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Dispositions Do Explain

Picking Up the Pieces after Hurricane Walter

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It is hard to choose between laughter and tears in reacting to Weimer's fulminations against dispositional concepts in psychology. I guess that turns on the importance one places in matters methodological on trying to get it right instead of putting on a show. Some folks admire polemical posturings and grand oratory; others prefer analysis and tight argument.

The gist of Weimer's message is this: (a) the proper aim of psychology, like that of all natural sciences, is to arrive at *causal explanations* for its observations. (b) Unlike theoretical/structural concepts, which deal with the underlying sources of surface events, dispositional ascriptions merely summarize observations and are incapable of explaining them. (c) Virtually all concepts exploited in mainstream psychology, behavioral concepts in particular, have been dispositional and thus scientifically worthless. I have no quarrel with (a) and could even find some small sympathy for (b) were the claim to be put with knowledgeable care. But (b) is not really tenable even for the narrowest construal of dispositions and sets new standards for indiscriminate fatuity when inflated into (c). Apparently, Weimer has skimmed a few position summaries out of the mid-century philosophic literature on dispositions without grasping the semantic, ontological, and epistemic problems for which these were hopeful but imperfect solutions; and he now undertakes a slash-and-

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burn critique of our indigenous conceptual practices in those terms without, evidently, understanding the force of working psychonomic constructs either.

1. Rudiments of Scientific Epistemology

If present debate is to achieve more than an exchange of pejoratives, we had best review recent theory on the nature of scientific knowledge. Early this century, concerns for epistemic reliability that have always been foreground for scientific practice became elevated in the newly emergent disciplines of empirical psychology and philosophy of science to an especially high level of self-conscious metatheory. Scientists and most philosophers are above all *epistemic engineers* who undertake professionalized responsibility for rational conclusions about chosen topics of concern. This demands critical attention not merely to the clarity, precision, and credibility of particular target propositions but also to refining the very principles of reason by which high-grade epistemic adjudications are to be made. Any verbally fluent layman can weave his received language into endless tapestries of soft speculation. But to shape up propositions that sustain sophisticated conviction requires some guidance from a tested technology of evidence and inference, even though our most advanced epistemic methodology is still actively evolving and its verbalized theory continues to lag behind its praxis.

Traditionally, both practitioners and philosophers of science agree that scientific knowledge rests on a foundation of *data,* comprising beliefs that are given more or less noninferentially to suitably positioned and prepared members of our epistemic community, and whose credibility in light of the circumstances of their production, is as high as researchers on the topic at issue are currently able to contrive. (To endorse this tradition is not to presume that data are ever certain, nor even to deny that holistic models of belief change may eventually prove normatively superior to the classic ideal that our synchronic beliefs should be partially ordered by credence dependency. It is simply to acknowledge, with respect, that grounding conclusions on reasons is the accepted epistemic norm in *all* established intellectual disciplines—law, medicine, agriculture, industrial engineering, etc., just as much as in philosophy and theoretical science.) Data prevailingly arise from perceptions of particular events, or records thereof, and for that reason are often called *observations.* But data in the present broad sense also include mathematical premises and other nonsensuous convictions to which we appeal today—always at risk of revocation tomorrow—in support of our reasoned judgments. The totality of sentences and other linguistic expressions constructable from just the concepts and syntactical devices (logical operators and connectives) used in a science's data statements is that discipline's observation language. Note, incidentally, that commonsense perceptual predicates such as *yellow, cold, smooth,* etc., fail to be "observational" for any science that proscribes them as too vague or imprecise for acceptance into its technical data statements.

The line between what expressions are or are not included in a science's observation language is never sharp and depends on local standards. Nevertheless, the premises one accepts for an argument make an enormous difference for what conclusions can be drawn; and both philosophers and psychologists have argued mightily about what concepts are properly admissible in our observation language, most conspicuously over the necessity (cf. nineteenth-century phenomenalism) or the inacceptability (cf. twentieth-century behaviorism) of mentalistic terms therein. Even so, negotiations over a science's data language are just preliminary to the main problem of scientific knowledge, namely, *What can we learn from data?* This is the issue that fueled the progression of philosophy of science from nineteenth-century pragmatism through early twentieth-century operationism and logical positivism to modern empirical realism (logical empiricism), and for which the theory of dispositional concepts has been most saliently paradigmatic.

Informal data interpretation in everyday life—that is, our intuitive inferences from whatever beliefs we momentarily feel pretty sure of—is something we do so repeatedly with so little conscious effort that it takes considerable critical expertise to appreciate just how modest is the degree of confidence these conclusions usually warrant. Our workaday epistemic frailty has three main sources: (a) First of all, commonsensical datum beliefs tend to be soft, impressionistic cognitions that easily crumble under critical challenge. (E.g., when you observe the blonde girl softly sobbing in the corner, just how sure are you—or should you be—that she is young enough to count as a girl or is not really a transvestite, that her hair is not in fact pale brown, that she is truly in the corner rather than just nearby, or that her respiratory spasms have an emotional origin?) (b) Moreover, everyday inferences are almost always ampliative, that is, not deductively valid; and how much credence their premises properly confer upon their conclusions is usually quite unclear even without adjustments for the uncertainty of those premises. (c) Finally, the cognitive force—the *meaning*—of these conclusions is often too obscure to sustain high conviction under any evidential or inferential circumstances. What bodily measurements can settle whether Mary is slender? And when you are advised, "Love in all its subtleties is nothing more, and nothing less, than the more or less direct trace marked on the heart of the element by the psychical convergence of the universe upon itself" (Teilhard de Chardin, 1955, p. 265), are you *able* to believe or disbelieve this? Technical science minimizes epistemic debility (a) by insistence on high-quality data and has also engineered remarkable advances in respects (b) and (c), even though theories of that praxis have remained largely inarticulate apart from the literature on statistical methodology. Meanwhile, the great wave of positivistic philosophy of science early this century was largely a pursuit of surcease from the intellectual *angst* of (b) and especially (c).

Classically, philosophical epistemology has sought nothing less than hard-core certainty, motivated by the idealistic presumption that incorrigible knowledge is ours for the taking if only we can become astute enough to discern it. This orientation puts a premium on logically valid deduction from impeccable data and in recent times has spun off a deeply comprehensive understanding of logical validity. But, unhappily, the epistemic needs of science go far beyond that in vital respects. For openers, scientific inferences are virtually never valid deductions and hence call for a normative theory of ampliative argument forms. This has proved difficult enough to develop even for conclusions that are merely statistical generalizations of sample data. But worse, scientific explanations for observed events often incorporate "theoretical" concepts that neither occur in any of the science's data statements nor are technically disqualified as observational merely by substandard clarity. How might we acquire theoretical knowledge of this sort? To do so we must reason by what I shall here call *creative ampliations*, or, arguments that confer high credibility on conclusions that discriminatively contain descriptive (nonlogical) terms not found in the argument's premises.

• By *discriminative* here I mean that the conclusion's added terms are specifically called for by the argument's epistemology, rather than being arbitrary imports as they are, for instance, when the argument's primary conclusion is a universal generalization into which any terms in the range of its quantifiers can be inserted, or when the conclusion is of disjunctive form *p*-or-*q* for an arbitrary sentence *q* when our premises suffice to infer *p* alone. What makes creative ampliations distinctively enigmatic, if ever normatively acceptable at all, is that if 'C(*a*)' is an inductive consequence of premises in which descriptive term '*a*' does not occur, it seems as though these premises should by the same ampliation form equally support any other proposition 'C(*b*)' constructed by replacing '*a*' in 'C(*a*)' by some other expression '*b*' of the same logical type as '*a*.' It is important to appreciate that statistical inductions are *not* creative in this sense of discriminative concept importation except insofar as the concept of probability, as distinct from relative frequency, is allowed to occur in some statistical conclusions.

Further, if such explanatory conclusions are even to be conceivable, we must have a way to make semantically meaningful the theoretical terms that occur in them. Creative ampliations achieve that as part of the total credence they confer upon their conclusions; but without a theory of *how* that can be done, this is a *modus tollens* reason for doubting that creative ampliations can occur at all.

2. The Semantics of Scientific Constructs

The skeptical temperament that pervades science and analytical philosophy takes these epistemic-engineering problems of explanation very seriously indeed. For lacking a plausible theory of how explanatory knowledge can be attained in practice, it is not unreasonable to doubt that this is ever possible, or at least to abstain from its endeavor. Logical positivism courageously sought to face down this doubt in the only way that classical epistemology could envision, namely, by proposing that theoretical sentences which seem to make claims about unobserved entities do not really do so. According to positivism, the only good concept is an observational concept—that is to say, only locutions recognizably in or translatable into our data language have the semantic quality that enables sentences using them to be true or false. If so, all explanations that exploit theoretical terms must prove under careful analysis to be shorthand for data-language statements in that their problematic words, or possibly larger contexts in which these occur, are semantically equivalent to (i.e., explicitly definable by) constructions using just the observational vocabulary. For otherwise these theoretical sentences would literally be nonsense and perforce could not explain anything. On the other hand, if theoretical conclusions do translate under analysis into observational statements, it becomes plausible that they can in principle be highly confirmed from our data merely by deduction and statistical induction.

The thesis that theoretical expressions are always equivalent to data-language constructions if meaningful at all was a large promissory note whose lack of cash backing eventuated in bankruptcy for positivism during the 1950s. Yet for quite some time, that note seemed close to redemption by its cosigner, early operationism. Both in technical science and in everyday life, we judge the applicability of many theoretical predicates to particular objects by how these respond to certain tests or operations performed on them. Indeed, test/outcome couplings often seem to constitute the meanings of such predicates under a schema something like

(1) Object x has property D_{SR} at time $t =_{\text{def}}$ If x is Sd at t, then x Rs at or just after t,

or more formally

(1a) $$D_{SR}(\_\_) =_{def} \text{If } S(\_\_) \text{ then } R(\_\_),$$

wherein 'S' and 'R' are placeholders for simple or complex predicates the nonlogical terms of which are either observational or have a form-(1) construction recursively prior to 'D_{SR}.' Definitions of form (1), or perhaps conjunctive compounds thereof, are what, following Bridgeman, philosophers of science have called *operational definitions;* and operationism, expanding upon the less articulate but otherwise identical pragmatism of Peirce and James, was the thesis that virtually all theoretical concepts in scientific practice can be explicated by operational definitions. Then if (1)'s *if/then* connective can be analyzed as a construct in our data language, as was long hoped by almost everyone who thought about the matter, operationism entails positivism.

Moreover, the evolution of operationism is also the modern history of disposition theory. Commonsensically, dispositions are whatever we denote by adjectives with endings such as *-able, -ile, -ive,* and *-ous.* But philosophers agree that *analysis* of dispositions intimately links them with *if/then* predications of the sort schematized by (1). Accordingly, we may say that dispositional concepts are whatever predicates seem most amenable to operational definition, even if increasing sophistication in this matter demands some improvement on basic operational form (1). This does not, however, mirror any sharp, received distinction between dispositional and nondispositional theoretical concepts; at most, it suggests that there may be some gradations to be acknowledged.

Although the theory of dispositions is thus largely coincident with the analysis of theoretical terms throughout most of the short history of philosophy of science, some recent divergence has accrued from efforts to cope with technical inadequacies in schema (1). Most notoriously troublesome has been explication of its conditionality: It is easily seen that the material conditional '$p \supset q$' (i.e., 'Either not-p or q') is not an acceptable reading of 'If p then q' in (1); and all efforts to define an extensional connective having the wanted subjunctive or counterfactual force (i.e., 'Were p then would q') have met with abject failure. Eventually, many philosophers of science simply *postulated* an *if/then* connective '$\rightarrow$' that aspires to express some causal coupling under which one state of affairs is nomically sufficient for another. From there, the revision of (1) that became popular in the late 1950s and still enjoys widespread though by no means universal favor among philosophers today (cf. Tuomela, 1978), is

(2) $$D_{SR}(\_\_) =_{\mathrm{def}} (\exists\phi)\{\phi(\_\_)\ \&\ (\forall x,t)[S(x,t)\ \&\ \phi(x,t) \rightarrow R(x,t)]\}$$

The right-hand side of (2) says that its argument (i.e. any object whose name goes in the blank) has some property whose conjoining with input property S always produces output condition R. (To keep the formalism simple, we disregard that this causal production never seems stronger than probabilistic in practice.) Any property B that enjoys this particular causal efficacy, that is, satisfies the second-order predicate

$$(\forall x,t)[S(x,t)\ \&\ \_\_(x,t) \rightarrow R(x,t)]$$

over which (2) existentially quantifies, is known in the current literature as a *base* of disposition D_{SR}.

Schema (2) differs from the formula proposed by Pap (1959) and endorsed here by Weimer only insignificantly in notational details. Observe, however, that dispositional concepts so defined are not positivistic data-language constructions, insomuch as the righthand side of (2) contains the flagrantly theoretical connective '$\rightarrow$.' And contrary to Weimer's poor-mouthing of disposition-ascriptions as mere data summaries ("bunches of facts"), neither can we ever *deduce* from extant data that some particular object x has D_{SR} at time t. For '$D_{SR}(x,t)$' asserts that x-at-t possesses a base of D_{SR}; and not only is any D_{SR}-base B almost certainly not an observable property of x at t, establishing that B is indeed a disposition-base is the same as demonstrating a universal law. Inference of $D_{SR}(x,t)$ from observations on x and perhaps similar objects is at the very least a large statistical induction and quite possibly an ampliation more ambitious than that.

Meanwhile, although virtually all theoretical constructs in scientific practice have analytic *if/then* implications, seldom do theoretical meanings seem to be *exhaustible* by a conjunction of test/outcome conditionals in the fashion envisioned by early operationism, not even when the conditionals are taken to be causal. But explicit definition is not the only way in which new concepts can be derived from old ones; and as the possibility of less straitened forms of definition became appreciated, logical positivism gave way in the 1950s to the empirical realism long championed by Feigl (cf. 1950, 1956) under the title "logical empiricism." This is the thesis that although theoretical terms get their meanings from the data-language contexts in which they are used, what they semantically *designate* are causal features of natural reality generally concealed from perception but knowable through their data consequences.

In idealized cases according to this view, one or more theoretical terms 'τ_1', . . . , 'τ_n' are implicitly defined by a theory '$T(o_1, \ldots, o_m, \tau_1, \ldots, \tau_n)$' in which are conjoined all the sentences we take to be criterially true of the τ_i, including in particular statements telling how these are related to observational entities $o_1, \ldots, o_m$. Substituting placeholders for all the terms introduced by this theory leaves us with a complex sentence schema '$T(o_1, \ldots, o_m, \_\_, \ldots, \_\_)$' that constitutes the meaning of terms 'τ_1', . . . , 'τ_n' by specifying their conceptual roles. But for theory '$T(o_1, \ldots, o_m, \tau_1, \ldots, \tau_n)$' to be *true*, '$\tau_1, \ldots, \tau_n$' must designate an n-tuple of entities that satisfy n-adic predicate '$T(o_1, \ldots, o_m, \_\_, \ldots, \_\_)$'. Empirical realism insists that so long as reality provides such entities this theory establishes them as the referents of its open terms and thereby does indeed attain full-blooded semantic truth.

In brief, then, empirical realism's claim is that theoretically (implicitly) defined concepts semantically function much like denotative descriptions whereby we fashion reference to a not necessarily observable entity e out of our conception of e's salient attributes: Theoretical terms are *about* whatever features of the world have the observationally describable character that their defining theory says they have. At the same time, insomuch as there may not in fact *be* anything like that, the theory's assertion makes a significant claim about reality. When theory '$T(o,\tau)$' is false, it is so not from τ failing to satisfy '$T(o,\_\_)$' but by virtue of 'τ,' so defined, failing to succeed at reference; thus, in chemistry, the phlogiston theory expired not from the discovery that phlogiston's properties were other than as originally conjectured but from the conclusion that phlogiston did not exist.

This brief sketch of empirical realism scarcely hints at the enormity of philosophic complications which arise when one attempts to sort out its technicalities:

- To begin, we must allow a recursion of theoretic definitions under which the old nonlogical terms {'o_i'} from which the new terms {'τ_j'} introduced by theory '$T(o_1, \ldots, o_m, \tau_1, \ldots, \tau_n)$' draw their meaning can include lower-level theoretical concepts as well as words in (if there be such) our rock-bottom data-vocabulary. This is not so straightforward in detail as it seems in principle; and especially problematic is how conjectures about the nature of causal connection fit in.
- At present, it is extraordinarily difficult to develop the nontranslational semantic theory needed to fathom the cognitive character of the-

oretical expressions (cf. Rozeboom, 1970a, p. 203ff) without formalizing all nonlogical linguistic elements as nominals and adopting a Platonistic ontology wherein, for example, if John is tall and Mary is blonde, one thing John and Mary have in common is *tall-or-blondeness*. Eventually we shall be able to forego such discomforting idealizations, but not until the simplistic models of semantic relations still favored by most philosophers of language have received extensive foundational alterations.

• Cases wherein the predicate '$T(o,\_\_)$' defining 'τ' under theory '$T(o,\tau)$' has multiple satisfiers, and hence endorses a plurality of candidates for the identity of τ, threaten to unstick the classic conviction in philosophical semantics that designation is a many–one function from denotative language into reality, that is, that no nominal has more than one referent on any one occasion of its usage. This multiple-reference complication has major import for the theory of language; yet apart from an abortive pass in Carnap's last testament on theoretical concepts (Carnap, 1961; criticized in Rozeboom, 1964) and my own past cries of concern (e.g., Rozeboom, 1960, 1962, 1970a), it has been totally repressed in the modern literature. (See Lewis, 1970, for a prominent and especially egregious example.)

• Whereas logical positivism could reasonably hope that statistical induction is all the ampliative inference that science needs, empirical realism's insistence that a theory's referential reach generally exceeds that of its supporting data reactivates the traditional empiricist doubt ("Science can only describe, not explain") that theoretical knowledge can ever be attained. Deplorably, post-positivistic philosophy of science has completely ignored the problem of creative ampliation, especially at the level of specific induction forms that govern data interpretation in research practice. (The large modern literature on abstract confirmation theory has been mainly a pursuit of rational foundations for statistical induction and Bayesian conditionalization. And the traditional hypothetico-deductive model of theory confirmation is demonstrably worthless as a guide to data interpretation—cf. Glymour, 1980; Hesse, 1970; Rozeboom, 1970b, pp. 93ff, 1982.) In my own work, however, I have been able to show how practical inference in science and everyday life is indeed often controlled by determinate creative ampliation forms that convert the patterning manifest in data into highly credible explanations for those phenomena (Rozeboom, 1961, 1972). And the most primitively compelling of these is precisely the ampliation form by which we infer properties that seem most paradigmatically dispositional (Rozeboom, 1961, pp. 362ff.; 1972, pp. 108ff.; 1973, pp. 66f.).

Despite these rough edges, empirical realism has become the epistemology of choice for most philosophers of science today—except, that is, for the far-too-many who indulge in a carefree realism that accedes equal semantic status to all concepts of the same grammatical type without much concern for how that is possible.

Within psychology, moreover, contrary to the mythology widely propagated by ignorant and/or malevolent bystanders, the methodology of scientific constructs practiced by leading behaviorists and other theorists of operationist persuasion (except a few vocal extremists such as Skinner) was an indigenous empirical realism long before philosophers learned how to distinguish this from positivism. When Hull and Tolman sought to develop behavior theory in terms of "intervening variables," they understood this label *not* in the positivistic sense later proposed for it by MacCorquodale and Meehl (1948), but as demarcation for hypothesized causal mediators ("hypothetical constructs" in MacCorquodale and Meehl's sense).

• That Hull's intervening variables were unobserved sources of perceptible events is unmistakable on pp. 21–23 of Hull, 1943. And although Tolman initially hoped for a positivistic externalization of mentality (cf. Tolman, 1925, p. 37), the metatheory in which he introduced "intervening variables" to psychology shortly thereafter (Tolman, 1936) designed these to be functional identifications of molar behavior's internal origins. Tolman reemphasized this in later years (Tolman, 1959, pp. 97f.), but it is plain enough in his original text if one knows what to look for.

In explicit opposition to positivist doctrine, the view that psychology's theoretical constructs designate real underlying causes through their conceptual roles in a "nomological network" was forcefully articulated by Cronbach and Meehl (1955). And continued efforts by psychologists to pin down what they meant by *operationism* (which few comprehended in the philosophers' narrowly technical sense) eventually clarified this, too, as our version of empirical realism. Garner, Hake, and Eriksen (1956, p. 158) put it well: "A concept has no meaning beyond that obtained from the operations on which it is based. . . . [Yet a plurality of] converging operations can lead to concepts of processes which are not directly observable." See also Campbell and Fisk (1959, especially p. 101), and Campbell (1959, p. 175ff.).

3. The Unabashed Realist's View of Dispositions

When the original operationist analysis of dispositions is updated to reflect operationism's empirical-realist turn, the model that emerges differs from (2) subtly but importantly. For we now have that when object x is hypothesized to have disposition δ_{SR} at time t, predicate 'δ_{SR}' is implicitly defined by a *theory* something like

(3) x has δ_{SR} at t, and δ_{SR} is a base of D_{SR} which also has properties Q,

in which 'Q' expresses whatever we consider true of δ_{SR} beyond its potentiating x-at-t to R if Sd. Theory (3) is analytically equivalent just to its first clause '$\delta_{SR}(x,t)$' (since its remainder only unpacks the meaning already implicit in ascriptions of 'δ_{SR}') and entails that x-at-t has some Q-kind property that causes Ring when triggered by S. Conversely, nothing more is needed for (3) to hold than for x-at-t to have such a property. So it is analytically true that

(4) x has δ_{SR} at t if x at t has some kind-Q base of D_{SR}.

When 'Q' is empty, that is, if it is analytically true that $(\forall\phi)Q(\phi)$, this becomes simply

(5) $\delta_{SR}(x,t)$ iff $D_{SR}(x,t)$

where 'D_{SR}' continues to be defined by (2). (Since D_{SR}-theory, too, can be Q-wise enriched by including additional constraints on 'ϕ' within the scope of (2)'s existential quantifier, the null-Q condition on (5) is not essential.) Yet the two sides of (5) do not say the same thing about x-at-t. '$D_{SR}(x,t)$' makes only the existence claim that x-at-t has a base of D_{SR}; *whereas if x-at-t does have some D_{SR}-base B, 'δ_{SR}' designates B and thereby enables '$\delta_{SR}(x,t)$' to signify x's having B at t.*

An *If-S-then-R* disposition can thus be understood in (at least) two distinct ways: In the semantically emasculated sense explicitly defined by (2), ascription of dispositional predicate 'D_{SR}' merely claims the presence of *some* property with the relevant causal effects without specifying which one. In contrast, a semantically potent dispositional predicate 'δ_{SR}' implicitly defined by theory (3) either fails at reference altogether (on occasions when its ascription is in error) or names the specific causal-source factor involved. That is, if x Rs at t in response to S, '$D_{SR}(x,t)$' says only that this behavior has an explanation of a certain sort, whereas '$\delta_{SR}(x,t)$' actually *gives* (part of) that explanation. For a fuller treatment of this semantically tricky distinction, see Rozeboom (1973).

In practice, seldom if ever do we make dispositional assertions for which enrichment predicate 'Q' in (3) is empty. At minimum, the creative ampliation by which we discover that x has δ_{SR} at t will specify the δ_{SR}-status of some things additional to x-at-t. And '$Q(\delta_{SR})$' may also include deeper information about δ_{SR}, notably, other input/output regularities that it likewise disposes and even, if our theory of S-R coupling is sufficiently advanced, what its microstructural nature may be. Variation in the content of 'Q' is what makes the dispositionality of theoretical constructs a matter of degree. When 'Q' is nearly empty we think of δ_{SR} as bare potential having no more substantiality than voiced by the right-hand side of (1). But as 'Q' anchors δ_{SR} by an increasing profusion of nomological-network strands, our knowledge (or surmise) of what δ_{SR} *is* infuses this erstwhile bare disposition with as much ontological solidity as any perceptible feature of the world. Moreover—a point of supreme importance for the actual *doing* of science—our ability through explanatory induction (creative ampliation) to initiate a concept of causal factor δ_{SR} and diagnose its instantiations when 'Q' is meager is precisely what makes it possible for us to study δ_{SR} empirically and *learn*, through a recursion of explanatory inductions, how 'Q' should be fleshed out.

The typical impoverishment of 'Q' in δ_{SR}-theory's early stages of development has led many philosophers to speak of theoretical concepts as *open* and provides some basis for Weimer's stigmatizing dispositional concepts as "incomplete." But all nonlogical terms of language-in-use, observational and theoretical alike, are open/incomplete in this way. For surely there is nothing comprehended by us with such perfected *verstehen* that we cannot conceive it even more richly through enhanced *wissen* thereof. There are no differences of cognitive kind to be found here, only differences in degree.

The foolishness of Weimer's contempt for dispositional psychology should now be evident. Regardless of what explanatory value dispositional predications may have when understood in the emasculated sense of (2), this is simply not what psychonomic scientists *mean* when they use the constructs that Weimer chooses to categorize as "dispositional." That label itself is not inappropriate; for psychologists who are serious about the epistemic merit of their work have generally tried to keep their constructs operational, that is, closely connected by creative ampliation to hard data. But the implicit definitions of these constructs in terms of overt behavior does not make them *about* that behavior; rather, they are expressly developed with a force like 'δ_{SR}' in (3) to *explain* psychonomic data—just as Weimer quite properly exhorts psychology to do.

4. Seeing Through the Glass Less Darkly

Weimer's global misportrayal of psychology's concept methodology embeds several local howlers that are too significant to let pass uncorrected:

- On p. 171, Weimer declares, "It is easy to show that no disposition statement can occur in the explanans of a causal explanation." This thesis is the foundation of Weimer's case against dispositions; yet he supports it only by appending "Pap's ingeniously simple proof of this claim." But Pap's alleged proof (1959, p. 286) consists of nothing more than the bare assertion (rewriting his formalisms in the notation of (2) with '(x,t)' condensed to 'x') that

> It is impossible to deduce from such an existential statement [namely, explication '$(\exists\phi)[\phi x$ & $(\forall y)(Sy$ & $\phi y \rightarrow Ry)]$' of '$D_{SR}x$'] conjoined with 'Sx', the description of the actual response Rx. Such a deduction can be made only from a specific law of the form $(\forall y)(Sy$ & $By \rightarrow Ry)$.

Even were this so, it would not preclude deducing 'Rx' from 'Sx & $D_{SR}x$' together with statement of law $(\forall y)(Sy$ & $D_{SR}y \rightarrow Ry)$, which Pap would accept as genuinely explanatory so long as the latter is indeed a law; so Weimer's extrapolation is a nonsequitur in any case. But not even Pap's more limited denial here is correct. For it is elementary deductive logic that 'Rx' is a valid consequence of 'Sx & $(\exists\phi)[\phi x$ & $(\forall y)(Sy$ & $\phi y \supset Ry)]$'. Therefore if '$p \rightarrow q$' analytically entails '$p \supset q$', as Pap clearly intends for '$\rightarrow$' when he deduces 'Rx' from 'Sx & Bx & $(\forall y)(Sy$ & $By \rightarrow Ry)$' and indeed makes explicit on p. 280 of Pap (1962), replacement of '$\rightarrow$' by '$\supset$' in '$(\forall y)(Sy$ & $\phi y \rightarrow Ry)$' is an analytic entailment under which premises 'Sx' and '$(\exists\phi)[\phi x$ & $(\forall y)(Sy$ & $\phi y \rightarrow Ry)]$' do indeed jointly entail 'Rx'.

It is surprising that a philosopher of Pap's stature should have made so obvious an error (or that Weimer should have praised it so uncritically). But judged from the global force of this passage, Pap merely misspoke himself due, in all likelihood, to his workload pressure under failing health. Almost certainly what he meant to say here was not that 'Rx' cannot be deduced from 'Sx' and explication (2) of '$D_{SR}x$', but rather that there exists no *causal law* $(\forall y)(Sy$ & $D_{SR}y \rightarrow Ry)$ to serve under the covering-law model of explanation as the major premise in an explanation of Rx by Sx & $D_{SR}x$. For although $(\forall y)(Sy$ & $By \rightarrow Ry)$ is a causal law for any *base* B of disposition D_{SR}, we cannot plausibly substitute D_{SR} for B therein because intuition is adamant that D_{SR} is not itself a base of D_{SR}. I concur with Pap's intuition here and in fact have made the same point in Rozeboom, 1973 (albeit the theory of molar causality still has something to say to the contrary). This is precisely why the difference between the philosopher's armchair reading D_{SR} of dispositions and the working scientist's understanding δ_{SR} thereof is so crucial. But to *prove* that Sx and $D_{SR}x$ do not jointly cause Rx requires an analysis of causality far deeper than Pap or any other philosopher has yet made public.

• Another serious misrepresentation, this time wholly of Weimer's own making, occurs in his conflation (page 181f.) of Carnap's 1936–37 and 1956 views on dispositions while intimating also that these agree with his own. Carnap's 1936–37 classic sought to meet early objections to (1) by his device of "reduction sentences," details of which do not matter here beyond our noting that dispositional predicates so introduced are not explicitly defined and are manifestly open to further enrichment. Two decades of debate eventually persuaded Carnap that Feiglian realism was the way to go; and although he did not propose a designation model for theoretical terms until 1961, he acknowledged in 1956 not merely that theoretical postulates can make nonobservational terms meaningful but also that his reduction sentences were a special version of this. (Hence if dispositional concepts were in fact adequately characterized by Carnapian reduction sentences, they would be full-blooded theoretical terms.) However, Carnap also felt in 1956 that "pure" dispositions were so nearly observable that they should be distinguished from theoretical states having less conclusive testability. But he did not formalize his new view of dispositions well enough to demonstrate (as Weimer would have it) their difference in kind from theoretical entities, and indeed left this importantly ambiguous in ways I cannot try to detail here. Even so, it is clear that Carnap does not propose an analysis of dispositions anything like (2), especially in that his account omits all mention of causal connection. Nowhere in Carnap, early or late, conflated or told straight, do we find support for the Pap/Weimer treatment.

• As Weimer sees it, functional definition gives us concepts that have genuine theoretical status and must hence be sundered from any rapprochement with dispositional concepts. According to Weimer, "functional definition of terms relates them . . . to meanings that deal with the goals, intentions, ends of action, and the like of the theorists [sic] involved" (p. 186). (By "theorists involved," Weimer presumably means the organisms theorized about, else this notion would conflate what a theory is about with its proponents' private lives.) Much of Weimer's insistence at this point (p. 186) that behaviorists have always defined responses functionally in practice (despite his earlier claim on p. 167 that "the concept of behavior . . . is dispositional") confounds responses defined in terms of causally antecedent purposes (i.e., goal-intentions) in the philosophical sense of *action* with responses conceived as Brunswikian distal achievements. The latter, not the former, have prevailed in behavioral research; and mechanisms of achievement-emission are straightforwardly, albeit complexly, amenable to construction in dispositional terms (cf. Rozeboom, 1970b, pp. 136–156). But Weimer's most unhappy solecism here is his interpretation of *functional.* Possibly he has assimilated this from the mid-century theory-of-explanation literature, wherein *functional explanation* demarked accounting for a system's properties in terms of what these seem to be *for* (cf. Nagel, 1961, pp. 23f., 520ff.; also Achinstein, 1977). There are also historical precedents within psychology proper (cf. Boring, 1950, p. 555), although scarcely unambiguous ones, for Weimer's usage. But in modern philosophical psychology, *functionalism* is, roughly speaking, the thesis that mentalistic concepts draw their meanings from what we think their referents *do* as causal mediators. Thus,

> Functional states [are sometimes said to be] characterized solely by their role in the production of output and by their relations to each other . . . ; to put it crudely they are characterized by their positions as intermediaries in the chain connecting stimulus to response. (Lycan, 1974, p. 50)

> Functionalism in the philosophy of mind is the doctrine that mental, or psychological, terms are, in principle, eliminable in a certain way. . . . [The claim is that] names for mental states and relationships . . . can be treated as synonymous with definite descriptions, each such description being formulable, in principle, without the use of any of the mental vocabulary. (Shoemaker, 1975, p. 306f. Shoemaker repeatedly speaks of this program as providing "functional definitions" for mental notions. See also Fodor, 1981.)

This usage is far from uniform—Block (1978) and now Shoemaker (1981), for example, prefer to equate functional states with generalized dispositions akin to D_{SR} rather than to δ_{SR} (albeit for the reason noted above that withdraws their right to attribute causal efficacy to functional states). Even so, for philosophers of mind, 'functional definition' is just another name either for theoretic (implicit, operational) definitions that pick out whatever designata have the causal roles ascribed to them in a behavioristic nomological network or, less wisely, for explicit behavioristic definitions that (2)-wise predicate the existence of such source factors. Either way, the presumption is that some of psychology's words get their meanings from others; and, correctly or not (cf. Rozeboom, 1977, p. 470f.), all versions of modern psychological functionalism allocate primacy to physical language—just the reverse of functional definition as Weimer describes it.

• Finally, Weimer's denial that dispositions can provide any novelty, productivity, or creativity in behavior merits a scoff or two. Precisely what he has in mind by this is left obscure; but it is something to the effect that organisms have infinite competences (notably, in sentence production) which any dispositional account thereof would have to characterize by an infinite number of dispositions even though "there is simply no way that an infinite number of anything can be seriously said to be 'contained' in a finite organism (p. 185)." One might try to rebut Weimer on his own level of amorphous abstraction by pointing out, for example, the infinitude of points classically contained in a finite spatial region. But instead, I shall call attention to a commonplace example of physical disposition that challenges Weimer to explain why it should not count as an infinitely creative competence. Specifically, I give you electrical resistance.

It is found empirically that for any given chunk x of hard stuff, if one performs a series of conductivity tests on x wherein an electrical voltage drop is applied across two separated points on x and the resultant amperage of current flow through x measured, the voltage drop $V(x,t)$ across x on each testing occasion t is related to the amperage $I(x,t)$ through x at t approximately in accord with the formula $V(x,t)/I(x,t) = constant$, where *constant* varies from chunk to chunk but *not*, for the most part, from time to time for the same chunk. (This generality has boundary constraints on the extremity of voltage, ambient temperature, placement of electrodes, and so forth, that would be impractical to detail here even if I knew them.) Physicists have accordingly inferred by explanatory induction that there exists a quantitative "resistance" variable Ω such that

(6) $(\forall x,t,v,\omega)[A(x,t) \,\&\, (V(x,t) = v) \,\&\, (\Omega(x,t) = \omega) \rightarrow (I(x,t) = v/\omega)]$

is a causal law under which, for any object x satisfying boundary constraints A at time t, x's having a particular voltage v and ohmage ω at t causes a current flow through x at t of amperage equal (approximately) to v divided by ω. Since any x's value of Ω can be diagnosed by $V(x,t)/I(x,t)$ whenever x-at-t satisfies A, and is believed on strong inductive grounds to be independent of t, much can be learned empirically about resistance, starting with its determination primarily by the chunk's minerological character.

Three aspects of this real-life example warrant contemplation. First, introduction and use of the concept of *resistance* in electrical theory has not essentially differed from that of trait concepts in factor-analytic personality research, except that the latter's initiating theory is richer (more network-wise complex) than was the former's; and the same is true, for example, of Pavlovian reflexes and Hullian habits. Does Weimer still want to insist that theories "based on the mechanism of associationism and the dispositional analysis of behavior were not the same sort of theories found in explanatory physical sciences" (page 186)? Or are physicists not allowed to treat resistance as an explanatory factor? Secondly, note that resistance is an *infinite set* of dispositional properties, not just one. For it is instantiation of some particular *value* of the resistance variable, not Ω as such, that disposes particular amperage outputs in response to particular imposed voltage drops. Since each x has only one of these Ω-values at t, this does not confute anything Weimer has said; but it illustrates how in scientific practice the laws governing even the purest of dispositions are usually far more complicated than acknowledged by philosophers' model (2). And finally—the salient point here—observe that the input and output variables in (6) are just that—*variables*—that subsume unlimited ranges of values even with Ω fixed at a particular value ω, so that the property $\Omega(\_) = \omega$ of having ω ohms resistance is in effect an infinity of form-(2) dispositions. That is, '$\Omega(\_) = \omega$' is defined theoretically by *(inter alia)* an infinite conjunction of the 'i'-indexed sentences

$$\{(\forall x,t)[A(x,t) \,\&\, (V(x,t) = v_i) \,\&\, (\Omega(x,t) = \omega) \rightarrow (I(x,t) = v_i/\omega)]\}.$$

So any finite chunk of matter having resistance $\Omega(\_) = \omega$ thereby also has infinitely many specific *If-A-and-voltage-such-and-such-then-current-so-and-so* dispositions. And this single resistance property, *having-ω-ohms-resistance,* correspondingly confers upon x-at-t the competence for infinitely many different current conductions in creative adaptation to infinitely many prospective imposed voltages, scarcely any of which x has encountered previously. Indeed, even if voltage drops did come in only finitely many alternatives, x's having a particular ohmage ω of resistance would still potentiate x's production of infinitely many different amperage "sentences"—that is, temporal sequences of varied current flow—in response to the infinitude of voltage-drop histories that might alternatively be x's lot.

Before Weimer assures us that organisms run by Hullian or Tolmanian behavior dispositions never respond creatively, therefore, I suggest (a) that he give us some specifics about how the creativity/productivity/novelty conferred upon people by internal structures differs in kind from that conferred upon chunks of matter by their electrical resistances and (b) that he acquaint himself with the specifics of how Hullian and Tolmanian organisms behave in radically new situations. For efficient adaptation—intelligent flexibility, not rote perseveration of a finite repertoire of responses reinforced previously—is precisely what successive editions of these theories were designed with increasing sophistication to achieve (cf. Rozeboom, 1970b, pp. 103–136). Whether they ever became as clever at this as the structural theories endorsed by Weimer is another question. But then I do not know what counts as a "structural theory" for Weimer unless it be simply any model of the causal machinery within. If so, there never have been and never will be any nonstructural theories in psychology; there are only differences to be commended or deplored in how simplistic, pretentious, inoperative, promissorily programmatic, or blue-sky fanciful are the structures proposed.

5. Epitaph

There is much to be learned from Weimer's essay. But the lesson is mainly how metatheory should not be done. The task he has undertaken is by rights worthy enough: It is important to criticize how psychology and other sciences run their epistemic economies. But the latter's praxis has advanced far beyond man-on-the-street competencies, and effective participation in its continuing perfection demands care and respect—care to put precision and accuracy into one's own contributions, respect for the detailed complexities of the issues and for the technicalities that dedicated professionals in these matters have previously worked out at the cutting edges of their often considerable expertise. Weimer's deliverance of prescriptive metatheory by homily and party slogan carries all the weight of a faith healer's decrials of modern medicine.

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