Meehl, P. E. (1991). Preface to *Selected philosophical and methodological papers* (C. A. Anderson and K. Gunderson, Eds.). Minneapolis: University of Minnesota Press. Pp. *xv-xxx*

Preface

There are three reasons why a psychologist puts on the philosopher's hat, or sometimes precariously wears both hats simultaneously: (I) Metalinguistic discourse in the ordinary practice of scientific writing; (II) Psychological research and theory in a substantive domain which is intrinsically connected with what are usually considered "philosophical" concepts or questions; and (III) Intrinsic interest in philosophical problems aside from any relevance they have to psychology.

I. In the Course of Doing Science

When discussing psychological theory, and even when interpreting empirical findings in the discussion section of a research article, a considerable portion of the statements made consist of metalinguistic discourse. Theoretical or experimental articles in psychology contain mainly object language proofs in the formalism, description of apparatus, subjects, and procedure, numerical tables concerning measurements and summary statistics, and theoretical text belonging to the substance of the theory. But there are also statements in the metalanguage concerning what something proves or does not prove, whether a technical term is being employed with such and such meaning, reduction of one set of concepts to another, whether a protocol should be received into the corpus, evidentiary weight, probability of inference, the methodological merits of this or that school of inferential statistics, the appropriateness of a significance level, the consistency of such and such claims, the adequacy of certain conceptual definitions, and so on. Many years ago, in *Logical Syntax of Language* (1934/1937), Rudolf Carnap pointed out how frequently scientific discourse not purportedly methodological or philosophical in subject matter contains a sizable amount of what he called the "formal mode of speech." He writes (p. 286), Accordingly, we distinguish *three kinds of sentences*:

1. *Object-sentences*	2. *Pseudo-object-sentences* = quasi-syntactical sentences	3. *Syntactical-sentences*
	Material mode of speech	*Formal mode of speech*
Examples: "5 is a prime number"; "Babylon was a big town"; "lions are mammals."	Examples: "Five is not a thing, but a number"; "Babylon was treated of in yesterday's lecture." ("Five is a number-word" is an example belonging to the autonomous mode of speech.)	Examples: "'Five' is not a thing-word, but a number-word"; "the word 'Babylon' occurred in yesterday's lecture"; "'A . ~A' is a contradictory sentence.)

See also the striking example (pp. 328-30) from the initial sentences of Einstein's 1905 relativity paper.

Metalinguistic remarks appearing in a purely "scientific" paper are about, or make use of, concepts usually considered to be the province of the philosopher. The "linguistic turn" of philosophy that we connect with the names of Bertrand Russell, the ordinary language philosophers, the logical positivists, and more broadly analytical philosophy in general, usually held that clarification of terms and of the conceptual relations between statements was all that philosophy could consist of. This position is succinctly and dramatically expressed by the early Wittgenstein, *Tractatus*: 4.0031, "All philosophy is critique of language"; 4.112, "The object of philosophy is the clarification of thoughts. Philosophy is not a theory but an activity. A philosophical work consists essentially of elucidations. The result of philosophy is not a number of 'philosophical propositions,' but to make propositions clear"; 4.1121, "psychology is no nearer related to philosophy than is any other natural science."

One may ask: Why, then, does so much more explicitly metatheoretical discourse take place in the social sciences than in physics, chemistry, or genetics? To answer that, one must first have answered the question, What is it to philosophize? I think it generally agreed that there are two characteristics of discourse that would ordinarily be considered philosophical: First, the extent to which it is metalinguistic, that is, not about facts, formulas, experiments, or laws of nature, but about the relations between concepts, definitional or inferential, about matters of proof, validity, fallacy, inference, meaning, and the like; and second—an aspect that those who made the strong linguistic turn preferred to liquidate or at least not think about—some basic concepts that may or may not be metalinguistic, that are ontological or descriptive or refer to aspects of the physical and mental world, but are of a very high order of generality, the kinds of concepts that used to be called *metaphysical,* in no pejorative sense. Aristotle's categories are the obvious example of the latter; but there is quite a list of object language predicates and object language terms for entities, predicates, and relations that are used in all empirical sciences, and questions raised regarding them do not tip us off about what particular science we are discussing. These interesting terms of high ontological generality are one reason to doubt the adequacy of the Ramsey Sentence as a complete rendering of empirical scientific theories. Examples of these terms are: 'accelerate,' 'adjoin,' 'be composed of,' 'evolve,' 'inhibit,' 'interact with,' 'interval,' 'potentiate,' 'produce,' and 'separate.' See Meehl (1990a) for a more complete list and discussion.

If to philosophize means to engage in metatalk about the relation of concepts and statements to one another, or object language talk of this highly general pervasive metaphysical sort, it should not surprise us that more explicitly methodological discourse takes place in the social sciences than in the more developed sciences of physics, chemistry, astronomy, physiology, and genetics. The more trouble you are in, the more you are pushed to question and clarify the most general ideas and the most general rules of the scientific game. So far as I am aware, there are no theoretical disputes in chemistry which are so deep and intractable that chemists holding two different theoretical positions can hardly have a rational conversation with each other. But it is well known that a solid-gold Skinnerian operant behaviorist can hardly have a civilized conversation with an equally orthodox Freudian. Nor can it be said that this is because the behaviorist has a science and the Freudian does not. A conversation between a typical operant behaviorist and a psychometrician engaged in factor analysis of individual differences traits would be almost, although not quite, as difficult to conduct. One has to engage in a certain amount of method talk willy-nilly, whether one chooses to label it "philosophy" or not.

A philosophically sophisticated psychologist is uncomfortably aware of how much shoddy, home-baked, third-rate philosophy, bulging with mistakes, is committed by psychologists who are proud of avoiding philosophical matters and even contemptuous of philosophy of science as a discipline. Mark Twain, speaking of the concept of a "self-made man," said that, in his experience, it normally illustrates the horrors of unskilled labor. I am not suggesting that all psychological scientists must perforce do a large amount of philosophizing. I am only saying there are some kinds of questions that arise in psychological science that are *inherently* epistemological or logical in character, and that cannot be deprived of these intrinsic features by the psychologist disclaiming an interest in philosophical questions. When the unphilosophical psychologist finds himself unavoidably stuck with such questions, it is my experience that what he produces under these conceptual pressures is likely to be rather shabby.

The proportion of metatheoretical discourse varies from one area of psychology to another and depends upon a number of factors which I will not develop here. When a colleague of mine studies the electrophysiology of the retina, very few occasions arise when he is forced to raise questions of a metatheoretical nature. When an equally "scientific" colleague in the field of differential psychology is faced with the task of adopting a solution to the rotation problem in multiple factor analysis, he has to talk about things like scientific realism and the so-called law of parsimony whether he likes it or not, because, for example, the Varimax solution is a quantitative generalization of Thurstone's simple structure, and the rationale of simple structure is not mathematical, *and cannot be made purely mathematical since it is a methodological preference*. When I was a graduate student, we had hot disputes about whether one could reduce the various Freudian defense mechanisms to learning theoretical concepts. It wasn't hard to Skinnerize or Hullianize the mechanisms of repression or displacement; it was a little more difficult to deal with something like reaction formation; and it was extremely difficult to deal with identification. The metaproblem that arose with the latter was, "In incorporating identification into our model of the mind, what weight should be given to the fact that we cannot at this time reduce it to learning theoretical terms?" Similarly, experimental psychologists, or clinicians like myself with a history of rat experimentation and learning theory concerns, tended to view Freud's *Inhibitions, Symptoms, and Anxiety* (1926/1959) as of major importance in his theoretical development, because of the shift from the hydraulic notion of libido being converted into anxiety (my colleague, David Lykken, likes to say, "anxiety was rancified libido") to the notion that the anxiety signal puts the defense mechanism into gear and *it* dams up the libido (i.e., inhibits the impulse expression except as a distorted derivative). This latter theory is much easier to interpret with a suitable mix of classical and operant conditioning than is the transformed libido doctrine. Query just how much weight should be given to ease of conceptual reducibility from the Freudian level of analysis to the learning theory level? That is inherently a "philosophical" question, involving the aims of science, the concept of *theoretical reduction,* optimal strategy at a given stage of scientific growth.

II. "Philosophical" Concepts Intrinsic to a Substantive Domain at the Interface

The branch of scientific psychology that is most likely to require explicit and frequent use of "philosophical" concepts is what has come to be known as cognitive psychology including AI, both the weak and strong form. Empirical research and theorizing about the cognitive processes of perception, reasoning, problem solving, remembering, and formulating experiences, unavoidably have to talk about logical and epistemological matters. Likewise, a philosopher who works in areas such as perception epistemology needs to know something about the substance of scientific psychology. Failure to see this overlap at the interface of the two disciplines makes for bad mistakes. Thus, one may read a philosopher discussing perception and reality and using an example such as a tilted penny appearing elliptical rather than round; in fact a tilted penny appears round to a human observer and can be made to "look elliptical" only by strenuous efforts to undo what the brain automatically does by way of correction for the angle of perspective. It is worth pointing out in this connection that even the currently derided "armchair" or "a priori" epistemology of traditional philosophy was always *empirical*, when the latter term is used in its dictionary sense. Psychologists have an obnoxious tendency to identify the empirical with the experimental, and sometimes with the quantitative, which leads to such absurd statements as "Freud has an interesting theory although it is not empirical." What they mean is that it was based upon listening to patients on the couch rather than setting up experimental manipulations or carrying out statistical analyses. Some may think that Freud was a clumsy, inefficient, or biased "empiricist," but to say that he was not proceeding empirically is careless language. The great philosophers in the Western empirical tradition relied upon ordinary life observations and generalizations that for the most part did not require systematic experimentation in the laboratory or quantitative or statistical treatment. It is a misuse of language to say that Locke, Berkeley, and Hume did not rely on empirical facts in conducting their epistemological investigations. One easy way to see this is via what I call Maxwell's Thunderbolt (to be included with Occam's Razor and Hume's Guillotine as basic principles). While I was still enough of a mixed positivist and Popperian to say that I wanted my epistemology to be a priori and not based on empirical sociology or psychology, Grover Maxwell dealt with that position deftly and permanently, with his question "Well, Paul, tell me what epistemological or methodological principles you can derive from the postulates of Whitehead and Russell's *Principia Mathematica*?" The answer, obviously, is "None."

I am inclined to doubt that cognitive psychology will have very much to offer on the positive side to the philosopher of science engaged in the enterprise of criticism, rational reconstruction, and the formulation of helpful advice or guidelines for the working scientist. My prediction is that the main contribution of cognitive psychology will be negative, e.g., the body of research, already sizable, that testifies to the ubiquity of non-optimal strategies and irrational mental habits among humans, including scientists (Dawes, 1988; Faust, 1984; Hogarth, 1987; Kahneman, Slovic, & Tversky, 1982; Lord, Ross, & Lepper, 1979; Nisbett & Ross, 1980).

There is a little semantic problem about semiotic here, because discourse that we would normally count as metalinguistic (because it treats relations of evidence, proof, inference, subsumption, etc., between object language concepts and statements) will appear in the object language of the cognitive psychologist. Thus a given string of words could be considered either object language or metalanguage, or both; that is a little messy, but there it is. I suppose there is no harm in this so long as everybody is clear about what is happening and why.

Even animal behavior cannot be safely excluded from the subject matter domains that force us to make some use of the philosopher's kit of tools. For example, in attempting to formalize expectancy theory of learning, MacCorquodale and I wanted to do justice to Tolman's intentions, but at the same time to be as "operational" about his behaviorism as possible in delineating the concept of response, and in saying how one can, without hidden imputation of anthropomorphic intentionality to the rat, give an adequate characterization of the behavior flux. We found that discussants at the Dartmouth conference and critics of drafts of our chapter on Tolman in the Dartmouth conference book (MacCorquodale & Meehl, 1954) had no trouble permitting a portion of the behavior flux to be characterized by a conjunction or disjunction of two topographical properties or two manipulandum events; but they were troubled by a conditional "If ..., then ..." The easiest way to diffuse that anxiety was to remind them that the horseshoe of material implication—better called the conditional—is explicitly definable by means of conjunction and negation, or disjunction and negation. Again, in discussing the dependency of operant strength upon reinforcement probability, one wants to be clear in a biological framework about what "teleological" formulations as to the adaptive character of such an adjustment are permissible without anthropomorphizing the rat or dragging in the Great Designer. A final example would be our explaining to the skeptic what we mean by referring to an unconscious motive, so that whether he *believes* or not, at least he will not find it *conceptually objectionable* metatheoretical grounds.

III. Intrinsic Philosophical Interests

A psychologist whose subject matter does not interface with philosophy, and whose theorizing is so close to the facts that almost no metalanguage discourse is involved except to point out that a certain mathematical derivation is or isn't valid (a question we hardly think of as "philosophical"), may still enjoy putting on the philosopher's hat because of an intrinsic interest in philosophical questions. In this respect the psychologist interested in philosophy is not different from a geologist interested in chess, a chemist interested in figure skating, or an astronomer interested in politics. I must emphasize to psychological readers that this third way of wearing the philosopher's hat is often present with me. I have always been careful to explain to students in my philosophical psychology class that I avoid the claim that *all* my philosophical interests have helped me—although I hope they have not hindered me!—in my work as a psychologist. Thus, for example, I don't know any psychologist or, for that matter, any other social, biological, or physical scientist who really worries, *qua* scientist, about Hume's problem of induction, or the related problem of the nature of nomic necessity. All the psychologists I know presuppose—they don't even explicitly premise, they *presuppose*—that there are laws of nature, that these do have some kind of necessity, and that it is important to distinguish causal laws from "mere statistical associations," what the philosopher formulates as the difference between natural laws and accidental universals. When I have my psychologist's hat on and am considering the strengths and weaknesses of a statistical technique such as path analysis for unscrambling the causal order in a complex social network, I don't worry about Hume's problem of induction either. Nevertheless, when I put on the philosopher's hat for this third kind of reason, I find Hume's problem fascinating in its own right.

Thinking Better Under Two Hats

It may be said that the question of what hat you have on is already prejudicial and, perhaps, needless. The linguistic turn doubtless improved the quality of philosophical writing (e.g., I dare say no future philosopher will make the kinds of bloopers that Hegel made in confusing the *is* of predication with the *is* of identity and erecting a whole system upon that undergraduate mistake). But from a post-positivist viewpoint, one might prefer to say there is only one kind of question after all, and that we can ask a meaningful and important question without worrying about what building on the campus we inhabit. Here we find Sir Karl Popper strongly opposed to the position taken by Wittgenstein in the quotes above. Popper writes:

> Language analysts believe that there are no genuine philosophical problems, or that the problems of philosophy, if any, are problems of linguistic usage, or of the meanings of words. I believe that there is at least one philosophical problem in which all thinking men are interested. It is the problem of cosmology: *the problem of understanding the world—including ourselves, and our knowledge, as part of the world*. All science is cosmology, I believe, and for me the interest of philosophy as well as of science lies solely in the contributions which they have made to it. For me, at any rate, both philosophy and science would lose all their attraction if they were to give up that pursuit. Admittedly, understanding the functions of language is an important part of it; but explaining away our problems as merely linguistic "puzzles" is not. (Popper, 1935/59, 15)

The papers reprinted in this collection are samples of all three of the above ways in which a psychologist wears a philosopher's hat. Perhaps a word is appropriate about my general stance as an amateur philosopher. Like most bright people of my generation, I was a logical positivist in my youth, although I was already having doubts about the meaning and value of the verifiability criterion of meaning when the great Vienna positivist Herbert Feigl came to Minnesota in the academic year 1940–41, my senior year at Minnesota. I certainly cannot label myself today as a positivist, logical empiricist, Popperian, Lakatosian, or whatever. My general stance is broadly "analytic philosophy," if that language is not taken to mean a self-imposed injunction against asking interesting questions, whoever's bailiwick they fall into. My thinking as a psychologist—whether I was interpreting experiments on latent learning in the white rat, formulating a theory of schizophrenia that would be testable by appropriate taxometrics, trying to clarify the problem of psychoanalytic evidence in the interview, or trying to understand what we mean when we claim validity for a psychological test—as been greatly helped by the analytic philosophers, many of whom disagree strongly with one another on major issues. I have never, I think, employed philosophy of science as a club, in polemics about substantive psychological issues, a practice common in the heyday of logical empiricism and operationism, as these were abused in some quarters. I think that now the views of Thomas Kuhn are being similarly employed in an illegitimate way. I expect this influence to be far worse than that of the positivists, because it can be employed for obscurantist purposes, which theirs never were.

Nor is my reliance upon concepts and principles of metatheory merely window dressing. Some readers (more likely philosophers, but some of the younger generation of psychologists) may wonder why they find almost no reference to the ordinary language philosophers. There is no point in pussyfooting about that, and in fact I feel a certain obligation to speak out strongly, especially for the benefit of my brethren in the "soft" fields of psychology (clinical, counseling, personality, social, developmental, community psychology). I have not found the writings or lectures of ordinary language philosophers the least bit helpful in my work as a psychologist, and-of less interest, being merely autobiographical—I have found them pretty boring when I put on the philosopher's hat of intrinsic interest in philosophical problems. (The exception is ethics, and perhaps political theory—I am unclear why.) I would be suspicious of myself, given that there is quite a lot of ordinary language philosophy around, except for the fact that I am not identifiable as an adherent of any particular philosopher's metatheory. I have not been, except for a few months as a Popperian, after the English translation of Popper's *Logic of Scientific Discovery* in 1959, and the logical positivism I held as an undergraduate. Relying either on introspection, or the detailed protocols of the early sessions of the Minnesota Center for Philosophy of Science in the 1950s, and most of all upon my publications in psychology, I could easily prove to any fair-minded person that when I am putting on the philosopher's hat as an adjunct to my work as a psychologist, *I treat the philosopher's table as a cafeteria or smorgasbord*—I gladly take anything I can get, whatever its source and however it is labeled, however popular or unpopular it may be within the philosophers' own trade union, if it helps me with what I am doing as a psychologist. Thus I can cite chapter and verse of my writings showing the marked influence of Bergmann, Carnap, Feigl, Hempel, Nagel, Pap, Popper, Reichenbach, and Sellars (older generation of analytical philosophers) as well as Glymour, Grünbaum, Hacking, Kitcher, Kordig, Lakatos, Laudan, Maxwell, and Salmon (younger generation of the same). So I am certain that I do not approach Austin, Hampshire, Malcolm, Moore, Strawson, Toulmin, Wisdom, or the later Wittgenstein with a bias because I am a committed positivist, Popperian, Reichenbachian, or disciple of Lakatos. If the ordinary language philosophers had anything helpful to say to me, I would welcome it; it is simply a fact that they do not. Wittgenstein's *Tractatus* was almost a Bible to the Vienna circle (although they did some eisegesis, beginning with the very first section on atomic sentences!). If I approached the later Wittgenstein with any prejudices, they were positive. Recently, I have been rereading the *Tractatus*, along with Max Black's *Companion*, with pleasure and profit. It seems to me that the early Wittgenstein had some important insights and, perhaps equally important (certainly for anyone with Popperian sympathies), his incorrect "insights" were profound and interesting, so that seeing what makes them incorrect is itself illuminating. It is strange that the ordinary language philosophers have been so little help to me, because I have done theoretical and empirical work in six different areas of psychology which would seem obvious candidates for illumination by the analysis of ordinary language: trait attribution, forensic psychology, psychoanalytic interpretation, the interpretation and validation of psychological tests, the concept of mental illness, and the distinction between dimensions and types in personality and psychopathology. I find it hard to conceive of any branch of science—social, biological, or physical—in which the nature of the problems seems better suited to aid from ordinary language philosophy than these. All I can say is that it just isn't so. *Examples*: In considering the distinction between psychosis and neurosis, which is of both forensic and theoretical importance, one might suppose that the question "What does the ordinary speaker mean if he says somebody is crazy?" would be helpful. Well, perhaps a smidgen, but no more. If one does not move from that level to the crudest clinical refinement by the technically trained psychologist or psychiatrist, the ordinary language gloss is of no value. Similarly, when discussing the concept of general intelligence, one starts by delimiting a broad class of tasks that can be labeled “cognitive,” but from that point on the psychometrics (factor analysis and predictive statistics) take over, powerfully controlling all further interpretive discourse. Even the initial delimitation of what is a “cognitive” task does not rely on ordinary language in any strong way. Suppose somebody objects to the original list of subtests in an omnibus intelligence test on the grounds that it merely represents “middle-class school teachers’ biases” (an erroneous cliché of the frenzied egalitarians). One simply asks those who think something else is cognitive to devise some feasible tests to get at it, and we throw them into the candidate pot. It turns out that if we put endurance of pain or rapidity of speech or color discrimination or balancing on a narrow board into the pot, the subsequent statistical analysis informs us that, whatever these tasks are getting at, they are not getting at *whatever tapped* the ability to solve puzzles, to discern analogies, to understand the meaning of words, to duplicate a complex visual pattern, to remember facts, or to give appropriate commonsensical solutions to everyday occurrences.

There is also a metatheoretical question about the claims of ordinary language philosophy that I have yet to see answered. Why should one presume that ordinary language is adequate to tasks of even moderate conceptual complexity? This has not turned out to be the case with any domain investigated by the sciences (see Maxwell & Feigl, 1961). One defender told me that it ought to be in pretty good shape for matters like human trait description, because all of us, whether psychologists or not, have dealt with quite a few other people and have been practically forced to attribute dispositions to them. I find that no more persuasive than arguing that I should read the Farmer’s Almanac instead of consulting a meteorologist, on the grounds that we have all been exposed to quite a bit of good and bad weather. But this is not the place to mount an assault upon ordinary language philosophy. I suppose I should adopt the stance “let a hundred flowers bloom,” except that if it turns out that they are mostly weeds when grown in the soft areas of psychology, I have a scholarly interest in stamping them out. I have already refereed some silly (Bertrand Russell’s word) papers on schizophrenia which were based on ordinary language, and which would, if published and believed, exert a thoroughly malignant influence on scientific research.

I am not a historian of science, so I do not know how accurate Thomas Kuhn’s historical views are, although it is a matter of common knowledge that many historians of science, perhaps the majority, have not been persuaded. Whatever the validity of Kuhn’s view about history of physics, I am convinced that the application of his precepts to the “soft areas” of psychology is not only unhelpful, it is positively bad. A glance at some of the current literature in the soft fields, and even more, serving as a referee for manuscripts submitted to psychological journals, will convince anybody who is not a devout Kuhnian that the psychologists who are fondest of quoting Kuhn—also misquoting or misattributing—are those who have a stake in obscurantism. I don't notice my colleagues in the really scientific areas of psychology quoting Kuhn. Kuhn-citing almost always comes from psychoanalysts, social psychologists, personality theorists, etc., and it is practically always linked with a (usually uninformed) variant of what Laudan calls "positivist-bashing." (Of course there are exceptions, e.g., Donald T. Campbell, David Faust, James J. Jenkins.) As a clinician who practices psychotherapy and came into the field because of an interest in Freud, but who also knows something about mathematical statistics and genetics, I am aware that one of the most difficult tasks for clinical psychology is to become more scientific. By this I do not prescribe some narrow, hyper-operationalist philosophy of science (I don't hold one). By "scientific" I merely that the initial, basic data and the relation of those data to the theories can command the assent of almost all informed rational minds. I count myself a 40% Freudian (who used to be a 60% Freudian), and I still put some of my patients on the couch and proceed more or less classically. But I do not and never did hold the illusion that psychoanalysis is a science, nor that it is a technology based upon a science. It is now a century since Freud's earliest cases of modifying the cathartic technique into what became classical analysis, and no fair-minded person could say that we now *know* what proportion of his theoretical concepts are substantially correct. This could fairly be described as a scientific scandal. But rather than admitting that it is, and trying to figure out some way to clean it up, the current tendency among some defenders of psychoanalysis is to quote Kuhn and company, which enables them to say that of course one's observations are permeated with theory, so how can you expect a non-Freudian to make the right observations or interpret them properly, and so on and on. I don't even accept Kuhn's thesis that all observations are theory laden; that may hold for nuclear physics and for astrophysics because of their very special character, but certainly does not hold for psychology. I refer the reader to my paper on subjectivity in psychoanalytic inference (essay 11 in this volume).

I cannot resist the impulse to say something in the interest of historical justice in response to the current positivist-bashing. Psychologists grinding an obscurantist ax because they chafe under the burden of proof usually have an acquaintance with logical postivism or logical empiricism largely confined to a superficial reading of its critics. I read psychologists who refer to Hans Reichenbach as a positivist, whereas anyone who has even glanced at his great 1938 book *Experience and Prediction* kows that he consistently used the word 'positivist' to describe his Viennese adversaries! I have even heard references to Karl Popper as a logical positivist, whereas in his autobiography he raises the question "Who killed logical positivism?" and proudly confesses to have been the murderer. What am I to think of psychologists who are so sloppy in their scholarship as to make attributions of this sort? These obscurantists attribute to the logical positivists or logical empiricists views that, so far as I know, none of them held, or only a few of them held for a short period of time, and they take these views as the core features of the movement. *Example*: I have heard psychologists say that according to the logical empiricists, science is simply and solely a kind of automatic buildup from a small set of incorrigible protocols; that this being so, there is no possibility of being mistaken except by some sort of clerical mistake; that scientists routinely proceed by concocting a formal calculus and then setting alongside it a separate text which is its interpretation; that all allowable terms must be directly linked to ("operationally defined by") observable predicates which it requires no special skill or training to reliably discriminate; that the simple straight rule of induction will invariably generate empirical truths; that all statements in mentalistic language can be completely rendered by the description of behavioral dispositions; and the like.

Now, Herbert Feigl was a core member of the Vienna Circle, and in fact it was Feigl and Kaufmann who suggested formalizing the group and having regular weekly meetings. The very phrase "logical positivism," the English language designation of the Vienna Circle, was invented by Feigl and presented in the first paper on the Vienna Circle's views ever published in the English language (Blumberg & Feigl, 1931), five years before Ayer's (1936) influential *Language, Truth and Logic*. I don't know how many hundreds of hours I have spent in Herbert Feigl's company, first as his student and subsequently as a coinstructor in the philosophy department and during the meetings of the Minnesota Center. I can assert that Feigl did not hold any of the views stated above. To the extent that some things some positivists had said sounded close to any of them, he saw them as mistakes which had, fortunately, been rectified.

In his critique of a recent paper of mine, Donald T. Campbell (1990) kindly refers to my paper with MacCorquodale on hypothetical constructs (essay 8, this volume) and my subsequent article on construct validity with Cronbach (Cronbach & Meehl, 1955) as "liberating," as freeing him and other psychologists of his generation with methodological interests from the constraints of logical positivism. Personally, I never felt much "constrained" even when I was a logical positivist as an undergraduate, especially after Feigl came to Minnesota. Between those two articles, there appeared (partly inspired by discussions of my 1948 paper with MacCorquodale) Feigl's (1950) much neglected paper on existential hypotheses.

Most theories in the soft areas of psychology are not supported by the sort of evidence that would suffice to persuade every open-minded, rational, but critical person that anyone of them has high verisimilitude. If any psychologist claims to have such a theory, I would challenge him to show me how he would have been estopped from engaging in his theorizing, or conducting empirical research to test his theory, by the teachings of the logical empiricists as amended, say, from the middle 1930s. I offer that as a serious challenge to the younger generation of psychologists who are positivist-bashers. Cronbach and I were criticized by some hyper-positivistic psychologists for our construct validity position, and they were particularly offended by my strong defense (people say they know which text is Cronbach's and which is Meehl's) of Pap's *open concepts*. I daresay that some psychologists, unfamiliar with the history of the Vienna Circle, would say that a defense of open concepts is strongly against the core ideas of the logical positivists. This shows they are unfamiliar with Carnap's (1936) classic paper on testability and meaning which was explicitly the takeoff point for Pap's (1953) seminal paper and Waismann's (1945) related paper on open texture. My current research in the taxometric approach to schizophrenia genetics stems from *combining the metatheory of open concepts with the Popperian emphasis upon strong tests* (Meehl, 1989, 1990b, 1990c; Meehl & Golden, 1982). To the extent it represents "a loosened tolerant empiricism," it is in the tradition of the later Carnap, Feigl, Reichenbach, and others. To the extent that it aims to tighten up loose concepts and, when that is not immediately feasible, *to objectify the kind and degree of looseness* via the use of new kinds of taxometric statistics, it can be considered neo-Popperian. I am much indebted to the tradition of the positivists, and to the views of Sir Karl Popper, as the metatheoretical framework within which that taxometric research is being carried out. But I repeat, nothing I have found in Kuhn and Co., or the later Wittgenstein and Co., has given me help in those endeavors. One might have supposed, for instance, that Wittgenstein's "family resemblance" would have been useful to someone struggling with the open concept of an entity like schizophrenia, but it was not. I once tried to see where it would lead me, and came to the conclusion that, if I were a better logician, I would be able to prove that taking Wittgenstein's idea of a family resemblance literally, you could prove that everybody had a family resemblance to everybody else. Some symbolic logician should apply himself to that task. It is not clear, reading Paragraphs 65-67 of the *Investigations* how the family resemblance problem of "games" (or "Hapsburgs") is distinguished from the nonproblem that 'game' is also used to characterize rotting meat. Clearly, the mere multivocality of a word is not what he is calling attention to. Since it's not that, and yet—as he correctly argues—not a common property *or strict logical disjunction of properties*, what is it?

I came to know Herbert Feigl's intellect as intimately as I have known anyone's, over a period of thirty years. Because of the Minnesota Center, I was privileged to exchange views orally with other members of the logical empiricist movement and other spinoff analytical philosophers, so that I have spent time in discussion—frequently in strong disagreement—with Carnap, Hempel, Feyerabend, Pap, Popper, Salmon, and others. On the basis of personal contact plus considerable familiarity with their writings, as well as discussion with psychology students who have studied under them (e.g., Gustave Bergmann at Iowa), I am prepared to make a strong statement about the logical empiricists: Whatever was the matter with them as a group or as individuals, *not a single one of them was stupid*. When a positivist-basher expounds the position in a way that implies they were stupid, I have to conclude that the positivist-basher's position is stupid.

But enough of polemics. On the positive side, what do I tell the students in my philosophical psychology class about the value of philosophy of science for them? I tell them that it probably won't help them to concoct clever theories or design ingenious experiments; that its main function other than being fun is critical; but that it may be *prophylactic* against buying a half-baked (and home-baked) philosophy of science that is roughly a half-century out of date in some respects (e.g., simplistic, intolerant "operationalism" as to scientific terms). I also tell them that, while I disapprove of using metatheory as a cudgel to beat up on the substantive opposition in one's field, it is all right to employ it as a *defensive* instrument because there is so much abuse of philosophy of science by those psychologists who do employ it as a cudgel. Thus, for instance, when I first began publishing my views about schizophrenia, its open concept character was the basis of criticism by psychologists who disliked the whole idea of mental illness and had swallowed the Szasz dogma that there is no such thing. These critics said there couldn't be such a thing as schizophrenia inasmuch as nobody had ever given a "strictly operational definition" of it. It was helpful to refer them to Carnap's "Testability and meaning" (1936–37) and the further development of it by Arthur Pap (1953, 1958, chapter 11) and to Hempel's *Fundamentals of Concept Formation in Empirical Science* (1952). As for metatheory in its own right—not merely as a handmaiden of the psychologist who is confronted with a mixed substantive-methodological problem—I am unabashedly old-fashioned in my belief in Reason, that science differs from superstition in being more rational, and that the only important way that metatheory differs from nonphilosophical "history of science" is in its aim of rational reconstruction, which in turn eventuates not in strict rules of "scientific method" but in guidelines, principles, and helpful advice to the theorizing scientist (Meehl 1984, 1990a, 1990d).

I am deeply indebted to my philosopher colleagues Tony Anderson and Keith Gunderson for their initiative in proposing this collection of writings by an avocational (non-union card) philosopher, and for their competence and diligence in seeing it through. Thanks are also due to Philip Kitcher and Wesley Salmon for accepting the onerous task of reviewers and for their enthusiastic recommendations to go ahead with the project.

Paul E. Meehl
Minneapolis, Minnesota
March 6, 1990

References

Ayer, A. J. 1936. *Language, Truth, and Logic*. New York: Oxford University Press.

Ayer. A. J. 1940. *Foundations of Empirical Knowledge*. New York: Macmillan.

Black, M. 1964. *Companion to Wittgenstein's "Tractatus."* Ithaca, N.Y.: Cornell University Press.

Blumberg, A. E., and Feigl, H. 1931. Logical Positivism. *Journal of Philosophy* 28: 281-96.

Campbell, D. T. 1990. The Meehlian Corroboration-Versimilitude Theory of Science. *Psychological Inquiry* 1: 142-47.

Carnap, R. 1936–37. Testability and Meaning. *Philosophy of Science* 3: 420-71; 4: 2-40. Reprinted with corrigenda and additional bibliography, New Haven, Conn.: Yale University Graduate Philosophy Club, 1950. Reprinted in *Readings in the Philosophy of Science*, eds. H. Feigl and M. Broadbeck. New York: Appleton-Century-Crofts, 1953, 47-92.

Carnap, R. 1937. *The Logical Syntax of Language*. London: K. Paul, Trench, Trubner & Co. (Originally published in German in 1934.)

Cronbach, L. J., and Meehl, P. E. 1955. Construct Validity in Psychological Tests. *Psychological Bulletin* 52: 281-302. Reprinted in P. E. Meehl, *Psychodiagnosis: Selected Papers*. Minneapolis: University of Minnesota Press, 1973, 3-31.

Dawes, R. M. 1988. *Rational Choice in an Uncertain World*. Chicago: Harcourt Brace Jovanovich.

Faust, D. 1984. *The Limits of Scientific Reasoning*. Minneapolis: University of Minnesota Press.

Feigl, H. 1950. Existential Hypotheses: Realistic versus Phenomenalistic Interpretations. *Philosophy of Science* 17: 35-62.

Freud, S. 1959. "Inhibitions, Symptoms and Anxiety." In *Standard Edition of the Complete Psychological Works of Sigmund Freud* (Vol. 20), ed. and trans. J. Strachey. London: Hogarth Press, 87-172. (Original work published 1926.)

Hempel, C. G. 1952. *Fundamentals of Concept Formation in Empirical Science*. Chicago: University of Chicago Press.

Hogarth, R. M. 1987. *Judgment and Choice: The Psychologv of Decision*. New York: Wiley.

Kahneman, D., Slovic, P, and Tversky, A. (eds.) 1982. *Judgments under Uncertainty: Heuristics and Biases*. Cambridge, England: Cambridge University Press.

Lord, C. G., Ross, L., and Lepper, M. R. 1979. Biased Assimilation and Attitude Polarization: The Effects of Prior Theories on Subsequently Considered Evidence. *Journal of Personality and Social Psychology* 37: 2098-2109.

MacCorquodale, K., and Meehl, P E. 1954. "Edward C. Tolman." In *Modern Learning Theory*, eds. W. K. Estes, S. Koch, K. MacCorquodale, P. E. Meehl, C. G. Mueller, W. N. Schoenfeld, and W. S. Verplanck. New York: Appleton-Century-Crofts, 177-266.

Maxwell, G., and Feigl, H. (1961). Why Ordinary Language Needs Reforming. *Journal of Philosophy* 58: 488-98.

Meehl, P. E. 1984. "Foreword." In Faust, *The Limits of Scientific Reasoning*, Minneapolis: University of Minnesota Press.

Meehl, P. E. 1989. Schizotaxia Revisited. *Archives of General Psychiatry* 46: 935-44.

Meehl, P. E. 1990a. Corroboration and Verisimilitude: Against Lakatos's "Sheer Leap of Faith." (Working Paper MCPS-90-01.) Minneapolis: University of Minnesota, Center for Philosophy of Science.

Meehl, P. E. 1990b. "Schizotaxia as an Open Concept." In *Studying Persons and Lives*, eds. A. I. Rabin, R. Zucker, R. Emmons, and S. Frank. New York: Springer, 248-303.

Meehl, P. E. 1990c. Toward an Integrated Theory of Schizotaxia, Schizotypy and Schizophrenia. *Journal of Personality Disorders* 4: 1-99.

Meehl, P. E. 1990d. Appraising and Amending Theories: The Strategy of Lakatosian Defense and Two Principles that Warrant Using It. *Psychological Inquiry*, 1: 108-41.

Meehl, P. E., and Golden, R. 1982. "Taxometric Methods." In *Handbook of Research Methods in Clinical Psychology*, eds. P. Kendall and J. Butcher. New York: Wiley, 127-81.

Nisbett, R. E. and Ross, L. 1980. *Human Inference: Strategies and Shortcomings of Human Judgment*. Englewood Cliffs, N.J.: Prentice-Hall.

Pap, A. 1953. Reduction-Sentences and Open Concepts. *Methodos* 5: 3-30.

Pap, A. 1958. *Semantics and Necessary Truth*. New Haven, Conn.: Yale University Press.

Popper, K. R. 1959. *The Logic of Scientific Discovery.* New York: Basic Books. (Original work published 1935.)

Reichenbach, H. 1938. *Experience and Prediction.* Chicago: University of Chicago Press.

Waismann, F. 1945. Verifiability. *Proceedings of the Aristotelian Society* 19 (Supplement): 119-50.

Wittgenstein, L. 1933. *Tractatus Logico-Philosophicus.* New York: Harcourt, Brace.

Wittgenstein, L. 1953. *Philosophical Investigations* (3rd ed.). New York: Macmillan.