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UNDERSTANDING THE "COGNITIVE REVOLUTION" IN PSYCHOLOGY

JOHN D. GREENWOOD

In this paper it is argued that the "cognitive revolution" in psychology is not best represented either as a Kuhnian "paradigm shift," or as a movement from an instrumentalist to a realist conception of psychological theory, or as a continuous evolution out of more "liberalized" forms of behaviorism, or as a return to the form of "structuralist" psychology practiced by Wundt and Titchener. It is suggested that the move from behaviorism to cognitivism is best represented in terms of the replacement of (operationally defined) "intervening variables" by genuine "hypothetical constructs" possessing cognitive "surplus meaning," and that the "cognitive revolution" of the 1950s continued a cognitive tradition that can be traced back to the 1920s. © 1999 John Wiley & Sons, Inc.

In this paper I provide a characterization of a recent historical episode that I believe has been much misunderstood: the so-called "cognitive revolution" in psychology. Although it is a matter of debate whether there was a genuine "revolution" in the usual sense in which this term is employed in the history of science (the dramatic overthrow and replacement of prior theories and methods), I believe it is important to recognize that the advent of cognitive theories in the 1950s did mark a fairly radical theoretical discontinuity, and precisely the sort of theoretical discontinuity that is characteristic of many revolutionary episodes in the history of science (Cohen, 1985).

My argument, however, is not really intended as a contribution to the general "continuity versus discontinuity" debate about the development of the cognitive revolution. General debates about whether significant episodes in the history of science constitute a progressive and continuous development or a discontinuous transformation are not particularly illuminative, as Peter Galison (1997) has forcefully argued. Galison has noted that radical breaks in theoretical practice have occurred during periods of continuity in experimental or technological practice (and vice versa), and has suggested that the history of a discipline as a whole is better represented as "an irregular stone fence or rough brick wall rather than as adjacent columns of stacked bricks" (1997, p. 19). This seems to be especially true of the history of psychology. Significant historical episodes represent continuities as well as discontinuities, and on a variety of different levels: the task of the historian is to discern their contingent interrelation or "intercalation" (Galison's term) at different periods. Thus, although the account offered in this paper is critical of competing accounts of the cognitive revolution in terms of continuous development versus radical discontinuity, its main goal is to suggest some neglected discon-

JOHN D. GREENWOOD *is professor of philosophy and psychology at City College and the Graduate School of the City University of New York. Educated at the Universities of Edinburgh and Oxford, his teaching and research interests are in the philosophy and history of social and psychological science. His most recent works include* Realism, Identity and Emotion: Reclaiming Social Psychology *(1994) and (as editor)* The Mark of the Social: Invention or Discovery? *(1997). Address for correspondence: Department of Philosophy, City College, 138th Street & Convent Avenue, New York, NY 10031. E-mail: greenjd@erols.com*

tinuities and continuities that hopefully improve our understanding of this complex but very significant episode in the history of modern psychology.[1]

To endorse Galison's historiographical perspective is not, however, to accept that all putative historical accounts of the cognitive revolution merely describe some aspect of it, and in this paper I reject some traditional, reactionary, and revisionary accounts. Many traditional accounts treat the cognitive revolution as a radically discontinuous break with the hegemony of behaviorism, in which a novel cognitive "paradigm" displaced the previously entrenched behaviorist "paradigm." Many scholars treat the cognitive revolution in psychology as having essentially evolved out of independent developments in computer science and linguistics, while some have suggested, and others have complained, that it marked a return to the form of "structuralist" psychology practiced by Wilhelm Wundt and Edward B. Titchener in the early decades of the twentieth century. More recent revisionary accounts have argued that the cognitive revolution developed naturally and continuously out of the increasingly liberal attitudes to "internal" cognitive states adopted by later behaviorists. I believe that these accounts are seriously flawed, and in this paper I try to explain why.[2] In their place, I offer an account of the cognitive revolution (albeit partial and provisional) in terms of a neglected discontinuity between behaviorism and cognitive psychology with respect to the content of theories, and a neglected continuity with a tradition of cognitive theory (distinct from "structuralism") that began in the early decades of the twentieth century.

Paradigms and Theoretical Realism

The movement from behaviorism to cognitivism that is often characterized as the cognitive revolution is not best represented in terms of a Kuhnian "paradigm shift" (Lachman, Lachman, & Butterfield, 1979; Palermo, 1971; Weimer & Palermo, 1973) in which one theoretical paradigm gives way to another under the pressure of an empirical anomaly or set of anomalies (Kuhn, 1970).[3] The various anomalies that eventually faced behaviorism, such as the "discovery" of biological limits on conditioning (Breland & Breland, 1961; Garcia & Koelling, 1966), and doubts about the ability of conditioning theory to accommodate linguistic performance (Chomsky, 1959; Lashley, 1951), did not result in the abandonment of the central principles of operant or classical conditioning theories—the core theoretical elements of the behaviorist paradigm. Moreover, behaviorists continued to maintain their in-house journals, their own APA division, and a sizable professional membership (Leahey, 1997). Nor were these recognized anomalies the primary stimulus for the development of cognitive theories in the 1950s, which was provided by outside developments in artificial intelligence and the computer simulation of cognitive abilities (Baars, 1986; Gardner, 1985).

1. It is, of course, an episode that continues to develop, and in historically interesting ways. Some have claimed that the development of "connectionist" challenges to traditional "rules and representations" theories of cognition in the 1980s amounted to a second cognitive revolution (Rumelhart & McClelland, 1986; Schneider, 1987). Others have suggested that developments in connectionist theory and more recent dynamical systems theory have marked a return to earlier forms of associationist psychology (Haselager, 1997). I do not find either of these claims at all convincing, but consideration of them is beyond the scope of the present paper, which is focused on the initial development of the cognitive revolution. However, these important later developments will need to be accommodated by any comprehensive historical account of the progress of the cognitive revolution.

2. References for these different historical accounts are provided at the points that they are considered in the text.

3. As is well known, Kuhn himself thought that psychology was "pre-paradigmatic."

Certainly, the relation between behaviorism and cognitive psychology is not best represented as a conflict between competing and *exclusive* theoretical paradigms, on analogy with historical conflicts between, for example, the physical theories of Newton and Einstein in the early twentieth century, or between wave and particle theories of light in the early nineteenth century. The evidence that favored and led to the adoption of Einstein's theory and the wave theory of light appeared to demonstrate the general inadequacy of Newtonian theory and the particle theory of light, and thus led to their *complete rejection* by most scientists. Yet nobody—not even dedicated cognitivists—seriously imagined that either the anomalies noted above, or their theoretical biological and cognitive resolutions, demonstrated the general inadequacy of theories of classical or operant conditioning.[4] These recognized anomalies, and their theoretical biological and cognitive resolutions, only led to a delimitation of the scope of explanations in terms of conditioning (albeit long overdue), and the extension of underdeveloped biological and cognitive explanations to those domains for which conditioning theory had proved to be inadequate.

It was only because too many behaviorists grossly overestimated the scope of conditioning explanations, and presumed that conditioning theory enabled us (in principle, if not in practice) to explain virtually all forms of animal and human behavior, including complex forms of human behavior such as language, that behaviorism faced these empirical problems and challenges.[5] Behaviorists were not always so intellectually imperialistic, and biological limits on learning and the possible inability of conditioning (or "habit") theories to explain higher cognitive processes were recognized by the early pioneers of conditioning theory, such as Conwy Lloyd Morgan (1896), who conceived of such laws of learning as covering only a limited range of human behaviors. Indeed, one useful consequence of the cognitive revolution was to return conditioning theory—and comparative psychology in general—to the much more reasonable position advocated by Lloyd Morgan in the 1890s (B. F. Skinner and his "radical behaviorist" followers aside), which recognized possible qualitative differences between human and animal psychological processing, as well as obvious continuities between them. In fact, some neobehaviorists were already returning to that position, as the cognitive revolution was emerging, and as they began to engage more complex behaviors such as language or symbolic behavior (Kuenne, 1946; Miller, 1959; Osgood, 1957; Spence, 1937). "Morgan's Canon" (Morgan, 1894) was originally merely a caution against overestimating the application of cognitive explanations in the animal kingdom, transformed by theorists such as Edward L. Thorndike, John B. Watson, Clark L. Hull, and B. F. Skinner into an effective prohibition against cognitive explanations in both the animal and human realm.

The cognitive revolution is also not best represented as a revolution in terms of a paradigm shift with respect to attitudes towards theories, in the sense of a shift from an *instru-*

4. Because of this feature, the de facto maintenance of theories of classical and operant conditioning by contemporary behaviorists in the midst of the cognitive revolution does not itself represent a serious historical anomaly—as if there continued to be many supporters of the Newtonian theory in the midst of the "Einsteinian revolution" in physics. What would be historically anomalous is any behaviorist who still maintained that classical and operant conditioning can explain *every* form of animal and human behavior—he or she would be like a contemporary physicist who believed that Newton's theory is about to make a comeback and displace Einstein's theory.

5. See, for example, Clark L. Hull, who maintained in the *Preface* of *Principles of Behavior* (1943a, p. v):

> . . . that all behavior, individual and social, moral and immoral, normal and psychopathic, is generated from the same primary laws: that the differences in the objective behavioral manifestations are due to the differing conditions under which habits are set up and function.

mentalist to a *realist* conception of theories, that is, from the treatment of theories of cognitive and biological states and processes as mere linguistic instruments that facilitate the integration and prediction of empirical laws, to their treatment as theoretical references to putatively real cognitive and biological states and processes.[6] Although this is the usual historical account advanced by those in the cognitive science community (Baars, 1986), and popularized by Jerry Fodor (Fodor, 1975), it is of doubtful validity. It is quite clear that Hull (1943b) and Edward C. Tolman (1948), for example, were realists (in the above sense) about biological states (drives) and cognitive states (cognitive maps) respectively. Thus, to quote Tolman, for example:

> For the behaviorist, "mental processes" are to be identified and defined in terms of the behaviors to which they lead. "Mental processes" are, for the behaviorist, naught but inferred determinants of behavior, which ultimately are deducible from behavior. Behavior and these inferred determinants are both objectively defined types of entity. (Tolman, 1932, p. 3)

Not even the more radical behaviorists denied the existence of cognitive states. When John B. Watson (1925) equated thoughts with movements of the larynx, his central claim (following the Russian reflexologists Ivan M. Sechenov and Vladimir M. Bechterev) was that cognitive states are motor or behavioral responses rather than centrally initiated states, and thus non-candidates for the explanation of motor or behavioral responses. B. F. Skinner's objection to cognitive states, the existence of which he never denied, was based upon the redundancy of putative explanatory references to cognitive states (the "second link"), when these are operationally defined in terms of stimulus inputs (the "first link") and behavioral outputs (the "third link"):

> 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. (Skinner, 1953, p. 35)

Conversely, early cognitivists were, in fact, extremely wary about committing themselves to the existence of cognitive states. Roy Lachman, Janet Lachman, and Earl Butterfield (1979), for example, after reviewing dozens of experimental studies of perception, memory, linguistic processing, and the like, nonetheless concluded that it was too early to say if cog-

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6. It ought to be stressed that the term "realist" is employed in this paper only to mark the position that treats theoretical terms as putatively referential, as opposed to the "instrumentalist" position that treats theoretical terms as nothing more than (non-referential) linguistic instruments that facilitate the integration and prediction of empirical laws. It is not employed to mark any of the bewildering variety of different positions that are often also characterized as "realist": for example, positions in classical debates about the nature of universals, our knowledge of the existence and properties of physical objects in the external world, and in modern debates about whether the history of science demonstrates the general or approximate truth of contemporary scientific theories. The realist versus instrumentalist debate about the putative reference of scientific theories was the type of debate engaged by Andreas Osiander and Christopher Clavius over the status of the Copernican theory in the sixteenth century. Osiander was an instrumentalist who maintained that the Copernican theory was a useful and economical calculation device that did not purport to give a true description of the positions of the planets (and was thus not heretical). Clavius was a realist who maintained that the Copernican theory did purport to describe the true positions of the planets, and was false (and thus heretical). Similar disputes occurred over the status of the atomic and quantum theories in the nineteenth and twentieth centuries.

nitive states really exist! Some cognitive theorists, such as John Anderson (1981), continued to maintain an agnostic view. Some were simply inconsistent: Richard Nisbett and Lee Ross (1980), for example, after having appealed to dozens of experimental studies that they claimed demonstrated that persons regularly make inaccurate judgments *about* their cognitive states, then declared themselves agnostic on the question of whether there are any cognitive states!

THE REAL REVOLUTION: FROM "INTERVENING VARIABLE" TO "HYPOTHETICAL CONSTRUCT"

In consequence, it is perhaps natural and tempting to see the move from behaviorism to cognitive psychology as a progressive liberalization of attitudes toward theories, particularly theories postulating intersubjectively unobservable internal states. On this increasingly popular view, cognitive psychology is characterized as having emerged or evolved continuously out of more liberalized forms of neobehaviorism, which allowed for the introduction of "internal variables" such as "internal" or "mediating" r-s connections, so long as these were operationally defined (Amsel, 1989; Kendler & Kendler, 1975; Leahey, 1992; Miller, 1959). However, while it may be true that later behaviorists might have felt more comfortable and less apologetic about postulating internal states than their earlier fellows (although neither Hull or Tolman appear to have been especially reticent in this respect), this characterization also seems to be seriously flawed. There was a very significant difference between the types of "internal" states postulated by even the most liberal neobehaviorists and by cognitive psychologists from the 1950s onward.

It is true that only Watson and Skinner, for their diverse but essentially extreme positivistic reasons, were truly antitheoretical with respect to postulated internal states that are not intersubjectively observable. Most other behaviorists and neobehaviorists, and famously Hull and Tolman, allowed for the introduction of theoretical terms, or postulated "intervening variables," so long as these were, at least ideally, rigidly and exhaustively defined operationally, via principles or "laws" relating stimulus inputs to internal states and internal states to behavioral outputs (Bergmann & Spence, 1941; Hull, 1943b; Pratt, 1939; Stevens, 1935; Tolman, 1936). It was Kenneth MacCorquodale and Paul Meehl (1948) who recognized the serious inadequacy of this characterization of theoretical terms. If intervening variables are rigorously and exhaustively defined in terms of empirical laws, they cannot function as substantive (non-vacuous) explanations of empirical laws, or be creatively developed to generate novel empirical predictions. (As noted earlier, this was essentially the basis of Skinner's justified dismissal of cognitive intervening variables as vacuous—and thus predictively redundant—"explanatory fictions.") In order to generate substantive explanations and have the potential for further development, genuine theories, or "hypothetical constructs," must possess "surplus meaning."[7] Where this "surplus meaning" comes from in psychological science is a matter of some dispute, but that genuine theories poses such surplus meaning is not—for this is precisely what accounts for their explanatory power and creative predictive potential.

In natural science, this substantive surplus meaning is often created via the exploitation of theoretical models and analogies, as in Niels Bohr's "planetary" theory of the atom and the "wave" theory of light. The constructive resources of analogy and metaphor enable sci-

7. The notion of "surplus meaning" employed by MacCorquodale and Meehl derives from Hans Reichenbach 1938.

entists to introduce meaningful descriptions of intersubjectively unobservable phenomena by exploiting, via analogy and metaphor, the semantics of our descriptions of properties of familiar and discriminable systems (Campbell, 1920; Harré, 1970; Hesse, 1966, 1976; Boyd, 1979; Holyoak & Thagard, 1997; Gentner & Markman, 1997). This enables scientists to introduce meaningful theoretical descriptions such as "electric charge" (on analogy with a charge of gunpowder), "curvature of space" (on analogy with curvature of a sphere), "and so on by continuous steps to the most esoteric terminology of modern physics" (Hesse, 1976, p. 8).

This account can be extended to theories in cognitive psychology, many of which postulate representational states—such as memories and beliefs—that are held to have some of the semantic and syntactic properties of linguistic structures. Indeed, such an account has been explicitly developed by Richard Boyd, with respect to "computational" theories in cognitive psychology:

> A concern with exploring analogies, or similarities, between men and computational devices has been the most important single factor influencing postbehaviorist cognitive psychology. Even among cognitive psychologists who despair of the actual machine simulation of human cognition, computer metaphors have an indispensable role in the formulation and articulation of theoretical positions. These metaphors have provided much of the basic *theoretical vocabulary* of contemporary psychology. (Boyd, 1979, p. 360, my emphasis.)

It is in precisely this area that we seem to find the very real discontinuity between even "liberalized" forms of neobehaviorist theory and cognitive theories developed from the 1950s onward. Liberalized forms of behaviorist theory never really possessed significant surplus meaning. Concepts such as "drive," "habit strength," "divergent habit family hierarchy," "pure stimulus act," and the like, and all the internal variables of "mediation theory," were provided with rigorous operational definitions, no matter how awkward and unwieldy they proved to be. Even those neobehaviorists who rejected the extreme antitheoretical or atheoretical positions of Watson and Skinner maintained that surplus meaning had no place in psychological science. They insisted that "the only meaning possessed by intervening variables is their relationship to both independent and dependent variables" (Kendler, 1952, p. 271) and that "valid intervening variables . . . are the only kinds of constructs admissible in sound scientific theory" (Marx, 1951, p. 246).

In contrast, cognitive theories from the 1950s onwards did possess surplus meaning—despite the frequent *rhetorical* avowals of a commitment to the operational definition of theories by cognitive psychologists. Such avowals in practice amounted to nothing more than a recognition of the need to provide empirical operational *measures* of cognitive constructs, and a means of testing predictions derived from them (in conjunction with a variety of auxiliary hypotheses and background assumptions). Alan Newell, Clifford Shaw, and Herbert Simon (1958), for example, advertised their computational theory of problem solving as a "thoroughly operational theory of human problem-solving." However, all that Newell, Shaw, and Simon provided was a set of behavioral predictions derived from their theory and a means of empirically assessing them. Neither the axioms of logic described by the theory (adapted from Bertrand Russell and Alfred N. Whitehead [1925]), or the rules of the sentential calculus employed in the derivation of theorems, were defined operationally.

While theoretical definitions of the sensory register, attention, long- and short-term memory, depth grammar, cognitive heuristics, visual perception, propositional and imagery coding, episodic and semantic memory, template-matching, procedural networks, inference, induc-

tion, and the like have abounded in the cognitive psychological literature, operational definitions—as opposed to specified operational measures—of these phenomena have been virtually non-existent. So there is a significant discontinuity between behaviorist and cognitive theories. To put it bluntly, so-called liberalized neobehaviorist "theories" did not, in general, constitute genuine and substantive psychological theories at all.[8] Cognitive psychological theories from the 1950s onward generally did.

The neobehaviorist error was to confuse the reasonable requirement for operational measures of postulated theoretical states and processes with the peculiar notion that the meaning of theoretical descriptions must be (can only be) specified in terms of such operational measures—a confusion neobehaviorists inherited from scientific empiricist philosophers of science such as Rudolph Carnap (1936, 1937).[9] This confusion is very clear in neobehaviorist articulations of the supposed rationale for operational definitions, in terms of the need to provide a determinate meaning for theoretical descriptions:

> It is evident that this equational mode of anchoring symbolic constructs to objectively observable and measurable antecedent and consequent conditions or phenomena is necessary, because otherwise their values would be indeterminate and the theory of which they constitute an essential part would be impossible of empirical verification. (Hull, 1943, p. 273)
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> . . . an operational psychology will be one that seeks to define its concepts in such a manner that they can be stated and tested in terms of concrete repeatable operations by independent observers . . . The behaviorism I am going to present seeks, then, to use only concepts which are capable of such concrete operational verification. (Tolman, 1951, p. 89)

Although it is, of course, desirable that theories have determinate meaning, their determinate meaning need not be (and rarely is) fixed via operational definition. The meanings of the theoretical descriptions employed in Bohr's theory of the atom, for example, were determined—via theoretical modeling—independently of the operational measures employed in the empirical tests of the predictions derived from Bohr's theory of the atom (in conjunction with "auxiliary hypotheses" and other background assumptions). Of course, it is obviously also desirable that such operational measures be employed in the empirical testing of scientific theories, but these two desiderata—determinate meaning and operational measures—are quite independent (and are generally recognized to be so in the natural and biological sciences).

Operational measures of theoretical constructs such as "electron" or "short-term memory" do not in general determine the meaning of such theoretical constructs, as evidenced by

8. For an early statement of this point, see Kurt Lewin (1940), who complained that although Hull's theoretical constructs were well defined operationally, they lacked conceptual properties.

9. Thus S.S. Stevens (1935, p. 517), claimed that "a term or proposition has meaning (denotes something) if, and only if, the criteria of its applicability or truth consist of concrete operations that can be performed." Laurence D. Smith (1986) has convincingly argued that Hull and Tolman essentially developed their theoretical views independently of the influence of logical positivism (which later came to be called "scientific empiricism"), appealing to the positivist account as a sort of post hoc justification of their cautious theoretical positions. I think there is a fair degree of truth in this (although I believe Smith's case is somewhat overstated). But, it also seems very clear that the general positions of Hull and Tolman, like the positions of Watson, Skinner, Kendler, Osgood, Miller and just about every other avowed behaviorist or neobehaviorist, are clearly positivist in spirit, insofar as they treated intersubjectively observable behavior as the bedrock of scientific psychology, and rejected any form of putative theoretical description of unobservables that was not exhaustively defined in terms of observables. It is not essential to my account, however, that the types of theories introduced by neobehaviorists were actually influenced by the logical positivist account—it is sufficient that, as most neobehaviorists (correctly) maintained, their theories did generally satisfy the restrictive criteria of the logical positivist account.

the fact that the meaning of such theoretical constructs remains invariant when different operational measures are employed in different empirical or experimental contexts. No one supposes (at least, not these days) that theoretical references to electrons, for example, change their meaning from experiments in which electrons are indicated via spectroscopy to experiments in which they are indicated by tracks in a Wilson cloud chamber, or that theoretical descriptions of short-term memory based upon experimental studies employing verbal measures differ in meaning from theoretical descriptions of short-term memory based upon experimental studies employing written measures.[10] Many neobehaviorist reconstructions of the cognitive revolution correctly stressed the genuine cognitivist commitment to operational measures and empirical evaluation. Yet although cognitive psychologists were committed to operational measures and empirical evaluation, they were not constrained by neobehaviorist restrictions of the content of theories to the content of operational definitions of theoretical constructs.

If the content of theoretical constructs were, in fact, restricted to the content of their operational definitions, as maintained by neobehaviorists, theories would be incapable of substantive explanation of the empirical predictions that can be derived from them, and incapable of generating novel predictions. To illustrate this point, it is worth considering Howard Kendler's famous claim that the dispute between supporters of Hull and Tolman concerning "what is learned" in maze-running by rats was a "pseudo-issue," because both Hull's theory in terms of stimulus-response connections and Tolman's theory in terms of "cognitive maps" were semantically equivalent, being operationally defined in terms of the same observable stimulus and response variables:

> . . . the construct of learning, whether it be conceived in terms of modifications in cognitive maps or S-R connections, does not refer to an object, thing, or entity as is suggested by those who are concerned with the question of what is learned. These intervening variables possess no meaning over and above their stated relationships between the independent and dependent variables. The basic error underlying the problem of what is learned is the assumption that these intervening variables are entities capable of being described and elaborated on, independently of their operational meaning. (Kendler, 1951, p. 271–272)

If this really were the case with respect to the theories of Tolman and Hull (which is in fact doubtful, especially in the case of Tolman), then it would have stifled the empirical evaluation and development of these theories, as of course it would with respect to *any* two competing theories that shared the same explanatory empirical domain. The absurdity of this position, accepted by many neobehaviorists, is perhaps best illustrated by considering an equivalent suggestion to the effect that there was nothing at issue between the Copernican and Ptolemeic astronomical theories, or the wave and particle theories of light, just because they shared the same observational explanatory domains (the same empirical predictions could be derived from both theories). The differences between the Copernican and Ptolemeic astronomical theories, and the wave and particle theories of light, were precisely differences in surplus meaning, and it was the "elaboration" of these differences that led to the different predictions (about the stellar parallax and the speed of light in air and water) that eventually enabled these theoretical disputes to be adjudicated effectively. The actual inconclusiveness of the original Hull-Tolman debate is probably best explained in terms of the contingent

10. Percy Bridgman (1927) toyed with this idea but quickly abandoned it.

temporary ability of both theories to accommodate failed predictions via auxiliary hypothesis modification—the best explanation of the long periods of time during which the debates between the Copernican and Ptolemeic astronomical theories, and the wave and particle theories of light, were also empirically undecidable (as Kendler [1981, pp. 315–316] later appeared to admit).

However, I do not wish to belabor the claim that neobehaviorist theories were not really theories at all, because they never really functioned as hypothetical constructs. This is because, for present purposes, the claim is both too strong and too weak. It is too strong because, as MacCorquodale and Meehl (1948, p. 99) themselves note, even an implicit existential claim about the existence of an internal "mediating" state is sufficient to transform a putative internal variable into a hypothetical construct, and it is clear that Hull, Tolman, Charles Osgood, Neal Miller, and other neobehaviorists were realistically committed to the existence of causally potent internal states. It is too weak insofar as my main point (for present purposes) is not merely that neobehaviorist theories lacked significant surplus meaning, but that they specifically lacked significant *cognitive* theoretical meaning.

As Skinner has never tired of pointing out, a mere reference to internal states defined in terms of observable stimuli and observable behavioral responses is vacuous as an explanation of behavior and provides no basis for novel prediction. In order for theories of internal states to constitute significant theories providing substantive explanations and capable of generating novel predictions, they must possess surplus meaning additional to mere existential commitment to causally potent internal states.[11] In order for such theories to constitute cognitive theories, this "surplus meaning" must be cognitive in nature.

However, even the most liberalized forms of neobehaviorist theory were never "cognitive" in the sense in which this term is generally employed within cognitive psychology. Whatever surplus meaning these theories may be said to have had, it was not cognitive in nature. Although much (and perhaps too much) has been made of the difficulty of providing a universally acceptable definition of cognition or cognitive theory (Amsel, 1989; Denny, 1986; Dinsmoor, 1983; Jenkins, 1980), for present purposes at least, a cognitive theory may be reasonably defined (following Fodor, 1991) as any theory that postulates representational states that are semantically evaluable—that can be characterized as true or false, or accurate or inaccurate—and rules, heuristics, or schemata governing the operation of such representational states, as they are held to be involved in receiving, processing, and storing information. By this measure, most of the states that have been postulated by cognitive psychologists since the 1950s have been unproblematically and unambiguously cognitive: perceptions, concepts, beliefs, memories, and even images can be characterized as having contents evaluable as true or false (or accurate or inaccurate), and are held to be processed according to transformation rules, representative and availability heuristics, disjunctive and conditional rules, and so forth. Most of the internal variables postulated by neobehaviorists—such as "drive," "habit strength," "divergent habit family hierarchy," "pure stimulus act," and the like—were not cognitive in this sense, with one obvious exception. Tolman's "cognitive maps" are semantically evaluable—for this reason, Tolman's system was often justly treated as a precursor of contemporary cognitive psychology.[12] It also seems fairly clear that the

11. Neobehaviorists such as Melvin Marx (1951, p. 246) indirectly admitted as much by granting the heuristic role of "surplus meaning," while insisting in the same breath that it ought to be eliminated from "sound scientific theory"!

12. As was the work of I. Krechevsky (1932) on discrimination learning in rats in terms of "hypotheses."

theoretical concept of a cognitive map is not capable of exhaustive operational definition. Tolman, in fact, never managed (or even seriously attempted) to provide one, and in later years maintained that his theoretical references to cognitive maps were "hypothetical constructs" rather than "intervening variables."[13]

Hull (1920), for example, claimed to study the formation of "concepts": the developed ability of children to learn "a 'meaning' for the word 'dog'." Hull operationally defined "concept formation" in terms of stimulus discrimination and verbal association: a child was held to grasp the meaning of the concept "dog" when he or she could discriminate "dog" stimuli and associate the verbal label "dog" with them. Consequently, Hull's studies of concept formation were essentially studies of discrimination learning and associated verbal labeling. They naturally followed the pattern of learning for any associative habit (Goldstein & Scheerer, 1941), whether or not the child—or parrot, for that matter—grasped the meaning of the verbal label "dog" (and ignored the commonplace distinction—in contemporary cognitive psychology—between concept meaning and word meaning).

In fact, if this particular form of associative learning that happens to employ verbal responses counts as concept formation, then virtually all forms of discriminative learning count as instances of concept formation. This demonstrates that it was not concept formation per se that was being studied—only at best a possible precursor to it. Hull's operational definition of a concept in terms of a conditioned verbal response was quite different, for example, from the genuinely cognitive theoretical definitions of a concept advanced by Homer Reed (1946), in terms of a word or idea that stands for any one of a group of things, or by Edna Heidbreder (1946, p. 173), as a "logical construct which, through signs or symbols or both, is transferable from situation to situation and communicable from person to person."

It is true that neobehaviorists (and neobehaviorists other than Tolman) did employ terms like "cognition," "thought," "purpose," "representation," "meaning," and the like. However, this is insufficient to establish that neobehaviorists employed genuinely cognitive theories, for when defined purely operationally, such terms bore little relation to their later employment in cognitive psychological theoretical discourse (or to their employment in everyday "folk-psychological" discourse). Hull's (1930) "pure stimulus act," the "internal substitute stimulus" that supposedly serves to enable organisms to respond to the "not here and not now," may indeed increase an organism's degrees of (behavioral) freedom, but this hardly suffices for an internal stimulus state to count as "thought" or "symbolism" (however rudimentary).

The "pure stimulus act," like Osgood's (1953, p. 695) "representational mediation process" (itself derived from Hull's notion of a "pure stimulus act") may elicit a behavior (or "fractional portion of a behavior") normally elicited by a stimulus, in the absence of that stimulus (or the whole stimulus sequence), or prior to its appearance. However, that causal property is not sufficient for thought, or representation, or meaningful use of verbal labels associated with the original stimulus, since the causal property appealed to is just the causal property of *any* classically conditioned stimulus (Osgood, 1957, p. 96). An "internal stimulus s," or an "external verbal stimulus S" associated with an original stimulus situation S (the label "hammer" associated with hammers, for example), only "means" or "represents" an object or associated behavior in the sense that a tone "meant" or "represented"—or was a "sign" for—food or salivation for Ivan Pavlov's dogs.

13. See Edward C. Tolman (1949, p. 49): "to use MacCorquodale and Meehl's distinction, I would now abandon what they call pure 'intervening variables' for what they call 'hypothetical constructs'."

That is, "pure stimulus acts"/"rudimentary thoughts" or "representational mediational states" only "mean" or "represent" objects or behaviors insofar as they are (conditioned) causal indicators or anticipators of objects or behaviors. Such internal states "represent" or "mean" objects or behaviors only in the fashion that smoke may be said to "represent" or "mean" fire, or dark clouds may be said to "mean" or "represent" rain—they are causal signs for fire or rain. Osgood (original emphasis) explicitly acknowledged this:

> The professor comes out upon his doorstep one morning and, looking up, sees dark rain-laden clouds; he ducks back into his house and forearms himself with rubbers and umbrella before setting out for the campus. Are the *dark clouds* a sign of rain? The response made to them as a stimulus is again relevant to something else, and we may legitimately say that the dark clouds signify or mean rain . . .
>
> What is it that is common to the learning situations that involve sign-processes and yet lacking in those situations that do not? *The distinguishing condition is the presence or absence of a representational mediation process in association with the stimulus*. The buzzer elicits an "anxiety" reaction—part of the "fear" response originally made to the shock—and it is by virtue of this fact that it means or represents shock. (Osgood, 1953, p. 695)

Yet although, no doubt, thoughts or shouted warnings about bulls in the field may sometimes function as causal indicators of forthcoming bulls in the field, this is a purely contingent feature of very few conceptual representations and meaningful linguistic utterances, not a necessary or defining feature of representational or meaningful states. Representational states involved in thought, memory, judgment, inference, and the like are symbols *of* objects, not causal signs for them. The concept of "buzzer" and the word "buzzer" represent and mean *buzzers*, independently of whether or not they have come to be associated via conditioning with shock. Even when they *have* come to be associated with shock, the concept and word continue to represent buzzers and not shock. In short, there was simply nothing cognitive or representational about the "internal stimuli" postulated by neobehaviorists, which is to say that postulating internal states with causal properties is not equivalent to postulating cognitive states, whatever one cares to call them.

Hull, like many contemporary neobehaviorist apologists, appears to have thought that a cognitive or representational state has automatically been described if one has described an internal causally potent state that could be instantiated in a mechanistic device to produce behaviors formally equivalent to those produced by human cognitive agents (a " 'psychic' machine") (Hull, 1930, p. 517; 1937, p. 29). Yet this is plainly not the case. Although programmed computers may be able to reproduce cognitive states and consequently produce artificially intelligent behavioral outputs, traditional mechanical dolls (or the hydraulic statues in the Royal Gardens at St. Germain that so impressed René Descartes) clearly do not have this capacity. Hull (1937, p. 31) also seems to have supposed that the only alternative to his conditioning theory of cognition was an appeal to "non-physical" entities, but of course it is not, as the cognitive revolution has itself clearly demonstrated.

The Cognitive Revolution and Structuralism

While the cognitive revolution was real enough, and represented a fairly radical form of theoretical discontinuty, it is also important to stress that the cognitive revolution did not mark any return to earlier forms of "introspective" or "structuralist" psychology, as practiced by Wundt and Titchener, as Skinner (1985) and more reactionary neobehaviorists (Amsel,

1989) were inclined to claim.[14] Wundt and Titchener's forms of psychology were tied to two related eighteenth-century dogmas about cognition. The first was the view that concepts—and thus concept-employing cognitive states—are essentially imagistic in nature (being derived from sensory experience).[15] The second was the view that all such states are necessarily conscious (in the sense that imagistic phenomena were held to be necessarily conscious).[16] It was precisely because of the identification of cognition and conscious images that the work of Oswald Külpe and the Würzburg School created so much of a theoretical problem for the form of psychology practiced by Wundt and Titchener. Cognitive theories in the structuralist tradition could not accommodate the discovery that some subjects engage in cognitive tasks without any awareness of images (Mayer & Orth, 1901), which generated the so-called "imageless thought" controversy (Ogden, 1911; Woodworth, 1906).

In contrast, the form of cognitive psychology that developed from the 1950s onwards did not identify cognitions and images, and did not presume that all cognitive states and processes are conscious (including images). On the contrary, one of the achievements claimed by contemporary cognitive psychology has been the experimental demonstration that persons have limited introspective access to their own cognitive states and processes: "The accuracy of subject reports is so poor as to suggest that any introspective access that may exist is not sufficient to produce generally correct or reliable reports" (Nisbett & Wilson, 1977, p. 233).

This is not to say that cognitive psychologists dismissed introspective or verbal reports as nothing more than forms of behavior (Watson, 1925), or as mere sources of cognitive hypotheses having no evidential value. Their attitude was rather more complex than that of traditional behaviorism or neobehaviorism: the question of whether particular forms of subject "protocols" in specific experimental situations have evidential value was held to be a matter that can be decided only in a "bootstrapping" fashion by developed cognitive theory based upon experiments (Ericsson & Simon, 1980).

Nonetheless, although the development of cognitive psychology since the 1950s did not mark a general return to the forms of theory and experimental practice of structuralist psychology, it is worth stressing that it really did mark a return to (or reemphasis of) the *subject matter* of structuralist psychology: namely, the nature and operation of cognitive processes. Like earlier structuralist psychologists, cognitive psychologists since the 1950s have aimed to develop and evaluate theories of cognitive processing. They have not merely aimed to develop cognitive explanations of behavior to replace traditional behaviorist S-R (or neobehaviorist r-s) explanations of behavior. That is, from the point of view of cognitive psychol-

14. Of course the term "introspective psychology," though commonly employed, is quite inappropriate as a description of the forms of experimental analysis of consciousness practiced by Wundt and Titchener. There are also very significant, but often neglected, differences between the systems of Wundt and Titchener (Blumenthal, 1975). However, the important differences between the systems of Wundt and Titchener are not of direct relevance to the present analysis. For convenience, in what follows I employ the term "structuralism" to describe the forms of experimental analysis of consciousness practiced by Wundt, Titchener, and their followers.

15. See, for example, David Hume (1739, p. 1):

> All the perceptions of the human mind resolve themselves into two distinct kinds, which I shall call *impressions* and *ideas*. The difference betwixt these consists in the degree of force and liveliness with which they strike upon the mind, and make their way into our thought and consciousness. These perceptions, which enter with most force and violence, we name impressions; and under this name I comprehend all our sensations, passions, and emotions, as they make their first appearance in the soul. By idea, I mean the faint images of these in thinking and reasoning.

16. See, for example, John Locke (1690, II, xxvii, 9): "Consciousness . . . is inseparable from thinking, and, as it seems to me, essential to it; it being impossible for anyone to perceive without perceiving that he does perceive. When we see, hear, smell, taste, feel, meditate, or will anything, we know that we do."

ogists since the 1950s, cognitive states and processes are as legitimate *objects* of theoretical explanation as are observable behaviors. They have not been treated merely as constructs introduced to facilitate the explanation of observable behavior (albeit realistically construed), as maintained by just about every behaviorist, neobehaviorist, and "mediational" theorist.[17]

Central to behaviorism as a methodological movement were two criterial principles: First, that intersubjectively observable behaviors, not intersubjectively unobservable thoughts, images or feelings, are the only legitimate subject matter of psychological science; second, that cognitive constructs are only legitimate in psychological science (if they were recognized as legitimate at all) as operationally defined internal states postulated to explain observable behaviors. The cognitive revolution in psychology was not just a movement to an increasingly more liberal approach to the introduction of explanatory cognitive constructs (in fact, as argued earlier, it was not really that at all). The cognitive revolution in psychology also involved the rejection of the behaviorist restriction of the subject matter of psychology to observable human behavior.

Cognitive psychologists, beginning in the 1950s, were as much (if not more) concerned with the explanation of cognitive processing as they were with the explanation of observable behavior. They wanted to know whether images are processed phenomenally or sententially, whether semantic memory is accessed via the activation of hierarchically related conceptual nodes or feature lists, whether errors in inference are due to interference or the employment of distorting heuristics, whether the prediction of the behavior of others is grounded in simulation of their cognitive processes or "information based" theoretical modules, and so on and so forth. These sorts of questions were scarcely even *raised* within behaviorism, far less seriously addressed. Moreover, it may be argued that there is a relation between the earlier point about the development of cognitive surplus meaning in contemporary cognitive psychological theories and the reemphasis on cognitive subjective matter: namely, that contemporary cognitive psychologists needed to introduce substantive cognitive theories in order to explain the complex cognitive processes that they took to be legitimate objects of psychological science, as opposed to the fairly elementary forms of adaptive animal behavior that formed the traditional focus of behaviorist psychologists.

This is not to suggest that cognitive psychologists neglected the critical evidential role of observable behavior in the empirical evaluation of cognitive theories of cognitive processes. As noted earlier, they did not; in fact, they often stressed that they employed appropriate behavioral operational measures (not definitions) of cognitive constructs (Mandler, 1979). However, it is also true that they have often focused on observable behavior only with respect to the evidential value of that behavior, as a means of assessing theories of human and animal cognitive processing. The latter fact is soundly bemoaned by contemporary neobehaviorists, who have seen this as a return to the sins of "subjectivism" and "anthropomorphism," supposedly exorcised by neobehaviorists via the application of Morgan's Canon. Thus, for example, Abram Amsel has complained that:

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17. This is true of even the most cognitive neobehaviorists such as Tolman. Consider Tolman's (1932, p. 3) well-known examples of the "molar" behaviors studied by behaviorist psychologists:

> A rat running a maze; a cat getting out of a puzzle-box; a man driving home to dinner; a child hiding from a stranger; a woman doing her washing or gossiping over the telephone; a pupil marking a mental-test sheet; a psychologist reciting a list of nonsense syllables; my friend and I telling one another our thoughts and feelings . . .

No mention of drawing an inference, solving an arithmetical problem, recalling the order of speakers at a wedding ceremony, or any other form of cognitive behavior or process.

> These animal cognitivists, who recognized a paradigm shift when they saw one, adopted the language and the models of human cognitive psychology and information processing. Consequently, instead of using animals as models for human function, they were now back to the kind of subjectivism and anthropomorphism the behaviorists had rejected: They were using animal behavior as a vehicle for their introspections, for understanding the mind, much as the artificial intelligence people do so elegantly with computers; and they were using humans as models for animal cognition. This use of humans as models for animals can happen *only in psychology*! (Amsel, 1989, p. 39)

There is, however, nothing particularly shocking about this at all—except from the standpoint of behaviorist mythology about Morgan's Canon. There is nothing intrinsically subjective about using a theoretical model (the wave theory of light is hardly subjective or unscientific just because it is based upon an analogy with the properties of water waves), and there is nothing intrinsically wrong with ascribing some of the properties of humans to animals—after all, humans have biological functions, drives, fears, and the like that are properly ascribable to animals, and have been ascribed to animals by behaviorists. Ascribing purpose, memory, or inference to an animal is no more or less a "vehicle" for "introspections" than ascribing a fear or respiratory function to it. The only basis for calling such cognitive attributions "subjective" or "anthropomorphic" in a pejorative sense is the supposed scientific illegitimacy of ascribing cognitive states to animals, according to Morgan's Canon.

Yet Morgan's Canon, as noted earlier, in the original formulation, did not rule out the theoretical attribution of cognitive states to animals, but only cautioned that this ought to be avoided when simpler explanations—in terms of "intelligent adaptation through association," for example—are available:

> In no case may we interpret an action as the outcome of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale. (Morgan, 1894, p. 53)

Although Morgan himself did not believe that animals (other than humans) perceive relations among phenomena or engage in "conceptual thought," he acknowledged that "there is a small . . . outstanding percentage of cases, the explanation of which seems to involve the attribution to animals of powers of perception and thought." He certainly did not think this supposition scientifically illegitimate, but rather a matter to be decided on the basis of "systematic investigation and carefully controlled experimental observations" (1894, p. 53). Morgan's charges concerning the subjective and unscientific nature of the work of George Romanes (1894) were based upon Romanes's over-reliance on anecdotal evidence, and his charge of anthropomorphism was based upon his judgment that Romanes had consequently exaggerated the degree to which cognitive states could be reasonably ascribed to animals on the basis of the available evidence—*not* upon any fixed belief about the illegitimacy of ascribing such cognitive states in the first place.

Amsel's contemporary criticism is just a restatement of Hull's critique of Kurt Lewin's attribution of cognitive states such as "expectancy" to animals: in the case of "inarticulate organisms," Hull (1943b, p. 287) complained, the attribution of such "subjective entities" would "seem to degenerate to sheer anthropomorphism."[18] This repetition of behaviorist mythology perhaps best illustrates the superficiality of neobehaviorist reconstruals or recon-

18. It is not surprising that Ansel's criticism in the 1980s was largely directed towards Robert Rescorla's work in the 1970s purporting to demonstrate that some form of "expectancy" is necessary for classical conditioning in animals (such as dogs) (Rescorla, 1978).

structions of the cognitive revolution, as supposedly comprising little more than the liberalized development of mainstream neobehaviorist theory, that ought to be continually monitored for unscientific overindulgence of cognitive theoretical content. Anyone who grasps the role of theory in science in general, and psychological science in particular, can have no *in principle* objections to the ascription of cognitive states to animals, whatever their own particular beliefs about the actual cognitive capacities of animals.

A Cognitive Tradition

Contemporary cognitive psychology is thus clearly distinguishable from cognitive psychology in the structuralist tradition, although it does mark a return to the subject matter of the earlier structuralist tradition. Nonetheless, contemporary cognitive psychology does not appear to have been created ex nihilo, or as a mere byproduct of developments in artificial intelligence and computer simulation. Even Newell and Simon (1972), the pioneers of computer simulation in psychology, acknowledged earlier psychological antecedents that played a significant role in the development of their logic and chess-playing programs. In fact, contemporary cognitive psychology appears to be continuous with a cognitive tradition of sorts that began around the time of the demise of the structuralist psychology of Wundt and Titchener.

The "imageless-thought" controversy is usually credited with playing a major role in the historical demise of structuralist psychology. However, at least as serious a problem for early cognitive theories in this tradition was their inability to provide any account of the processing of contentful (and thus semantically evaluable) cognitive states such as beliefs, memories, and the like. Early theories in the structuralist tradition simply lacked the theoretical resources to account for a wide range of linguistic phenomena and logical reasoning processes, namely, those that depend upon semantic and syntactic relationships. It was simply assumed, and regularly stated, that mechanisms such as association by contiguity and similarity could do the job, although no real evidence was presented by the original historical advocates of this view, such as Thomas Hobbes, David Hume, David Hartley, Herbert Spencer, James Mill, John Stuart Mill, and Alexander Bain.[19] Lots of instances of association (usually anecdotal) were documented, of course, but no systematic attempt was made to employ such principles in the explanation of reasoning processes, or to empirically evaluate such explanations.

For this reason, it may be suggested that it was the experimental studies of the Würzburg school on "determining tendencies" or "directed thought" (Ach, 1905; Watt, 1904) as much if not more than the discovery of "imageless thoughts," that really undermined structuralist

19. Locke was the exception here. He was alone among the classical empiricists in maintaining that the principles of the "association of ideas," based upon contiguity, similarity and the like, do not provide a general explanation of the processes of human reasoning. According to Locke (1689, II, xxxiii, 5), most of our reasoning is based upon semantic connections between ideas grounded in our empirical knowledge of the world:

> Some of our ideas have a natural Correspondence and Connection one with another: It is the Office and Excellency of our Reason to trace these, and hold them together in that Union and Correspondence which is founded in their particular beings.

The principles of the "association of ideas," such as contiguity and resemblance, apply only to "accidental" associations based upon "Chance and Custom," such as the association in some minds between "surfeiting with honey" and "Sickness, and Vomiting"; between "Goblines and Sprights" and "Darkness"; and between "the Pain they endured at School" and the "Books they were corrected for" (1689, II, xxxiii, 7). In this context, it is a little bizarre for historians of psychology to deny Locke "credit" for the discovery or systematic employment of associationist principles. Who needs it!

cognitive theories essentially based upon traditional associationist theories of thinking.[20] This is because such studies demonstrated that directive instructions can *override* idiosyncratic associations based upon images, and, moreover, would override them specifically when the instructions directed subjects to draw sentential inferences or explicate conceptual entailments (such as to state a subordinate or superordinate category).[21] As Oswald Külpe put it:

> The importance of the task and its effects on the structure and course of mental events could not be explained with the tools of association psychology. Rather, Ach was able to show that even associations of considerable strength could be overcome with a counteracting task. The force with which a determining tendency acts is not only greater than the familiar reproductive tendencies, it also derives from a *different* source and its effectiveness is not tied to associative relations (Külpe, 1964, p. 216.)

However, it is also true that Külpe and other members of the Würzburg school recognized the possibility of a cognitive psychology concerned with the processing of thought contents *independently of image association*:

> The fact that thoughts are independent of the signs in which they are expressed, and that they have peculiar and fluid interrelations, uninfluenced by the laws of the association of images, demonstrated their autonomy as a special class of conscious contents. (Külpe, 1964, pp. 212–213)

According to Külpe, the structure of our thought is determined by the structure of the task (which may of course be self-assigned rather than experimentally assigned), which results in the formation of a "determining tendency:"

> Such a task is not some ordinary type of reproductive motive. It must be accepted, the subject must report it, and it gives his activity a certain direction. Sensations, feelings and images are not given tasks; a task is set for a subject, whose central character does not dissolve into these contents, but whose spontaneity alone can adopt the instructions and execute them. Since in all thinking such determining viewpoints play a role, since abstraction and combination, judgment and conclusion, comparison and differentiation, the finding and construction of relations, all become carriers of determining tendencies, the psychology of the task became an essential part of the modern investigation of thinking. (Külpe, 1964, pp. 215–216)

Or, as one might fairly translate this into modern parlance, the psychology of rule-governed, intentionally directed and contentful representational activity, rather than the association of sensational images, became an essential part of the "modern investigation of thinking."[22] Moreover, although the Würzburg psychologists continued to talk of conscious-

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20. It is often maintained that one of the major differences between Wundt and Titchener is that Wundt rejected the British associationist tradition, whereas Titchener embraced it. This is true insofar as Wundt did not think "passive" principles of association capable of explaining higher cognitive functions such an analysis and judgment, which require "active" apperception involving a form of "creative synthesis." Nonetheless, he did maintain that associationist principles play a significant role.

21. The demonstration that inferences based upon content and syntax cannot be accommodated by essentially idiosyncratic associative links—one of the major achievements of the Würzburg School—was precisely what was forgotten by neobehaviorists. Thus Osgood (1953, p. 696), like David Hartley and Hume before him, supposed that the content of a concept like "spider" could be grounded in idiosyncratic conditioned associations, such as his own association between spiders and anxiety. Yet you and I can share the thought that spiders are arachnids, even though your thought about spiders has associations with anxiety and mine has associations with dusty cupboards (we don't think *different* thoughts because of this).

22. Useful excerpts and commentaries on the work of the Würzburg psychologists can be found in Mandler & Mandler (1964) and Humphrey (1951).

ness of contents and rules, the consciousness involved was conceived as quite different from the phenomenalistic form of consciousness characteristic of the awareness of sensational images:

> But consciousness of rule is not thinking *of* a rule, rather it is thinking a rule or according to a rule. The object of consciousness of a rule is not the rule, but rather the state of affairs, the object, that the rule describes, on which it is used, from which it might possibly be derived. (Bühler, 1907, pp. 339–340)

Indeed, it would not be stretching matters very far to describe the "modern investigation of thinking" as the investigation of the information processing "programs" employed in thinking. If this seems a bit extreme or anachronistic, it is worth noting, as Newell, Shaw, and Simon themselves noted, that the concept of a "program" as a set of cognitive operations has nothing essentially to do with computers. The implementation of programs such as the "Logic Theorist" on computers merely lent respectability to an autonomous cognitive construct:

> Our position is that the appropriate way to describe a piece of problem solving-behavior is in terms of a program: a specification of what the organism will do under varying environmental circumstances in terms of certain elementary information processes it is capable of performing. This assertion has nothing to do—directly—with computers. Such programs could be written (now that we have discovered how to do it) if computers had never existed. (Newell, Shaw, & Simon, 1958, p. 153)

Moreover, Newell, Shaw, and Simon (1958) in their pioneering paper on the computer simulation of thought processes, often held to herald the beginning (or at least a beginning) of the cognitive revolution, continued and acknowledged a tradition that goes back at least to the "thought-psychology" of Otto Selz (1922a; 1922b), a much-neglected member of the Würzburg school, which included his student Adriaan de Groot (1946), who developed prototype "programs" in his analysis of problem-solving by chess players.[23]

This form of content-based cognitive psychology, concerned with the inferential processing of the contents of (non-imagistic) representational states (such as beliefs, memories, and the like), survived the demise of the Würzburg school in the 1920s. Although Wundt's and Titchener's imagistic structuralist psychology was rejected, and a very great deal of emphasis was placed upon theories of learning derived from the experimental analysis of animal behavior, psychologists pursued content and rule-based forms of cognitive psychology throughout the 1920s, 1930s, and 1940s.

These early forms of cognitive psychology postulated theoretical states that are unambiguously cognitive according to the definition provided earlier. These psychologies clearly anticipated many contemporary forms of cognitive psychology, both in terms of general orientation and topics studied, including, for example, problem solving (Duncker, 1935; Durkin, 1937; Luchins, 1942; Rees & Israel, 1935; Seashore, 1940; Selz, 1922a, 1922b, 1927; Weaver & Maden, 1949; Wertheimer, 1945), general reasoning (Maier, 1930, 1931, 1933, 1945), concept formation (Hanfmann & Kasanin, 1937; Heidbreder, 1945, 1946a, 1946b, 1947; Long, 1937; Reed, 1946a, 1946b, 1946c), inductive reasoning (Morgan, 1944;

23. Selz was a student of Külpe, and de Groot attended Selz's seminars when Selz taught at the Municipal University in Amsterdam during the years 1939–1941. Selz had emigrated there after being deprived of his position at the Mannheim Institute for Psychology and Pedagogy because he was Jewish: he was arrested by the Nazis in 1943 and died in Auschwitz on August 27, 1943. See Nico H. Frijda and Adriann D. de Groot (1981). The Frijda and de Groot volume contains excepts from Selz's writings and a variety of useful articles on his work, including one by Herbert Simon on "Otto Selz and Information-Processing Psychology."

Welch & Long, 1943; Youtz, 1948), transfer of logical organization (Salisbury, 1934), and logical reasoning (Janis & Frick, 1943; Lefford, 1946; Morgan & Morton, 1940; Sells, 1936; Wilkins, 1928; Woodworth & Sells, 1935). Moreover, most of these studies were concerned primarily with *human* cognitive processes.

That is, from the 1920s onwards one can trace the development of a cognitive tradition that is continuous with contemporary cognitive psychology, albeit decidedly attenuated during the heyday of behaviorism, and distributed to a degree across a variety of "peripheral" journal outlets (although this is by no means exclusively the case). Just how extensive and how well integrated this tradition was has yet to be established. It appears to have overlapped with the development of gestalt psychology, which itself anticipated some of the themes of post-1950s cognitive psychology (Murray, 1995). Psychology in this tradition was very likely influenced to some degree by the development of psychoanalytic theory, an indisputably cognitive theory whatever one thinks of its empirical merits (or lack thereof[24]), and it may have been influenced by the development of the unambiguously cognitive theories employed by social and development psychologists from the 1920s onward, concerned with attitudes, cognitive dissonance and consistency, episodic memory, and the like. While all these matters remain to be determined by historical research, they do lead one to wonder about the degree to which the much-touted historical "hegemony" of behaviorism from the 1920s to the 1950s adequately reflects the rich diversity of research interests and practices during this period, and the degree to which it merely reflects, for example, factors such as the rhetoric of APA addresses by those who had managed to attain positions of presentational (and consequently representational) power within the profession.

This account of the cognitive tradition is partial and provisional. It is also somewhat of a simplification. I have talked throughout this paper of cognitive psychology and the cognitive revolution as if they represented a unitary disciplinary matrix and a unitary historical episode. While there is justification for this treatment, it is also true that the forms of cognitive psychology that developed in the 1950s and 1960s were concerned with rather different theoretical issues and linked to somewhat different traditions, which came to be jointly reinstated and invigorated in the 1950s and 1960s. The cognitive revolution itself seems to have been comprised of a number of originally fairly distinct developments and movements that only eventually become interrelated and interlocking.

Computational theories of information processing—or the sort pioneered by Newell, Shaw, and Simon, for example—were stimulated by developments in artificial intelligence and computer simulation. They powerfully influenced the adoption of the general "information-processing" paradigm, but seem to have developed (at least initially) largely independent of the development of interest in perception and cognition—conceived as an active "hypothesis-testing" process—pioneered by Jerome Bruner, Jacqueline Goodnow, and George Austin (1956). Computational theories of information processing also seem to have developed largely independently of the work of Noam Chomsky (1957) and George Miller (1962) on psycholinguistics, which did so much to legitimize the "rule-following" explanatory paradigm for the explanation of linguistic processing and other symbolic processes that came to be exploited by computational theories of cognition. For example, Bruner makes no mention of developments in artificial intelligence and computer simulation in his contribution to the 1955 *Colorado Symposium on Cognition*, recorded in *Contemporary Approaches to Cognition* (Bruner

24. I have developed my own doubts about the empirical adequacy of psychoanalytic theory (and other theories of therapy) in Greenwood (1996, 1997).

et al., 1957), nor did any of the other contributors. Ulric Neisser's magisterial *Cognitive Psychology* (1967), which documented state of the art developments in perception, memory, and psycholinguistics in the mid-1960s, explicitly dissociated itself from these developments.

The studies of short-term memory and attention developed by Miller (1956) and Donald Broadbent (1957) represented the revamping of a tradition that can be traced back to Wundt's studies of "apperception." This in turn was but an aspect of a broader tradition in psychophysics originating with Ernst H. Weber, Franciscus C. Donders, and Gustav T. Fechner, developed by Wundt and his American students, and continuously maintained within American psychology up to the present day. This movement, which eventually promoted the resurgence of interest in "consciousness" within cognitive psychology, also appears to have developed largely independently of the computational movement, despite the fact that both developed out of advances in applied engineering in the 1940s and 1950s.

Eventually these developments came together, as links were forged and accommodations made, but the story needs to be qualified to take into account these somewhat autonomous developments, including the question of the degree to which these apparently distinguishable early phases of the cognitive revolution are properly distinguishable as separate traditions prior to it.

CONCLUSION

In this paper I have characterized the cognitive revolution as having represented both a radical theoretical discontinuity between behaviorism and cognitive psychology, and the dramatic development and expansion of a continuous cognitive tradition that can be traced back to the 1920s. While I believe the broad outline of this account is generally correct, it is presented as a provisional challenge to traditional historical accounts. I do not pretend that this account provides the whole story about the cognitive revolution in psychology, but I hope it has illuminated two neglected strands of the "tangled web" of continuities, discontinuities, projections, and recapitulations that make up the history of this significant episode in the history of psychological science.

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