

NEEDS (MURRAY, 1938) AND STATE-VARIABLES (SKINNER, 1938)^{1,2}

PAUL E. MEEHL

University of Minnesota

Summary.—Skinner's concept of drive as a state-variable and his powerful rationale for introducing it agree closely with Murray's treatment of need. Operant behaviorists' usual depreciation of motivation in favor of stimulus control arises partly from features of parameters, insufficiently explored in some regions, of Skinner box research. For human adults on rich reinforcement schedules, response selection is chiefly controlled by the regnant motive. Skinner's life-long interest in inner events and translating psychodynamic concepts into behaviorese was obscured by his metalanguage philosophy of science (behaviorism).

It was my good fortune to be an undergraduate and graduate student at Minnesota (1938-1945) during B. F. Skinner's period on the faculty (1936-1945), and later my first job offer came from Skinner the year he began his chairmanship at Indiana. While a student, in addition to hearing his classroom lectures on verbal behavior, I spent many exciting hours of discussion in his office and was privileged to belong to the little group of TAs who met weekly evenings at his home (Skinner, 1979, p. 266). The only clinicians in that group were Howard Hunt and myself, both markedly (and permanently) influenced by Skinner but not "orthodox believers." I had come to psychology via the potent bibliotherapy of Karl Menninger's *The Human Mind* (1930; Meehl, 1989), and the impact of such a first-class intellect as Skinner on my Freudian commitments led to major cognitive dissonance. I did my best to resolve it, and that resolution, imperfect as it must of course be, is the subject of this paper.³

Skinner's *magnum opus*, *The Behavior of Organisms* (1938), appeared the same year as Henry A. Murray's *Explorations in Personality* (1938), and the psychodynamic clinical students at Minnesota freely spoke both languages, the latter under the influence of a young PhD, Robert E. Harris, and two students with MAs from Radcliffe. I suspect the Minnesota group was almost unique in this theoretical bilingualism. Even today, it is hard to conceive a solid-gold third-generation operant behaviorist and a corresponding representative of the Murray tradition engaging in meaningful communication; but since I do

¹ I am indebted to David Faust, Celia Gershenson, William Grove, David Lubinski, Richard Meisch, Bruce Overmier, Gail Peterson, and Travis Thompson for stimulation, advice, references, and helpful criticism of this paper in draft, although I have not always followed their advice, and they do not all agree with all of my conclusions.

² ~~Address reprint requests to Paul E. Meehl, University of Minnesota, Department of Psychology, N218 Elliott Hall, 75 East River Road, Minneapolis, MN 55455.~~

³ My psychotherapeutic orientation is now roughly one-third Freud, one-third Albert Ellis, and one-third Skinner. These fractions refer not (mainly) to subsets of patients, but to aspects of the same patient, and—via approximate translations—to the *same state or process* in a given patient. The term 'eclectic' means, for me, not an indiscriminate heap of thoughts and tactics (Schofield, 1988) but the organized result of conceptual parsing and integration by the therapist.

both, I am here going to explain how and why. *Motivation* is the variable selected, and Murray will serve as an adequate proxy for Freud, especially since I do not want my needs-defense-behavior analysis to be entangled with Freud's metapsychology. The latter could hardly be translated into Skinnerese, and I myself do not subscribe to much of it.

Some may consider this a feckless endeavor, it being "obvious" that the clash between behaviorism and psychodynamics is basic, pervasive, and irresolvable, involving issues of both substance and method. The glaring difference is in what are acceptable as explanatory entities: Murray invokes *inner causes*, whereas Skinner usually rejects such in favor of *external stimulus control*. In almost all his later writings, and increasingly over the years, Skinner castigates non-behaviorists (not only Freudians) for having recourse to postulated internal causes of behavior, labeling such pejoratively as "fictional," "unobservable," "lacking in physical dimension," "useless for control," "circular," "redundant," "tautological," "imaginary." He devoted one book, probably the most controversial he ever wrote, almost wholly to the omnipotency of external stimulus control ("schedules"), going so far as to say that if we admire a person's achievements we should bestow our praise not on the individual but on the shaping and maintaining environment (Skinner, 1971). A whole section of *Contingencies of Reinforcement* (1969, pp. 64-67) is devoted to criticizing the need concept. *About Behaviorism* (1974, pp. 164-166) has a section on "The uselessness of inner causes." Referring to powerful *theoretical constructs* (which inner states or events must be, for behavior science) in the *physical sciences* as "third-stage" concepts, Skinner writes:

There are few, if any, clear-cut examples of comparable third-stage concepts in psychology, and the crystal ball grows cloudy. But the importance of the stage is indicated by the fact that terms like *wants, faculties, attitudes, drives, ideas, interests, and capacities* properly belong there. When it is possible to complete a theoretical analysis at this stage, concepts of this sort will be put in good scientific order. This will have the effect of establishing them in their own right. At present they need external support. Some of them, like wants and attitudes, come to us trailing clouds of psychic glory, and a wisp or two of the psychic can usually be detected when they are used (1961, p. 235).

In traditional terms an organism drinks because it *needs* water, goes for a walk because it *needs* exercise, breathes more rapidly and deeply because it *wants* air, and eats ravenously because of the promptings of *hunger*. Needs, wants, and hungers, are good examples of the inner causes. ... Sometimes the inner operation is inferred from the operation responsible for the strength of the behavior ... it is sometimes inferred from the behavior itself.... So long as the inner event is inferred, it is in no sense an explanation of the behavior and adds nothing to a functional account (1953, pp. 143-144).

The commonest inner causes have no specific dimensions at all, either neurological or psychic. When we say that a man eats *because* he is hungry, smokes a great deal *because* he has the tobacco habit, fights *because* of the instinct of pugnacity, behaves brilliantly *because* of his intelligence, or plays the piano well *because* of his musical ability, we seem to be referring to causes. But on analysis these phrases prove to be merely redundant descriptions (1953, p. 31).

The practice of looking inside the organism for an explanation of behavior has tended to obscure the variables which are immediately available for a scientific analysis. These variables lie outside the organism, in its immediate environment and in its environmental history ... such terms as "hunger," "habit," and "intelligence" convert what are essentially the properties of a process or relation into what appear to be things. Thus we are unprepared for

the properties eventually to be discovered in the behavior itself and continue to look for something which may not exist ... (1953, p. 31).

In ontogenic behavior we no longer say that a given set of environmental conditions first gives rise to an inner state which the organism then expresses or resolves by behaving in a given way. We no longer represent relations among emotional and motivational variables as relations among such states, as in saying that hunger overcomes fear. We no longer use dynamic analogies or metaphors, as in explaining sudden action as the overflow or bursting out of dammed-up needs or drives (1961, p. 182-183).

Piling up additional excerpts that reveal this outer-over-inner emphasis would be superfluous; readers can find scores of them in any systematic treatment of Skinner's position. The message conveyed by these passages is loud and clear: reference to inner causes (states, events) should be avoided by the behavioral scientist, being at best redundant and at worst counterproductive, obscuring the real, effective controlling variables and deflecting us from focusing research attention on them.

But matters are not quite so simple as these "anti-innards" passages collectively suggest. One may assume that in a paper devoted to the topic of operational method in psychology, Skinner would have been especially careful in formulating his position respecting inner processes generically. In the 1945 operationism symposium (Skinner, 1945; see also my comments on that paper, Meehl, 1984, and his reply, Skinner, 1984) he explains—in one of the cleverest pieces he ever wrote—how it is possible for the verbal community to reinforce verbal operants tacting inner ("private," as Boring [1945] called them) events, despite their public inaccessibility. He then shows that the four modes of setting up such discriminations are unavoidably stochastic and approximative, hence the relatively poor accuracy of introspective reports. But he insists, against Boring's "methodological behaviorism" (which asserts there *are* private events but that they cannot be part of science), that a radical, consistent behaviorism *can*, and *must*, deal with them. In that symposium, Skinner's reply to Boring includes the famous witticism that behaviorist Skinner can be "interested in ... Boring-from-within." In "Behaviorism at 50" (1963) Skinner characterizes "methodological" behaviorism's refusal to include inner events as an unwise strategy. In *About Behaviorism* (1974) he says "radical behaviorism ... can ... consider events taking place in the private world within the skin" (p.16), and that it is incorrect that it "ignores consciousness, feelings, and states of mind" or that it "has no place for intention or purpose" (p.4).

The objection to inner states is not that they do not exist, but that they are not relevant in a functional analysis. We cannot account for the behavior of any system while staying wholly inside it; eventually we must turn to forces operating upon the organism from without. Unless there is a weak spot in our causal chain so that the second link is not lawfully determined by the first, or the third by the second, then the first and third links must be lawfully related. If we must always go back beyond the second link for prediction and control, we may avoid many tiresome and exhausting digressions by examining the third link as a function of the first. Valid information about the second link may throw light upon this relationship but can in no way alter it (1953, p. 35).

With respect to *drive* as a mediating state-variable, I am unaware of any recantation of the *Behavior of Organisms* analysis, in which two whole chapters are devoted to that concept. I do not dispute a shift in quantitative emphasis over time; I merely insist that we find no qualitative retraction of the drive *concept*, and in *Science and Human*

Behavior (1953) we find a whole chapter devoted to it (pp. 141ff), although entitled “deprivation and satiation”—denoting the input operations—rather than “drive,” the state. Furthermore, in his later writings the same basic rationale as was offered in 1938 for introducing drive (also the other state-variable emotion) is presented, substantially unchanged.

[A “drive”] is simply a convenient way of referring to the effects of deprivation and satiation and of other operations which alter the probability of behavior in more or less the same way. It is convenient because it enables us to deal with many cases at once. There are many ways of changing the probability that an organism will eat; at the same time, a single kind of deprivation strengthens many kinds of behavior. The concept of hunger as a drive brings these various relations together in a single term.... A drive is a verbal device with which we account for a state of strength ... (1953, p. 144).

There is nothing wrong with an inner explanation as such, but events which are located inside a system are likely to be difficult to observe....

Eventually a science of the nervous system based upon direct observation rather than inference will describe the neural states and events which immediately precede instances of behavior. We shall know the precise neurological conditions which immediately precede, say, the response, “No, thank you.” These events in turn will be found to be preceded by other neurological events, and these in turn by others. This series will lead us back to events outside the nervous system and, eventually, outside the organism (1953, pp. 27-28).

Given our Murray/Skinner theme and the fact that Freud’s theory is obviously one of the most “inner”-oriented (and mentalistic) theories, Skinner’s attitude to Freud is of special relevance. Here again we find paradoxes (I do not say *contradictions*, for reasons given below). On the very first page of *Behavior of Organisms* appears the interesting classification of Freud’s ego, super-ego and id as concepts “which have remained the subject of scientific investigation” (1938, p. 3). True, this surprising remark occurs in the context of criticizing our failure to develop a science of behavior at its own level. Nevertheless, Freud’s kind of “inner organism,” Skinner says, has “become in turn the subject matter of a science,” unlike the freely willing inner organism, or the vague *self*, of the man on the street. I can only interpret his position as being that Freud’s theory is science, but rather poor science, like the science which invokes the “conceptual nervous system (C.N.S.)” (1938, pp. 4, 421). In *Cumulative Record* (1961, p. 243) Skinner goes so far as to say of Freud (paraphrasing the economists on Keynes), “Certain general points have been made—in some sense we are all Freudians—but the facts and principles which have been salvaged can be stated in relatively non-technical language.” The chapter on psychotherapy in *Science and Human Behavior* offers behavioral translations of several defense mechanisms, plus brief accounts of wit, dreams, and parapraxes (1953, pp. 376-378). In that book we find (p. 293) a truly astonishing concession to Freud, about dream interpretation: “Freud could demonstrate [!] certain plausible relations between dreams and variables in the life of the individual. The present analysis is in essential agreement with his interpretation.” As a 33% Freudian who interprets dreams (see, e.g., Meehl, 1983) but who is also semi-Popperian in metatheory, I would never use such a strong verb as ‘demonstrate’ for what Freud accomplished.

Skinner took Freud seriously. I can supplement textual evidence with many clear recollections from personal contact during Skinner’s Minnesota period. He was fascinated by “Freudian slips” (he called them that, unabashedly) and had collected an

amusing batch of good ones to illustrate *thematic strengthening* of verbal operants in his verbal behavior class. Queried by me as to their being “unconscious,” he didn’t bat an eye, responding quickly, “Of course, there’s no law that a human can always tact the variables of which his behavior is a function.” Meehl: “Surely not. But Freud’s ‘unconscious’ means more than that. Suppose one has learned to tact such a variable, inner or outer, but avoids doing so ‘in the interest of the defense,’ as we say. *That’s* the kind of unconscious we clinicians care about—which differs from a ball-player’s inability to report on how his brain does a numerical integration of the baseball’s path. He never learned calculus. But the neurotic has learned to tact mother, father, and inner states, but doesn’t *now* do it, defensively.” Skinner: “Yes, that’s a more interesting case, requiring a more complex analysis. It’s a case of aversive control. What you call ‘defense’—and I have no objection to your term, as long as we’re quite clear what is being described by it—is either a case of the (expected, otherwise available) tacting operant being kept at low strength by punishment, or an incompatible operant—your ‘defensive maneuver’—being at high strength because it has a history of negative reinforcement (escape or avoidance).” This kind of dialogue occurred regularly between Skinner and the psychodynamic clinicians. In fact, I often found it easier to discuss Freud’s concepts with Skinner than with my strongly anti-Freudian doctoral advisor Starke Hathaway. The fact is that Skinner was very much interested in Freud, to the point of applying as a candidate for psychoanalytic training at the Boston Psychoanalytic Institute in the late 1940s. I shall charitably refrain from comment on their refusal. If the arch-behaviorist, with his intellect, had had some reinforcing couch time (given an analyst not defensive, doctrinaire, or dumb), who knows how both disciplines might have been benefited?

I do not argue that Murray and Skinner have no real differences, that it’s “merely semantics,” nor that everything Murray (or Freud) asserts can be translated without loss of meaning into behaviorese. (Translation in the other direction is never discussed, an interesting fact in itself.) What I do argue is (a) that there is more adequate translatability than is generally thought; (b) the surplus meaning—while theoretically interesting—is usually of slight clinical importance; and (c) the constraints Skinner imposes that prevent more complete translation are metatheoretical, reflecting his behaviorist philosophy more than they do scientific substance. I permit myself this distinction, relying on the opening sentence of *About Behaviorism* (1974, p. 3): “Behaviorism is not the science of human behavior; *it is the philosophy of that science* [italics added].” In the language of logicians, I might express the point by saying that cognitive collision occurs between Murray’s object language statements and certain metalinguistic proscriptions by Skinner that attempt to impose his philosophy of science on others. The crucial distinction here is between the modest line, “My conspicuous technological success—as shown by these object language generalizations—leads me to advocate such-and-such a research strategy as conducive to success,” and a more dogmatic triumphalist line, “Whatever your theoretical interests may be, you are *mistaken*, you sin against *scientific method*, if you allow yourself conjectures outside the class of constructs to which my strategy restricts itself.” It is, of course, this second negative, forbidding, exclusionary kind of statement that arouses the ire of psychologists who want to study cognitions, traits, psychodynamisms, and social institutions.

In reconciling the paradoxical quotations above, I adopt the working assumption that Skinner is, with rare exceptions, consistent. I would say he was one of the most consistent

thinkers I have ever known. (Our philosopher friend Herbert Feigl used to say, “Fred has a behaviorist Midas touch; everything psychological he touches turns into behavior.”) But I suggest that he sometimes taketh away with the (metalinguistic) left hand what the (object language, “translating”) right hand giveth. I consider *needs* and *state-variables* a good example. This is a conceptual analysis, not a review of empirical literature. I examine (as was Skinner’s practice) simple cases, recognizing that even for “simple” animal drives, such as hunger, matters have become quite complicated. I have to rely on many hours of clarifying conversation with Skinner for reassurance that my textual interpretation is not Meehl’s tendentious eisegesis. Wilson’s (1959, p. 532) Principle of Charity—that one should assume an author does not contradict himself—and the theologians’ hermeneutic principle, “Scripture interprets Scripture,” are in order here. My question, then: What is the relation between a Murray need and a Skinner state-variable?

The *locus classicus* for Skinner on state-variables is *Behavior of Organisms* (pp. 22-25). I doubt that anyone has ever set out such a clear, concise, and compelling rationale for introducing them. One thesis of this paper is that the later Skinner never rebutted these powerful arguments in favor of motivational variables. Consider his paradigm example, the hunger drive. The basic observation, having once discovered the conditioning and extinction processes, is that the strength of the “reflex” (as he then called both operants and respondents) may exhibit marked variability without further elicitations (i.e., no opportunity for extinction, reconditioning, discrimination, or shaping). The variable strength is found to be controlled by the operations of “feeding and fasting,” which take place in the home cage, where neither the discriminative stimuli (light, buzzer) nor the manipulandum (lever) are available. Some “state” of the rat is induced by these extra-box operations, and we call that state “the hunger drive.” This one kind of datum would suffice to warrant introducing a state-variable into our behavioral concepts. However, it turns out to be associated with another striking fact, namely, a *set* of reflexes, perhaps widely different in effector topography or controlling stimuli, covary with the state of hunger. It would not puzzle us that hard and soft pressing covary, or that pressing in the presence of two similar light hues covaries. This is “induction,” based on S or R similarity. But what if turning right in a T-maze, pulling a chain, lever pressing, and standing up all covary strongly? If independent investigation shows these behaviors do not possess sufficient mutual induction to explain their covariation, something else must be going on. The “something else” is, of course, that all of them have been conditioned employing food as a reinforcer. This means that some covariations—those not attributable to topography or stimulus equivalence—must be referred to the *conditioning history* (which could “go back” years), considered jointly with the *recent deprivation* and the *current stimulation* (in the box). Further investigation reveals that deprivation time since eating to satiety, or eating a fixed amount, are less effective controllers of hunger than feedings adjusted so as to maintain the rat at a fraction (say, 80%) of its *ad lib.* weight. Then we note that a variety of dissimilar operations, not involving deprivation, may influence drive (see, e.g., the list of thirst-manipulators in Skinner, 1953, p. 32). It turns out that even momentary exteroceptive stimulation can arouse or increase “drive,” as when the sight of a fancy dessert “stimulates the appetite” following a heavy meal. This is stimulus control, but not operating in quite the same way as a non-drive-mediated S^D . Since the research of Frank Beach and others (see, e.g., Ford & Beach, 1952), psychologists realize how much the sex drive is stimulus controlled; we know now that

“distended seminal vesicles” due to deprivation are no more the basis of sexual motivation than “stomach contractions” are the core of hunger. (I once asked a Benedictine monk at St. John’s University how he dealt with the attractive female secretaries, and his reply was “Simple—one learns not to look.”) For the various complicated relations that arise, which for extralaboratory cases often render the analysis largely programmatic, see Skinner (1953, Chapters 9, 14, *et passim*) and Reynolds (1975).

Where does this leave us in examining the relation between Murray needs and Skinner state-variables? I shall give primacy to *Behavior of Organisms* over some (not all!) of the later writings and try to avoid what appear to be inconsistencies by parsing theoretical object-language and metatheoretical dicta. Of course, Skinner may at times be literally inconsistent or change his mind without so announcing. (I suspect all daring, creative Grand Theorists fall into inconsistencies. Einstein down-played the corroborative 1919 eclipse in favor of the beauty and armchair plausibility of his theory; yet he said that if the red-shift did not occur, his theory would be dead. But I am applying Wilson’s Charity Principle, avoiding seeing contradictions whenever possible.) Putting *Behavior of Organisms*, *Science and Human Behavior*, and other writings together, we distill the following:

1. Motivational variables control families of operants and respondents, which may thereby covary in strength despite negligible overlap in their (stimulus and/or response) properties.

2. Motivational variables are controlled by various operations, including deprivation, current stimulation, and various physiological interventions; while the paradigm experimental “drives” (thirst and hunger) are customarily manipulated via deprivation, motivational states differ in the extent of their “spontaneous growth” versus exteroceptively controlled excitation.

3. The main determiner of motivational operant covariation is historical. Those operants which have been strengthened, shaped, and brought under discriminative control by food reinforcement will tend to covary with hunger, and hence with operations that influence hunger.

4. In the real physical causal chain, the motivational variable is a state of the organism—hence, an “inner condition,” the immediate causal ancestor of the changes in operant strength.

5. Despite its undoubted existence inside the organism, to control the operant via the motivational variable we must manipulate it by external operations (e.g., feeding and fasting). Insofar as our aim is control, a research strategy emphasizing reinforcement schedules, deprivation, discriminative stimuli, and drive conditioning is preferable to a strategy that focuses attention on the inaccessible state-variable itself.

Note that (1) through (4) are empirical generalizations belonging to the science of behavior, whereas (5) is a metatheoretical move, of course *influenced* by that science’s success, but, in its content, part of what Skinner calls his philosophy of science, i.e., behaviorism. (A philosopher would classify it as belonging to the realm of pragmatics. It may be *suggested* by the facts, but its *content* is a kind of metatheoretical advice.)

Aversive control is of special importance in psychodynamics and psychotherapy, as Skinner points out (1953, pp. 359-383). We could almost define neurosis in terms of appetitive/aversive balance—a behaviorist (partial) translation of Freud’s “economic” generalization that the neurotic’s ego suffers a depletion of psychic energy because so

much must be expended in warding off anxiety by means of the rigid and hyperalert defensive system. Aversive control is based on the contingencies of negative reinforcement. A negative reinforcer is defined by the fact that its removal (or, later, avoidance) exerts the usual family of reinforcing effects, i.e., it strengthens, shapes, and chains operants, and establishes their discriminative controls. In addition to this “operational” *defining* property, negative reinforcers typically possess several other efficacies as well. Their contingent presentation depresses appetitively controlled operants, the effect commonly called punishment. They may elicit certain “emotional” respondents, chiefly (autonomically mediated) smooth muscular and glandular respondents. They can confer positive and negative reinforcing power on neutral stimuli. Of particular relevance for our discussion is that *noncontingent* presentation of a negative reinforcer also exerts a depressing effect on positively maintained operants. This interesting property at least suggests (as it did to Estes and Skinner, 1941) as a sixth property the elicitation of an aversive state-variable, commonly labeled ‘anxiety.’

To forestall misunderstanding, let me emphasize that I am stating Skinner’s 1938 position, which he continued to hold in its essentials, for the purpose of relating it to Murray’s views of the same year. I am not here attempting to summarize, let alone explain, the vast body of experimental research bearing on—and importantly qualifying—these basic postulates. For example, a paradoxical strengthening effect of a normally “punishing” stimulus (foot shock) was discovered over half a century ago (Muenzinger, 1934) in the old discrimination apparatus, and later independently re-discovered in the Skinner box (Kelleher & Morse, 1964). The parametric complexities arising here are so great as to defy any simple categorization of stimulus events or even of highly general second-order functional relationships determining their reinforcing properties (cf. Barrett & Katz, 1981). But I conjecture that parametric modifications of the simple positive/negative reinforcer dichotomy may make the above list of principles, as amended, more compatible with certain concepts in psychodynamics (see, e.g., Freud 1920/1955; Meehl, 1990a; Menninger, 1938; Rado, 1956; Reik, 1941; Sincoff, 1990).

Similarly, on the positive reinforcement side, I do not discuss Premack’s differential probability principle (1959; and see, e.g., Staddon & Ettinger, 1989), Herrnstein’s matching principle (1961; and see, e.g., Staddon & Ettinger, 1989), Nevin’s behavioral momentum (Nevin, Mandell, & Atak, 1983), the conservation or molar equilibrium principle, or the various “economic” formulations of behavior choice (Allison, 1979, 1983; Allison, Miller, & Wozny, 1979; Collier, Johnson, Hill, & Kaufman, 1986; Mazur, 1979; Rachlin, 1989; Staddon, 1979a, 1979b, 1979c; Staddon & Ettinger, 1989; Timberlake, 1980). Whether these developments affect a Murray-Skinner rapprochement is unclear, although my hunch is they would facilitate more than impede (partly because of Murray’s teleological “adaptive” emphasis, anathema to Skinner). As Staddon points out (1979a), Skinner was not enthusiastic about formal approaches to operant reinforcement principles (Skinner, 1961, pp. 250-252; 1966, pp. 27-29).⁴

⁴ Despite my disclaimers, a reviewer complains that I do not deal with the developments in operant behavior analysis in recent years. Although I have not run a rat since the 1960s, and do not monitor that literature, I am of course aware that much has happened by way of expansion, amendment, detail, and application. But this paper (as its title clearly indicates) is *about* the conflicting, polarizing views of two eminent thinkers as set forth in their classic works over a half-century ago. I believe psychologists’ perspectives can be improved by reflecting upon the history of ideas (e.g., it may save us from reinventing the

One eager to discern—or force—a conceptual Murray-Skinner rapprochement is tempted to interject here: “*Well*, that’s better than I had thought of arch-behaviorist Skinner. The whole business turns out to be teleological after all, concepts saturated with purpose throughout.⁵ It looks as if Skinner’s distaste for teleological, goal-directed formulation is, again, mostly in the metatheoretical commentary, merely his behaviorist prejudice.” While this is a plausible gloss on the text, it is terribly mistaken. The element of teleology in Skinner is wholly derivative, applying simple probability theory to the basic concept, *contingent reinforcement*. I have not found a single instance of Skinner’s invoking “aim,” “goal,” “purpose,” “utility,” “adaptation,” “adjustment,” “directedness,” “least action,” or similar concepts to explain an experimental fact. The only context in which the adaptiveness of a behavioral law gets mentioned is phylogenetic, i.e., a second-order biological characterization of the behavior laws and an explanation of their origin *in the species*. The “adaptive value” of a behavioral law is not part of the law and is only stochastic, so one does not attempt—wrongly—to predict or control an individual organism/situation outcome by invoking adaptive categories. This is one of the merits of Skinner’s system, since it avoids the pseudopuzzles of maladaptive behavior, which abound both in the “normal” and pathological realms. Much neurotic behavior finds easy explanation via reinforcement contingencies, so we are not continually being baffled by counterproductive instances, tempting non-Skinnerian clinicians to hypothesize self-destructiveness, wish to be ill, moral masochism, fear of success, let alone Freud’s mythical “death instinct.” Already in the 1930s, Skinner showed that a rat will starve to death, with food available, if shifted directly from CRF to FR192, a ratio adequate calorically and attainable when approached gradually. If we “think Skinnerian,” we are not tempted to explain this as *thanatos* at work (cf. Meehl, 1962).

Setting aside some experimental niceties, paradoxes, and complex cases, our knowledge situation can be formulated in its essentials thus: a state-variable (e.g., *hunger*) is introduced to deal with four empirical findings: (1) response strength exhibits marked variability apart from in-the-box operations of reinforcement or extinction; (2) this variation is controllable by extrabox operations (feeding and fasting) not involving the operant; (3) the substance, object, event, or activity whose deprivation controls strength is that which reinforced the operant (increased its strength, shaped its topography, brought it under discriminative control, linked it in a chain, etc.); (4) a family of operants differing in stimulus and response sides will covary if the deprivation schedule involves a shared reinforcer, i.e., a common *behavioral history*. These four bases for introducing a state-variable also function jointly to *identify* it; they provide its *content*, so to speak. The characterization of a “drive” involves a 4-term conceptual structure whose components are response, discriminative and reinforcing stimuli, and deprivation. Allowing ourselves to speak in the vernacular, we could say, “In a certain situation the organism wants something, does something, and gets what it wants.” Describing these “somethings” in

wheel every generation) in addition to the intrinsic scholarly interest of such a pursuit. Furthermore, except for abandoning the reflex reserve and down-playing (*not eliminating*) state-variables, Skinner retained the core ideas of his 1938 book in all subsequent writings; the same is true of Murray and his position. I am not here addressing the opinions of their second and third generation followers, or evaluating their merits.

⁵ Today’s students are amazed when one informs them that Skinner considered his “system” to be more akin to Tolman’s (1932) than to any other (Skinner, 1938, p.437; personal communication, 1943). They shouldn’t be.

their concurrent and historical relations specifies a motive. This is why Tolman (1932) said that purpose is immanent in behavior.

SOME HELP FROM THE PHILOSOPHERS

To his credit, Skinner never employed “official” philosophy of science as a bludgeon against substantive opponents, a polemical vice to which some psychologists—disciples of Hull, Spence, Tolman, and even Freud—were prone. In fact, he took a rather dim view of the philosophers’ contribution to psychology, holding that both it and the Fisher design-of-experiments emphasis were unhelpful to the behavioral scientist, if not harmful. In reviewing Smith’s *Behaviorism and Logical Positivism* (1986), Skinner (1987a) agreed with the main thesis that the influence of logical positivism on the development of American behaviorism was slight, contrary to the received opinion. The logical positivism of the early 1940s had liberalized itself to such an extent that in the Minnesota discussions Feigl and I would often be defending the hypothetical constructs of Hull, Freud, and the factor analysts against Skinner’s disapproval of them in favor of an astringent “pure intervening variable” approach (cf. MacCorquodale & Meehl, 1948). The only quasi-philosophical concept Skinner relied on was operationism, which he carefully defined for behavioral science thus: “Operationism may be defined as the practice of talking about (1) one’s observations, (2) the manipulative and calculational procedures involved in making them, (3) the logical and mathematical steps which intervene between earlier and later statements, and (4) *nothing else*” (Skinner, 1945). Whether that methodological prescription comports with his insistence—against Boring’s contribution to the symposium—on internal states and events being included in behavior science, I do not discuss (but see my query about it [Meehl, 1984] and his reply, “I pass” [Skinner, 1984]).

It is fortunate for Skinner’s position that he carefully refrained from reliance on “general philosophy of science,” because by 1950 (at the latest) one could hardly find a competent logician, philosopher, or historian of science who subscribed to operationism in anything like Bridgeman’s 1927 formulation, which Bridgeman himself had radically revised years later (1936). Skinner was fully aware of the extent to which other sciences (astronomy, physics, chemistry, genetics) employ hypothetical constructs to advantage. But he also knew that at certain stages of their development they did quite well at a descriptive level. Chemistry flourished, using Mendeleev’s table, before valence—a dispositional concept—was explained in terms of completing electron shells; and the electron itself, while a hypothetical construct in the philosopher’s eyes, was almost “operationally defined” by the experiments of another science (physics) quite apart from chemical combining properties. Phenomenological thermodynamics was a powerful quantitative enterprise before its explanatory “reduction” to statistical mechanics. Population genetics was a rich quantitative science before Watson and Crick. Although the gene—earlier, merely a “factor”—is, strictly speaking, a theoretical construct, it was very closely tied to direct cytological observation via the statistics of trait linkage (cf. geneticist Snyder’s 1951 arguments, listed by Meehl, 1990b, pp.25-27). Genetics provides a nice example for Skinner’s position because it illustrates (pre-Watson and Crick) how an obvious mapping of concepts at two levels in Comte’s Pyramid of

Sciences⁶ can occur. It requires no forcing, and little conjecture, when the observational evidence is in. Psychologists have often misunderstood Skinner's view about the nervous system, which is hard to excuse because he has stated it clearly from his doctoral dissertation (Skinner, 1931) on. I know from many hours of discussion exactly how he saw it. The replicable generalizations of behavioral science stand on their own feet. In fact, they *constrain* the neurophysiologist's theorizing, since a brain-theory incapable of explaining behavioral laws must have something wrong with it. (That this argument applies in the reverse direction, although in a more complicated way, never apparently occurred to him; but I pass that here.) At an advanced stage of development of both behavior science and neurophysiology, each having been pursued "at its own level," the presumed isomorphism between the two sets of concepts will become obvious. That scheme is very like what happened in the identification of Mendelian factor = gene at a chromosomal locus = cistron as an ordered sequence of codons.

When Skinner defines behaviorism as the philosophy of science for a science of behavior, he is not *deriving* it from a general philosophy of science, a procedure he knows cannot be carried through, for the simple reason that general philosophy of science does not prescribe strict operationism and does not proscribe hypothetical constructs. On the current philosophical scene, it is recognized that we need to distinguish *general* philosophy of science from *special* philosophy of science, the latter suited for analyzing and reconstructing particular disciplines (Stegmüller, 1979). My suggestion is to consider Skinner's behaviorism as a *research strategy for those interested in controlling behavior*, a kind of "methodological advice" given such-and-such specified aims. The proscriptive component of his meta-discourse, which admittedly sounds quite forbidding at times, I charitably construe as, roughly, "We are in the stage comparable to phenomenological thermodynamics, population genetics, Mendeleev's Table; for the foreseeable future, the best strategy is to analyze our subject matter at its own level of description, with minimal use of hypothetical constructs, whether mentalistic or neurophysiological." That this is a fair reading is suggested by Skinner's classification of Freud's "psychic institutions" (ego, super-ego, id) as scientific explanations of behavior, which astonishes those young operant behaviorists who have never read *Behavior of Organisms* (Skinner, 1938, p. 1). How can we reconcile this freely volunteered concession with his strong distaste for such constructs? On my reading, easily. Freud's psychic institutions are not excluded by any general "rules of scientific method." They are just not good science; they are like phlogiston rather than valence.

My friendly disagreements with Skinner over half a century include the value of philosophy of science, which I prefer to call metatheory, given the current view that it is the empirical theory of scientific theorizing, a branch of social science (see logician Sneed, 1976, 1979). Its being an empirical theory, whose data consist of accepted *and abandoned* scientific theories and episodes in their history, does not, of course, preclude inclusion of logic, mathematics, statistics, and even "armchair epistemology" in meta-

⁶ Auguste Comte (1788-1857), inventor of the terms 'positivism' and 'sociology,' envisaged a pyramid of the sciences with physical sciences as the base, sociology as the apex, and biology in the middle layer. While not a thoroughgoing reductionist, he emphasized dependence of the concepts and laws of one "level" upon the sciences below it (e.g., one cannot fully understand physiology without chemistry). See Comte (1830-42/1974, 1830-54/1983), Oldroyd (1986, Chapter 5), and, for a strong exemplar in genetics, Meehl (1990b).

theoretical reconstruction. (Armchair epistemology—of Locke and Hume, and even continental rationalists Descartes and Kant—is in fact empirical, despite its not being experimental or quantitative.) Theoretical concepts in the developed, successful sciences are rarely defined operationally, by direct linkage to observational predicates or functors. A proper subset are so specified, and it is these ties to observation that make the theory testable. In the older language of logical empiricism, they are variously labeled “operational definitions,” “coordinating definitions,” “meaning postulates,” “reduction sentences,” “bridge laws.” (Whether these last should be considered pure stipulations or theoretically derivable theorems is an open question, which need not be decided for our purposes.) The subset of theoretical constructs not operationally defined are said to be *implicitly* defined via their role in the network of theoretical statements. In the received view of logical empiricism, their empirical content derives from their (indirect) linkage, through derivation chains, to observational statements. They were said to be “partially interpreted” that way. In the early days of the Minnesota Center for Philosophy of Science, I christened this the *upward seepage* theory of empirical meaning, and argued against it, as I think most philosophers would today. The reason is that many (most?) theoretical terms derive part of their content from an interpretative text that is not operational (cf. Meehl, 1990a, 1990b). Even in the case of simple, close-to-data theories, implicit definitions present a problem. If there are two or more theory-to-fact linkages (as in Carnap’s classic reduction sentences [1936-37]), what were intended as alternative meaning-stipulations yield, considered jointly, a factual consequence. If, say, hunger is implicitly defined by Bousfield’s voracity coefficient (Skinner, 1938, p. 351) and by rate of lever pressing, what if the two indicators disagree? Our little two-postulate pure dispositional “theory” has generated a contradiction in the presence of the observational facts. There are two ways around that (although we could most simply elect to say that the theory has turned out to be false). Taking a purely intervening variable approach, we could delete either stipulation, retaining the other. Say we retain voracity as our hunger measure. Then we can have a revised rate statement, only stochastic in claim; or we say “*ceteris paribus*,” expecting the rate statement to hold sometimes but not always; or we abandon the rate statement entirely. The *ceteris paribus* strategy immediately suggests a research program to identify the interfering variables. Skinner (1938, p. 25) examines a simple case of this, dichotomizing the three relevant factors conditioned/extinguished, hungry/satiated, afraid/unafraid, to give eight combinations, seven of which yield failure to press the lever.

Skinner and his orthodox disciples have a strong methodological preference for intervening variables over hypothetical constructs, and Skinner’s system comes as close to being a pure intervening variable theory as any I know of, with the possible exception of some (I think inconsistent) formulations in classical psychometrics. Even when an unobserved state or event is invoked for explanatory purposes, it is almost always either *observable* (e.g., a competing response, such as scratching a flea, that the apparatus does not record) or a neurophysiological process fairly directly observable in that discipline and known—not speculatively—to be highly correlated with observable behavior (e.g., proprioception as part of the S^D that underlies the autocatalytic effect of rapid responding on FR schedules). This preference for intervening variables conjoined with Skinner’s aseptic operationism impels one to offer strict, simple definitions of each core concept.

But there is a price paid for this that can produce oddities. For example, if any concept is “core” (Lakatos, 1970, 1974, 1978; Meehl, 1990a, 1990b) to the system, it is surely *reinforcement*. The purported operational definition (repeated early, almost ritualistically, in almost all articles or book chapters by operant behaviorists) is the well known increase in operant strength. That is offered as an explicit definition of the concept, and the fact that reinforcers have other interesting and important powers is regularly treated as a (nondefinitional) empirical finding. Over a quarter-century ago, my colleague Roy Pickens complained that a referee insisted the term ‘reinforcement’ be stricken from a paper on effects of cocaine self-administration in the rat, on the ground that the short term effect was a *decrease* in rate below the unconditioned operant level (see Pickens & Thompson, 1968). I could not resist needling him a bit, “Roy, those hyper-operational pigeons have come home to roost,” which he took in good spirit. This amusing episode illustrates an important methodological point, relevant to our analysis. Reinforcement as an operation exerts a *related family of effects*, short-term and long-term, that include strengthening the operant, shaping it, bringing it under discriminative stimulus control, conferring the reinforcing property on discriminative stimuli, chaining, eliciting biologically relevant respondents (Pavlov’s salivating dog!), countervailing aversive stimuli (e.g., Brady & Conrad, 1960; Olds, 1959), and so on. The increase in strength is the obvious “big” effect, and no doubt deserves to be considered a *privileged* indicator; but the Pickens paradox and semantic hassle can be liquidated by avoiding a strict single-property operational definition and substituting an *open concept* specification of meaning (Pap, 1953, 1958, Chapter 11; Meehl, 1972, p. 21 [1973, p. 195] and references cited therein, 1978, pp. 815-816). With that approach, if cocaine does some of the other things in the “reinforcement family,” we do not scruple to label it a reinforcer, despite the anomaly of decreased rate. In the philosophers’ metatheory of open concepts, a member of the indicator set may be privileged (i.e., receive a heavier weight) but can sometimes be outweighed by some subsets of the others. While the metatheory of open concepts can be treated purely as a statistical problem (cf. Meehl & Golden, 1982), it usually has a substantive component also, because in the life sciences most causal influences are stochastic rather than nomological (Meehl, 1978, pp. 812-814) due to the ubiquity of uncontrolled and unmeasured variables that may interrupt the causal chain of interest. That being so, it is scientifically legitimate (and often profitable) to depart from the pure intervening variable approach and state explicitly that the term being introduced designates a hypothetical construct, which is causally linked, but only stochastically, to each of the (fallible) indicators of its presence and magnitude.

This analysis brings up the vexed problem of *surplus meaning* in theoretical terms (Beck, 1950; Feigl, 1950; MacCorquodale & Meehl, 1948; Reichenbach, 1938). Given a set of observational particulars (considered unproblematic for present purposes) and theoretical statements said to be based on them, logicians recognize different kinds and degrees of surplus meaning. First, it is a truism in philosophy that any lawlike statement has surplus meaning over any finite collection of particulars, even if every term is in observational language. That’s the old induction problem and has little scientific interest. Second, when a lawlike statement is extrapolated to a new *domain* (e.g., to humans from rats), we begin to have a substantive scientific interest in its ampliative character. Third, when nonobservational terms are found in the theoretical network, we have hypothetical constructs. Their surplus meaning arises from two sources. The first is that almost all

empirical theories have one-way derivability, theory-to-general-fact but not the reverse. (This is a logical and mathematical fact, not a matter of taste, as some psychologists seem to think.) The mystery of how a network of theoretical statements can simultaneously *assert* and *define* is solved by a technical device known as the Ramsey Sentence, to discuss which is beyond our scope here (interested readers may see Carnap, 1966, Chapter 26 and pp. 269-272; Glymour, 1980, pp. 20-29; Lewis, 1970; Maxwell, 1962, pp. 15ff, 1970, pp. 187-192; Stegmüller, 1979). But in addition to the surplus meaning entailed by logical structure, a further source is *interpretive* text associated with the formalism, in which the theoretical entities “partially defined implicitly” by their role in the network are characterized substantively and the admissible interpretations further narrowed. This means that the empirical content of the theoretical terms is not provided solely by “upward seepage” from the observation statements, as was mentioned above (see Meehl, 1990a, 1990b).

LATER SKINNER VERSUS 1938

The later Skinner never refuted the powerful and incisive analysis in *Behavior of Organisms* of the rationale for introducing state-variables. Further, while his meta-discourse came increasingly to deprecate them, along with other “hypothetical inner causes,” many passages in the later works admit—nay, insist, against critics—that behavioral science can and does treat such inner states and events. We might fault him for an inconsistency between his object-language (dealing with the facts as best he can) and his metatheoretical stance (pushing his “philosophy of science of a behavior science”). More charitably, we could interpret the difference as between his over-all research *strategy* and *preference* and his honesty as a scientist when faced with certain facts. Because he shares with other scientists a belief in spatiotemporally continuous causal chains, he, of course, knows that the only way being deprived of food during the preceding 23 hours can influence lever pressing rate is via a present organic state (“hunger”). It is simply that he does not believe in ESP, spooks, or action at a distance. Speculating about the change, my hunch is a convergence of influences on him over the years. I believe he became tired (bored? irked?) at having to answer the same arguments repeatedly. (Having given up explaining statistical prediction to clinicians after so many years of trying [Dawes, Faust, & Meehl, 1989; Meehl, 1954, 1986a] I know the feeling.) I also suspect that Skinner became somewhat intellectually isolated with age, conversing and corresponding less, and mostly with persons who agreed with him (Daniel Wiener, personal communication, April, 1991). In the 1940s’ Minnesota discussions, I had the impression that he was a bit embarrassed by the demise of his reflex reserve concept, which he had introduced (somewhat apologetically) in *Behavior of Organisms*, as not quite in the strict operational spirit of his system:

I shall speak of the total available activity as the *reflex reserve*, a concept that will take an important place in the following chapters. In one sense the reserve is a hypothetical entity. It is a convenient way of representing the particular relation that obtains between the activity of a reflex and its subsequent strength. But I shall later show in detail that a reserve is clearly exhibited in all its relevant properties during the process that exhausts it and that the momentary strength is proportional to the reserve and therefore an available direct measure. The

reserve is consequently very near to being directly treated experimentally, although no local or physiological properties are assigned to it (Skinner, 1938, p. 26).

When the postulated reserve was not “clearly exhibited in all its relevant properties” (dramatically in the pigeon bomb project and other extreme schedules), I suspect he felt “burned” after having introduced a hypothetical construct and resolved to be very careful in the future.

These context-of-discovery speculations aside, the experimental result that could “rationally” lead to down-playing drive as a quantitatively important factor was the surprising potency of schedules. Combining the hard facts of scheduling with a behaviorist strategy to minimize reference to inner causes, a monolithic emphasis on the controlling environment is an understandable result. When we can produce fantastically high response rates and (in pigeon) *thousands* of unreinforced responses by schedule manipulation, the traditional weight given to motivation is overcome. (Although we note that operant behaviorists do not make a routine practice of running satiated animals!) The minimizing of drive as a factor was sometimes carried to extremes by Skinner’s disciples, even to unedifying polemical comments. (See, e.g., Reynolds [1975]: here an otherwise excellent book is marred by such statements as, “Many apparently erratic shifts in the rate of responding, which had formerly been ascribed to nebulous motivational variables or to “free will,” have been traced by experiment to the influence of schedules of reinforcement” [p. 66], a tendentious association of the motivation concept with the anathema, unscientific notion of free will. A similar remark is the concluding sentence of the book.)

As to the rational implications of schedule potency for the role of motivation, I raise two skeptical considerations that I trust are not tendentious. First, there are biological constraints on drive manipulation that are absent or looser for schedules. We can impose large ratios (already in *Behavior of Organisms* Skinner employed FR192) several orders of magnitude beyond what the animal’s nonlaboratory ecology provides. We can set them so high that the rat runs calorically behind, maintaining life by extra-box feedings. But we cannot make a rat “100 times as thirsty or hungry” (assuming one knows what that number means) without killing it. The hunger drive is not linear in deprivation time, and a body weight *ad lib.* percentage will not work either.

The second caveat concerns Brunswik’s problem of representative design, sampling the “natural” ecology of situations (Barker, 1960; Brunswik, 1947, 1955; Collier, Johnson, Hill, & Kaufman, 1986; Hammond, 1954; Postman & Tolman, 1959; Sells, 1966). When an operant behaviorist asserts (as some have in the past) that Skinner-box experiments have shown motivation to be a rather minor, unimportant factor, what does such a statement mean? That it does not mean “It’s safe to ignore motivation” is shown by our routinely running hungry animals. Everyone takes it as a matter of course that a satiated rat manifests very low strength whatever the schedule, perhaps even below pre-reinforcement operant level. Does it mean “The range of strengths producible by varying schedules exceeds the range we can get by manipulating drive?” Well, in the apparatus, using hunger and thirst, that generalization has been pretty solidly corroborated. On this basis, can we say to Murray: among human adults functioning in “normal” (usual, standard, representative) conditions, motivational variables play a minor role in determining *what* a person will do *when*, and *how*? We are not entitled to conclude that by direct extrapolation from the Skinner-box research. When we move from rats in the box to

persons in the clinic, some big parametric problems arise.⁷ My point is a quantitative one, urging caution in extrapolating a quantitative claim (“schedules are more potent than drives”) when the extrapolated context exhibits several nonnegligible quantitative differences from the original. Assume that the activities of a healthy nonincarcerated human instantiate Skinner’s behavior laws, that is, his behavior system is qualitatively correct, including verbal behavior. Here are some structural and parametric differences from the rat-in-box case:

1. The range of schedules’ reinforcement probabilities is usually small, compared to the rat (or pigeon!) in the box.
2. For the class of operants associated with a particular motive-reinforcer combination, schedules remain relatively constant, barring major societal catastrophe.
3. Hundreds of different operants are involved in a typical 24 hour period.
4. Hundreds of discriminative stimuli are controlling.
5. There are scores, if not hundreds, of reinforcing stimuli, mostly secondary.
6. Most of these secondary reinforcers are themselves discriminative stimuli controlling succeeding operants in long chains.
7. Many chains share partially overlapping sequences; and the final reinforcers may be associated with disparate motives.
8. Human adults locomote, moving amongst various “social Skinner boxes” presenting different operanda, S^Ds, and reinforcements.
9. Many discriminated operants S^D.R have been shaped and maintained by distinct motive-reinforcement operations, so the same or similar behaviors come under different motivational control. (This is one of the main reasons why clinical interpretation is so difficult at times.) For example, a driver stops the car upon seeing a neon sign “Fisbee’s Bar and Grill” and locomotes into the place. Why? The reason could be (a) hunger, (b) wanting a beer, (c) urinary pressure, (d) check World Series on TV, (e) make a phone call, (f) seeking social company, or (g) seeking a sex partner. All of these motive states except possibly (g) are on nearly a CRF schedule in the presence of visual stimulus “bar and grill,” and the “Fisbee’s” stimulus component may signal that even (g) is rich.
10. A large portion (I conjecture almost all) of instrumental acts performed by adult humans involve temporally remote goals, and the reinforcement is self-administered contingent on achieving “subgoals,” as when completion of an onerous task required by one’s job is reinforced by an internal tact such as, “Well, *that* is done!”
11. Although partial reinforcement schedules are probably the rule rather than the exception, the normal ecology of healthy, freely moving human adults rarely imposes such extremely lean reinforcement probabilities as experimenters tend to employ in the box. A large part of our daily behavior resembles Skinner’s paradigm CRF example S^D:visual pencil→R:reaching→S*:tactile pencil. Lacking a large scale (unobtrusive) Barker (1968) type of monitoring of adults’ activities over 24 hours, I can only rely on introspection and opinion for a rough estimate. Inquiry among fourteen psychologists as to what percent of their operants—all kinds, all reinforcers—are reinforced yielded a

⁷ I am not here making the common clinician’s complaint that the experimental findings don’t apply to the “real world.” As my late colleague, MacCorquodale, used to remind clinical students, everything that occurs in spacetime is *real*. An event or generalization does not somehow lose reality by taking place in a building called a laboratory! See Skinner, 1961 (pp. 242-257).

range from 40% to 99%, median 91%. (The only estimate <50% was by a resigned department chair serving his last two onerous months in office, and I'm not sure that he used 'reinforce' as Skinner does. I could have deleted his estimate with a clear conscience.) A reinforcement probability of $P = .90$, say on the commonest kind of schedule in daily life, is VR1.1, far richer than those we are accustomed to see in *Journal of the Experimental Analysis of Behavior*. When operant behaviorists assert that "Of course we run hungry animals, but the big point is that reinforcement schedules are far more important than motivational variables," this is a *quantitative* claim whose ecological validity is parameter dependent. To test that claim "in the box," we should perform experiments in which schedules running CRF, VR1.1, 1.5, 2.0, (densifying in the small integer region) are set to countervail drive levels. I am told by several knowledgeable operant behaviorists that no such "competitive" experiments have been reported. Until they are, the down-playing of drive by extrapolation from Skinner-box research is, it seems to me, unwarranted. Being a neo-Popperian, I will venture a risky conjecture: For schedules in the range CRF–VR5, drive will be controlling. I predict a hungry, nonthirsty rat will press the food-bar rather than the water-bar, almost exclusively, despite their schedules being VR2 and VR1.1, respectively. And my philosophy of science self cannot resist commenting that the absence of parametric studies in that region—considering the thousands of experiments ringing the changes on complicated schedules of a sort no animal outside the lab will ever come across—is a nice example of how theoretical orientation can determine what facts will be collected.

I don't claim the list is complete, but these eleven differences, especially the eleventh, alert us to the danger in immediate extrapolation of *quantitative* generalizations about motives versus schedules. But the biggest difference is a twelfth, relating to Murray, Tolman, and McDougall's way of identifying purpose by cessation of goal-seeking activity.

12. Given stable human schedules (mostly low VR \approx CRF, very rich), when a chain runs off, the equivalent of a huge food-pellet is usually delivered, reducing the motive to near-zero intensity. In Murray language, a certain need is regnant for a limited time interval and terminated by achieving an adequate subgoal. Example: A clerk is thirsty, suspends clerking behavior to approach a water cooler, drinks, abates thirst, goes back to clerking. The *n Water* was regnant for a couple of minutes, and during that short interval no "competition" with other needs or operants was involved.

It is not as if we were always playing off schedules against schedules, or influence of drive strength against potency of schedules, as some writers imply. The clerking operants usually "work," as does the water seeking operant. The clerking behavior is in some respects like repetitive lever-pressing under a steady state motive; the drinking chain is not. Whenever a controlling motive is essentially terminated by adequate "satiating" reinforcement, the situation resembles Thorndike's (1898) cat escaping from a cage or Tolman's rat reaching the goal box more than it does Skinner's pigeon repeating the same response rapidly hundreds of times to get a small pellet that leaves hunger unabated (cf. Skinner, 1930). Skinner's substitution of a continuously available manipulandum for the traditional "trial" (maze, shuttle box, discrimination chamber, obstruction box), with the consequent cumulative record instead of a "learning curve," had great fertility for several reasons, especially by providing a highly sensitive measure of instantaneous operant strength. While not deprecating that major advance, I observe that—like most good instrumentation ideas—it carried with it a disadvantage; it is an experimental paradigm

for situations that involve one or two operants, a few stimulus controls, and repetition of the response with minimal reinforcement *so that the relevant drive is not satiated*. I conclude that we cannot validly dismiss motive as an unimportant causal factor, on the usual Skinner-box grounds.

A final reason why Skinner later played down state-variables is his emphasis on *control*. Most psychologists want to explain, predict, and control, but the mix varies. Murray (like Freud) was more interested in explaining than in controlling. (There is no epistemological difference between predicting and controlling, since the former entails the latter *if* you have manipulative access to the determining variables, otherwise not.) While the control emphasis is present in *Behavior of Organisms*, one has a distinct impression that it increased over the years, so that later Skinner aimed to locate everything that matters in the organism's environment. Those who comfortably invoke inner states (like motives, fantasies, anxiety) are, he thought, deflected from searching for the environmental controllers. Granting (repeatedly) that "hungry" has a referent, and this state is in the causal chain, Skinner argued that we control hunger by deprivation and that *manipulable* external variable is what counts in controlling the food-reinforced operants. Well and good, and no one—Murray included—could dispute that, or would wish to. However, absent such control of deprivation, we can at least *predict* the operant strength if we can assess the state-variable, as *Skinner explicitly asserted* (e.g., 1953, pp. 199-200). A clinician can inform the sentencing judge that a delinquent is a high risk for recidivism, given his rap sheet, impulsive Q-score on the Porteus Maze, and pure culture acting out 49" profile on the Minnesota Multiphasic Personality Inventory (MMPI). These are useful predictions, but they utilize R-R stochastic laws rather than reference to the controlling stimuli, or even the history of reinforcements causing delinquency. (At Minnesota, Skinner was as interested in a student's Miller Analogies score as trait-oriented Hathaway or Paterson.)

His later deprecation of state-variable explanations is one of the few places where I have to fault Skinner, usually the most consistent of writers, for an inconsistency. It occurs in his meta-talk, but it is a real meta-inconsistency. This is his labeling of motive explainers as "circular," "tautological," "redundant" *when we lack manipulative access via deprivation*. We say "That rat presses the lever because he's hungry." If we haven't made him hungry, not having experimental access to the deprivation schedule, in some loci Skinner decried that as empty, circular, etc. (e.g., 1953, pp. 33-35, 143-144), but it is not. There are several knowledge situations in which "he presses because he's hungry" is a perfectly legitimate scientific statement. For example, he presses the food-delivering lever but not the water-delivering one. In all such explanations, we understand a particular in terms of a well-tested universal (law of behavior), inferring the existence of the explanatory particular via the law. The inferential process is exactly the same as in saying, "That rat must have been reinforced for lever pressing, given the rate at which it's now pressing." Of course such inferences rely on an empirically corroborated network of lawlike statements, whether they are statements about reinforcement, drive, shaping, discriminating, or whatever. But once having introduced a state-variable 'hungry' (for the cogent reasons in *Behavior of Organisms*), there is nothing scientifically sinful in relying on laws concerning it to infer an unobserved particular. In physics, we must first verify Hooke's law experimentally. Having done so, we do not apologize for inferring a hidden heavy weight from seeing the stretched spring. If Skinner's "tautology" criticism were

applied across the board, he would be estopped from invoking a history of reinforcement, since reinforcement is defined by its behavior effect. This criticism he anticipated (and answers correctly) in *Behavior of Organisms* (1938, p. 62; cf. Meehl, 1950). If it were illicit in science to infer the value of an unobserved (causal) particular x from an observed (effect) particular y , having repeatedly corroborated a relation (xRy), we could not have ascertained the chemical composition of the stars. Relying on Hooke's Law ("stress is proportional to strain"), if I see the visible portion of a stretched spring double its length, I infer that the unobserved weight has been doubled.

Operant behaviorist colleagues reading this paper in draft took mild umbrage about "control," as if I were imputing a purely technological interest to Skinner and his disciples. This is not my intention, and I am well aware that Skinner was at some pains to characterize himself as a *theorist*, albeit not of the hypothetico-deductive kind (Skinner, 1969, pp. vii-xii). But I do not think my remarks here are misleading, given their context. Given his repeated "theoretical" admissions that a state of thirst really exists, inside the organism, as part of the causal chain from input to output, to then disparage invoking it on the ground that we can only *influence* the state by deprivation or other input manipulations, elevates *effective control* to special status. This Murray (or Freud) would never have done, and I insist that it reflects a *preference*, not a defensible epistemological *criterion* of conceptual legitimacy.

THE MURRAY NEEDS

Our principal focus being Skinner, I shall treat the Murray needs summarily and only as their conceptualization relates to Skinner's 1938 notion of state-variables, urging the reader to have a look at the extended discussion in *Explorations in Personality*. I must enter a caveat to operant behaviorists about to read Murray: His thought modes and, even more, his choice of words will probably turn you off. It is a truism among historians and philosophers of science that if one wishes to grasp the substance of a scientist's theory so as to analyze and utilize it, and especially to relate it to another theory, one must attend to what the theorist says about what he *did* and what he *observed*, rather than what he *says about what he says* (i.e., meta-talk). In Skinner's case, we had to parse his arch-behaviorist "philosophical" meta-talk from his scientific discourse, lest his "minimizing" tendency prevent our seeing how much his translations endeavor to include. Skeptical, aseptic, and devoted to a "pure intervening variable" theory, his methodological advice is precisely opposed to a philosopher of science like Popper, who advocates "risky, low probability conjectures" (and even holds—wrongly, as I think—that there is no such process as induction!). One of the few polemical remarks in *Behavior of Organisms* pokes fun at hypothesizing scientists "whose curiosity about nature is less than their curiosity about the accuracy of their guesses" (Skinner, 1938, p. 44). Murray, a very different sort of mind, obviously luxuriates in speculations, analogies, metaphors, cross-connections. By my lights, much of the meta-talk in the *Explorations in Personality* theory section is unhelpful and, to one in the "tough-minded" tradition, even counter-productive or obscurantist. Should an operant behaviorist complain that Murray's talk about a "holistic," "purposive," "organismic," "Gestalt," "dynamic" viewpoint cuts no ice, I will cheerfully agree. One of the book's dedicatees is Alfred North Whitehead, thanked for his "philosophy of organism." I doubt that a single experimental procedure or a single personality questionnaire item found in the book derived from Whitehead's

philosophy. So I advise the tough-minded reader to ignore most of Murray's meta-talk, remembering charitably that the date was 1938, when most psychology departments—Harvard's included—considered “personality” hardly a fit topic of scientific study. Interestingly, Skinner seems to feel obliged to defend his introduction of state-variables against a skeptical, minimizing methodology *that he largely shares*; while Murray's meta-talk has the same defensive intent, against the same methodology, but which he does *not* share.

Bypassing the defensive meta-talk, we note immediately three encouraging points in Murray's introduction of the need concept. First, while he offers seven main reasons (and 16 adjuvant arguments[!], some rather light makeweight) for introducing such a concept, he insists that the *main* basis for inferring, defining, and identifying a need is *behavioral*. Second, the core structure of this behavioral basis is that in a situation the organism emits some behavior B which brings about an effect E, i.e., some stimulus *change* is contingent on something the organism does. *Prima facie*, that reads pretty much like a good Skinnerian. Third, when it comes to *characterizing* the behavior thus identified, he emphasizes that we (normally) do not specify the effector patterns (“actones,” divided into “motones” and “verbones”) but instead the *molar effect*. It is particularly significant that in defending that level of behavior analysis, Murray cites Skinner's classic paper (1936) on the generic nature of the concepts of stimulus and response—one of the few mentions of an experimental psychologist in the entire 105 pages of the theoretical chapter in *Explorations in Personality*! It is an interesting historical fact that 99% of all psychological research since Ebbinghaus, on humans or infrahuman animals, has taken as the dependent variable a *molar achievement class* (turn in maze, depression of lever, utterance of a “word”) rather than a response class specified by *effector pattern* (MacCorquodale & Meehl, 1954, “Excursus: The response concept,” pp. 218-231). Of course in the Skinner box this “works” because the (mixed) class of effector activities that suffices to get the lever down is, via the apparatus structure, the class that is reinforced.

It may be objected that while his tripartite sequence $S \rightarrow R \rightarrow S^*$ looks familiar, Murray over-emphasizes the *termination* of activity as a consequence of goal attainment, not a usual operant behaviorist focus. True enough, and I have no wish to obscure that fact. It arises, however, not from any basic *conceptual* difference, but rather from the difference in the situational contexts typical for Murray and for Skinner. As explained above, in the box we deliver small amounts of reinforcement lest we satiate drive so quickly as to obscure the quantitative features of the cumulative record (and their determining contingencies) that we wish to study. This being merely a routine “technical” procedure in animal experimentation, we are tempted to underestimate its conceptual significance when we are relating two theories developed on largely disjunct domains (species, operants, reinforcers, *parameters*). Skinner's first publication showed the decelerated curve of ingestion due to satiation (Skinner, 1930, and 1938, Figs. 120 and 121, pp. 344-345). Murray, having in mind cases such as a free moving adult human going to lunch, properly thinks of reinforcements that satiate the regnant need or at least reduce its intensity so that other competing needs become regnant. The main point is that the *content* of a need is provided by the empirical fact that, given a certain situation(-class), one or another behavior sequence occurs, having a *tendency* to bring about certain changes.

Murray was influenced by Tolman (1932) who in that classic work gave multiple criteria for “purposive behavior,” taken in turn from McDougall (1923), whom Murray also cites. It is interesting in our Murray-Skinner comparison context to note that Tolman lays most stress on *docility* (de-emphasized by McDougall), i.e., the *change* in behavior dispositions produced by reward (stressed by Perry, 1918). This test, plus the “operational” definition of intervening variables, is perhaps why Skinner thought his system closely akin to Tolman’s. Murray considers the empirical fact of *alternate routes* to the goal-situation to be one of the main arguments for postulating a need, exactly the point made by Skinner in his 1938 justification of state-variables (cf. the Hull-like “fanning” input-output diagram, p. 24).

Several ways of classifying needs are developed by Murray, some of which are less important today (e.g., viscerogenic versus psychogenic), but the big division is abient/adiant, *getting away from* versus *seeking* a certain state of affairs. This maps onto Skinner’s negative and positive reinforcers and includes the empirical fact of correlated “emotional” respondents. Murray’s terminology reflects this, the labels for abient needs containing the suffix ‘-avoidance’ (e.g., *n Blamavoidance*, *n Harmavoidance*, *n Infavoidance*). There is some tendency for the adiant needs to be partly endogenous, increasing due to sheer lapse of time (e.g., hunger, thirst, sex, activity), whereas the avoidant needs are almost wholly aroused by concurrent stimuli (e.g., predator, noxious insect, human aggressor, other threat of tissue injury). But this stochastic tendency is not built into the adiant/abient definition, and the trend may be partly an artifact of psychologists’ traditional choice of which drives and correlated reinforcers to study.

Operant behaviorists accustomed to studying rats in the Skinner box are sometimes put off by the complex, vague, and variable character of those states of affairs that constitute the “goals” of Murray needs, but this is an empirical identification problem, not something conceptually out of harmony with Skinner’s system. Social reinforcers in the human case can at times be hard to pin down, as Skinner points out in discussing generalized reinforcers (1953, p. 77-81). He writes, “It is difficult to define, observe, and measure attention, approval, and affection.... Their subtle physical dimensions present difficulties not only for the scientist but ... also for the individual who is reinforced by them” (p. 79). Skinner is quite willing to allow reinforcing states of affairs of the Murray kind when human social behavior is involved. “That ‘having one’s own way’ is reinforcing is shown by the behavior of those who control for the sake of control (1953, p. 79). “The submissiveness of others is reinforcing even though we make no use of it” (p. 81). This all sounds very like Murray’s *n Dominance*. Obviously the effector topography of a conspecific’s behavior that suffices to make it “submissive” will be far more difficult to describe than the delivery of a food pellet or the turning off of a bright light in the box (cf. Meehl, 1954, pp. 43-44).

The concept of *subsidiation* [= S] is not readily translated into Skinnerese, although one could make considerable headway by reference to chaining. Consider the following (amusing, some may think far fetched) Murray example of subsidiation:

A politician removes a spot from his suit (*n Noxavoidance*) because he does not wish to make a bad impression (*n Infavoidance*), and thus diminish his chances of winning the approval and friendship of Mr. X (*n Affiliation*) from whom he hopes to obtain some slanderous facts (*n Cognizance*) relating to the private life of his political rival, Mr. Y, information which he plans to publish (*n Exposition*) in order to damage the reputation of Mr. Y (*n Aggression*) and

thus assure his own election to office (*n Achievement*): (*n Nox S n Inf S n Aff S n Cog S n Exp S n Agg S n Ach*) (Murray, 1938, p. 87).

Whether we are to conceive all of these needs to be concurrently regnant while the politician removes the spot, I shall not try to decide. Theoretically, we may suppose that whether the links in such a molar chain include arousal of a need, however briefly, should be investigable by applying the usual tests for operation of a state-variable as set out by Skinner for the simpler case (e.g., covariation among sufficiently different operants stochastically effective at the same chain locus, such that if the initially stronger “fails,” another replaces it). The complications we face in such tests are enormous, but this is not a ground that *either* Murray or Skinner allows as invalidating the *conceptual* analysis. Both thinkers were fully aware of the programmatic and problematic character of extrapolation to complex cases.

Except for his defensive meta-talk, the most troubling aspect of Murray’s need concept, for a behaviorist, is his emphasis on regnancy as a *brain state*.⁸ Writing in 1938, Murray is combating “peripheralist” interpretations, as found in the theories of Watson, Guthrie, or Hull. *Centralism* occurs in his meta-talk, but it is actually part of the theory as it was in Tolman’s. (I knew a Spence-Hullian who insisted that whether a Tolmanian expectancy took place in the brain or instead as a damped chomping or slurping, as in r_g , was “irrelevant imagery, not part of the cognitive content.” This is a rather high-handed proceeding, to inform a theorist that, when he makes a perfectly good factual assertion—stating the physical locus of a critical mediating event—he does not mean what he says, or at least *we* can forbid him to do so!) But what is there to fuss about here? Despite Skinner’s meta-talk, we saw in the quotes above that he admits inner states and events are in the causal chain, and even, on occasion, *insists* on including them (e.g., as against Boring, in the operationism symposium). Now assuming, as I am throughout, that the Skinner who wrote *Behavior of Organisms* gave sound arguments for state-variables, and assuming that the empirical role of, say, thirst is shown to control operant $S^D_1.R_1$ at time t_1 whereas hunger controls $S^D_2.R_2$ at t_2 , what if Murray wants to say “When hunger is regnant, it’s a brain state; when thirst is regnant, it’s a brain state, a different one”? Of course Skinner does not need, or want, to deny this. He knows as well as Murray does that the rat’s muscles are controlled by the brain, not the gall bladder. My view here is that, offensive as the “conceptual nervous system” is to Skinner, what we should irenically pronounce about Murray’s regnancies being located in the brain is that such an assertion is true, trivial, useless, but harmless. I cannot refrain from including a funny story about regnancies, which shows (as I noted in his verbal behavior class) that Skinner was quite capable of playing the Freudian game when he wanted to.

A letter I wrote to Murray is perhaps an extreme example of the freedom we enjoyed in our exchanges. He had given a colloquium on his theory of “regnancy,” and I saw a chance to demonstrate a bit of Freudian theorizing. I said there were some things about himself that I felt he ought to know. From his use of the term “regnancy” it was clear that as a child he had been led to believe that it was urine which entered the female during sexual intercourse, and that his unconscious mind was still struggling to separate *p* from pregnancy. (He told me many years later that it was the rudest letter he ever received.) (Skinner, 1979, pp. 13-14).

⁸ Delete this, and what remains is the inference that one state-variable, rather than another, is momentarily controlling. Once we have admitted state-variables at all and specified their roles in the system, such an inference is on a par with one concerning discrimination or reinforcement and is unobjectionable.

NEEDS AS TRAITS

A regnant need is a *disposition*, more precisely, an inner state controlling a family of correlated dispositions (operants and respondents). Experimental and statistical studies of human and infrahuman species typically yield individual differences, and this empirical fact leads us to consider the arousability and strength of a need as a trait, in which persons (and rats) differ. Since the trait is the disposition to manifest the family of first-order dispositions (that identify the need, specify its "content"), the trait is a *second-order disposition*, in logician's terminology (Broad, 1933; Meehl, 1972). Here again we confront Skinner's antiseptic meta-discourse, where he dislikes appealing to traits as explanatory entities; but there is nothing in his substantive theory to conflict with the trait concept when properly articulated. He includes psychometric data as one of the six sources of evidence for behavioral science (1953, p. 37), a concession that entails the meaningfulness of trait concepts. I need not pile up argument to that effect, since in his review of the MacCorquodale *Festschrift* (Thompson & Zeiler, 1986) he says (1987b, p. 505) of my contribution "Meehl makes a strong case for traits as response classes" (see also Skinner, 1953, pp. 199ff). The dispute about traits versus situations, in which some social psychologists and many operant behaviorists have been "antitrait" (for different reasons), is methodologically unsound as usually formulated. Since traits are (S.R) dispositions, their careful semantics must *include* the situational side, however broadly characterized (or, as often, implicitly presupposed). For a detailed treatment of the problem I must refer readers to my paper (Meehl, 1986b; cf. Lubinski & Thompson, 1986; Tellegen, 1991). But there is an important point not made there that relates to our present topic. There exists a vast body of quantitative evidence on the correlation between psychometrically measured motivational traits and other aspects of behavior (experimental, life history, social impact, and non-motivational psychometric traits). An operant behaviorist is not at liberty to ignore these data, since a logically relevant fact cannot be made irrelevant by fiat any more than an irrelevant one can be arbitrarily declared evidentiary. These relationships include very diverse matters: college students with high MMPI hostility scores are five times as likely to suffer coronary disease in later life as those with low psychometric hostility (Barefoot, Dahlstrom, & Williams, 1983; Barefoot, Dodge, Peterson, Dahlstrom, & Williams, 1989). Subjects with hysteroid (31' code) MMPI profiles have more perceptual defense against aversive words presented tachistoscopically than subjects with a 27' code (Byrne, 1980). College students with 49' profiles ignore a dean's letter three times as often as those with elevated psychasthenia scores, but the latter record twice as many "doubtful" judgments in performing a weight-discrimination task (Griffith & Fowler, 1960; Griffith, Upshaw, & Fowler, 1958). Male Broadway actors average a mean femininity score (MMPI scale 5) elevated 2.67 standard deviations on Minnesota norms, but only two of the 60 items on that scale refer to acting (Chyatte, 1949). The Edwards Personal Preference Schedule, constructed to measure Murray needs by forced-choice technique, predicts a variety of other behavior dispositions. The scientific touchstone for an entity's reality, as Skinner cogently expounded in discussing stimulus and response classes (Skinner, 1936; 1938, pp. 33-43) is empirical *orderliness* (curve smoothness, covariation, functional relations). The objective existence of distinguishable traits as psychometrically assessed has received strong support in recent years from research in behavior genetics (e.g., Bouchard, Lykken, McGue, Segal, & Tellegen, 1990; Tellegen, Lykken, Bouchard, Wilcox, Segal, & Rich, 1988).

Relationships of these kinds can be found by the thousands in the literature. They cannot plausibly be explained in terms of verbal reinforcement schedules alone, eschewing motivational variables. When one contemplates the individual verbal items on the Strong Vocational Interest Blank, MMPI, California Personality Inventory, Multidimensional Personality Questionnaire (Tellegen & Waller, 2008 [cite updated]), Edwards Personal Preference Scale, etc., one realizes that they cannot all be functioning in the same way, as atomistic indicators of the traits measured. Some are tacts of the subject's own behavior dispositions. Some tact external variables of which his behavior is a function. Some tact inner stimuli and state-variables. Still others, whether tacts or mands, function psychometrically *not by virtue of their tacting accuracy but as indirect indicators of defense* (e.g., the MMPI *Hy*-subtle items that detect the hysteroid's preference for the defense mechanisms of denial and repression [Meehl, 1945]). In one statistical study the best "genotypic" predictors of phenotypic resemblance between patients was their relative preference among defense mechanisms, and the second best was the relative strength of Murray needs (Meehl, 1960). Excluding needs and defenses from one's theoretical constructs would result in either a severe attenuation of psychometric validities or, retaining need-indicator items for technology, a Procrustean forcing of their causal interpretation into a pure "accurate tacting" mold [cf. discussion of tests as "signs" versus "samples" by Cronbach and Meehl (1955)]. For reasons discussed by Skinner in connection with complex cases, the lawful relations involving human motivational variables (whether studied psychometrically, experimentally, or in clinical work) are usually weaker than those obtainable in the Skinner box with rats or pigeons—just as meteorology is more stochastic than mechanics and thermodynamics, and clinical medicine is fuzzier than physiology and biochemistry. Nevertheless, the correlations are there, in large numbers (Bolles, 1967; Cofer & Appley, 1964; McClelland, 1985; Valle, 1975; Weiner, 1972), and we cannot afford to impoverish our science by ignoring them.

THE TRANSLATION PROBLEM

Any attempt to relate two theoretical systems purporting to cover an observational domain is faced with the problem of translation. Rigorous and effective criteria for synonymy of terms and equivalence of statements is a deep and unsettled technical problem for logicians, but at the level of "scientific method" in the study of behavior, we cannot avoid saying something about it. This is especially important because one of Skinner's favorite metatheoretical moves is to comment, "We often say sentence S_1 but we could [better!] say S_2 ," where S_1 is a statement in the vernacular, or psychodynamic, or trait-theoretic language, and S_2 is in behaviorese. A variant of this locution—ubiquitous in his writings and conversation—is "we say S_1 , but we *need not* say that, we could as well say S_2 ," where "need not" points to a component of surplus meaning that Skinner urges avoiding. Here again, we must distinguish between an expression of research strategy and some sort of metatheoretical rule, a general philosophical *argument* that the surplus meaning conveyed by S_1 is somehow scientifically illicit. The latter is, of course, impossible to provide, since no finite set of observational particulars deductively entails any theoretical statement, even one of pure intervening variable type. (We cannot attribute to Skinner ignorance of a truism that one learns in an elementary logic class.) If a Murray disciple were confronted with the objection "Given observational facts O_1, O_2, O_3 you say S_1 , but you need not say that," the proper Murray reply would be, "Of course

I need not, since O_1 , O_2 , O_3 do not deductively entail S_1 . Hence I cannot *compel* you, Skinner, to assert S_1 . But while I ‘need not’ assert S_1 , I may, if I choose, assert it, claiming that O_1 , O_2 , O_3 tend to corroborate S_1 , by usual standards of empirical inference. We can, if you like, examine that corroborative relation on the merits. But pending such a critical examination, you cannot forbid me to assert S_1 merely on the ground that it is not the semantic equivalent of O_1 , O_2 , O_3 . I never said it was.” This is a solid-gold, 100% valid reply, whether one looks to logic, epistemology, or history of science for rough criteria of admissibility of statements in empirical science. We are left with Skinner’s philosophical preference for minimizing surplus meaning based on his empirical claim that other research strategies do not work well for psychology, and a reasoned discussion about the substantive merits of S_1 as an explainer of O_1 , O_2 , O_3 .

Given that the objection to surplus meaning cannot be dispositive but is only advisory, one has to examine each proposed translation of psychodynamics into behaviorism on its merits. The assertor of S_1 may consider a translation inadequate, either because it says something not intended or because it leaves out something intended. Of course, the assertor has a right to be heard (prima facie, he “knows what he meant”), but that does not settle the matter. It may turn out that his attempted explication of the surplus meaning is so fuzzy that even our allowance of open concepts does not countenance it as a scientific concept. Or, we may conclude that the concept is admissible, but when the several theoretical components are parsed carefully, it contributes nothing. For example, in the Meehl-Skinner dialogue above about the translation of ‘unconscious,’ an orthodox Freudian might complain that Skinner’s amended definition, while now including the defensive (anxiety-avoidant) function of repression, omits to mention that *the ego* is what puts the repression mechanism into operation due to the inner anxiety signal. Skinner would doubtless reply that this additional surplus meaning (about one of Freud’s “psychic institutions”) is useless; and here I would agree with him. Skinner would likely emphasize the lack of enhanced power to *control*, whereas I would argue that *explanation* is not helped by postulating the psychic institution ‘ego.’ Both of us would incline to say that the ego probably does not exist, i.e., there is no such entity in the world.

What did Freud intend? In the 1933 lectures he makes his intention crystal clear, that the three psychic institutions are not merely fictions; and Skinner’s comment on this passage (Skinner, 1956, pp. 77-78) is, I believe, a compelling reply to Lindzey’s defense of psychoanalytic concepts as useful scientific fictions (Lindzey, 1953). But we must be clear what goes on here. In agreeing with Skinner against Freud, I do not rely on any blanket meta-rule forbidding all surplus meanings, such a rule having no valid epistemological status. Also I am not adopting a general theoretical policy of “avoid hypothetical constructs even if the logicians and historians of science consider them acceptable.” I am simply saying that, in this particular instance, the postulated entity ‘ego’ does not appear to be sufficiently evidenced by the facts to induce (not *compel*) me to accept it. Is this a Skinnerian analysis? It depends where in Skinner we read, as he is not, I suggest, entirely consistent in his treatment of Freud’s conceptions.

Given the current emphasis in metatheory on implicit definition of theoretical terms, a better approach to the translation problem looks at the whole law network rather than at single sentences (whether universal or particular). In discussing psychotherapy, Skinner (1953, pp. 359-383) centers his analysis on the concept of punishment, the main property of psychotherapy being that the therapist does not punish behaviors—including verbal

operants tacting behaviors or their controlling variables—as the patient’s parents and other acculturaters did. I dare say at least 95% of psychotherapists would agree with that as a big, perhaps the biggest, factor.⁹ The most important thing about Freud’s (1926/1959) revision of the anxiety theory from the experimental psychologist’s standpoint is the shift from a conception of anxiety as the result of repression (producing “rancified libido,” as my colleague Lykken puts it) to the inner anxiety signal as initiating the repression.

However, from the psychoanalytic standpoint the nonpunishing procedure is only half the story. A good psychoanalytic interpretation has the effect of attriting resistance, of “teaching the ego to tolerate less distorted derivatives.” Reformulating this in learning theoretic terms, we consider two levels (and *kinds*—operant and respondent) of conditioning. The defense is an operant, acquired and maintained by negative reinforcement, i.e., by escape, and then avoidance, of social punishment. The initial member of the chain is a Pavlovian conditioning (Skinner’s Type S) of anxiety as a state-variable to certain external and internal stimuli (e.g., proprioceptive consequences of the initial damped punished act, inner verbal operants tacting the drive or its correlated reinforcer). An effective analytic interpretation “works” in two concurrent ways. The defensive maneuver fails because the analyst calls attention to it and (partially) to what is being warded off. The technical dictum “Interpret defense before impulse” is rarely taken literally as a rule of *time sequence*, as Waelder (1960, p. 240) points out, since one usually at least hints at the warded off content. (My analytic supervisor being Rado-trained, I often label both defense and impulse fairly explicitly.) In Fenichel’s example, while “You are in a state of resistance” is not as clumsy as “You are trying to kill me with your silence,” better than either is “You cannot talk because you are *afraid* that thoughts and impulses might come to you which would be directed against me” (Fenichel, 1941, p. 38). So the defensive operant gets an extinction trial, not (completely) “succeeding” to avoid the anxiety signal. But the patient is not *punished* for having this bit of negative transference, so the underlying classical conditioning also gets an extinction trial. The message is, put crudely, “You cannot repress your aggression here, but then, you do not need to.” This simultaneous extinction trial of the linked operant and respondent requires the elicitation of some anxiety, but not too much. Hence we speak in psychoanalysis of an optimal *dosage* of anxiety. If the patient is continually experiencing intense dread of interpretations, the analyst is being clumsy, or perhaps has a hostile countertransference. If the patient is never the least bit anxious, the analyst is “playing along with the resistance.” As Fenichel puts it, “He who will perform surgery must not fear to shed blood.”

I have devoted some space to this mini-instance to make concrete the translation problem. Given Skinner’s expressed willingness to treat internal stimuli, covert responses, and [1938!] selected state-variables, I suggest the example shows how what might at first seem an undoable translation—psychoanalése into behaviorese—is actually quite feasible. I do not, of course, claim that *everything* Murray wants to say is translatable into Skinnerese. That would be absurd and pointless even when practicable. But I

⁹ Cf. Carl Rogers’ “unconditional positive regard” (1951, 1957), the classical analyst’s “neutrality” (see, e.g., Auld & Hyman, 1991; Fenichel, 1941; Fine, 1971; Freud, 1912/1958; Menninger, 1958; Reik, 1948), Alexander and French’s “corrective emotional experience” (1946). Even in active, challenging, “intellectualizing” Rational Emotive Therapy (see, e.g., Ellis, 1962), one does not say to the patient, “Shame on you for having these silly, irrational ideas. If you loved me, you wouldn’t say such dumb things.”

do hold that most of the facts the two address in their (1938) discussions of needs and state-variables are the same and are employed evidentially in pretty much the same way.

In Murray's system the important concept *press* denotes a component of the stimulus situation that presents "a *threat of harm* or *promise of benefit* to the organism" (Murray, 1938, pp. 40-41). That aspect of *press* is close enough to Skinner's discriminative stimulus to pose little difficulty. However, *press* also incites drive, functioning as a need-arouser (p. 42), so it plays a double role, concurrently. A particular *press*-need occurrence is an *episode*, and the "dynamic structure" shared by similar episodes is a *thema*. Whether that enriched meaning is translatable into Skinnerese depends on the factual question whether an S^D can possess an *eliciting* power over a state-variable. (Already in 1938 it was known that a discriminative stimulus acquires the reinforcing property [Skinner, 1938, pp. 246ff], so the notion of its arousing the correlated drive is a plausible extension.) I find neither assertion nor denial of this possibility in Skinner's writings, although he prefers whenever possible to use the neutral language *strong behavior* where Murray would infer a *press*-elicited need. The "priming" function of feeding, and the temporally remote "drive-conditioning" function (Anderson, 1941a, 1941b; Meehl & MacCorquodale, 1953) have been studied in the maze, but these results are not unambiguously interpretable as drive conditioning. The summary of research by Cravens and Renner (1970) is not encouraging, nor is their clarifying methodological analysis. The large and robust "conditioned thirst" effect achieved by Seligman, Ives, Ames, and Mineka (1970) and Seligman, Mineka, and Fillit (1971) loses some of its theoretical interest given the authors' conclusion that the effect is mediated by "poisoning" (Mineka, Seligman, Hetrick, & Zuelzer, 1972), hardly the typical case of interest. Common observation of humans clearly suggests a need-arousing stimulus function, sometimes persisting (although with regnancies turning "on" and "off" depending on competing needs and discriminative stimuli) over several days. I recently came across a printed reference to potato salad and emitted several verbal and locomoting operants on different occasions over several days to obtain this food. Skinner's frequent discussion of *appetite* versus *hunger* (e.g., sweet dessert after a full meal) suggests that he would have had no trouble with a finding that a stimulus may function as a state elicitor, but I never discussed it with him.¹⁰

The *second learning process* concept (Rescorla & Solomon, 1967) is sometimes taken to include conditioned drive, although more often called "emotional," perhaps because, for some reason, it has more often been studied in aversive experiments. By conditioned drive I *include* the drive property of anxiety but do not require any "emotional" component for the appetitive case. I do not tie conditioned motivation to the autonomic nervous system, nor to any effector activity (smooth or striped muscle or glandular). I further stipulate that the conditioned inner state must be relatively reinforcer specific, thereby linked to a designated drive, whether or not it also exhibits the "general energizing" power sometimes postulated. The necessary and sufficient condition for

¹⁰ Here is a Skinner-box design that might answer the question: For one group of animals, we establish $S^D_1.R_1$ at high rate reinforced by S^* under high drive, and $S^D_2.R_2$ at low or moderate rate, also reinforced by S^* but under a low drive level. A control group is trained and maintained on $S^D_2.R_2$ at a comparable low rate but under a different reinforcer S^\dagger and its correlated drive. We then present S^D_1 (with the R_1 -operandum unavailable) while the animals are maintaining a low steady rate on $S^D_2.R_2$. Do we get an increase in rate of responding on the R_2 -operandum in the experimental group?

inferring conditioned hunger is that an exteroceptive stimulus arouses or increases the state-variable “hunger,” an effect to be shown by appropriate indicators. The experimental difficulties that impede a definitive parsing of simple stimulus control and a motivational state-variable are well known [see the clarifying analyses by Trapold and Overmier (1972) and by Mackintosh (1974, pp. 222-233)], but that does not invalidate the concept. In such cases the universal testimony of human introspection deserves considerable weight. It is puzzling that so few experiments have been done to test the appetitive drive-conditioning conjecture, although the “slow onset” (as contrasted with conditioned anxiety based on presenting an aversive stimulus) suggested by Cravens and Renner is a plausible explanation.

A final point about translation is, while “technological,” of great significance to clinicians. Restating a psychodynamic formulation in behaviorese is often quite a complicated task, and sometimes (e.g., unusual cases) may be problematic even after prolonged reflection. During psychoanalytic therapy, one’s timing and wording of reflections and interpretations (or even requesting associations, “what comes to mind about the green Tyrolean hat”) are of crucial importance. Analytic “listening with the third ear” (Reik, 1948) is molar, phenomenological, involving the therapist’s skills in empathy, recipathy, imagery, analogy, metaphor, punning, etc. (Freud, 1900/1953, 1912/1958; Meehl, 1983; Sharpe, 1951; Siegelman, 1990). If we required ourselves to verbalize (internally) behavioral translations of every conjectured momentary impulse/defense conflict, every regnant Murray thema, every speculative historical reinforcement, we would be paralyzed as interpreters. (I simply could not do it.) Skinner replies to criticisms of his using the vernacular—“I have chosen ...,” “I have in mind ...,” “I am aware of the fact ...”—by pointing out the needless cumbersomeness of regularly translating into behaviorese; although one should presumably be *able* to do it when a conceptual or empirical question is at issue. The heating engineer thinks in terms of BTUs and cubic feet of air moved, not forcing himself to express these concepts in terms of kinetic theory. An economist manipulates Jacobians without repeatedly verbalizing to himself the definition of a partial derivative. This pragmatic justification for thinking in terms of needs and defenses cannot be dismissed as mere “context of discovery” (Reichenbach, 1938), an objection totally alien to Skinner’s approach to metatheory, as shown by his remark on logic in the 1945 symposium: “... If it turns out that our final view of verbal behavior invalidates our scientific structure from the point of view of logic and truth-value, then so much the worse for logic, which will also have been embraced by our analysis” (1961, p. 282).

WHY DOES IT MATTER?

Translating—and, when that’s impossible, distinguishing—theories created by two very different sorts of intellect who relied on almost totally disjunct factual bases (except where each appeals to common knowledge) may seem rather pointless. I do not propose a detailed translation of Murray-Skinner, which would serve no more useful purpose than the “botanizing of reflexes” that Skinner rejected (1938, pp. 10, 46). The present meta-discussion, with examples, has four purposes. First, in honoring a teacher from whom I learned much, including how to clarify and test my own views when faced with his tough-minded disagreements and ingenious translations, I hope to defuse some total rejections of Skinner by psychologists of other traditions who mainly know about his

metatheoretical strictures and dismiss his substantive contributions as “mere technology.” (A technology this powerful can hardly be brushed off as “mere,” can it? I once trained a pet specimen of *Felis catus* to sit up, shake paws, and jump through a hoop, to the astonishment of dog lovers. It was not my Ellis or Murray side that enabled me to do this!) So I write partly in the spirit of “doing historical justice” to a pair of major contributors.

Second, there is some theoretical interest, and even more metatheoretical value, in examining two influential conceptual systems with an eye toward future integration. But, as Skinner said of behavioral science and neurophysiology, we should not force that by premature efforts.

Third, I think our effectiveness in dealing with societal problems hinges on our ability to use both kinds of approach. For instance, drug abuse is of great social importance today. If the “war against drugs” is to be more than politicians’ rhetoric, medical and social scientists must provide a preventive and intervention technology that is grounded in causal understanding. No society has ever succeeded in preventing humans from seeking sex, gambling, or using mood altering chemicals. But we would like to prevent drug use in its present epidemic proportions. The conventional psychodynamic emphasis on anxiety-reduction (and uncovering psychotherapy to “alleviate the underlying source”) is no longer widely held, but this mechanism is doubtless involved for *some* abusers of *some* drugs. We do not presently understand the state called “craving”—is it *merely* a matter of strong (powerfully reinforced) behavior, or does it possess state-variable properties? Is the “emotional” state-variable *anxiety* an essential component of craving? These and many related questions will be better researched if psychodynamic clinicians and experimental psychopharmacologists are on good speaking terms, with neither group commanding or forbidding classes of investigable conjectures.

We should surely start by thinking in Skinner’s terms, because the perennial, ubiquitous, inevitable attraction of sex, gambling, and drugs is easily understood when we consider (a) they are qualitatively potent reinforcers, (b) two are on rich (nearly CRF) and one (gambling) on VI schedules, (c) there is temporal immediacy, and (d) the instrumental requirements are minimal. (Compare the ease, certainty, and intensity of a chemical “rush” with studying for a calculus midquarter examination—let alone learning to play the violin!) Within that broadly Skinnerian framework, it would be unwise strategy to exclude state-variables (including anxiety) from consideration. While our concern with inferred inner states arises mainly from the fact that humans talk about them, powerful methods of distinguishing and quantifying one class of inner states have been developed by operant behaviorists studying the behavioral pharmacology of infrahuman animals (see, e.g., Morse, Goldberg, & Katz, 1985; Stewart & de Wit, 1987; Young & Sannerud, 1989). Morse, *et al.* contrast “some early behaviorists” (in their blanket rejection of introspective reports on inner states) with “the modern emphasis [which] derives from B. F. Skinner” (p. 150), improving the reliability and validity of reports of introspections and feelings by differentially reinforcing them. In the Minnesota laboratory, in an ingenious experiment Lubinski and Thompson (1987) taught pigeons to communicate with one another by tacting their drug-induced inner states (see also their clarifying theoretical discussion, Lubinski & Thompson, 1993 [cite updated]).

Fourth, speaking as a practitioner, I would think it unfortunate if most psychoanalytic therapists could hardly engage in discussion with an operant behaviorist, and if neither

group would much want even to try. After a half-century of clinical practice, I have come to believe that most of our (nonpsychotic, outpatient) clientele, especially those of Schofield's (1964/1986) YAVIS syndrome ("young, attractive, verbal, intelligent, successful"), suffer from a combination of psychodynamics, irrational life postulates, and inadequate instrumental behaviors. These three are not equally important over all patients, but very few persons seeking help are totally free of any one. Freud's biggest error, I opine, was to conceive the mind as a repository of memories, drives, and emotions, but hardly at all as one of *beliefs* and *habits*. These latter are relegated to a rather minor subdivision called "cognitive and executive functions of the ego." The notion is that when the infantile material is dredged up and the defenses so attrited that the ego can "decide" what to do, it can proceed effectively. This is, alas, just not so empirically. Of course, from the standpoint of experimental psychology (learning, motivation, perception) there is no reason to expect such a result. Instrumental behavior (including interpersonal) has to be strengthened, shaped, chained, brought under "realistic" discriminative control, appropriately drive-linked, etc., and the results are based on complicated stochastic relations imposed by the social environment. If a neurotic's defensive system and infantile strivings have largely prevented effective behavior from being emitted and reinforced, the attrition of defense and the labeling of motives has little or no tendency to provide the behavior. On the other hand, I believe that some (not all) of the resistance to effective self-disputation in Rational Emotive Therapy has psychodynamic sources that Ellis, having abandoned his early Freudian orientation, does not consider.

My admittedly informal "clinical trials" lead to the following conjecture: there is a quantitative asymmetry in the causal influence among (a) psychodynamics, (b) irrational postulates, and (c) ineffective behavior among YAVIS patients. While all $2\binom{3}{2} = 6$ pairwise two-way influences operate for bad (or good, with therapeutic success), the adverse influences $a \rightarrow b$, $a \rightarrow c$, $b \rightarrow c$ are much stronger than those in the reverse directions. This suggests a future integrated therapy that begins psychoanalytically, shifts to Rational Emotive Therapy, and concludes with behavior modification. (Elaboration of this proposal must be left for another paper.) Whether one therapist can do all three I do not know, although I have for some years tried combining the first two with a number of patients. Analyst Wachtel (1977, 1982) has written two provocative treatises on psychodynamic and behavior therapy. I find very few of either behavioral or psychoanalytic therapists ever heard of analyst Alexander Herzberg's fascinating *Active Psychotherapy* (1945) which combines analysis with the "Method of Tasks," classifying the tasks assigned under various theoretical headings (e.g., tasks directed against maintaining impulses, against secondary gains, against obstacles to satisfaction). It is well known that Freud himself, in discussing phobias, says that the patient must sometimes be induced to put himself in the feared situation, even after the phobia has been analyzed. None of these questions about intervention can be answered unless therapists and theoreticians of different orientations at least feel comfortable talking to each other. Some therapists do not feel anxiety-free even when *reading* views different from their own, so they simply avoid such reading. This is surely unfortunate all round, both for the development of theory and the improvement of therapy. I have tried here to alleviate some of those doctrinal anxieties, by showing that the clash between two major thinkers is to a considerable extent in their meta-theoretical polemics, understandable in terms of the state of things a half-century ago, including philosophy of science. For one central psycho-

dynamic concept, the Murray need, I have argued that it is fairly close to a Skinner state-variable, as he defended the latter in his 1938 book.

A reader of this article in draft attributed to me the idea that Murray and Skinner did not really disagree about anything. That is, of course, absurd, as several of the Skinner quotations should suffice to show. They disagreed a great deal, and strongly. My position is that at a deeper level, *operating with a sophisticated philosophy of science*, Skinner's 1938 concept of a drive (and his criteria for identifying one) is *at its core* very similar to Murray's 1938 concept (and identifying criteria) for a need. To this thesis I adjoin several correlated claims that help us explain the polarization as conventionally received (e.g., parametric features of Skinner box schedules versus adult human ecology, the antagonists' meta-talk stemming from the centralist/peripheralist controversy of the 1930s, and some genuine inconsistencies within Skinner's own formulations, especially with respect to Freud's ideas and the category of internal events).

During the last year of Skinner's life, I had a too brief correspondence with him about writing a paper on the state-variable problem, in which I complained that he never made a real rebuttal to his 1938 defense of them. We disagreed about my schedule-stretching argument above, and he reiterated his dislike for using 'hunger' as a noun. But he concluded with "Unlike most of my critics, you—as usual—know how to focus on the important questions." Reception of this pellet had two effects. The short term effect was a satiation of my *n Recognition* for several days. The long term effect was to strengthen the verbal operants that comprise this paper.

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