditioned reflexes and psychoanalytic phenomena, leading eventually to the complex social institutions of law, economics, and religion.¹²³ This hierarchy made possible a division of labor by subject matter. Hull's group would cover the range of topics from reflexes to psychoanalytic

phenomena, leaving the rest to Dollard's group. Hull himself would attempt to derive certain higher-level postulates from the more basic ones. At each level, postulates would be isolated and then deduced from those of a more basic level. In this way, wrote Hull,

the looseness will pull from the top and we shall secure a very tight integration far up and down the hierarchy of the social sciences. Note: This ought to be a very valuable formula for the coordination of effort of the different groups.¹²⁴

After an initial division of labor was made according to subject matter, efforts could be further divided according to one's role in the deduce-and-test method of science. Thus, Hull employed mathematicians and a logician to help with the deductions and psychologists to run the empirical tests. Mark May later described the procedure used in developing the rote learning theory as

quite analogous to that used in the planning and construction of a building. The senior author [Hull] has acted as the planning and supervising architect while his collaborators have checked the plans and supplied much of the technical skill necessary for their execution. The work has been carefully coordinated throughout with the result that the end product is a unified whole.¹²⁵

May and Hull worked closely together to implement their vision of integrated research at the IHR. Revealed as a division of labor, their notion of scientific cooperation derived from and reflected not only the hierarchical structure of scientific theory itself, but also the method of testing it. If theories were a type of mechanism in

Hull's view, so too were the social groups which devised and tested them. Each scientist was like a gear in the machine of science, the output of which was the conceptual machine known as scientific theory. Needless to say, Hull's status in the hierarchy of research suited his ambitions well. Driven by fears for his health, Hull's rush toward a comprehensive theory of psychology left no time for personal involvement in experimentation. The best hope of fulfilling his ambitions lay in his supervision of the most direct possible assault on the integration of psychological theory.¹²⁶

Conclusion

Like many of the logical empiricists, Clark Hull was a philosophizing scientist. He shared with them a scientific attitude which colored all his thinking. Given their shared intellectual background--especially in British empiricism--and their mutual interest in logic, it is not surprising that their respective philosophies of science developed along similar lines. This chapter has described Hull's views in terms of four themes which, to varying degrees, he held in common with the European scientific philosophers.

Like the physicalism of the Vienna Circle, Hull's materialism stood in sharp opposition to the idealism and vitalism which were considered antithetical to the

"scientific attitude." In its anti-idealist aspect, Hullian materialism was most like the version of physicalism espoused by Otto Neurath. Being a genuine metaphysical commitment, it was decidedly unlike Carnap's ametaphysical brand of physicalism. Hull was simply not a positivist in the Vienna sense of the term. The logical empiricists were sympathetic to mechanistic conceptions of organic phenomena, but none developed a mechanistic philosophy to any degree.¹²⁷ Hull's mechanism, on the other hand, was centrally related both to his materialistic philosophy and to his research program.

A very close intellectual convergence between Hull and the logical empiricists can be seen in their views on scientific theory and methodology. The similarity of these views stems from their shared emphasis on the logical structure of theories and extends to the associated doctrines of the distinction between discovery and justification, of the hypothetico-deductive method, and of the denial of scientific status to implicit knowledge. Despite these close parallels, however, there were important differences between Hull and the logical positivists in their views of logic and scientific theory. The following chapters will treat these divergences.

Just as the logical empiricists viewed the elimination to metaphysics from philosophy as the key to philosophical progress, Hull viewed its elimination from science as the

key to scientific progress. They alike took physics as a model of scientific progress and saw unwarranted emotionalism as a serious hindrance to the extension of a physics-based philosophy of science to the social sciences. They further shared the Comtean belief that the cultivation of the "scientific attitude" was a basic ingredient of social progress.

Finally, Hull and the logical positivists held in common a series of closely related views on the unification of science. They believed first that a single method could be successfully applied in all the sciences, indeed that success in science was contingent on a unity of method. They believed secondly in the eventual unification of various disciplines through the arrangement of their laws in hierarchies of deductive explanation. The third shared view was that the unity of science should be manifested at the level of cooperation among groups of scientists. To the extent that their respective views on this matter were explicitly formulated, they coincided in remarkable detail. It was this congruence of beliefs about scientific cooperation, more than any other aspect of their shared views, that led to the actual interaction of Hull with the logical positivists.

Like logical positivism, the Hullian philosophy of science represented a culmination of strong tendencies in Western thought. They both evolved out of and blended

together strains of British associationism and scientific monism, but their respective developments were parallel and independent. Hull and the logical empiricists similarly added to these traditions a conspicuous emphasis on logical methods. Without the actual logical expertise of the logical positivists, Hull remained perhaps closer philosophically to the German scientific monism of the nineteenth century, particularly in his melding of materialism, mechanism, and Comtean positivism. But, in itself, this was a relatively minor difference of focus. Especially after the broadening of the early logical positivism into the Unity of Science movement, the Hullian and logical empiricist views of science showed a clear and mutually recognizable kinship.

Notes for Chapter 6

1. Richard W. Coan and Salvatore V. Zagona, "Contemporary Ratings of Psychological Theorists," Psychological Record 12 (1962): 315-322. See also R. B. Ammons, "Psychology of the Scientist: II. Clark L. Hull and his 'Idea Books'," Perceptual and Motor Skills 15 (1962): 800-802.
2. Clark L. Hull, "Psychology of the Scientist: IV. Passages from the 'Idea Books' of Clark L. Hull," Perceptual and Motor Skills 15 (1962): 807-882, on p. 826. This article contains portions of Hull's Idea Books selected and edited by his secretary Ruth Hays. These passages represent only a very small proportion of the total corpus. In what follows, I have given references to these published excerpts whenever possible, rather than to the original unpublished sources.
3. Sigmund Koch, "Psychology and Emerging Conceptions of Knowledge as Unitary," in Behaviorism and Phenomenology: Contrasting Bases for Modern Psychology, ed. T. W. Wann (Chicago: University of Chicago Press, 1964), pp. 1-41, on p. 11.
4. Clark L. Hull, "Clark L. Hull," in A History of Psychology in Autobiography, vol. 4, ed. Edwin G. Boring, Herbert S. Langfeld, Heinz Werner, and Robert M. Yerkes (Worcester, Mass.: Clark University Press, 1952), pp. 143-162, on p. 144. Much of the present section is based on this autobiographical sketch.
5. In 1937, Hull recorded his intention to write an article giving "the derivation of the principles of morals from the conditioned reflex" ("Psychology of the Scientist," p. 863). The closest he ever came to fulfilling this intention was his "Value, Valuation, and Natural-Science Methodology," Philosophy of Science 11 (1943): 125-141.
6. Hull, "Clark L. Hull," p. 145.
7. Hull, "Psychology of the Scientist," p. 814.
8. Clark L. Hull, "Quantitative Aspects of the Evolution of Concepts: An Experimental Study," Psychological Monographs 28 (1920): 85 pp.

9. Clark L. Hull, Aptitude Testing (Yonkers, N.Y.: World Book, 1928) and Hypnosis and Suggestibility, An Experimental Approach (New York: Appleton-Century, 1933).

10. Rodney G. Triplet, "The Relationship of Clark L. Hull's Hypnosis Research to His Later Learning Theory: The Continuity of His Life's Work," Journal of the History of the Behavioral Sciences 18 (1982): 22-31.

11. Clark L. Hull, "Goal Attraction and Directing Ideas Conceived as Habit Phenomena," Psychological Review 38 (1931): 487-506, on p. 502.

12. J. A. Gengerelli, "Graduate School Reminiscences: Hull and Koffka," American Psychologist (1976): 685-688, on p. 685.

13. Clark L. Hull, "Behaviorism: Seminar Notes, 1925," Clark L. Hull Papers, Sterling Memorial Library, Yale University, New Haven, Conn., pp. 1-6. On the claim that consciousness requires explanation, see also Clark L. Hull, "Mind, Mechanism, and Adaptive Behavior," Psychological Review 44 (1937): 1-32. Hull's texts for the seminar were John B. Watson, Psychology from the Standpoint of a Behaviorist, 2nd ed. (Philadelphia: Lippincott, 1924) and A. A. Roback, Behaviorism and Psychology (Cambridge: Mass.: Sci-Art, 1923).

14. Hull, "Psychology of the Scientist," p. 834.

15. Ibid., pp. 835, 823.

16. Hull, "Clark L. Hull," p. 154.

17. Gengerelli, "Hull and Koffka," p. 686. In discussing consciousness and will in his idea book of 1926-27, Hull remarked: "The Watsonian tradition would deny the existence of any such things and thereby dismiss the problems as non-existent. This is as vicious as to be content with a false solution--both inhibit further investigation" ("Idea Book IX," Clark L. Hull Papers, pp. 185-194). Hull also said that behaviorism "has Watson's negative tradition to live down" ("Psychology of the Scientist," p. 845).

18. Hull, "Idea Book XIII," Clark L. Hull Papers, pp. 94-98. Hull's unpublished idea books will hereafter be referred to only by title, volume number and pages.

19. Ivan P. Pavlov, Conditioned Reflexes, trans. G. V. Anrep (Oxford: Oxford University Press, 1927).
20. Hull, "Clark L. Hull," p. 152.
21. Triplet, "Hull's Hypnosis Research and Learning Theory," p. 25; Hull, "Psychology of the Scientist," pp. 828-829.
22. Clark L. Hull, "A Functional Interpretation of the Conditioned Reflex," Psychological Review 36 (1929): 498-511.
23. Hull referred to his work on conditioned habit as "the very apple of my eye and the thing which I hope my main scientific reputation will rest upon" (Psychology of the Scientist," p. 850).
24. Hull, "Psychology of the Scientist," p. 855.
25. Ibid., pp. 852, 855. In his biographical sketch of Hull, Frank A. Beach flatly stated that "Hull never conducted experiments himself," but this claim is probably correct only for the later part of Hull's career. See "Clark Leonard Hull," Biographical Memoirs (National Academy of Sciences), vol. 33 (New York: Columbia University Press, 1959), pp. 125-141, on p. 133.
26. Hull, "Idea Book X," p. 206.
27. Clark L. Hull, "Psychology Seminar Memoranda, 1940-1944," Yale University Library, 1952 (mimeographed), p. 1.
28. Hull, "Idea Book IX," p. 206.
29. Clark L. Hull, "Simple Trial-and-Error Learning: A Study in Psychological Theory," Psychological Review 37 (1930): 241-256, on p. 256.
30. Hull, "Psychology of the Scientist," p. 849.
31. Ibid., p. 835; Hull, "Goal Attraction and Directing Ideas," pp. 502-503.
32. Beach, "Clark Leonard Hull," pp. 128-129; Gengerelli, "Hull and Koffka," pp. 686-687.

33. Aruthur S. Eddington, The Nature of the Physical World (New York: Macmillan, 1928); James H. Jeans, The Mysterious Universe (New York: Macmillan, 1930).
34. Clark L. Hull to Kenneth W. Spence, 11 April 1939, Spence Papers, Archives of the History of American Psychology, Akron, Ohio.
35. Hull, "Psychology of the Scientist," p. 846.
36. See, for example, Hull's "Knowledge and Purpose as Habit Mechanisms," Psychological Review 37 (1930): 511-525 and "The Mechanism of the Assembly of Behavior Segments in Novel Combinations Suitable for Problem Solution," Psychological Review 42 (1935): 219-245.
37. Sigmund Koch, "Clark L. Hull," in Modern Learning Theory, by William K. Estes, Sigmund Koch, Kenneth MacCorquodale, Paul E. Meehl, Conrad G. Mueller, Jr., William N. Schoenfeld, and William S. Verplanck (New York: Appleton-Century-Crofts, 1954), pp. 1-176, on p. 163.
38. Hull, "Psychology of the Scientist," p. 837.
39. Hull, "Memoranda, 1940-1944," p. 2; Principles of Behavior: An Introduction to Behavior Theory (New York: D. Appleton-Century, 1943), pp. 26-27, 29; "The Concept of the Habit-Family Hierarchy and Maze Learning," Psychological Review 41 (1934): pp. 33-52 and 134-152, on p. 39.
40. Hull, "Psychology of the Scientist," p. 841.
41. *Ibid.*, pp. 829, 837, 838. In referring to an "epidemic" of psychical machines, Hull was engaging in hyperbole, although there were a handful of such machines being developed around this time. Published reports of two of these may be found in J. M. Stephens, "A Mechanical Explanation of the Law of Effect," American Journal of Psychology 41 (1929): 422-431 and Albert Walton, "Conditioning Illustrated by an Automatic Mechanical Device," American Journal of Psychology 42 (1930): 110-111.
42. Hull, "Trial-and-Error Learning," p. 256.
43. Hull, "Clark L. Hull," p. 146. Sellars may have been the source of some of Hull's philosophical views, e.g., his emergent materialism. See Roy Wood Sellars, Evolutionary Naturalism (Chicago: Open Court, 1922). For an informed conjecture on the type of design employed by Hull in his logic machine, see Martin Gardner, Logic Machines, Diagrams and Boolean Algebra (New York: Dover, 1958), p. 124.

44. Hull, "Psychology of the Scientist," pp. 828, 820.

45. The Hullian impulse to design psychological mechanisms is today carried out in the language of computer programming; hierarchical control is expressed in sub-routines, iterative loops and the like. On the founding of cybernetics in the 1940s, see Herman H. Goldstine, The Computer from Pascal to von Neumann (Princeton, N.J.: Princeton University Press, 1972) and The Encyclopedia of Philosophy, s.v. "Cybernetics," by Keith Gunderson.

46. Clark L. Hull and H. D. Baernstein, "A Mechanical Parallel to the Conditioned Reflex," Science 70 (1929): 14-15.

47. Hull, "Psychology of the Scientist," p. 833.

48. Hull, "Trial-and-Error Learning," p. 256.

49. Hull, "Mechanism of Assembly," p. 231.

50. Douglas G. Ellson, "A Mechanical Synthesis of Trial-and-Error Learning," Journal of General Psychology 13 (1935): 212-218.

51. Hull, "Memoranda, 1940-1944," p. 2.

52. Ellson, "Mechanical Synthesis," p. 216.

53. Clark L. Hull and Robert G. Krueger, "An Electro-Chemical Parallel to the Conditioned Reflex," Journal of General Psychology 5 (1931): 262-269, on p. 267. Another of Hull's simulational mechanisms is reported in H. D. Baernstein and Clark L. Hull, "A Mechanical Model of the Conditioned Reflex," Journal of General Psychology 5 (1931): 99-106.

54. Hull, Principles of Behavior, pp. 27-28.

55. Hull, "Psychology of the Scientist," p. 825.

56. Hull, "Idea Book XI," pp. 177-182.

57. Hull, "Psychology of the Scientist," pp. 828-829.

58. Hull, "Idea Book XI," p. 181. Hull was apparently successful in conveying this impression in public. One observer who witnessed Hull's demonstration of a conditioning machine during his presidential address to the American Psychological Association has commented that "it is my contention that Hull--an engineer turned psychologist--conceived and carried out the construction of this model primarily for the fun of it." See Alphonse Chapanis, "Men, Machines, and Models," in Theories in Contemporary Psychology, ed. Melvin Marx (New York: Macmillan, 1963), pp. 104-129, on p. 119. Chapanis also remarked that the machine had an "electric effect" on the audience at the APA and expressed his own regret that the machine was not even mentioned in the published version of the address.

59. Hull, "Psychology of the Scientist," p. 852.

60. Robert S. Woodworth, Contemporary Schools of Psychology, 8th ed. (London: Methuen, 1951), p. 109.

61. Hull, "Psychology of the Scientist," p. 815.

62. Hull, "Idea Book I," p. 104.

63. Beach, "Clark Leonard Hull," p. 129.

64. Hull, "Idea Book IX," p. 133.

65. Hull, "Idea Book X," p. 86.

66. Hull, "Idea Book XI," pp. 75-78.

67. Beach, "Clark Leonard Hull," pp. 128-129.

68. Bertrand Russell and Alfred North Whitehead, Principia Mathematica, 2nd ed., vol. 1 (Cambridge: Cambridge University Press, 1927), p. v.

69. Koch, "Clark L. Hull," pp. 12-13.

70. These systems were presented in Clark L. Hull, "The Conflicting Psychologies of Learning--A Way Out," Psychological Review 42 (1935): 491-516 and Hull, "Mind, Mechanism, and Adaptive Behavior."

71. It has been argued that contemporary cognitive psychology rests on assumptions that are entirely continuous with those of behaviorism. See Thomas H. Leahey, "The Revolution Never Happened--Information Processing Is Behaviorism," paper presented at the Eastern Psychological Association, New York, 23 April 1981.

72. The objection might reasonably be raised here that Hull's mechanistic conception of organismic behavior was unduly influenced by the physical sciences. But to say that his methods were inappropriate to his metaphysical conception of psychology appears to be incorrect, and to say that his metaphysical conception was inappropriate is to make a claim which is in itself metaphysical. The subject matter of psychology, like its methods, can only be articulated within the confines of a set of metaphysical commitments. Neither the domain nor the methodology of a science is a given (although both are, of course, historically conditioned at any given time). Metaphysical views can be challenged only from another metaphysical standpoint, and such a challenge can rarely, if ever, be expected to provide a direct confrontation (see Chapter 10 below).

73. Hull, "Psychology of the Scientist," p. 856.

74. Ibid., pp. 856-857. The Heidbreder work referred to is Edna Heidbreder, Seven Psychologies (New York: Century, 1933). Heidbreder has recalled her reactions to Hull's APA talk:

. . . I distinctly remember Hull's presentation of his 'miniature system' on rote learning at the APA meeting in New York in 1934. My enthusiasm for his talk was less for the particular system he presented than for the kind of system he was advocating--one in which a coherent, logically rigorous system could, by way of hypotheses deduced from it, be put to empirical, experimental test, and confirmed, disconfirmed, or modified according to the data obtained. At that time I believed--or at least hoped--that such a system was possible and that Hull's proposal might be a step in that direction, a step toward a comprehensive conceptual scheme within which psychological research might be ordered and integrated, and productively pursued. It was the possibility of such a comprehensive conceptual scheme that I found exciting.

Heidbreder's favorable response was, like Tolman's, conditioned by an awareness of logical positivism. However, she reports that she did not personally know before 1934 any psychologists who showed an interest in logical positivism. (There were a few psychologists--e.g., Boring, Stevens, and Skinner--who were following developments in the Vienna Circle philosophy at that early a date.)

Heidbreder's account of the 1934 APA meeting continues with the following anecdote:

74. (cont'd.)

At Hull's address, I was sitting beside Prof. Woodworth who, during its course, handed me an envelope he had fished out of his pocket and on the back of which he had written: "Is this Hullism?" I have no doubt whatever that he intended the triple pun: Hull, whole, holism. I took this to mean that Woodworth too thought that Hull was heading toward a comprehensive system, (Edna Heidbreder to Laurence D. Smith, 10 July 1981).

75. Hull, "Psychology of the Scientist," p. 857.

76. Ibid., p. 857.

77. Abram Amsel, "On Inductive versus Deductive Approaches in Neo-Hullian Behaviorism," in Scientific Psychology, ed. Benjamin B. Wolman and Ernest Nagel (New York: Basic Books, 1965), pp. 187-203, on p. 190. Another researcher in the Hullian tradition has stated that "Hull, perhaps because he began his work in learning theory rather late in his career, was impatient and impulsive, not hesitating to formulate theoretical principles when the available evidence was slight, or to engage in ambitious, but unsophisticated, mathematical theorizing" (Howard H. Kendler, "Kenneth W. Spence: 1907-1967," Psychological Review 74 [1967]: 335-341, on pp. 336-337).

78. These presentations were made at the New York branch of the American Psychological Association in the Spring of 1934 and at the American Association for the Advancement of Science in Pittsburgh on 28 December 1934.

79. Hull, "Psychology of the Scientist," p. 858.

80. Hull, "Mind, Mechanism, and Adaptive Behavior."

81. Hull, "Seminar Notes: Institute of Human Relations," p. 142, Clark L. Hull Papers.

82. Clark L. Hull, Carl I. Hovland, Robert T. Ross, Marshall Hall, Donald T. Perkins, and Frederic B. Fitch, Mathematico-Deductive Theory of Rote Learning: A Study in Scientific Methodology (New Haven, Conn.: Yale University Press, 1940), pp. xi, 305; see also Hull, "Psychology of the Scientist," p. 868.

83. Hull et al., Mathematico-Deductive Theory, p. 11.

84. Hull, "Trial-and-Error Learning," p. 241.

85. Hull, "Conflicting Psychologies," p. 511.
86. Hull, "Trial-and-Error Learning," p. 252
(italics added).
87. This approach was akin to that of Karl Popper, who stresses the meaning of propositions over the meaning of concepts. See, for example, Karl Popper, Conjectures and Refutations: The Growth of Scientific Knowledge (New York: Harper & Row, 1963), pp. 18-19.
88. Hull, "Conflicting Psychologies," pp. 497, 512-513.
89. Hull, "Mind, Mechanism, and Adaptive Behavior," p. 5.
90. Hull, "Conflicting Psychologies," p. 512.
91. Hull, "Idea Book XIII," p. 226.
92. Hull, "Conflicting Psychologies," p. 492; Carl Murchison, ed., Psychologies of 1930 (Worcester, Mass.: Clark University Press, 1930).
93. Hull, "Trial-and-Error Learning," p. 252.
94. Hull, "Conflicting Psychologies," p. 494.
95. Hull et al., Mathematico-Deductive Theory, p. 6; Hull, Principles of Behavior, p. 24.
96. Hull, "Conflicting Psychologies," p. 501; Hull, "Memoranda, 1940-1944," p. 9.
97. Hull, "Conflicting Psychologies," pp. 514-515.
98. Clark L. Hull, A Behavior System (New Haven, Conn.: Yale University Press, 1952), p. 352.
99. Brian D. Mackenzie, Behaviourism and the Limits of Scientific Method (Atlantic Highlands, N.J.: Humanities Press, 1977), p. 183.
100. Edward C. Tolman, "The Determiners of Behavior at a Choice Point," Psychological Review 45 (1938): 1-41, on pp. 3, 14-15.
101. Donald K. Adams, "Note on Method," Psychological Review 44 (1937): 212-218.

102. In 1930, Hull had laid out plans for dealing with criticism. He wrote: "I must pay very little explicit attention to critics and enemies or to local and temporary situations likely to be forgotten within thirty or forty years." He also noted that "a stupid opponent . . . may be turned over to a younger partisan for chastisement" ("Psychology of the Scientist," pp. 836, 835). There is ample evidence that he applied both of these strategies in subsequent years.

103. Hull, "Conflicting Psychologies," pp. 492, 495.

104. Clark L. Hull to Kenneth W. Spence, 20 May 1941, Spence Papers, Archives of the History of American Psychology, Akron, Ohio.

105. Hull, "Clark L. Hull," p. 162.

106. Hull, Principles of Behavior, p. 380.

107. Hull, "Clark L. Hull," p. 154.

108. Hull, "Idea Book XII," p. 46.

109. Hull, "Trial-and-Error Learning," p. 241.

110. Hull, "Memoranda, 1940-1944," p. 1.

111. Arne Naess to Laurence D. Smith, 7 April 1981.

112. Hull, "Mind, Mechanism, and Adaptive Behavior," p. 10.

113. See Hull, "Trial-and-Error Learning," which was written in 1929.

114. Hull, "Clark L. Hull," pp. 156-157; Hull et al., Mathematico-Deductive Theory, p. xi.

115. See, for example, Hull, "Seminar Notes: Institute of Human Relations," p. 3. Hull was by no means the first to attempt an integration of behaviorist and psychoanalytic approaches. Robert R. Sears has identified G. V. Hamilton as the "first architect" of such a fusion. A paper of 1934 by Sears himself is claimed to have sparked Hull's interest in the area (Robert R. Sears, "Psychoanalysis and Behavior Theory: 1907-1965," paper presented at the American Psychological Association, Los Angeles, 27 August 1981). Hull's IHR seminar of 1936 focused on the integration of behaviorism and psychoanalysis. From this seminar there evolved several well-known works, including John Dollard, Leonard W. Doob, Neal E. Miller, O. Hobart Mowrer, Robert R. Sears, Clelland S. Ford, Carl I. Hovland, and

115. (cont'd.) R. T. Sollenberger, Frustration and Aggression (New Haven, Conn.: Yale University Press, 1939).
116. Hull, "Conflicting Psychologies," p. 515.
117. Hull, "Mind, Mechanism, and Adaptive Behavior," p. 29.
118. Robert R. Sears, "Robert R. Sears," in A History of Psychology in Autobiography, vol. 6, ed. Gardner Lindzey (San Francisco: W. H. Freeman, 1979), pp. 395-433, on p. 410. Another participant in the seminar has written that investigators outside the IHR "did not understand the passion to integrate which drove us." See John Dollard, "Yale's Institute of Human Relations: What Was It?" Ventures (Winter, 1964): 32-40, on p. 39.
119. Clark L. Hull and O. Hobart Mowrer, "Hull's Psychological Seminars, 1936-1938," Yale University Library, 1952 (Mimeographed), p. 56.
120. See, for example, Memorandum from Edmund E. Day to Mr. Fosdick, 28 July 1936, Rockefeller Archive Center, North Tarrytown, N.Y. The Mathematico-Deductive Theory was rushed to press in late 1939, in part, in an effort to persuade Rockefeller Foundation officials to renew their funding of the IHR. This and several other measures met with minimal success, however. Whereas the original grant had been for \$4.5 million, the renewal was for only \$700,000.
121. Hull, "Seminar Notes: Institute of Human Relations," p. 123. The desirability of explicit theory may have been generally recognized, but at least some participants in the seminar appear to have found Hull's logical approach "cumbersome." It has been suggested that Miller and Dollard offered the simple "frustration-aggression hypothesis" as an antidote to the complexities of Hull's approach (Sears, "Psychoanalysis and Behavior Theory").
122. Hull, "Seminar Notes: Institute of Human Relations," pp. 166-167. Hull later attributed this strategy to IHR director Mark May ("Psychology of the Scientist," p. 880).
123. Hull, "Seminar Notes: Institute of Human Relations," pp. 123 ff.

124. Ibid., p. 128.

125. Hull et al., Mathematico-Deductive Theory, p. ix.

126. Hull's strategy for supervising integrated research included the offering of experimental equipment to investigators outside of Yale in exchange for their conducting specified experiments. This approach generated resentment among researchers whose work, unlike Hull's, was not well-funded during the Depression. Fred Keller wrote to B. F. Skinner that Hull had made him such an offer. Skinner replied: "Your news about Hull made me damned mad. . . . to bribe you into doing his exp.--that's a little too much. However, if you can screw him for the apparatus, why not. Get anything you can . . . you prostitute, but keep your fancy free" (quoted in B. F. Skinner, The Shaping of a Behaviorist [New York: Alfred A. Knopf, 1979], p. 205). Another psychologist who angrily viewed Hull's overtures as bribes was Tolman's student Krechevsky, who said that "Hull was trying to buy disciples" (David Krech, "David Krech," in A History of Psychology in Autobiography, vol. 6, ed. Gardner Lindzey [Englewood Cliffs, N.J.: Prentice-Hall, 1974], pp. 219-250, on pp. 233, 234).

127. As early as 1925, Moritz Schlick speculated on the relation of minds to machines, but his remarks were brief and he did not pursue the ideas (much less develop a mechanistic philosophy). See Moritz Schlick, General Theory of Knowledge, trans. Albert E. Blumberg (New York: Springer-Verlag, 1974), pp. 135-147. (This work was originally published in 1925.

CHAPTER 7

HULL AND LOGICAL POSITIVISM

In terms of the events which brought about the interaction of European logical empiricism with American psychology in general, and Hullian behaviorism in particular, the years 1935 and 1936 were of great significance. On the American side, 1935 saw the beginning of a proliferation of essays on operationism in psychology. One of S. S. Stevens's influential papers of that year contained references to Carnap's work, thereby introducing logical positivism to the readership of American psychological journals. In the same year, Yale's Institute of Human Relations began its renewed push for the integration of the social sciences. Hull's leading role in the Institute's efforts at unifying science led naturally to an interest in the Unity of Science movement.

On the European side of the Atlantic, the assassination of Moritz Schlick in 1936 shifted the focus of logical positivism from the Vienna Circle proper to the broader movement for the unity of science. As Charles Morris wrote to Neurath upon receiving word of Schlick's death, "The fate of the Wiener Kreis now becomes linked with the fate of the international movements--Vienna is no longer its home."¹ Whether or not Schlick's murder was an

explicitly political act, it clearly portended the increasingly inhospitable climate of Austria for those associated with the Vienna Circle.

At the same time the movement was being forced to expand geographically, it was beginning to broaden also in terms of the areas of science which were represented. The Vienna Circle had been comprised predominantly of logicians and phsycists. The leadership of the Unity of Science movement, namely Morris and Neurath, began an effort in 1935 to recruit more biological and social scientists into their ranks. In addition to adding breadth to the movement, the inclusion of objectively oriented behavioral scientists would help to reduce skepticism about whether the recently adopted physicalist language could be a truly universal language, one that was as adequate for psychology as for physics. As Morris was quick to point out, the expansion of the movement would necessarily involve attracting more American members since European science was considered weakest in the biological and social disciplines. Even so, Morris acknowledged that where social scientists were involved it would "be difficult to keep the material on a scientific level."²

The important period at mid-decade reveals that American behavioral scientists and European positivists had much to offer each other. The Americans could help build the international base needed by the positivists to ensure

the scope and plausibility of their movement, as well as its very survival in the face of a hostile environment. In return, the Europeans appeared to offer American psychologists a means of guaranteeing scientific status to their field, a model of scientific cooperation, and the logical expertise to integrate the fruits of their theoretical labors with those of the advanced sciences. The seeds of mutual interest were beginning to sprout.

Hull and the Unity of Science Movement

Given the extent of interests shared by Hull and the logical empiricists, it was natural for them to join forces. The Third International Congress for the Unity of Science, which was held in Paris in 1937, was devoted to "the problems of scientific co-operation, especially in connection with the Encyclopedia and the unification of logical symbolism."³ With his long-term interest in logical methods and his more recent involvement in the integration of science, Hull surely found the program attractive. His attendance at the Congress initiated a period of personal and intellectual contact with various of the figures and ideas associated with the Vienna Circle. At the level of personal contact, the interactions included visits to the Institute of Human Relations by Neurath in 1937, J. H. Woodger in 1938, Arne Naess in 1938-39, and Gustav Bergmann in 1939. Hull also took on a modest role in the Unity of

Science movement, in which he occasionally contributed papers and served on committees.

Despite these interactions, the influence of logical positivism on Hull was, perhaps not surprisingly, more to corroborate certain views he already held than to suggest new ones to him. Specific influences can be detected in certain additions to the vocabulary of Hull's methodological exhortations and in the limited assistance he received from J. H. Woodger in formalizing the rote learning theory (see following section). But, as we shall see, the explicit elaboration of a coalition between behaviorism and logical positivism awaited the works of Gustav Bergmann and Kenneth Spence, one of whose contributions was in fact the clarification of the divergence between Hullian theory and theory as classically conceived by the Vienna Circle.

The aim of the present section is simply to review Hull's involvement in the Unity of Science movement. The account will indicate the scope and limitations of this involvement, but the suggestion of intellectual reasons for its limited extent will be deferred until the next chapter.

One immediate consequence of Hull's attendance at the Paris Congress was his adoption of the phrase "logical empiricism" to describe the scientific procedure he was advocating. The phrase first appeared in a memorandum to his seminar in October 1937, less than three months

after the Congress. It was also invoked in a published article which appeared in 1938 but was completed in November 1937. The expression "logico-empirical method" began to show up in his idea books not long thereafter. Similarly, in the year following his trip to Paris, Hull began using the term "Geisteswissenschaft" to designate unscientific approaches to psychology. In contrast, the accepted approach at the IHR was, as Hull noted, "a Geisteswissenschaft without the Geist."⁴

A second consequence which quickly followed Hull's attendance at Paris was his being drawn into the activities of the Unity of Science movement. Shortly after the congress, Neurath reported to members of the movement's permanent committee that Hull was "very interested in our work," that he had been "very happy to meet with Woodger," and that he had invited Neurath to come stay with him and visit the Institute.⁵ Neurath visited the IHR that fall, at which time he asked Hull to contribute to the pamphlet series Einheitswissenschaft and to become a member of the movement's Advisory Committee.⁶ Hull accepted both invitations.

Hull's brief contribution to Einheitswissenschaft appeared in 1938. Being based on his remarks at the Paris Congress, the paper summarized the research program of the IHR as it had developed with Hull's guidance and pointed out its similarities to logical empiricism. Hull began:

There has recently grown up in America a scientific development which shows certain resemblances to the approach of the Vienna Circle. This movement tends to center around the Institute of Human Relations, at Yale University.

Proceeding to a discussion of the difficulty involved in coordinating researchers of diverse backgrounds, Hull wrote;

From this difficulty has gradually evolved a methodology of integrating scientific effort which involves an intimate combination and coordination of logical and empirical procedures. The methodology consists of three phases--two empirical and one logical.

In the first phase, postulates representing basic laws of behavior are determined directly by experiment. Data from conditioning experiments are fitted by mathematical equations which serve as "first-approximation" postulates. In the second phase, "theorems" about observable phenomena are derived from the postulates "by the procedures of symbolic and of ordinary mathematics." In the third phase, the attempt is made "to determine by observation whether the deduced sequels really occur." When deductions fail to conform to fact, the postulates are recast with the aid of further experiments on the basic laws. "Following such new postulate determinations," wrote Hull, "new implications are drawn, new verificational investigations are set up, and so on in continuously recurring cycles."⁷

Hull then summed up the three-phase procedure as follows:

The methodology begins with an empirical determination of its postulates and ends with an empirical check on the objective validity of its theorems; between the two there lies the integrating symbolic structure of logic and mathematics. Thus arises the kinship to "logical empiricism,"⁸

Hull's characterization of the relationship between his methodology and logical empiricism as a kinship was an accurate one. The relationship was certainly not one of identity. While the logical empiricists would have felt sympathetic to hypothetico-deductive method in any of its variants, and would have especially applauded its use in the social sciences, they also must have found Hull's methodology remote from their own views in some respects. In particular, they must have been struck by the odd notion that postulates could be generated directly from experimental data by curve-fitting. Even if relations between experimental variables could plausibly be construed as scientific laws, such "laws" were far from qualifying as postulates in any formal sense. The standard logical positivist view of the early thirties had held that the axioms of a theory receive empirical significance when the terms in them are explicitly defined as equivalent to expressions in the observation language. But not even this simplistic view countenanced the Hullian idea that the level of theoretical postulate might be directly invaded by experimental method, that lawfulness itself could rise from empirical relation to law and then to postulate so as to ensure

empirical content to whole postulates and not merely to the terms in them.⁹ Needless to say, the notion of recurring cycles of this procedure would also have sounded strange to philosophers who were accustomed to applying logic to the rational reconstruction of extant, highly developed theories in the physical sciences.

If Hull's views on methodology sounded somewhat odd to the logical positivists, his views on the unity of science and scientific cooperation must have struck a chord with them. The second half of his paper was devoted to these topics. Hull wrote:

Ideally, each investigator takes his postulates from the level just below his specialty and the verified theorems deduced from them by him at his own level become the postulates of the men working at the next higher (more complex) level, and so on throughout the whole range of the phenomena under investigation. Thus a theoretical integration joining all levels becomes possible.

The separate theorems of such a system, together with the postulates from which they have been derived, present automatically an integrated empirical research program. . . . The methodology of "logical empiricism" as here interpreted accordingly presents a means of solving the problem of making effectively a simultaneous integrated and coordinated attack by many workers over a wide range of phenomena.¹⁰

Hull then recited in order of complexity a list of twenty problem areas ranging from the physiology of motivation to the comparative study of primitive cultures.

Given that the theme of the conference emphasized scientific cooperation in connection with the unification

of logical symbolism, Hull's contribution could hardly have been more fitting. Here was a clear statement of his suggestion that the coordination of scientific efforts be based on the logical integration of theory. Indeed, logical integration was said to provide "automatically" for social cooperation. It mattered little that Hull's prescription was based more on faith than on accomplishment or that his methodology was peculiar. In Hull's statement was a vision of unity that went beyond the mere unity of terminology promised by the doctrine of physicalism. Furthermore, it was coming from a psychologist, one who was influential in America and who was pressing the objectivist attack into the heart of the social sciences. It is thus not surprising that Neurath wasted no time in making contact with the Institute and enlisting Hull's aid in promoting the Unity of Science movement.

When the movement held its first congress in America in 1939, Hull served on its American Organizing Committee. However, he was unable to attend the congress because of psychology meetings being held in California at the same time. The next congress was held at the University of Chicago in 1941. Neurath and Morris planned as part of it a special symposium on psychology and scientific method. Hull was invited to be a principal speaker, along with Lewin and Brunswik. After accepting the invitation, Hull wrote to Morris that he "would like very much to

make the acquaintance of the active American members of this exceedingly important movement."¹¹ This remark suggests that while Hull was an enthusiastic supporter of the movement, he was not highly involved in it.

The title originally submitted for his presentation was "The Objective versus Subjective Approach to a Scientific Theory of Behavior." The title captured a familiar but perhaps overly general theme and was later changed to "The Problem of Intervening Variables and of Intuitional Irrationality in Molar Behavior Theory." This change was evidently made in an attempt to target the anthropomorphism which Hull saw as the great shortcoming of the Berkeley approach; but when Brunswik avoided any semblance of anthropomorphism in his address, Hull again revised the title and published the paper as "The Problem of Intervening Variables in Molar Behavior Theory."¹²

The paper began predictably with an acknowledgement of the general convergence between logical positivism and behaviorism. Hull wrote:

There is a striking and significant similarity between the physicalism doctrine of the logical positivists (Vienna Circle) and the approach characteristic of the American behaviorism originating in the work of J. B. Watson. Intimately related to both of the above movements are the pragmatism of Peirce, James, and Dewey on the one hand, and the operationism of Bridgman, Boring, and Stevens, on the other. These several methodological movements, together with the pioneering experimental work of Pavlov and the other Russian reflexologists, are, I believe, uniting to produce in America a behavioral discipline

which will be a full-blown natural science; this means it may be expected to possess not only the basic empirical component of natural science, but a genuinely scientific theoretical component as well. It is with the latter that the present paper is primarily concerned.¹³

Hull felt that the empirical component of psychology had more than enough defenders and viewed himself as a rare champion of the theoretical component. Accordingly he was eager to argue his own brand of theory against Brunswik's, particularly in regard to two points. First, whereas Brunswik had claimed that the laws of molar behavior were necessarily probablistic, Hull held them to be uniform and exact. Secondly, whereas Brunswik was (at that time) claiming to eschew intervening variables, Hull was eagerly embracing them. These two points, the uniformity of law and the legitimacy of intervening variables, were closely related in Hull's thinking. The equations which would express the exact causal laws were presumed to provide the determinate linkages between intervening variables and their "anchors," namely the observable antecedent and consequent variables. Causal uniformities and intervening variables were mainstays of his Principles, which he was writing at that time, so he was duly eager to defend these positions in advance of the book's appearance.

On the first point of dispute, Brunswik had asserted that since the environment is composed of probabilistic event sequences, the organism can adapt to it at best

optimally, but never perfectly. As a consequence, he maintained that the laws of molar behavior should themselves be probabilistic.¹⁴ Hull, of course, viewed such an approach as an unjustified and defeatist resort to probabilism. He preferred to carry on the search for uniform causal laws expressed as mathematical equations. Any variability in these laws, he argued, could be attributed to limitations on the precision with which values of variables and constants could be determined. The equations themselves would continue to capture causal sequences even when the measured values which went into the equations were less than exact.¹⁵ In a typical rhetorical gesture, Hull intimated that he could derive from his own behavioral laws the type of correlations found by Brunswik in his vision studies. "If Brunswik is prepared publicly to challenge the possibility of such a derivation," Hull wrote, "I am prepared to attempt it and publish in this journal the outcome, whatever it turns out to be."¹⁶

For his part, Brunswik felt that his views had been misunderstood and misrepresented by Hull, and that they were complementary rather than contradictory to Hull's. Indeed, Brunswik and Hull seemed to be speaking past each other, as often happens when differences spring from submerged presuppositions. In this case, those presuppositions involved no less than deeply rooted conceptions of nature itself. Under the influence of Reichenbach and with the

collaboration of Tolman, Brunswik viewed the natural world as a loosely woven texture of probabilistic event sequences. As a committed mechanist, Hull saw the world at large as a machine governed by a rigid structure of causality. Because both men were ardent environmentalists, they believed alike that the behavior of organisms takes on the structure of the environment. Without realizing it, they also agreed that the structure of behavioral theories should mirror the behavior in their domain. Hence, it was agreement on intermediate issues that brought the deeper divergence to the surface in the form of an unfruitful "methodological" dispute.

The second point of contention between Brunswik and Hull, namely that of the legitimacy of intervening variables, likewise rested on a deeper issue. Brunswik's major objection to their use seemed to be that since they represented intra-organismic processes, they would draw attention away from the all-important environment. In Brunswik's terminology, "psychological ecology" was properly a precursor to "ecological psychology." But there was another, perhaps more compelling reason for a probabilistic functionalist to doubt the practicability of intervening variables. For an advocate of strict causality like Hull, the widely acknowledged drawback of intervening variables--their unobservable status--was compensated for by the fact that they could be causally linked to observables. For the

advocate of probabilism, however, they could be only probabilistically related to observables, in which case their inherent unobservability counted seriously against them.

Despite this added difficulty of intervening variables on the probabilist view, Brunswik did not object in principle to their use. Hull even argued, on dubitable grounds, that Brunswik had invoked them in his studies on visual constancy.¹⁷ In any case, Brunswik's skepticism concerning their value provided Hull with an occasion to rise to their defense. One line of defense was to argue, as had Tolman, that intervening variables were in fact widely used, implicitly or explicitly, in theories of behavior. To this end, Hull presented diagrams purportedly representing the role of intervening variables in the formulations of various theorists. Perhaps to highlight the kinship of behaviorism and logical positivism, Hull included Carnap among them. Referring to Carnap's treatment of anger in Philosophy and Logical Syntax (1935), Hull depicted anger (see figure) as a variable intervening between antecedent stimulus conditions (\dot{S}) and the observable response (\dot{R}). The functions f_1 and f_2 linking anger



to observables were described by Hull as "quantitative functional relationships."¹⁸

At the crudest level, Hull was correct in stressing that he and Carnap agreed on the legitimacy of introducing unobservables into psychological theory. At any other level, however, Hull had misrepresented Carnap's approach and had done so in ways that minimized the differences between their approaches. In his 1935 monograph, Carnap had spoken only of equivalences, either analytic or synthetic, between statements about anger and statements about bodily states or dispositions.¹⁹ Nowhere had he specified those equivalences as quantitative functions. Nor had he spoken at all of antecedent stimuli or consequent responses. Thus, Hull not only showed a limited understanding of Carnap's early view of theoretical concepts but he also showed no awareness of the important changes in Carnap's position after 1935.²⁰

In sum, Hull's involvement in the Unity of Science movement is revealing in both its extent and its limitations. Hull shared with the movement's major figures a general outlook and certain specific views on the integration of science. He recognized in these shared ideas a broad confluence of movements which he enthusiastically welcomed. He happily lent his name and prestige to the Unity of Science movement and contributed papers to its congresses. But the kinship which Hull perceived between behaviorism and logical positivism was no more than a kinship. He adopted snippets of the jargon of logical positivism, but he was in

no way a serious student of its philosophical stances and appeared not even to follow important developments in its basic doctrines.

Woodger and Formalized Theory

Shared Views of Science

In 1929, the British biologist Joseph Henry Woodger published a book entitled Biological Principles²¹ in which he applied the linguistic analysis characteristic of the Cambridge philosophers to various concepts and issues of biology. He claimed to show that many biological disputes were due to the lack of clarity in the concepts used or to the intrusion of unnecessary metaphysical influences. In the further pursuit of these claims, he turned during the early thirties to the application of the new symbolic logic to the formalization of biological theory. With the help of Carl Hempel and Olaf Helmer, who were associated with the Berlin Society for Empirical Philosophy, Woodger studied the Principia Mathematica. At the First International Congress for the Unity of Science in 1935, he gave a paper on "An Axiom System for Biology." He then enlisted the assistance of Rudolf Carnap and Alfred Tarski in expanding the work into his 1937 volume The Axiomatic Method in Biology.²²

It was in 1937, at the Third International Congress for the Unity of Science, that Woodger met Hull in what seemed to be a highly auspicious encounter. At that time, Hull was, as we have seen, becoming disillusioned with the geometrical method as it had been used in his miniature systems. The 1935 system, as he put it, "revealed serious defects . . . as to the versatility of the geometrical methodology for mediating quantitative deductions in the field of behavior."²³ Already for some years he had been impressed with the potential of the PM logic for systematizing his theoretical efforts. Here at the congress was Woodger who had just completed his application of logistic technique to biological theory. Hull's inclination toward symbolic logic was sharply strengthened by this meeting with Woodger. Somewhat later he recorded that Woodger "had convinced us of the indispensability of the methods and symbolism of symbolic logic for the rigorous formulation of definitions and postulates in any genuinely scientific system."²⁴

The two men were obviously drawn together, not only by their interest in logic, but more importantly by its application to "dynamic natural-science systems" in the life sciences.²⁵ For both, symbolic logic was no mere tool for reconstructing previously established theories but rather a potent means for clarifying and evaluating new ones; it offered a methodology for scientific activity,

not just a post hoc system for displaying results.²⁶ That this attitude toward the use of logic came from two life scientists was no coincidence. Whereas the mathematicians and physicists of the Vienna and Berlin groups had a stock of highly developed theories on which to perform their rational reconstructions, Woodger and Hull were restricted to the relatively inchoate theoretical efforts of the social and biological disciplines. As a consequence, their adaptation of logistic techniques involved an important, but not clearly perceived, shift in emphasis from the use of those techniques for reconstruction per se to their use in methodology. As we shall see, the resulting ambiguity later contributed to misunderstandings about the use of "axiomatic" methods in psychology.

Hull found in Woodger not only an expert in applied symbolic logic but also one who shared his optimism about the benefits of deductive methodology. Like Hull, Woodger was convinced that the clarity introduced by such methods would effectively combat undesired metaphysical influences and eliminate needless controversy. He wrote, for instance, that

a wider diffusion of a knowledge of modern discoveries relating to logic would dispel a good deal of the prevailing confusion (which shows itself from time to time in the controversies which disfigure the pages of Nature) concerning topics which are usually included under the title 'methodology,' such as the relation of mathematics to natural science, "the logic of science," the relation of science to metaphysics, and kindred subjects.²⁷

The key to realizing such benefits was the development of "an ideal scientific language" because, according to Woodger, "if we have a perfect language we need not dispute, we need only calculate and experiment."²⁸ Hull's vision of scientific salvation was more focussed on deductive method itself, and Woodger's more on linguistic construction, but the outcome was the same. Science would become a sort of smooth-running conceptual and social machinery; unity of method and theory would give rise to scientific and social cooperation. Hull and Woodger had arrived in parallel at a modern version of the Leibnizian dream.

The division of labor which Hull had conceived and in part enacted at the Institute of Human Relations had its counterpart in Woodger's thought. In his contribution to the Encyclopedia of Unified Science, Woodger concluded with a colorful revision of Francis Bacon's fantasy New Atlantis, in which Bacon had laid out a blueprint for cooperation at a futuristic college of science. In Woodger's version, certain knowledgeable individuals called Lamps are assigned the duty of conceiving new hypotheses. Each hypothesis so conceived is written on a card in the universal symbolic language and sent to a worker called a Calculator. Woodger continued:

The Calculator operates a gigantic calculating machine, like our machines but of very much wider scope, being capable of working out the consequences of any hypothesis which can be formulated in the universal notation. . . . The consequences of the new hypothesis . . . are then shot out of the machine printed on cards.²⁹

From among these, the Sorters then select novel and non-trivial consequences and send them to the Inoculators for experimental tests. The empirical results are given to the Lamps for use in future hypothesizing as well as to the Compilers who keep an index of all the postulates and scrutinize them for "mutual relations." Woodger's picture of science was quite close to Hull's, especially in its striking emphasis on the related unities of scientific method and activity, and even on the automatic qualities of each.³⁰

Both Hull and Woodger were motivated by the perceived imbalance in their respective disciplines between the relatively well-developed empirical component and the regrettably underdeveloped theoretical component. Both looked to logic, in particular that of the Principia Mathematica, as the means of systematizing their fields. With that background and the shared views of science which grew out of it, they were similarly drawn toward the Unity of Science movement. But their shared belief that logic would ensure clarity, understanding, and cooperation did not automatically guarantee that they would themselves come to a full mutual understanding, much less that others would understand their uses of logic. As it turned out, logic in either its pure or applied form was itself subject to controversy and conflicting interpretations. Consequently it proved to be no surer a route to forging a consensus on scientific matters than did operationism.

The Problem of Definition in Formal Theory

Woodger's book on axiomatic technique in biology was completed in July 1937, the same month in which he met Hull at the Paris Congress. Like Neurath, he wasted little time in getting to the IHR to pursue the interests he held in common with Hull. He applied to the Rockefeller Foundation for a fellowship to work with Hull and Fitch on systematizing the rote learning theory, gained approval of the grant in September of that year, and spent nine weeks at the IHR in early 1938.³¹ Woodger had felt that two or three months would be adequate time to effect an axiomatization, but there arose various difficulties which prevented an easy accomplishment of the task. Hull later wrote that Woodger

worked with me on the problem a good many weeks but was forced to return to London before anything publishable was written. Largely because of the delay of the mail in crossing the Atlantic at that time we were unable to complete what Woodger had begun, so that . . . Fitch . . . finally wrote out the eighty-six definitions and formulated the eighteen postulates in symbolic logic.³²

Woodger did eventually complete a formalization of a small portion of the rote learning theory and presented it at the 1938 Congress for the Unity of Science.³³ But by then, the application of symbolic logic to the theory was in Fitch's hands.

With his background in the Cambridge style of conceptual analysis, as pursued in his 1929 book, Woodger's

formal approach placed emphasis on the clarification and definition of terms from ordinary and scientific discourse rather than on the analysis of axiom systems or quantitative laws. Even with his subsequent interest in axiomatics, Woodger viewed the construction of formal vocabulary from the scientific languages as prior to the use of that vocabulary in statements or deductive hierarchies of statements. "In employing the logistic method," Woodger wrote, "we make use of the results of the logistic analysis of ordinary language for the more exact systematization of the branch of science with which we are concerned."³⁴ In effect, Woodger was working upward from the conceptual base rather than downward from an axiom system. His approach was that of constructive rather than deductive axiomatics.³⁵

Partly as a consequence of Woodger's focus on concepts per se, he and Hull met with difficulties in working out the equations in Hull's system. As we have seen, one of the stated reasons for Hull's dissatisfaction in the mid-thirties with the geometrical method was that it had proven awkward in dealing with quantitative deductions. Spurred by Tolman's development of the intervening variable paradigm, Hull was increasingly using equation and had even hired mathematicians to help with their formulation. Woodger, on the other hand, had formalized only non-quantitative systems. The seriousness of the problems confronting Hull and Woodger is suggested in a passage

in Hull's idea books in which the attempt is made to revise the basic dependent variable of reaction potential (E). The passage is titled "New start on a theory of E" and begins with the following statement:

The great difficulty encountered by Woodger in securing a logical statement of \bar{E} because of the finite size of R forces us to seek some other way of stating the matter, because the mathematics seems to work out all right except that some of the expressions can't be solved.³⁶

While such technical problems occupied much of the attention of Hull and Woodger, a deeper confusion concerning the use of equations in theoretical psychology involved intervening variables, which by their very nature were unobservable. Hull had already arrived at his general notion that unobservables could be legitimized by being linked to antecedent and consequent observables. But for a rigorous way of applying this vague dictum to definitions in his system, Hull looked to Woodger. From Woodger's rather ambiguous statements on the matter of definition Hull drew an interpretation of the place of unobservables in a formal system which would become a significant source of misunderstanding.

Early in his visit to the IHR, Woodger wrote and distributed to Hull's seminar a lengthy memorandum entitled "Brief Statement of What is Involved in the Formalization of a Theory."³⁷ In it, he outlined, in the order to be carried out, the four basic steps in formalizing a theory.

They were (1) the choice of fundamental (undefined) terms; (2) the construction of defined terms from the fundamental terms via explicit definitions; (3) the formulation of statements using the vocabulary thus defined and the choice among them of those statements which conform empirically to the facts and logically to the rest of the theory; and (4) the search for consequence relations among the accepted statements so that they might be ordered into a deductive system. Even from this scant summary, it is clear that Woodger's formalization differed sharply from the Hilbert style of deductive axiomatics, which began with the choice of an uninterpreted deductive system and worked downward by a process of empirical interpretation. Woodger's emphasis on explicit definition (in the sense of definition by biconditional) as the means of building up the language from primitive terms was also significant, for it neglected important liberalizations in the logical positivist notion of empirical definition. Woodger did in fact give passing mention to Carnap's recently devised reduction sentences in a footnote of his memorandum;³⁸ but as we have seen, Hull was interested in rigid logical connections between concepts and remained oblivious to the newer and looser versions of empirical definition.

Hull's immediate problem was to ensure the legitimacy of intervening variables in the face of their inherent

unobservability. In Woodger's exposition of fundamental terms, Hull found the following account of how meaning is imparted to them:

Their meaning should be clear to whoever is constructing the system and it should be possible to make their meaning clear to anyone else either by verbal explication (which would not itself be part of the system) or through the part they play in the system when the latter is finally constructed.³⁹

The second alternative amounted a peculiar sort of implicit definition, not the usual sort in which the term gets its meaning from its place in an abstract axiom system set up ab initio, but rather from its role-to-be as the system is developed. The first alternative was also not a typical feature of the standard deductive axiomatics. For Woodger, it would have involved explicating the term as it is commonly used, i.e., performing a bit of ordinary language analysis. For Hull, who was not well-versed in either logistic technique or linguistic analysis, the two alternatives were rather ambiguous directives.

This ambiguity left Hull with ample room for interpretive license in applying the two strategies to the problem of the intervening variables. As he pushed the rote learning monograph to its completion in late 1939, he alternatively tried out the verbal explication technique and an adaptation of the implicit definition approach. When he turned to the method of verbal explanation, he saw it as a way to fit operational definition into his

postulational technique. As we shall see below, the attempt to do so resulted in the confusing and incongruous situation of his giving operational definitions to the "undefined" terms in his system. The following section will review Hull's efforts, in the wake of Woodger's visit to the IHR, to systematize the rote learning theory. The key problem in this enterprise proved to be that of defining the intervening variables.

The Definition of Intervening Variables

In the fall of 1938, Hull drew up a concise statement of the roles played by mathematics and symbolic logic in scientific method. As recorded in his idea book, the statement read:

Scientific method in theory needs:

1. Mathematics to mediate metricized implications of the postulates.
2. Symbolic logic to mediate,
 - A. the drawing of implication from qualitative postulates
 - B. the precise formulation of all postulates
 - C. the differentiation of the terms left undefined from those to be defined.
 - D. the actual definition of the terms to be defined.⁴⁰

The notion represented in point 2C to the effect that logic could distinguish terms to be defined from those to be left undefined was characteristic of Hull's overly optimistic faith in logic. Woodger had left the determination of undefined terms, in the spirit of the principle of tolerance, as a matter to be decided by careful consideration of the system being formalized. In any case, Woodger's

visit had failed to produce any consensus on the issue, and Hull continued to struggle with the problem.

In Hull's view, intervening variables could be assured of having scientific status by being linked to antecedent and consequent variables. This meant that, in principle at least, the intervening variables could always be given definitions in terms of observables. As Hull put it, "all unobservables must ultimately [be] linked logically in a strict manner in a more or less complicated way to either one, or a combination of, the observables in order to mean anything." But, he added,

while this seems to be almost certainly true, it happens that, at least in the rote learning theory, such a form of definition would be exceedingly involved and quite unintelligible to all but the most sophisticated. It is fairly clear that such a procedure is an exceedingly inconvenient form of procedure, to say the least. For this reason it becomes very desirable to see whether the canon of being able to observe both [antecedent and consequent link-ages] cannot be conformed to when the unobservables are placed in the undefined notions?⁴¹

This comment, coming shortly after the above quoted formulation of the role of logic, indicates that Hull was prepared to give up the presumed logical criteria for differentiating defined and undefined terms and adopt instead a pragmatic basis for the decision. This was, of course, not the only case in which Hull gave expedience priority over rigor, and without an accompanying reduction in the rhetoric of objectivism. But it was a significant

case because it involved the very roots of his claim to objectivity, namely his systematic approach to theory.

The possibility of leaving unobservables undefined went against Hull's own conditions of linkage to observables, but he pursued the idea nonetheless. Speaking of those conditions of observability, he wrote:

Upon further reflection and conference with Dr. Fitch, it rather seems as if [they] may be satisfied perfectly well even though the unobservables were all placed among the undefined notions. After all, whether [they] are conformed to depends, in the final analysis, upon the statement of the theorems. If they conform, everything will be all right.⁴²

Then, after considering a couple of examples, Hull concluded, "Thus, apparently, the problem finds solution at last."⁴³ Given the emphasis placed by Hull himself and other methodologists of the time on the empirical definability of unobservables, this was a peculiar solution indeed. In effect, it constituted a sort of implicit definition, as in Woodger's second alternative, by which the terms derived their meaning from their context in the theory, but in the absence of the usual uninterpreted axiom system.

Having worked on the rote learning monograph for a period of several years, Hull viewed it as a long overdue predecessor to an eventual magnum opus, a ground-breaker to establish the viability of his deductive methodology. As he stated in the preface of the work, "its chief value

consists in the large-scale pioneering demonstration of the logico-empirical methodology in the field of behavior."⁴⁴

As to the problem of defining unobservables, the "solution" arrived at by him and Fitch was realized in the text by placing such terms as "stimulus trace" and "inhibitory potential" among the undefined concepts.⁴⁵ The hypothetical and unobservable nature of these concepts was explicitly recognized in the material accompanying their introduction as undefined concepts. For example, Hull wrote:

The concept stimulus trace has substantially the status of a symbolic or logical construct. While there are physiological indications that the expression represents an entity which may ultimately be observable in some indirect manner, for the present purposes it may be regarded as an unobservable. The existence of this hypothetical entity is explicitly assumed by Postulate 1.⁴⁶

As will be discussed in the following section, various criticisms expressed during the writing of the Mathematico-Deductive Theory made Hull aware of the inappropriateness of assigning unobservables to the category of the undefined terms. In his idea book of that time, Hull wrote the following entry:

It seems very clear from the criticisms which have been leveled at my two miniature formal systems together with the experience in the writing of the rote-learning monograph, that I really must make the greatest possible effort to state all the undefined notions in terms such that they may be directly observed for this is the real virtue of the "operational" movement.⁴⁷

Hull was just coming to grips with operationism at this time (see below), and he viewed it as a means of explicating the undefined terms in his system, much in the manner of Woodger's first alternative strategy. At the time it provided a way to avoid the awkward treatment of the unobservable concepts as undefined. This had become apparent to Hull by the time the rote-learning volume was nearing completion. In the conclusion to it, he wrote:

In the statement of the groundwork of the system, the undefined notions are formally defective in a number of cases in that they do not represent observable objects, processes, or operations, either logical or experimental. More specifically, it is believed that several of the undefined notions (logical signs), including all those representing unobservables, should have appeared among the defined terms. Presumably the next attempt at a formalization of rote-learning theory should make this correction one of its earliest objectives. . . . Here, evidently, is a place where the principles of "operationism," with their very real scientific virtues, should be applied.⁴⁸

Without acknowledging them as such, Hull had already employed operational definitions in the rote learning theory. Indeed, of the sixteen "undefined" concepts, the majority were actually given operational definitions. To cite but one example, the concept of "syllable exposure" was characterized as a "class of events each of which may be described as the stationary presence in the window of a memory machine . . . of a syllable consisting of a vowel placed between consonants in a combination not used as a word by the subject."⁴⁹ As Bergmann and Spence were to

point out shortly thereafter (see below), most of Hull's defined and undefined terms were in reality operationally defined, the difference between them consisting for the most part only in the length of their defining expressions. Thus, Hull was not only placing unobservable terms among the undefined terms, in abrogation of the usual empiricist methods, but he was also placing operationally defined terms among them, in contrast with the usual formalist practice of giving undefined terms only implicit definitions.

To summarize, Hull's confusing use of axiomatic technique came about as the result of a rather peculiar set of circumstances. When Woodger came to the IHR to help axiomatize the rote learning theory, he brought with him an emphasis on constructive axiomatics and ordinary language analysis, neither of which was typical of the standard formalist approach to axiomatization. As a formalizer of biological theory, he lacked any particular experience with formalizing the sort of equations and quantitative methods which Hull was becoming interested in at the time. He was apparently not especially alert to the more sophisticated versions of empirical definition, or at least did not convey them effectively to Hull as a possible means of treating intervening variables. By the time Woodger returned to England, the hoped-for systematization had not materialized, and the task was

left to Hull and Fitch. The final decision on which terms to use as primitives had not been made--even though this was to have been the first step in a formalization--so in accordance with their apparent license in the matter, Hull and Fitch chose on grounds of expository convenience to include unobservables among the undefined terms. Adapting Woodger's strategy of verbal explication to the current operationist drive in psychology, Hull gave what amounted to operational definitions, without calling them such, to the observable undefined concepts. Woodger had contributed to this latter difficulty in part by speaking of the defined concepts of a system as being explicitly defined (i.e., by biconditionals) in terms of the primitive (undefined) concepts; many of Hull's concepts could not literally qualify as defined terms under this strict view of definition and so were relegated, with or without operational definitions, to the realm of undefined terms.

Hull's curious use of axiomatic methods was thus a cumulative effect of a series of ambiguities and partial misunderstandings. There was (1) an underlying ambiguity as to the nature of axiomatics, depending on whether it worked constructively from the empirical language upward or deductively from an axiomatic calculus downward. As a consequence, there was (2) a conflation of the empirical and formal senses of the distinction between defined and undefined concepts. Despite his generally constructivist

orientation, Woodger seemed to make explicit definability the criterion of definability, in the manner of deductive axiomatics. This encouraged in Hull the misleading tendency to indiscriminately give operational (i.e., empirical) definitions to defined and undefined terms. This confusion was heightened by (3) a general lack of sophistication regarding the problem of empirical definition.

Hull's system was a relatively complex one and might have benefitted from the application of refined techniques of empirical definition. But Woodger's use of linguistic analysis, in particular his strategy of verbally explicating undefined concepts, reinforced Hull's conception of operationism, a conception made somewhat naive by its neglect of such developments as reduction sentences.

Hull and Fitch were both acquainted with Principia Mathematica and Woodger's Axiomatic Method in Biology, but had not become directly familiar with the axiomatic technique, e.g., of Carnap or Tarski, that was more typical of the Vienna approach. The logics to which they made reference in the Mathematico-Deductive Theory were those of Cohen and Nagel, John Dewey, and Lewis and Langford.⁵⁰ As we shall see in the following chapter, this fact is symptomatic of their general rejection of certain logical views held by the Vienna Circle. For our present purposes, the significance of this fact is that Hull and Fitch were not aware, and perhaps had no reason to be

aware, of the differences between the usual logical positivist approach to axiomatics and their own approach (or that of Woodger) to the systematization of theory. The task of clarifying that divergence fell to those who were intimately familiar with the Vienna Circle's account of deductive theory, namely Gustav Bergmann and Feigl's student Sigmund Koch (see next section).

As has already been shown, Hull and Woodger shared a good many views on the nature of science in general and a strong desire to systematize theory in the life sciences. If anyone could have brought the abstract machinery of modern logic to bear on the intricacies of behavior theory, Woodger and Hull seemed to be the ones to do it. Yet they failed for a variety of reasons to close that gap between the formally structured world of logic and the complex and fluid world of empirical data. This failure has been recalled by two of those who worked with Hull on his efforts at systematization. According to the logician Fitch,

Woodger tried somewhat in vain to teach logic to Hull. Woodger believed that logic, especially that of the Principia Mathematica, was essentially teachable, but he did not realize that some people, such as Hull, find it hard to assimilate. Finally Woodger gave up and returned to England somewhat disillusioned. This more or less ended the relationship between Hull and Woodger.⁵¹

The behaviorist Ellson, who also worked on the formalization of Hullian theory, has stated that "Woodger was rejected by Hull because his theorizing was only vaguely (or in

principle) connected to data."⁵² From the logical perspective, Hull was seen as insufficiently versed in logical technique; from the perspective of psychology, Woodger was seen as insufficiently in contact with the necessary empirical groundwork. Thus, there is evidence that the confusion and misunderstanding, as reviewed above, which stemmed from their joint efforts was partly an unfortunate but natural outcome of their respective limitations in the knowledge of each other's field of expertise.

In important respects, Hull's interaction with Woodger can be viewed as representative of the relationship between behaviorism and logical positivism. As we saw in the preceding section, Hull's involvement with the Unity of Science movement was based on a general confluence of views on science, but not on either agreements on detail or any serious mutual scholarly interest. Much the same might be said of Hull's involvement with Woodger. The two held strikingly similar views of science in general, of the role of deductive methods, of the inadequacies of theories in the life sciences, and of the unity of science. They shared an ideal vision of science and employed similar rhetoric to advance their ideals. But those ideals proved difficult to realize. The shared notion that deductive methods could force consensus on scientific matters could not be made practicable, at least in psychology, by two of its more able and zealous advocates. The reasons for this failure were not simple, but they began with the fact

that axiomatization was not itself an unequivocal enterprise and extended through nearly every facet of its attempted implementation. As proved to be the case with operationism, the very means of eliminating controversy turned out to be controversial in its own right.

For all of Hull's talk about the use of symbolic logic in behavior theory, the actual achievements of Hull and Fitch in that respect were rather limited. The definitions and postulates of the Mathematico-Deductive Theory were stated in symbolic logic, but the theorems and corollaries were not. Only one of the theorems was derived in symbolic logic. Concerning the ideals of logical methodology that were laid out in the introduction of the volume, Hull noted somewhat apologetically, "Despite our best efforts we have probably attained none of these ideals completely, and a number have not been approached even closely."⁵³ Hull had expended a great deal of effort in what had been conceived as a relatively simple demonstration of formal methods, and it had cost him years in the pursuit of his magnum opus.

With the rote learning book, in all its acknowledged imperfection, behind him, Hull turned to his work on the Principles of Behavior. In it, he judiciously abandoned the relatively unfruitful emphasis on formalization, although he still followed the practice of stating postulates and theorems. In the Principles and subsequent

work, Hull's intense penchant for rigor found expression instead in his use of equations and quantification. His retreat from explicit formalism was not, however, accompanied by a retreat in the rhetoric of deductive methodology. He was well aware that by this time he had become the leading proponent of deductive psychology, and fanned by the input of logical positivism, the blooming preoccupation of American psychologists with postulational technique created a lively interest in Hullian theory. In spite of its shortcomings, the Mathematico-Deductive Theory enjoyed a generally favorable reception,⁵⁴ and Hull's faith in his methodological ideals remained firm. The interest jointly spurred by Hull's ideals and the inception of logical positivism gave rise to a body of literature in the American psychology journals on logical methods. This literature is treated in the following section.

The Logic of Theory Construction

The Logic Boom in Psychology

In the psychological literature of the late thirties, the theoretical efforts of Hull and Kurt Lewin were often taken as signs of an important new turn in theoretical psychology. The apparent rigor of their formulations signified to many observers of the psychological scene that

psychology had reached a new level of maturity. One aspect of this maturity was psychology's entering a new working relationship with philosophy, a relationship viewed as like that between physics and philosophy. For those observers who had been conscious, and perhaps critical, of psychology's divorce from philosophy, the use by Hull and Lewin of logic and mathematics suggested a cautious but promising return to the mother discipline. In his presidential address to the APA in 1938, John F. Dashiell depicted psychology returning to philosophy, not for content, but for methodology and logic. One writer in the Psychological Review of 1939 asserted that "The fact that psychology was cut off from its formerly more intimate contact with logic has been particularly detrimental to the progress of psychology," but that the situation was currently being corrected. Another writer in that volume saw in "the erection of systematic constructions" the promise of a "new fusion between science and philosophy."⁵⁵

The theoretical formulations of Hull and Lewin were also celebrated as marking the end of a period of over-concern on the part of psychologists with the mere collection of facts. The imbalance which Hull and Woodger had perceived between the empirical and theoretical efforts of their sciences was also perceived by those who welcomed the use of rigorous logic in psychological theory. For example, James Grier Miller wrote that:

Techniques for obtaining extremely accurate data have been developed in many fields of psychology, but the theoretical tools with which these data are manipulated have not received commensurate development. Clumsy theoretical treatment of accurate results has often rendered insignificant the numbers in their decimal places, and made of no avail the experimental care taken to make these values accurate.⁵⁶

Miller went on to recommend the use of Principia Mathematica logic for achieving precision in theoretical formulations. Similarly, L. O. Katsoff decried the accumulation of "fact piled on fact" in the absence of theoretical systems. "The result of the revolt against systems without facts," he wrote, "seemed to be facts without systems."⁵⁷ Both Miller and Katsoff pointed to Hull's work as a major step in the direction of correcting this imbalance.

Hull's theoretical efforts had clearly struck a chord with those who longed for rigorous formulations in psychology. His work, like Lewin's, was seized upon as an auspicious indicator of future advances. Psychology was said to be "in the throes of a far-reaching methodological renaissance," and it was claimed that "the demand for 'postulates,' 'derivations,' and 'theorems' has already become a must in the 1939 psychological dialect."⁵⁸ Just how widespread this enthusiasm for theoretical rigor really was is difficult to assess in retrospect; certainly Hull had found it widespread enough to make propagandistic use of it. Now in the late thirties, Hull's approach to theorizing was beginning to be associated in the literature

with logical positivism.⁵⁹ As a consequence, the growing popularity of logical positivism, its prestige, and the zealous style with which it was advocated contributed to the enthusiasm for the Hullian approach to psychology.

But despite the various prominent endorsements of rigorous theorizing, the literature on symbolic technique in psychology exhibited remarkably little unanimity on where Hull's accomplishments stood with respect to the ideals of logical technique, or, for that matter, just what those ideals were. As Hull had done in his miniature systems of 1935 and 1937, Katsoff took geometry as the ideal model for a postulate system. But he criticized Hull, first, for failing to demonstrate the consistency of his postulates and, second, for failing to define his concepts in terms of protocol statements (which Katsoff took to be equivalent to experimental operations).⁶⁰ Miller agreed with Hull that scientific disputes could be settled through the use of deductive technique and that Principia Mathematica logic was the ideal tool for the job. But he found in Hull's miniature systems illicit intrusions of undefined terms, intrusions which he said created the false impression that Hull's "simple mechanical notions" were capable of explaining complex human behavior.⁶¹ Other critics denied that logical methods could force agreement or claimed that Hull's operational definitions were adequate but that his implicit definitions were not.⁶²

From even this brief review of the literature on logical methods in psychology, it is clear that there was an enthusiasm for those methods and broad agreement on their utility and promise; but at the same time there was controversy and misunderstanding about how the methods were to be put into practice. In these respects, the writers on deductive technique were playing out in public the same pattern of discord-within-congruence that characterized Hull's encounter with Woodger. And the reasons for the pattern were much the same: the different individuals brought to the undertaking differing perspectives and degrees of expertise; the logical tools were more refined than the rough formulations to which they were to apply; and enthusiasm for the techniques masked the difficulty of their application. Furthermore, as Sigmund Koch has noted, psychology's turn toward logical methods was "not supported by especially expert scholarship in the relevant sources."⁶³

Among the few writers in the psychological literature who were well acquainted with logic and the philosophy of science were Koch himself and the one-time Vienna Circle member Gustav Bergmann. Although they were not the first to speak of Hull in connection with logical empiricism, their sympathetic analyses of Hullian theory were carried out from the perspective of logical empiricism. As such, their analyses were instrumental in getting Hull's

name associated with logical positivism in the minds of American psychologists. As we shall see below, their works had this effect despite the fact that they showed Hull's theoretical formulations to be remote from the logical empiricist ideal.

Logical Empiricism: The Iowa Connection

During the 1930s, the University of Iowa became a sort of center for the "new view" in the philosophy of science. Herbert Feigl, who took a position there in 1931, was the first proponent of logical empiricism to arrive. The following year, he offered a seminar on philosophical problems of psychology and began to apply the precepts of logical positivism to the mind-body problem and psychological methodology.⁶⁴ In the mid-thirties, Kurt Lewin arrived at Iowa, where he wrote on problems of theory construction in the realm of psychology. Lewin had been a student of Max Wertheimer and Ernst Cassirer in Germany. Although he was far from being a logical positivist in any usual sense, he had been an active participant in Reichenbach's Society for Empirical Philosophy in Berlin, and his expositions on theory became associated with the "new view" in psychology.

Lewin's interest in applying topological concepts to psychological phenomena was responsible for Gustav Bergmann's coming to Iowa. Bergmann had received a Ph.D.

in mathematics from the University of Vienna, where he had regularly attended meetings of the Vienna Circle from 1925 to 1931. Having specialized in topology, Bergmann was considered to be an ideal candidate to assist Lewin with his theoretical endeavors. Feigl and Paul Lazarsfeld arranged for him to come to Iowa in 1939. The planned collaboration with Lewin did not work out, but Bergmann stayed on at Iowa, assuming Feigl's post when Feigl left for Minnesota. At Iowa, Bergmann met Kenneth Spence and the two collaborated actively until Spence's death in 1967.

Spence, of course, had been a student of Hull at Yale and is widely regarded as having been Hull's chief disciple. Although Spence did not take his Ph.D. under Hull, he enjoyed a close personal and intellectual comradeship with Hull dating from 1932. In Hull's seminar of that year, Spence delivered a paper on scientific explanation in which the Newtonian style of deductive theorizing was upheld as an ideal for psychology and the positivism of Karl Pearson was denigrated for its overemphasis on the purely factual side of science. Hull encouraged Spence to publish the paper (which he never did), and from then on the two were close allies in the campaign for deductive technique in psychological theory. Spence's paper made no reference to any of the logical positivists, but after gaining a position at Iowa in 1938 he found in Bergmann

a willing and able compatriot for advancing a (liberalized) version of logical empiricism in the context of psychology. With their well-known papers on theoretical psychology, Bergmann and Spence became leading figures in the coalition between behaviorism and logical empiricism.

The year 1938 saw the arrival at Iowa of yet another would-be expositor of the "new view" in psychology. Sigmund Koch came in that year to undertake graduate studies in the philosophy of psychology with Herbert Feigl. By the time he left Iowa in 1939 (to pursue a Ph.D. in psychology at Duke), Koch had worked with Spence and Lewin as well as with Feigl. Like Bergmann and Spence, Koch viewed the somewhat confused state of theoretical psychology as an inducement to apply the logical positivist prescriptions for science to psychological concerns. The crisis in psychology, he believed, "could hardly hold its own against a little clear thinking." His Master's thesis, produced under Feigl, "sought to convey to psychologists the logical positivist codification of scientific theory more accurately than had been done by the few psychologists who had already touched on this theme."⁶⁵

Hull's System: The Logical Empiricist Perspective

The theoretically inclined philosophers and psychologists who assembled at Iowa during the 1930s constituted a nucleus of interest in the application of the philosophy of science to method in psychology. Out of this group came numerous contributions to the literature on theory construction in psychology. Among these contributions, the most important, both philosophically and historically, were the published version of Koch's thesis and a paper by Bergmann and Spence entitled "Operationism and Theory in Psychology." These works which appeared side by side in the Psychological Review of 1941, became classics in the psychological literature. Koch has reported that "almost instantly" his published thesis "began to appear on the required reading lists of advanced courses and proseminars in psychology departments around the country."⁶⁶ The Bergmann and Spence paper likewise received widespread attention and was reprinted in volumes on psychological theory.⁶⁷

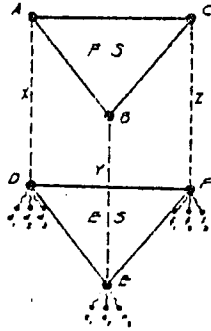
Both of these works were viewed by their authors as responses to the confusions in the literature on theory construction and as assessments of Hullian theory from the perspective of logical empiricism. As of the late thirties, the literature on operationism and the literature on postulational technique had remained in almost total

isolation from one another. Because Koch and Bergmann and Spence regarded logical empiricism as providing the key for the integration of these two movements, they also viewed their works as attempts to clarify the relationship between the empirical and logical components of scientific theory.

Koch began his article with a summary of the standard logical positivist view of theories as interpreted formal systems. Formal systems were said to consist of a set of postulates (implicit definitions), a set of explicit definitions of concepts appearing in the postulates, rules of inference, and a set of theorems derivable from the postulates and definitions by means of the inference rules. Koch characterized a formal system as a "kind of grinding machine, the function of which is to grind out the implications contained in the relationships defined by the postulate set." In accordance with the standard Vienna Circle view of theories, Koch took a formal system to be an "intricate tautology."⁶⁸ Once such a system is given an empirical interpretation, however, it becomes an empirical theory capable of generating testable predictions. The interpretation is achieved by means of coordinating definitions which relate the concepts of the formal system to empirical constructs.

Koch presented this view of theories in a simplified diagram of the sort that was commonly invoked by logical

positivists. A formal system of concepts (A, B, C) is correlated with a system of empirical constructs (D, E, F) by the coordinating definitions X, Y, and Z.



The aim is to establish an isomorphism between the empirical and formal systems so that the derived theorems of the formal system can be expected to describe observable states of affairs. It is only at this point that operationalism comes into play. The wavy lines in the diagram represent the operational definitions connecting the empirical constructs with their "observable symptoms." The multiplicity of symptoms for each construct reflects Koch's recognition of Carnap's arguments for the reduction of empirical constructs to sets of conditional reduction sentences.⁶⁹

Even in this simplified form, the logical positivist account of theories was obviously remote from scientific-- or at least psychological--practice. Koch acknowledged that his account represented "a logical reconstruction of the processes involved in theory building, rather than an historically accurate description of how theories are

brought into being."⁷⁰ But by speaking of the account in terms of the "processes involved in theory building," he reinforced the impression that the account was quite relevant to the methodology of the practicing scientist. According to Koch, there are two kinds of procedure for constructing systems of the sort described. In the first, the "interpretive" procedure, the investigator begins with an extant system, say, from mathematics, then searches for coordinating definitions for relating the system to the domain of interest. This method was said to be "the one most commonly employed in physics," but Koch identified Kurt Lewin as "the only psychologist who has ever proceeded more or less explicitly along these lines."⁷¹

The second method, referred to as the "telescopic" procedure, was not so directly related to the reconstructed account of theories. In this procedure, which was said to be "rarely used in physics,"

Elaboration of the formal and empirical aspects of the system proceed simultaneously. The two steps of development of the formal system and interpretation are, in a sense, telescoped into one. In this case the scientist usually proceeds by asserting as postulates either certain empirical laws which appear in his field or prior assumptions as the functional relationships holding between certain of the empirical constructs, or both.⁷²

As we have seen, it was just this sort of procedure that Hull had advocated at the Paris Congress when he spoke of using equations which had been fitted to experimental

data as "first-approximation" postulates in his system. Hull's later practice of giving operational definitions to the "undefined" primitive terms in his system was likewise a telescopic procedure. In fact, with reference to Koch's exposition of theory, it was telescopic in the extreme: in operationally defining the terms in his postulates, Hull was collapsing Koch's theoretical structure from the very top to the very bottom. Koch called attention to this aspect of Hull's approach and identified Hull as psychology's major practitioner of the telescopic method. As he put it, Hull "seems to identify hypothetico-deductive procedure with what we have called 'telescopic procedure' both in his general discussions of scientific theory, and in the 'miniature systems' which he has actually worked out."⁷³

Given the substantial disparity between Hull's actual methods and systems, on the one hand, and the relatively intricate account of method and theory presented by Koch, on the other, it is perhaps surprising that Koch proceeded nonetheless to analyze and evaluate Hull's systems on the basis of that account. But psychologists were looking for theory and logical positivism appeared to have the recipe for it. The process of constructing full-fledged theories in psychology had to begin somewhere, and the efforts of Hull and Lewin were the most obvious points of departure. Koch's conclusion to his assessment of Hull's

miniature systems reflected this state of affairs:

[The] limitations of Hull's theories only accentuate the vast gap between theory and what currently passes for theory in psychology. For the distance between Hull and strict theory is infinitesimal in comparison with the distance between most psychological theory and Hull. Even if the deductive procedure that Hull so energetically advocates could contribute no more to present-day psychology than the institution of a warmer intimacy between theory and fact, his enthusiasm will have been well invested.⁷⁴

Koch's thesis left two somewhat conflicting impressions, namely that Hull's systems fell far short of the ideal of theory and that they were relatively advanced and commendable efforts. The attempt to fit Hull's work into the logical positivist mold and to measure it by that standard created a presumption among psychologists and philosophers that Hull's view of science was closely associated with logical positivism. Indeed, they were closely related views; but as we shall see in the following chapter, there lay between Hull's view and that of the logical positivists deep differences which were obscured by the juxtaposition of them in the literature on theory construction.

Bergmann and Spence's paper analyzing Hullian theory proceeded along much the same lines as Koch's critique. Paralleling Koch's distinction between the interpretive and telescopic procedures, Bergmann and Spence distinguished between two senses of "hypothetico-deductive method" and "postulational technique." They wrote that a "certain

amount of confusion is apt to arise, and indeed has arisen, from an ambiguity in the meaning of these terms" and that the clarification of them is "one of the most important tasks" for a discussion of scientific method in psychology.⁷⁵

The first sense of these terms, one corresponding to Koch's interpretive procedure, is that of Hilbert's axiomatics. Hypothetico-deductive method in this sense involves the statement of postulates which implicitly define the basic terms of the system, and the subsequent interpretation of these terms by means of coordinating definitions. As Koch had done, they pointed out that such a procedure is rarely found in actual scientific practice:

. . . there are few, if any, instances of such a method being exclusively relied upon in the development of the empirical sciences. As a matter of fact, even in geometry, the postulational method was a late achievement, born out of the need for systematic organization and epistemological clarification.⁷⁶

The "mathematico-deductive" method employed by Hull, on the other hand, was said to consist of "making guesses or hypotheses as to the choice of constructs (variables) and the mathematical relationships holding between them."⁷⁷ In actual operation, Hull's methodological practice was not an unusual one, but his use of such expressions as "postulates" and "undefined terms"--expressions deriving from his interest in geometry--gave his method the appearance of being formal in the stronger sense.

The confusions and conflicts which had arisen in the psychological literature on theory construction could be attributed, according to Bergmann and Spence, to Hull's misleading use of terminology associated with geometry and the formal axiomatics of Hilbert. They wrote:

Misunderstanding might have been avoided if there had always been clear recognition of the fact that Hull's theorizing is hypothetico-deductive only in the second meaning outlined above. Hull does not begin with a set of purely formal terms, having no other meaning than that imparted to them by a set of implicit definitions, from which are then derived new terms and theorems made testable by means of co-ordinating definitions. Instead he actually begins with terms directly operationally defined. Unfortunately, he called them "undefined concepts," and thus created the erroneous impression that he started with purely formal terms which are never given the necessary co-ordinations to empirical constructs.

To this passage Bergmann and Spence added a footnote concerning Hull's definitions in the Mathematico-Deductive Theory, a work which appeared shortly before their paper.

In the recent monograph on rote learning Hull . . . uses the terms "undefined concepts" and "definitions" (defined concepts). Both undefined and defined concepts consist largely in what might be described as directly operationally defined concepts and there is no essential methodological difference between them. Apparently the idea underlying Hull's distinction between these two categories is that the undefined concepts are those most directly point-at-able, i.e., involve the shortest defining sentences. . . . By and large, however, operationists will correctly interpret Hull by substituting "operationally defined" for his expressions "undefined concepts" and "definitions."⁷⁸

Thus, Bergmann and Spence emphasized, as did Koch, that Hull had incongruously given operational definitions to his "undefined" terms. The historical circumstances in which this curious practice arose have already been described. Hull had been a late-comer to the operationist movement and had developed a peculiar view of how operationism was to fit into his deductive method. When Woodger stated that the meanings of primitive terms must be made clear to anyone using a given system, Hull took operationism to be the appropriate method for doing so. As a result, Hull placed operational definitions, which the logical positivists understood as the final link between a theoretical structure and its empirical grounding, at the top of the theoretical structure. The loftiest parts of the presumed structure--the implicit definitions--were in Hull's work completely conflated with the parts closest to the empirical bedrock--the operational definitions. Everything in between, the empirical constructs and coordinating definitions, seemed to vanish in the process.

In attempting to fit Hull's systems into the logical positivist mold, Koch had spoken of this conflation as the "telescopic" method; the upper and lower levels in the theoretical structure were said to be telescoped into one. Under the influence of the account of explanatory levels which Feigl was developing at the time, Koch wanted

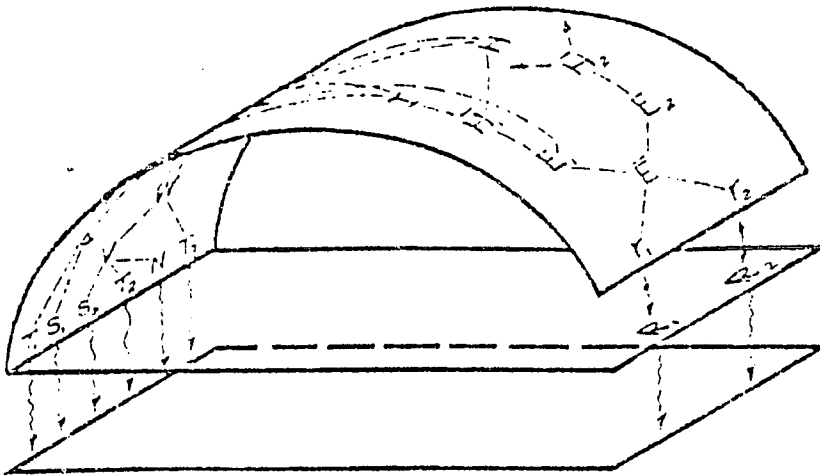
to maintain the level of postulate as distinct from the level of empirical law. But this was a distinction not to be found in Hull's practice, despite Hull's claims to be seeking a Newtonian hierarchy of theory and law.

Recognizing that Hull in fact had no such hierarchy, Bergmann and Spence developed a new means of depicting the structure of theories, one better suited than Koch's for conveying the actual structure of Hull's systems.

In the pictorial scheme presented by Bergmann and Spence, operational definitions (wavy lines) are shown occupying both the final and initial stages of the theoretical structure. The edifice of theory, rather than rising continuously upward from an observation base, arches over the empirical base, bending downward to meet it at both ends. The left-most letters are the operationally defined variables of antecedent conditions (stimulus intensities, motivation levels, and such). From these initial variables are defined the various intervening variables, which are related to each other by chains of hypothesized functions (dotted lines). The final intervening constructs (r_1, r_2) are then compared by means of experiments to the operationally defined response variables R_1 and R_2 (amplitude and latency of response, for instance). Bergmann and Spence characterized the empirical test of a theory as follows:

These two sets of formally different terms (r's and R's) are then identified and the success of the construction depends upon whether this identification is borne out by the experimental data. If so, the gap at the right end of the bridge is closed and the desired formulation of the empirical law has been attained.

Whereas Koch had responded to Hull's peculiar use of operational definitions by referring to the telescoping of layers in the theoretical hierarchy, Bergmann and Spence responded by turning the traditional



vertical structure of theory into an explicitly horizontal structure. This horizontal structure was still said to represent "the hierarchic order of the terms involved," but it was a hierarchy of intervening variables running parallel to the empirical base and not one of constructs rising from the "soil" of observation upward to general theoretical principles.

If Bergmann and Spence's scheme was truer to Hull's practice, it was certainly remote from the standard logical positivist view of the structure of theory. By beginning with temporally antecedent variables and ending with consequent variables, the new schema invited speculations about the existential status of intervening variables. Those psychologists who took them to be temporally and physically existing states of the organism began to speak of them as hypothetical constructs. These constructs in turn became conflated in the psychological literature with the high-level theoretical constructs which stood at the top of the traditional schema. In the ensuing discussion of theory construction in psychology the new schema became the standard view of the structure of theories, but the important differences between this new view and the logical positivist's standard view often went unrecognized.

The general view that psychological systems consist of sets of intervening variables and specifications of the functions relating them to each other and to independent and dependent variables was of course originally articulated by Tolman. Bergmann and Spence acknowledged this fact and looked with optimism upon the growing consensus on the suitability of the intervening variable paradigm for psychology. Tolman's description of the paradigm was, they said, "fully in line, indeed identical, with the picture

of Hull's actual procedure as outlined by our preceding analysis." In concluding their paper, they stated that "Such essential convergence between two theoretical viewpoints which are often regarded as being far apart strongly suggests that essential agreement on this level of general methodology is about to be reached in psychology."⁸⁰ As was the case with Koch's thesis, the Bergmann and Spence paper showed Hull's theoretical efforts to be remote from the logical positivist standards for deductive theory and yet it warmly endorsed those efforts. But just by virtue of having provided a discussion of Hull's theories in the context of logical empiricism (and moreover by a former member of the Vienna Circle), the paper, like Koch's, had the net effect of reinforcing the association between Hull and the logical positivists in the minds of the psychological public.

Hull and the New Methodologists

As the preceding sections indicate, the burgeoning interest at Iowa and elsewhere in the application of logical technique to psychological theory drew considerable attention to Hullian theory and generated a climate of optimism in which Hull's work came to be associated with that of the logical positivists. Hull was, of course, not himself a logician, but he was at the peak of his interest in deductive methods around 1940, and he naturally

followed these developments with interest. Logical positivism was by the late 1930s enjoying great popularity in America, and the difficulties which had faced him in writing the Mathematico-Deductive Theory must have convinced him that the expert advice of its adherents was well worth seeking out.

Because of his close contact with Spence, Hull was privy to at least some of the developments at Iowa before they appeared in print. Koch has reported that

Spence . . . was in my humble office virtually every afternoon during the writing of the thesis in quest of discussion and bibliographic advice. He advised me to publish the masterpiece at the earliest possible date, seeming actually to believe that the fate of psychology would be deeply affected by the event.

After finishing his thesis under Feigl, Koch transferred to Duke at just the time that Hull was rushing to complete the problem-ridden manuscript of the Mathematico-Deductive Theory. Koch's account continues:

Immediately upon arrival at Durham (fall, 1939), I learned that Spence had been trying to reach me (I had spent a month or so in New York). Spence had shown my thesis to Hull at some point after my departure; Hull wanted the author at Yale; neither knew the author's whereabouts. I contacted Hull and was advised to stand by in Durham while the difficult problem of finding a stipend at that late date was addressed. For a few weeks I did not unpack, but the upshot was negative.⁸¹

Hull was thus apparently eager to have on hand a methodologist, someone with whom he could consult on the technical aspects of definition and theory construction.

By the summer of 1939, Hull had already had the benefit of consulting with Gustav Bergmann on such matters. After arriving at Iowa in April 1939, Bergmann quickly began his association with Spence, who arranged for Bergmann to visit the IHR. Bergmann has recalled: "During the summer of 1939 I was for two months an (unofficial) guest at Hull's Institute at Yale and found him as eager a questioner and listener in all matters philosophical and methodological as Spence remained all his life."⁸² During this visit, Bergmann undoubtedly conveyed to Hull both the clarifications and endorsement of his theoretical efforts from the logical empiricist perspective, probably much as they appeared in the Bergmann and Spence paper the following year.

The influence of Koch's thesis and especially of Bergmann's actual presence at Yale can be discerned in the Mathematico-Deductive Theory. Under the exigencies of an impending decision on the renewal of Rockefeller funding, Hull sent the book to press in December 1939 despite its flaws which had been made apparent by the critiques of Bergmann and Koch. These were the problems to which Hull called attention in the conclusion of the work. Among the "two or three major defects of the present system," Hull noted that

. . . several of the undefined notions (logical signs), including all those representing unobservables, should have appeared among the defined terms. Presumably the next attempt at a formalization of rote-leaning theory should make this correction one of its earliest objectives. . . . Here, evidently, is a place where the principles of "operationism," with their very real scientific virtues should be applied.⁸³

As has already been described, Hull's placement of terms that had actually been given definitions among the "undefined" terms and his failure to adequately integrate operationism into his approach were shortcomings which Bergmann and Spence pointed out in print shortly thereafter.

One other figure who was associated with logical empiricism and who was involved in methodological discussions with Hull was the Norwegian philosopher Arne Naess. In 1938-39, Naess spent a year in Berkeley and New Haven studying the groups around Tolman and Hull and the competition between them from a social psychological perspective. Naess had been a participant in the Vienna Circle, as Hull recognized, and was engaged by Hull in discussions of deductive methods. Naess's reactions to Hull's methodological notions were much the same as those of Koch and Bergmann and Spence: he was generally encouraging of Hull's attempts at formal rigor and yet critical of Hull's equivocal use of the language of formalization. After having a series of discussions of methodology with Hull in the spring of 1938, Naess wrote to Hull summarizing his impressions. The letter stated:

It is quite common to praise the hypothetico-deductive method as the only one which is strictly scientific. You are the only one who makes the bold jump from theory to practice, and whatever the results of this jump may be, they will certainly be of first-rate importance to methodology.

At the same time, Naess pointed out the problems involved in Hull's confusing use of the terminology of postulational technique and his failure to distinguish full-blown formalization from ordinary hypothetico-deductive method.

Misunderstandings are to a large extent due to incompleteness and ambiguity of your methodological remarks themselves. You underestimate the difficulties involved in an unambiguous description of methods. There is an enormous literature on scientific method and every word used in a brief outline of a method must be expected to cause large aggregates of associations to occur to the reader familiar with that literature. . . .

If you use the word "formalization" in the wide sense of "making explicit postulates and inferences connecting postulates, definitions, theorems, corollaries, and predictions" you could distinguish between the high grade formalizations requiring symbolic logic and inferior grade formalizations possible without this tool.⁸⁴

This reaction to Hull's methodological pronouncements was, as we have seen, later reinforced by other writers. Taken collectively, these critical responses seem not to have shaken Hull's belief in the basic soundness of his approach, but they were probably instrumental in encouraging Hull's retreat away from explicit formalization in the years following 1940.

This section has briefly reviewed Hull's interest during the late thirties in the new methodology that was

inspired by logical positivism. While he was struggling with the formalization of learning theory which appeared in his system of 1940, he actively sought advice from the proponents of logical positivism. But as has already been pointed out, Hull's interest in logical positivism focused on the logical; the advice he sought concerned primarily deductive technique rather than empirical definition. Hull was simply not a positivist of the operationist bent that was common among other psychologists. Hull's views on the limited value of operationism are treated in the following section.

Operationism, Positivism, and Quantification

Operationism and Positivism

Hull was clearly an empiricist and he identified himself as such. But the empiricism with which Hull allied himself was the philosophical empiricism of the British associationists, and it was their explanatory theoretical principles rather than their views of the experiential foundations of knowledge that attracted his favor. Hull was not at all an adherent of the more inductive or positivistic brands of empiricism, such as James's radical empiricism or Mach's experiential positivism. In fact, Hull took pleasure in deriding the descriptive approach to science, an approach which he felt

could only add "one more potato to the bin" and thereby contribute to the tonnage of data but not to its systematization.⁸⁵

The operationist movement in psychology was viewed by Hull in much the same way. Hull did eventually embrace operationism (albeit in a heterodox way); but unlike the majority of operational psychologists who viewed operationism as a means of infusing concepts with meaning from the empirical ground up, Hull saw it as a limited tool in light of the inherent capacity of theoretical systems to convey such meaning and status from the top down. According to Hull, concepts are shown to be scientific when they are incorporated into the postulates of a deductive system.⁸⁶

Hull's outlook on these matters was most clearly expressed in a letter he wrote to Tolman in 1936, responding to Tolman's newly formulated operational behaviorism. In the letter, he referred to Stevens's two articles which had in the previous year triggered the infatuation of psychologists with operationism. He wrote:

It has occurred to me as possible that we may, through a false analogy, be badly misled by Bridgman's approach to the current problems of theoretical physics. Bridgman is laying about him in the presence of an elaborately systematized science. To a very large extent the appropriateness of the categories has already been determined by the technique of physical theory. In psychology, however, this systematization has not yet occurred and therefore we have little indication as to what the appropriate

categories will turn out to be. From my own conception of positivism, I don't see how it can go very far in determining the correctness or incorrectness of categories. It has, of course, the immense virtue of squeezing mythology out of terms, and so cannot possibly go wrong. The only thing which I would be inclined to urge is that operationism should be combined with parallel attempts at theoretical systematization. This, it seems to me, is the chief weakness of the attempts of Stevens. It is not that Stevens makes error; it is rather, that if his positivism were combined with a vigorous parallel attempt at theoretical systematization his work might be very much more fruitful. To say the same thing in brief: without a vigorous parallel attempt at theoretical systematization the positivist may waste immense amounts of effort trying to refine a concept which may turn out to have no deductive potentialities.⁸⁷

Thus, the mark of a worthy scientific concept for Hull was its "deductive potentialities," a feature which took precedence over the concept's capacity for being observable or operationalizable. In Hull's methodological scheme, operationism was simply not a very important tool, although its value as a weapon against subjectivity and metaphysics was recognized. As Hull later noted to himself, "It appears that this operationism business is what gives the final coup de grace to the subjective by driving it from its last hiding place the ambiguity of the private."⁸⁸

There is evidence that Hull was a relative latecomer to operationism and that he never fully comprehended the role assigned to operationism by the logical positivist scheme of science. The influence of operationist thought on him was first apparent in the published version of his

APA presidential address of 1937, but operationism was not mentioned by name until the Mathematico-Deductive Theory of 1940. Even then, as we have seen, the operational definitions he gave were not recognized as such by him. As Koch put it in 1941, "Hull nowhere devotes sufficient attention to the problem of how the empirical constructs appearing in the postulate set are introduced (i.e., the question of operational definition)." ⁸⁹ This judgment still stood in 1954 when Koch made his final assessment of the Hullian system. ⁹⁰ It is instructive to note in this context that Hull's discussion of operationism in the Principles of Behavior was confined to a single paragraph appended to the second chapter. In that passage, Hull expressed misgivings over the misuse of operationism by psychologists, and in a curious error that epitomizes Hull's inattention to operationism, he claimed that Bridgman's Logic of Modern Physics was written in 1938. ⁹¹ The general lack of interest shown by Hull in operationism is perhaps puzzling if Hull is viewed as having striven to emulate the logical positivist model of science; but as will be argued in the following chapter, Hull was actually operating under a psychological model of science, a model which differed importantly from the logical positivist model and which gave no significant role to operationism.

If Hull was not an operationist in any important sense, neither was he a positivist in any usual sense. He opposed the descriptive positivism of Mach and Karl Pearson. His materialist metaphysics was incompatible with the ametaphysical positivism of Carnap. In using his materialism to argue against idealist metaphysics, Hull was perhaps close to Neurath's physicalistic positivism. But Neurath's physicalism was a version of descriptive positivism. Neurath conceived of science as a set of statements of physicalistic correlations. For Neurath, the "world has no depth," whereas for Hull the world was a rich structure of material hierarchies. Hull's radical scientism, his advocacy of a science-based ethics, his views on science as a vehicle of social integration, and his linking of social progress with scientific progress were all views which brought him close to the positivism of Auguste Comte. But Comtean positivists, having worn their metaphysical commitments openly, were an embarrassment to twentieth century positivists, and many of their philosophical views were no longer taken seriously.⁹² All in all, Hull could not fairly be deemed a positivist in any sense current in his time.

As was pointed out in Chapter 2, the logical positivists encountered serious difficulties in combining their empiricism, which for them involved the testability of scientific claims, with their formalism, i.e., the demand

that theories be cast in a definite logical structure. This problem of reconciling the formal and testable aspects of theory was also a continuing vexation to Hull. His labored efforts to define the intervening variables of the 1940 system in such a way as to ensure their observability were described above in some detail. This problem was one that was made all the more difficult by Hull's failure to give careful consideration to issues of empirical definition. By and large, Hull's theoretical systems were aimed more at generality and systematicity than at empirical specificity and descriptive adequacy. However, Hull was by no means neglectful of the desirability of clearly connecting his postulates with empirical phenomena. The sort of empirical contact that he sought, especially in his later formulations, was that of measurement. But, as Hull realized, measurement required a scheme for quantifying behavior, and it was such a scheme that was the goal of his activities in the years after 1940. Hull's attempts to quantify behavior are the topic of the following section.

The Quantification of Behavior

As we have seen, Hull's theoretical efforts can be divided into three phases. Chapter 6 described the first phase, in which deductions were laid out informally and in close relation to Hull's mechanical conception of

behavior. In the second phase, discussed above, Hull's theorizing became more formal, involving geometrical method and later the techniques of symbolic logic. In the third phase, the emphasis was on the mathematical and quantitative aspects of theory. Hull had aimed for a quantitative system since early in his career, but this intent became prominent only in the later thirties after Tolman had introduced and popularized the intervening variable paradigm. It was in this period that Hull hired mathematicians and undertook the extensive use of equations that dominated his later work.

As we have already seen, the problem of giving formal definitions to intervening variables proved to be a difficult one for Hull. But given his mechanistic conception of theory, intervening variables were inherently well-suited for adoption into the Hullian scheme. Hull, it will be recalled, believed that the structure of theory should mirror the structure of the mechanistic behavior in question. Just as the internal gears of a behavior-generating machine would intervene between the inputs which set it in motion and its behavioral output, so would the intervening variables of a prediction-generating conceptual machine mediate between its antecedent-variables and its predicted-behavior output. The fact that intervening variables were unobservable was, in principle at least, no more a source of consternation

for Hull than the fact that a machine's inner workings are normally hidden from view. Hull's widely known maxim that intervening variables should be securely linked to antecedent and consequence observables in effect mimicked the determinate linkage of machine parts via gears and levers. As one commentator on Hull has astutely observed, "it is possible to appreciate that if we treat the chains of intervening variables that form part of the mature theory as parts of a mechanism it is possible to advance a completely deterministic picture of behavior."⁹³ The linkage of choice for Hull was of course the mathematical equation.

Although intervening variables posed no philosophical difficulty for Hull, the quantification which accompanied their use proved to be a formidable obstacle in practice. Although he had early voiced the desirability of quantification, Hull's early machine-modeling efforts engaged in it, if at all, only to elucidate how adaptive mechanisms could potentially be simulated.⁹⁴ The numbers were merely illustrative, representing hypothetical response tendencies, and there was no attempt at, or even point in, assigning specific values to actual behavior. Hull's informal derivations sufficed to show how adaptations could be exhibited by inorganic devices and occasionally even indicated the general form of some behavioral function. In this early work, he was guided by qualitative

results of experiments (performed mainly by others) but not by actual response measures, which were of scant relevance to issues of abstract design.⁹⁵ Had Hull been satisfied to remain at this level of abstraction, he might well be recognized today as a founder of modern cybernetics. But Hull was not satisfied with such an approach, and as a consequence his theories suffered from a tension between his deductive mechanistic thrust and the demand for empirical specificity and predictive precision.

This tension was exacerbated, on the one hand, by Hull's opposition to narrow empiricism and, on the other hand, by his skepticism about the booming operational empiricism which surrounded him from the mid-thirties on. The essence of Hull's scientific style was to invent and systematize, to contrive and reason through. Tying his ratiocinations to specific detailed response measures constituted for him a difficult challenge, the pursuit of which turned out to be unnecessary, ill-advised, and at odds with his earlier and more fruitful mechanistic approach. But the promise of rigor on which Hull had risen to prominence could not be paid off entirely in programmatic deductive theorizing, no matter how elaborately it may have been axiomatized. As conceived in the late 1930s, scientific rigor involved not only formal methods but also operational definition, measurement, and quantification. Hull acknowledged the need for this kind of

rigor, but he subordinated it to the deductive rigor which was his forte. Had he ever been tempted to overlook the need for rigor on the empirical side, the bedrock empiricist wing of the movement for the "new methodology" was there to remind him of it. Even worse, from Hull's point of view, the empiricist wing was threatening to steal away the banner of objectivity in the name of operationism.

Spurred by these circumstances, Hull forged ahead with attempts at quantification. Principles of Behavior was richly infused with the trappings of quantification (definitions of variables, units of hypothetical scales for intervening variables, etc), but as Koch has amply shown,⁹⁶ these efforts fell far short of genuine quantification. One difficulty among others was that the theoretical variables--independent and dependent as well as intervening--remained unattached to measures of actual behavior. Hull was aware of the sketchiness of his account, but his confessions to that effect were expediently submerged in the rhetoric of rigor, power, and generality.⁹⁷ In the Principles, Hull had devoted a chapter to behavioral variability or oscillation, which he took to be a universal characteristic of molar behavior. Whereas other psychologists interpreted such variability as reason for giving up deterministic laws as the goal of psychology, Hull emphasized the lawful nature of

variability by construing it as an inhibitory tendency with normally distributed strengths. The assumption of normality in behavioral oscillations opened the way for applying statistical methods to the problem of response scaling, specifically for adapting L. L. Thurstone's psychophysical method of paired comparisons to the task. In 1945, at the age of sixty, Hull arrived at what he felt was a breakthrough:

. . . I seem at last to have devised an experimental technique and a parallel statistical technique whereby I shall probably be able to penetrate the hitherto inaccessible rings of constants of my system in such a way as to measure s_{HR} , D , s_{ER} , s_{OR} , and so on. This seems at present to be a really major achievement.⁹⁸

The remainder of Hull's career was devoted to seeking a reconciliation of his mensurational technique with his theoretical postulates. Essentials of Behavior (1949) was a major effort in that direction, but not a very successful one. As Koch has argued in detail, not only did the method of measurement rest on a grossly inadequate data base and several unjustified theoretical assumptions, but the theory itself suffered from being adapted to it. In Koch's words, the changes in the theory from the 1943 version were "enforced by the relentless demands of an infeasible and prematurely over-elaborated quantificational methodology."⁹⁹

In the transition from the 1943 version of Hull's

system to the 1949 version, there was, as Koch has noted, an increased "localism" in the theory language.¹⁰⁰ Systematic variables presumed to represent general processes in the determination of behavior were, in the new version, found to have exceedingly narrow empirical definitions. But these changes were not accompanied by any explicit retraction of Hull's stated aim of ascertaining general principles of behavior. To emphasize the extent of the new localism and its incompatibility with any pretense of generality, Koch offered the example of Hull's definition of the wat, the unit of the systematic dependent variable (reaction potential):

The wat is the mean standard deviation of the momentary reaction potential (sE_R) of standard albino rats, 90 days of age, learning a simple manipulative act requiring a 10 gram pressure by 24-hour distributed trials under 23 hours' hunger, water available, with reward in the form of 2.5-gram pellet of the usual dry dog food, the mean being taken from all the reinforced trials producing the habit strength from .75 to .85 habs inclusive.¹⁰¹

By way of contrast, the 1943 system specified the wat only as a unit on a 100-point scale of reaction potential ranging from no response tendency to an asymptotic limit. Unlike the later quantification, which was unjustifiably tied to the specifics of both organism and apparatus, the 1943 version modestly employed an arbitrary scale on a hypothetical response dimension. In this modest form, it served Hull well as a conceptual device for developing

and expressing various consequences (e.g., general functional forms) of his postulates.¹⁰²

Significantly, Hull's last book, which appeared just months after his death in 1952, represented a retreat from his previous excesses. First, he showed a realistic skepticism about the quantificational and formal aspects of his earlier systems. The efforts at quantifying behavior were admitted to be a "small and tentative beginning." Speaking of the type of axiomatization attempted in the rote learning monograph, Hull confessed, "It is probably too early to do this on a large scale."¹⁰³ Second, he gave up much of the pretense of generality found in his previous work, returning instead to the set of limited problem areas which had been addressed in his earliest papers.¹⁰⁴ Research in the Hullian tradition continues today at this more restricted level of scope and formality. As one prominent researcher in this tradition has put it, "much of the current neo-Hullian thinking is closer to the kind of theorizing contained in the writings of Hull, Spence, Miller, and others in the thirties than it is to the 1943 (Principles of Behavior) Hull and its revisions."¹⁰⁵

Hull's early and deep interests in mechanical and logical hierarchies were closely linked in his vision of psychology, but he never found a clear expression of their intimate relationship. His vision was blunted and diverted by an undue stress on methodological formality

and by an excessive concern for empirical specificity. Hull has commonly been criticized for an overemphasis on theory, but, after all, his genius lay in deriving the consequences of his mechanistic insights. His sounder impulses were admittedly distorted by his theoretical excesses, but certainly no more so than by his empirical excesses. Unable to resolve the tension between broad theory and narrow fact, Hull was never able, despite some eleventh-hour efforts, to get his program back on its original track, that more modest middle road which had been laid out in his early papers.

Logical empiricism contributed to this derailment by reinforcing both the formal and empirical tendencies in Hull. The former influence was, as we have seen, exerted directly through J. H. Woodger as well as indirectly through the postulational movement in psychology. As for the later influence, logical positivism encouraged psychologists to operationalize concepts and undertake exact measurement. It helped create the climate in which these practices were often demanded by psychologists and in which Hullian theory with its quantificational machinery flourished. But this influence on Hull was mainly indirect. Unlike the measurement theory of S. S. Stevens, which was worked out largely in connection with the Unity of Science movement,¹⁰⁶ the measurement technique of Hull was devised on his own from the earlier methods of Thurstone. The

popularity of Hull's formal and empirical methods certainly benefitted from the American reception of logical empiricism, but the net effect of this influence was to push the extension of those methods to untenable extremes.

Conclusion

This chapter has reviewed Hull's interactions with various of the figures associated with logical positivism. The involvement of Hull with logical positivism took the form of his actual participation in the Unity of Science movement, direct interactions with certain adherents of logical positivism, and indirect influences mediated by the literature on theory construction in psychology.

Hull's activities in the Unity of Science movement took place during a period in which logical positivists and behaviorists had much to offer each other in terms of mutual support for their respective causes. The behaviorist and logical positivist movements were based on similar presuppositions concerning the nature of science, the unification of science, and the general inculcation of the "scientific attitude." The kinship between them was clearly recognized by their proponents, who actively sought to strengthen the bonds between them. As a result, Hull was drawn into the Unity of Science movement. He served on its committees, spoke at two of its congresses, wrote about the integration of science, and hosted visits from

its members. Hull found support for his views on logical methods, and the logical positivists found support for the extension of their doctrine of physicalism to psychology and the social sciences. But for the most part Hull's association with the Unity of Science movement remained at the level of jargon, rhetoric, and alliance against common perceived enemies. Hull's interest in the Unity of Science movement appears not to have extended to an interest in the more substantive issues of logical positivist philosophy. He referred to the philosophical literature only very rarely, and when he did so he showed little understanding of its content and no appreciation of the technical subtleties and difficulties with which it was beset.

Hull's closest and most sustained contact with logical positivist ideas occurred during his collaboration with Woodger on the formalization of rote-learning theory. To a remarkable degree, Hull and Woodger shared common views on the co-operative aspect of science, an enthusiasm for the deductive systematization of theory, and an optimism about the benefits of systematizing even the relatively immature theories of the biological and psychological sciences. But despite their enthusiasm and concerted efforts, their collaboration did not result in a fruitful formalization of Hullian theory. Woodger's visit apparently failed even to convey to Hull a clear

understanding of the more technical details of formalization, Hull also seemed to remain unaware of the difference between commonplace hypothetico-deductive method and full-fledged formalization.

Hull's advocacy of deductive systematization and his apparent success with his "miniature systems" of the thirties helped to excite a literature in the psychology journals on logical technique. On the whole, this literature contained varied responses to Hull's theoretical efforts. Of the writers who responded to Hull's systems, those who were closest to the Vienna Circle philosophy--Koch and Bergmann and Spence--produced the most influential assessments. Their conclusions were of a similar thrust: Hull's systems fell far short of the logical positivist ideal for theory, but Hull's attempts were on the right track and were thus to be commended. These conclusions were also conveyed informally to Hull by Arne Naess, another philosopher closely associated with the Vienna Circle. These critiques were probably influential in Hull's retreat after 1940 away from explicit formalization, but they had the effect of engendering a widely held perception that Hull was closely linked with the logical positivist philosophy. The enthusiasm for Hull's deductive approach to psychology, coming as it did despite the critical evaluations of his actual accomplishments, suggests that the inception of logical

positivism in America combined with the quasi-formal techniques of Hull and others to produce a strong demand and respect for what was perceived as rigorous objectivism in psychology.

Hull had helped to create an exaggerated demand for rigor and unrealistic expectations about the prospects for a rigorous approach to psychology. Once the demand was established, Hull became as much its pawn as its perpetrator. One consequence of this strong demand was the subsequent attempt by him to quantify his system of adaptive behavior. The substantial disparity between Hull's claims for his quantified theory and its actual limited utility and generality soon became apparent. Largely because of Koch's persistent efforts to point out this disparity (and other weaknesses in the system), Hullian theory quickly fell during the fifties from its previous stature. In the long run, Hull thus became a victim of the very climate of rigor which he and the logical positivists had jointly inspired. This climate appears to have dissuaded Hull from pursuing his sounder insights both by encouraging inappropriate strategies for elaborating his theoretical scheme and by discouraging the more appropriate strategies. On the one hand, it encouraged him first to formalize and then to quantify his systems far beyond the extent that was warranted, given the modest degree of their development. On the other

hand, it diverted Hull's attention away from those strategies which grew out of, and were inherently better suited to, his conception of behavior based on the organism-machine analogy. As will be argued in the following chapter, Hull's mechanical metaphor was a potent heuristic device from which he drew his views of organismic behavior and even his views of scientific knowledge. But the dominant logical positivist model of science assigned no significant role to heuristic, analogy, and metaphor; as a result, Hull downplayed the role of his mechanical metaphor in his thought and ceased his explicit pursuit of the implications of that metaphor.

Hull's involvement with logical positivism thus exhibited a more complex pattern than would be expected if Hull were simply identified as an adherent of logical positivism. At the level of rhetoric and general views of science, Hull and the logical positivists were clearly allies. Within that convergence over broad issues, however, there was little mutual understanding or genuine cooperation on substantive matters. Despite his interactions with several notable logical empiricists, Hull showed no signs of following even the more basic developments in the evolution of logical empiricist thought. All the same, Hull came to be identified with logical positivism. He did nothing to dispel this widespread impression, and assessments of his work were regularly

made from the perspective of logical positivism. Hull chose not to emphasize his differences with the logical positivists, but they were deep differences and his views on science and psychology can not be adequately understood without recognizing them. It will be argued in the following chapter that Hull's "philosophy of science" was in reality a "psychology of science"-- a fact which is crucial for understanding the extent and limitations of his relationship to logical positivism.

Notes for Chapter 7

1. Charles W. Morris to Otto Neurath, 10 August 1936, Unity of Science Collection, Regenstein Library, University of Chicago, Chicago, Ill. In subsequent references to this collection, only the title of the collection will be given.
2. Charles W. Morris to Otto Neurath, 25 May 1936; Charles W. Morris to Otto Neurath, 26 January 1936, Unity of Science Collection.
3. Joergen Joergenson, "The Development of Logical Empiricism," in Foundations of the Unity of Science, vol. 2, ed. Otto Neurath, Rudolf Carnap, and Charles Morris (Chicago: University of Chicago Press, 1970), p. 892.
4. Clark L. Hull and O. Hobart Mowrer, "Hull's Psychological Seminars, 1936-38" (Mimeographed), Sterling Memorial Library, Yale University, New Haven, Conn., p. 103; Clark L. Hull, "The Goal Gradient Hypothesis Applied to Some 'Force-Field' Problems in the Behavior of Young Children," Psychological Review 45 (1938): 271-299, on p. 298; Clark L. Hull, "Psychology of the Scientist: IV. Passages from the 'Idea Books' of Clark L. Hull," Perceptual and Motor Skills 15 (1962): 807-882, on pp. 865, 866, 868; Clark L. Hull, "Logical Positivism as a Constructive Methodology in the Social Sciences," Einheitswissenschaft 6 (1938): 35-38, on p. 35. Significantly, only the last of these references contained any mention of the Vienna Circle philosophy, and none of them included references to specific logical empiricists. These omissions appear to be attributable to the fact that Hull independently developed the positions he held in common with the logical positivists and thus felt no need to cite their work.
5. Otto Neurath, Memorandum to Rudolf Carnap and others, 11 August 1937; Otto Neurath to Charles W. Morris, 12 August 1937; Otto Neurath to Charles W. Morris, 9 September 1937, Unity of Science Collection.
6. Otto Neurath to Charles W. Morris, 9 September 1937, Unity of Science Collection. Neurath had sought Egon Brunswik's advice on Hull's suitability for membership in the Advisory Committee and had received from Brunswik a highly favorable reply. Whether Hull's committee membership involved anything more than just lending his name to the movement is doubtful.

7. Clark L. Hull, "Constructive Methodology," pp. 35-36.
8. Ibid., p. 36.
9. As will be discussed below, Hull's apparent conflation of the theoretical and empirical levels of theory was cause for critical comment in the psychological literature.
10. Hull, "Constructive Methodology," p. 37.
11. Clark L. Hull to Charles W. Morris, 2 August 1941, Unity of Science Collection.
12. Clark L. Hull to Charles W. Morris, 16 August 1941, Unity of Science Collection; Clark L. Hull, "The Problem of Intervening Variables in Molar Behavior Theory," Psychological Review 50 (1943): 273-291.
13. Hull, "Intervening Variables," p. 273.
14. Brunswik's symposium address appeared as Egon Brunswik, "Organismic Achievement and Environmental Probability," Psychological Review 50 (1943): 255-272.
15. Although it had not been explicitly advanced by Brunswik, Hull addressed the argument that the laws of molar psychology must remain inexact because they rest on as yet unknown laws of neuroanatomy and physiology. While admitting the force of this point, Hull argued that it could be circumvented by pooling data, thereby averaging out variability produced by the operation of unknown laws, and then formulating reasonably precise laws of central tendencies. The results, according to Hull, would be exactly analogous to the laws of thermodynamics in gases. Oddly enough, Hull even introduced into his system an "oscillation factor" in order to model the variability which "submolar" random processes impart to behavior. See Clark L. Hull, Principles of Behavior: An Introduction to Behavior Theory (New York: D. Appleton-Century, 1943), Chapter 17.
16. Hull, "Intervening Variables," p. 276.
17. Ibid., p. 280. Brunswik has commented on Hull's misrepresentation of his (and Carnap's and Tolman's) views on intervening variables. See Egon Brunswik, "The Conceptual Framework of Psychology," in Foundations of the Unity of Science, vol. 1, ed. Otto Neurath, Rudolf Carnap, and Charles Morris (Chicago: University of Chicago Press, 1970), p. 740.

18. Hull, "Intervening Variables," p. 278.
19. Rudolf Carnap, Philosophy and Logical Syntax (London: Kegan Paul, Trench, and Trubner, 1935), p. 90.
20. Brunswik's reaction to Hull's distortion of Carnap's position was mentioned in note 17 above. Having been prevented from attending the Chicago Congress by a back injury, Carnap was unable to publicly respond to Hull's characterization of his views (Rudolf Carnap to Charles W. Morris, 24 August 1941, Unity of Science Collection).
21. J. H. Woodger, Biological Principles (London: Kegan Paul, Trench, 1929).
22. J. H. Woodger, The Axiomatic Method in Biology (Cambridge: Cambridge University Press, 1937), p. x.
23. Clark L. Hull, Carl I. Hovland, Robert T. Ross, Marshall Hall, Donald T. Perkins, and Frederic B. Fitch, Mathematico-Deductive Theory of Rote Learning: A Study in Scientific Methodology (New Haven, Conn.: Yale University Press, 1940), p. xi.
24. Ibid., p. xi. Hull's encounter with Woodger is briefly described in Clark L. Hull, "Clark L. Hull," in A History of Psychology in Autobiography, vol. 4, ed. Edwin G. Boring, Herbert S. Langfeld, Heinz Werner, and Robert M. Yerkes (Worcester, Mass.: Clark University Press, 1952), pp. 143-162, on p. 157.
25. Hull et al., Mathematico-Deductive Theory, p. 10.
26. J. H. Woodger, "The Technique of Theory Construction," in Foundations of the Unity of Science, vol. 2, ed. Otto Neurath, Rudolf Carnap, and Charles W. Morris (Chicago: University of Chicago Press, 1939), p. 521.
27. Ibid., pp. 453-454.
28. Woodger, Axiomatic Method, p. vii.
29. Woodger, "Theory Construction," pp. 529-530.

30. Of course, Hull and Woodger did not necessarily develop their views completely independently of one another. It is clear, however, that the views of each were well established in general outline prior to their meeting in 1937.

31. Fellowship Cards, Rockefeller Archive Center, North Tarrytown, New York.

32. Hull, "Clark L. Hull," p. 157.

33. An abstract of this presentation appeared as J. H. Woodger, "The Formalization of a Psychological Theory," Journal of Unified Science (Erkenntnis) 7 (1937/38): 195-198.

34. Woodger, Axiomatic Method, p. 14.

35. The distinction between constructive and deductive axiomatics, originally due to Hans Reichenbach, was applied to Woodger's work by Martin Strauss, "Review of Woodger's 'The Axiomatic Method in Biology' and 'The Technique of Theory Construction'," Journal of Unified Science (Erkenntnis) 8 (1939/40): 372-377.

36. Hull, "Idea Book XVIII," p. 282. Clark L. Hull Papers, Sterling Memorial Library, Yale University, New Haven, Conn. Subsequent references to this collection will be identified only by title, volume number, and pages.

37. This memorandum is contained in Hull and Mowrer, "Hull's Psychological Seminars, 1936-38," pp. 106-111.

38. Woodger's neglect of reduction sentences in his published works was a source of some critical comment: "it [is] regrettable that such important a tool for theory construction as are the reduction sentences introduced by Carnap has not been mentioned" (Strauss, "Review of Woodger," p. 375). It was noted above that Hull, unlike Tolman, also neglected reduction sentences.

39. Hull and Mowrer, "Hull's Psychological Seminars, 1936-38," p. 106.

40. Hull, "Idea Book XIX," p. 82.

41. Ibid., pp. 192-193.

42. Ibid., p. 193.

43. Ibid., p. 194.
44. Hull et al., Mathematio-Deductive Theory, p. xi.
45. Ibid., pp. 3, 22-24.
46. Ibid., p. 23.
47. Hull, "Idea Book XX," p. 59.
48. Hull et al., Mathematio-Deductive Theory, p. 306.
49. Ibid., p. 22.
50. Morris R. Cohen and Ernest Nagel, An Introduction to Logic and Scientific Method (New York: Harcourt-Brace, 1934); John Dewey, Logic, The Theory of Inquiry (New York: Henry Holt, 1938); C. I. Lewis and Cooper H. Langford, Symbolic Logic (New York: Century, 1932).
51. Frederic B. Fitch to Laurence D. Smith, 4 December 1980. Woodger's belief that Principia Mathematica logic was teachable was so strong that he actually advocated that it be taught in secondary schools. Simple logical statements in PM notation can be found in Hull's idea books, but these instances are infrequent, and on balance it appears that Hull never acquired any substantial facility with symbolic logic.
52. Douglas G. Ellson to Laurence D. Smith, 17 December 1980. Ellson was himself involved in attempts to systematize Hullian theory. Two places where his role is acknowledged are Hull, "Mind, Mechanism, and Adaptive Behavior," Psychological Review 44 (1937): 1-32, on p. 15 and Frederic B. Fitch and Gladys Berry, "Towards a Formalization of Hull's Behavior Theory," Philosophy of Science 17 (1950): 260-265, on p. 260.
53. Hull et al., Mathematio-Deductive Theory, p. 22.
54. One prominent reviewer, for instance, proclaimed Mathematio-Deductive Theory to be "a foretaste of what psychology will be like when it reaches systematic, quantitative precision . . ." (Ernest R. Hilgard, "Review of Mathematio-Deductive Theory of Rote Learning," Psychological Bulletin 37 [1940]: 808-815, on p. 815).

55. John F. Dashiell, "Some Rapprochements in Contemporary Psychology," Psychological Bulletin 36 (1939): 1-24; Oliver L. Reiser, "Aristotelian, Galilean, and Non-Aristotelian Modes of Thinking," Psychological Review 46 (1939): 151-162, on p. 151; L. O. Katsoff, "Philosophy, Psychology and Postulational Technique," Psychological Review 46 (1939): 62-74, on p. 63. These developments have been discussed in relation to the evolving relationship between philosophy and psychology in my "Philosophy and Psychology: Toward a Realignment, 1905-1935," Journal of the History of the Behavioral Sciences 17 (1981): 28-37.

56. James Grier Miller, "Symbolic Technique in Psychological Theory," Psychological Review 46 (1939): 464-479, on p. 464.

57. Katsoff, "Postulational Technique," pp. 63, 62.

58. Sigmund Koch, "The Logical Character of the Motivation Concept. I," Psychological Review 48 (1941): 15-38, on p. 15.

59. The first instance in the psychological literature of a discussion which placed Hull and the logical positivists in the same context appears to have been Katsoff's paper "Postulational Technique." In the same year, S. S. Stevens parenthetically noted Woodger's interest in formalizing Hull's system ("Psychology and the Science of Science," Psychological Bulletin 36 [1939]: 221-263, on p. 263). Stevens was evidently unaware, at least at that time, that Hull and Woodger had actually worked together.

60. Katsoff, "Postulational Technique," pp. 65, 70, 69.

61. Miller, "Symbolic Technique," pp. 465, 478.

62. E. W. Hall, "Some Dangers in the Use of Symbolic Logic in Psychology," Psychological Review 49 (1942): 142-169, on pp. 150-152; Kurt Lewin, "Formalization and Progress in Psychology," University of Iowa Studies in Child Welfare 16 (1940): 9-42.

63. Sigmund Koch, "Psychology and Emerging Conceptions of Knowledge as Unitary," in Behaviorism and Phenomenology: Contrasting Bases for Modern Psychology, ed. T. W. Wann (Chicago: University of Chicago Press, 1964), pp. 1-41, on p. 10.

64. Herbert Feigl, Inquiries and Provocations: Selected Writings 1929-1974, ed. Robert S. Cohen (Dordrecht: D. Reidel, 1981), p. 88.

65. Sigmund Koch, "Vagrant Confessions of an Asystematic Psychologist: An Intellectual Autobiography," paper presented at the 85th annual meeting of the American Psychological Association, San Francisco, 28 August 1977, p. 19.
66. Ibid., p. 20.
67. Gustav Bergmann and Kenneth W. Spence, "Operationism and Theory in Psychology," Psychological Review 48 (1941): 1-14. Reprinted in Psychological Theory: Contemporary Readings, ed. Melvin H. Marx (New York: Macmillan, 1951), pp. 54-66.
68. Koch, "Logical Character. I," p. 18. Koch has recently called this paper and its companion piece "the silliest and most superficial documents I have ever written" ("Vagrant Confessions," p. 20.)
69. Ibid., pp. 27-31.
70. Ibid., p. 19.
71. Ibid., p. 20.
72. Ibid., p. 20.
73. Ibid., p. 22.
74. Sigmund Koch, "Logical Character of the Motivation Concept. II," Psychological Review 48 (1941): 127-154, on p. 141.
75. Bergmann and Spence, "Operationism and Theory," pp. 7, 6.
76. Ibid., p. 7.
77. Ibid., p. 7.
78. Ibid., pp. 8-9.
79. Ibid., p. 12.
80. Ibid., p. 13.
81. Koch, "Vagrant Confessions," pp. 19, 20.
82. Gustav Bergmann to Laurence D. Smith, 11 November 1980.

83. Hull et al., Mathemático-Deductive Theory, p. 306.
84. Arne Naess to Clark L. Hull, 6 June 1938 (a copy of this letter was kindly provided to the author by Professor Naess).
85. Hull and Mowrer, "Hull's Psychological Seminars, 1936-38," p. 58.
86. Ibid., p. 63.
87. Clark L. Hull to Edward C. Tolman, 8 October 1936 (carbon copy sent to Spence by Hull), in Kenneth W. Spence Papers, Archives of the History of American Psychology, Akron, Ohio.
88. Hull, "Idea Book XX," p. 103.
89. Koch, "Logical Character. I," p. 22.
90. Koch, "Clark L. Hull," in Modern Learning Theory, by William K. Estes, Sigmund Koch, Kenneth MacCorquodale, Paul E. Meehl, Conrad G. Mueller, Jr., William N. Schoenfeld, and William S. Verplanck (New York: Appleton-Century-Crofts, 1954), pp. 1-176, on pp. 25, 41, 49, 112.
91. Hull, Principles of Behavior, p. 30. The actual date of publication of Bridgman's book was 1927. Hull's error was not simply a typographical one, for the date is also given as 1938 in the references. Hull probably had read the 1938 edition of the work.
92. Herbert Feigl has reported that he ceased referring to himself as a "logical positivist" after a French philosopher exclaimed at him: "Les positivistes, ce sont des idiots!" The philosopher obviously had in mind the followers of Comte. This incident, which took place at the First International Congress for the Unity of Science in Paris in 1935, is recounted in Feigl, Inquiries, p. 38.
93. John A. Mills, "Hull's Theory of Learning as a Philosophical System: I. An Outline of the Theory," Canadian Psychological Review 19 (1978): 27-40, on p. 32.
94. Clark L. Hull, "Simple Trial-and-Error Learning: A Study in Psychological Theory," Psychological Review 37 (1930): 241-256, on p. 247.

95. Today's cognitive psychologists engaging in simulations have recognized this sort of limitation on empiricism, and their counterparts in philosophy have codified this point in the modern philosophy of functionalism.

96. Koch, "Clark L. Hull," pp. 63-82.

97. Ibid., p. 107.

98. Hull, "Psychology of the Scientist," p. 871.

99. Koch, "Clark L. Hull," p. 155.

100. Ibid., p. 107.

101. Quoted in Koch, "Clark L. Hull," p. 109.

102. Hull, Principles of Behavior, p. 239. Just such a scale has been fruitfully utilized in the recent Rescorla-Wagner model of Pavlovian conditioning. This model, a fairly direct descendant of Hullian theory, provides a clear example of the continuing viability of the Hullian tradition (with, of course, suitably curtailed ambitions). See Robert Rescorla and Allan R. Wagner, "A Theory of Pavlovian Conditioning: Variations in the Effectiveness of Reinforcement and Non-Reinforcement" in Classical Conditioning II: Current Theory and Research, ed. Abraham F. Black and William F. Prokasy (New York: Appleton-Century-Crofts, 1972).

103. Clark L. Hull, A Behavior System (New Haven, Conn.: Yale University Press, 1952), pp. 354, 3.

104. See Koch, "Clark L. Hull," pp. 167-169. On Hull's decision to take this approach, see Hull, "Psychology of the Scientist," p. 874.

105. Abram Amsel, "On Inductive versus Deductive Approaches in Neo-Hullian Behaviorism," in Scientific Psychology, ed. Benjamin B. Wolman and Ernest Nagel (New York: Basic Books, 1965), pp. 187-203, on p. 187.

106. S. S. Stevens, "S. S. Stevens," in A History of Psychology in Autobiography, vol. 6, ed. Gardner Lindzey (Englewood Cliffs, N. J.: Prentice-Hall, 1974), pp. 395-420, on p. 409.

CHAPTER 8

HULL'S BEHAVIORAL PSYCHOLOGY OF SCIENCE

The preceding chapters have documented Hull's anticipation of several cardinal positions of logical empiricism and his actual interactions with the logical empiricist movement. We have seen that, despite the remarkable confluence of general positions, Hull's involvement with and understanding of logical empiricism was in fact quite restricted. In effect, he was eager to contribute to the mutual support that behaviorism and European positivism could provide for each other, but he was by no means a student of logical positivism and apparently never delved into its technical literature. Hull's philosophical views--his empiricism, associationism, and materialism--were well fixed in the 1920s, before the influence of the Vienna Circle was felt in America. From his philosophical views, Hull evolved a rudimentary theory of knowledge of his own, a behavioral theory of knowledge which, although based on the elementary concepts of conditioning, aspired to account for the higher forms of scientific reasoning and theorizing. Never fully articulated by Hull, this theory went beyond logical empiricism in subsuming theory itself under a naturalistic psychology of science. More importantly, it conflicted

with logical empiricism by further subsuming logic under psychology, thereby constituting a behavioristic psychology. Hull's behavioral psychology of science is the topic of this chapter.

It is well to remember that Hull's original ambition in psychology was to establish a theory of knowledge on a materialistic basis. He had read and admired the systematic philosophies of Locke and Hume, and had endorsed their attempts to ground philosophy on the facts of psychology. But he felt that their failures to arrive at uncontroversial epistemological theories stemmed from their adoption of conscious experience rather than action as their factual base. Believing that something like Hume's system could be made viable by placing it on a behavioristic foundation, Hull planned to model his magnum opus on Hume's Enquiry concerning Human Understanding, following its organization and chapter headings. He viewed his dissertation on abstraction and concept formation as a step in that direction. But when this research failed to draw the attention he thought it deserved from other psychologists, he became disillusioned with the prospects for his goal and turned temporarily to research in aptitude testing and hypnosis. Around 1930, he judged conditions favorable for taking up the challenge once again. He wrote:

The more I think about it, the more convinced I become that the time is ripe for a new work on the problems which Locke and Hume struggled with as reported in their great classics. . . . The time is evidently here for a searching naturalistic account of the manner that men acquire the various types of knowledge and the nature of this knowledge, done in the modern manner.

From this point on, Hull's idea books make it clear that his ultimate aim was to achieve a behavioristic theory of knowledge and that his research on conditioned habits was for him an attempt to realize what he called "my hope to achieve a major contribution to the theory of knowledge."¹

Knowledge as a Habit Mechanism

From the time of his earliest ruminations on a behavioristic epistemology, Hull took a very Humean strategy as a model well-suited to the enterprise. "After all," he wrote in 1927, "a good deal of modern behaviorism is implicit in Hume and a considerable amount of it is explicit also, probably much more than is ordinarily realized." Hume had spoken of unitary impressions becoming associated and concatenated according to the laws of a mental chemistry. To invert the associationist framework onto a materialist basis, Hull needed some physical equivalents of Hume's atomic units as well as principles by which they could become associated. This need was met, in Hull's view, by Pavlov's classic

Conditioned Reflexes, which became a sort of bible for Hull upon the appearance of its English translation in 1927-28. Adapting Pavlov's work to Hume's associationism, Hull spoke of "small unitary stimulus-response units . . . being aggregated into larger and larger units always operating on the same general principle."² The principle, of course, was that of Pavlovian conditioning. With characteristic ambition and the ingenuity of an engineer, Hull set out to derive the whole range of mental phenomena and types of knowledge from these simple elements and principles.

Hull never came very close to his goal. When conditioning turned out to be a much more complicated affair than he had initially supposed, his energies became diffused into the details of his narrower theory of conditioning. But he did make a substantial start on his behavior-based epistemology during the 1930s, and there is ample evidence that he continued for the rest of his career to view science from the perspective of that epistemology, despite its sketchiness and its shortcomings.

The basic mechanism of knowledge was laid out in Hull's paper of 1930 entitled "Knowledge and Purpose as Habit Mechanisms."³ Given a causal sequence of stimulus events in the world and a sensitive organism receiving those stimuli, Hull reasoned that each event (S) would set up within the organism a corresponding response (R). The

result would be a sequence of responses ordered at this point only by the causal sequence of stimuli producing them. But, Hull continued, since higher organisms possess internal receptors for sensing their own responses, each R will give rise to an internal stimulus (s). By virtue of being paired with the succeeding external event S, each of these internal stimuli acquires the capacity to evoke (i.e., to cause) the succeeding R (see figure). In this way, contact

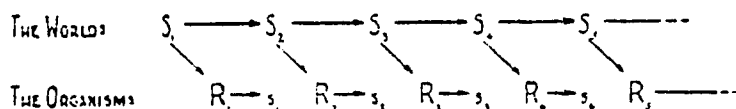


Figure 1

with the environment instills in the organism a chain of classically conditioned reactions which copies the external sequence and is capable of running its course independently of the world. In summarizing this mechanism, Hull wrote:

. . . it may be said that through the operation of a variety of principles and circumstances, the world in a very important sense has stamped the pattern of its action upon a physical object [the organism]. The imprint has been made in such a way that a functional parallel of this action segment of the physical world has become a part of the organism. Henceforth the organism will carry about continuously a kind of replica of this world segment. In this very intimate and biologically significant sense the organism may be said to know the world. No spiritual or supernatural forces need be assumed to understand the acquisition of this knowledge. The process is entirely a naturalistic one throughout.⁴

The biological value of such a mechanism is readily apparent. Once established, the internal sequence can

proceed more rapidly than the external sequence. This phenomenon, according to Hull, is what is commonly referred to as foresight or fore-knowledge. Defensive reactions provide a clear example. If the last event in the world sequence (S_5) is, for example, a threatening or noxious stimulus, then the final response (R_5) of, say, flight will be all the more adaptive if it takes place before the actual occurrence of S_5 . For such cases, Hull noted that the intervening responses in the internal sequence (R_2, R_3, R_4) have no value other than to set up stimuli which maintain the integrity and independence of the internal sequence. Since the only function of such responses is to serve as stimuli for subsequent responses, Hull called them "pure stimulus acts" or equivalently "pure symbolic acts."⁵

Although pure stimulus acts were clearly inventions rather than observed responses, they played a crucial role in Hull's learning theory and later in his psychology of science (see following section). They provided a behavioristic equivalent of ideas in the sense of their being internal events which could guide action with respect to a goal. "While indubitably physical," said Hull, "they occupy at the same time the very citadel of the mental."⁶ Furthermore, pure stimulus acts provided a way to account for the flexibility and spontaneity of

organismic behavior. As Hull saw it,

the advent of the pure stimulus act into biological economy marks a great advance. It makes available at once a new and enlarged range of behavior possibilities. The organism is no longer a passive reactor to stimuli from without, but becomes relatively free and dynamic.⁷

Clearly, such a conceptual device would facilitate explanations of the higher forms of behavior, but this advantage was gained by Hull at the cost of invoking unobservables in his theorizing, a ploy which was to draw criticism from his fellow psychologists.

In fact, it might be said that it was Hull's use of the pure stimulus act as an explanatory concept that became "relatively free and dynamic." In one especially laborious and arcane application of the device in 1935,⁸ Hull claimed to have derived the phenomenon of insight, but only by virtue of having invoked a multitude of pure stimulus acts drawn from three distinct but complexly interacting internal sequences. Hull was cognizant of the unobservable status of these supposed acts, and accordingly took great pains to insist that they could be materially realized in the construction of highly sophisticated physical machines.⁹ Nevertheless, the uninhibited use which Hull made of them understandably generated some skepticism among other psychologists. Tolman, for instance, recorded his misgivings in his presidential address to the APA. Speaking of Hull's intricate diagrams of pure

stimulus acts, Tolman said, "They are very clever and can be invented, as I know to my cost, to explain practically any type of behavior, however far distant from an instance of conditioning such a behavior might at first sight appear."¹⁰

During the early 1930s, Hull described two further knowledge mechanisms which were to supplement the basic mechanism outlined above. The first of these auxiliary mechanisms, that of "short-circuiting," was a consequence of biological economy. Hull noted that the internal chain offered two possibilities for adaptive efficiency.¹¹ First, the intervening responses of which it is composed could undergo an energy-saving reduction in magnitude, as long as they remained just strong enough to set up the succeeding stimulus in the chain. Second, energy and time could be saved by the dropping out of intervening responses until there existed the bare minimum number necessary for the maintenance of the chain up to the final instrumental act. This short-circuiting would tend to occur, according to Hull, when there was intraserial competition¹² among the members of the chain and when there was a persisting or "purposive" stimulus (either a goal stimulus or a drive stimulus) which could help bind together the chain.

The second auxiliary mechanism of knowledge was Hull's well-known concept of the habit-family hierarchy.¹³

According to this notion, response chains which share a common initiating stimulus and a common final goal response may be grouped together in a habit family. Within the family, the chains would be ordered into a hierarchy which would represent the relative strengths of the habit tendencies and could subsequently govern the selection of a habit from the family under particular circumstances (as when, for example, external inhibition is applied to certain of the chains). Since the chains in a family could differ qualitatively as well as quantitatively (e.g., in length), the habit-family hierarchy contained the possibility for considerable flexibility in the attaining of a goal. The divergence of the chains suggested--as Hull recognized--a strong kinship between the habit-family hierarchy and Tolman's means-end-field. But whereas Tolman preferred the spatial metaphor of "fanning" lines of causal texture, Hull fell back on his affinity for geometry and chose to think of his families in the mathematical sense. He noted:

The term 'families' is here used in much the same sense that geometers use the term to designate a series of curves, such as parabolas, which originate at the same point but thereafter follow different courses, all being generated by a single formula but each having a different value for one of the parameters.¹⁴

Although Hull never formally developed his notion of habit-family hierarchies as derived from a single formula, the notion clearly suited his conception of the organism

as a deterministic machine, one whose behavior could be described and integrated under a simple mathematical expression.

To summarize, Hull's theory of knowledge was in its general outlines a behavioristic version of traditional associationism. The theory rested on three basic mechanisms: (1) chains of internal responses, the pure symbolic acts, which were said to copy the external world through a process of associative conditioning; (2) short-circuiting, by means of which the chains were pared down by the forces of biological economy to their minimal effective length and magnitude; and (3) hierarchies in which a flexible family of alternative habit sequences was arranged and ordered. These mechanisms were not the only ones discussed by Hull, but they became the most important in his psychology of science.

Behaviorist Theory of Theory

Theory as a Habit Mechanism

Hull recognized early on that the core problem of a materialistic psychology of science was that of giving an account of theory itself. In preparing his seminar on behaviorism in 1925, Hull read A. P. Weiss's newly published A Theoretical Basis of Human Behavior. When Weiss claimed that mental properties could not be integrated into science's "system of natural law," Hull responded by

asking, "Behavioristically, what is a 'system of natural law'?"¹⁵ Weiss and other behaviorists (Watson, for example) were content, at least for the time being, to let behaviorism hold the same epistemological views toward theory as those held in the established sciences of physics, chemistry, and biology. It was characteristic of Hull's ambition and intellectual courage that he faced the necessity for behaviorism to account for the activity of theorizing--even though to do so called for the peculiar activity of theorizing about theorizing. An adequate and self-consistent behaviorism, wrote Hull, requires

as a background at least a basic epistemology, i.e., a theory of the nature of theory itself. This is one of those cases where the very thing under controversy is involved in the method of proof through no fault of anyone. Clearly in this system is a new phase of pragmatism but based on a sound and rigorous basis.¹⁶

Undaunted by the apparent circularity of the situation,¹⁷ Hull proceeded to pursue his theory of theory through the later half of the 1920s.

Hull's first extended consideration of the matter appeared in his idea book of 1926-27, under the title "Behavioristic theory of the 'nature of theory'." The ideas were roughly formulated, but they contained the germ of his later thoughts on theory. The passage begins:

I have been haunted for many days by the profound importance of ideas and theories being nothing but habits--mainly symbolic habit activities. This means that all science, all mathematical processes, are at bottom nothing but symbolic habits.¹⁸

In elaborating this notion, Hull took up the example of formulas, which according to him, "may be taken as the essence of theory." Hull viewed formulas as representations of generalized habits.

A formula is merely an objectified stimulus which will touch off a specialized series of habits. This can be constructed by a genius and employed by any person who has acquired the sub-habits. Note:--A generalized habit is one that does the duty of a formula only internal.¹⁹

Thus, for example, the formula for a right triangle integrates the symbolic sub-habits of squaring and adding into a generalized habit. The outcome in a given instance is a symbolic action equivalent to the gross action of actually measuring off the distance. "It is thus," said Hull, "an alternative and more economical method."²⁰

In its economical aspect, Hull's early concept of theory adumbrated the later notion of short-circuiting. But the development of the short-circuiting notion came only after the mechanism of associative chaining had assumed central importance in his system.

Similarly, the idea that the sequence of internal habits governed by a formula (or theory) is an alternative, equivalent in outcome, to actual observation or measurement constitutes a clear precursor to the habit-family hierarchy.

Hull asserted that under appropriate initiating conditions a symbolic formulation of theory

will evoke a series of habit units each of which, in turn, will result in an act which will, in conjunction with the formula and the internal pattern (purpose) evoke another definite reaction and so on, until the final reaction will be the same, or practically the same, as if the subject had made a symbolic representation of what would have resulted from actually making the experiment.²¹

Sharing a final goal response and initiating conditions, the routes of theoretically calculating a value and empirically ascertaining it comprised--despite the qualitative differences between them--what Hull would later call a habit family. But once again, the explicit development of this concept came only after Hull had formulated the basic mechanism of internal copies conditioned by association with the external world.

This latter mechanism was not long in coming. In 1927, Hull wrote:

It has just struck me very forcibly that the pragmatic theory of theory itself really goes back to Hume. . . . The moral . . . is that I should study Hume with a good deal of care before proceeding much farther with my system.²²

What he found in Hume, of course, squared neatly with what he found in Pavlov. As was mentioned above, Pavlov's stimulus-response bonds and conditioning procedures provided Hull with materialistic equivalents of Hume's impressions and associations. Diagrams of internal copies

arising from external sequences (such as in Figure 1) soon began to appear in the idea books, along with remarks about their translatability into actual machines. As for the theory of theory, Hull's task became one of making plausible the notion that theories can be construed as internal copies of this sort.

This problem became an enduring one for Hull and one which constituted a stumbling block in his attempts to give an explicit public formulation to his psychology of science. By the time of his paper on "Knowledge and Purpose" in 1930, he felt the metatheory was sufficiently crystallized to begin alluding to it in print. In that paper, he urged that the pure stimulus act, the building block of internal copies of the world sequence, be construed as "an organic, physiological--strictly internal and individual--symbolism." He was quick to add, however, that this "peculiarly individual form of symbolism is not to be confused with the purely stimulus acts of social communication," which he admitted were "so complex as to preclude consideration here."²³ Despite this qualification, Hull's strategy was clear. A scientific theory about causal sequences in the world was, according to the metatheory, to be viewed first as a generalized sequence of internal symbolic responses and then--through an as yet unspecified mechanism--as a public copy of the world expressed in social symbolism.

Hull's enthusiasm for this approach was dampened when he presented it, shortly before its appearance in print, to a group at Harvard. His audience there was apparently unconvinced by his diagram of knowledge as a functional copy of the world sequence and by his concept of short-circuiting. As he recorded in his notes, "They objected to the propriety of my calling the temporal antecedence of R_5 to S_5 a case of foresight, or of the parallelism of the lower series a case of knowledge." The conclusion he drew from the episode was that "I must recast and improve my presentation of the notion that the organism can acquire a replica of the law operating in the external world which is a functioning type of knowledge, though not conscious at all."²⁴

The net effect of Hull's encounter at Harvard was to make him more cautious in advancing his system of knowledge and to tone down his ambitions about writing a major work on the behaviorist epistemology. In 1933, he was still planning to write an article on the theory of knowledge,²⁵ but even this plan was eventually abandoned. All the same, the Harvard experience seems not to have shaken Hull's faith in the general soundness of his epistemological views. On the contrary, they continued to underlie his thoughts on science and to find expression both in his private writings and more obliquely, in his published works. As such, these views are crucial for understanding

the peculiarities of his approach to science and his relationship to logical positivism, even though he never fully developed or expressed them.

By claiming that pure stimulus acts were a rudimentary form of symbolism, Hull felt that scientific theories could be reduced to associatively conditioned habit sequences. But the fact that theories are socially shared linguistic entities continued to pose a problem for his metatheory. Lacking a theory of social symbolism, Hull was never able to close the gap, which he had acknowledged in 1930, between the internal symbolic system of pure stimulus acts and the external symbolic system of language. The best he could do in this respect was to speak elliptically and loosely of pure stimulus acts and their "graphic equivalents." Presumably these socially communicable equivalents could become attached to the internal symbols through some process of associative conditioning; but Hull nowhere defended such a view in detail or in public.

This lacuna in his theory of theory did not prevent his continued adherence to it. During the push for integration of social science at the IHR in 1936, Hull invoked his metatheory in characterizing the sought-for integration. In science, he asserted, the

integrative medium is a structure of implicative pure stimulus acts (or their graphic equivalents) which set out with a small number of concepts and postulates and from these derive the same symbolic actions (or their equivalents) as are evoked in the investigator when the relevant aspect of the external world is exposed to his senses.²⁶

In this assertion is found a concise summary statement of Hull's theory of theory. The pure stimulus acts which comprise a scientific theory run parallel to some general causal sequence in the world. They are arranged in an implicative structure because genuinely scientific theories are deductive systems which mirror the hierarchical causal organization of the world.²⁷ The output of such an implicative structure, whether embodied in internal or social symbols, is a pure stimulus act like the one that would arise in the investigator making the relevant empirical observation. That is to say, the output is a theoretical prediction, a specialized kind of foresight made possible by the mechanism of short-circuiting. Humans can couch their theories in graphic symbols, but at root a scientific theory is no different from "a replica of the law operating in the external world" which any higher organism can acquire. As was the case with Tolman's empirical epistemology, Hull refused to draw a sharp distinction between the higher forms of human knowledge and the kinds of knowledge attributable to lower organisms. Like Tolman, he was striving to interpret science as a psychological phenomenon rather than as a logical one.

A Behaviorist Theory of Truth

Traditionally, accounts of science have included, at least implicitly, some attempted solution to the problem of what constitutes truth in science. Seeing the need to address this fundamental issue, Hull extended his psychology of science to a behavioristic approach to truth. His characterization of scientific truth involved the notion that different activities of the scientist could lead to the same outcome. The symbolic actions of theorizing and computing predictions, said Hull, could give rise to a final symbolic act which under optimal conditions would be the same as the pure stimulus act arising from empirical observation. Accordingly, the independent processes of theorizing and observing were said to converge on a final common act and thus form a habit-family hierarchy. For Hull, then, the test of truth was the convergence of multiple processes on the same outcome. Since these processes were construed as action sequences, either overt or covert, he called it "a behavioristic theory of truth" and viewed it as part of "a behavioristic pragmatism."²⁸ Just as a lower organism typically achieves a goal by means of various alternative routes, so the scientist approaches true statements through various means.

Hull's view of truth placed the burden of theorizing on the ability of a theory to produce novel predictions which could be approached and confirmed via the empirical route. This prediction-generating capacity constituted for Hull the "crucial test" of a theory.²⁹ Since predictions, qua symbolic act calculations, could be carried out within an individual and then checked by that individual, Hull's theory of truth was not inherently one of truth-by-agreement.³⁰ But given Hull's advocacy of a scientific division of labor between theoreticians and experimentalists (see Chapter 6), the theory in its application had a social dimension in which agreement was mediated by the overt symbols of social communication.

Hull's setting of the problem of truth in the context of adaptive behavior gave his theory the flavor of various pragmatic theories of truth. In its emphasis on the predictive aspect of scientific theories, his version of truth was somewhat similar to John Dewey's characterization of theories as leading principles. Although Hull sometimes spoke of the internal symbolic sequence as a "copy" or "replica" of the world, he was usually careful (as was Dewey) to avoid the implication that ideas and theories copy or image the world. Instead, Hull generally spoke of the internal reaction sequence as no more than a "functionally parallel event" with respect to the outer-world sequence."³¹ As in any habit family, the alternative

habits of theorizing and observing bore no necessary qualitative relation to each other. In Hull's words, the "several habit sequences in effect (not actually) run a parallel course."³²

Aside from these general similarities between Hull's view and that of the pragmatists, it is not clear what grounds Hull had for calling his system a behavioristic pragmatism. More often than not, Hull's views on science seemed to qualify his system as a brand of realism. His materialism and mechanism, his occasional reference to internal "copies" of the world, his notion that theories could "collide" with "stubborn facts," and his holding out for physiological interpretations of his intervening variables--all of these views made him sound more like a realist than a pragmatist. Being freely extrapolated from a theory of conditioning which itself was not highly developed, Hull's psychological metatheory of science simply failed to conform clearly to the philosophical categories of his time. When it was pointed out to him in 1930 that the realistic elements of his view made dubious his characterization of it as a pragmatism, Hull expressed interest but remained unconvinced.³³ He was not especially well-versed in contemporary philosophy, and in any case felt that an adequate behaviorism would provide objective solutions to philosophical problems.

Hull's theory of truth and the role of the habit-family hierarchy in it were formulated during the late 1920s. During his summer at Harvard in 1930, he proposed the theory to the realist philosopher Edward G. Spaulding. The example he used in explaining the theory was that of multiplying 5×6 , a feat which could be performed (by human or machine) in alternative ways such as counting five groups of six or adding six five times. According to Hull, the theory was "scouted with great scorn." In the face of this negative reaction, Hull resolved to write a "clear and convincing" article on the behavioristic theory of truth.³⁴ When he introduced the concept of the habit-family hierarchy in a paper of 1934, he asserted his belief that "the habit-family hierarchy constitutes the dominant physical mechanism which mediates such tests of truth and error as organisms employ--that it provides the basis for a purely physical theory of knowledge." But he added, "This matter is reserved for elaboration in a subsequent paper."³⁵

The proposed paper was never to appear. Like other aspects of his psychology of science, Hull's theory of truth became submerged during the 1930s under a morass of details and controversies about conditioning theory itself. Hull had assumed in the late twenties that the simple but powerful conceptual apparatus derived from conjoining Humean associationism with Pavlovian conditioning

would lead directly and rather quickly to his major work on a behavioristic theory of knowledge. But conditioning proved to be no simple phenomenon, his psychological theory of science was not gaining a favorable reception, and by the end of the thirties the logical positivist-operationist theory of knowledge had ascended to a position of widespread acceptance among psychologists. As a result, not only did his major work on knowledge never appear, but his more modest plan for a series of papers on the subject was also scrapped. In his published writings, the theory of knowledge is presented obliquely, in bits and pieces, and as asides in works devoted to experiment and theory in the narrower context of conditioning theory.

But if Hull gave up on explicitly advancing his epistemology, he did not surrender his faith in it or his implicit reliance on it in his published works. For example, Hull's theory of truth appeared in the Principles of Behavior, but without being referred to as such and without reference to the habit-family hierarchy. Using his favorite example of Newtonian mechanics, Hull described how the shape of Neptune could be equivalently determined through empirical or theoretical channels. He continued:

The critical characteristic of scientific theoretical explanation is that it reaches independently through a process of reasoning the same outcome with respect to (secondary) principles as is attained through the process of empirical generalization. . . . The fact that, in certain fields at least, practically the same statements or propositions can be attained quite independently by empirical methods as by theoretical procedures is of enormous importance for the development of science. For one thing, it makes possible the checking of results obtained by one method against those obtained by the other.³⁶

Hull's theory of truth was a peculiar one in that it could sound like many other theories of truth, depending on which aspect was emphasized in a given expression of it. When Hull focused on the multiple determination of truth, or on prediction as the crucial test, or on the biological value of theory, it sounded like a pragmatism. When he spoke of comparing theoretical claims with the facts or of facts correcting theories, he seemed to adhere to a correspondence theory. And sometimes he emphasized the logical and deductive aspect of theory, as when he stated that "it is evident that in its deductive nature systematic scientific theory closely resembles mathematics"³⁷; in these cases his theory seemed like a coherence theory. Perhaps this seeming equivocation is attributable to Hull's lack of training in philosophy or his inattention to detailed philosophical distinctions. But from the standpoint of philosophical discussions of truth, the ambiguity in Hull's theory stemmed in large measure from the fact that the theory was based on Hull's

own peculiar extrapolation of conditioning principles-- a situation in which the ambiguity in Hull's formulation was exacerbated by his failure to publicly acknowledge the behavioristic grounding of the theory.

It was the psychological basis of Hull's version of truth that set it apart from any of the logical positivist theories of truth. The logical positivists never settled on an "official" doctrine of truth, but none of their views ever approached the psychologism that was characteristic of Hull's. This was particularly the case for the correspondence view which Carnap had adopted from Alfred Tarski after the latter had convinced him of the legitimacy of semantics.³⁸ On this view, truth was a matter of correspondence, formulated in a metalanguage, between a proposition and a state of the world. The psychology of perception, to be sure, entered into the ascertainment of the state of the world, but the correspondence notion of truth itself was strictly a logical matter. The other version of truth current among logical positivists was the coherence theory advanced primarily by Otto Neurath.³⁹ According to Neurath, the truth of an assertion was to consist in its cohering with a body of statements which is then empirically confirmable only when taken as a whole. Neurath's account called for the observation sentences, the protokolsätze, of a theory to be formulated in the third person, even when reported by the observer himself.

In this limited sense, the approach was behavioristic; but the stipulation amounted to no more than another assertion of physicalism, a doctrine which by Neurath's own insistence was independent of any particular version of behaviorism. Neurath's "behaviorism" was a recommendation for linguistic reform and not a behavioristic epistemology or any other form of psychologism. Hull's behaviorism, on the other hand, was a world-view, a theory of behavior, and a conceptual system to which philosophical problems themselves could allegedly be referred for solution.

Theory and Organism as Parallel Machines

As we have seen, Hull was an ardent advocate of a mechanistic conception of nature. He felt that Newton had shown the inanimate world to be an elaborate machine. In Hull's eyes, the organic world was equally a part of the world-machine, even in its most complex manifestations. Knowledge in general, and science in particular, represented adaptations of one part of the physical world to another. When Hull wrote about the mechanisms of knowledge, he typically appended remarks to the effect that the realization of these mechanisms as actual machines would be a straightforward matter in the hands of a skilled engineer. The arrangement of simple knowledge mechanisms into sophisticated, highly flexible knowing machines would

require diligence and cleverness, but there was otherwise no impediment to such an achievement. Higher knowledge itself was thought to be an intricate but mechanical interplay of pure stimulus acts, and scientific theory was an especially effective conceptual machinery based on these symbolic acts.

For Hull, the relation obtaining between a successful scientific theory and its subject matter was a special kind of congruence. He spoke not of a theory describing or picturing the world of empirical phenomena, but rather of a theory "paralleling" a dynamic situation in the world.⁴⁰ In psychology, qua science of behavior, this meant that a theory was a conceptual machinery or system which would run parallel to the organismic behavior in its domain. By putting initial conditions, e.g., stimulus values, into the theoretical machinery a series of computations could be set off resulting in a prediction (or possibly an explanation) of an organismic action. Internal mechanisms mediating the target behavior could be paralleled by the theory's intervening variables, which were accordingly taken to reflect physiological processes underlying the behavior. In sum, a theory of behavior and a behaving organism were viewed as parallel machines.

This conclusion can be elucidated by examining the common features attributed by Hull to machines, theories, and organisms. First and most obvious, all three were strictly material entities. Like literal machines, theories

and all other forms of knowledge were structured sequences of material events; no nonmaterial forces were needed to account for them. Likewise, organisms were seen as complex arrangements of pure matter.

A second feature which Hull saw as common to machines, theories, and organisms was that all three were said to evolve through a process of trial-and-error. This was a somewhat peculiar notion as applied to the development of machines, but it was just such a process by which Hull and his assistant constructed the correlation machine which later figured so prominently in Hull's mechanistic thought. In his autobiography, Hull described the gradual process of manual trial-and-error that went into the time-consuming production of the device.⁴¹ Theories, too, were said to evolve by trial-and-error, albeit in a somewhat more complicated way. The postulation of principles represented a kind of symbolic trial; when their deduced implications were in error, they were revised and adjusted and tested again. Through this process, theories evolve toward truth status. "Thus," wrote Hull, "the determination of scientific principles is in considerable part a matter of symbolic trial-and-error."⁴² The phylogenetic evolution of organisms took place by a process of variation and selection, a kind of trial-and-error. The ontogenetic evolution of adaptive behavior was similarly a matter of trial-and-error, or equivalently a process of excitation

and inhibition. In his first paper on behavior theory (1929), Hull wrote that the excitatory and inhibitory phases of learning operate as an "automatic trial-and-error mechanism which mediates, blindly but beautifully, the adjustment of the organism to a complex environment."⁴³

As for the third property shared by machines, theories, and organisms, Hull believed that all three were capable of generating novelty through a computational interplay of simple elements. In this sense, a sophisticated machine could yield higher order "mental work," such as logical calculations or correlation coefficients, by means of simpler operations combined according to a small set of mechanical principles. Hull saw such output as genuinely novel or "emergent" with respect to the simpler elementary operations, just as multiplication might be said to be emergent from addition. In a similar vein, the set of principles making up a theoretical system could combine in appropriate ways to generate, via the computational procedures of math and logic, novel predictions about scientific phenomena. It was just these predictions which Hull took to be the crucial test of a scientific theory. And of course, Hull insisted that higher organisms were constituted in such a way that they could produce novel adaptive responses. Emergent behaviors such as goal-seeking, purpose, and insight were generated from complex but structured interactions of simple sub-habits.

The fourth and final aspect which made machines, theories, and organisms alike in Hull's view was that all three were seen as hierarchically structured. As was described in Chapter 6, Hull expressed the notion as early as 1926 that the various sub-mechanisms of a machine needed to be placed under some hierarchy of control. This lesson was undoubtedly learned in the course of actually constructing his early machines, although his early fascination with hierarchies probably predisposed him to think in these terms.⁴⁴ The hierarchical character of theory was a prominent motif in Hull's work. He often spoke of a theoretical system as a cluster of statements which resembled geometry in exhibiting a hierarchical structure of primary principles, secondary principles, and so on. Finally, the hierarchical structure of organisms was the crucial property by which the combination and interplay of atomic S-R habits could issue forth in emergent adaptive behavior.

Hull's conceptions of machine, theory, and organism were of course not independent developments in his thought. Theoretical systems in psychology could be said to parallel the behavior over which that ranged because the systems and the responding organisms were construed alike as machines. As such, they had all the characteristics attributed by Hull to complex mechanisms. The inter-translatability of machine design and theoretical principle and the conception of

organisms as machines led naturally to the metaphor of theory paralleling behavior--a metaphor derived ultimately from Hull's underlying mechanistic world-view. As often happens with fundamental metaphors, Hull's mechanism tended to recede with time from the foreground of his thought and writings, thereby making the discernment of its metaphorical nature all the more difficult. But it was no less operative for its state of lessened visibility. The historical reasons for the recession from prominence of Hull's mechanism are surely complex, but it seems quite likely that the emphasis of the thirties on the testability of scientific claims helped to discourage Hull from publicly airing his mechanical metaphor.⁴⁵ The view of science advanced by operationists and logical positivists simply made no room for metaphor--which was after all metaphysical--or for research heuristics.⁴⁶ Thus, despite its claim to ensure public scrutability in matters of science, the new view of science popularized in the thirties very likely had the effect of pushing Hull's basic metaphor of organism as machine--a metaphor which animated all his work--farther from the arena of public criticism and possible understanding. Finally, since the plausibility of Hull's psychology of science depended intimately on the mechanistic metaphor, it, too, gradually receded from the realm of public scrutiny.

An Empirical Interpretation of Logic

The Psychology of Logic

As we have seen, Hull placed great stress on the role of logic in his theoretical system. Given this fact and his overall aim of developing an epistemology based on behavioral principles, it is not surprising that he saw the necessity of offering a behavioral account of logic itself. His efforts in this direction began in the mid-twenties along with his ruminations on the general theory of theory. In fact, his account of logic was, except for matters of detail, the same as his account of theories; for Hull, the principles of logic had an epistemological status no different from that of the principles of science.

The essential elements of his interpretation of logic were already evident in his idea book of 1926-27. At that time he wrote:

Logical and mathematical theory is nothing but certain habits, found by trial to suit the world, [which] have been isolated, objectified by means of graphic symbols and in this way able to stimulate others, (and the maker also at different times). This computation is but a series of stimulus response combinations.⁴⁷

As is apparent from this brief passage, Hull conceived logical theory as akin to scientific theory proper in several respects: it is arrived at by trial, it applies in some sense to the world, it can be conveyed across time and individuals by means of graphic symbols, and it is

comprised of computations based on stimulus-response units. Implicit in the passage is the notion that logic shares with theory the underlying knowledge mechanism by which sequences of pure stimulus acts parallel the causal sequences of the environment. In this way, Hull believed, logic reflects the causal structure of the world in the same way as ordinary theory, but it does so at a more general level.

This peculiar view of logic meant, of course, that the principles of logic were inherently contingent. Hull was willing to accept this implication, although he insisted that the causal uniformity of nature was sufficiently strong to confer a very high degree of reliability on the laws of logic. Thus, said Hull:

Logical necessity is no stronger than the tendency of one symbol to evoke another. Of course in the world there seems to be a uniformity which tends to set up in normal organisms certain habits more or less independent of time and space.⁴⁸

The fact that these time- and space-independent habits could capture generalized causal uniformities in the environment thus formed the basis of an empirical logic, but it also conferred a biological advantage upon those organisms which acquired the habits. Hull noted, for example, that "a (logical) inconsistency or (better) a logical contradiction is two series of symbolic activities which lead to different and incompatible action from the same stimulus

pattern."⁴⁹ The habits underlying logic were for Hull not merely a source of abstract rules; they provided for the adaptive conformity of action to the causal structure of the world-machine.

In 1938, Hull wrote a note to himself saying that the idea of logical rules capturing causal sequences in the world should be included in his book as part of the chapter on thinking and logic.⁵⁰ As we have seen, no such chapter was ever published. Nonetheless, there is good reason to believe that Hull never actually gave up his empirical interpretation of logic. Even in his posthumously published A Behavior System (1952), Hull spoke of theory as a "logico-causal hierarchy."⁵¹ Logic, like theory, was still being viewed as a conceptual machinery, a causal hierarchy of pure stimulus acts.

The Confirmatory Status of Logical Principles

Believing as he did that the principles or rules of logic had empirical content, Hull was also committed to their being subject to empirical test. In fact, Hull did advance an account of how logical rules are confirmed, over the long run, through experience. In deducing a prediction from a scientific theory, Hull emphasized, we use not only the principles of that theory but also the rules of logic. Hull recognized, as had Pierre Duhem,⁵² that if the resulting prediction fails to agree with the

facts, the fault may rest either with the theory or with the other assumptions used in the derivation--and for Hull this included the logical assumptions. If the prediction is correct, Hull added, both the theory and the rules of logic thereby receive an increment in the probability that they will correctly mediate future predictions. Hull continued:

It thus comes about that the rules of logical syntax are validated in exactly the same manner and, indeed, at exactly the same instant, that the scientific postulates are validated.

The reason why we get the illusion that these "laws of thought" or logic are innate and of an entirely different order of validation is that the above process has been going on more or less since man has had the use of speech so that there has been an immense amount of trial-and-error which has molded these rules of syntax to a high degree of precision. The result of this trial-and-error has been incorporated into the culture, both informally in ordinary speech habits and formally as an explicit and self conscious methodology taught in the schools. We pick much of this up as speech habits with no recollection of the circumstances under which the habit was picked up. Accordingly when the habits function we are apt to think the habit tendency always was in our possession, and is therefore inherent in the nature of things.

By "the nature of things," Hull meant in this case the nature of the mind, i.e., that logic represents inherent laws of thought. He was quick to deny such a view of logic since logic in his view arose from the causal regularities of the external world. With this in mind, he appended the following paragraph to the above passage:

As a matter of fact, of course, even as things now appear, they [the rules of logic] presumably do reflect in some sense "the nature of things"--in the sense that by a selective trial-and-error process those symbolic processes represented by the rules of logical syntax have been selected which will mediate symbolic conclusions in agreement with the causal sequences in the world.⁵³

This passage was written in 1938, a time when Hull was feeling the influence of logical positivism (as evidenced by his use of the expression "logical syntax"). It is especially significant, therefore, that he was espousing an interpretation of logic so clearly at odds with the logical positivist view of logic as an empirically vacuous tautology.

Further exposure to logical empiricism did not dissuade Hull from his view of logic. Even after working with Woodger, he included a statement on the empirical status of logic in the introduction of the Mathematico-Deductive Theory of 1940.⁵⁴ The same statement appeared almost unchanged in Principles of Behavior three years later. It appeared near the end of the first chapter in a brief section entitled "The 'Truth' Status of Logical Principles or Rules." The statement was as follows:

Despite much belief to the contrary, it seems likely that logical (mathematical) principles are essentially the same in their mode of validation as scientific principles; they appear to be merely invented rules of symbolic manipulation which have been found by trial in a great variety of situations to mediate the deduction of existential sequences verified by observation. Thus logic in science is conceived to be primarily a tool or instrument useful for the derivation of dependable expectations

regarding the outcome of dynamic situations. Except for occasional chance successes, it requires sound rules of deduction, as well as sound dynamic postulates, to produce sound theorems. By the same token, each observationally confirmed theorem increases the justified confidence in the logical rules which mediated the deduction, as well as in the "empirical" postulates themselves. The rules of logic are more dependable, and consequently less subject to question, presumably because they have survived a much longer and more exacting period of trial than is the case with most scientific postulates. Probably it is because of the widespread and relatively unquestioned acceptance of the ordinary logical assumptions, and because they come to each individual investigator ready-made and usually without any appended history, that logical principles are so frequently regarded with a kind of religious awe as a subtle distillation of the human spirit; that they are regarded as never having been, and as never to be, subjected to the tests of validity usually applicable to ordinary scientific principles; in short, that they are strictly "self-evident" truths. . . . As a kind of empirical confirmation of the above view as to the nature of logical principles, it may be noted that both mathematicians and logicians are at the present time busily inventing, modifying, and generally perfecting the principles or rules of their discipline. . . .⁵⁵

The final sentence concluded with a reference to the 1935 edition of Principia Mathematica. The logical positivists had greeted this work with enthusiastic acclaim, but had viewed it in line with the principle of tolerance as an example of the freely chosen, conventional character of logical systems. Hull, on the other hand, viewed it as part of a long-term process of trial-and-error, a process in which old logics are superseded by new systems of superior empirical validity.

Two other features of this passage call for comment. First, the only substantive change in the passage from its

appearance in the 1940 volume was the insertion of the world "invented" into the first sentence. This change appears to have been a concession to the conventionalism of the logical positivists,⁵⁶ although it was unaccompanied by any softening of the claim for logic's empirical status. The second, and related, comment is that the passage makes no reference to Hull's notion that logic arises in individual organisms as sequences of symbolic habit acts conditioned to causal sequences in the world. By 1943, Hull had dropped his plans to publish a behaviorist epistemology and was no longer even alluding to it. In public, he accepted instead the idea that logical rules arise through invention, but continued to maintain that their validity could be assessed only through empirical test.

The Hullian View of Logic

As we have seen, Hull believed that scientific principles and logical principles were strictly on a par with respect to their epistemological standing. They were viewed alike as subject to validation through empirical trial-and-error. Sharing the same underlying knowledge mechanism, they were also alike in having empirical content. And being composed of pure stimulus acts or the equivalent social symbols, they were equally material entities. For all its complexity and power, logic required

nonmaterial mediation no more than theory.

The relationship between theory and logic was an intimate one, for it was by means of logic that the propositions making up a theory could be arranged in their hierarchical structure. In fact, this structure was what made a theory a "system," in Hull's terminology, as opposed to a mere collection of statements. As Hull wrote in summarizing Principles of Behavior, "scientific theory in its ideal form consists of a hierarchy of logically deduced propositions which parallel all the observed empirical relationships composing a science." But the hierarchical organization of theory was no mere matter of logical form. It was a crucial property of a theory which aimed to parallel a hierarchically organized subject matter such as complex adaptive behavior. Just as a machine or organism would need a hierarchy of control to govern its sub-mechanisms, so would a theory of the machine or organism require a system of control to govern the relative action or interaction of its various principles. "The logical procedure," said Hull, "yields a statement of the outcome to be expected if the several principles are jointly active as formulated."⁵⁷

In order to determine a single predicted outcome from the combined action of several principles, the logical system into which they were arranged needed to have the capacity for computing and generating novelties. Hull did

attribute this capacity to logic. In his paper of 1930 on "Simple Trial-and-Error Learning," Hull wrote:

The deductive process is a true generative activity. The known principles give rise to new knowledge as the result of a causal sequence in a high-class re-integrative organism. According to one plausible hypothesis, principles are symbolic habits which, as a result of their functional interaction within the organism possessing them, give rise to new and distinct habits. These latter constitute the new knowledge. Thus the new knowledge, while derived from the original principles, is not the principles, but something newly come into existence. By the accumulation of these bits of deductive explanation, scientific systems become enlarged very much as have systems of mathematics.⁵⁸

Being aware in 1930 of the work of Leibniz,⁵⁹ Hull was probably encouraged in his views by Leibniz's notion of logic as a universal computational calculus. Certainly Hull's logic machine of some fifteen years earlier would have enabled him to think of logic as generative yet strictly material. Furthermore, Hull's immersion in British associationism would have disposed him to think of logic as having empirical content; indeed, he cited John Stuart Mill's Logic in a paper of 1935.⁶⁰ In any case, for Hull neither the material nature nor the empirical content of logic disqualified it from having the capacity to generate genuine novelties.

From what has been said, it should now be evident that, according to Hull, logic possessed all the critical characteristics of scientific theory proper (and, for that matter, those of machines and organisms). It was obviously hierarchical in form and was seen as having evolved through

trial-and-error. Being a system of symbolic habits it was a strictly material phenomenon, and since symbolic habits were the computational units of mental action, it was also generative. By framing theories in logical form, the scientist not only made them testable but also imparted to them a generative capacity in accordance with the general causal structure of a machine-like world.

To summarize: For Hull, logic was non-tautological. In terms of content, it was therefore capable of being generative, because it embodied empirical regularities. Logic was nonetheless neutral because it was machine logic. That is, it was a kind of machinery of material events, it could be built into a literal machine, and it paralleled the causal sequences of the world-machine. Most important was the fact that it was manifested in pure matter. As such, it was as objective, as free of ghosts, spirits, entelechies, moral judgment, and bias as was any machine. The conception of logic embedded in Hull's thought reached for the best of both worlds: the empirical content of Mill's logic and the neutrality, the power of arbitration, found in the Vienna Circle's conception of logic. The equation of organismic logic with machine logic meant psychologism without subjectivity.

One of the usual objections to a psychologistic interpretation of logic was that it would make logic subjective. One response to this unfortunate implication of psychologism

was to suggest, as did Holt, that logical propositions are ontologically neutral entities. But Hull had no need for such a notion. By giving logic a psychologistic interpretation in terms of behavior rather than introspective psychology, he felt that he had overcome the traditional difficulties of psychologism. The notion of logic as a material machinery made it objective for Hull. His materialism and mechanical metaphor, however, were metaphysical views which others could share or not share. Given his metaphor, Hull could have his logic both ways, objective and psychological, but his metaphysics could not force consensus outside its own boundaries. Unfortunately for Hull, the philosophical consensus of his time tended to favor the anti-psychologism of the logical positivists rather than any sort of metaphorically based neo-psychologism.

Conclusion: Hullian Logic versus Vienna Circle Logic

In previous chapters we have seen that Hull shared with the logical positivists a great stress on logic and its scientific applications. The present chapter, on the other hand, has revealed a number of important respects in which Hull's view of logic differed from that of the logical positivists. The philosophers who associated with the Vienna Circle drew a fundamental distinction between logical propositions and scientific propositions; the former were viewed, unlike the latter, as tautologous, empirically

empty, and not subject to empirical validation. For Hull, the propositions of science and logic were on an equal footing in terms of their epistemological standing; they were equally non-tautologous, equally bearers of empirical content, and equally subject to the test of experience. In logical empiricism, logical necessity was possible because logic had no empirical content, no dependency on the world of contingent events. In the Hullian system, logical necessity was possible because the empirical world from which it was derived was seen as exhibiting a highly reliable structure of causal determinism. The logical empiricist view of logic as tautological meant that logical conclusions could contain nothing not already present in the premises. The Hullian view of logic as empirical meant that logical processes could generate truly novel conclusions going beyond the content of the premises. All told, Hull and the logical positivists diverged deeply in their perspectives on logic.

On the logical positivist view, the tautological character of logic meant that it was subject only to the test of internal coherence. Like other formal systems, logical systems had to come and go as a whole and could not be adjusted piecemeal through a process of trial-and-error. To be sure, formal systems could be abandoned or adopted at will--this much was guaranteed by the principle of tolerance. The choice between formal systems could be

made, as a matter of convention, according to their relative utility in dealing with scientific systems. But this was the only manner in which formal systems could be made responsive to scientific findings; the system had to be changed at the root, not merely brought into alignment with empirical results through partial adjustments. For purposes of reconstructing the highly developed, largely formalized theories of the physical sciences, the logical positivist view of formal systems seemed at least plausibly appropriate. In less developed sciences like psychology, such a view was out of place. For all its shortcomings, the Hullian view was inherently more appropriate in the realm of psychological theorizing. Genuine reconstructions were simply not applicable to the relatively crude and unsystematic formulations which passed as "theory" in most of psychology. In their inchoate state, these formulations needed the flexibility of low-level trial and error, not the rigid structure of formal systems. Somewhat incongruously, Hull recognized this point--even at the peak of his interest in formalization--when he called attention to the tentative, trial-like character of the Mathematico-Deductive Theory.⁶¹ Thus, despite his considerable interest in formal technique, Hull disavowed any intention of using logic to finalize and reconstruct his system. Logic was viewed, rather, as an instrument of ongoing research, as a technique "which will prevent the

freezing of a system."⁶² Writing in the appendix of the Mathematico-Deductive Theory, Fitch was no doubt reflecting Hull's opinion as well as expressing his own when he asserted that "symbolic logic is a tool not only for verification of the old but for discovery and invention of the new."⁶³

Given Hull's views on logic, it is not surprising that the works on logic to which he referred in his published writings were not those of the logical empiricists. He did, of course, cite Principia Mathematica and Woodger's The Axiomatic Method in Biology, but by far the logic treatise most often cited by Hull--and one which he recommended to his readers--was Dewey's Logic, The Theory of Inquiry. Dewey's interpretation of logic was much more in accord with Hull's own and indeed may have helped shape it.⁶⁴ In a passage which Hull cited, Dewey wrote of the laws of logic that "like mathematical axioms, their meaning, or force, is determined and tested by what follows from their operative use."⁶⁵ Like his characterization of scientific theories as leading principles, Dewey's view of logic emphasized its capacity to guide the conduct of investigation and hence its testability by experience, at least in a broad sense. The kinship of this conception of logic to Hull's helps to highlight the fact that while Hull's stress on formalism was great relative to other psychologists, it was not great relative to that of the

logical positivists. Hull's reputation for formal rigor among psychologists was in large measure a product of his propagandizing and rhetoric; in actual practice, his orientation was not far from the pragmatistic outlook that was common in American psychology.

Despite his avowed interest in logic, Hull never acquainted himself with it in any depth. As we have seen, he accordingly brought in Fitch to apply symbolic logic to the rote learning theory. Since Fitch was the one logician who worked most closely with Hull, it is of interest to note that he too rejected the interpretation of logic as an empty tautology. In Fitch's own words:

I was not myself very sympathetic with logical positivism because it seemed to me to be a too naive application of logic to the great and deep problems of philosophy, and in fact it largely ignored most of these problems, treating them as imaginary problems or delusions. The idea seemed to be that logic was, in some overly simple way, to be exploited in seeking answers to these great problems (as far as they could be answered), and that logic itself had no real content, but that statements of logic were essentially empty. How logic could both be the key to these problems and still be empty, I never understood. . . . My own view was that logic has real and important content and is a powerful tool in dealing with all problems in all the sciences, including psychology, and also in philosophy.⁶⁶

Fitch had been influenced in these views by his teacher F. S. C. Northrop, another logician to whom Hull had been exposed at Yale. On the whole, then, the philosophers whom Hull was reading and having contact with were not of a logical positivist persuasion. Their conceptions of science were much closer to his own.

At root, the greatest difference between Hull's view of logic and that of the logical positivists was that Hull insisted on a psychologistic interpretation. Under the influence of Frege and the early Wittgenstein, the logical positivists had equally insisted on an anti-psychologistic interpretation, and this stance was not among the many positions which underwent liberalization during the late thirties and forties. The related distinctions of necessary-contingent, analytic-synthetic, and logical-scientific were so fundamental to logical positivism--or even the modified logical empiricism--that to abandon them was to give up on the entire enterprise. To agree with Quine's 1951 attack on the "dogma" of the analytic-synthetic distinction was to no longer be a logical empiricist.

To be sure, the logical positivists had a place for the empirical study of the use of linguistic forms, including logic. Such an undertaking would be assigned to the realm of scientific investigation, in the branch of psychology or linguistics, but the task was in no case to be confused with that of logic proper. Logic was more fundamental than science; it was the very basis for dividing the universe of propositions into the logical, the scientific, and the meaningless. For Hull, the situation was quite the other way around. With the perspective of nineteenth-century naturalism, Hull felt that all philo-

sophical issues, in particular those having to do with knowledge, could be resolved by submitting them to science, in particular to psychology. As is amply evident, Hull had no qualms about extending this strategy to logic itself. His behavioral psychology of logic was intended not as a mere description of the conditions under which logic is acquired and used but rather as a primary account of the genesis and ultimate nature of logical processes.

Relevant to the present context, there can be discerned among the historical variants of psychologism three distinct types. First, there was Mill's psychologism, in which logic was taken to be an empirical description of the laws of thought. These laws were in turn construed as introspectible regularities in the succession and association of ideas. Second was the physiological psychologism of such nineteenth-century materialists as Heinrich Czolbe.⁶⁷ According to this brand of psychologism, logic was simply a matter of neural mechanisms which, in some unspecified way, yield concepts, judgments, and inferences from perceptual input. Finally, there was the newer behavioristic psychologism, of which Hull's version was one example. The anti-psychologism of Frege and Wittgenstein, and hence of the logical positivists, was directed against the first two of these types.⁶⁸ There is no evidence that the logical empiricists ever passed judgment on the behavioristic version of psychologism,

and--especially since Hull suppressed his own version-- they may have never even come into contact with such a view. But, despite their sympathies with behaviorism, it is doubtful that this psychologism of the third kind would have been any more acceptable to them than the other kinds. The only behaviorist thesis they were committed to--namely logical behaviorism--was after all a logical thesis; that is, it was a peculiar "behaviorism" based on the demands of logic, and was far from a logic based on behavior. Their strict distinction between the propositions of logic and those of science ruled out any brand of psychologism, no matter how objective and rigorous the psychology on which it was based.

In sum, Hull's adherence to a psychologistic interpretation of logic placed a deep gulf between his view of science and the logical empiricist view. Despite the otherwise remarkable parallels between those views, this gulf ran deep because it was a profound difference on the very topic--logic and its application to science--which underlay the parallels. Paradoxically, the very emphasis which Hull and the logical positivists shared most was also the most profound, if not conspicuous, source of their divergence. When Hull asserted that in science "the integrative medium is a structure of implicative pure stimulus acts," he was stating a notion very different from what the logical empiricists were advancing as the

unity of science through logic. All in all, there was little in Hull's relationship with logical positivism to warrant the claim that his conception of science "owes a great deal to the work of Moritz Schlick and other members of the Vienna Circle."⁶⁹

Notes for Chapter 8

1. Clark L. Hull, "Psychology of the Scientist: IV. Passages from the 'Idea Books' of Clark L. Hull," Perceptual and Motor Skills 15 (1962): 807-882, on pp. 823, 822.
2. Ibid., pp. 823, 822.
3. Clark L. Hull, "Knowledge and Purpose as Habit Mechanisms," Psychological Review 37 (1930): 511-525, on pp. 511-514.
4. Ibid., p. 514.
5. Ibid., p. 515.
6. Hull, "Goal Attraction and Directing Ideas Conceived as Habit Phenomena," Psychological Review 38 (1931): 487-506, on p. 502. The pure stimulus act is perhaps better known today as the anticipatory goal reaction, into which it evolved.
7. Hull, "Knowledge and Purpose," pp. 516-517.
8. Clark L. Hull, "The Mechanism of the Assembly of Behavior Segments in Novel Combinations Suitable for Problem Solution," Psychological Review 42 (1935): 219-245.
9. Clark L. Hull, "Simple Trial-and-Error Learning: A Study in Psychological Theory," Psychological Review 37 (1930): 241-256, on p. 517; "Mechanism of Assembly," p. 231.
10. Edward C. Tolman, "The Determiners of Behavior at a Choice Point," Psychological Review 45 (1938): 1-41, on p. 13-14. Tolman added: "I have, therefore, the greatest respect for them. . . . I find myself continually being intrigued and almost ready to change my mind and accept them and Hull after all" (ibid.).
11. Hull, "Knowledge and Purpose," pp. 517-518, 523, 525.

12. Competition or mutual interference among items in a series is commonly invoked by psychologists to explain order effects such as the serial position effect. In this phenomenon, the learning of items at the ends of a list is better and more rapid than the learning of items near the middle.

13. Clark L. Hull, "The Concept of the Habit-Family Hierarchy and Maze Learning," Psychological Review 41 (1934): 33-52 and 134-152.

14. Ibid., p. 39.

15. Hull, "Behaviorism: Seminar Notes, 1925" (pages not numbered), Clark L. Hull Papers, Sterling Memorial Library, Yale University, New Haven, Conn. (subsequent references to this collection will be identified only by title, volume number, and pages); A. P. Weiss, A Theoretical Basis of Human Behavior (Columbus, Ohio: R. G. Adams, 1925) p. 4.

16. Hull, Idea Book X," p. 75.

17. Whether there is actually anything circular about the situation and, if so, whether it is a vicious circularity are important issues which will be considered in Chapter 10 below. Hull seemed to think of it as circular but not problematically so. He approved of the notion, which he attributed to C. I. Lewis, that "circularity is the characteristic of a good logical system--it is consistent with itself" ("Psychology of the Scientist," p. 846).

18. Hull, Idea Book IX," p. 195.

19. Ibid., pp. 198, 196.

20. Ibid., p. 197.

21. Ibid., p. 200.

22. Hull, "Psychology of the Scientist," p. 823.

23. Hull, "Knowledge and Purpose," p. 517.

24. Hull, "Psychology of the Scientist," pp. 842, 843.

25. Ibid., p. 851.

26. Hull, "Seminar Notes: Institute of Human Relations," p. 150.
27. Clark L. Hull, Principles of Behavior: An Introduction to Behavior Theory (New York: D. Appleton-Century, 1943), pp. 6-7.
28. Hull, "Psychology of the Scientist," pp. 841, 844.
29. Clark L. Hull, "The Conflicting Psychologies of Learning--A Way Out," Psychological Review 42 (1935): 491-516, on p. 495. As was mentioned in Chapter 1, there is a widespread impression that Hull believed in "crucial experiments" in the classic sense of experiments that can decide between competing theories. Given Hull's general views on logic and scientific method, this presumption is not groundless. It appears, however, to be incorrect.
30. In fact, Hull asserted that if Newton had been Robinson Crusoe he would have needed logic just the same (Principles of Behavior, pp. 8-9). Perhaps not coincidentally, Skinner two years later argued against the notion of truth-by-agreement by claiming that a Robinson Crusoe would need operationism as much as anyone else.
31. Hull, "Knowledge and Purpose," pp. 523, 512.
32. Hull, "Psychology of the Scientist," pp. 841-842.
33. Ibid., p. 846.
34. Ibid., p. 841.
35. Hull, "Habit-Family Hierarchy," pp. 40-41.
36. Hull, Principles of Behavior, p. 5.
37. Ibid., p. 7.
38. See Rudolf Carnap, "Intellectual Autobiography," in The Philosophy of Rudolf Carnap, ed. Paul A. Schilpp (LaSalle, Ill.: Open Court, 1963), pp. 1-84, on pp. 60-61.
39. See, for example, Otto Neurath, "Protocol Sentences," in Logical Positivism, ed. A. J. Ayer (New York: Free Press, 1959), pp. 199-208.

40. Hull, Principles of Behavior, p. 381; "Psychology Seminar Memoranda, 1939-1940," p. 7; "Memoranda, 1940-1944," p. 3. The latter two references are bound mimeographed volumes in the Sterling Memorial Library, Yale University, New Haven, Conn.
41. Clark L. Hull, "Clark L. Hull," in A History of Psychology in Autobiography, vol. 4, ed. Edwin G. Boring, Herbert S. Langfeld, Heinz Werner, and Robert M. Yerkes (Worcester, Mass.: Clark University Press, 1952), pp. 143-162, on p. 151. The evolved character of Hull's conditioning machines has been noted by Alphonse Chapanis, who remarked that "Hull's model actually went through some evolutionary changes." See his "Men, Machines, and Models," in Theories of Contemporary Psychology, ed. Melvin H. Marx (New York: Macmillan, 1963), pp. 104-129, on p. 119.
42. Hull, Principles of Behavior, p. 12.
43. Clark L. Hull, "A Functional Interpretation of the Conditioned Reflex," Psychological Review 36 (1929): 498-511, on p. 498.
44. Hull, "Psychology of the Scientist," p. 815.
45. Sigmund Koch has stated that the "Hullian robot" played "an important heuristic role in Hull's thinking" and that an early draft of Principles of Behavior "had the robot frolicking about through the course of an entire chapter." See Sigmund Koch, "Clark L. Hull," in Modern Learning Theory, by William K. Estes, Sigmund Koch, Kenneth MacCorquodale, Paul E. Meehl, Conrad G. Mueller, Jr., William N. Schoenfeld, and William S. Verplanck (New York: Appleton-Century-Crofts, 1954), pp. 1-176, on p. 16. However, the published version of the book contained only a brief discussion of the robot (Hull, Principles of Behavior, p. 27).
46. Recent philosophies of science have stressed the role of heuristics, metaphors, and analogies in the process of scientific discovery. See Chapter 10 below.
47. Hull, "Idea Book IX," p. 195.
48. Hull, "Idea Book X," p. 81.
49. Hull, "Idea Book IX," p. 197.
50. Hull, "Idea Book XIX," p. 86.

51. Clark L. Hull, A Behavior System (New Haven, Conn.: Yale University Press, 1952), p. 341.
52. Pierre Duhem, The Aim and Structure of Physical Theory, trans. Philip P. Wiener (New York: Atheneum, 1962), Chapter 6. This work was originally published in 1914.
53. Hull, "Idea Book XIX," pp. 84-86.
54. Clark L. Hull, Carl I. Hovland, Robert T. Ross, Marshall Hall, Donald T. Perkins, and Frederic B. Fitch, Mathematico-Deductive Theory of Rote Learning: A Study in Scientific Methodology (New Haven, Conn.: Yale University Press, 1940), p. 7.
55. Hull, Principles of Behavior, pp. 13-14. Hull also claimed that definitions require empirical validation of the same sort (Mathematico-Deductive Theory, p. 4).
56. The conventionalistic view of logic was not altogether new to Hull. As early as 1931, he spoke of the laws of logic as "elaborate symbolic inventions," but this construal was never typical of his views on logic (Hull, "Idea Book XIII," p. 10).
57. Hull, Principles of Behavior, p. 381.
58. Hull, "Trial-and-Error Learning," p. 242. A "redintegrative" organism was one capable of forming conditioned reactions to any of the various stimuli impinging on its receptor organs; it was thus the rat-runner's equivalent of the associationist's tabula rasa.
59. See Hull, "Psychology of the Scientist," p. 836.
60. Hull, "Mechanism of Assembly," p. 220.
61. Hull et al., Mathematico-Deductive Theory, pp. 12, 305-308.
62. Ibid., p. 308.
63. Ibid., p. 310.

64. Hull taught at Columbia University in the summer of 1929 and may have come under Dewey's influence at that time. Ernest Hilgard has claimed that the Hullians looked for philosophical guidance to Dewey much more than to the logical positivists. Hilgard regards the influence of the logical positivists on Hull as so slight that he has never drawn a connection between them in his own writings (personal interview, Los Angeles, 25 August 1981).

65. John Dewey, Logic, The Theory of Inquiry (New York: Henry Holt, 1938), p. 157. This passage is cited by Hull in Mathematico-Deductive Theory, p. 7. Dewey participated in the Unity of Science movement, but, significantly, his pragmatism conflicted with the formal emphasis of many of its members. In 1941, he expressed his desire "to keep the movement from foundering on the sands of formalism" (John Dewey to Charles W. Morris, 24 April 1941, Unity of Science Collection, Regenstein Library, University of Chicago, Chicago, Ill.).

66. Frederic B. Fitch to Laurence D. Smith, 4 December 1980.

67. See Hans Sluga, Gottlob Frege (London: Routledge and Kegan Paul, 1980), pp. 26-32.

68. Ibid., p. 31.

69. The Encyclopedia of Philosophy, s. v. "Psychological Behaviorism," by Charles Taylor.

CHAPTER 9

B. F. SKINNER: RADICAL BEHAVIORIST PSYCHOLOGY OF SCIENCE

If Hull's claim to rigor in theoretical matters was in part responsible for his prominence among psychologists during the 1930s and 1940s, then it was also partly responsible for his loss of influence during the 1950s. By the mid-fifties, research in the Hullian tradition, including Hull's own A Behavior System (1952), was clearly retreating from its previously grander aims for comprehensive theory. Sigmund Koch's 1954 critique¹ of Hullian theory documented its shortcomings in massive technical detail, and by the end of the decade, researchers in the behaviorist tradition were beginning to look toward an approach that was free of the sort of cumbersome theoretical structure found in the Hullian system. The approach that was just then starting to take over the position of dominance in behaviorism was that of B. F. Skinner.

The transition of reigning influences in behaviorism was a remarkable one, for it constituted in many respects a complete reversal of guiding attitudes toward the nature of science and scientific method. The deep differences between Hull and Skinner in their views of science were rooted in their respective backgrounds and were already evident in their earliest thoughts on behaviorism. Although

Skinner was born twenty years later than Hull, they both came to behaviorism in the late twenties and published their first papers on animal behavior within a year of each other.² But whereas Hull was guided by a Newtonian model of science, Skinner adopted a Baconian-Machian model. This difference was manifested in their views on many issues: the nature of explanation, the value and role of theory, the use of unobservables, and scientific method itself. On the deductive-inductive dimension, Hull stood very close to the deductive pole and Skinner was equally close to the inductive pole. Because of Skinner's inductivist, radical empiricist approach, his work enjoyed very little popularity during psychology's Age of Theory; but once the elaborate theoretical systems began to fall from favor in the fifties, Skinner's approach made him well-suited to succeed Hull as the dominant figure in behaviorism.

Since the 1950s, Skinner's influence on American psychology has been monumental. Laboratories and journals devoted to operant conditioning have thrived, and Skinnerian research has been applied to a wide variety of settings in programs of "behavior modification." Skinner has advocated broader applications of conditioning research, such as the design of cultures, and his views have provoked interest and debate both within and outside of the scholarly world. He has exercised his literary skills in

producing two popular works, the Utopian novel Walden Two (1948) and the controversial best-seller Beyond Freedom and Dignity (1971).³ His ideas are known throughout much of the world, and it has been conjectured that history will judge him to have been "the major contributor to psychology in this century."⁴

Skinner's Background and Turn to Behaviorism

Background

Skinner was raised in Susquehanna, Pennsylvania, where his father worked as a lawyer. As a boy, he derived considerable enjoyment from building various toys and devices, thereby acquiring skills which he later put to use in constructing the pieces of psychological apparatus he is now well known for.⁵ Skinner's parents encouraged him in his schoolwork, and he was rather well read even as a youth. In school, he especially enjoyed literature and biology. During the eighth grade, Skinner's interest in the hypothesis that the works of William Shakespeare were written by Francis Bacon led him to read some of Bacon's works. Skinner reports that although he did not grasp Bacon well at the time, Bacon "was to serve me in more serious pursuits later on."⁶ While in high school, he delved into Darwin's Voyage of the Beagle (1845) and The Expression of the Emotions of Man and Animals (1872),

the latter of which was Darwin's major work on psychology.⁷

Skinner went to Hamilton College in New York state. There he continued to read widely and to pursue his interests in literature and biology. His contact with psychology as an undergraduate was limited to a course in "Philosophy, Psychology, and Logic" taught by a former student of Wilhelm Wundt. Skinner has recounted that "the only psychology in the course was a brief demonstration of the two-point limen, in which Dr. Squires applied a pair of dividers to his forearm and quickly returned them to his desk."⁸ In a development far more relevant to his eventual career, Skinner was directed by a professor of biology to the works of Jacques Loeb. Loeb was a German-American biologist whose research on, and mechanistic interpretation of, animal motion made him an important forerunner of behaviorism. Although Skinner had no explicit interest in psychology at the time, he was "impressed by the concept of tropism or forced movement."⁹

Loeb's influence would not become manifest for some time. Skinner's immediate future was to be determined by his literary interests. He began to write a fair amount of fiction and poetry, and gradually became known as an "aspiring young poet." During one of his college summers, he attended a writing school at Bread Loaf, Vermont. There he met Robert Frost, who read some of Skinner's fiction and encouraged him in his efforts. This encouragement

proved to be a determining factor in Skinner's decision to try out a career in writing upon graduating from college. He had planned to spend a year writing a novel, but found that he had nothing to write about and suffered through what he would later refer to as his "Dark Year." After his unsuccessful attempt at writing fiction, he undertook, at his father's suggestion, the task of writing a digest of legal decisions. The work was tedious but it yielded two benefits: the job of classifying grievances and decisions turned out to be "not far from a Baconian classification of scientific facts" and thus spurred his interests in science; and the work produced royalties which would enable him to return to school for graduate work.¹⁰

Skinner considered the possibility of pursuing graduate study in English, but eventually settled on psychology instead. The choice of psychology followed Skinner's realization that what intrigued him about literature was actually human behavior, a topic that could be approached more suitably through science. Skinner was told by a friend that "science is the art of the twentieth century," and Skinner took the notion seriously. This idea was later reinforced when Skinner read an article by H. G. Wells in which Wells posed the dilemma of whom should be saved if George Bernard Shaw and Ivan Pavlov were drowning on opposite sides of a pier. Wells opted for Pavlov, and so did Skinner.¹¹

Turn to Behaviorism

Skinner's undergraduate coursework in philosophy had failed to arouse his interest, but the scientifically oriented philosophical writings of Bacon had appealed to him. He had discovered Bacon through literature, and it was again his literary interests that led him to discover Bertrand Russell and, as a consequence, Pavlovian reflexology and Watsonian behaviorism. The Dial, a favorite literary magazine of Skinner's, published in 1926 a book review in which Russell referred to Watson's Behaviorism (1924) as "massively impressive." Skinner subsequently purchased and read Behaviorism and Russell's Philosophy (1927).¹²

Philosophy was one of Russell's lesser-known works, but it was to have a crucial and lasting impact on Skinner. As Skinner has described the book, "it begins with a careful statement of several epistemological issues raised by behaviorism considerably more sophisticated than anything of Watson's."¹³ But the book was more than just a statement of issues. It was a lengthy, detailed, and direct application of Watsonian behaviorism to the traditional problems of epistemology. Russell argued that in most respects behaviorism provided an adequate and fruitful account of both ordinary and scientific knowledge. Scientific knowledge was to be regarded as the end product of a chain of processes consisting of perceptions, memory, testimony, and inference. Each of these links in the chain was the subject of a chapter in Philosophy.

Conceiving perception in a behavioristic fashion, Russell regarded it as a type of sensitivity, not unlike that exhibited by scientific instruments. Animals, including scientists, were acknowledged to differ from instruments in possessing learned reactions, but Russell believed that this factor did not crucially complicate the situation. Memory, the second stage in the knowledge process, was said by Russell to be essentially the recurrence of a conditioned response under appropriate stimulus conditions. Testimony, regarded as the linguistic behavior of scientists, was viewed as a requisite for establishing the intersubjectivity of scientific knowledge. Russell saw no difficulty with the behaviorist interpretation of language, calling Watson's treatment of words as conditioned stimuli "the only satisfactory way to treat language."¹⁴ Finally, inference or induction was said to be indispensable for science simply because "every scientific law is established by its means."¹⁵ Just as Hume had attributed induction to "animal habit," Russell claimed that "as a practice, induction is nothing but our old friend, the law of conditioned reflexes or of association" and that "scientific induction is an attempt to regularize the above process, which we may call 'physiological induction!'"¹⁶ All told, Russell felt that behaviorism could account, more or less, for the major epistemological processes. He completed this picture by giving knowledge as a whole

a behavioristic interpretation:

In ordinary life, knowledge is something which can be tested by examinations, that is to say, it consists in a certain kind of response to a certain kind of stimulus. This objective way of viewing knowledge is, to my mind, much more fruitful than the way which has been customary in philosophy. . . . [I]f we wish to give a definition of "knowing," we ought to define it as a manner of reacting to the environment, not as involving something (a "state of mind") which only the person who has the knowledge can observe.¹⁷

Within a few years, Russell had completely abandoned his behavioristic approach to knowledge, and Skinner had developed a non-Watsonian behaviorism to apply to epistemological problems. Nonetheless, Skinner's reading of Philosophy had two important effects. First, it led him to seriously consider behaviorism at a time when his psychological allegiances had not yet crystallized; "[Russell] had taken Watson seriously," wrote Skinner, "and so did I."¹⁸ Second, and more importantly, Russell's application of behavioral psychology to the problem of knowledge provided a model which Skinner has followed ever since. The details of Russell's account were soon thereafter rejected by Skinner, but the general notion of developing an empirical epistemology from a behaviorist basis has been a continuing theme throughout Skinner's career. As will be discussed below, Skinner's first book, begun in the early 1930s, was his (unpublished) "A Sketch for an Epistemology." He has since referred to an empirical theory of knowledge as his "first love" and has noted that

"I came to behaviorism . . . because of its bearing on epistemology, and I have not been disappointed,"¹⁹

By 1928, then, Skinner had decided that he would pursue a career in psychology--even though he had never had a course in it--and that he would approach it as a behaviorist with an eye toward epistemological issues. The royalties from the legal digest would finance his graduate education, and it remained only for him to choose a school. Without knowing anything about psychology at Harvard, he decided to go there on the advice of a former biology professor who had sent some of his premedical students there. Skinner arrived at Harvard in the fall of 1928, bringing with him the three books that he felt had prepared him for a career in psychology: Russell's Philosophy (1927), Watson's Behaviorism (1924), and I. P. Pavlov's Conditioned Reflexes (1927).²⁰ The further influences he came under at Harvard would serve to reinforce the biological and positivistic orientation that he was already developing at the time of his arrival. In Darwin and Loeb and in Bacon, Skinner had encountered the sorts of ideas which, when put together, would constitute the intellectual framework that guided his career. By the time he left Harvard, that framework had become fully intact and was being applied energetically to the problems of psychology.

Intellectual Roots: Biology and Positivism

Machian Positivism and Biological Economy

In reading Bacon, Skinner had been exposed to a view of science which emphasized observation, classification, the gradual inductive establishment of laws, and the avoidance of hasty overgeneralization and metaphysical dogma. Skinner's sympathies with such a view were greatly strengthened and refined by his reading of Ernst Mach during his graduate years at Harvard. While taking a course in the history of science, he was directed to Mach's Science of Mechanics (1883).²¹ Around the same time, he also read some of the works of Henri Poincaré and P. W. Bridgman's Logic of Modern Physics (1927), but it was Mach's work that served as a model for Skinner's doctoral dissertation of 1930 and, as we shall see, as a basis for his own positivistic views of science.

In the Science of Mechanics, Mach traced the development of mechanics from its primitive origins to its contemporary status. Arguing from a wealth of examples, he showed that physical concepts such as force have arisen from experiences in everyday work and craftsmanship and then been extended and refined through the historical evolution of scientific mechanics. For Mach, the development of mechanics exemplified the continuity between science and the practical commerce of humans in their environ-

ments. The study of its history could reveal the contingent, historically conditioned character of its concepts and laws, and make possible the salutary distinction between their genuine, experiential import and the superfluous metaphysical meaning that had been imputed to them. As Mach wrote in the preface of the Science of Mechanics, the aim of the treatise was to "clear up ideas, expose the real significance of the matter, and get rid of metaphysical obscurities."²²

Skinner's dissertation was a polemic urging that the concept of the reflex be extended to the behavior of intact organisms. All such behavior, he claimed, could be "adequately embraced" by a suitably clarified concept of the reflex.²³ The inspiration for this approach came from Bertrand Russell, who, according to Skinner, "pointed out that the concept of the reflex in physiology had the same status as the concept of force in physics."²⁴ Having read Mach and Bridgman, Skinner was receptive to Russell's claim and prepared to defend it in his dissertation. The first half of the dissertation applied to the reflex concept a historio-critical analysis, the method and aim of which were explicitly drawn from Mach. Skinner wrote in his introduction that

the chief advantage, first exploited in this respect by Mach, lies in the use of a historical approach. . . . [N]o attempt is made to give an exhaustive account of the history of the reflex. Certain historical facts are considered for two reasons: to discover the nature of the observations upon which the concept has been based; and to indicate the source of the incidental interpretations with which we are concerned.²⁵

As with Mach, Skinner's use of history as a tool for the clarification of concepts included the positive function of clarifying the experiential origin and basis of concepts and the negative function of disclosing their inessential metaphysical components. Skinner concluded that the observational import of the reflex was nothing more than an observed correlation of stimulus and response; the "incidental interpretations" that had been ascribed to the reflex were that it was "involuntary," "unlearned," and "unconscious."²⁶

Skinner's historical method was thus adopted directly from Mach, but more importantly it was used in the service of a broad positivistic view of science which was itself drawn largely from Mach. Skinner had read other positivists, but in terms of his overall approach to science, Mach had the major impact. It is no exaggeration to say that Skinner was profoundly influenced by Mach,²⁷ and the signs of that influence are spread throughout Skinner's work from 1930 on. The dissertation set the pattern for what was to come.

In what follows, it will be shown that the major features of Skinner's view of science can all be found in

the writings of Mach. The discussion will be organized along several themes, which, taken jointly, indicate the remarkable degree to which Skinner's approach echoes that of Mach. In laying out these themes, no sharp distinction will be drawn between those notions which Skinner adopted directly from Mach and those which Skinner, once having embraced the Machian framework, may have drawn on his own as implications of that framework. In addition to the Science of Mechanics, Skinner read Mach's Analysis of Sensations (1914) and parts of Knowledge and Error (1905), but it was the Mechanics that Skinner read first, and by his own reckoning, had the greatest influence on him.²⁸

The Origins of Science. For Mach, science is an outgrowth of the practical concerns of everyday life. In activities such as hunting and craftsmanship, humans interact with and manipulate their environments, and such actions constitute the rudiments of human knowledge.²⁹ Likewise, Skinner has written that:

. . . the earliest laws of science were probably the rules used by craftsmen and artisans in training apprentices.

When we speak of the knack of a craftsman or the lore of a hunter, we refer to the skillful or informed behavior usually acquired through direct contact with things rather than from instructions or rules. The behavior is not unlike that of other species, and it must have been exhibited by the human species long before language was available to make instruction or the formulation of rules possible.³⁰

Furthermore, according to Skinner, precursors of scientific knowledge in the realm of psychology also lie in skilled behavior.

We speak of similar unanalyzed skills or wisdom in dealing with human behavior. We say, for example, that a salesman "has a way with people," or that a politician "is a good judge of human nature." We mean that he can often anticipate what people will do or even arrange matters so that they will do certain kinds of things. Like the craftsman, he has acquired these skills from direct contact, and he often cannot explain what he does or teach others to do it.³¹

Skilled human behavior is thus the proximate source of science for both Mach and Skinner. But for neither is it the ultimate origin. Both, in fact, trace the roots of knowledge back beyond human history into the biological evolution of animal behavior.

That this is so for Skinner is not surprising. As a behaviorist, he is naturally disposed to viewing the evolution of knowledge, in light of the continuity of species, as a matter for behavioral biology as well as cultural history. Skinner's early sympathy with Russell's application of Pavlov's research on dogs to human epistemology is but one example of his belief in the essential continuity of human and animal knowledge. This belief is evident throughout Skinner's writings.

However, it is perhaps surprising that Mach--who is usually placed in the introspectionist, not behaviorist, tradition--also held such a view. His biological orientation was evident in the Science of Mechanics when he stated that in trying to account for knowledge he "found it helpful and restraining to look upon everyday thinking and science in general, as a biological and organic phenomenon."³²

In 1894, after describing the similarities between animals and humans in reacting to their environments, Mach wrote that "[s]uch primitive acts of knowledge constitute to-day the solidest foundation of scientific thought" and that "knowledge, too, is a product of organic nature."³³ By 1905, Mach had given serious attention to the comparative psychology of C. Lloyd Morgan, Jacques Loeb, and others, and he devoted many pages of his Knowledge and Error (1905) to descriptions of intelligent animal behavior.³⁴ From his excursions into comparative psychology, Mach drew conclusions that would be congenial to most behaviorists: humans and animals form concepts in the same way; the behavior of humans and animals is governed by associations acquired through experience and maintained by their biological utility; and in Mach's own words, "[t]he basic feature of animal and human behavior is [a] rigorously determined regular automatism."³⁵ The study of animal behavior was for Mach integral to a scientific understanding of the activity of the scientist--a type of activity which could be recognized "as a variant of the instinctive activity of animal and man in nature and society."³⁶ In Mach's view, the evolution of animal behavior and the history of physics are but two parts of a single historical line of epistemological development. As he wrote in Knowledge and Error:

Scientific thought arises out of popular thought, and so completes the continuous series of biological development that begins with the first simple manifestations of life.

Animals gather individual experience in the same way as humans. Biology and the history of civilization are equally valid and complementary sources for psychology and the theory of knowledge.³⁷

Associated with Mach's emphasis on the inescapably historical character of knowledge was a particular set of views concerning the nature of science. First, he attributed a larger role in the procuring of scientific knowledge to fortuitous accident than to careful logic.³⁸ Just as animals make and benefit from accidental discoveries, so too "it is by accidental circumstances . . . that man is gradually led to the acquaintance of improved means of satisfying his wants."³⁹ Second, for Mach, history shows that all propositions, even those of science and logic, are contingent because they are based on experience. The tendency to invest theories with "metaphysical garb," to assign a necessary or a priori status to them, is perhaps understandable in light of their "high practical value," but it arises only when their experiential origins are neglected.⁴⁰ Third, the historicity of all knowledge means that an understanding of the knowledge process calls for the investigation of specific instances of scientific discovery. Accordingly, Mach enjoins the epistemologist to study concrete cases of scientific behavior and to exercise caution in generalizing from them.

Fourth, the historical perspective on science reveals it to be undergoing continual revision and refinement. As a historical phenomenon, it is provisional and incomplete. Taken together, these four characteristics of science--its fortuitousness, contingency, particularity, and incompleteness--meant for Mach that science cannot be reduced to a formula or determinate set of methodological rules. Likewise, Skinner has viewed all knowledge as a product of history. In doing so, he has emphasized the same characteristics of science that Mach has, and has drawn the same conclusion that science cannot be captured by any formula, such as the hypothetico-deductive method. That Skinner in fact holds such views will be documented below, when his views are discussed in relation to logical positivism.

Biological Economy in Science. Having conceived of knowledge as primarily a historical and biological phenomenon, Mach proceeded to spell out the various ramifications of this conception for science. Fundamental to these ramifications is his principle of biological economy, a principle which he applied so frequently and consistently that he was accused of "riding his horse to death."⁴¹ Science for Mach is simply the economical description of facts. Economy in science, which amounts to little more than efficiency of practice and simplicity of expression, often comes to be seen as a goal in its own right; but even so it continues to rest on the economical satisfaction

of biological needs. As Mach wrote in the preface to Science of Mechanics:

Economy of communication and of apprehension is of the very essence of science. . . . In the beginning, all economy had in immediate view the satisfaction simply of bodily wants. With the artisan, and still more so with the investigator, the concisest and simplest possible knowledge of a given province of natural phenomena--a knowledge that is attained with the least intellectual expenditure--naturally becomes in itself an economical aim; but though it was at first a means to an end, when the mental motives connected therewith are once developed and demand their satisfaction, all thought of its original purpose, the personal need, disappears.⁴²

Just as the history of science is continuous with animal behavior, so is the intellectual parsimony of science continuous with the economy of biological needs. The organic roots of either may be forgotten, but only at the expense of a decrement in our understanding of science.

Scattered through Mach's works are expressions of skepticism toward attempts to set science apart from the organic world, to picture it as somehow transcending the biological needs in which it is rooted. "Our first knowledge," he says, "is a product of the economy of self-preservation." The economical collection and communication of such knowledge, says Mach, gives us "a clue which strips science of all its mystery, and shows us what its power really is."⁴³ Or again:

The biological task of science is to provide the fully developed human individual with as perfect a means of orienting himself as possible. No other scientific ideal can be realized, and any other must be meaningless.⁴⁴

Or stated more baldly: ". . . the ways even of science still lead to the mouth."⁴⁵ Science constitutes a continuing refinement and extension of biological knowledge, but it does not transcend the demands of economy and survival.

For Skinner, too, scientific activity is a special type of behavior, and all behavior is governed by the contingencies of reinforcement and survival.⁴⁶ Science promotes self-preservation, at the levels both of individual and culture, by providing economical means of fulfilling those contingencies. Just as Mach saw science as contributing to the orientation of the individual, Skinner asserts that the procedures of science benefit the individual in operating on the environment. As he puts it, "What matters to Robinson Crusoe is . . . whether he is getting anywhere with his control over nature."⁴⁷ But the contingencies of survival also operate on cultures:

A culture, like a species, is selected by its adaptation to an environment: to the extent that it helps its members to get what they need and avoid what is dangerous, it helps them to survive and transmit the culture. The two kinds of evolution are closely interwoven. . . .

Survival is the only value according to which a culture is eventually to be judged, and any practice that furthers survival has survival value by definition.⁴⁸

Science for Skinner is one such practice. In particular, an effective science of behavior can promote the survival of culture by enhancing control over survival-threatening behaviors, especially those whose consequences are dangerously remote.⁴⁹

But to be effective in promoting survival, Skinner argues, science must approach its subject matter in the most direct possible way. According to him, this may be achieved by bringing the scientist's behavior of collecting, ordering, and describing observations under the immediate control of relevant aspects of the environment. Science's external purpose of furthering survival translates directly into a set of internal standards for science. Skinner thus follows Mach in emphasizing efficiency of investigation, immediacy of observation, and economy of description and communication as desiderata for science. As it is for Mach, intellectual parsimony is rooted in biological economy. Mach's notion that every uneconomical formulation and superfluous concept or distinction involves a loss is echoed throughout Skinner's writings on science.⁵⁰ Thus, Skinner claims that the purely descriptive approach to science possesses "greater efficiency" than the hypothetico-deductive approach, that the use of unnecessary terms violates "that ultimate simplicity of formulation that it is reasonable to demand of a scientific system," and that the criteria for judging a system "are

supplied principally by the usefulness and economy of the system with respect to the data at hand."⁵¹ Mach's injunction against formulations involving "greater precision than fits the needs of the moment" finds its counterpart in Skinner's warning against making "a fetish of exactitude."⁵² Scientific practices and formulations which are acceptable to Skinner are characterized in such terms as "convenient," "expedient," "effective," "useful," and "economical"; unacceptable practices and formulations are said to be "unpractical," "supernumerary," "unnecessary," and even "clumsy and obese."⁵³ Skinner thus rejects as superfluous, or even harmful any scientific practice which interferes with the activities of observing and describing. Like Mach's positivism, Skinner's is one of biological expedience.

The major features of Skinner's positivistic view of science are all variations on the theme of biological expedience. These features and their kinship to Mach's philosophy of science will be considered in the following.

Cause as Function and Description as Explanation.

According to Mach, "For the investigator of nature there is nothing else to find out but the dependence of phenomena on one another."⁵⁴ Phenomena for Mach always occur in varying relationships of interdependence and are thus naturally described in terms of such dependencies. To adequately describe phenomena is to explain them: "Does

description accomplish all that the enquirer can ask? In my opinion, it does."⁵⁵ Mach recognized that his reduction of explanation to description would appear inadequate to those thinkers for whom "describing leaves the sense of causality unsatisfied."⁵⁶ Most people, he admits are accustomed to conceiving of a cause as pushing or pulling to produce its effects; but such a notion of cause and effect is metaphorical, superfluous, and to be rejected in any final scientific formulation. In the Machian scheme, cause and effect are simply correlated changes in two classes of phenomena, as when a geometer observes changes in the length of a hypotenuse when the opposite angle is varied. The relation of cause and effect can be economically replaced with the notion of a mathematical function.⁵⁷

The Machian views of explanation and causality were directly adopted by Skinner early in his career and have continued to figure prominently in his remarks on science. These views were the basis for Skinner's redefinition of the reflex as an observed correlation of stimulus and response. In his dissertation, he referred to description and explanation as "essentially identical activities" and embraced "that more humble view of explanation and causation which seems to have been first suggested by Mach . . . wherein, in a word, explanation is reduced to description and the notion of function substituted for that of

causation."⁵⁸ As it did for Mach, the functional notion of cause permitted Skinner to eschew the uneconomical and potentially misleading connotations of mechanical causation:

In general, the notion of a reflex is to be emptied of any connotation of the active 'push' of the stimulus. The terms refer here to correlated entities, and to nothing more. All implications of dynamism and all metaphorical and figurative definitions should be avoided as far as possible.⁵⁹

More recently, Skinner has used the Machian notion of causality to argue against the metaphorical interpretation of Darwinian selection as selection "pressure."⁶⁰ In any case, Skinner follows Mach in rejecting nondescriptive forms of explanation and causation because they are seen as impediments to the direct and economical contact between an investigator and a domain of phenomena.

Hypotheses and Theories. Closely related to the Machian reliance on description in science is the view that hypotheses and theories play a limited and inessential role in scientific investigation. Because the final aim of science is complete description, theories and hypotheses can serve only in the role of what Mach calls "provisional helps"; they will become superfluous as the final aim is reached, thereby exhibiting their "self-destroying function."⁶¹ Theories and hypotheses are simply not as economical as mathematical functions in expressing lawful regularities among phenomena.⁶² Furthermore, the interim use of hypotheses carries the risk of their

interfering with the essential process of observation. As Mach puts it: "We err when we expect more enlightenment from an hypothesis than from the facts themselves,"⁶³

Likewise, for Skinner, hypotheses and theories are, first, inessential and, second, likely to lead to ineffective behavior on the part of the scientist. Skinner defines unacceptable theory as "any explanation of an observed fact which appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions."⁶⁴ Any such theory is dispensible, in Skinner's view, because it goes beyond the task of complete description. Theories of behavior couched in physiological terms are rules out as irrelevant for a science of behavior (though they may be useful in their own domain), while theories couched in mentalistic or conceptualistic terms are eschewed as simply otiose.⁶⁵ A descriptive science can proceed without explanatory hypotheses or theories toward its goal of concisely organized facts and inductively established laws.⁶⁶

Skinner admits that "[t]heories are fun," but he insists that the activity of conjecturing is less efficient than that of observing. Conjecturing appeals to the investigator "whose curiosity about nature is not equal to his interest in the accuracy of his guesses."⁶⁷ Moreover, conjecturing is said to be "wasteful," to create

a "false sense of security," and to lead to "useless" and "misdirected" experimentation, "bootless theorizing," and the unnecessary loss of "much energy and skill,"⁶⁸ Not surprisingly, Skinner is critical of Hull's attempt to use "theoretical postulates" instead of observed functional dependencies. With a Machian eye toward economy, he characterized Hull's approach as "harmful," "glancing and ineffective," and "beyond its sphere of usefulness," and he said that it "diverted [Hull] from a frontal attack on crucial issues."⁶⁹ In sum, Skinner has followed Mach in questioning the value of hypotheses and theories on the grounds that their use violates the precepts of intellectual, and ultimately biological, economy.

Truth. Although Mach did not explicitly address the philosophical issue of truth, it should be clear from the foregoing review of his biological conception of knowledge that truth was for him a matter of promoting the adaptation of an individual or species to the surrounding environment. This means that Mach held a view closely related to what later became known as the pragmatist conception of truth--a conception which, in fact, he probably inspired through his influence on William James.⁷⁰ Mach's biographer John T. Blackmore has written that Mach "preferred the pragmatic notion that an assertion was true only to the extent that it satisfied 'human purposes,' 'human biological needs,' or contributed to the 'survival of

the human race or species'."⁷¹ Similarly, Skinner is reluctant to speak of truth, but when he does, he tends to adopt a pragmatic position. Thus, in considering scientific knowledge, he says that "there is a special sense in which it could be 'true' if it yields the most effective action possible." Or again: "An important part of scientific practice is the evaluation of the probability that a verbal response is 'right' or 'true'--that it may be acted upon successfully."⁷² In their shared predilection for dealing in particulars, both Mach and Skinner naturally shy away from offering generalized claims about truth--after all, biological expedience will be variously manifested according to the demands of the specific situation at hand. But their approaches to the issue of scientific truth are obviously closely related.

Psychologistic Epistemology. Being wary of general theories, Mach remained skeptical of broad philosophical accounts of the nature of knowledge. He denied that he was engaged in philosophical pursuits and insisted that knowledge be studied in concrete psychological terms. "Above all there is no Machist philosophy," he wrote. "At most [there is] a scientific methodology and a psychology of knowledge [Erkenntnispsychologie]; and like all scientific theories both are provisional and imperfect efforts."⁷³ In his Knowledge and Error, which was subtitled "Sketches on the Psychology of Inquiry," Mach summarized

his psychologistic, and largely behavioristic, approach to epistemology:

[W]e lay no claim to completeness, and indeed would rather guard against premature philosophizing and systematizing. Let us take an attentive walk through the field of scientific enquiry and observe the detailed behavior of the enquirer. By what means has our knowledge of nature actually grown in the past, and what are the prospects for further growth in the future? The enquirer's behavior has developed instinctively in practical activity and popular thought, and has merely been transferred to the field of science where in the end it has been developed into a conscious method. To meet our requirements we shall not need to go beyond the empirically given. We shall be satisfied if we can reduce the features of the enquirer's behavior to actually observable ones in our own physical and mental life (features that recur in practical life and in the action and thought of peoples); and if we can show that this behavior really leads to practical and intellectual advantages.⁷⁴

Mach believed that a purely empirical epistemology was possible, that philosophical preconceptions were not only unnecessary for the enterprise but a harmful source of prejudice. An epistemology based on economical observation and description would carry with it a methodology, which in Mach's words would be "furthered much more through specific living examples, rather than through pallid abstract formulae that in any case need concrete examples to become intelligible."⁷⁵

Like Mach, Skinner has held that an empirical epistemology can fruitfully be pursued by studying the actual behavior of scientists. As we have seen, he came to this belief, prior to his reading of Mach, under the influence of Russell. At the outset of his graduate

studies at Harvard, Skinner was told by Alfred North Whitehead that a young psychologist should follow developments in philosophy. Skinner replied that "it was quite the other way around--we needed a psychological epistemology."⁷⁶ But Skinner's interests in a psychologistic account of knowledge were no doubt strengthened by Mach's influence.⁷⁷ His approach to epistemology has, in any case, a decidedly Machian character. For both Mach and Skinner, epistemology is a subject for empirical (generally behavioral) psychology rather than for philosophy, it focuses more on concrete instances than on general propositions, and it can provide guidance in methodological matters. The empirical epistemology developed by Skinner in the course of his career will be discussed later in this chapter.

The present section has described several aspects of the close relationship between Mach's and Skinner's positivistic views of science. Some of Skinner's views were directly influenced by Mach and some perhaps only indirectly, but it is clear that his positivism is a Machian positivism. The logical positivists had claimed Mach as a forerunner of their philosophy of science, but Mach's positivism conspicuously lacked the strong formal emphasis found in logical positivism. Whereas the logical positivists pursued their epistemology and their rejection of metaphysics through logical analysis, Mach pursued these ends

through empirical observation and description. Much the same can be said of Skinner: his positivism, like Mach's, is of a descriptive rather than logical variety.

The various specific views that are common to the approaches of Mach and Skinner all constitute little more than elaborations of a biologico-economical conception of knowledge. For both, the intellectual parsimony that constitutes their positivism is an outgrowth of the demands of efficient adaptation. Speculative theories and reified concepts are eschewed in the belief that they would interfere with the direct study of the subject matter at hand and thus lead to ineffective behavior.

If the view of knowledge held by Skinner and Mach is based on intellectual economy, then one might well ask what this notion of economy is based on. Is it not a philosophical principle of the sort that Skinner and Mach claim to avoid? Mach denies that it is. For him, the "principle" of intellectual economy is a contingent, empirical description of biological adaptation. Characterizing the principle of economy, one prominent Mach scholar has written that:

It has no ontological status. It is not a physical law. . . . Mach claims, as Avenarius had done earlier, that the principle of economy of thought is but a description of animal behavior. . . . The evolution of science thus becomes a special case of the larger biological process of self-preservation through mental adaptation.⁷⁸

Refusing as always to appeal to transcendental principles, Mach draws upon description for guidance even in what would appear to be regulative matters.⁷⁹

Once again, Skinner's approach to this issue echoes that of Mach. He regards knowledge as a special kind of behavior and behavior, in turn, is known descriptively to be a product of the adaptive process of shaping by the phylogenetic and ontogenetic contingencies of survival. Just as adaptation in general is most efficient when these contingencies are satisfied in the most economical way, so is scientific knowledge most efficient when it meets its ends through economical observation, description, and communication. Far from being a logical positivism, Skinner's positivism is grounded in biological expedience. It arises from a biological conception of organismic behavior, the same conception that has guided his empirical research on the process of adaptation through operant conditioning. In other words, Skinner's positivistic methodology is indigenous to his deep-seated conception of psychology.

Positivist Biology of Behavior: Loeb and Crozier

As a behaviorist, Skinner naturally views his work as falling in the tradition of biology, and this intellectual heritage is perhaps stronger in his behaviorism than in Tolman's or Hull's. Whereas Tolman and Hull had studied

engineering and philosophy in college, Skinner had studied literature and biology. After arriving at Harvard, he found that his interests and sympathies lay more with physiology than with psychology. Most of his research at Harvard was conducted in the newly established Department of Physiology, and he seriously considered transferring to that department before learning that his research would be accepted for a dissertation in Psychology.⁸⁰

In sketching the history of behaviorism, Skinner has emphasized the contributions of Darwin, Lloyd Morgan, Watson, and Pavlov. From Pavlov, Skinner learned the lesson: "control your conditions and you will see order."⁸¹ But Pavlov had claimed to be studying the cerebral cortex by means of his experiments on the conditioned reflex. Such an inferential treatment of neurophysiology through behavioral studies violated Skinner's Machian insistence on developing accounts in science which remain close to one's observations.⁸² Furthermore, Skinner argued, if behavior itself is orderly, it can be treated as a subject matter in its own right, without appeal to another level of explanation. In these views, Skinner was greatly influenced by his teacher, the Harvard physiologist W. J. Crozier, and indirectly by Crozier's teacher Jacques Loeb. It was the positivistic behavioral biology of Loeb and Crozier more than the comparative psychology of Darwin's immediate successors that Skinner drew upon in his own research.⁸³

Loeb's energetic application of a mechanistic determinism to the biological realm had made him a controversial figure in Europe as well as in America following his immigration in 1890. In the United States, he became one of John B. Watson's teachers and later wrote his influential The Organism as a Whole (1916), a book which Skinner read as an undergraduate.⁸⁴ In that work, Loeb reported his extensive experimental research on tropistic movement and, discussing the implications of tropisms for psychology, suggested that the behavior of higher organisms was simply a sum of stimulating forces in the environment. Furthermore, Loeb believed that these causal factors could adequately be expressed as variables in equations representing functional relations between environment and behavior. The view of causation that Loeb was applying here was taken directly from Mach, whose ideas on science were described by Loeb as a source of "inspiration" and "energy" to him.⁸⁵ Like Mach before him and Crozier and Skinner after him, Loeb insisted that equations of this sort should contain no "arbitrary constants" and that all variables in them must have testable reference to observable phenomena.⁸⁶

Even before his arrival at Harvard, Skinner had thus encountered the notion of cause as function and the use of the behavior of whole organisms as a dependent variable. But Loeb had not been entirely consistent in his positivism. He still tended to speak in physiological terms and to

supplement his descriptive equations with causal explanations in terms of chemical and mechanical metaphors.⁸⁷ As Loeb's disciple, Crozier carried on in the Loebian tradition but sharpened the Machian emphasis in it. In a piece written around the time that Skinner studied with him, Crozier eliminated explanatory metaphors and defined the organism as "a system of relations," which were Mach's relations of functional dependence.⁸⁸ Crozier was extending the tropism conception to mammals, experimentally establishing an expanded set of controlling variables, and stressing the quantitative and predictive power of the functional equations. It was this ultrapositivist form of Loebian biology that Skinner encountered at Harvard. A prominent student of Skinner's has written that:

Crozier seemed to be fleshing out Loeb's stark conceptual framework with quantitative, empirical fact. He contributed more than data, however, for his version of Loebian biology was more distinctive. In Crozier's hands, it became especially mathematical in the sense of functional relations between the physical measures of stimulus and response . . . , concerned more with behavior as behavior rather than as manifestation of something else (such as a nervous system) . . . , and experimental rather than statistical. . . . The line of behaviorist descent as regards actual research passes more conspicuously from Loeb via Crozier to Skinner, than via Watson.⁸⁹

Skinner followed Crozier in using carefully controlled conditions and single organisms to isolate sources of variability and thus obviate the need for statistics. In its emphasis on the whole organism, Crozier's discipline of "General Physiology" was an unusual brand of

physiology, but it was just what Skinner was looking for. Except in one important regard, Crozier's "Analysis of Conduct" became Skinner's "Analysis of Behavior."⁹⁰ The important exception was that Skinner regarded the tropism as too restricted a concept to apply to behavior in general; for that purpose, he preferred to retain the reflex concept in its purified meaning of an observed functional relation between stimulus and response.⁹¹ From the reflex, Skinner gradually developed the notion of the operant-- a notion that would become the basic concept of his empirical epistemology.

By the time Skinner finished his degree at Harvard, his orienting attitudes toward science were fairly well established. The Machian framework he adopted was largely drawn from his reading of Mach, but he also encountered it in applied form in the behavioral research of Crozier. Skinner's early acquaintance with Darwinian biology made him receptive to Mach's biological positivism. His conceptions of learned behavior as biological adaptation and of scientific activity as expedient investigation were but two sides of his deeply held biological view of behavior, and they have remained so. The basic Machian orientation has not changed in Skinner's thought. But it is important to bear in mind that the Machian framework explicitly includes a provision for the gradual development of an empirical, psychological epistemology. Just as Mach took

up the challenge of developing a descriptive epistemology, so too has Skinner; and in this respect there have been changes in his thoughts on science. As we shall see, Skinner's relationship to logical positivism depends on both his Machian framework and the behavioral epistemology which was licensed by, and later developed out of, that framework. It was only after Skinner developed his psychologistic epistemology that the deep differences between his view of science and that of logical positivism became fully evident.

Skinner's Relation to Logical Positivism

Early Interest

It will be recalled that the logical positivists acknowledged Mach's experiential positivism as an influence on their own positivism and even for a time referred to themselves as the "Verein Ernst Mach." They drew upon Poincaré's arguments for conventionalism and against metaphysics, and in Bridgman's operationism they found a close parallel to the empiricist side of their logical positivism. Mach, Poincaré, and Bridgman also served as sources of Skinner's positivism, so it is perhaps not surprising that he at times showed an interest in logical positivism. Skinner has rarely addressed himself to logical positivism and has never done so at any length; but it is still possible to piece together his attitudes

toward it on the basis of his occasional remarks.

Even at the time of his arrival at Harvard, Skinner was deeply interested in the nature of science in general. He took courses in the history of science under L. J. Henderson and George Sarton, joined the History of Science Society, and acquired back issues of the Society's journal Isis.⁹² Skinner has written that: "I also planned to observe the history of science as it unfolded and, following Francis Bacon a little too closely, to take all knowledge to be my province."⁹³ That plan proved to be too boldly encompassing, but in his "early enthusiasm for scientific method and epistemology" he did become a charter subscriber to Erkenntnis and somewhat later, Philosophy of Science.⁹⁴ The former journal, which began publishing in 1930, was of course the journal of the logical positivists, and the latter, founded in 1934, also published many articles by logical positivists. Skinner was thus exposed to logical positivist notions at an early date. But characteristically, he responded to them from a psychological rather than philosophical viewpoint. Speaking of these journals, Skinner wrote:

Causality was a common theme, and I thought it was clearly a behavioral one. I wrote about observations not unlike those which the Belgian psychologist [Albert] Michotte would later publish in his book on the perception of causality.⁹⁵

In a similar manner, Skinner responded to Rudolf Carnap's Erkenntnis paper on "Psychology in Physical Language" (1932) by calling him the "latest behaviorist" and including him in a listing of psychologists who had recently converted to behaviorism.⁹⁶ In doing so, Skinner was apparently ignoring the differences between behaviorism as an approach to psychology and behaviorism as an aspect of the philosophical doctrine of physicalism.

However, Skinner did accurately perceive that, at least at a general level, logical positivism held important common ground with his own operational approach to science. Referring to his approach as it stood in the early thirties, Skinner has written:

In Mach and in Henri Poincaré's Science et méthode I found early versions of what was beginning to be called operationism. The philosophers of the Vienna Circle, not yet dispersed by Hitler, were taking a rather similar line and calling it logical positivism, and Russell, who had introduced me to behaviorism had been influenced by another, if renegade, Viennese, Ludwig Wittgenstein.⁹⁷

And again, in writing of his stance in the mid-thirties, Skinner has stated: "As far as I was concerned there were only minor differences between behaviorism, operationism, and logical positivism."⁹⁸ But this remark was followed by a discussion of operationism which suggests that Skinner was here referring to the general similarity between operationism and the logical positivists' emphasis on verifiability. There is no evidence that Skinner's sympathy

with logical positivism ever extended to its formal side.

Skinner was personally acquainted with two of the major figures in logical positivism, Rudolf Carnap and Herbert Feigl. After receiving his Ph.D., Skinner was elected to the Harvard Society of Fellows, of which the philosopher-logician W. V. O. Quine was also a member. Through Quine, who had studied with Carnap in Prague, Skinner met Carnap at Harvard during the summer of 1936. In a letter written some months later, Skinner reported, with perhaps a touch of overstatement, that Carnap "is the only European I have ever met who grasps the significance of modern behavioristic psychology and its implications for the problem of thought." In a remark that was more typical of his reactions to philosophers, Skinner added: "I have little hope of reconciling logic with psychology, however, except by convincing the logician that most of his problems are essentially psychological--and that is not like to be successful!"⁹⁹ Skinner subsequently referred to Carnap in his Behavior of Organisms (1938) and in a paper of 1945, but in both cases he was being critical of Carnap's views on the unity of science.¹⁰⁰

Skinner's relationship with Feigl began in the early forties when they were both at the University of Minnesota.¹⁰¹ There they became close friends. Together they read and discussed Skinner's Walden Two and engaged in friendly arguments over philosophical issues related to psychology.

But they never reached any substantial agreement on those issues, and it is doubtful whether Skinner absorbed much logical positivism through his contact with Feigl. For his part, Feigl has referred to Skinner as "America's most brilliantly and consistently positivistic psychologist," and he has summarized his relationship with Skinner by saying, "We disagreed sharply on philosophical issues of psychology, but this never disturbed our personal relations."¹⁰² Skinner has acknowledged the support and impetus provided by Feigl for his work on verbal behavior, but stated that "[h]e and I had never fully resolved the differences between logical positivism and behaviorism, and each of us, as Feigl put it, continued to cultivate his own garden. . . ." ¹⁰³

In sum, Skinner had early sympathies with logical positivism, but aside from its kinship with operationism, it was of no great interest to him and appears to have had little influence on his work. Skinner had scant need for a logical positivism because, by 1930, he was already committed to a Machian variety of positivism which, in its descriptive and biological thrust, suited his Baconian and Darwinian biases. Moreover, Machian positivism was not only devoid of the formalist emphasis found in logical positivism, but it was actually antiformalist in important respects. Skinner's attitude toward formal characterizations of science and his continued adherence to a Machian

positivism are discussed in the following section.

Reaffirmation of Mach: Skinner's "Case History"

In 1952, The American Psychological Association and the National Science Foundation decided to undertake joint sponsorship of "a thorough and critical examination of the status and development of psychology." In what became known as "Project A," prominent theorists in psychology were invited to contribute detailed statements of their systems. Under the directorship of Sigmund Koch, these statements were to be collected in a series of volumes which would serve as an "aid to the scholars and research workers who are striving to increase the rigor and further development of scientific psychology."¹⁰⁴ Prospective contributors were sent a set of guidelines suggesting that they address such topics as "the type of formal organization . . . considered best suited to requirements for systematization," the "[s]tatus of the system with respect to explicitness of axiomatization, and of derivational procedures employed," and "a reconstruction of the roles of 'implicit' (i.e., 'postulational') definition, 'explicit' definition, empirical or 'operational' definition, and, in certain cases, 'coordinating' definition, as these are respectively realized within the system."¹⁰⁵ In other words, the participating psychologists were being asked to represent their systems in the framework of logical positivism.

Skinner was asked to contribute to the series. Following Mach in favoring "specific living examples" over "pallid abstract formulae," he turned to old notes and records of his research in order to examine his own behavior as a scientist. But he found little in the record of his own research that could be fit into the framework suggested in the project guidelines, and offered instead a review of the rather haphazard development of his approach to psychology. It was published under the title "A Case History in Scientific Method," and began with the following introductory remarks:

A scientist is an extremely complex organism, and his behavior still resists a successful empirical analysis. Nevertheless, if anything useful is to be said about him, either in trying to understand his behavior or in inculcating similar behavior in others, it will be by way of an empirical, rather than formal, analysis. As an antiformalist, it would be inconsistent of me to describe my own scientific activity in the formal framework of Project A. I have therefore reacted to the proposal of the director by illustrating my own philosophy of science with a personal history.¹⁰⁶

Before recounting his personal history, however, Skinner stated his general position on formal accounts of scientific method--"model building," "theory construction," "experimental design," and the like--and their relationship to scientific practice.

. . . it is a mistake to identify scientific practice with the formalized constructions of statistics and scientific method. These disciplines have their place, but it does not coincide with the place of scientific research. . . . As formal disciplines, they arose very late in the history of science, and most of the facts of

science have been discovered without their aid. . . . It is no wonder that the laboratory scientist is puzzled and often dismayed when he discovers how his behavior has been reconstructed in the formal analyses of scientific method. He is very likely to protest that this is not at all a fair representation of what he does.¹⁰⁷

True to his descriptive empiricism, Skinner went on to assert that methodological issues are issues about the behavior of scientists and that the empirical account of science that is needed to settle such issues is not available.

If we are interested in perpetuating the practices responsible for the present corpus of scientific knowledge, we must keep in mind that some very important parts of the scientific process do not now lend themselves to mathematical, logical, or any other formal treatment. We do not know enough about human behavior to know how the scientist does what he does. Although statisticians and methodologists may seem to tell us, or at least imply, how the mind works--how problems arise, how hypotheses are formed, deductions made, and crucial experiments designed--we as psychologists are in a position to remind them that they do not have the methods appropriate to the empirical observation or functional analysis of such data. These are aspects of human behavior, and no one knows better than we how little can at the moment be said about them.¹⁰⁸

Once again, Skinner here takes a stance that is decidedly Machian in character. It is explicitly antiformalist and specifically critical of the hypothetico-deductive method. It refuses to leave the level of description even for epistemological analysis and yet acknowledges that no adequate empirical account of knowledge exists. Mach wrote in the Science of Mechanics that the mark of a

scientist is the "toleration of an incomplete conception of the world," and Skinner emphasized the danger of letting formal treatments of methodology blind the scientist to its incompleteness.¹⁰⁹

The "Case History" itself is largely a story of the different pieces of apparatus built by Skinner, his changes of interest as he proceeded, the alteration and redesigning of apparatus, the devising of efficient means of recording data, and gradual reconceptualizations of units of behavior. Looking for observed consistency of data and little else, the story goes, Skinner was exploring behavior in an evolving interaction between himself, his rats, and his apparatus. He followed his interests, found ways to save labor, and benefitted from happenstance. Examples of serendipitous findings crop up repeatedly. The rat lever, the response manipulandum that became a standard of operant conditioning research, was originally devised not for the study of operant conditioning but rather in order to investigate the rate of an eating reflex.¹¹⁰ Smooth extinction curves were first produced by accident when a piece of equipment for delivering food failed to operate properly.¹¹¹ Schedules of intermittent reinforcement, which would later become a major topic of research, were discovered when Skinner was forced to stop reinforcing every response because of an unforeseen shortage of food pellets.¹¹²

Skinner's review of his own behavior as a researcher seemed to show that he had not been following any particular method and that some of what turned out to be important discoveries were unplanned and incidental to his intentions of the time. From the story he extracted five "unformalized principles of scientific practice":

1. when you run into something interesting, drop everything else and study it
2. some ways of doing research are easier than others
3. some people are lucky
4. apparatus sometimes breaks down
5. serendipity--the art of finding one thing while looking for something else¹³

Given the suggested framework for Project A, Skinner's "principles" were perhaps humorous, but there was a point to be made. The formalized principles of hypothetico-deductive and statistical methodology failed to give an accurate picture of at least one researcher's method in practice. Perhaps formalized methods were intended only to be prescriptive, but any attempt to prescribe method was unacceptable to Skinner unless it was derived inductively from systematic observations of successful scientific practice. Summarizing his own practice, Skinner wrote:

I never faced a Problem which was more than the eternal problem of finding order. I never attacked a problem by constructing a Hypothesis. I never deduced Theorems or submitted them to Experimental Check. So far as I can see, I had no preconceived Model of behavior--certainly not a physiological or mentalistic one, and I believe, not a conceptual one.¹⁴

As for the notion of a unity of method in science, Skinner concluded his "Case History" with the statement that "it may be best not to try to fit all scientists into any single mold."¹¹⁵

Nowhere in his paper did Skinner directly address logical positivism, but he was clearly dissociating himself from many of the logical positivist notions of science. He would remain a Machian positivist, continuing to view knowledge above all else as a product of individual and collective history. His case history and his unformalized principles emphasized the same historically conditioned character of knowledge that Mach had stressed. For both Skinner and Mach, the empirical study of knowledge reveals its fortuitousness, contingency, particularity, and incompleteness.

Skinner did not refer to logical positivism by name in his case history, but he has done so on other occasions, usually in the context of dissociating himself from it. Thus, he has stated that "[t]he physicalism of the logical positivist has never been good behaviorism" and that "behaviorism is not to be identified with logical positivism."¹¹⁶ He has also spoken of the need for an empirical account of scientific verbal behavior "to straighten out the Logical Positivists."¹¹⁷ At other times, he has alluded to logical positivism's inadequate treatment of issues concerning language--issues which, in his view, call for behavioral analysis. With regard to the issue

of meaning:

Modern logic, as a formalization of "real" lang-
 usages, retains and extends [the] dualistic theory
 of meaning and can scarcely be appealed to by the
 psychologist who recognizes his own responsibility
 in giving an account of verbal behavior.¹¹⁸

And on the problem of definition:

To be consistent the psychologist must deal with his
 own verbal practices by developing an empirical
 science of verbal behavior. He cannot, unfor- :
 tunately, join the logician in defining a definition
 for example, as a "rule for the use of a term"
 (Feigl); he must turn instead to the contingencies
 of reinforcement which account for the functional
 relation between a term, as a verbal response,
 and a given stimulus. This is the "operational
 basis" for his use of terms; and it is not logic
 but science.¹¹⁹

As these passages suggest, the basis for Skinner's dis-
 agreement with logical positivism lies in his indigenous,
 psychological account of science, an account which rests
 largely on his analysis of verbal behavior. Skinner's
 earlier limited sympathy with logical positivism was
 due to its limited overlap with his Machian positivism;
 but it was this same Machian positivism which called for
 the development of a descriptive epistemology and thus
 also led to his antipathy to logical positivism. Skinner's
 psychologistic epistemology will be discussed in the
 following section. The discussion will begin with a brief
 description of the development of the operant concept--
 a concept that plays a fundamental role in his theory of
 knowledge.

Skinner's Behavioral Epistemology

The Concept of the Operant

As was discussed previously, Skinner has followed Mach in expressing the desirability of developing an empirical account of knowledge. For both of them, epistemology is a part of psychology rather than a branch of philosophy. As such, it must remain a "provisional" and "imperfect" enterprise, but one that is important and well worth undertaking. Recognizing this provision nature, Skinner has referred to his own empirical epistemology as "a crude theory of knowledge," but he has often commented on the potential benefits to be derived from an adequate account of knowledge, and has championed his own version as an important advance toward a more complete account.¹²⁰

As we have seen, Tolman applied his basic epistemological concept of a map indifferently to humans and animals, and Hull was equally consistent in applying his notion of a serially conditioned response to cases of human and animal knowing. Skinner has been no less consistent in this respect. For him, animal and human behavior, including the behavior we call knowledge, is based on operant conditioning, a process in which behavior is selected and maintained by its consequences.

The important concept of an operant was first adumbrated by Skinner in an article of 1935, "The Generic

Nature of the Concepts of Stimulus and Response." In that paper, which is the only part of his "Sketch for an Epistemology" ever to be published,¹²¹ Skinner addressed the problem of defining a unit of behavior. Beginning with his earlier notion of a reflex as an observed correlation of stimulus and response, he considered various possible strategies for defining the response. One possibility was to define it in terms of its topography, as a specifiable movement through space and time. This was perhaps a feasible strategy for the physiologist who could ensure the reproducibility of the correlation by restraining the subject and restricting the response to a (perhaps surgically) isolated muscle or group of muscles. But Skinner was working with intact, unrestrained organisms and needed a response definition suited to the behavior of the organism as a whole. Rather than a topographical definition he opted for a generic definition, according to which a response is defined as a class of movements. The problem then became one of how to restrict the class appropriately. Skinner's solution was to accept as a response whatever class was found to exhibit orderly stimulus-response relations when some third variable (drive, for example) was varied. These lawful relations, revealed as smooth curves in the data, would emerge at a unique point in the progressive restriction of the response class. In this way, one could demonstrate the

"experimental reality" of the defined unit by determining "the natural lines of fracture along which behavior and environment actually break."¹²² Skinner then offered a revised definition of the reflex as "a correlation of stimulus and response at a level of restriction marked by the orderliness of changes in the correlation."¹²³

The key to this crucial orderliness was that with an appropriately restricted response class, the members of the class would be "quantitatively mutually replaceable."¹²⁴ This meant that the actual movement performed in making a given response could vary from occasion to occasion--as would naturally happen when whole unrestrained organisms were used as subjects--but without altering the smoothness of the functional relation between the response and its determining variables. Recognizing the generic nature of the response and the intersubstitutability of the members of the response class was the key to Skinner's shift from the reflex tradition to molar behaviorism. The generic operant was his experimentally derived version of Tolman's multiple-track-ness and Hull's habit-family. These three largely equivalent notions were arrived at by different means, but they were all formulated within a five-year period and they were the conceptual cornerstones of molar behaviorism.¹²⁵

Skinner did not actually use the term "operant" in his 1935 paper. The term first appeared in a 1937 paper and again in the 1938 Behavior of Organisms, wherein Skinner clarified the role of the consequent

stimulus in defining the operant class. "The operant," he wrote, "becomes significant for behavior and takes on an identifiable form when it acts upon the environment in such a way that a reinforcing stimulus is produced."¹²⁶ The operant, then, was conceived as a class of movements which have a common effect on the environment and which, as a class, can be shown to vary lawfully in their dynamic relations to other variables. The actual movements involved in pressing a lever, for example, might vary from instance to instance (e.g., left paw, right paw, nose), but they are equivalent with respect to producing reinforcement and they demonstrably function together in the face of changing conditions. Similarly, the operant of mailing a letter presumably includes such topographically distinct instances as driving to the post office and walking to the mailbox, instances which are nonetheless equivalent in producing return mail. In the terminology of operant conditioning, the consequences of a response are said to be "contingent" on the response, and the functional dependencies of consequences on responses are called "contingencies of reinforcement."

With the shift of emphasis from the elicitation of the response by antecedent stimulus conditions to its function of producing consequences, the operant takes on the character of an act rather than a colorless movement.

The outcome of the act, what might ordinarily be referred to as its "purpose" or "intended" consequence, is built into the definition of the operant. In fact, Skinner has written that "operant behavior is the very field of purpose and intention," and he has been interpreted as an act psychologist in the tradition of Franz Brentano.¹²⁷ In any case, Skinner clearly views the operant as a concept that is potentially capable of covering cases of purposeful human action, including cases involving the complex verbal and nonverbal behavior of scientists.

Operant Psychology of Science

In 1932, Skinner drew up plans for his research of the subsequent thirty years. Inspired by Russell's discussion of epistemology in behavioral terms, he included in his plans a project on "Theories of Knowledge," both scientific and nonscientific, with a note to publish on the subject late in his career.¹²⁸ But by 1932, he was already working on his "Sketch for an Epistemology."

For Skinner:

Behaviorism and epistemology were closely related. Behaviorism was a theory of knowledge, and knowing and thinking were forms of behavior.¹²⁹

Through the Society of Fellows, Skinner became acquainted with Alfred North Whitehead, and in his "Sketch" he called for a

description of the activity of science wholly in terms of the behavior of scientists. . . . Examine some of Whitehead's statements about what science did and get at them in terms of the behavior not of Science but of scientists.¹³⁰

In 1935, Skinner published part of the "Sketch" as his paper on the generic definition of the response, but by then he had dropped his plans to publish the "Sketch" as a book.¹³¹ In doing so, however, he was deferring--not abandoning--his project on epistemology. In 1938, for example, he noted that the "relation of organism to environment must be supposed to include the special case of the relation of scientist to subject matter."¹³² And in his contribution to a symposium on operationism in 1945, he gave an early statement of his gradually evolving theory of knowledge.¹³³

Skinner recognized that any account of human knowledge would require an account of verbal behavior. The activity of science, after all, is carried out largely by means of language behavior. Under the impetus of a challenge from Whitehead, Skinner began work on a book on verbal behavior in 1934. He was warned that such a work might require as long as five years to complete, and he remarked during the thirties that "I write so slowly that I hesitate to predict when I'll have a decent draft finished."¹³⁴ In 1947, he based his William James lectures at Harvard on the manuscript, but it was not until 1957 that it appeared in print as Verbal Behavior. The book contains

Skinner's most detailed statement of his behavioral epistemology, and he has expressed the belief that it will prove to be his "most important work."¹³⁵ Since 1957, his views on knowledge and science have been further developed in Contingencies of Reinforcement (1969) and About Behaviorism (1974).

For Skinner, knowledge is behavior, and in its most basic form this knowing is simply adaptation to an environment. Speaking of the relation between a knower and an environment, Skinner has written:

The world which establishes contingencies of reinforcement of the sort studied in an operant analysis is presumably "what knowledge is about." A person comes to know that world and how to behave in it in the sense that he acquires behavior which satisfied the contingencies it maintains.¹³⁶

Skinner calls such behavior "contingency-shaped" and views it as "personal knowledge," somewhat like that characterized by Michael Polanyi.¹³⁷ Skinner continues:

Contingency-shaped behavior depends for its strength on "genuine" consequences. It is likely to be nonverbal and thus to "come to grips with reality." It is a personal possession which dies with the possessor.

But there is, according to Skinner, a second kind of knowledge: behavior controlled by rules which are transmitted to the individual by the social environment.

The rules which form the body of science are public. They survive the scientist who constructed them as well as those who are guided by them. The control they exert is primarily verbal, and the resulting behavior may not vary in strength with consequences having personal significance.¹³⁸

In Skinner's scheme, these rules are verbal operants which describe contingencies in their environment and can thus enable an individual under their control to respond appropriately. At any particular time, the scientist's behavior is likely to be under the joint control of both contingencies and rules.¹³⁹

Science, says Skinner, is "a corpus of rules for effective action," and such rules are what are often referred to variously as the facts, laws, or theories of science.¹⁴⁰ Facts, laws, and theories all describe contingencies in the sense of specifying consequences that result from particular actions, and they differ only in their degree of confirmation (see below) or perhaps in the durability and generality of the contingencies they describe.¹⁴¹ As rules for action, the laws of science have no special ontological status or relation to their subject matter; they simply control the operant behavior of those who use them. Skinner writes:

Scientific laws . . . specify or imply responses and their consequences. They are not, of course, obeyed by nature but men who effectively deal with nature. The formula $s = \frac{1}{2} g t^2$ does not govern the behavior of falling bodies, it governs those who correctly predict the position of falling bodies at given times.¹⁴²

This behavioristic variant of the instrumentalist view of scientific laws is surely an unusual construal of laws, but Skinner remains consistent in elaborating his position. Following Bacon in likening the laws of science to those of government, he writes:

The difference between a scientific and a governmental law is not that the one is discovered and the other made, for both are discovered. A government usually "makes a law" only when the culture is already maintaining or disposed to maintain the contingencies the law describes. The law is a description of prevailing . . . practices.¹⁴³

Both types of law are rules relating response to outcome. Behavior controlled by either type is more likely to achieve beneficial outcomes and avoid noxious ones, and this fact, according to Skinner, helps account for the shaping and maintenance of the verbal operants which express the rules.

Contingency-shaped and rule-governed behaviors both take place in scientific activity, and they may involve physically similar responses. But, says Skinner, even when the behaviors are of similar form, they are different operants because they are controlled by different variables. The distinction between the two is readily seen by noting that, historically, contingency-shaped behavior precedes rule-governed behavior. Following Mach, Skinner discusses how early versions of the law of the lever may have originally arisen to supplement the natural contingencies involved in the practical work of farming, masonry, and

the like.¹⁴⁴ The two kinds of behavior usually differ also in the details of their execution. Contingency-shaped behavior is likely to be smoother and more precise than behavior governed by a rule describing the same contingency (because of the limited precision of description). But rule-governed behavior, Skinner argues, enjoys distinct advantages over its contingency-shaped counterpart-- advantages that are characteristic of science. Rules can be learned rapidly and transmitted efficiently. They obviate the need to undergo the risks that are often involved in learning by contingency. And they can describe contingencies in which the consequences of behavior are remote and therefore unlikely to be effective in controlling behavior through direct shaping.¹⁴⁵

Whether spoken or written, rules are verbal behavior. According to Skinner, such behavior is shaped and maintained by a particular subset of the environmental contingencies, namely those maintained by the verbal community. Different verbal communities generate verbal behavior appropriate to them by enforcing special contingencies. The scientific verbal community differs from others, such as the literary community, in emphasizing the practical consequences of verbal behavior. "In the history of logic and science," writes Skinner, "we can trace the development of a verbal community especially concerned with verbal behavior which contributes to successful action."¹⁴⁶ A

particularly important way in which scientific verbal behavior is made more likely to yield beneficial outcomes is for the community to sharpen the control of that behavior by antecedent events.

The scientific community encourages the precise stimulus control under which an object or a property of an object is identified or characterized in such a way that practical action will be most effective. It conditions responses under favorable circumstances, where relevant and irrelevant properties of stimuli can usually be manipulated. To dispose of irrelevant controlling relations, it sets up new forms of response as arbitrary replacements for the lay vocabulary. . . . 147

The superfluous, historically acquired connotations of terms from ordinary language are symptoms of the irrelevant relations controlling their emission. For scientific terms, the narrower the range of variables controlling them, the more precise and expedient their use.

Scientific verbal behavior is most effective when it is free of multiple sources of strength; and humor, wit, style, the devices of poetry, and fragmentary recombinations and distortions of form all go unreinforced, if they are not punished, by the scientific community. 148

Stimulus control of verbal operants can be sharpened by the provision of schemes for classifying phenomena. The resulting "classificatory operants" can profitably undergo generic extension, but "metaphorical, metonymical, and solecistic extensions are usually extinguished or punished." Here again Skinner reveals his Machian aversion to metaphor in science. Citing the molecular theory of gas,

he admits that metaphorical extension may take place in science, but he asserts that the metaphor will be "robbed of its metaphorical nature through the advent of additional stimulus control."¹⁴⁹

Regardless of the conditions that first evoke a given instance of scientific verbal behavior, it may then become subject to the process of confirmation. For Skinner, confirmation is not a matter of some sort of relation between a proposition and a body of evidence but rather the strengthening of a verbal operant. According to him:

We confirm any verbal response when we generate additional variables to increase its probability. Thus, our guess that something seen at a distance is a telescope is confirmed by moving closer until the weak response I think it's a telescope may be replaced by the strong I know it's a telescope.¹⁵⁰

A given verbal response, say, a particular sentence, may be part of two different operants if it is controlled by different functional relations. In this case, it may be strengthened in different ways. In Skinner's terminology, it is strengthened as a "tact" when observations make it more probable, that is, when the control of it by external antecedent stimulus conditions is increased. Alternatively, it is strengthened as an "intraverbal" when it is derived as a prediction from other verbal formulations. Skinner has written:

A series of verbal manipulations respecting the orbits of the known planets may lead to a statement of the position and size of a hypothetical planet. With the aid of a telescope a response of similar form may be made as a tact. Subsequently the astronomer may emit such a sentence as There is a planet of such and such a size at such and such a place as a response with at least two sources of strength: the observational data with respect to which the response is a tact and the calculations which construct a comparable response.¹⁵¹

Skinner's notion here of a sentence being confirmed by different means is similar to Hull's habit-family conception of truth, according to which a sentence is confirmed when a symbolic derivation and an observation give rise to the same symbolic habit (see Chapter 8).¹⁵²

Along with his account of scientific verbal behavior, Skinner offers a rudimentary account of logical verbal behavior. Remaining true to his antiformalist position, Skinner from the start asserts that logicians and linguists are misguided in their attempt to give a formal analysis of language. They commit what he calls the "Formalist Fallacy" by treating the products of verbal behavior in a vacuum without reference to the functional relations that control their emission.¹⁵³ In the absence of information about the actual circumstances of their production, sentences are then analyzed purely for form. Worse yet, in Skinner's view, this unfruitful sort of analysis leads to the extraction of rules which are said to govern form and these rules are assigned a causal role in the production of verbal behavior. But, for Skinner, verbal

behavior can profitably be studied only through the same sort of Machian causal (i.e., functional) analysis that applies to other forms of behavior. Formal analyses not only fail to consider the all-important causal variables governing actual behavior but they draw attention away from them.

In his own account of verbal behavior, says Skinner, "no form of verbal behavior is significant apart from its controlling relations."¹⁵⁴ Under certain circumstances, the form of an utterance may be separately conditioned by the verbal environment, but this is not as common a phenomenon as logical analysis would imply. Skinner admits that verbal behavior can be "about" other verbal behavior--that it is evoked by and acts upon other verbal behavior--and he calls such instances "autoclitic" behavior.¹⁵⁵ Verbal behavior is sometimes partially controlled by "autoclitic frames," which are akin to the syntax of the logician and linguist. But these frames never wholly determine actual verbal behavior. In Skinner's words, "The relational aspects of the situation strengthen a frame, and specific features of the situation strengthen the responses fitted into it."¹⁵⁶ For Skinner, then, the autoclitic framework of verbal behavior is the domain of logic, although a functional analysis of autoclitics bears little similarity to a formal analysis. Skinner proceeds to consider such traditional topics as assertion, negation, and quantification ("all," "some," etc.), but they are treated

as autoclitic responses and analyzed behaviorally in terms of their causes and effects rather than syntactically.¹⁵⁷

Like Mach, Skinner denies that the study of science or logic in its own right requires transcending the empirical realm in any way. Accordingly, he emphasizes that no sort of meta-analysis is needed for giving an account of language or logic. Talking about talking, which is really just behaving in reference to behavior, raises no special difficulties beyond those posed by the complexity of the subject matter. In particular, autoclitic behavior is simply another kind of behavior. Referring to Carnap's Logical Syntax of Language (1934), he writes:

A distinction is sometimes made between a language which talks about things and a language which talks about language. This is essentially the force of Carnap's distinction between object language and metalanguage. . . . This is not, however, the distinction carried by the term autoclitic. Once verbal behavior has occurred and become one of the objects of the physical world, it can be described like any other object. We have no reason to distinguish the special vocabulary or syntax with which this is done.¹⁵⁸

Even when verbal behavior is about verbal behavior, it is to be accounted for at the level of description and in psychological, not logical, terms.

In line with his Machian heritage, Skinner characterizes the nature of logic in terms of his own ostensibly descriptive concepts. In regard to the formal aspect of logic, he says that logic "is concerned with interrelations among autoclitics, usually without respect to the primary

verbal behavior to which they are applied." He refers to the symbolic manipulations of the logician as "extremely complex behaviors" which have to be "laboriously conditioned by the verbal community." Logic, he says, deals with the "analysis of the internal, and eventually tautological, relationships among autoclitic frames," but any such analysis must in the long run be grounded in practical action. According to Skinner, "the behavior of both logician and scientist leads at last to effective nonverbal action, and it is here that we must find the ultimate reinforcing contingencies which maintain the logical and scientific verbal community." Science is effective verbal behavior, and if logic is to contribute to effective scientific activity, its techniques must be "adapted to the phenomena of verbal behavior."¹⁵⁹ Skinner continues:

Autoclitic frames need to be studied and practices need to be devised which maximize the tautological validity or truth to be inferred from relationships among such frames. But all such analyses, together with their products, are verbal behavior and subject to some such analysis as the present.¹⁶⁰

In sum, logic does not approach verbal behavior from a separate perspective or a different level of analysis-- it is verbal behavior and can best serve the practical ends of knowledge by being adapted to verbal behavior itself.

Conclusion: The "Bootstrap" Nature of the Epistemological Enterprise

Traditionally, epistemological views carry with them a closely associated set of methodological standards and practices; to understand knowledge is to understand the means for achieving it. As we have seen, Skinner's early positivist methodology was integrally related to his conception of knowledge as biological behavior. But Skinner has maintained that continuing advances in knowledge permit changes and refinements in methodology. In Verbal Behavior, Skinner sketched the process as follows:

Three steps appear to lead to this sort of methodological inquiry: (1) some kinds of verbal behavior, including appropriate relational and quantifying autoclitics, prove to have important practical consequences for both speaker and listener, (2) the community discovers and adopts explicit practices which encourage such behavior, being reinforced for this by even more extensive practical consequences, and (3) the practices of the community are then studied and improved, presumably also because of increasingly successful consequences.¹⁶¹

The study of verbal behavior can result in a knowledge of the empirical conditions for effective scientific behavior and communication. In this way, methodology takes on a dual status as both a guide for psychology and a subject matter of psychology. As such, methodology will be--as Mach put it--"provisional and imperfect," but it will have as an aim the production of more and more effective formulations. Skinner has projected the goal of such an enterprise:

One of the ultimate accomplishments of a science of verbal behavior may be an empirical logic, or a descriptive and analytical scientific epistemology, the terms and practices of which will be adapted to human behavior as a subject matter.¹⁶²

Systematic investigation must, of course, begin with certain methods, but they do not provide an unchanging foundation for further research; rather they evolve under the impetus of empirical findings they generate.

In Skinner's radical empiricism, there is no possibility of escaping the level of description in order to establish a higher-order perspective for guiding the quest for knowledge. He has written that:

It would be absurd for the behaviorist to contend that he is in any way exempt from his analysis. He cannot step out of the causal stream and observe behavior from some special point of vantage. . . . In the very act of analyzing human behavior he is behaving. . . .¹⁶³

For Skinner, logic is a part of scientific methodology, but it arises out of actual practice and is not prior to practice in any sense, either temporal or logical. It is often claimed in discussions of epistemology that the knower has a different status from that which is known, that the possibility of knowledge requires the possession of presuppositions which are necessary for the process of knowing to begin. In particular, it is sometimes said that epistemology presupposes logic from the outset. But for Skinner knowledge begins and ends in the empirical realm; science can be studied only through science. The

self-reflexive nature of a descriptive epistemology is acknowledged by Skinner, and he addresses the issues raised by his approach in a direct fashion:

The philosopher will call this circular. He will argue that we must adopt the rules of logic in order to make and interpret the experiments required in an empirical science of verbal behavior. But talking about talking is no more circular than thinking about thinking or knowing about knowing. Whether or not we are lifting ourselves by our own bootstraps, the simple fact is that we can make progress in a scientific analysis of verbal behavior. Eventually we shall be able to include, and perhaps to understand, our own verbal behavior as scientists. If it turns out that our final view of verbal behavior invalidates our scientific structure from the point of view of logic and truth-value, then so much the worse for logic, which will also have been embraced by our analysis.¹⁶⁴

This bold statement was written in 1945, at a time when the logical positivist view of science was rather widely influential. In it, Skinner clearly departs from that view and sets out a position which remains controversial and raises important issues concerning the nature of science. In the following chapter, these issues will be discussed in relation to 1) the indigenous epistemologies of Tolman and Hull, 2) the epistemology of the logical positivists, and 3) the epistemological views which have succeeded logical positivism as dominant interpretations of science.

Notes for Chapter 9

1. Sigmund Koch, "Clark L. Hull," in Modern Learning Theory, by William K. Estes, Sigmund Koch, Kenneth MacCorquodale, Paul E. Meehl, Conrad G. Mueller, Jr., William N. Schoenfeld, and William S. Verplanck (New York: Appleton-Century-Crofts, 1954), pp. 1-176.
2. Hull, "A Functional Interpretation of the Reflex," Psychological Review 36 (1929): 498-511; T. C. Barnes and B. F. Skinner, "The Progressive Increase in the Geotropic Response of the Ant Aphaenogaster," Journal of General Psychology 4 (1930): 102-112.
3. B. F. Skinner, Walden Two (New York: Macmillan, 1948); Beyond Freedom and Dignity (New York: Alfred A. Knopf, 1971).
4. M. H. Hall, "An Interview with 'Mr. Behaviorist' B. F. Skinner," Psychology Today 1 (1967): 21-23, 68-71, on p. 21.
5. B. F. Skinner, Particulars of My Life (New York: Alfred A. Knopf, 1976), passim. Skinner's apparatuses, among which the best known are the "Skinner box" and the cumulative record, played an extremely important role in the development of his psychological views, as will be discussed in what follows. Skinner himself has acknowledged the importance of apparatus in shaping his thought and has even claimed that his decision to take his Ph.D. in psychology was largely due to the high quality of the machine shop in Harvard's Department of Psychology. See B. F. Skinner, The Shaping of a Behaviorist: Part Two of an Autobiography (New York: Alfred A. Knopf, 1979), p. 31.
6. Skinner, Particulars, pp. 128-129.
7. Ibid., p. 148.
8. Ibid., p. 216.
9. Ibid., p. 295.
10. Ibid., pp. 203, 248-249, 265, 287. For an analysis of Skinner's "Dark Year," see Alan C. Elms, "Skinner's Dark Year and Walden Two," American Psychologist 36 (1981): 470-479.

11. Skinner, Particulars, pp. 287, 291, 300-301.
12. Skinner, Shaping, p. 10; Particulars, pp. 298-299. The book review in which Russell spoke highly of Watson was Bertrand Russell's "The Meaning of Meaning," Dial 81 (1926): 114-121, on p. 121.
13. Skinner, Shaping, p. 10.
14. Bertrand Russell, Outline of Philosophy (New York: World Publishing, 1960), p. 46. Originally published as Philosophy (New York: Norton, 1927).
15. Ibid., p. 14.
16. Ibid., pp. 83, 86.
17. Ibid., p. 20.
18. B. F. Skinner, "A Case History of Scientific Method," in Psychology: A Study of a Science, vol. 2, ed. Sigmund Koch (New York: McGraw-Hill, 1959), pp. 359-379, on p. 362. By his own admission, Skinner did not read the last section of Philosophy, in which Russell argued that behaviorism, despite its virtues, was inadequate as a complete account of psychology. When Skinner met Russell years later and told him that Philosophy had converted him to behaviorism, Russell replied, "Good heavens, I thought it had demolished behaviorism" (Shaping, p. 224).
19. B. F. Skinner, Notebooks, B. F. Skinner, ed. Robert Epstein (Englewood Cliffs, N.J.: Prentice-Hall, 1980), p. 274; "The Experimental Analysis of Operant Behavior: A History," in Psychology: Theoretical-Historical Perspectives, ed. R. W. Rieber and Kurt Salzinger (New York: Academic Press, 1980), pp. 191-202, on p. 200.
20. Skinner, Particulars, pp. 301-302; Shaping, p. 4.
21. Skinner, Shaping, p. 66.
22. Ernst Mach, Science of Mechanics, 9th ed., trans. Thomas J. McCormack (La Salle, Ill.: Open Court, 1942), p. xiii. Original German edition published in 1883.
23. B. F. Skinner, "The Concept of the Reflex in the Description of Behavior," in Cumulative Record, enlarged edition (New York: Appleton-Century-Crofts, 1961), pp. 319-346, on p. 346. This paper, drawn directly from Skinner's dissertation of 1930, was originally published in the Journal of General Psychology 5 (1931): 427-458.

24. Skinner, Cumulative Record, p. 319. This statement is part of Skinner's comments introducing the reprinted "Concept of the Reflex."
25. Skinner, "Concept of the Reflex," p. 321.
26. Ibid., p. 346.
27. See Willard F. Day, Jr., "The Historical Antecedents of Contemporary Behaviorism," in Psychology: Theoretical-Historical Perspectives, ed. R. W. Rieber and Kurt Salzinger (New York: Academic Press, 1980), pp. 203-262, on p. 227.
28. Skinner, Shaping, p. 116. That Skinner read the three works of Mach mentioned here and that the Science of Mechanics was the most important of them was confirmed during a personal interview with the author (28 April 1982).
29. Mach, Mechanics, pp. 5-6; Knowledge and Error: Sketches on the Psychology of Inquiry (Dordrecht, Holland: D. Reidel, 1976), p. 55. Original German edition published in 1905.
30. B. F. Skinner, Science and Human Behavior (New York: Free Press, 1953), p. 14; About Behaviorism, college ed. (New York: Alfred A. Knopf, 1974), p. ix.
31. Skinner, About Behaviorism, p. ix.
32. Mach, Mechanics, p. 594.
33. Ernst Mach, Popular Scientific Lectures, 5th ed., trans. Thomas J. McCormack (La Salle, Ill.: Open Court, 1943), pp. 190, 217, Original German edition published in 1894.
34. Mach, Knowledge and Error, pp. 1, 18-21, 24-26, 37-42, 51-55, 79-80, 92-93, 135. See also John T. Blackmore, Ernst Mach: His Work, Life and Influence (Berkeley: University of California Press, 1972), pp. 68-71.
35. Mach, Knowledge and Error, pp. 93, 52, 20.
36. Ibid., p. xxxi.
37. Ibid., pp. 1, 51.

38. See Erwin N. Hiebert, "Mach's Philosophical Use of the History of Science," in Historical and Philosophical Perspectives of Science, ed. Roger H. Stuewer (Minneapolis: University of Minnesota Press, 1970), pp. 184-203, on p. 189.
39. Mach, Popular Scientific Lectures, p. 265; Knowledge and Error, p. 222.
40. Mach, Popular Scientific Lectures, p. 222.
41. Charles S. Peirce, Essays in the Philosophy of Science, ed. V. Tomas (New York: Liberal Arts Press, 1957), p. 228.
42. Mach, Mechanics, p. 7.
43. Mach, Popular Scientific Lectures, p. 197.
44. Ernst Mach, The Analysis of Sensations (New York: Dover, 1959), p. 37. Original German edition published in 1885.
45. Ibid., p. 23.
46. B. F. Skinner, Contingencies of Reinforcement: A Theoretical Analysis (New York: Appleton-Century-Crofts, 1969), pp. 172-217.
47. B. F. Skinner, "The Operational Analysis of Psychological Terms," in Cumulative Record, pp. 272-286, on p. 285. Originally published in the Psychological Review 52 (1945): 270-277, 291-294.
48. Skinner, Freedom and Dignity, pp. 123, 130.
49. Ibid., p. 137. Environmental pollution and the arms race are two salient examples of survival-threatening behaviors with remote consequences.
50. See Mach, Knowledge and Error, p. 212.
51. B. F. Skinner, The Behavior of Organisms: An Experimental Analysis (New York: Appleton-Century-Crofts, 1938), pp. 44, 436, 438.
52. Mach, Popular Scientific Lectures, p. 206; B. F. Skinner, "The Generic Nature of the Concepts of Stimulus and Response," in Cumulative Record, pp. 347-366, on p. 361. Originally published in the Journal of General Psychology 12 (1935): 40-65.

53. Skinner, Behavior of Organisms, pp. 44; "Operational Analysis," p. 283; Behavior of Organisms, pp. 10, 436, 7, 7.
54. Ernst Mach, History and Root of the Principle of the Conservation of Energy (Chicago: Open Court, 1911), p. 63. Original German edition published in Prague in 1872.
55. Mach, Popular Scientific Lectures, pp. 252-153.
56. Ibid., p. 253.
57. Mach, Mechanics, p. 325; Knowledge and Error, p. 211.
58. Skinner, "Concept of the Reflex," pp. 337-338. The role of Skinner's deterministic beliefs in his acceptance of this formulation of explanation and causality has been discussed by Judith L. Scharff, "Skinner's Concept of the Operant: From Necessitarian to Probabilistic Causality," Behaviorism 10 (Spring, 1982): 45-54.
59. Skinner, Behavior of Organisms, p. 21.
60. For Example, see Skinner, About Behaviorism, p. 37.
61. Mach, Mechanics, p. 590; Knowledge and Error, p. 181.
62. See Blackmore, Mach, pp. 177, 313.
63. Mach, Mechanics, p. 600.
64. B. F. Skinner, "Are Theories of Learning Necessary?" in Cumulative Record, pp. 39-69, on p. 39. Originally published in the Psychological Review 57 (1950): 193-216.
65. Ibid., pp. 39-41.
66. Skinner, Behavior of Organisms, p. 437.
67. Skinner, "Are Theories Necessary?" p. 69; Behavior of Organisms, p. 426.
68. Skinner, "Are Theories Necessary?" p. 41; Behavior of Organisms, p. 426.

69. B. F. Skinner, "Hull's Principles of Behavior," in Cumulative Record, pp. 384-389, on pp. 385, 386, 387. Originally published in the American Journal of Psychology 57 (1944): 276-281.
70. Blackmore, Mach, 126-127.
71. Ibid., p. 28.
72. Skinner, About Behaviorism, p. 235; Verbal Behavior (Englewood Cliffs, N.J.: Prentice-Hall, 1957), p. 428. Skinner's approach to the problem of truth has been discussed in detail by G. E. Zuriff, "Radical Behaviorist Epistemology," Psychological Bulletin 87 (1980): 337-350, especially pp. 343-346. On Mach's view of truth, see Blackmore, Mach, pp. 27-28. As noted by Zuriff and Blackmore, the views of Skinner and Mach on truth contain strains of the correspondence theory despite their overall pragmatic approach.
73. This particular translation of Mach's statement, and the words interpolated in it, are found in Hiebert, "Mach's Use of History," p. 194. A slightly different version appears in the recently published English translation of Knowledge and Error cited above.
74. Mach, Knowledge and Error, p. 11.
75. Ibid., p. xxxi.
76. This incident is reported in Skinner, Shaping, p. 29.
77. Mach's psychology of knowledge is presented in its most explicit and complete version in Knowledge and Error, which Skinner has read only partially. But the notion of a psychological epistemology also appears in the Mechanics, as, for example, when Mach speaks of developing a "theory of theory" (p. 594).
78. Hiebert, "Mach's Use of History," p. 202.
79. Some of the philosophical issues raised by such a descriptive approach will be considered below, briefly at the end of this chapter, and in Chapter 10.

80. Skinner, Shaping, pp. 16-20, 25-26, 38. Skinner has remarked that his research at Harvard was not supervised by anyone, apparently because the physiologists thought the psychologists were keeping track of his activities, and vice versa (*ibid.*, p. 35). This claim accords with Skinner's Baconian conviction that the scientist learns more from nature than from other scientists.

81. Skinner, Behavior of Organisms, p. 4; "Case History," p. 362.

82. Skinner was critical of Pavlov in this regard (Behavior of Organisms, p. 427).

83. Day, "Antecedents of Behaviorism," p. 222.

84. Skinner, Particulars, p. 295. For a useful sketch of Loeb's controversial career, see Donald Fleming, "Introduction," in Jacques Loeb, The Mechanistic Conception of Life, ed. Donald Fleming (Cambridge: Harvard University Press, Belknap Press, 1964), vii-xlii. On Loeb's connection with Watson, see Robert I. Watson, The Great Psychologists, 3rd ed. (Philadelphia: Lippincott, 1971), pp. 426-428.

85. See Blackmore, Mach, p. 130.

86. *Ibid.*, pp. 131, 172; Skinner, "Are Theories Necessary?" p. 69; "Hull's Principles of Behavior," p. 388; W. J. Crozier and Hudson Hoagland, "The Study of Living Organisms," in A Handbook of General Experimental Psychology, ed. Carl Murchison (Worcester, Mass.: Clark University Press, 1934), pp. 3-108, on pp. 4, 98.

87. Blackmore, Mach, p. 131.

88. Crozier and Hoagland, "Study of Organisms," p. 3. Mach held a similar "egoless" concept of the individual, as does Skinner (Blackmore, Mach, p. 37; Skinner, Verbal Behavior, p. 313; Cumulative Record, p. 236).

89. Richard J. Herrnstein, "Nature as Nurture: Behaviorism and the Instinct Doctrine," Behaviorism 1 (1972): 23-52, on pp. 45-46.

90. One of Skinner's courses at Harvard was Crozier's "Analysis of Conduct," a course which Skinner described as "right along my line" (Shaping, p. 44). Skinner's sympathetic description of Crozier's nonphysiological approach to behavior is given in Shaping, pp. 16-17.

91. Skinner, Behavior of Organisms, p. 435.
92. Skinner, Shaping, p. 49.
93. Ibid., pp. 49-50.
94. Ibid., pp. 248, 115.
95. Ibid., p. 115. The book referred to is Albert E. Michotte, La Perception de la Causalité (Paris: J. Vrin, 1946).
96. Skinner, Shaping, p. 149. Skinner does not cite Carnap's 1932 paper, but it would appear to be the work being referred to since Skinner's remarks were written shortly after the paper appeared.
97. Ibid., p. 66.
98. Ibid., p. 161.
99. Ibid., p. 213.
100. Skinner, Behavior of Organisms, p. 428; Cumulative Record, p. 273.
101. Feigl, Inquiries and Provocations: Selected Writings 1929-1974, ed. Robert S. Cohen (Dordrecht, Holland: D. Reidel, 1981), pp. 90-91; Skinner, Shaping, p. 248. Even though Feigl and Skinner were both at Harvard in 1930, they did not know each other at that time.
102. Feigl, Inquiries and Provocations, pp. 41, 90-91.
103. Skinner, Shaping, pp. 248, 342.
104. Dael Wolfle, "Preface," in Psychology: A Study of a Science, vol. 2, pp. vi-vii.
105. "Appendix: Suggested Discussion Topics for Contributors of Systematic Analyses," in Psychology: A Study of a Science, vol. 2, pp. 667, 671, 672.
106. Skinner, "Case History," p. 359.
107. Ibid., p. 360. Skinner's skepticism about formalized reconstructions of scientific method echoes that of Mach. See, for example, Mach's statement that "there can be no widely effective instructions for enquiry by formula" (Knowledge and Error, p. 223).

108. Skinner, "Case History," pp. 360-361.
109. Mach, Mechanics, p. 560.
110. Skinner, "Case History," p. 366. Skinner's intention was to examine the possibility that the rate of eating was determined by the refractory period following each instance of ingestion the insertion of the lever press before ingestion would permit the recording of a component of feeding behavior that had no refractory period. The relevant studies were originally reported in B. F. Skinner, "Drive and Reflex Strength," Journal of General Psychology 6 (1932): 22-37 and "Drive and Reflex Strength: II," Journal of General Psychology 6 (1932): 38-48.
111. Skinner, "Case History," p. 367.
112. Ibid., pp. 367-369.
113. Ibid., pp. 363, 364, 365-366, 367, 369.
114. Ibid., p. 369.
115. Ibid., p. 379.
116. Brand Blanshard and B. F. Skinner, "The Problem of Consciousness--A Debate," Philosophy and Phenomenological Research 27 (1967): 317-337, on p. 325; B. F. Skinner, "Discussion," in Behaviorism and Phenomenology, ed. T. W. Wann (Chicago: University of Chicago Press, 1964), p. 42.
117. Skinner, Shaping, p. 245.
118. Skinner, "Operational Analysis," p. 273.
119. Ibid., p. 281.
120. See, for example, Skinner, Notebooks, p. 275.
121. Skinner, Shaping p. 146.
122. Skinner, "Generic Nature," pp. 355, 347. The actual term "operant" was not coined by Skinner until 1937, in his "Two Types of Conditioned Reflex: A Reply to Konorski and Miller," Journal of General Psychology 16 (1937): 264-272. This latter paper is reprinted in Cumulative Record, pp. 376-383.
123. Skinner, "Generic Nature," p. 360.

124. Ibid., p. 351.
125. This is not to suggest that the three notions were necessarily arrived at totally independently of one another, but they did emerge out of different intellectual contexts and practices.
126. Skinner, Behavior of Organisms, p. 22.
127. Skinner, About Behaviorism, p. 55; Day, "Antecedents of Behaviorism," pp. 216-218.
128. Skinner, Shaping, p. 115.
129. Ibid.
130. Ibid., p. 116.
131. Ibid., p. 159.
132. Skinner, Behavior of Organisms, p. 43.
133. Skinner, "Operational Analysis."
134. Skinner, Shaping, pp. 149, 151, 150.
135. Skinner, "Operant Behavior: A History," p. 198.
136. Skinner, Contingencies of Reinforcement, p. 156.
137. Ibid.
138. Ibid., p. 157. The consequences need not have personal significance because they have significance for the culture or subculture that arranges for them.
139. Ibid.
140. Skinner, About Behaviorism, p. 235.
141. Skinner, Contingencies of Reinforcement, pp. 157, 162-163.
142. Ibid., p. 141.

143. Ibid., p. 159. Skinner also notes that Bacon, who was a lawyer, "seems to have been the first to speak of the laws of science" (About Behaviorism, p. 123). For another discussion of the legal origins of the concept of "natural law," see Edgar Zilsel, "Problems of Empiricism," in Foundations of the Unity of Science, 2 vols., vol. 2, ed. Otto Neurath, Rudolf Carnap, and Charles Morris (Chicago: University of Chicago Press, 1970), pp. 803-844, on p. 811.

144. Skinner, About Behaviorism, p. 124; Mach, Mechanics, pp. 11-32.

145. Skinner, About Behaviorism, p. 124.

146. Skinner, Verbal Behavior, p. 418.

147. Ibid., p. 419. Skinner's discussion of the training of scientists by a scientific community bears important similarities to the account given by Thomas S. Kuhn in his "Second Thoughts on Paradigms," in The Structure of Scientific Theories, 2nd rev. ed., ed. Frederick Suppe (Urbana, Ill.: University of Illinois Press, 1977), pp. 459-482.

148. Skinner, Verbal Behavior, p. 420.

149. Ibid., p. 419.

150. Ibid., p. 425.

151. Ibid., p. 426.

152. Also like Hull, Skinner claims that the rules used in symbolic derivations are confirmed when the observations resulting from those derivations are confirmed (see Verbal Behavior, p. 427).

153. Skinner, Contingencies of Reinforcement, pp. 89-90; About Behaviorism, pp. 98-99; Verbal Behavior, p. 4.

154. Skinner, Verbal Behavior, p. 221.

155. Ibid., p. 313.

156. Ibid., p. 336.

157. Ibid., pp. 322-330.
158. Ibid., p. 319.
159. Ibid., pp. 329, 423, 428-429, 429, 430.
160. Ibid., p. 431.
161. Ibid., p. 430.
162. Ibid., p. 431.
163. Skinner, About Behaviorism, p. 234.
164. Skinner, "Operational Analysis," pp. 281-282.

CHAPTER 10

CONCLUSION

The preceding chapters have provided detailed historical accounts of the views of science held by the major neobehaviorists. In the course of these accounts, considerable evidence has been accumulated to suggest that there was in general no close intellectual association between behaviorism and logical positivism. Without a doubt, there were certain specific influences of logical positivism on these neobehaviorists as well as personal interactions between them and certain figures in the logical positivist movement. But on the whole, the neobehaviorists' views of science were drawn from their own deep-seated presuppositions about organismic behavior. Tolman, Hull, and Skinner all embarked on careers in psychology with strong interests in epistemology. Their early epistemological interests and the directions in which they subsequently pursued those interests were determined far more by their conceptions of psychology than by any external philosophical or methodological views they encountered during their interactions with logical positivism. The sympathies which they did have with logical positivism can be understood as

having been generated and restricted by their psychological views of knowledge. And, in the final analysis, their psychologizing of the knowledge process placed a deep gulf between their indigenous epistemologies and the epistemological views of the logical positivists.

In the first part of this concluding chapter, the relationship between behaviorism and logical positivism will be reconsidered in light of the historical account presented in the foregoing chapters. In this reconsideration, it will be useful to introduce certain broad notions that have emerged from recent work in the philosophy of science. First, the revised account will be summarized in terms of the relationship between method and metaphysics in behaviorist thought. Second, the standard account will be reviewed and its major themes assessed. Third, some comments will be made concerning the perpetuation of the lore about behaviorism and logical positivism. The second part of the chapter deals with broader issues concerning psychologism in epistemology and the philosophy of science. Issues that will be briefly addressed are the contrast between psychological and logical epistemologies, psychologistic trends in post-positivist philosophy of science, and the implications of psychologism for attempts to ground epistemology on firm foundations.

Neobehaviorism and Logical Postivism:
The Alliance Reconsidered

Metaphysics, Metaphor, and Method in Neobehaviorism

"The testing of hypotheses has been the glory of methodologists, but it remains a sterile glory so long as little or nothing is said of the primitive roots--both imaginal and ideological--from which testable ideas spring."

--Howard E. Gruber (1980)¹

"Philosophy and science are interlocked in such a way that they can only be separated if we make a number of more or less arbitrary distinctions."

--Arne Naess (1972)²

Prominent among the shared outlooks of behaviorism and logical positivism was their avowed distaste for traditional metaphysics. There was nothing particularly novel in their anti-metaphysical tendencies; in many respects, they were simply restating views that went back to Hume and their positivist ancestors of the nineteenth century. But especially in the 1920s and early 1930s, these views were stated with renewed vigor in the polemics of Watson, Carnap, Neurath, and others. Yet, despite their avowed intentions, the behaviorists and logical positivists were never able to escape the metaphysical presuppositions which underlay, at least implicitly, their own views on nature and science. This was particularly clear in the case of psychologists. To reject one type of metaphysics was to have already accepted another, wittingly or not. Even in the early thirties, Grace Adams was able to point

out, with considerable justification, that:

Though the experimental physiologists would have psychology renounce its happy intimacy with metaphysics for a few paltry data about reflexes and sensations, most psychologists have never really accepted the severance.

Some psychologists, she added, were "still swimming, some floating, others with bold overhand strokes" in the "waters of metaphysics."³ Even those who publicly endorsed the severance of metaphysics from psychology frequently relied on deep-seated, essentially metaphysical preconceptions in developing their theoretical approaches to psychology.

That the neobehaviorists were immersed in metaphysical currents--whether swimming or treading water--ought to be apparent from the foregoing accounts of their views on science. In each case, the historical record of their intellectual development reveals that their epistemologies, their psychologies of science, their theories of behavior, their selection of problems for study, their methods, and even specific types of apparatus used in their investigations were all intimately connected with their underlying metaphysical views about the nature of the world and organismic adaptation to the world. It was these fundamental pre-theoretical conceptions that they turned to in developing their basic concepts, their metaphors and heuristics for problem-solving, and their methods of inquiry. Perhaps most importantly, it was their metaphysical views that lent cohesion and power to their systems of psychology.

In a very broad sense, the world-views of the major neobehaviorists were all inspired by Darwinian biology. Knowledge, for all of them, was a form of adaptation to an environment. But beyond this general agreement, each neobehaviorist's reception and elaboration of Darwinian thought depended on his own intellectual background and disposition. Thus, for Tolman, the Darwinian impulse was filtered through Jamesian pragmatism and then through the neorealism and contextualism of those who followed James in the pragmatist tradition. The result was Tolman's view of knowledge as a sort of relatively successful guided exploration of a complex and ambiguous environment consisting of richly interwoven strands of causal texture. In Hull's thinking, Darwinism was wedded to a mechanical philosophy of nature, so that adaptation--and hence knowledge--became a matter of a knowing machine adjusting to or running in parallel with a known part of the world-machine. In the case of Skinner, the Darwinian world-view came both from his own knowledge of biology and through Darwin's profound influence on Mach. The outcome was a positivistic theory of knowledge framed in terms of expedient adaptation of organisms to environments..

The heuristic power of these world-views was revealed in the fertile metaphors and heuristic devices which they helped generate and which in turn contributed to their refinement and articulation. James's functionalist view of

mind found expression in the neorealists' belief that mind is out there in the world which it operates on. This in turn gave rise to Tolman's metaphors of mazes and maps. The world for Tolman was a complex maze and knowledge of it was a sort of map. These metaphors were important sources of hypotheses for him and they suggested strategies for solving particular problems that arose in subjecting psychological phenomena to a behavioral analysis. Addressing a problem often became simply a matter of designing an appropriate maze. Hull's metaphor of the organism as an intelligent mechanism in a world-machine played an analogous role in his systematic thought. It arose from his Darwinian-Newtonian metaphysics, and it led to hypotheses and problem-solving strategies. For Hull, addressing a problem often meant designing a machine that would exhibit a certain type of behavior. Skinner's use of metaphor and heuristic is less obvious than in the case of Tolman and Hull, but his Machian-Darwinian metaphysics are certainly not without implications in this regard. When faced with a problem, the Machian heuristic is to manipulate and observe wherever possible, then to make cautious descriptive extensions to new areas. Despite the Machian injunction against metaphor, these descriptive extensions are largely metaphorical in nature. Skinner's notion of selection by consequences--even when divorced from the obviously metaphorical connotations of selection "pressure"--

remains a metaphorical extension of the Darwinian notion to the behavior of individual organisms.⁴

If the neobehaviorists' metaphors and problem-solving strategies were deeply linked with their metaphysical beliefs, so too were their general views on methodology. Tolman's early operationism was a direct outgrowth of his pre-theoretical view that cognitive relations subsist out in the observable world. As his conception of cognition shifted toward the view that the cognitions of other organisms must be inferred, his operational methodology underwent corresponding modifications. Similarly, Hull's deductive methodology was intimately connected with his view of the world as a deterministic and hierarchically arranged mechanical system. A logically ordered theory, in his scheme, would mirror the structure of the relevant part of the world, and to deduce an observable consequence of a theory was to mimic the unseen mechanical process by which the world-machine produces its manifest phenomena. Skinner's positivist methodology, like Mach's is grounded in his biologicistic conception of the knowledge process as a type of efficient adaptation to an environment--a process unencumbered by unnecessary inferences and concepts. For him, the venerable methodological device of Occam's razor is finely honed by the demands of biological expedience.

The fact that Tolman, Hull, and Skinner were all working within a broadly Darwinian framework should not be allowed to obscure the important differences between them. Darwinian thought gave impetus to a number of intellectual trends, not all of which were compatible in their implications for psychology. In the hands of Mach and others, Darwinism was taken to support a strict positivism according to which mental constructs and deductive methods were to be eschewed as uneconomical extravagances. In the functionalism and pragmatism of James and his followers, Darwinism meant that mind was an instrument of adaptation. Cognitive activity could be observed in the mind's operations on the environment and mind could be known, perhaps only inferentially, in terms of its manifest functioning in the world. For biological mechanists like Hull, Darwinism called for demonstrations that appropriate arrangements of matter could exhibit the seemingly intelligent phenomena of adaptation. In Hull's case, this meant the use of deductive reasoning to construct mechanisms capable of cognitive behavior, but to do so in such a way that explicitly cognitive concepts were not needed to explain the behavior.

For the neobehaviorists, Darwinism pointed to a behavioristic approach to psychology, but it led to different behavioral methodologies depending on how its implications were construed. The extent of those methodological

differences is suggested in the table below. In it are represented three major methodological issues: the acceptability of inferred constructs in theorizing, the use of explicitly cognitive concepts, and desirability of deductive methods. The table shows that on each of these issues a position held by one of the major neobehaviorists was opposed by the other two. Thus Skinner's eschewal

| | Tolman | Hull | Skinner |
|---------------------|--------|------|---------|
| Inferred constructs | + | + | - |
| Cognitive concepts | + | - | - |
| Deductive methods | - | + | - |

of inferred constructs was not shared by Tolman and Hull, who had no qualms about employing suitably defined unobservables. Tolman's use of cognitive concepts was opposed by Hull and Skinner, who preferred to treat cognitive phenomena as derivative of more basic conditioning principles. And Hull's endorsement of deductive methods was opposed by Tolman and Skinner, in whose pragmatic and anti-mechanistic orientation such methods were regarded as unbeneficial or even harmful. As was pointed out in the Introduction of the present work, neobehaviorism cannot properly be understood without careful attention to the differences between the various neobehaviorists' methodologies or, for that matter, to the deeper differences from which the methodologies arise. On a logical positivist reconstruction, such differences are represented as mere disputes over

selected issues within the context of a generally accepted philosophy of science. But in the richer historical context, the methodological differences can be seen as integral aspects of deeper, fundamentally metaphysical divergences.

The (at least partial) dependence of methodologies on deeper conceptions of nature has a profound bearing on certain traditional conceptions of the role of methodology in the knowledge process. It has often been assumed that specific methods transcend specific theoretical viewpoints within science. In its strongest form, this assumption entails the possibility of crucial experiments, that is, experiments whose outcomes can decide conclusively between two opposed theories. On the logical positivist view, such experiments become possible when rival theories are sufficiently well formalized to permit unambiguous derivations of contradictory observable consequences from them. This possibility was a part of Leibniz's dream--a dream revived by Woodger and others--of a universal language and calculus of thought by which controversy could be eliminated. But if the methods for assessing theories are themselves derivative of and relativized to the same pre-theoretical notions which spawn the theories in question, then the capacity of those methods to arbitrate impartially between theories is seriously undermined. There would no longer appear to be any antecedently defensible neutral means

of choosing between theories. Even logic, which has long been assumed to be the ultimate arbitrator, is susceptible to different interpretations in different world-views. And this is not necessarily a problem only in principle. Hull's notion of a "logical" test of a theory was drastically different from Skinner's idea of developing a scientific psychology by means of "an empirical logic, or a descriptive and analytical scientific epistemology." This was a difference in actual scientific practice, and a difference that suffused the daily research activities of Hull and Skinner. Likewise, Tolman's refusal to heed Hull's call for the systematization of theory and the derivation of consequences reflected the basic differences between them on the "logic" of research and ultimately the differences in their fundamental beliefs.

The conclusions being drawn here about the non-independence of method and theory and the implications of this nonindependence for traditional metatheories of science may strike the positivistic scientist as novel and disturbing. But for the historian of science, as well as those philosophers of science who give serious attention to the history of science, these claims will be familiar. In fact, contemporary philosophers of science routinely acknowledge at least a heuristic, if not also a constitutive or regulative, role to metaphysical beliefs in the scientific enterprise.⁵ The best-known example of such

a philosopher is Thomas S. Kuhn. For Kuhn, the unit of analysis for the philosophy of science is the paradigm. Like a theory, a paradigm includes symbolic generalizations which the paradigm's adherents are committed to testing and articulating. But unlike a theory in the logical positivist sense, a paradigm also includes the meta-physical commitments of its adherents and a set of methodological "values" shared by them.⁶ That is, paradigms come with their own set of methodological standards. For this and other reasons, paradigms are said by Kuhn to be "incommensurable" in the sense that there is no neutral basis for comparing them.⁷ The standards of assessment for a given theoretical approach are thus shifted from a between-paradigm basis to a within-paradigm basis. Or, as one philosopher has put it, "to win is to win in relation to a world view, not to any world view."⁸

Now the psychological theories of Tolman, Hull, and Skinner scarcely qualify as Kuhnian paradigms in any strict sense. But it is revealing nonetheless to consider the respects in which even the rough theoretical formulations of these neobehaviorists exhibit some of the characteristics of paradigms. The explicit theoretical notions they advanced were intimately bound up with implicit world-views. The theoretical notions also came with their own methodological standards, which differed according to their world-views and thus overlapped only partially. As a conse-

quence of these differing standards and the different values attached to the importance of various theoretical problems, the approaches of Tolman, Hull, and Skinner resisted any straightforward comparative assessment, at least by purely empirical means. Arne Naess, who in 1938-39 performed a first-hand study of the rivalry between the Hullian and Tolmanian approaches, has commented that:

Historically, defenders of competing theories do not agree about what is derivable. An instructive example is the systematic and persistent disagreement between the camps of E. C. Tolman and C. L. Hull during the 1930s as to what were the consequences of each other's theories of learning.⁹

Despite the general similarity of broad outlook among the neobehaviorists, the attempt to evaluate them against one another was largely a rather futile effort to compare world-views--or, perhaps more accurately, to compare significant variants of a single broad world-view. To call such an attempt futile is not necessarily to say that in general no means for a comparison exists, but only that mere experiment and empirical observation are inadequate to the task.

The notion raised by Kuhn that methodological standards are not independent of the context of presuppositions surrounding a theory is currently being given serious consideration in the philosophy of science. Following Kuhn's general line, Thomas Nickles has remarked that "it might be suggestive to extend some well-worn metaphors to say

theories are 'value-laden' and scientific values (that is, methodological rules and constraints) are 'theory-laden'."¹⁰ Nickles goes on to point out that the "theory-neutrality" of method was occasionally questioned during the nineteenth century by figures such as Whewell and Peirce. But with the advent of logical positivism in this century, the dominant belief in the essential independence of theory and method was reinforced. There was to be one philosophy of science for all the sciences and a unity of method to apply to them all. Not until a more historical emphasis began to replace the logical emphasis in the philosophy of science did the theory-neutrality of method begin to be seriously questioned once again. Along with Kuhn, other philosophers began to note the great diversity of methods and styles of reasoning revealed in the history of science. Stephen Toulmin, for example, wrote in 1964 that:

In the natural sciences . . . , men such as Kepler, Newton, Lavoisier, Darwin and Freud have transformed not only our beliefs, but also our ways of arguing and our standards of relevance and proof: they have accordingly enriched the logic as well as the content of natural science.¹¹

To be sure, recognizing the diversity and partial theory-ladenness of methodology does not mean that there is no historical continuity or cross-discipline unity of methods in science. But it does raise the possibility that not only do methods change with time but that the change may not even be unidirectional. One of the major points of

the new philosophy of science is that science does not seem to provide cumulative knowledge or knowledge that approaches some final end state. To admit an interdependence of theory and method is to admit that the same points may apply to method. As Nickles puts it:

We might even ask a familiar sort of question in a new form: is the growth of insight and sophistication of our philosophical accounts of methodology cumulative? Such questions are perhaps too large to call for immediate commitment one way or another. ¹²

Indeed, much detailed study of historical cases will be necessary in order to assess the prevalence and significance of the theory-ladenness of method. In any case, the present study can be considered as something of a contribution to that task.

There is one respect in which the present study brings to light a new dimension of the problem raised by the interdependence of theory and method. Those who have remarked on the problem have stressed the fact that historically scientists have adapted theory to method and vice versa. ¹³ This mutual adaptation of theory and method can, and presumably does, take place in any discipline of science. But when the science in question happens to be psychology, the problem is raised in an even more virulent form: by its very nature, psychology studies, inter alia, the knowledge process as part of its subject matter. The means of acquiring knowledge, including methods for doing so, and the conditions under which knowledge is more or less

reliable are topics for inquiry which every significant finding in the psychologies of learning, cognition, and perception have potential bearing on. Among the neo-behaviorists, Skinner has raised this issue most explicitly in his view that methodology is both a guide and a subject matter for psychology. Even if less explicitly, the issue is equally raised by the case histories of Tolman and Hull. A clear example lies in Tolman's invoking of hypotheses about the cognitions of others only after he had begun to attribute them to his subjects. The reflexive relation between psychological accounts and psychological methods is, of course, not limited to neobehaviorist approaches. The Gestalt psychologists also saw a deep connection between their psychological views and their views on scientific epistemology and method.¹⁴ The general point may be made by saying that the problem of reflexivity between theory and method goes beyond the problem of mutual adaptation of theory and method--and the problem of reflexivity is more acute in psychology than in other disciplines.¹⁵ If this assertion is even roughly correct, it suggests that historical analyses of the theory-ladenness of method would find a wealth of highly relevant cases in the history of psychology.

A Reassessment of the Standard Account

In reconsidering the standard account of the behaviorist-logical positivist alliance, it is important to remember that the theories of the major neobehaviorists have traditionally been reconstructed and evaluated from the perspective of logical positivism. This is not surprising given that both movements reached their heydays around the same time, but it is a fact of deep significance for the way behaviorism has been interpreted. Of further significance is the fact that the logical positivist perspective on science was considered to be the perspective on science; philosophy of science was spoken of rather uncritically as "the logic of science." When the most important and thorough analyses of learning theories appeared in Modern Learning Theories in 1954, the authors presented an outline of the analysis to which each theory was subjected. Among the topics for consideration were those of standard logical positivism: the theory-neutrality of the data language, the reducibility of theoretical concepts to physical language, the implicit and explicit definition of primitive terms, the explicitness of axioms and of the derivation of consequences, and so on. Following this outline, there appeared these remarks:

The foregoing outline is undoubtedly somewhat arbitrary insofar as the breakdown into dimensions or categories is concerned. Despite the arbitrariness of the classifications, however, we believe wide agreement would obtain among current writers in the logic of science that an adequate review of any scientific theory must include essentially the same features.¹⁶

In other words, the logical positivist framework was taken to be a neutral, and except for matters of detail, necessary perspective from which to judge theories. The neobehaviorists were all being forced into the same mold, despite the fact that they themselves held standards that differed both from those of logical positivism and from those of one another.

In light of the general similarities between behaviorism and logical positivism--especially in the case of Hull's deductive behaviorism--this was perhaps a natural strategy for evaluating behavioral theories. However, it has had some unfortunate effects. First, the inevitable failure of behavioral theories to fit the logical positivist mold led to the conclusion that, on the one hand, the theories were failures as scientific theories and, on the other hand, that the behaviorists have misinterpreted logical positivist epistemology. Both of these conclusions have been drawn at one time or another by Sigmund Koch,¹⁷ although as noted earlier he views behaviorism as an implausible and self-discrediting position anyway. The second unfortunate effect of judging behaviorism by logical positivist standards

was that it created the presumption that behaviorists have attempted to fulfill those standards in developing their theories. This, in turn, has reinforced the impression that the behaviorists imported methods by which those standards could supposedly be attained.

As we have seen, the thesis that the behaviorists imported their methodologies from logical positivism is one of the chief claims of the standard account. Each of the major neobehaviorists did indeed hold methodological views that were similar to certain views advanced by logical positivism, and each of them had significant personal contact with major figures in logical positivism. Yet they arrived at their views prior to their contact with logical positivism and from perspectives that were quite different from that of logical positivism. Thus, they had no need to import their methodological views because those views had already evolved, in at least rough form, in the context of their own respective presuppositions about epistemology. To be sure, those views were on occasion subsequently expressed in the language of logical positivism, but the importation of terminology does not mean the views themselves were imported.

In describing the importation of methodology from the philosophy of science, Koch has characterized the allegedly imported "new view" as an "uneven fusion" of

logical positivism, neopragmatism, and operationism. This is by no means a wholly misguided characterization. The neobehaviorists did, after all, embrace a sort of neopragmatism; but it seems odd to say that they "imported" it, for behaviorism was all along an integral part of the pragmatist tradition. Similarly, behaviorists were in fact working operationists before Bridgman officially founded operationism, so their post factum "importation" of Bridgman's formulations was more a matter of terminology than of practice. According to Koch, the "dominant contours" of the imported viewpoint were provided by logical positivism. While it may be the case that American psychology as a whole embraced much of logical positivism, the major neobehaviorists were not importing logical positivism; rather they were developing indigenous epistemologies that were actually incompatible with logical positivism.

In further characterizing the importation of the logical positivist-neopragmatist-operationist viewpoint, Koch has stated that "psychology's selections from this cluster of formulations was spotty, adventitiously determined, and not supported by especially expert scholarship in the relevant sources."¹⁸ If this statement is intended to apply to the neobehaviorists--as it appears to be from the context--then it is only partially correct. What the neobehaviorists responded favorably to in these formulations was indeed

spotty: it was exactly those aspects which were consonant with their own world-views and the epistemologies which they drew from them. The statement is also correct in that the neobehaviorists never devoted serious attention to the logical positivists' writings or acquired any expertise in applying their logical distinctions. But the selective responses of the neobehaviorists to the "new view" were anything but adventitiously determined. On the contrary, they were determined by the deepest of considerations, namely, the pre-theoretical beliefs that animated the entirety of their systematic thought.

Closely related to the importation thesis is the second major claim of the standard account. According to the subordination thesis (Mackenzie), the neobehaviorists' importation of logical positivist methodology led them to subordinate subject matter to method. The revised account shows this claim, too, to be problematic. For each of the neobehaviorists considered here, method was deeply linked to substantive views about behavior via an indigenous behavioral epistemology. Rather than subordinating method to subject matter, they were actually coordinating the two. Indeed, once the epistemological views of Tolman, Hull, and Skinner were made explicit by analyzing their presuppositions in the context of their historical development, a remarkable degree of consistency can be discerned in the overall systems they espoused. Their

theories, their general methods, their research findings, and their metaphysics were all brought into harmony by continual adjustments and extensions of their thinking. When inconsistencies did arise in their systems--as when, for a brief period, Tolman held the subject's hypotheses to be knowable without hypotheses on the part of the observer--they were usually quick to make the necessary readjustments.

As part of the subordination thesis, it is usually maintained that behaviorists have held an inappropriate conception of psychology's subject matter. Claims to this effect have been directed against behaviorism since its inception, but despite their prima facie plausibility they only serve to beg deeper issues. Underlying such claims is the misguided and dangerous assumption that there is some preordained single correct way to construe the domain of psychology. But the notion that there is some particular "way the world is" has been roundly discredited by recent work in the philosophy of science.¹⁹ Contrary to the assumptions of traditional empiricist and inductivist approaches to knowledge, a domain of psychology, or any other discipline, can be delineated only with reference to a guiding set of presuppositions. Only within the context of a world-view can certain empirical phenomena and problems be identified as interesting and worthy of pursuit. Putting the matter somewhat crudely, there is

a sense in which the claim that behaviorism has an inappropriate conception of psychology's subject matter amounts to little more than a statement of preference for some other (often implicit) metaphysical viewpoint. Of course, such issues of preference are of deep significance for science, but the statement of preference for one world-view against another cannot in itself show one conception of psychology to be inappropriate. On the contrary, only an explicitly articulated world-view can challenge the appropriateness of another.

It would be difficult to underestimate the extent to which this realization transforms the nature of the problem raised by the critics of behaviorism. In light of the historical analysis provided by the present work, it would appear that the major neobehaviorists can not fairly be accused of inappropriately importing alien methods into psychology and thereby distorting its subject matter. Given the context of their presuppositions, their methods were eminently suited to their subject matter, indeed, carefully coordinated with their subject matter. What can be argued fairly is that their world-views were imported from biology (Tolman and Skinner) or from biology and physics (Hull). To some degree, these world-views certainly were imported from, or at least strongly shaped by, broad conceptions from these disciplines. But whether they were therefore inappropriate for psychology is a different

question--and it is certainly not a question to be decided by legislation. Which particular set of assumptions a scientist works under will depend in fact on the individual's cultural and educational background, but in principle the scientist's adoption of a world-view is free of legislated constraints. What matters is the consistency with which the implications of that world-view are followed up and the long-term consequences of its adoption. Given that the major neobehaviorists were reasonably consistent in articulating and extending their world-views, the problem of assessing behaviorism's world-views then becomes one of evaluating their consequences.

The third major thesis of the standard account of behaviorism and logical positivism is concerned with this question of evaluating behaviorism. The thesis of "linked fates" states that the failure of logical positivism as a philosophy of science implies the untenability of behaviorism (Koch), or that the failure of behaviorism as an approach to psychology discredits logical positivism (Leahey), or perhaps both. The revised account strongly suggests that the intellectual links between behaviorism and logical positivism were far too weak to support the "linked fates" thesis in any of its version. The limited views that behaviorism and logical positivism did hold in common were ones that history shows to have developed in profoundly different contexts. Because of this fact, those parallel

views can be evaluated together only when they are misleadingly reconstructed as having been alike. But to base any evaluation of behaviorism on such a reconstruction is to seriously jeopardize the evaluation from the outset.

With respect to the third thesis of the standard account, the present work can show only the independence of the fates of behaviorism and logical positivism; to pass judgement on their fates would require much analysis beyond the scope of what is presented here. But it is perhaps of interest to briefly sketch the sort of analysis that would be required. As suggested above, the assessment of an intellectual tradition and the world-view from which it arises must focus, in some sense, on its long-term consequences. Of course, what constitutes a consequence, good or bad, is a major philosophical problem. The consequences of a scientific tradition for society as a whole are relevant--indeed behaviorism has been both attacked and defended on this score.²⁰ But for the philosopher of science, the major concern is with those consequences which are (relatively) internal to the scientific enterprise. A promising approach to the evaluation of intellectual traditions has recently been proposed by Larry Laudan.²¹ In barest outline, Laudan's suggestion is that research traditions be evaluated in terms of their relative effectiveness in generating and solving significant cognitive problems. On this view, a tradition is

rationality "pursuitable" (i.e., worthy of the investment of scientific resources] to the extent that current theories within it maximize the rate of problem-solving and minimize the frequency of anomalous problems, relative to competing traditions. By virtue of this last clause, Laudan's approach deems it irrational to abandon a tradition, despite its unresolved anomalies, unless there exists a tradition of greater effectiveness to which the scientist's allegiance may be switched. There is no provision for truth in Laudan's accounts, only for the pragmatic comparative assessment of long-term problem-solving capacity.

Now to apply some such an approach to the research traditions of logical positivism and behaviorism is obviously no easy task.²² Nevertheless, in important respects the task is already being carried out in the case of logical positivism.²³ The secondary and historical literature on logical positivism affords substantial grounds for concluding that logical positivism failed to solve many of the central problems which it generated for itself. Prominent among the unsolved problems was the failure to find an acceptable statement of the verifiability (later confirmability) criterion of meaningfulness. Until a competing tradition emerged (around the late 1950s), the problems of logical positivism continued to be attacked from within that tradition. But as the new tradition in the philosophy of science began to demonstrate its effectiveness-- by dissolving and rephrasing old problems as well as by

generating new ones--philosophers began to shift allegiances to the new tradition, even though that tradition has yet to receive a canonical formulation.

Partly due to a relative paucity of critical secondary and historical literature in psychology, behaviorism is not nearly so amenable to such an assessment. Many psychologists, including non-behaviorists, would affirm that current behavioral theories have continued to generate and solve interesting problems--examples include the problems of self-control and learned helplessness.²⁴ But other enduring problems, such as the nature of reinforcement, have resisted solution.²⁵ It is difficult to say how such successes and shortcomings should be weighted in an overall assessment of behaviorism. More serious complications arise from the ambiguous status of pretenders to the role of competing research traditions. Contemporary cognitive psychology is often upheld as a tradition to rival behaviorism, but serious questions have been raised as to the extent to which it differs from behaviorism as a research tradition.²⁶ Similarly, Chomskian psycholinguistics has been construed as a rival paradigm to behaviorism, but the overlap of problem areas between the two is so small as to cast doubt on the degree of rivalry involved. In short, no serious evaluation of the fate of behaviorism is currently available from the perspective of contemporary philosophy,²⁷ and any attempt to provide such an assessment

would encounter many difficulties. Nonetheless, it is possible to see how such an effort might proceed according to Laudan's approach.

The foregoing considerations do make clear that behaviorism cannot simply be dismissed with purely philosophical arguments or ridicule based on preferences of taste. To be sure, there are indications that behaviorism is a tradition in decline. In the long run, behaviorism may even turn out to be a self-discrediting position, but it is doubtful whether anyone could know that to be the case, much less demonstrate it to others, in the absence of a great deal of serious scholarly inquiry of the sort suggested above. Commenting on the fate of behaviorism, Koch has written:

In my humble opinion, behaviorism is finished. If there is residual motility, it is only that the corpse does not understand my arguments.²⁸

But it takes more than arguments and opinions to bring a tradition to a close. If Laudan is correct, it takes no less than an alternative tradition of demonstrable effectiveness to draw adherents and resources away from the older tradition. Behaviorism would then presumably perish from attrition as scientists abandoned it in an effort to maximize the problem-solving effectiveness of their research activities.²⁹ It remains for future historians to determine which alternative research tradition or traditions will have proved to be instrumental in luring

human and other resources away from behaviorism. In any case, the demise of behaviorism would prove to be a process more dignified and considered than the suggested scenario of its being "laughed out of existence" because of "the ludicrousness of the position."³⁰

Remarks on the Lore of Behaviorism and Logical Positivism

In the Introduction of the present work, it was noted that the alliance of behaviorism and logical positivism has occupied a central place in the lore of twentieth century American psychology. In light of the historical argument developed in the succeeding chapters, the perdurance or even existence of the legend of behaviorism and logical positivism may appear surprising and puzzling. Accordingly, it may be helpful to briefly examine a few of the various sources and modes of transmission of this lore. Of course, Sigmund Koch's works from 1941 to the present have constituted a major focus of the lore. Since these works have been discussed throughout much of the preceding, no further consideration will be given to them here except to reiterate that Koch's own close affiliation with both logical positivism and behaviorism helps account for his confounding of the histories of the two movements.

Boring and Stevens. E. G. Boring and S. S. Stevens, Boring's student and later colleague, were among the first psychologists to take a serious interest in logical positivism

and its implications for psychology. Prior to the American inception of logical positivism, Boring had been introduced to the experiential positivism of his teacher, E. B. Titchener. Thus predisposed toward positivism, he responded favorably to logical positivism and Bridgman's operationism. As a historian of psychology and an experimental psychologist in the field of perception, he embraced logical positivism as a needed historical successor to Machian positivism. The doctrine of physicalism, he felt, would provide a key to making the psychology of perception as objective as behaviorist psychology. In fact, according to Boring, both fields would simply be assimilated to logical positivist psychology or "behavioristics." As early as 1942, Boring was emboldened to write that "[b]ehaviorism ultimately disappeared, in part because in the 1930's it got to be accepted as psychology, and in part because modern positivism became the sophisticated substitute for it."³¹ Stevens, who was a psychophysicist and a proponent of logical positivist psychology, also thought of himself as something of a behaviorist. Like Boring, he felt that sensations could be made objective by reducing them to "discriminated responses."³² Thus, both Boring and Stevens conspicuously identified behaviorism with logical positivism, even though neither was a practicing behaviorist nor even particularly sympathetic to the substance of a behaviorist approach as it was practiced by the major behaviorists.³³

Bergmann and Spence. As was noted in Chapter 7, the articles coauthored by Gustav Bergmann, a major figure in logical positivism, and Kenneth Spence, a major behaviorist, were instrumental in reinforcing the association of behaviorism and logical positivism in the minds of American psychologists. Unlike the three neobehaviorists treated in detail in the present work, Spence appears to have established a genuine intellectual rapport with the adherents of logical positivism and to have accepted it as a philosophy of science--or at least as an ideal toward which psychology could profitably strive. There is no evidence that Spence ever developed an indigenous psychological epistemology. His failure to do so, as well as his acceptance of logical positivism (or "scientific empiricism"), were reflected in the following statements which he published with Bergmann in 1944:

In the schema outlined by the scientific empiricist the experiences of the observing scientist do indeed have a privileged, even unique position. . . . [T]he empiricist scientist should realize that his behavior, symbolic or otherwise, does not lie on the same methodological level as the responses of his subjects. . . . 34

It was just such efforts to draw distinctions between the activities of the knowing scientist and those of the subject that were opposed by the indigenous epistemologies of Tolman, Hull, and Skinner. For them, the scientist's behavior does lie on the same methodological level as the

subject's behavior; as Skinner put it, the behaviorist can not step outside of the causal stream.

But even if Spence must be excluded from the general arguments of the present study, it should be borne in mind that his adoption of logical positivism was, as a matter of historical fact, independent of his adoption of behaviorism. He was a confirmed behaviorist before he completed his graduate work at Yale in the mid-thirties and well before his encounter with Bergmann. Moreover, in the years Bergmann was collaborating with Spence, Bergmann was not advancing logical positivism as a source of methodological prescriptions for the practicing scientist. One of the large number of students who took their doctorates under Spence has recollected Bergmann's teachings as follows:

I remember something Bergmann said to some graduate students at Iowa about fifteen years ago. It was to the effect that, perhaps, in the end--after we had taken his courses in history and systems of psychology, and philosophy of science, and had been led through the methodological elegancies of the distinctions which were possible at a philosophical level between various theoretical approaches to the study of behavior--perhaps, after all of this, we should go into our laboratories as philosophically naive as possible, and leave the philosophy of science to the philosophers. He was saying, as I recollect, that ours was a scientific job that might better be approached with an attitude of doing what comes naturally to the well-trained scientist: accepting the reality of the natural phenomena with which he begins and being more concerned with what he can discover and understand than with what form the discovery must take.³⁵

Feigl and Hempel. The logical positivists Herbert Feigl and Carl Hempel have commented in their writings on the impact of logical positivism on psychology and in particular on behaviorists. The manner in which these comments came about provides an interesting case study in the inadvertent inflation of the lore of behaviorism and logical positivism. Feigl has reported that at the time he brought logical positivism to America in 1930, the behaviorists were among the "closest allies our movement acquired in the United States."³⁶ In itself, this statement made no claim for an actual intellectual influence of logical positivism on behaviorism. In another place, however, Feigl cited the Tolman, Ritchie, and Kalish paper of 1946 as an example of the "applications of Carnap's original analysis [of reduction sentences] to specific psychological concepts."³⁷ Feigl's statement was picked up by Hempel, who in turn wrote that "it is of considerable interest to note with Feigl that Carnap's ideas--and, I should add, Feigl's as well--have met with considerable interest among psychologists and have found applications in the work of such investigators as . . . Tolman, Ritchie, and Kalish. . . ." Twice again in Hempel's classic Aspects of Scientific Explanation (1965), the Tolman-Ritchie-Kalish study was cited as an example of Carnap's influence.³⁸ What is ironic about this situation, of course, is that neither Kalish nor Ritchie was a

behaviorist, and Tolman was not responsible for the use of Carnap's reduction sentences or even particularly sympathetic to such logical distinctions. Yet this series of remarks connecting the name of Tolman with that of Carnap could easily give the impression--in this case, an illusory one--that Tolman had an important intellectual debt to Carnap in regard to reduction sentences.

Neobehaviorist Epistemology and the New Psychologism

Psychological Epistemology and the New Image of Science

The decline of logical positivism and its rather restrictive ways of thinking about science has made room for a rich proliferation of new conceptions of the scientific enterprise. The changes in the philosophy of science during the last twenty-five years have been so dramatic as to qualify as a major intellectual upheaval, if not a full-scale revolution. Amid this upheaval, it is difficult to say whether the wide array of metatheories that have been produced will soon, or ever, coalesce into a single canonical formulation. But despite the diversity of new views there exists among them certain broad consonances that have led to their being grouped together as the "new image" of science.³⁹ In one or another of its versions, the new image admits as relevant to the task of giving a general account of science a host of factors

that were banned from the logical positivist account: the perceptual, cognitive, and social psychology of the scientist; the sociology of scientific groups; the ideological values of scientists and their culture at large; the scientist's metaphysical beliefs; the process of discovery; and the colorful, and sometimes alarmingly quirky, history of scientific activity. In contrast to the "neat image" of science that logical positivism was able to maintain by suppressing such factors in favor of tidy logical distinctions, the family of views which make up the new image has also been referred to as the "gaudy image."⁴⁰

Under whatever name, the new philosophies of science tend to share a set of views by which they are distinctly set apart from logical positivism. Foremost among these views is the notion that science is fundamentally a human activity rather than a linguistic product of such activity. As a result, the appropriate modes of analyzing science are taken to be historical, sociological, and psychological rather than logical. Empirical studies have replaced formal investigations as the substance of metatheories of science. This profound shift of emphasis has been accompanied by various other shifts. Thus, the functions or consequences of scientific activity have taken priority over the structure of theories; the effectiveness of science at solving particular problems has displaced a concern with global truth or certainty; and the discovery of knowledge

is considered more important than its justification.

The new image has relied heavily on the results of investigations in the history of science and has gained from them a deep appreciation of the temporal character of science and the importance of scientific change. Whereas the logical positivists treated science as a static commodity to be dissected by post hoc analyses, the proponents of the new image depict science as an inherently temporal, organic process in which change in its many forms is the rule rather than the exception. Not surprisingly, evolutionary models have been proposed as metatheories of science.⁴¹ In various of these models, research traditions (or concepts or paradigms) are said to vary, to undergo selection, to compete with rivals, to become extinct, and so on. As in biological evolution, the process may typically lead to diversity rather than unity, so that the image of branching becomes more appropriate than that of convergence. Within any one branch or "line of descent," there will be a continuity but not necessarily a cumulative growth of knowledge. Because individual branches are isolated from one another, there will be no global standards of objectivity that can be applied either within or across them. However, evolution within the context of a given line of descent may be mediated by rational decision making. Accordingly, progression can be exhibited within

a line of descent, but this does not mean that progress is being made toward a goal.⁴² No allowance is made for final truth, or even for some fixed state of the world toward which science makes an asymptotic approach.

Even though the history of science has played the leading role in the reconceptualization of science, most of the philosophers who have shaped the new image have been prepared to assign a large role to psychology in the elucidation of science. This assignment is sometimes implicit and sometimes explicit. Laudan, for instance, makes no mention of the psychology of science, but his model of rationality--which he takes to be, in part, a descriptive model--clearly involves assertions as to the cognitive weightings of the problem solutions and anomalies which enter into the decisions of the scientist. Kuhn, on the other hand, has been explicit about the central role of psychology in the metatheory of science:

Already it should be clear that the explanation [of scientific progress] must, in the final analysis, be psychological or sociological. It must, that is, be a description of a value system, an ideology, together with an analysis of the institutions through which that system is transmitted and enforced. Knowing what scientists value, we may hope to understand what problems they will undertake and what choices they will make in particular circumstances of conflict. I doubt that there is another sort of answer to be found.⁴³

Kuhn's emphasis in this passage is on the sociological side of the psychology of science, but elsewhere he has drawn upon experimental work in the psychology of perception

in developing his naturalized account of science. Exactly what role psychology might be expected to fulfill in any future standard version of the new image is, of course, impossible to foresee. In any case, the previous hegemony of logical positivism in the philosophy of science ought to make philosophers skeptical of the desirability or even possibility of standardized epistemologies. The role of psychology in the new image of science can presumably be expected to depend both on specific developments in the metatheory of science and on developments in whatever areas of psychology are deemed relevant.

Needless to say, it is unlikely that any of the specific formulations of Tolman, Hull, and Skinner will figure in current versions of the new image--if for no other reason than that they do not represent the latest developments even in their own tradition. Nonetheless, their overall strategy of developing psychological epistemologies and applying them to science itself has been vindicated in some measure by the recent turn of events in the philosophy of science. Indeed, their efforts in that direction stand as a tribute to the boldness and independence of their thought. It has been written that after around 1920

[t]here was . . . the increasing professional insularity of academic philosophers, and their relative conviction that disciplines such as psychology and sociology, which had played a major role in earlier epistemological theories, had no interesting insights to offer. (This insularity was further promoted by the guileless duplicity of scholars in other fields, who were all too prepared to bequeath "the problem of knowledge" to the professional philosophers.)⁴⁴

In their unwillingness to relinquish epistemology to philosophy, the major neobehaviorists were resisting the anti-psychologistic tenor of their time.

In addition to having anticipated general trends in epistemology, Tolman, Hull, and Skinner developed various specific views on science which are prominent features of current accounts.⁴⁵ All three of them, for example, gave attention to the context of discovery in expressing their methodological views. Thus, Tolman discussed the heuristic value of intervening variables for discovering the functions by which they combine to produce behavior; Hull advocated the designing of automata as a means of discovering principles of conditioning and later described his deductive methods as a tool of discovery; Skinner has concerned himself with such issues as which formulations of concepts will lead to effective behavior on the part of the investigator. This concern of neobehaviorists with scientific discovery came naturally to them in large part because they were all pragmatists. It was pragmatism, after all, that emphasized the capacity of science to lead the investigator on the new discoveries.

Many of the points of congruence between the neo-behaviorist epistemologies and the new image are simply by-products of the fact that both approaches are psychological and, at least in broad terms, pragmatistic.⁴⁶ Like the neobehaviorists, the proponents of the new image view science as an activity to be studied empirically, and most of them reject any transcendent notion of truth in favor of a more pragmatic conception. But not all of the neobehaviorists' anticipations of the new image were of a merely general nature. On the contrary, each of the neobehaviorists held certain specific views which have recently been viewed as insights characteristic of the new image. As we have seen, Tolman was viewing theories as maps as early as 1932, more than three decades before two of the founders of the new image--Stephen Toulmin and N. R. Hanson--made prominent use of that metaphor in their reconceptualizations of science. Another remarkable anticipation of the currently favored perspective on science may be found in Tolman's statement, also of 1932, that each theory in science

is so obviously bound to be wrong. It is twisted out of plumb by the special cultural lack of building materials inherent in the time and place of its origin, as well as by the lack of skill of its individual architect or architects.⁴⁷

Similarly, Hull's early attempt to formulate an empirical epistemology by studying the formation of concepts was a remarkable anticipation of the recent view that the

evolution of concepts is a central concern of the philosophy of science. As one advocate of the new image has stated the problem, "If we can understand how human beings generate their concepts, we will understand the method of inference employed by both scientific and common sense reasoning."⁴⁸ This was precisely the aim of Hull's doctoral research--published in 1920--on the "evolution of concepts." Kuhn's recent attempts to model the process of concept formation are strongly reminiscent of Hull's attempts of half a century before.⁴⁹ Important anticipations of the new image may also be found in Skinner's notion of a scientific verbal community, by means of which a set of concepts and practices are transmitted, and in his insistence on the futility of formal analyses of science.⁵⁰

Ironically, another psychologist who was a relatively early and persistent advocate of the necessity of incorporating psychology into the philosophy of science was Sigmund Koch. In response to the early stirrings of what later became the new image, Koch wrote that
 scholarly culture

seems to be working toward, or inviting into existence, a redefinition of knowledge based on an empirical analysis of inquiry of a sort which must largely depend on psychological modes of analysis. Indeed, extant efforts in this direction everywhere involve psychological commitments, often of a rough and ready sort.

To this passage, Koch added the following remarks:

Yet psychology seems hardly cognizant of the challenge implicit in these circumstances. Or of the circumstances.⁵¹

The irony, of course, lies in the fact that the major neo-behaviorists--two of whom Koch had studied with considerable acuity--had not only been aware of the challenge of a psychological epistemology but had pursued that challenge throughout their careers. Unfortunately, Koch had had scant opportunity to be acquainted with the epistemological efforts of the neobehaviorists. Hull never published his behavioral epistemology, probably out of ill-advised (but realistic) deference to the dominant logical positivism. Tolman's epistemology was largely only implicit in his writings and in any case could not be appreciated in its full extent without historical insight into the development of his thought. And by the time Skinner's epistemological writings appeared in clear and explicit form, Koch's disaffection with behaviorism was apparently so great as to preclude his taking them seriously.

Psychologism and the Foundations of Knowledge

"The most common objection to psychologism was that it failed to give an adequate account of the objectivity of knowledge."

--David Lindenfeld (1980)⁵²

"There is no less room for pluralism in meta-science than in science."

--Arne Naess (1972)⁵³

The re-emergence of psychologism in this century is an intellectual phenomenon of considerable significance for both philosophy and psychology. It promises to bring the two disciplines closer together than they have been since their divorce began in the late nineteenth century. The division of labor that started to crystallize at that time is no longer viewed as favorable to the combined productivity of the two disciplines, even though no new arrangements have been finalized. The traditional complaint that psychologism cannot do justice to the objectivity of knowledge is no longer given the ground it once was. From the psychologist's perspective, the complaint seems to lose ground as a better understanding of the knowledge processes is secured by means of continuing research in the relevant areas of psychology. From the philosopher's perspective, on the other hand, the complaint fails more because the traditional standards of objectivity are losing the stature previously accorded to them. Thus, it would appear that the general problem of objectivity which has confronted psychologism is gradually being dissolved from both ends. All that is needed to round out the new image of science, it seems, is a firmer understanding of the psychological components of science. As Koch has put it: "philosophy and, more generally, the methodology of science are beginning to stand on foundations that only psychology could render secure."⁵⁴

In the short run, some such arrangement may appear quite workable. But in the long run, it would seem that an arrangement characterized in this way would be inherently unstable. By the new image's own account, psychology can be expected to evolve and undergo continued diversification into a variety of mutually incommensurable research traditions. Each tradition will have its own gradually evolving context of theoretical, methodological, and metaphysical presuppositions. These interacting presuppositions form the important historical backdrop for the tradition, but they cannot properly be said to serve as its foundations in any stronger sense. But if psychology is itself without stable foundations, then it can scarcely serve as even a partial foundation for the new image. Either the new image must give up its psychologism or else it must give up any claim to rest on stable foundations.

Western philosophy has routinely sought to ground knowledge on firm foundations. Throughout much of its history, substantive principles--theories such as Euclid's geometry or Newtonian mechanics--have been invoked as solid bases for the construction of knowledge. When the development of non-Euclidean geometries and relativistic physics cast doubt on the feasibility of substantive foundations, the attempt was made (by Karl Pearson and others) to shift the foundations of knowledge from theory to methodology. For the logical positivists, all real knowledge was to be

erected on the two foundations of logic and empirical observation. Given the force of this tradition, there is an understandable reluctance to abandon the notion that an absolute, or at least stable, basis can be found for knowledge.

The new image has achieved considerable enlightenment by questioning the general feasibility of a foundational approach to epistemology and it has done so with respect to science--the Western world's most cherished exemplar of knowledge. What it has not generally achieved, however, is a clear application of its new approach to itself. The impression that the new image, or some version of it, will provide a final formulation of the nature of knowledge--albeit at the level of metatheory--can only invite the presumption that the foundations of knowledge will reside at the meta-level. But to understand the implications of the new image is to have serious reservations as to the possibility of any final account of knowledge.

The failure of the new philosophy of science to make plain its implications for itself has been discussed by Arne Naess in his provocative monograph The Pluralist and Possibilist Aspect of the Scientific Enterprise (1972). Taking the Kuhnian approach as an example, Naess argues that just as the paradigms of science color the observations of scientists working within their confines, so too the metatheoretical paradigms of historians and philosophers

of science color their observations of scientific activity. In other words, the history of science--the major body of evidence for the new image of science--cannot yield neutral data any more than scientific observation can. Yet Kuhn treats the historical evidence for his metatheory as conferring validity on it and as disconfirming the "text book history of science," even though he denies that such a straightforward evidential relation can obtain for the scientist except within the limited confines of a paradigm.

Naess writes:

In spite of the role of the new historiography as a new paradigm of historical and systematic research of science ('science of science'), it is not explicitly conceived as such by its representatives. Their own perspective, which stresses the broad historical relativity of the narrow absolutistic pronouncements of paradigm-makers and their followers, is not applied reflexively to the pronouncements of the representatives and advocates of the new historiography.⁵⁵

Kuhn's belief that the conception of science advanced by him "can emerge from the historical record of the research activity itself" reveals his confinement within a meta-theoretical paradigm;⁵⁶ the presuppositional context licenses such an inductivist view of evidence, but it also makes his belief a relative, not general, one. Referring to Kuhn, Naess continues:

He is caught in his own relativism. So far as I can see, the new historiography cannot, on Kuhn's premises, properly claim any special status in relation to other historiographies. Therefore the outsider, for instance the metahistorian, who is interested in the history of conceptions of history is justified on Kuhn's premises in taking his picture as only one of the many that have been offered since Aristotle.⁵⁷

But if Kuhn is caught up in his own metatheoretical paradigm, his account must be admitted to suffer the consequences. A paradigm, to be sure, is a useful set of presuppositions, but in no way can it be considered uniquely valid or universal or to be a final means of accounting for a domain of phenomena. In short, it cannot serve as an ultimate foundation for knowledge.

If Naess's argument is at all applicable to other versions of the new image, it suggests that they too are ineligible to qualify as final or foundational accounts of science. Furthermore, it suggests that they may be superseded by any one of an unlimited number of possible alternative images of science. To use the new image's own evolutionary model, there must be significant variants in the conceptual pool if the evolution of metascience is to progress. But this is not, of course, to say that progress is toward any final state. Like theories in science, theories of science proceed in a sort of bootstrap operation with no endpoint.

The implication of the foregoing considerations is that psychologism can be reintroduced into epistemology with-

out endangering its foundational status, simply because there will not be any such status to worry about. Once the new image is applied to itself, it becomes clear that at root psychology and epistemology share the same status as knowledge systems, and the way is cleared for a consistent psychologism.⁵⁸ This continuity of science with metascience means that both enterprises involve a pluralism of partially insulated research traditions. In both cases, these traditions can be conceived as presuppositional contexts which are to be assessed in terms of their ability to generate and solve cognitive problems.

In the case of psychology and epistemology, it remains to be seen which of the several viable traditions in these fields will yield up cognitive problems deemed worthy of the investment of resources by the traditions of the other field. Only very recently has the process begun in earnest.⁵⁹ Perhaps the most that can be said at this point is that after a period of relative hegemony of dominant traditions in psychology and epistemology, these disciplines seem to be undergoing a healthy proliferation of novel variations.⁶⁰ Such pluralism makes available a large number of incipient research traditions that can be combined across disciplines in an even larger number of ways for the possible sharing of significant unsolved problems. The phenomenon of knowledge appears to be an extremely complex one which offers an inexhaustible number of aspects for possible investigation.

Perhaps it is only appropriate then that this complexity be matched by a proliferation of cross-fertilizing approaches within psychology and philosophy. Should some such arrangement prove to be workable, then the two disciplines might be able to combine resources and understanding in a fruitful relationship of a sort that behaviorism and logical positivism never managed to achieve.

Notes for Chapter 10

1. Howard E. Gruber, "Darwin on Psychology and Its Relation to Evolutionary Thought," in Psychology: Theoretical-Historical Perspectives, ed. R. W. Rieber and Kurt Salzinger (New York: Academic Press, 1980), pp. 145-174, on p. 155.
2. Arne Naess, The Pluralist and Possibilist Aspect of the Scientific Enterprise (Oslo: Universitetsforlaget, 1972), p. 107.
3. Grace Adams, Psychology: Science or Superstition? (New York: Covici, Friede, 1931), p. 282.
4. The metaphorical character of Darwin's own biological concepts has been discussed in Howard E. Gruber, "Darwin's 'Tree of Nature' and Other Images of Wide Scope," in On Aesthetics in Science, ed. J. Wechsler (Cambridge, Mass.: MIT Press, 1978), pp. 127-131. Other examples of Skinnerian metaphors are his Machian notion of "biological economy" and his notion of the "shaping" of behavior by phylogenetic and ontogenetic contingencies. An obvious intrusion of Humean-Machian metaphysics into Skinner's systematic psychology is his notion of "adventitious" or "superstitious" reinforcement, a notion which rests heavily on radical empiricist assumptions about causality. See B. F. Skinner, "'Superstition' in the Pigeon," Journal of Experimental Psychology 38 (1948): 168-172. This notion has been seriously challenged by recent research on the nature of reinforcement. See J. E. R. Staddon and Virginia L. Simmelhag, "The 'Superstition' Experiment: A Reexamination of Its Implications for the Principles of Adaptive Behavior," Psychological Review 78 (1971): 3-43.
5. See, for example, Marx W. Wartofsky, Models: Representation and the Scientific Understanding (Dordrecht, Holland: D. Reidel, 1979). This work contains a number of chapters which address the crucial role of metaphysics in the construction of scientific theories.
6. Thomas S. Kuhn, "Postscript--1969," in The Structure of Scientific Revolutions, 2d ed., enl. (Chicago: University of Chicago Press, 1970), pp. 174-210, on pp. 184-186.

7. It should be pointed out that Kuhn's own arguments for the incommensurability of paradigms rest more on the alleged lack of a neutral observation base (i.e., on the theory-ladenness of perception) than on the absence of neutral methodological standards (the theory-ladenness of values). It is the latter issue that is of particular relevance for the present work.

8. Naess, Pluralist and Possibilist Aspect, p. 40.

9. Ibid., p. 72. For a summary of Naess's study of the rivalry between the Hullian and Tolmanian camps, see *ibid.*, pp. 37-38.

10. Thomas Nickles, "Heuristics and Justification in Scientific Research: Comments on Shapere," in Structure of Scientific Theories, 2d ed., enl., ed. Frederick Suppe (Urbana, Ill.: University of Illinois Press, 1977), pp. 571-589, on p. 579.

11. Quoted in *ibid.*, p. 581.

12. Ibid., pp. 581-582.

13. Larry Laudan, Progress and Its Problems (Berkeley: University of California Press, 1977), pp. 59-60.

14. See W. W. Bartley, III, "Theory of Language and Philosophy of Science as Instruments of Educational Reform: Wittgenstein and Popper as Austrian Schoolteachers," in Method and Metaphysics: Methodological and Historical Essays in the Natural and Social Sciences, ed. Robert S. Cohen and Marx W. Wartofsky (Dordrecht, Holland: D. Reidel, 1974), pp. 307-337, on pp. 14, 21.

15. Perhaps social psychology and sociology (especially cognitive sociology) should also be grouped with psychology in this statement, although the present work does not bear directly on this issue.

16. "Introduction," in William K. Estes, Sigmund Koch, Kenneth MacCorquodale, Paul E. Meehl, Conrad G. Mueller, Jr., William N. Schoenfeld, and William S. Verplanck, Modern Learning Theory (New York: Appleton-Century-Crofts, 1954), pp. i-xv, on p. xiv.

17. These conclusions may be found, respectively, in Sigmund Koch, "Clark L. Hull," in Modern Learning Theory, pp. 1-176 and "Value Properties: Their Significance for Psychology, Axiology, and Science," in The Anatomy of Knowledge, ed. Marjorie Grene (Amherst, Mass.: University of Massachusetts Press, 1969), pp. 119-148, on p. 119.

18. Sigmund Koch, "Psychology and Emerging Conceptions of Knowledge as Unitary," Behaviorism and Phenomenology: Contrasting Bases for Modern Psychology, ed. T. W. Wann (Chicago: University of Chicago Press, 1964), pp. 1-41, on p. 10.

19. The most widely known example of such work is, of course, that of Thomas Kuhn, who claims that the world is constructed according to the assumptions of one's paradigm and that one's world changes with a change in paradigm (Structure of Scientific Revolutions, especially Chapter 10). But almost all the recent historically oriented work in the philosophy of science bears out a similar conclusion as to the non-uniqueness of world-views. The classic source on this notion, one written from the perspective of analytic philosophy rather than a historical perspective, is Nelson Goodman, "The Way the World Is," Review of Metaphysics 14 (1960): 160-167. This conclusion is also supported from the perspective of psychology, particularly by recent research on the constructive nature of perception and memory.

20. Thus, some scholars complain that behaviorism has a dehumanizing influence on society, while others claim that the application of behavioral principles to society is the only effective way to achieve humanistic aims. The issue of conflicts between scientific research traditions and general cultural world-views has been briefly discussed by Laudan, Progress and Its Problems, pp. 61-64.

21. Ibid.

22. Although it may strike some readers as odd to evaluate a philosophical research tradition in the same general terms as one would evaluate a scientific research tradition, Laudan explicitly claims that his model is appropriate for nonscientific as well as scientific enterprises (ibid., pp. 8, 13, 189-192).

23. See, for example, Harold I. Brown, Perception, Theory and Commitment: The New Philosophy of Science (Chicago: University of Chicago Press, Phoenix Books, 1977). Brown's lengthy discussion of logical positivism (pp. 15-77) focuses on its failures to solve the significant problems which it set for itself as a research tradition (see especially pp. 10, 76-77).

24. See, for example, Howard Rachlin and Leonard Green, "Commitment, Choice and Self-Control," Journal of the Experimental Analysis of Behavior 17 (1972): 15-22; M. E. P. Seligman, Helplessness (San Francisco: W. H. Freeman, 1975). Moreover, these problems (especially the latter) have had a widespread influence on psychology, including the areas of social and clinical psychology.

25. For example, there is still no widespread agreement on the issue of whether Pavlovian and operant conditioning are two separate processes or, for that matter, whether the commonly studied "operant" behaviors are truly operants, or Pavlovian responses, or even biological artifacts. For a good introduction to current theoretical issues in conditioning, see Barry Schwartz, Psychology of Learning and Behavior (New York: W. W. Norton, 1978).

26. See note 8 in Chapter 1 above.

27. In his unpublished doctoral dissertation, Daniel Rochowiak has examined classical behaviorism in its relation to the tradition of Darwinian evolutionary theory, but his analysis does not include the various neobehaviorisms that are considered here. See Daniel Rochowiak, "Evolutionary Psychology and Behaviorism: The Methodology of Interdisciplinary Research," Ph.D. dissertation, University of Notre Dame, 1980. In a passing remark on behavioristic psychology, Larry Laudan suggests that behaviorism has shown a high rate of problem-solving but that its overall adequacy remains low (Progress and Its Problems, p. 107). However, Laudan gives no justification for this assessment.

28. Sigmund Koch, "Psychology as Science," in Philosophy of Psychology, ed. S. C. Brown (New York: Barnes & Noble, 1974), pp. 3-40, on p. 4.

29. To judge by some psychologists' talk about the replacement of behaviorism with cognitive psychology, one might get the impression that such attrition is currently proceeding full-scale. An empirical analysis of such relevant phenomena as journal submissions, graduate training, and decisions of granting agencies could presumably shed light on the accuracy of this impression. Of some relevance

29. (Cont'd.) here is a study by Martha Chappell Dean. Basing her conclusions on a statistical analysis of citations within a single major behaviorist journal, Dean suggests that the behaviorist research tradition scores high on various "indicators of epistemological health." However, this study does not address the wider issue of whether the behaviorist tradition is suffering attrition relative to competing traditions. See Martha Chappell Dean, "A Quantitative Analysis of Theory Change in Experimental Operant Psychology," paper presented at the fifth annual meeting of the Society for Social Studies of Science, Toronto, October 17-19, 1980.

30. Koch, "Emerging Conceptions," p. 20.

31. Edwin G. Boring, Sensation and Perception in the History of Experimental Psychology (New York: Appleton-Century, 1942), pp. 13, 12.

32. S. S. Stevens, "The Operational Definition of Psychological Concepts," Psychological Review 42 (1935): 517-527.

33. B. F. Skinner has noted that the Boring-Stevens brand of operationism has been referred to as "an attempt to climb onto the behavioristic bandwagon unobserved"; but he argues that it was in fact "an attempt to acknowledge some of the more powerful claims of behaviorism (which could no longer be denied) but at the same time to preserve the old explanatory fictions." See B. F. Skinner, "The Operational Analysis of Psychological Terms," in Cumulative Record (New York: Appleton-Century-Crofts, 1959), pp. 272-286. This article appeared originally in the Psychological Review 52 (1945): 270-277, 291-294.

34. Gustav Bergmann and Kenneth W. Spence, "The Logic of Psychophysical Measurement," in Readings in the Philosophy of Science, ed. Herbert Feigl and May Brodbeck (New York: Appleton-Century-Crofts, 1953), pp. 103-119, on p. 105. This article was originally published in the Psychological Review 51 (1944): 1-24.

35. Abram Amsel, "On Inductive versus Deductive Approaches in Neo-Hullian Behaviorism," in Scientific Psychology, ed. Benjamin B. Wolman and Ernest Nagel (New York: Basic Books, 1965), pp. 187-203, on p. 188.

36. Herbert Feigl, "The Wiener Kreis in America," in The Intellectual Migration: Europe and America, 1930-1960, ed. Donald Fleming and Bernard Bailyn (Cambridge: Harvard University Press, Belknap Press, 1969), pp. 630-673, on p. 661.

37. Herbert Feigl, "Principles and Problems of Theory Construction in Psychology," in Current Trends in Psychological Theory (Pittsburgh: University of Pittsburgh Press, 1951), pp. 179-213, on p. 192.

38. Carl Hempel, "Logical Positivism and the Social Sciences," in The Legacy of Logical Positivism: Studies in the Philosophy of Science, ed. Peter Achinstein and Stephen F. Barker (Baltimore: Johns Hopkins Press, 1969), pp. 163-194, on p. 183; Aspects of Scientific Explanation and Other Essays in the Philosophy of Science (New York: Free Press, 1965), pp. 187, 189.

39. See Brown, Perception, Theory and Commitment, p. 10.

40. Naess, Pluralist and Possibilist Aspect, pp. 9-10.

41. Prominent examples are Laudan, Progress and Its Problems; Stephen Toulmin, Human Understanding: The Collective Use and Evolution of Concepts (Princeton, N.J.: Princeton University Press, 1972); and Donald T. Campbell, "Evolutionary Epistemology," in The Philosophy of Karl Popper, ed. Paul A. Schilpp (La Salle, Ill.: Open Court, 1974), pp. 413-463.

42. The implications of the non-goal-directed character of evolution for evolutionary models of science have been spelled out by Kuhn, Structure of Scientific Revolutions, pp. 170-173.

43. Thomas S. Kuhn, "Logic of Discovery or Psychology of Research?" in Criticism and the Growth of Knowledge, ed. Imre Lakatos and Alan Musgrave (Cambridge: Cambridge University Press, 1970), pp. 1-23, on p. 21.

44. Laudan, Progress and Its Problems, p. 1.

45. In speaking here of the neobehaviorists' "anticipations" of the new image, I am using the term only descriptively. That is, I do not mean to imply that any of the neobehaviorists discussed herein were prophetic, or even especially insightful, in advancing their respective views of science. (In fact, in each case their views were so strongly grounded in nineteenth-century thought that they could reasonably be considered to have been regressive.) Rather, their views are being compared with those of the new image in order 1) to emphasize their divergence with the logical positivists and 2) to illustrate that their notions of science continue to be fruitful ones.

46. It should be pointed out that the pragmatistic aspects of the new image, unlike those of the neobehaviorist views of science, are not necessarily biological in character. Thus, Laudan endorses a broadly pragmatic notion of truth, but vehemently denies the traditional pragmatist idea that the aims of science are largely utilitarian and eventually grounded in biological necessity (Progress and Its Problems, pp. 223-225). It should also be stressed that many proponents of the evolutionary model of science advocate it as simply an analogy--as opposed to, say, Skinner, for whom science is literally a part of biological evolution.

47. Edward C. Tolman, Purposive Behavior in Animals and Men (New York: Century, 1932), p. 394.

48. Walter B. Weimer, Notes on the Methodology of Scientific Research (Hillsdale, N.J.: Lawrence Erlbaum, 1979), p. 70.

49. Hull's dissertation research was reported in Clark L. Hull, "Quantitative Aspects of the Evolution of Concepts: An Experimental Study," Psychological Monographs 28 (1920): p. 85. Kuhn's efforts are reported in Thomas S. Kuhn, "Second Thoughts on Paradigms," in The Structure of Scientific Theories, 2d ed., enl., ed. Frederick Suppe (Urbana, Ill.: University of Illinois Press, 1977), pp. 459-482.

50. As far as I know, the only published work to point out the similarities between Skinner's views of science and some of the more recent formulations is Mark Burton, "Determinism, Relativism and the Behavior of Scientists," Behaviorism 8 (Fall 1980): 113-122. That the views on science of so major a psychological figure as Skinner have been routinely ignored by philosophers is in itself an interesting indication of the mutual isolation of philosophers and psychologists.

51. Koch, "Emerging Conceptions," p. 5.
52. David F. Lindenfeld, The Transformation of Positivism: Alexius Meinong and European Thought, 1880-1920 (Berkeley: University of California Press, 1980), p. 101.
53. Naess, Pluralist and Possibilist Aspect, p. 84.
54. Koch, "Emerging Conceptions," p. 5.
55. Naess, Pluralist and Possibilist Aspect, p. 116.
56. Kuhn, Structure of Scientific Revolutions, p. 2. Kuhn even goes so far as to speak of "deriving" observable consequences from his own metatheory, in much the same way that a logical positivist would speak of the relationship between fact and theory: "My descriptive generalizations are evidence for the theory precisely because they can also be derived from it, whereas on other views of the nature of science they constitute anomalous behavior" ("Postscript--1969," in *ibid.*, p. 208). Elsewhere, Kuhn does acknowledge that his anomalous cases cannot by themselves falsify the competing logical positivist theory of science because the logical positivists can always make ad hoc modifications of their theory (*ibid.*, p. 78). However, he still shows no signs of applying this somewhat more sophisticated insight to his own view of science.
57. Naess, Pluralist and Possibilist Aspect, p. 117.
58. Most advocates of the new image have been explicit, although in varying degrees, about assigning an (at least approximately) equivalent status to scientific enterprises (e.g., psychology) and philosophical enterprises (e.g., epistemology). For example, Brown writes that his recent book constitutes "one argument for the thesis that there are fundamental similarities between scientific method and philosophic method (Perception, Theory and Commitment, p. 11). Exactly how the division of labor between science and philosophy will be drawn in the future remains to be seen. In any case, the demarcation, if any, can probably be expected to evolve and change through time.
59. See, for example, Lee Ross and Richard E. Nisbett, Human Inference: Strategies and Shortcomings of Social Judgment (Englewood Cliffs, N.J.: Prentice-Hall, 1980); Stephen P. Stich and Richard E. Nisbett, "Justification and the Psychology of Human Reasoning," Philosophy of Science 47 (1980): 188-202; and Ryan D. Tweney, Clifford R. Mynatt, and Michael E. Doherty, On Scientific Thinking (New York: Columbia University Press, 1981).

60. The desirability, or even inevitability, of a pluralism of conceptual viewpoints in psychology has long been a prominent theme in the writings of Sigmund Koch. For a recent statement, see his "Language Communities, Search Cells, and the Psychological Studies," in Nebraska Symposium on Motivation 1975, ed. William J. Arnold (Lincoln, Neb.: University of Nebraska Press, 1976), pp. 477-559. It is perhaps significant that, despite the differences between Koch's rendering of the behaviorist-logical positivist alliance and the interpretation presented here, the prescriptive conclusions to which I have been led through a consideration of this episode are in substantial accord with those previously drawn by Koch.

ANNOTATED BIBLIOGRAPHY

SOURCES

This section will provide a review of the various sources of material relevant to a history of behaviorism and logical positivism. This review is not intended as an exhaustive survey but rather as a selective overview designed to convey the scope of available resources. As of this writing, some of the sources cited here have been extensively researched in the secondary literature, while some remain as yet uninvestigated.

Primary sources (volumes). Although the roots of logical positivism go back at least to David Hume, the important immediate forebears of the movement are Ernst Mach (1883, 1886) and Henri Poincaré (1902/1952). Important anticipations of ideas central to logical positivism are also found in Gottlob Frege (1884), Moritz Schlick (1918), and Ludwig Wittgenstein (1922). The doctrines most characteristic of logical positivism found their clearest expression in a series of books by Rudolf Carnap (1928a, 1928b, 1935), whose productivity and logical acumen made him the central intellectual figure of the Vienna Circle (whereas Schlick is usually acknowledged to have been the central social figure). Carnap's counterpart in the Berlin branch of the logical empiricist movement was Hans Reichenbach (1938). Among the important figures who

strongly influenced the Vienna Circle from its periphery are Wittgenstein, Bertrand Russell (1921, 1927), and Karl Popper (1935). A. J. Ayer's (1936) forceful exposition of logical positivist views had a great influence on philosophy in the English-speaking world. Some American philosophers, such as W. V. O. Quine and Charles Morris, were predisposed toward logical positivism by the pragmatism of C. S. Peirce, William James, and John Dewey, and had already encountered a close approximation to the verifiability principle in the operationism of Percy Bridgman (1927). Perhaps the crowning achievement of logical positivism is the International Encyclopedia of Unified Science (1955), conceived and edited (with others) by Otto Neurath. Foremost among a number of anthologies devoted, in part or whole, to logical positivism is a volume edited by Ayer (1959b) which contains an extensive bibliography on the topic.

On the psychological side, the first book-length articulation of behaviorism was John B. Watson's Behavior: An Introduction to Comparative Psychology (1914). Watson elaborated his behaviorism in two subsequent books (1919, 1924), the latter of which introduced behaviorist ideas to the Vienna Circle. Watson shied away from philosophy, but the major post-Watsonian behaviorists acknowledged their affinity for positivistic philosophy. The magnum opuses of these neobehaviorists are Tolman's Purposive Behavior in Animals and Men (1932), Skinner's Behavior of

Organisms (1938), and Hull's Principles of Behavior (1943). Of subsidiary importance are works by Tolman (1966), Skinner (1953, 1957), Hull (1951, 1952), Guthrie (1935), and Spence (1960). Although neither was a practicing behaviorist, E. G. Boring (1933) and C. C. Pratt (1939) wrote books which, in a spirit congenial to behaviorism, applied operational and positivistic analyses to psychological issues.

Primary sources (journals). Among the philosophical journals relevant to the topic of behaviorism and logical positivism, the most important is the Vienna Circle organ Erkenntnis, which was edited by Carnap and Reichenbach during the 1930s. Its name was changed to The Journal of Unified Science before succumbing to the war, and it was revived under the title Erkenntnis in the 1970s. This journal contains a multitude of classic papers, including Carnap's "Elimination of Metaphysics Through Logical Analysis of Language" (1931-32/1959), Schlick's "Positivism and Realism" (1932-33/1959), and Neurath's "Protocol Sentences" (1932-33/1959). Many of the works which provided the background of logical positivism appeared in the journals Mind (G. E. Moore, Bertrand Russell), The Monist (Russell, C. S. Peirce, John Dewey), and the Journal of Philosophy, Psychology, and Scientific Method (William James, various neorealists). The latter became The Journal of Philosophy in 1920, and printed in 1931 Herbert Feigl and Albert

Blumberg's important "Logical Positivism, A New Movement in European Philosophy," From its founding in 1934, the American journal Philosophy of Science has published important articles by philosophers (Carnap, Feigl, Gustav Bergmann) and psychologists (Tolman, Hull, S. S. Stevens). Other journals which carried developments in logical positivism include Analysis, Philosophical Review, Philosophy and Phenomenological Research, and Synthèse.

Because of their theoretical thrust, the psychological journals most relevant to the developments discussed herein are Psychological Review and Psychological Bulletin. The former contains most of the important theoretical statements of Hull and Tolman, as well as the Presidential Addresses of the American Psychological Association. It also contains papers given by Skinner (1945) and others at the important Symposium on Operationalism. The latter contains early debates on the relation between psychology and philosophy and Stevens's (1939) famous integration of psychology, operationism, and logical positivism. The late-thirties explosion of articles on psychology and philosophy of science nearly filled the pages of both of these journals. The Journal of General Psychology published B. F. Skinner's early writings, including his two crucial theoretical papers (1931, 1935), as well as a few by Hull and Tolman. Other neobehaviorist works have appeared in

The Journal of Experimental Psychology, The Journal of Comparative Psychology, and (more recently) The Journal of the Experimental Analysis of Behavior.

Secondary sources. The most detailed previous works on the topic of behaviorism and logical positivism are those by Koch (1959, 1961, 1964) and Brian D. Mackenzie (1972, 1977). Koch's admittedly elliptical historical reviews depict the alliance of these positions as one of expedience and polemic rather than a convergence of understanding and intellectual achievement. These reviews are based largely on Koch's earlier conceptual analyses of Hullian theory (1941, 1954) and his survey of the status of systematic psychology (1959). As a consequence, his historical reviews tend to emphasize the behaviorism of Hull over other versions (especially Skinner's) and to rely more on conceptual analysis than historical analysis. Additionally, these reviews are unavoidably limited by their brevity to fairly broad characterizations of behaviorism and logical positivism.

Mackenzie's volume (1977) does not suffer as badly from limitations of length, but its treatment of behaviorism in relation to logical positivism is limited in two very general respects. First, the treatment is intended to be more philosophical than historical and is strongly evaluative rather than descriptive. Second, Mackenzie's attention to logical positivism is necessarily restricted by the broad scope of the book, which includes discussion of such topics

as behaviorism's background in comparative psychology and a defense of the introspectionist tradition.

Although there has been little secondary literature devoted to the topic of behaviorism and logical positivism, historical accounts of either domain alone are not lacking. For example, the history of logical positivism has been traced in essays by Feigl (1943) and Ayer (1959a) and in brief volumes by Joergenson (1951) and Kraft (1953). The history of behaviorism has been treated, for example, by O'Neil (1968), Herrnstein (1973), and Roback (1937). Papers with a historical orientation have occasionally appeared in many of the journals, both philosophical and psychological, cited above as primary sources. Two relatively new journals containing relevant articles are The Journal of the History of the Behavioral Sciences and Behaviorism.

For general background, Passmore's (1966) history of recent philosophy admirably situates logical positivism in its intellectual context. Schneider's (1963) history of American philosophy describes some of the developments which conditioned America's receptivity to logical positivism. Two recent works (Morris, 1970; Thayer, 1968) on pragmatism discuss its relation to both the behavioral sciences and logical positivism. The Encyclopedia of Philosophy contains numerous articles on topics and persons central to logical positivism.

Useful general accounts of behavior theory are provided by Hilgard and Marquis (1940) and Hilgard and Bower (1966). A set of lengthy critical essays on the major neobehaviorist positions appeared as Modern Learning Theory (Estes et al., 1954), a work which coincided with, and probably contributed to, the demise of behaviorism's "systems." Melvin Marx's series of anthologies (Marx, 1951, 1963; Marx & Goodson, 1976) contains several examples of neobehaviorist theorizing, including Clark Hull's address to the Sixth International Congress for the Unity of Science. This series of anthologies is of interest in part for the significant changes in material selected for inclusion.

Biographies and autobiographies. The autobiographies of Carnap (1964), Popper (1976), and Ayer (1977) contain valuable inside information on developments in logical positivism. A recent biography (Cohen, 1979) of John B. Watson provides material on the early development of behaviorism. The second volume (1979) of B. F. Skinner's autobiography covers the important period from 1928 to 1947 and includes brief accounts of his interactions with numerous figures involved in the Unity of Science movement (Tolman, Hull, Fiegl, W. V. O. Quine, Carnap, L. J. Henderson, Russell, I. A. Richards, etc.). Edited selections (1962) from Hull's "idea books" give a fascinating glimpse of his ambitions and frustrations. The multi-volume A History of

Psychology in Autobiography includes autobiographical statements by several behaviorists (e.g., Tolman, 1952).

Archival sources. A number of archival sources have been tapped for this dissertation. Prominent among these are Charles Morris's Unity of Science Collection at the University of Chicago and various collections at the Archives of the History of American Psychology in Akron, Ohio. The former contains mostly letters between Morris and supporters of the movement, including Hull, Brunswik, S. S. Stevens, Bridgman, Neurath, Feigl, Dewey, and Russell. Many deal with practical matters such as publicity and fund-raising for the International Congresses. The Akron archives contain correspondence, course bibliographies, manuscripts, and lecture notes for such figures as Tolman and Brunswik. Some of Tolman's personal library remains at the University of California, but unfortunately the bulk of his papers remain in the hands of a would-be biographer in England. An extensive collection of Hull's papers resides at Yale University. Among these papers is Hull's complete set of "idea books," E. G. Boring's papers including his correspondence with his student and fellow champion of operationism, S. S. Stevens, are a useful collection housed at Harvard University.

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