

An Introduction to the Work of William W. Rozeboom

The papers reprinted here from the published work of William W. Rozeboom were selected to introduce readers to his notion of “explanatory induction” (EI), a fundamental form of inference that he took to be a major facet of human intelligence, whether in the rudimentary forms it takes in the “common-sense” inferences we make every day without a second thought, or in the more advanced and self-conscious forms it takes in the construction and confirmation of scientific theories. Essentially, EI is an inborn tendency to make inferences from observed patterns of data to the existence of theoretical entities (states, qualities, dispositions, abilities, and so on) that would explain *why* the data are patterned in this way. This is how we infer attitudes from behavior, for example, and how a doctor diagnoses illnesses from symptoms. Rozeboom (WR) assumed that EI was a gift of natural selection, much of it an unconscious feature of everyday life that is, as he put it, no more in need of justification than our breathing. But he also found it to be poorly understood and underdeveloped in the philosophy of science, and sometimes openly discouraged in the special sciences.

Whereas reasoning akin to EI is taken for granted in physics and biology, in psychology and the human sciences it has frequently met with concerted resistance. This is partly due to inherent difficulties with the “common-sense mentalism” we use to explain people’s behavior by attributing beliefs, attitudes, moods, and other mental states to them (a prime example of EI), and partly because of the dramatic rise to prominence of statistical inference in the latter half of the twentieth century. In addition, this was a period in which the philosophy of science turned its back on the many forms of inductive inference used by competent researchers in the professions and special science, preferring instead to discuss scientific inference in a broad historical setting only, and within the strict confines of deductive logic. For in the philosophic model of science that has prevailed until recently, no method can be detected in the way scientists come by their theories, and even if it could, it would in any case be epistemically irrelevant, disclosing perhaps some personal details about the “discovery” of the theory, but nothing at all about its “justification,” a subsequent phase of research in which observable implications of the theory are deduced and then verified or falsified—the so-called “hypothetico-deductive” account of scientific inference. WR disagreed. In his view science advances primarily in a cumulative fashion through inferences that are made directly from patterns of data to their underlying causes, a process in which discovery and justification are largely the same—although by no means free of error.

But if the philosophy of science may have been partly responsible for the lack of workable models of inductive inference for practicing scientists, the human sciences had themselves taken a decision that added greatly to the problem. Just when they were emerging from the Logical Positivism that had dominated early 20th-century philosophy, they opted for a seemingly robust model of inference, namely statistical hypothesis testing, a decision that disastrously constrained their scope for rational inference of the common-sense variety. For in WR's broad account of non-deductive inference it soon becomes clear that statistical induction has only a prefatory role to play in the development of theories, and attempting to make it into a general model of professional inference in any given domain of research must lead inevitably to an impoverishment of its conceptual structure and technical vocabulary. The core of a scientific theory, he argued, lies in its concepts, in other words, in the way it conceives of the *kinds* of things it studies, and this is built up mostly by inductive analysis of hypotheses, not by decisions to accept or reject them.

WR's work is radically interdisciplinary, bringing together the disciplines of philosophy and psychology in a way not previously seen. As an accomplished philosopher of science he provides the first formal description of the logical and epistemic structure of EI, while as a life-long practitioner of empirical psychology he protests the discipline's subservience to null-hypothesis testing and the resulting damage to its natural development as a science. All of the most heavily-worked concepts of empirical psychology come under his scrutiny sooner or later, those that define its content (stimulus, response, behavior, habit, trait, disposition, perception, memory, belief, attitude, meaning, and intentionality) and those that define its method (datum, variable, scale, observation, inference, law, theory, metatheory, evidence, and confirmation). Yet in the process of his analysis he also finds contemporary philosophy of science poorly equipped for the task, principally because of its lack of familiarity with the research methods of the sciences, but also on account of its obsession with deductive logic and the history of science—the primary sources of its own resistance to EI. WR retained this dual perspective, philosophical and psychological, in all of his work, and papers of both sorts are included here, ordered only by the dates of their first publication.

Although WR was critical of both disciplines, his work can scarcely be described as controversial. Mostly it was met with silence—if it was read at all. The papers are sometimes difficult, and they can leave the reader with uncomfortable choices between basic principles of correct reasoning and accepted practices in the conduct and publication of research. Worse still, they are split between disciplines, making it easy for psychologists to dismiss them as “philosophical stuff,” and vice versa for the philosophers. But times are changing. Bayesian and personalist approaches to probability continue to gain ground alongside exploratory and graphic methods of data analysis. There is also a steady increase in the number of papers and

books on scientific method written by philosophers who have a good knowledge of quantitative methods, and more generally, a greater willingness in the philosophy of science to examine the inductive methods of the special sciences. The time seems right, therefore, for a volume of WR's papers, philosophical *and* psychological, on the nature of EI and its implications for the conduct of empirical research.

William Warren Rozeboom was born of Dutch stock in Ottumwa, Iowa, on the 29th of February, 1928 and completed his Ph.D. in psychology at the University of Chicago in 1956 with a thesis under Howard Hunt on latent learning in animal behavior. This was followed by an enormously rewarding two-year postdoctoral National Science Foundation fellowship at the Minnesota Center for Philosophy of Science then headed by Herbert Feigl, and teaching positions at St. Olaf College (1958-1961) and Wesleyan University (1961-1964). He moved to Canada in 1964, where he was Professor in the University of Alberta's Psychology Department and Center for Advanced Study in Theoretical Psychology until his retirement in 1993. A full list of his publications is contained in Appendix A, p. 431.

The remainder of this introduction gives an overview of WR's work, based principally on the papers included in this volume. It is divided into seven sections.

Explanatory Induction

Confirmation Theory

The Subject Matter of Psychology

Language and Cognition

Philosophy and Psychology

Psychometrics

Science and Common Sense

Some suggestions on reading the papers follow.

Explanatory Induction

The concept of explanatory induction (EI) appears in WR's work in the early 1960s and has been his most enduring preoccupation ever since. It is also the principal source of his critique of contemporary psychology and philosophy of science. The term "induction" in "EI" refers, as usual, to the various forms of non-deductive inference that are commonplace in science and everyday life. Personality traits, such as intelligence or self-consciousness, are inferred from behavior in this way, and so are the theoretical states attributed to the material and social worlds. EI

occurs whenever a pattern of data urges us to believe in the existence of conditions, generally unobservable, that would account for it. Initially, WR referred to EI as “ontological” induction in order to distinguish it from statistical induction, a related but more limited variety of induction that permits generalization from samples to populations without, however, positing any new entities. He took heart from the success of mathematicians in describing the logic of statistical induction, which he considered to be “one of man’s truly remarkable achievements” (1972a, p. 98 263).¹ But he was struck too by its severe limitations when compared with EI. Throughout his career, therefore, he sought to rectify the situation by proffering formal descriptions of the most common forms of non-statistical induction and demonstrating their ubiquity in empirical research.

In particular, he sought to uncover the fundamental role played by EI in the construction of scientific theories, and in doing so set himself at odds not only with established practice in psychology but more generally with the hypothetico-deductive view of scientific inference that has prevailed in recent philosophy of science. According to the hypothetico-deductive paradigm, science proceeds principally through the formulation and testing of hypotheses. WR’s objection to this was simply that it ignores the central role played by routine processing in the daily work of competent practitioners in the special sciences and professions.

The fact of the matter is, however, that the theoretical structure of a science grows for the most part by small, piecemeal accretions along the edge of what has already been firmly established, like ice freezing on a pond, in which local segments can be modified or scrapped with little threat to the remaining growth, and where the more spectacular unifications which may appear in the mature science flesh out and tie together theoretical notions which have already taken form in lower levels of theory . . . The conclusion is unmistakable that far from being spontaneous, untrammelled inspirations, the workaday theories around which the no-nonsense research scientist organizes his professional activities are actually pre-formed to a high degree by the corpus of data already accumulated. (1961b, p. 338 29).

And so, in “Ontological Induction and the Logical Typology of Scientific Variables” (1961b) and “Scaling Theory and the Nature of Measurement” (1966b) he provides the first formal description of “the otherwise mystifying emergence of theoretical concepts from routine data-processing” (1961b, p. 359 48).

Fundamentally, EI manifests itself in a tendency to attribute group properties to individuals. For example, it frequently happens that a group mean invites

¹Italic page numbers refer to pages of this volume. References that do not contain the author’s name are to works of WR.

a secondary interpretation as evidence for a theoretical property that may be attributed to each member of the group. True, the invitation is typically weak and easily resisted, yet it grows stronger if, for example, the data are rich enough to show inter-group differences, suggesting a certain “cohesiveness” within groups, and stronger still when we move from univariate to multivariate data, such as correlations, and from monadic to polyadic data (recorded in forms such as “*s* prefers *a* to *b*”) that record relationships. For observed relationships are often most readily explained by supposing that their arguments to be located in a theoretical space in which their distances from each other determine the patterns that are to be observed in the data.

Formal descriptions of the logic of EI are provided in the papers mentioned, with a focus on two of its best-known varieties, “factorial decomposition” and “parameter conversion,” illustrated by familiar examples from psychology, physics, and genetics. Further developments are found in “The Art of Metascience” (1970), “Good Science is Abductive, not Hypothetico-Deductive” (1997), “Meehl on Meta-theory” (2005), and “The Problematic Importance of Hypotheses” (2008), while a convenient introduction and summary is provided in “Scientific Inference: The Myth and the Reality” (1972a). In the paper on abduction (a term used by Charles Sanders Peirce to refer to a process closely related to EI) WR gives examples of EI from early studies in chemistry and electricity, where the implausibility of the hypothetico-deductive account is even clearer.

Early chemists did not learn about acids and alkalis by first speculating that such theoretical properties might exist, next deducing observable consequences of this hypothesis, and finally confirming those predictions as a triumph of hypothetico-deductive science. Rather, the alkali/salt/acid notions and their eventual refinement into a continuum of *pH* levels were an explanatory induction from observed patterns of reaction such as collated by Boyle. Or so I submit. (1997, p. 375–386)

WR was not alone in the early 60s in arguing that the beginnings of theoretical explanation are already to be found in raw data. The statistician John Tukey (1962) also called for a change of emphasis from “statistics” in the traditional sense (i.e. significance testing) to “data analysis” in a broader sense, in which graphic display, data transforms, and other varieties of “detective work” were used to bring to light patterns that are present in the data but not always immediately evident. In psychology, too, there was a shift from univariate to multivariate analysis, including factor analysis. I well recall the excitement generated by the work of the mathematical psychologist Clyde Coombs (1964) and the implementation of some of his multidimensional scaling methods in the Bell Laboratory programs based on the algorithm of Kruskal (1964). Here was a way of taking “pure” psychological data, recording preferences, judgments of similarity between things,

ability to solve a problem, and other “acts of the mind,” and discovering that the data they produce will often “unfold” into a very simple underlying space of one or two entirely theoretical dimensions on which the co-ordinates of subjects and stimuli jointly reproduce the raw data to a high degree of accuracy and may therefore be considered to “explain” them in that sense of the term.

Since the principal products of EI (the “factors,” “dimensions,” or “latent variables” in the examples above) are new theoretical concepts, WR held that its repression in psychology had greatly restricted the discipline’s conceptual development. The thrust of his “inductivist” project is always sub-propositional, towards the concepts contained in a hypothesis more than the surrounding hypothesis, let alone the grand theory. “Astute evidence appraisal focuses on select features of the hypothesis at issue with only secondary confidence adjustments, if any, in its remainder. Holistic acceptance/rejection is for amateurs” (2008, p. 99 426). The first question, therefore, is always the same: what kind of thing must this *be*, that it should appear to us in this way? Overwhelmingly, however, the major analytic techniques available to psychology students are not merely indifferent to conceptual development, they often implicitly discourage it.

Once a metricized criterion is available, Analysis of Variance places no demands on the researcher to increase the acuity with which he conceives his variables. Everyday distinctions will do, and no matter if it is not clear what to do with borderline cases, or what the full range of predictor alternatives may be, or whether our intuitive classifications under a given heading may not in fact be inconsistent or cut across and perhaps confound several importantly distinct dimensions of differentiation—we need simply make sure that the predictor categories included in the study are restricted to paradigm cases about which we feel minimal uncertainty, while anomalies which might otherwise shake our confidence in the whole conceptual scheme can then be shrugged off as lying outside the scope of the design. (1966a, p. 551)

In “Scaling Theory and the Nature of Measurement” (1966b) WR looks at the most basic of all concepts throughout the natural sciences, that of a “variable,” and shows that the operations by which variables are quantified are not epistemically prior to inference but are often themselves forms of EI. Even the physicist’s notion of “fundamental measurement,” sometimes illustrated in the philosophical literature by the procedure for weighing things (Campbell, 1920, Ch. 10), turns out to be a form of EI. Specifically, it is a factorial decomposition of dyadic data, namely the ordering of pairs of items determined by placing them on the pans of a balance, from which the property of weight is inferred as a latent variable. Moreover, it is the empirical comparisons (i.e. perceived degrees of similarity or difference) among the attributes being scaled that give the scaled values whatever

meaningfulness they may have as descriptions of the natural world. “Insomuch as ‘scaling’ is simply a methodology for expeditious processing of natural variables, our primary interest in formal scales lies in what we can infer from the properties of scaled data about the natural features of these data” (1966b, p. 181 *137*).

This is in stark contrast with the “theory of scale types” that is still imposed on students throughout the human sciences. Briefly, the theory warns students that they may not upgrade from ordinal scales to interval, or from interval to ratio, a prohibition famously described by WR as “complete nonsense” (1966b, p. 188 *142*). For in spite of the sophisticated application of the mathematics of invariance associated with this typology of scales, it is EI alone that encourages us to prefer one scale to another in an empirical setting. It is not because of mathematical properties of scales that we decline to measure intelligence on a ratio scale (with a meaningful zero) but because EI hasn’t yet enabled us to *conceive* of intelligence as something that would sustain such a scaling, permitting us to say, for example, that one person is twice as intelligent as another. “In short, the alpha and omega of scaling is significant content. If we have it, then we can formally process it in whatever fashion suits our whim. And if we do not, no incantations about scale types and admissible transformations will provide it for us” (1966b, p. 197 *149*).

Confirmation Theory

WR’s approach to the topic of confirmation is essentially Bayesian, and his notion of confirmation most succinctly expressed by the Confirmation Ratio (CR)

$$\text{CR}(p, q | k) =_{\text{def}} \frac{\text{Pr}(p | q \cdot k)}{\text{Pr}(p | k)}.$$

This ratio gives the extent to which proposition p confirms q relative to background knowledge k from all relevant sources, and Pr is a degree of rational probability. For $\text{CR} > 1$, therefore, p confirms q (at least to some degree); for $\text{CR} < 1$ it disconfirms it; and for $\text{CR} = 1$ it neither confirms nor disconfirms it (1971, p. 343 *234*). Despite the problems that still remain concerning the quantification of prior probabilities, WR still considered the Bayesian theory of conditional credibility, based on the same axioms as statistical probability, to be our most instructive model of rational belief change.

It is notable that whereas the term k (for prior knowledge) is often omitted from simplified Bayesian equations, WR is at pains to retain it. This is to ensure that the key role that it plays in rational belief is not overlooked. As he points out in “Nicod’s Criterion: Subtler than You Think” (1980), with the sole exception of logical entailments “one proposition does not confirm/disconfirm another *simpliciter* but does so only relative to a body of antecedent information,”

adding tersely that “any reader who honestly disagrees on this point is excused from further participation in this exercise” (p. 639 371). The point is well made, since he then goes on to show that indecision about k pervades the philosophical literature on confirmation theory, so much so that “there is little more to say about Hempel’s Paradox² in its original formulation except to bury it with the epitaph that its vast literature rests upon confusion” (p. 641 374).

He does not ignore the paradox, however, or dismiss it as an error as the Bayesian statisticians are prone to do. Instead, he looks for its origins, which he traces to aspects of common-sense confirmation theory that are not captured by logical entailment and therefore call for a less restrictive form of analysis. Common sense insists (incorrectly) that the colour of non-crows is irrelevant to the claim that all crows are black. But that is not the end of the matter for WR. “Until it is normatively clear what propositions should be confirmationally relevant/irrelevant to one another under what background information, we can ill afford as philosophers and methodologists of science to disrespect our practical intuitions about this” (1971, p. 367 255). And so, in “New Dimensions of Confirmation Theory” (1968) and “New Dimensions of Confirmation Theory II: The Structure of Uncertainty” (1971), the second of which is reprinted here (p. 233), he attempts to reconcile our intuitive notion of confirmation with its standard logical reconstructions. His conclusion is that common-sense confirmation theory can be defended only if “ p implies q ” is replaced by “ p brings q ,” in some sense of “brings” that implies a causal connection, in the way that ravenhood, in Hempel’s paradox, “brings” blackness, while blackness does not “bring” ravenhood. He finds many attractive features in the broader perspective, in particular its potential to unite the philosophy of causation with the theory of inference, and both of them with the emerging statistical theory of confirmational relatedness (1971, p. 367ff 255ff).

At a practical level, WR’s theory of confirmation has obvious and far-reaching consequences for the daily conduct of research, some of which it shares with Bayesian approaches generally, others that come from his own conception of EI. Given the presence of k in the Confirmation Ratio, theories must of course be fully “thought through” in light of what we know already. But in WR’s analysis this is not merely a matter of extracting falsifiable predictions or establishing meaningful prior probabilities—the latter virtually an impossible task, in his opinion. The “thinking through” that he requires takes us deeper into what the theory *says*, and how its conceptual elements are confirmationally related to each other and to the possible outcomes of the study. This, unfortunately, is rarely done. Yet without it, significance tests are pointless.

You have nothing to gain from concern for a statistic’s sampling uncertainty (save to oblige colleagues who want it) if you have little idea

²See below, pp. 237–240, 371–375.

what to do with its population value were you to know that . . . If you don't know how interpretation of your chosen statistics could profit from learning their precise population values, shouldn't you be trying to develop a conception of your inquiry's target that does? (1997, p. 385 395)

This was also the major theme of WR's best-known paper, "The Fallacy of the Null Hypothesis Significance Test" (1960b). Since the paper has been reprinted many times (Appendix A, p. 431) it is not included here. For WR, null hypothesis testing was merely another absurd by-product of hypothetico-deductivism. Essentially, it is an attempt to short-circuit the natural course of inductive inference in empirical research by introducing a decision-making routine that he describes elsewhere as "surely the most bone-headedly misguided procedure ever institutionalized in the rote training of science students" (1997, p. 335). Although the paper attracted considerable attention, subsequent discussion failed to challenge the hypothetico-deductive paradigm that underlies significance tests, or to address WR's main point, namely that science does not ask us to make decisions of any kind about our theories when new data become available, but only to adjust our estimates of the parameters they contain—provided they yield a plausible fit to the data.

Another product of the decision-making mentality, the prohibition on post-hoc hypotheses, is addressed in one of WR's most recent papers, "The Problematic Importance of Hypotheses" (2008). Many text-books still tell students that a hypothesis formulated *after* they look at their data is not confirmed to the same degree as it would have been, had they been sufficiently prescient to formulate it in advance. Yet a little reflection shows that the order in which a hypothesis h and possible data d pass through the mind of a researcher is entirely irrelevant to the increment of credibility that h would receive from d , were it found to be so. In any case, the role of unexpected findings is already well established in the history of science: "Post-hoc data interpretation is a glory of empirical science, not a sin. Don't shun interpretation of unpredicted data patterns; revel in the opportunity." (2008, p. 1124 427). He explores the prohibition at length, and being unable to find any rational argument for it can only suggest that it is yet another shadow cast over the broader domain of EI by preoccupation with null-hypothesis statistical testing, originating perhaps in the presumption that the probability distributions for a chosen statistic must be fixed before the "significance" of its observed values can be determined.

In "The Art of Metascience" (1970), "Meehl on Metatheory" (2005), and "The Problematic Importance of Hypotheses," (2008)—particularly in the latter's Appendix A: "A Tested Hypothesis's Haze of Variations"—WR deals with the internal confirmational structure of theories, that is to say, the *differential* enhancements of credibility that take place among their propositional components, explicit or

implied, when any one of them is confirmed. Here too, the hypothetico-deductive paradigm has little to say apart from stressing the need for theories to be “simple.” But without further clarification, this implies, by default, that every proposition in a theory is confirmed, at least to some degree, when one of these is confirmed.

Although few would care to defend such a blunt position, and WR has shown that it is easily disproved (see the Appendix to 2008, p. 1122 *427*), he argues that its influence is still everywhere to be seen in psychology.

I contend that most psychological theories do, in fact, suffer badly from this intellectual malady, and that those portions of theories which carry the color, excitement and challenge of a particular psychological perspective seldom have much overlap with those portions which are sustained by evidential support. (1970, p. 99 *227*)

To address the issue, he provides a rough calibration of the centrality of hypotheses within a theory by placing them in four classes: (1) propositions already confirmed through induction, statistical or explanatory, (2) propositions inductively implied by the former, (3) speculations that are in principle confirmable within psychology, and (4) speculations that are confirmable only in disciplines outside of psychology—if at all. On this reading, the core of a theory does *not* reside in its most abstract and comprehensive claims but rather in those that have achieved the highest degree of inductive confirmation.

In neither the behavioral sciences nor modern philosophy of science has it been adequately appreciated how our most firmly established theoretical concepts are sustained not by deliberated hypothetico-deductive reasoning but through natural abductive inferences from the characters of data patterns to concepts of entities (mainly attributes) taken to be their explanatory sources. (2008, p. 1123 *426*)

Conversely, while Class (3) speculations may eventually receive some evidential support and thus become a legitimate part of a scientific theory, speculations in Class (4), in spite of their prominence in “broad perspective” and interdisciplinary approaches to the subject, have no place in the theory since “it is intellectually fraudulent for a theory to include detachable premises which make a difference only for some extraneous area in which the theory’s proponents undertake no serious research responsibilities” (1970, p. 101 *230*).

There is a further discussion of the internal confirmational structure of theories in “The Art of Metascience” where he compares his own inductivist position with the only two alternatives, hypothetico-deductivism and the more recent “omnithoretical” approach as he terms it (1970, p. 66 *191*) that is found in the work of Kuhn, Hanson, Feyerabend, and others who reject the distinction between datum and theory assumed in both the inductivist and the hypothetico-deductive

approaches. Here he presents a detailed account of the logical and epistemic difficulties with “hypothetico-deductive holism,” i.e. the view that all the components of a theory receive some degree of confirmation when any one of them is confirmed. Regarding the omnitheoretic position, on the other hand, he has little to say since he considers it is not possible to submit a theory to *any* kind of rational evaluation if it no longer sustains a clear distinction between data and hypotheses. The undoubted appeal of the omnitheoretic position, he suggests, lies in its “bold rejection of past orthodoxies”—including the strictures of hypothetico-deductivism that he too would like to be consigned to the past, albeit for different reasons.

The Subject Matter of Psychology

Given the limited resources available to academic disciplines, WR’s calibration of the centrality of hypotheses within a theory, based on the degree to which it has already been inductively confirmed, raises the issue of a scientific discipline’s core content. In the case of psychology, he argues that this is defined jointly by “the mysteries of the behaving organism” (1960a, p. 159 *1*) and plain common sense, each of which sets limits to the subject’s scope and methods.

On the methodological side, the “behaviorism” endorsed by WR’s reference to “the behaving organism” requires that all areas of research in psychology, including cognition and other mental phenomena, must be based on behavioral data in the end, however valuable introspection may be in the construction of theories. This is merely to comply with the normal standards of objectivity that hold in science generally: “For psychology, the technically efficacious data base is behavioral, i.e. the organism’s outside-of-the-skin doings and history of environmental circumstances” (1972, p. 102 *342*). But in addition to the methodological role it plays in providing *all* psychological theories with their data, the concept of “behavior” also delineates an important research domain. In ordinary usage, “behavior” excludes reflexes and accidental actions such as tripping over things, yet it is difficult to say what makes behavior “smooth”, or “object-oriented,” or “intentional” in a way that excludes the former. Undoubtedly hierarchical structures are involved, and WR thought that progress in this area was made with the rise of cybernetics, most notably with the publication of “Plans and the Structure of Behavior” (Miller, Galanter, & Pribram, 1960). But he also noted a number of foundational issues that are still to be resolved before hierarchical structures (including “plans”) are capable of supporting a theory of directed behavior (1970, pp. 152-156).

WR’s attempts to bring psychology closer to common sense also has two aspects. The first is methodological and consists of his efforts to promote EI as the discipline’s main mode of inference, while the second in substantive and lies in his conviction that common-sense mentalism, as parsed by philosophic accounts of mental acts, is still the best first draft of psychology’s subject matter. He takes “mind,” the “psyche” in “psychology” or the “inner” organism, to be the disci-

pline's core content, namely, a complex of theoretical states in close interaction with each other and with the environment, past and present:

The concepts through which the organism's interior is to be technically described will be primarily of the sort which philosophers of science nowadays call "theoretical" or "dispositional," namely, hypothetical constructs which refer to the unobserved entities responsible for the data patterns on which the theory rests yet which characterize these underlying entities only functionally in terms of their nomic relations to the data variables. (1972b, p. 103 342)

He attached little importance to the distinction between mentalistic and behavioristic psychology. In an inductivist approach based on behavioral data, the evidential requirements for both varieties of theory are largely the same, and since the relationships established between observed behavior and its hypothesized sources within the organism are purely functional, the issue of whether the source attributes so conjectured are mentalistic (intentional) or non-mentalistic (physical) is of little importance. "Mental vs. physical is arguably a distinction more of semantics than of ontology, and some version of pan-psychism remains a serious possibility" (2008, p. 111n 411n). Yet none of this threatens the identity of psychology as a science as long as its theoretical constructs are functionally linked to observed behavior.

It is quite a different matter, however, when hypothesized states and processes of the *brain* are proposed as explanations of behavior. This cannot be done, he argues, without switching the topic of inquiry from behavior to neurophysiology. If our interest is solely in behavior, all we need are functional descriptions of theoretical dispositions that explain the behavioral phenomena under study. If we have that, we need nothing else: "The theory's further premise that the underlying mechanism which manifests these functional properties also has such-and-such a physiological constitution has import only for certain types of physiological observation which are of no concern for psychology as such" (1970, p. 100 229). WR's objection to physiological explanations of behavior, therefore, is not the usual one, namely that they are still in their infancy, but simply that they are not relevant. He adds only two qualifications. In connection with the topics of learning and memory he notes that the theories of neuropsychologists (Eccles, Lashley and Hebb, for example) are not always exclusively concerned with brain mechanisms but may also include "theories about the accumulated effects of environment upon living systems in general" (1965a, p. 331 71), a concern that brings them back into the domain of psychology. And secondly, he has nothing against psychology's distinguished tradition of interdisciplinarity, noting that "it is perfectly legitimate for psychologists to have physiological curiosities as well" (1970, p. 100 229). None of this, however, requires us to revise our views on the subject matter of psychology: it can only be the "inner organism," the *psyche* referred to in the subject's name.

WR does not share the common view that the human mind is one of science's "last frontiers." He believed that psychology, in its attempts to describe the "inner organism," faces no more problems than biology does, and that neither psychology nor biology is any different from physics in this regard. *All* science, as it proceeds to the study of the "insides" of things, animate or inanimate, is faced with increasing difficulty as it posits theoretical states that are further and further removed from direct observation (1972b, p. 102 342).

Language and Cognition

WR's interest in cognitive processes was not restricted to scientific inference. Indeed his work on that topic is best viewed as a special application of his conception of mental states generally, covering not only inference but also memory, perception, language, meaning, concept formation, decision making, and rationality. This is an approach based on the triad Content-Object-Mode of cognitive facets. "Content" is WR's preferred term for ideas, concepts, propositions, representations, symbols, and whatever else it is that gives "aboutness" to cognitive acts, that is, their capacity to refer to things. The "object" of a mental act is what (if anything) it refers to. And "mode" (short for "mode of intentionality") denotes the direction of deployment of a mental act, whether it is an act of believing, supposing, fearing, hoping, enjoying, etc.—a facet of mentation that philosophers of mind have labeled "propositional attitude." Of these three, it is a mental act's mode that determines the form it takes and its behavioral consequences.

Concerning the meanings of words and sentences WR argues, firstly, that the study of isolated word meanings has little to offer the psychology of language since sentence meanings are primary. This follows from the position he set out in "Do stimuli elicit behavior?" (1960a, p. 1) that behavior is a response to facts or events, *not* stimuli—a point that he develops with respect to language behavior in "The behavioral analysis of semantics," a concluding section to the article that is published here for the first time (pp. 13–16). He then suggests, contrary to virtually everything that had been said on the topic, that the celebrated distinction between *signs* and *symbols* does not lie in properties of the eliciting complex, such as the "naturalness" that is sometimes claimed for signs, in contrast with the arbitrariness and manipulability of symbols, or even in the kind of content that a symbol arouses; instead, it resides in the way that one of its modal facets is determined, namely belief-strength. For the difference between, say, *seeing* and *being told* that it is raining is that when we see that it is raining the same stimuli that arouse content also cause it to be presumed true, due to past experience with the consequences of rain-perceiving. The linguistic content, on the other hand, is aroused by the words uttered, and whether or not it is taken to be true depends on something entirely separate, namely the context of utterance, including the various indicators that the utterance is a bona fide attempt to pass on information

rather than, for example, a practical joke.

This, I propose, is the technical reality behind the often-voiced intuition that language frees our thinking from the here-and-now, or that signs are symptoms of events while symbols represent them. It is not that propositions with distant reference cannot be thought without words, but that when un verbalized they are likely to be evoked only by stimuli which also control the degree to which they are believed. Language is what makes contemplative thought a practical possibility. (1972b, p. 46 301)

As to content itself, namely the aspect of mental acts that allows them to *refer* to things or to *represent* or be *about* them, WR's spare triadic account offers few suggestions as to what kind of state or process it might be. It is simply too difficult to say what kinds of internal conditions are most likely to achieve aboutness. He distances himself at once from the dominant philosophical tradition of "phenomenalism" that tries to ground content in sensory images and the appearance of things, the so-called "sense data" of the philosophical literature. For him, this rests on a confusion of content and object, or at any rate a tendency to blur the distinction between them. If the two are kept separate, as they must be on both logical and ontological grounds, then "we have no reason to think that afferent events are any more intrinsically *of* something else than are *inter alia* affective and motor processes. For a semantic theory to suppose otherwise without argument is to signal that it has simply begged the question of aboutness" (1972b, p. 64 319).

Neither are there any good grounds for supposing mental content to be language-like, while the attempts to equate it with patterns of language use—whether in radical behaviorism, late Wittgenstein, or Quine—are little more than refusals to engage with the problem.

To ask for the "use," "rules of employment," or "role" of an expression is to grope for what ordinary language lacks resources to conceive and only psychological science will someday provide, namely, a functionally detailed account of the events which transpire during a person's interactions with cognitively meaningful stimuli" (1972, p. 54 309).

Of even less assistance is the philosophical tradition that WR refers to as "classic semantics," dating back to Frege, in which the imprecision of real-life meanings is disowned "by the imperialist expedient of treating our *de facto* concepts as flawed, subcognitive approximations to the Platonic perfections which alone are the business of philosophy" (1972b, p. 74 328). In "Studies in the Empiricist Theory of Scientific Meaning" (1960c) he shows that Empirical Realism, which has always assumed that a descriptive term cannot simultaneously have more than one

referent, must now revise this assumption or else abandon its entire epistemology (p. 365 25). This is by far the most serious challenge that WR has thrown down to philosophical semantics. It has never been taken up.

Philosophy and Psychology

WR's work draws its most distinctive feature from his dual identity, professional philosopher and professional psychologist. Philosophy was the stronger suit, but he was also a dedicated psychologist throughout his career and in fact his papers appear in roughly equal numbers in the mainstream philosophy and psychology journals.³ It is notable too that he rarely published in the interdisciplinary journals, for although he believed that the current separation of philosophy and psychology was unnatural and mutually destructive, he was not proposing a third forum. The philosophical skill he found most wanting in psychology was a capacity to analyze the subtleties of the discipline's core concepts and research methods, while the enhancement he wished for philosophy was merely a greater allocation of its traditional resources—epistemology, logic, and conceptual analysis in particular—to the study of induction and the semantics of theoretical constructs. Thus the kind of interdisciplinarity he wished to see between the two disciplines would require only a modest shift of emphasis *within* each of them, closer attention to the topic of induction in the case of philosophy, and greater use of conceptual analysis in psychology.

He encountered resistance from both sides. Psychology, it is true, continues its tradition of interdisciplinarity and openness with regard to new perspectives and new areas of research, nor can it be said that psychologists are reluctant to discuss philosophical issues or matters of methodology. But if they do, they keep such discussions at arm's length from their own work.

On the one hand, we find a great deal of enthusiastic, if unskilled, discussion *about* methodology, particularly about general issues having little immediate implication for psychological research. On the other hand, any attempt to *do* methodology in the course of an actual research problem is likely to meet with indifference, incomprehension, and at times open hostility. (1961a, p. 473)

And when it comes to formal analysis of the assertions that psychologists make when describing their data and theories, they were, as he said, about as interested “as a professional quarterback would be in a ballet dancer's assessment of his broken field running” (1961a, p. 475).

Similarly, he found it odd that philosophers of science were quite content to continue writing for non-scientists, showing little concern for the details of scientific

³See the list of WR's publications in Appendix A, p. 431.

praxis, and indeed often dismissing the topic as irrelevant. Instead, they preferred to discuss the history of science and the “revolutions,” “paradigm shifts”, and other dramatic changes that are alleged to be discernible in this broader perspective. WR thought this was a lost opportunity and a further unnecessary widening of the gulf between philosophy and the special sciences. The methods used by scientists *can* be described, he wrote, and if philosophers would only direct their attention to them, then they could write not just *about* science but *for* it (1961b, p. 376 68)—in the sense that is exemplified by his own work for the discipline of empirical psychology.

He also rejected the attempts to separate philosophy from psychology by reference to “psychologism,” the supposed confusion of psychological descriptions with epistemological norms. The entities that epistemology deals with *are* psychological, he says, namely acts and states of cognition, and no distinction between norms and descriptions can circumvent this brute fact.

One pattern of behavior does not become less a psychological attribute than another merely through being the more praiseworthy of the two, and neither do the prescriptive/validational aspects of a theory of knowledge diminish the psychological nature of what this is a theory about. As I hope to illustrate below, there are probably few significant problems of epistemology where philosophical progress is not seriously impeded by our lack of technically detailed understanding of the psychological mechanisms involved. (1972b, p. 26 279)

This is how he introduces “Problems in the Psycho-Philosophy of Knowledge” (1972), his most comprehensive statement on the working relationship that he envisages between philosophical and psychological approaches to the topic of cognition.

For the same reasons, he believed that attempts to use the analytic-synthetic distinction to keep psychology out of philosophy were futile, and largely the result of philosophy’s own impoverished conception of induction. In response to Putnam’s well-known attempt to separate the meaning of pain-words from pain-behaviors (Putnam, 1965), and thus to refute Logical Behaviorism, he argues in “The Synthetic Content of Analytic Statements” (1977b) that the analyticity of some assertions, i.e. the fact that their truth is inherent in their meanings, is a matter of degree only. In particular, he shows that analytic statements referring to contingent states of affairs—generally taken to be impossible in the philosophical literature—are actually a common feature of everyday inductions. This is the case, for example, when theoretical predications are initially taken to denote distinctive conditions revealed by certain observable indicators—as in the medical diagnosis of illness from symptoms, for example. In all likelihood, therefore, the child’s first pain-words have meaning-based links to observed pain-behaviors, to be replaced later by an introspective mental content that is taken (inductively and probabilis-

tically) to be the cause of these behaviors, without however losing its analytic base entirely. This would explain the characteristic immediacy of our inferences from behavior to mind, and why it is that having conceded minds to ourselves on such evidence, we are obliged by the same logic to concede them to others also—that is, he adds, “until we learn to argue like philosophers” (1977b, p. 471 350).

Apart from his efforts to reconstruct the notion of scientific inference in general, WR also sought to advance psychology by clarifying some of the discipline’s foundational concepts. He often does this as the occasion demands, when methodological concepts such as “variable” or “behavior” need some explanation in passing. But he has also written papers devoted exclusively to the conceptual analysis of one or another of psychology’s core concepts. This aspect of his work is represented here by two papers “Do Stimuli Elicit Behavior?—a study in the logical foundations of behavioristics” (1960a, p. 1) and “The Concept of Memory” (1965a, p. 69). In the former, he argues that the linguistic conventions of psychology, particular its reluctance to acknowledge the primacy of *facts* (rather than stimuli) in the elicitation of behavior, have “seriously undermined the ability of behavior theory to assimilate the higher mental processes”; in the latter, his point is that virtually all research on the subject of memory had failed to deal with the topic at all, having substituted retention of associations for memory proper, i.e. the recall of beliefs.

In “Problems in the Psycho-Philosophy of Knowledge” (1972b, p. 279) WR sets out the work that needs to be done to construct a psychonomically informed epistemology. Its empirical base should be a study of inductive inference in its natural setting, i.e. “the stuff of lab reports and research strategies” and “the technical arguments by which unromantic professionals persuade and criticize their colleagues” (p. 80 334). Yet from such modest beginnings he believed that the “psycho-philosophy of knowledge”, as he called it, stood “on the threshold to discoveries in inductive reasoning breathtakingly vast enough to put even the past century’s advances in deductive logic to shame” (ibid.).

Three short philosophical papers by WR are included in this collection, “Empirical Realism and Classical Semantics,” “The Synthetic Significance of Analytic Statements,” and “Nicod’s Criterion: Subtler than You Think.” These offer examples of his writing for a strictly philosophical readership in a non-controversial setting, at least in the sense that the issues addressed are well-known in the philosophical literature. Yet it is not difficult to find in the papers signs of WR’s dissatisfaction with that discipline, chiefly in his inductivist and Bayesian commitments. In all of them he rejects foundational orthodoxies, and as noted earlier in the section of Confirmation Theory, in some some of them, such as his classic 5-page comment on Nicod’s Criterion, he presents large areas of philosophic reasearch in a very poor light. Most tellingly of all, the response of the philosophical community to his papers to date has been almost total silence.

Psychometrics

In addition to his work in philosophy and psychology, WR published 19 statistical papers and a 600-page text-book on the mathematical theory of psychological testing, “Foundations of the Theory of Prediction” (1966a). These too are an integral part of his “inductivist” project, falling readily into place among his other attempts to incorporate the traditional work of psychologists into a general theory of inductive inference. Not surprisingly, therefore, he gives a lot of attention to the traits that that psychology claims to be able to infer from multiple indicators, while in a series of papers written in the 1980s and 1990s (see Appendix A, p. 431) he made major contributions to the theory and practice of multivariate analysis, including the computer program HYBALL that he designed for exploratory factor analysis (1991).

WR’s psychometric work is represented here by three papers, “Linear Correlations between Sets of Variables” (1965b, p. 111), “Domain Validity—Why Care?” (1978, p. 351), and “Sensitivity of a Linear Composite of Predictor Items to Differential Item Weighting” (1979, p. 359). In the first of these he uses Information Theory (or “Uncertainty Analysis,” as he preferred to call it) to present the familiar measures of linear relatedness, in particular the multiple correlation coefficient R , as a special case of a more general symmetric notion of relatedness between sets of variables. In doing so, he integrates the R -statistic, principal components, and canonical correlation in a single perspective that also provides an empirical approach to the study of hierarchical relationships between inferred variables, while demonstrating how little of the total relatedness that exists between sets of variables is captured in conventional regression analysis.

WR’s psychometric papers echo his critique of psychology in general, in particular its lack of explicit models of inductive inference, and its infatuation with mathematical formalisms. In “Domain Validity-Why Care” (1978, p. 351) he is at pains to point out, just as he was in “Scaling Theory and the Nature of Measurement,” that the primary concern of psychometrics is to give us a clearer idea of the trait we wish to measure, its relationship to similar traits, and the test-circumstances that are most likely to provide a valid measure of the quantities in which it exists in various situations. Everything else is secondary, particularly the restrictions and assumptions that are so frequently introduced in psychometric papers for purely mathematical reasons.

The history of test theory has repeatedly shown a proclivity to institutionalize theoretical fantasies in which some mathematical simplicity is embellished with little concern for what relevance, if any, it may have to the real world. I have no quarrel with such mathematical games in their own right—they are an inexpensive, joyful sport that at times can even be conceptually enlightening. But I do urge that this not be

mistaken for serious analysis of foundational issues. (1978, p. 87 356)

Here the term “foundational” does not refer to axioms and definitions but to the realities of test construction and administration. He is critical of a simplifying assumption made in Kaiser and Michael (1975) in order to derive the Tryon/Cronbach Alpha coefficient (a measure of consistency between test items) from a domain-sampling model of test construction, for when he probes the circumstances under which the assumption might conceivably obtain, the answer is “in all likelihood, never” (1978, p. 82 352). The theory, therefore, insofar as it relates to the trait being measured, has dubious empirical import.

In this instance, his dismay is all the more because he believed that the domain-sampling model of test structure, in its original formulation (Cronbach, Gleser, Nanda, & Rajaratnam, 1972), provided an elegant framework for “inquiry into the nature and interpretation of measurements” and had the potential to make a major contribution to what he termed “the theory of scientific perception,” i.e. Exploratory Induction. WR is referring here to the notion of “construct validity” first introduced into test theory in the 1950s in hope of providing an alternative to the prevailing operationalist and criterion-based approaches to test validity. In practice, however, the notion of construct validity was largely absorbed by the criterion-based approach, and came to be seen as a more abstract and global form of validity when in fact its thrust is precisely in the opposite direction, downwards, towards the origins of the trait-concept in more primitive forms of EI of which we still have little understanding or even awareness.

The notion of “construct validity” is far from the appeal to magical spirits or search for metaphysical absolutes that some writers with commendably skeptical temperament have taken it to be. It merely makes explicit—and hence amenable to clarification and correction—certain primordial inductive extrusions from past experience that shape our thinking irrespective of whatever conscious assent we give to them. (1966a, p. 209)

The third psychometric paper reprinted here, “Sensitivity of a Linear Composite of Predictor Items to Differential Item Weighting” (1979) is both a solution to a practical problem (the optimal weighting of predictor variables) and, as usual, a demonstration that some earlier claims made on the subject are true “only under arbitrarily special assumptions whose relevance for practical prediction is demonstrably almost nil” (p. 289 359).

WR’s largest single work, the 600-page “Foundations of the Theory of Prediction” (1966a) is both a text-book and an overview of the major epistemic and statistical issues that arise in the construction of psychological tests and the interpretation of test results. This takes us to the heart of EI once more, since

test results are simply structured sets of observations used to ascribe theoretical attributes to those taking the test, while the task of constructing good tests deals with the optimal structuring of eliciting complexes to ensure that they actually measure what they claim to measure. Thus he often refers back to the theorems and proofs of *Foundations* in his later work, while its remarkable 70-page *Epilog* can now be read as a program for much of his own work in the years that followed, laying out as it does the larger issues he was forced to pass over in the body of the book. Among these are the problem with so-called “objective” (frequency-based) probabilities and their associated sampling statistics, the limitations of linearity, matters of scaling and measurement, and his over-riding concern that the methods of data analysis most commonly used in psychology were not in the best interests of its development as an empirical science.

Concerning the role of quantification in science generally, WR notes that whereas mathematical formulae are often included in psychological theories with the intent of clarifying what exactly the theory is saying, mathematics is not suited to such a role. “A chief virtue of mathematical formulae,” he says, “is their extraordinarily compact condensation of ideas which would be of horrendous complexity if written out in logical fullness” (1961a, p. 476), a point he amply illustrates for some elementary mathematical formulae in “Ontological Induction and the Logical Typology of Scientific Variables” (1961b, p. 353–5 44–5). Unfortunately, when greater clarity is required it is only “logical fullness” that can provide it, with the result that the mathematical equations proffered in psychological theories tend to be cryptic at best, they don’t always say what their authors intend them to say, and in some cases they are difficult to interpret as empirical claims of any kind until suitable boundary conditions are extracted from the accompanying prose.

WR’s explanation for the “near-idolatrous” attitude towards quantification in science is more straightforward than most. Quantities, he says, arise from our need for variables with a transfinite number of values, without which we would be unable to conceive of the myriad of continua that confront us in the natural world.

With few if any significant exceptions, a transfinite-valued variable must either be intrinsically quantitative or have quantitative correlates in order for all its values to be individually conceivable by us. Wherever nature is more abundantly diversified than finite categories will express, we can know its fine structure only in quantitative terms. (1966b, p. 232 178)

Science and Common Sense

The phrase “in science and everyday life” rings out in WR’s writings, drawing attention to the continuity he saw between the two domains in their construction of reality, and the corresponding gulf that that has opened up between them and philosophy, psychology, and the human sciences. The continuity WR sees between science and common sense lies in their common dependence on EI, albeit at different levels of sophistication. In its most basic forms, EI is a biological fact: “Natural selection has gifted us with both statistical and ontological (explanatory) induction as hard-wired survival processes that we take to be rational, though not infallible, with no more need for metarational justification than we have need to justify our breathing” (2008, p. 1123–426). From our first contacts with our surroundings, EI generates a stream of traits, dispositions, abilities, and other theoretical entities that explain the events occurring around us in terms of their causal dependencies. And although the theoretical concepts of mature science, such as those referring to the structure of matter, have little in common with their common-sense counterparts, the inferential structure in which they are embedded remains the same. In both cases the concepts are justified by the adequacy of the causal explanation they provide for what is observed, while the accumulation of conviction that the explanation is correct follows the laws of conditional probability.

Consequently, WR saw little merit in the many attempts (made by philosophers and psychologists alike) to drive a wedge between science and everyday thinking. In philosophy, for example, it has often been argued that common-sense dispositional terms (“fragile,” “soluble,” “shy,” “friendly,” etc.) are vacuous, subjective, circular, non-falsifiable, and so on, in ways that makes them entirely different from the theoretical terms of a scientific theory. In WR’s view, on the other hand, the dispositions talked about in everyday terms differ from their scientific counterparts only in degree of sophistication (1973, 1984). Even the *vis dormitiva* of sleeping potions in Molière’s famous satire—still a source of mirth in the philosophical literature—may be a perfectly good theoretical term, provided it comes with some information on substances that have this property and those that lack it. In WR’s view, common-sense dispositional concepts are “the theoretical constructs of a primitive science” (1973, p. 59). Consciously or otherwise, they compel inference from patterns of events to unobserved states that would explain how the patterns came about. In other words, they are elementary products of EI.

WR takes the same position on common-sense psychology or “folk-psychology” as it is sometimes called. The fact that the inferred dispositions now refer to “meanings” and other states of “mind” poses no new problem in his inductivist/-behaviorist approach. Once it is accepted that behavior is elicited by facts rather than stimuli, the differences between mentalistic and behavioristic accounts of cognition are minimal, since the eliciting complex will need to have a fully propositional structure in either case. To be sure, the limitations of common-sense

psychology are severe and well-documented. For WR, however, its real-life functionality cannot be overlooked, and for this reason he has no hesitation in taking it to be the best starting point for an empirical psychology of cognition, thus taking up a position that is in stark contrast to “philosophy’s latter-day reluctance to countenance meanings as natural occurrents of any sort” (1972b, p. 63 318).

What holds for common-sense psychology holds also for what he calls “commonsense confirmation theory,” i.e. our first impressions about the kinds of observations that provide evidence for or against a given proposition. Although the theory can be mistaken, its importance “lies not in its support (or even recognition) by extant philosophic models of rational inference but in its existentialist operation as a rule we live by” (1971, p. 367 255). In this role, common sense is unlikely to be entirely mistaken, and even when it is, it “may well nonetheless provide (as common sense so often does) an essential first approximation to what needs be said by a more technically adequate account of the matter, without whose inspiration the latter would never get off the mark at all” (ibid.). This was the approach he adopted in his own work on confirmation theory (1968, 1971, p. 233) in which he seeks to loosen the grip of logical entailment on the formal theory of confirmation and thus to create a broader account that would accommodate both the entailment-based account *and* the “mistaken” intuitions of common sense.

On matters relating to the conduct of research, therefore, WR’s work encourages psychologists to do whatever seems reasonable to them according to their native standards of reasonableness and their current grasp of EI, and to resist all forms of peer-pressure that might take them in any other direction.

You have been practicing explanatory induction daily throughout your entire life (well, probably not *in utero*); and attempting to suppress it when seeking scientific explanations of empirical phenomena would be like mandating that we should never eat fresh fruit lest we might sometimes bite a worm. (2008, p. 1123 427)

Or again:

A scientist is ever so much more likely to grope, reason, intuit, plunge or blunder his way to novel discoveries and revolutionary insights if he does what comes naturally to his professional instincts than if he is constantly looking back over his shoulder to make sure that he hasn’t violated somebody else’s rules for the game. (1970, p. 74 200)

More specifically, his advice to the fledgling psychologist who has just completed a research project is to “resist with all your might, so far as supervisors, grant appraisers, and journal editors allow, actually interpreting your resultant data as a binary pro/con vote on some hypothesis proclaimed in advance” (1997, p. 383 393).

Instead, he thought students would be better advised to look more closely at the theoretical terms contained in their hypotheses and theories. Some of these will resist explicit definition, given that their meanings reside in functional relationships with other theoretical terms of the theory. More radically still, closer analysis will show that the tasks of definition and confirmation cannot always be assigned to different phases of research. In practice, they often proceed in tandem, each conceptual development being linked to a new feature of the data they purport to explain, since “in both everyday life and technical science, it is commonplace for data regularities to inductively disclose their own explanatory interpretations” (1977a, p. 202). Thus EI brings together what he termed “the two pragmatic directives for disciplined thought that should be engraved on the forebrain of anyone who fancies himself as a professional thinker, namely, What do I mean? and How do I know?” (p. 198).

Epilog

What stands out in WR’s account of EI is the glaring mismatch between the way individual researchers in the human sciences do their thinking, and the hypothetico-deductive format they use to dress it up when they report it to the scientific community. The latter, he argues, is pure facade: professional inference in science does *not* consist primarily of sequences of (1) speculation, (2) deduction, and (3) confirmation or disconfirmation; for the most part it proceeds by EI, directly from observed patterns of data to features of their underlying source, sparsely conceived at first and requiring additional research to disclose more about their nature. Failure to recognize this, he believes, has led to a preoccupation with prematurely inflated speculations and inadequate conceptual structures, many examples of which he explores in contemporary psychology.

Critical though it is, WR’s work is never pessimistic or cynical. Indeed it is often invigorating, as he works to restore the human sciences to their rightful place in the continuum of rational belief-systems that stretches from intelligent perception in everyday life to the physical sciences, based primarily on inductive inferences consistent with Bayesian principles of conditional credibility. Nor did he think that massive changes were required within psychology to set things right: explanatory induction has not been eradicated, merely forced to stay out of sight for the time being. As he saw it, his task was simply

to make evident that the methodological Emperor paraded in the standard textbook fable is quite naked, but that unbeknown to his Court followers, his working-class subjects have prepared for him a wardrobe of homespun which, although still clumsily tailored, is more than adequate to restore his comfort and dignity (1972a, p. 96 262).

In pursuing this objective WR’s primary analytic resources have been those of

contemporary philosophy of science, principally semantics, logic, and the theories of inference, evidence, and confirmation. He often finds them wanting, out of touch with the realities of empirical research and, in some of their current forms, of no help whatever to psychology but rather the opposite. Philosophy has yet to subdue its infatuation with deduction and the history of science, and it still underestimates the importance of common sense and the special sciences for an adequate account of inference and confirmation, while at the same time failing to appreciate how badly epistemology has been hampered by lack of psychonomic detail on the nature of human cognition.

Nonetheless, WR believed that philosophy had far more to contribute to the human sciences than they could repay in the short term. He tries endlessly to convince psychologists that the content and structure of their theories are just as important as the quality of their data, and his one wish for psychological theories of all kinds is simply that they should be “*analyzed*—exactly, sensitively, and exhaustively” (1970, p. 103 232). Moreover, he ventured that only a very modest amount of effort was required to achieve the standards of reflection he wished to see, scarcely more than “being aware of what one is doing and systematically looking for better ways to do it” (1961a, p. 473). Yet psychologists and their counterparts in the other human sciences still tend to see conceptual analysis as something quite foreign to their discipline. The tragedy here is that it is they who stand to lose most from the continuing stalemate. For philosophy’s disinterest in induction leaves it, at worst, with an incomplete account of scientific inference, while the lack of philosophic acuity in the human sciences poses a direct threat to the validity of their findings.

Reading the Papers

A word of advice for the reader. First of all, do not be put off by the mathematical and logical proofs that can be found in some of the papers. Even if your skills in these fields are limited, it does not follow that attempting to read the papers would therefore be a waste of time. The prose commentary always summarizes the content of the proofs, and you should therefore feel free to skip over them once you are clear about the conclusions they establish. In a body of work as highly integrated as WR’s, the most important thing is to understand the broad outline of the paper and the way its conclusions relate to those of the other papers, and this is something that can always be achieved while “reading around” the proofs and taking them on faith.

On the other hand, should any of the papers tempt you to brush up on the basics of set theory, predicate calculus, regression and multivariate analysis, matrix algebra, propositional logic, conditional probability, or any of the other formal languages used in the papers, then you may be assured you will be working in an

environment that is highly instructive. Given WR's regard for everyday inference, he does not introduce formal notation lightly, and when he does the exercise has a sense of purpose and a relevance that primers and introductory courses cannot always supply. For in the framework of Explanatory Inductions WR's incursions into formal methods are all concerned with just two extremely personal questions—the two posed in all forms of rational inquiry—namely (1) what *kinds* of things we think we are talking about, which takes us immediately into the theory of sets, relations, functions, scales, measures, tests multivariate analysis, factorial decomposition, and other forms of EI, and (2) how strongly we feel entitled to *believe* the claims we would wish to say about them, which takes us into confirmation theory and the calculus of conditional probabilities.

The diversity of the papers may leave you wondering where to begin. That depends on your interests, of course, but if you are still unsure you might try “Problems in Psycho-Philosophy of Knowledge.” Although written to make a very specific point, namely that the study of knowledge (in both science and everyday life) requires empirical psychology as well as philosophy, it also makes a good introduction to WR's work as a whole. And since WR is equally adamant that psychology cannot go far without conceptual acuity of the sort promoted in philosophy, the early papers “Do Stimuli Elicit Behavior” and “The Concept of Memory,” which make the point forcibly, would complement it nicely. Alternatively, there is an argument for beginning with the topic of Explanatory Induction, since it is the unifying theme in so much of WR's work. In that case “Scientific Inference: The Myth and the Reality” can be recommended, followed by “Good Science is Abductive,” “Meehl on Metatheory,” “Ontological Induction,” and “Scaling Theory.” Similarly, Confirmation Theory is another unifying theme of WR's work, and if one wished to begin with it, the sequence might be “The Art of Metascience” first, followed by “The Problematic Importance of Hypotheses,” and “Confirmation Theory II.”

Since the strength of WR's papers is their interdisciplinarity, you will probably have only limited success if you attempt to unpick the strands of philosophy, psychology, mathematics, and logic from which they are woven. And with that in mind, perhaps if you were simply to begin at the beginning, with the first paper, and skim them all as they come, in chronological order, to get a feel for their scope and their principal focal points, and then to read them more closely, with the help of the index in the areas that are of special interest to you, that might be as good a plan as any.

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