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Metathink—A Radical Alternative

Abstract

Psychonomic scientists have long recognized that empirical research needs guidance by an educated critical methodology if its product is likely to have much technical merit. But that *how we think* about psychonomic issues is equally in need of methodological monitoring—that the concepts we exploit and the manner in which we use them often suffer from clumsy ineffectually that a little critical analysis can easily set aright—has not yet sufficiently penetrated our professional awareness.

Les psychonomistes reconnaissent depuis longtemps que la recherche empirique a besoin d'être guidée par une solide méthodologie critique si elle veut que sa production ait beaucoup de valeur technique. Il reste cependant que notre profession n'a pas encore suffisamment pris conscience du fait que notre mode de penser *les problèmes psychonomiques a lui aussi besoin de contrôles méthodologiques et que les concepts exploités par nous ainsi que la manière dont nous en faisons usage souffrent souvent d'une grossire inefficacité dont il serait pourtant facile de se débarrasser par des analyses un tant soit peu critiques.*

It has fairly been said that the good workman respects his tools; and were time not at premium, I would amuse you with cute little parables about the carpenter who pounds with the wrong end of his hammer, the violinist who doesn't tune, the surgeon who lets his scalpel rust. The point to be made, however, goes beyond the obvious. Clearly the tools we use for a job are important; clearly their kind and quality place limits on what can be done. But note further that often one can complete a job with indifference to one's tools, even though an inferior

[1] With only minor changes, this is the text of my contribution to the Division 24 symposium, "Some Alternatives to the Traditional Science of Psychology," held on September 2nd of the 1974 American Psychological Association meeting in New Orleans. Organized and chaired by Stanley Well, with Don Dulany, Amedeo Giorgi, Klaus Riegel, and Joe Rychlak as the other participants, the symposium's intent was to appraise current outlooks on the scope and methods of psychology by reviewing the payoff of various alternatives to the "traditional paradigm" in the work of spokesmen for these. (Hence the sporadic egocentricity of what follows.) I have hesitated to publish this piece, since its profundity leaves much to be desired. But if as an undemanding overview it brings the art of concept analysis to the attention of readers who would otherwise be unaware that this is something one can systematically—and profitably—endeavor at all, it will have served a useful purpose.

product almost certainly results. It is entirely possible for a craftsman's guild to sustain surprisingly crude instrumental practices if those techniques do work after a fashion and no acknowledged examples exist to show how much the product can be improved through more sophisticated use even of tools already available, much less by ones that can be developed once need for them has become appreciated.

It is my thesis that modern psychology suffers from gross ineptitude in a major sector of its methodology, namely its *concept* methodology. By that I do not mean to derogate particular concepts now prominent in one psychonomic sector or another, or to echo the recently popular cliché that in our quest for scientific respectability we have lost touch with what is most meaningful in psychology. Neither am I disturbed by our persistent failure to submit precise definitions of our technical terms. (I *am* concerned in part about the frequent obscurity of such terms, but tidy dictionary definitions are no answer to that.) Rather, my distress is that we have not yet learned how to think effectively with our chosen concepts regardless of their content; and worse, that efforts to enhance our ideational proficiency are usually met by hostile rejection if they receive any notice at all.

Please don't mistake me. I'm not knocking the quality of intelligence in our profession. We have seen our share of brilliance and will continue to do so. But our discipline's intellectual achievements have been largely the result of *intuitive* thinking, often creating important new technical concepts to be sure, but exploiting these in ways that just happen naturally, untouched by self-critical examination of the process and its product. I have no quarrel with intuition. We can neither dispense with it nor should try to do so. It's just that intuition alone, like an untuned Stradivarius or a Rolls-Royce engine with dirty plugs and desynchronized timing, seldom produces anything near the power and beauty of performance it can develop under astute monitoring of its functional parameters.

Let me put it to you this way: Everything we do as psychologists, from problem selection and data coding to abstract theorizing, is fundamentally and inescapably grounded in a structured flow of ideas, i.e., our *thinking*. Surely it must be agreed that productive thinking—that is, thought directed toward some valued end, whether that end be semantic truth, existential fulfillment, esthetic elegance, or all of these and more—is a *skill* (more precisely a multiplex of skills) that can come in many grades of excellence. Now; do you know of *any* complex human skill that cannot be profoundly influenced by the quality of its training, especially training that is analytically engineered to diagnose and improve deficiencies in specific subskills that degrade excellence in the integrated whole? Would it not be bizarre to assume that productive thinking—surely the most advanced activity ever undertaken by living organisms—is an exception to that principle? Yet where in professional psychology is one instructed in how to think (not *what* to think, but *how*), or even made aware that such study might have value? Admittedly,

my question's rhetoric is exaggerated. We do teach and practice a sophisticated statistical methodology, though there is vastly more to human reason than just sampling-inferences about probability parameters. And discussion of research in particular content areas cannot help but condition students in the use of that topic's technical terms. But what little concept methodology we do recognize is fragmentary and mostly topic-specific. When I cite learning *how* to think, I mean a proficiency in the management of ideation that is content-independent, an operational grasp of *how ideas work* that allows one to think in his chosen medium toward his chosen goals without stumbling over his own conceptual feet.

My proposed "alternative" (really an "additive") to traditional psychonomic science, then, is *metathink*. That is, to think about our own thinking in ways that enhance its quality, to foster an intellectual climate wherein it is respectable to do this, and to provide our students with a smatter of its technique. This proposal would be fatuous if we were already near asymptote in our conceptual efficiency, or if there were no practical way to engineer such improvements; but neither is at all the case. Most substantive problems in psychology are perfused with confusions due to murky ideation that metathinking can expunge quite easily if its basic techniques are taken seriously and practiced to a level of modest proficiency. I haven't time to say much about these methods here, but that is no great loss. They are not verbal rituals to be mouthed, but something one learns to *do*; and I can no more convey even respect for their worth much less the actual know-how just by talking about them than you could teach a child to tie his shoestrings by verbal instruction alone. Their aim is simply to make one aware of what his concepts are doing for him, by what depth-grammar they operate and how well, appraised not by holistic good/bad assessments but through discriminating analysis of each concept-feature's role in the total structure of one's ideas on a given topic. Essentially, they are just ways to operationalize the two pragmatic directives for disciplined thought that should be engraved on the forebrain of anyone who fancies himself to be a professional thinker, namely, *What do I mean*? and *How do I know*? (The first-person-singular focus is important here, since metathink is most properly a form of *self*-discipline. For if its foremost aim is to make explicit what has been inarticulately intuitive, those intuitions must first of all be made accessible to inquiry. To concept-analyze behavior theory, or phenomenology, or psychoanalysis, or whatever, one need first of all to be able to think like a behavior theorist, phenomenologist, etc.) Metathink methodology includes such elementary but seldom-attempted moves as: (1) List the topic's most visible systematic/theoretic/explanatory terms, like "habit", "reinforcement", "stimulus", "plan", "schema", etc., which almost always name types of complex phenomena or categories of abstract entities, and then verbalize a few *specific instances* of those phenomena or categories. (Unless you have actually tried to do this, you wouldn't believe how difficult it can be.) (2) Construct some grammatically well-

formed sentences incorporating instances of these concept-categories and diagnose their detailed logical form, e.g., through paraphrase tests. (You'd be amazed at how confusedly elliptic this move will show our normal use of these concepts to be.) (3) Take some complete sentences using the concepts in question that you believe to be true, or at least can assume true for the sake of argument, and subject them to criticism. Do you really mean what they literally say, i.e., as a hostile lawyer would interpret them, and if not, what rewording of them is more literally defensible? What data or supplementary information would make it seem plausible that these statements are in fact true, and what further statements do they in turn make more credible? (That is, what are the inference relations that hold intuitively among propositions in this concept-system, and what logical forms and patterns of reasoning do these reflect?) What small changes in these sentences' wording (either in their grammar or in the particular terms used) make some difference in what they say, and what specific effect does that in turn have on their implications?

It would be highly impractical to subject most of one's ideation to this sort of analysis. Working through just one or two specific examples, simplified and idealized to avoid distraction from secondary complications, should suffice to reduce the noise in one's thinking on a given topic to a level which no longer masks the message-signal of genuine issues. Note, incidentally, that in this brief list of metathink tactics I did not speak of defining terms, which is the opening gambit in conventional concept criticism. Unfortunately, when psychologists proffer definitions, these are almost always explicit definitions of abstract nouns. But not only are our significant technical concepts usually theoretical notions whose meanings are "nomological network" roles not amenable to explicit definition, their cognitive muscle lies not in the abstract *nouns* we use for text-chapter titles but in the specific predicate components (especially verbs) which these nouns loosely categorize. Clarifying the meaning of those predicate components is indeed important, but to do so requires analyzing their behavior in grammatically complete sentences.

Although the subtleties of metathink reasoning are hard to develop succinctly in public print, I have persistently tried to sketch the process and exploit the product in my past writings. (See especially Rozeboom, 1960, 1961a, 1965, 1970, 1972a.) Unfortunately, I find it very difficult to get a hearing for such material from psychologists—the mainstream journals won't touch it, and psychonomic colleagues who read it out of politeness seldom comprehend its point. Of course, the discouraging reception my concept-analytic work has received might simply token inferior quality; but the occasional reader with some philosophical acumen seems usually to understand and appreciate it readily enough. Also, within the population of my own papers submitted to psychological journals (N = 10 at the time) I once computed a rank-order correlation of -.87 between my ease in getting them published and the innovative importance I considered them to have.

This communication barrier is why the title of my contribution to this symposium describes metathink as a "radical alternative" to traditional psychology. To call it that is absurd, as absurd as calling our mathematical tools an alternative. But that is how the psychonomic establishment insists on treating it.

There are perhaps three reasons why contemporary psychology is so rejective of concept analysis at the working level. One is that it smacks far too much of "methodology" (which to be sure it is), and "methodology" has acquired a bad taste from having seemed to put form before substance and to arrogate dogmatic constraints on what others are allowed to do. But concept methodology is no different from instrument methodology in that regard: Both only seek to do *well* whatever one chooses to do; and if study of method discloses that some intuitive practices simply don't work worth a damn, it is enough just to point this out and let their practitioners' own professional conscience take it from there. It is true that the behaviorist movement's middle period saw more than its share of intolerance for alternatives in the name of Method. But this should no more be held against concept analysis than Christian ethics can be blamed for the Spanish Inquisition. No human endeavor significant enough to attract adherents can avoid its fringe of bigots and extremists, whose caricature of the movement's ideals is then conveniently misconstrued as the real thing by equally intolerant outsiders.

A second reason why psychologists have become so refractory to metathink illumination is undoubtedly the wretched quality that has been the norm for our indigenous philosophy-of-science literature. Until recently our journals have carried a fair sprinkling of commentary on intervening variables, operationism, reductionism, and the like, usually at an abstract level far removed from specific substantive issues and unhappily, with extremely few exceptions, far too philosophically inept to do anything but confuse the issue further. We have no cause to feel shame for this material. It comprised honest efforts to meet genuine metatheoretic needs; technical philosophy of science on these topics was often not all that much superior at the time; and there is little profit in not trying at all. Just the same, to cope with today's torrent of publications, one must select ruthlessly to maximize expected personal payoff; and psychologists who haven't read more widely than our own literature have good sample-estimate reasons to expect little from papers of a "philosophical" turn.

Thirdly, I suggest that psychology's antipathy to concept criticism is most deeply a defense reaction in the service of ego survival. No one enjoys learning that his work contains even small blemishes, much less that he has made a royal muck of things. But even so, when a person has alternatives at his avail, if he can knowingly elect to do things one way rather than another, criticism of his method is no real threat-if his chosen tool proves to be less satisfactory than first expected, he can reappraise the situation and select a better one while remaining master of his

craft. For example, while it will grieve an experimenter to discover a confounding of factors in his research design, especially if he has already published data collected that way, working out appropriate revisions is routine for his job. He *wants* such defects in his work to be detected, even if it hurts, because he knows that they vitiate the epistemic merit of his conclusions. Psychonomic scientists have become sophisticated about research methodology, and can cope with problems of factor confounding, sampling bias, and the like, by thinking through design alternatives rather than by defensive refusal to admit that the problems exist at all. But if that same scientist has never tried his hand at metascience, and thinks intuitively as best he can in the only way he knows how, what is he to do when challenged that his thinking itself isn't all that it should be? He lacks metathink expertise to verify that this is so, and even if he did concede that his thinking might be defective in some respect, on what competences can he then fall back in order to improve it? Asking your journeyman scientist to consider revising not just isolated concepts but the manner in which he thinks them is like exposing an athlete to polio. It is a threat to his professional existence against which rejection antibodies are his sole defense. Of course the real menace of concept analysis is nowhere near that desperate; but I wonder if many psychologists don't suspect that it may be and can't afford to take chances.

Is there any hope for metathink in mainstream psychology? I still think so, even if not in my time or through my efforts. Sensitivity to how ideas work is already well developed in mathematics and analytic philosophy, and some of that appreciation cannot help but penetrate psychonomic science through its increasingly active contacts with those disciplines. (The join with mathematics is already well developed; with philosophy it is less so but I sense beginning to grow.) Once it becomes evident that concept engineering is not a parlor game for the boys who couldn't make the varsity team, but an advanced technical skill with real payoff in psychonomic applications, our profession will quite naturally internalize standards of excellence in this regard, just as it has already done for the methodology of research design.

I am sorry that time does not allow me to illustrate through analysis of fresh cases the gratuitous troubles we so often make for ourselves by uncritical mismanagement of our psychonomic verbal economy. But two examples developed at some length in my early work are: (*a*) the biased conventional phrasing of conditioning principles that inadvertently implies, quite without empirical support, that conditioned responding should blindly perseverate in altered circumstances where commonsense mentalism intuits an adaptive shift (Rozeboom, 1958); and (*b*) the standard behavior-theoretic "S-elicits-R" description of stimulus effects, which contains no linguistic provision for expressing the causal import of input *structure* and hence provides no means by which the potential complexities of behavioral regularities mediated, e.g., by cognition can even be conceived, much less stud-

ied in technical detail (Rozeboom, 1960, 1961a). The substantive issues in these cases, especially the latter, are major ones that require a modicum of conceptual hygiene to make accessible at all but cannot be mastered by that alone—which is one reason for saying no more about them here. A more superficial instance of slovenly concept management, that may or may not have impaired progress in behavior theory but in any case well illustrates several of my preceding abstract contentions, is our classic textbook definition of "learning."

For many decades right up to the present, with scarcely any significant exceptions, writer after writer has defined "learning" as *a relatively permanent change of behavior* brought about in some special way, usually "practice" or "experience." But that is just silly, for there are no "relatively permanent" changes of behavior. Unless an organism is dead, what he is doing changes from moment to moment in accord with the flux of his local environment; and even if we could arrange for an artificially stabilized stimulus variable to sustain persistence of some response, e.g., prolonged pupillary dilation brought about by prolonged exposure to darkness, we would still think of this as *elicitation* rather than learning. The retort I would expect this criticism to draw is an impatiently condescending, "Well, of course I don't mean *that*!" But if the definer doesn't mean what he literally says, then what does he mean, and why have two generations of textbook writers (with a few exceptions like Kimble) persisted in telling their readers something that isn't so? What this familiar pseudo-definition conceals is that the concept of "learning" really envisions change in some internal condition of the organism that is not directly observable like stimuli and responses but can only be inferred from the organism's local input/output regularities. (Most learning theorists have been at least dimly aware of this, but for many of an earlier era it was an embarrassment to their professed positivistic philosophy of science and hence not something to be acknowledged openly as an honest definition of "learning" would have required. For a more detailed development of this point, together with, I hope, some intimation of its methodological importance, see Rozeboom, 1965, pp. 338-344.) To bring out this and other basic logical properties of "learning," one should start not with this concept's abstract-noun embodiment but with its verb form in sentence-schemata like

John has learned _____

and then tease out what one construes to be a conceptually admissible way to fill the blank. This line of analysis also reveals considerable disparity between what virtually all technical theories of learning on one hand, and commonsense psychology on the other, respectively consider to be learnable. (E.g., "John has learned *a flashing-red-light → fear habit*" and "John has learned *a GUX-CEP association*" vs. "John has learned *that his sister has joined the police force*" and "John has

learned *how to memorize nonsense lists quickly.*") That is not necessarily a mark against extant learning theory, but it does surely deserve thoughtful discussion.

To conclude, I'd like to cite the two most important developments that concept analysis has sustained in my own work. The first is discovery that there exist determinate patterns of *explanatory induction* by which observed events inform us about their underlying sources (cf. Rozeboom, 1961b, 1972c). During my graduate research on the traditional "What is learned?" problem, comparing the formal structures of S-S vs. S-R interpretations of "latent learning" data led me to perceive that although such experiments cannot settle whether conditioned responding is mediated by cognitive expectancies, they can compel us to infer the involvement of a mediator—mentalistic idea, cue-produced response, or whatever—whose functional properties are those of an expectancy. Generalizing this discovery, I was then able to discern that in both everyday life and technical science, it is entirely common for data regularities to inductively disclose their own explanatory interpretations. If this finding can be sustained, its implications are enormous. It opens a whole new chapter in philosophic confirmation theory, superseding both the classic but demonstrably vacuous hypothetico-deductive model of scientific inference, and the radical empiricist denial that scientific explanation is possible at all, with a view of human reason that we can actually live by. It provides distinctive guidelines for the technical theory of data analysis with special support for recent developments in factorial decomposition of relational data. And it makes clear the importance of tying each distinctive conceptual feature of our serious substantive theories to some corresponding feature of the phenomena these purport to explain-a call for concept analysis in wholesale quantities. (Unhappily, unlike the Dionesian laxity of hypothetico-deductive theory construction, it also demands hard, disciplined work.)

The other main thrust of concept-analysis in my psychological work has been to search out the distinctive features of commonsense cognition theory and relate these to behavior phenomena which inductively require an internal explanatory mechanism essentially isomorphic to the classic mentalist account. Introspective intuition embodied in everyday language insists that mental events are paradigmatically of logical form

$$\text{person } o \text{ (at time } t) \ \phi\text{s that-}p,$$

in which that-p is a *proposition*—i.e., a cognitive meaning with the formal structure of a *sentence*—and ϕ characterizes the *mode* in which that proposition is being entertained. For example,

	$\underline{\phi s}$	$\underline{\text{that-}p}$
	observes	that it is raining.
John	remembers	that 29 is a prime number.
	doubts	that psychology is a natural science.
	hopes	that his licence is still valid.

in which all combinations of ϕs and that-ps are possible. What has traditionally seemed most distinctive about such "mental acts" is their *intentionality*, i.e., their generally being about something by virtue of their meanings. But whatever may be the nature of a cognition's intentional aboutness (and we do not yet have even the rudiments of a decent analysis of this—see Rozeboom, 1972b, pp. 55–59 and also my 1979 "On behavioral theories of reference" for criticisms of past psychonomic accounts, and 1972b, pp. 71f, for an incomplete outline of an approach that I hope will ultimately prove successful.), more psychonomically significant is its *logical structure*. The complex mental attribute, say, *believing-that-Mary-is-prettier-than- Jane-and-fearing-that-Jane-is-pregnant*, is intuitively much more than just simultaneous arousal of disconnected mental elements *belief, fear, Mary*-idea, *Jane*-idea, *prettier*-idea, and *pregnant*-idea, as can easily be sensed by permuting terms within this predicate. Yet what do we know technically about the psychonomic character of that something-more? We still need to diagnose specifically (1) what the pro-positional structure of a cognition contributes to its nomic force over and above the force of its constituent terms, and (2) what a cognition's mode-component does, and how. Until very recently, the psychonomic mechanisms envisioned by mainstream psychology have totally lacked internal-arousal stages more complex than what a simple list of terms can describe—though to be sure, the gestalt movement did its damnedest to get hold of the relational aspects of cognition and some intuitive albeit confused recognition of both mode and structure has been preserved by social psychology in attitude theory. Happily, explicit acknowledgement that people retain and process experience in *propositional* form has now begun to emerge in the tougher-minded psychonomic sectors like memory theory (notably, Anderson & Bower, 1973)—even if due more to the impact of the artificial-intelligence movement than to any preachings from the pulpit of indigenous psychology by the likes of Don Dulany (1968) and myself—and it may be expected that propositionally structured variables will soon become a psychonomic orthodoxy. Just the same, that change can be nothing but trendy, with little epistemic warrant, unless propositional structure can be shown to do a job in theories which postulate it that cannot be done just as well without it. That is, to justify this move we must be able to cite empirical phenomena which inductively demand an explanation with this special structure. For example, until we know how to diagnose which instances of animal behavior are mediated by ϕings-that-p, rather than by infra-cognitive mechanisms like associative chaining of sensori-motor elements, we cannot tell which instances of human behavior are properly interpreted

cognitively, either. It is scarcely conceivable that commonsense psychology would be so deeply committed to moded propositions as the basis of mental function if such phenomena do not in fact exist; but it still remains to make these psychonomically explicit. My own work on this (e.g., Rozeboom, 1967, 1969) is a start, but so far only a small one. Would that others could be induced to try as well.

References

Anderson, T. W., & Bower, G. H. (1973). *Human associative memory.* Washington, D.C.: H. V. Winston & Sons.

Dulany, D. E. (1968). Awareness, rules, and prepositional control: A confrontation with s-r behavior theory. In T. R. Dixon & D. Horton (Eds.), *Verbal behavior and general behavior theory.* Englewood Cliffs, N.J.: Prentice-Hall.

Rozeboom, W. W. (1958). "What is learned?"—an empirical enigma. *Psychological Review*, *65*, 22-33.

Rozeboom, W. W. (1960). Do stimuli elicit behavior?—a study in the logical foundations of behavioristics. *Philosophy of science*, *27*, 159–170.

Rozeboom, W. W. (1961a). Formal analysis and the language of behavior theory. In H. Feigl & G. Maxwell (Eds.), *Current issues in the philosophy of science.* New York: Holt, Rinehart, & Winston, Inc.

Rozeboom, W. W. (1961b). Ontological induction and the logical typology of scientific variables. *Philosophy of Science*, *28*, 337-377.

Rozeboom, W. W. (1965). The concept of memory. *Psychological Record*, *15*, 329–368.

Rozeboom, W. W. (1967). Conditioned generalization, cognitive set, and the structure of human learning. *Journal of Verbal Learning and Verbal Behavior*, *6*, 491–500.

Rozeboom, W. W. (1969). Compositional structure in recall. *Journal of Verbal Learning and Verbal Behavior*, *8*, 622–632.

Rozeboom, W. W. (1970). The art of metascience, or, What should a psychological theory be? In J. R. Royce (Ed.), *Toward unification in psychology.* Toronto: Toronto University Press.

Rozeboom, W. W. (1972a). Comments on professor Wilson's paper. In J. R. Royce & W. W. Rozeboom (Eds.), *The psychology of knowing.* New York: Gordon & Breach. (pp. 390–398)

Rozeboom, W. W. (1972b). Problems in the psycho-philosophy of knowledge. In J. R. Royce & W. W. Rozeboom (Eds.), *The psychology of knowing.* New York: Gordon & Breach.

Rozeboom, W. W. (1972c). Scientific inference: The myth and the reality. In R. S. Brown & D. J. Brenner (Eds.), *Science, psychology, and communi-*

cation: Essays honoring William Stephenson. New York: Teachers College Press.

Rozeboom, W. W. (1979). On behavioral theories of reference. *Philosophy of Science*, *46*, 175–203.