REIFYING THINKING:

A CRITIQUE OF COGNITIVE BEHAVIORISM AND THE STIMULUS-COGNITION-RESPONSE (S-C-R) MODEL IN PSYCHOLOGICAL THEORY

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Submitted to the Faculty of Graduate Studies and Research

In Partial Fulfilment of the Requirements

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by

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ABSTRACT

Reification is the apprehension of human phenomena as if they were things (Berger and Luckmann, 1966). Within psychological theory, the error may involve both the treatment of persons as things and the hypostatization of particular psychological concepts. The process of reifying in psychological theorizing normally has aspects of abstracting, forgetting, and positing a concrete entity. The error is considered common in psychological theory, and has been criticized by many writers, including Holzkamp (1964), Ingleby (1968), and Jacoby (1975).

The use of operational definitions by behaviorists has contributed significantly to the hypostatization of psychological concepts, despite the fact that such definitions were once regarded as a safeguard against reification. Bridgman's (1928) original notions of operational analysis required considerable alteration in order to utilize them within the context of methodological behaviorism (Stevens, 1939; Tolman, 1936).

Contemporary cognitive behaviorists have continued to rely upon the methods of operational definition developed by neobehavioristic theorists. The result has been the extension of reification to numerous concepts referring to private events, as well as the continuation of reification of public aspects of human activity. Increased interest in cognition among behaviorists has not resulted in a new paradigm of psychological research but rather in an extension of the neobehavioristic

stimulus-organism-response (S-O-R) model to what I call a stimulus-cognition-response (S-C-R) model of psychological functioning.

The most highly regarded contemporary theorists concerned with adult thinking and problem solving, Newell and Simon (1972), utilize an information processing approach within the general scope of operationistic behaviorism. Although they have contributed a computer program model of the mind which is a genuine scientific advance over previous behavioristic theory, they nevertheless hypostatize information processes as the "hidden cause" of actual human thinking, and reify human problem solvers as information processing systems.

Some non-behavioristic approaches to the study of thinking and problem solving are briefly considered, and I conclude that significant contributions to the scientific study of this area have been made without reifying. Gestalt theorists, thought-psychologists, phenomenologists, dialectical psychologists, and others who explicitly utilize self-reflection by psychologists upon their own thinking offer a variety of non-reifying alternatives. The phenomenological temporary suspension of belief in the objective world might offer a systematic antidote to reification. The reasons why reification has been so common in behavioristic studies of adult thinking probably lie outside the area of psychological theory, in the sociology of psychological knowledge.

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"A peculiar assumption ... . has been in many cases fatal to psychological theory. We may define this assumption briefly as the erroneous . . . attribution of the nature of things, to ideas." (Wundt, 1896/1902, pp. 14-15)

1. INTRODUCTION: REIFICATION AND PSYCHOLOGY

"All reification is a forgetting." ("Alle Verdinglichung ist ein Vergessen," Horkheimer and Adorno, 1947; p. 274)

1.1 Definition and scope of the work

Reification is a systematic error in the social construction of the content of consciousness. Berger and Luckmann (1966) describe the error as

> the apprehension of human phenomena as if they were things, that is, in non-human or possible supra-human terms. . . . Reification implies that man is capable of forgetting his own authorship of the human world, and further, that the dialectic between man, the producer, and his products is lost to consciousness. (p. 89)

This broad definition of reification is taken from the Western Marxist intellectual tradition (Adorno, 1973a, 1973b; Horkheimer and Adorno, 1972; Lukacs, 1971; Paci, 1972, etc.), and the term has been used with this meaning in recent English language psychological literature (Ingleby, 1968; Jacoby, 1975).

A slightly more specific emphasis is given in an ordinary dictionary definition, where "reify" is defined as "to convert into or regard as a concrete thing: to reify an abstract concept" (Random House, 1970). The two definitions are not incompatible, and the latter meaning has been employed by Ingleby (1968) and Jacoby (1975), as well as by behavioristic theorists (e.g., Kendler, 1952; Pratt, 1939; Stevens, 1935a). Since abstract concepts are constructed only by

persons, they are clearly human phenomena, and the first definition thus includes the second. I shall use the term reify in the broader sense, and, when a distinction is required, shall reserve the term hypostatize for the reifying of an abstract concept. Instances of both types of reification can be closely related in psychological theory; we shall encounter instances of the hypostatization of an abstract concept as a concrete thing and the complementary reification of humans and their activity as the hypostatized "thing."

In this thesis I shall be primarily concerned with the demonstration of the hypostatization of abstract concepts within certain areas of psychological theory, especially the study of adult thinking and problem solving. In Chapter 2, I shall discuss the relationship between reification and the use of operational definitions, which are widely employed in neobehavioristic psychological theory. In Chapter 3, I will examine the methodological continuity between the so-called "new paradigm" of cognitive behaviorism and classical neobehavioristic operationism. Chapter 4 will be focused upon reification in the works of the most prominent contemporary behavioristic theorists concerned with adult thinking and problem solving, and the final chapter will be devoted to consideration of some alternatives to their approach.

I believe that consideration of the error of reifying would be useful in other areas of psychological theory. However, the present work will be limited to the areas mentioned above, and I have decided to exclude extensive consideration of reification in a) the related cognitive fields of memory and perception, b) developmental approaches

to cognitive abilities, c) issues related to the practice of therapy, including cognitive behavior modification, d) related issues from the sociology of knowledge and the history and philosophy of science. I shall assume that part of the goal of human scientific activity is the avoidance of such errors, and shall, briefly, attempt to demonstrate that reification is not necessary for the scientific investigation of thinking. The rest of the present chapter will include an analysis of some differentiable aspects of the intellectual process of reifying, and a summary of some psychological literature concerned with reification.

1.2 Reification: The intellectual process

In a relatively formal intellectual enterprise such as psychological theorizing, three separable (though interrelated) aspects of the logical (as opposed to the social or psychological) process of reification can be distinguished. In formal theory, as opposed to "pretheoretical" reification (Berger and Luckmann, 1966), a formal abstraction is normally made. This step may be necessary, but is hardly sufficient for the error of reification, since abstraction is both necessary and legitimate in any theory. For psychological theory in particular, it is important to notice that concepts such as "stimulus" and "response" (Ingleby, 1968), "behavior" (Waters, 1958), "facts" (Kvale, 1976), and "reality" in the sense of an assumed transexperiential world or "reality$_1$" as described by Brandt and Metzger (1969) are all abstractions, not data given in experience.

—A second necessary aspect of reification is indicated by Horkheimer and Adorno's (1947/1972) famous claim that all reification involves forgetting. The legitimacy of this aspect of reification is somewhat ambiguous: a theorist must omit detail in the process of abstracting and generalizing, but s/he must remain aware of doing so. If one forgets rather than omits, or, to use a Laingian phrase, forgets, and forgets that one has forgotten, legitimate abstraction gives way to reification. The concept cannot be other than a nonhuman phenomenon describing the realm of things-in-themselves if one does not remember it as a human creation.

> The objectivity of the social world means that it confronts man as something outside of himself. The decisive question is whether he still retains the awareness that, however objectivated, the social world was made by men--and, therefore, can be remade by them. In other words, reification can be described as an extreme step in the process of objectivation, whereby the objectivated world loses its comprehensibility as a human enterprise and becomes fixated as a non-human, non-humanizable inert facticity. (Berger and Luckmann, 1966, p. 89)

Logically, it does not matter whether the abstracting and forgetting are performed by the same or different individuals; the essential error occurs when someone forgets that the initial abstraction was the product of human thought. Psychologically, it is probably easier to accept an abstraction as independent of human creation when one has personally played little role in the authorship of the abstraction; hence Jacoby's (1975) emphasis upon reification as social amnesia.

The third and final aspect of reification is the hypostatization of the concept per se: the apprehension of the concept as a concrete

thing. The classic example, almost definitive of the term reification, is Marx's (1967, p. 71ff.) discussion of the abstract concept "commodity," wherein "a definite social relation between men . . . assumes, in their eyes, the fantastic form of a relation between things" (p. 72).

The final step of positing the abstract concept as a thing may be explicit or may be merely implied. Some concepts such as memory trace or information processing system have been explicitly assumed to refer to a concrete thing; however, more often a psychological concept is implicitly given the status of physical reality, by measuring its size, by attributing causal properties to it, etc.

Even this brief introduction would be misleading if the impression was left that reification is merely an intellectual error, of interest primarily to logicians. In his influential essay of 1924, Lukacs (1971) claimed that reification is the most important problem of our time.

> There is no problem that does not ultimately lead back . . . to the riddle of commodity-structure. . . . the problem of commodities must . . . be considered . . . as the central, structural problem of capitalist society in all its aspects. . . . Its basis is that a relation between people takes on the character of a thing and thus acquires a 'phantom objectivity,' an autonomy that seems so strictly rational and all-embracing as to conceal every trace of its fundamental nature: the relation between people. (p. 83)

Thus reification may constitute a very serious portion of what James (1890, Vol. 2) called the "world of collective error" (p. 291). The intellectual critique of reification contains a) important criticism of positivistic approaches to science, b) questions which refer to the treatment of persons as things and which have been important themes

for existentialism and phenomenology, and c) a central thesis of Western Marxism about the nature of enlightened industrial society.

An example of reification which, although not from psychological theory, is important to psychology, has been described by Wartofsky (1968): the reification of science.

> Science is a human activity and has its roots in the ordinary human capacities we all share. As homely and obvious as this truth is, its significance often gets blurred in many of our definitions of science and in our attitudes toward it. . . .
>
> We think of science as universal and of scientific truths as independent of time, place, and circumstance. Such truths we take to be objective. . . . We also think of science and of scientific truth as cumulative, as having an independent existence, over and above the life span of particular scientists, and even of particular scientific communities. Thus, we conceive science to be continuous, autonomous, objective, universal. . . .
>
> In one interpretation of such an "objectivist" view, science comes to be taken as some transhuman or superhuman essence, as an entity in itself, or a "thing" apart from the matrix of human conditions, needs, and interests in which it originates and develops. There is a danger which lies in this reification of science. The continuity of science with common sense, or scientific understanding with the common understanding is broken. Practically, this reflects itself in the isolation of the scientist from the rest of the human community. . . . the divorce of some scientists from their roots in the human community has produced a serious social crisis. (pp. 23-24)

The abstraction <u>science</u> has its roots in human activity forgotten, and is apprehended as a nonhuman thing, with serious social consequences. We shall encounter psychological theorists who reify psychological science in this sense, as well as hypostatize particular psychological concepts.

1.3 Reification in psychological theory

In a specialized dictionary of psychological terms, English

and English (1958) add an editorialization under their entry for reification, pointing out that "the error is most insidious in psychology." They further provide an example, which is unfortunately less caricatural than it appears.

> No one is likely to think that, because some objects are thick, that there is an actual thickness apart from thick books, thick papers, or thick boards; but because there are "thick" heads, it is all too easy to suppose that "thickness" is what makes them "thick." (English and English, 1958, pp. 451-452)

While English and English discuss the hypostatization of a hidden cause of thickheadedness, Laing (1959) points toward the general tendency of many psychologists to consider persons as things.

> It seems extraordinary that whereas the physical and biological sciences of it-processes have generally won the day against tendencies to personalize the world of things or to read human intentions into the animal world, an authentic science of persons has hardly got started by reason of the inveterate tendency to depersonalize or reify persons. . . .
>
> Depersonalization in a theory that is intended to be a theory of persons is as false as schizoid depersonalization of others, and is no less ultimately an intentional act. Although conducted in the name of science, such reification yields false 'knowledge.' It is just as pathetic a fallacy as the false personalization of things. (pp. 23-24)

Laing, and English and English, while asserting that such errors are common in psychology, provide no actual examples or indications of possible reasons for hypostatizing psychological concepts and reifying persons in psychological theories. Criticism of actual examples of reification in certain psychological theories have been put forward by Holzkamp (1964), Ingleby (1968), and Jacoby (1975).

After elaborating a conception of the proper relationship between psychological theory and experimentation, Holzkamp (1964,

see also Brandt, 1967) points out that constructs within psychological theory are the mental constructions of psychologists, and treatment of such constructs as representing a hidden reality is an inadmissable reification, similar to the transcendental illusion opposed by Kant (1970) in which we "mistake the subjective necessity of a certain connection of our concepts . . . for an objective necessity in the determination of things in themselves" (p. 199). Holzkamp gives examples of reification from both psychoanalysis and behaviorism. Freud clearly writes of his major explanatory concepts as if they were real things in some passages in his works, although he also frequently refers to his conceptualizations as assumptions, speculation, and merely the attempt to work out certain ideas consistently (e.g., Freud, 1961, p. 18). Holzkamp is critical of behavioristic efforts to treat ideas which refer to the lifespace of individuals, such as anxiety, as objective entities like check marks on a rating scale or amount of rat excrement. He warns that psychological theorists must exercise great caution lest their ideas regarding causality or underlying physiological mechanisms are used in such a way as to imply that such conceptions are not explanatory conce but rather refer to concrete things.

In his critiq of reification in psychology and psychiatry, Ingleby (1968) contends

> that the ideological ends which psychologists (and other human scientists) unconsciously accept lead them to present a model of man which dehumanizes him in the same ways that their own society does, which obscures rather than clarifies the way in which that society's goals are mediated by the individual, and which attempts to reify its values under the guise of a spurious objectivity. (p. 159)

Ingleby defines reification as "the reduction of human realities to the order of things" (p. 165), describes it as the reduction of praxis to process, where process refers to the movement of things and praxis is human activity which is "purposive and accountable for only in terms of its meaningfulness" (p. 166). Ingleby's critique is directed primarily against behaviorism, and he discusses in some detail the reification of "stimulus," "response," and "reinforcement"--the basics of the behavioristic worldview. Ingleby concludes that "a return to concepts such as experience, awareness, or consciousness is dictated" (p. 172), although he seems to approve of both phenomenological (Gurwitsch, 1964) and behavioristic methods of dealing with such concepts (Miller, Pribram, and Galenter, 1960). Much of the "cognitive revolution" (Dember, 1974) within behavioristic psychology might be described by applying Ingleby's definition of reification to such concepts as experience, awareness, and consciousness (see Chapter 3; Hein, in press). Ingleby describes the reification of some concepts within behavioristic psychology which are not so central as those above, including "temperament," "personality trait," etc. (notable for its absence is "behavior," see Waters, 1958).

Ingleby also discusses what he calls "normative reification" which involves the reintroduction of value judgements into supposedly objective psychology, often by the use of biological or medical concepts such as normality, mental illness, intelligence, adjustment, etc. Ingleby is primarily concerned with the way in which pretheoretical social reification influences and is reinforced by reification within psychological theory. He is thus primarily interested in the

ideological role of reification in psychological theory. I shall not summarize his discussion of these problems in detail; Ingleby's major error seems to me to be his underestimation of the way in which information processing models can become the next line of retreat for an apologetic, behavioristic, ideology in psychological theory.

Jacoby (1975) emphasizes forgetting as the central aspect of reification within psychological theory:

> Reification in Marxism refers to an illusion that is objectively manufactured by society. This social illusion works to preserve the status quo by presenting the human and social relationships of society as natural--and unchangeable--relations between things. What is often ignored in expositions of reification is the psychological dimension: amnesia--a forgetting and repression of the human and social activity that makes and can remake society. The social loss of memory is a type of reification--better: it is the primal form of reification. (p. 4)

Jacoby finds a simple answer to the question of what is being forgotten in current psychological theory: Freud. Jacoby does not give his reasons for regarding the forgetting of Freud as more important than the forgetting of, e.g., William James; it is fairly clear that he wants to extend the Frankfurt School critique of neo-Freudianism to modern existential and humanistic theoretical approaches to psychotherapy. Although he believes behavioristic theory to be reifying, Jacoby hardly mentions it, since he wishes to criticize theories which are presented as alternatives to behaviorism, but which critical theory views as reifications of the ego which complement rather than genuinely oppose the behavioristic reifications of objectivity. Although Jacoby gives little consideration to phenomenology, Husserl (1970) had made a similar point that the Cartesian doctrine of two substances cannot be overcome

by efforts to demonstrate the primacy of one res over the other, and that neither res is an actually existing substance because abstracta are not substances at all (pp. 226-229).

By emphasizing forgetting rather than the positing of "things;" Jacoby is able to criticize reification in the works of existential and humanistic psychological theorists who are themselves critical of behavioristic reification (e.g., Laing, 1959). With Adorno's (1973a, 1973b) philosophic critique of existentialism and humanism as a guide, Jacoby finds that within psychological theory the existentialist, humanist, and neo-Freudian "revisionists" of psychoanalytic theory, like their Marxist counterparts, "edged toward empiricism, positivism, pragmatism, and a rejection of theory" (p. 12), and in this process repressed the critical dialectical heart of Freud's thought. Jacoby, like Ingleby, is extensively concerned with ideological and therapeutic aspects of reification in psychology, which are peripheral to my primary intentions in this thesis.

Finally, a number of behavioristic theorists have been concerned with the hypostatization of theoretical concepts in psychology (Kendler, 1952; MacCorquodale and Meehl, 1948; Marx, 1951, 1963; Pratt, 1939; Stevens, 1935a). Insofar as such criticism is related to modern cognitive psychology, I shall return to this question in Chapters 4 and 5. At this point I shall merely note that all of these behaviorists recommend the exclusive use of operationally defined concepts as a cure for reification in psychological theory. In the next chapter, I

shall attempt to show that such procedures cause, rather than cure, reifying in psychological theorizing.

2. OPERATIONISM

"Thus, we make explicit the distinction between the experimenter and the thing observed." (Stevens, 1939, p. 228)

2.1 Bridgman: Experienced operations versus reification

The half century long history of operational definitions, considered for its relevance to reification in psychology, is a rather sad story. Bridgman's (1928) original formulation was clearly inspired by a desire to establish a method for avoiding hypostatizations such as the Newtonian concept of absolute time, which Einstein's studies of simultaneity had revealed as a reification. However, in the hands of Stevens (1935a, 1935b, 1936, 1939) and other behaviorists (Langfeld, 1945; Marx, 1963) operational definition became a technique for the reification of psychological concepts much more in the spirit of classical mechanics than of modern physics (Brandt, 1973; Heisenberg, 1958).

Bridgman shared with positivism the belief that many metaphysical discussions are meaningless, but he never shared with behaviorism the desire to exclude mental operations from the legitimate reach of science:

> *In general, we mean by any concept nothing more than a set of* operations; the concept is synonomous with the corresponding set of operations. If the concept is physical, as of length, the operations are actual physical operations, namely those by which length is measured; or if the concept is mental, as of mathematical continuity, the operations are mental operations, namely those by which we determine whether a given aggregate of magnitudes is continuous. (Bridgman, 1928, p. 5)

For Bridgman all operations, not only mathematical ones, are experienced activities of individual scientists. "Operations themselves are, of course, derived from experience" (p. 36) and, since "all our knowledge is in terms of experience; we should not expect or desire to erect an explanatory structure different in character from that of experience" (p. 42).

Attempts to construct explanations which differ from experience frequently take mechanical and mathematical forms which scientists should not only avoid but actively resist.

> Just as the old monks struggled to subdue the flesh, so must the physicist struggle to subdue the nearly irresistible, the perfectly unjustifiable desire . . . for mechanical explanation which has all the tenacity of original sin. (p. 47)

"Mathematics sort of forces us to talk about the inside of an electron although physically we cannot assign any meaning to such statements" (p. 63).

Bridgman argues that concepts to whose referents we can ascribe physical reality (the stress on a bridge), and concepts to whose referents we cannot ascribe reality (electrical fields) are both useful and admissible in science. The latter concept is distinguished by the fact that no operations can be found, independent of those which entered into its definition, by which evidence for the existence of its referent could be obtained. The book closes with the warning that, while physics should seek operations which demonstrate the reality of our constructs, those which we cannot show to be real are neither to be abandoned nor reified.

> We must search for new physical facts which will give to our inventions the character of physical reality. In case prolonged search fails to disclose such phenomena we must then find some way of embodying explicitly in our thinking the fact that we are dealing with pure inventions and not realities. (pp. 225-226)

Bridgman's initial position concerning operational definitions generated interest and controversy, both in psychology and in other fields, and in 1936 he published a series of lectures designed to clarify his views. In this work he was even more explicitly in favor of the thesis that meaning, including but not limited to the meaning of scientific theories, can only be found in the activities and experiences of individuals.

> For me meaning is to be found in a recognition of the activities involved. These activities may be diffuse and nebulous and on the purely emotional level, as when I recognize that what I mean when I say I dislike something is that I confront myself with the thing in actuality or in imagination and observe whether the emotion that it arouses is one with which I associate the name "dislike." (Bridgman, 1936, p. 9)

Bridgman even supposes that his position might be legitimately considered solipsistic. Since "there is no such thing as public or mass consciousness," it follows that there is no such thing as public science, except as "a particular kind of the science of private individuals" (p. 13).

> In the last analysis science is only my private science, art is my private art, religion my private religion, etc. The fact that in deciding what shall be my private science, I find it profitable to consider only those aspects of my direct experience in which my fellow beings act in a particular way cannot obscure the essential fact that it is mine and naught else. (p. 13)

Although Bridgman does not use the term reification, it is very clear that he is concerned with developing a metatheory in order to emphasize the scientist's operations and avoid the hypostatization

of abstractions which are not given in the experience of individual scientists. He is particularly concerned with the reification of experience that is occasioned by language, and, although many aspects of this problem are beyond the scope of the present paper (see Chisholm, 1945; Korzybski, 1958; Whorf, 1967), I shall follow Bridgman far enough to show the difference between his intentions and those of the behaviorists who took over the name of operational analysis, but not its anti-reifying spirit.

Bridgman (1936) starts with the observation that "our experience is composed of activities of one sort or another . . . it is not static, but in continual flux" (p. 15). Then, without mentioning phenomenology, he virtually describes a phenomenological reduction:

> For example, to say "I see a horse," gets recognizably closer to the direct experience than to say "There is a horse," for the first describes my experience as an activity, whereas the second freezes my activity and substitutes for it something static, something which did not occur in direct experience, and something which itself constitutes a human invention, and is so far questionable. (pp. 17-18)

Language, although it often distorts and reifies experience, is, of course, nonetheless necessary:

> It would appear that every noun in language requires a certain amount of construction and abstraction. We do not experience things; things are a construction of ours the function of which is to emphasize the resemblance between aspects of our present immediate experience and aspects of our past experience. . . . If, then, language does not reproduce experience with fidelity, to what does it owe its success in dealing with experience? It seems to me that it owes whatever success it attains to its ability to set up and maintain certain correspondences with experience. . . . The operational meaning of the concept of "thing" involves merely a description of the fact that it is possible to maintain such correspondences. . . . An essential distinction between language and experience is that language

> separates out from the living matrix little bundles and freezes them; in doing this it produces something totally unlike experience, but nevertheless useful. That is, language as language is divorced from the activity which is the basal property of all our experience. Although language has this essential characteristic, nevertheless language used is obviously an activity, and as an activity may reacquire some of those properties of continuous flux and change which as language proper it sought to divest itself of. (pp. 18-24)

In summation, Bridgman proposed the analysis of concepts into operations, or operational definition, as a method of keeping a close correspondence between our scientific language and actual individual experience, and of avoiding reification, to the extent that language allows this, by focusing upon the active operations involved in the experience of individual scientists.

2.2 Behavioristics: Operationism and reification

Despite Skinner's (1945) contention that his doctoral thesis "was the first explicitly operational analysis of a psychological concept" (p. 291), the primary champion of the use of operational definitions within behavioristic psychology during its early history was Stevens (1935a, 1935b, 1936, 1939). Stevens sought to incorporate Bridgman's method of analysing concepts into a grandiose "revolution that will put an end to the possibility of revolutions" (1935a, p. 323) within psychology, and to establish this new psychology, behavioristics, as a propaedeutic "Science of Science." Stevens gave this project the name operationism; Bridgman (e.g., 1954) disassociated himself from such pretentiousness.

Stevens opposed Bridgman's notion of private science, and put

forward public social agreement as the ultimate criterion of science. While Bridgman felt that an infinite number of mental or physical operations could be used by an individual scientist to define a concept, Stevens sought to replace these with the one fundamental operation of differential response, or discrimination, as observed by "the other one." Stevens efforts were extremely successful, and all major neobehavioristic theorists accepted the necessity for operationally defining psychological concepts in the manner advocated by Stevens (see Chapter 3). The jargon of operational definition became firmly and quickly entrenched within the dogma of behaviorism; as early as 1941 Koch wrote that "almost every psychology sophomore knows that it is bad form if reference to 'definition' is not qualified by the adjective 'operational'" (p. 15).

Concerning the question of reification Stevens and Bridgman are at opposite extremes. In direct opposition to Bridgman's concern for the way in which language reifies experience, Stevens (1939) assumes that all scientific experience can and should be reduced to verbal propositions, and that all such sentences can be further reduced to "what is Steven's discussion of the appropriate e., sentences about things. a horse provides one example of the st language to describe sighting a horse provides one example of the striking contrast between his and Bridgman's approaches.

> All objects or events satisfying certain criteria we call members of a class and to that class we assign a name or symbol. Common nouns originate in precisely this fashion. . . . Classification can proceed only when we have criteria defining the conditions for class-inclusion, and these criteria are essentially operational tests. Thus the statement, "Dobbin is a horse," asserts that Dobbin is a member of a class. This proposition is empirically

> meaningful only provided its truth or falsity can be demonstrated by concrete procedures. Does Dobbin satisfy the criteria of the class horse? If he is a certain size and shape, is covered with hair, feeds on oats and hay, etc., we are happy to acknowledge him as a full fledged horse. But how do we know that he meets our tests? Here we resort to that fundamental operation we have already called discrimination. If we can discriminate crucial differences between Dobbin and other animals we have named horses, we reject Dobbin as something not horse. In other words we "correlate" our discriminations . . . and the "goodness" of fit determines where we shall classify the beast. (Stevens, 1939, p. 233)

Bridgman's (see above, p. 15) concern with the actual sighting of a horse, and the subject of this experience, are both forgotten, all in the name of defending Bridgman's conception of empirical operations. Moreover, for Bridgman, the operations upon which an operational definition depends are always the operations of an individual scientist; Stevens subtly begins to shift some of the responsibility for performing the defining operations to the subject of research. It is Dobbin who must perform the operations of feeding on hay and oats, etc., which become part of the operational definition of the conception of horse. This type of confounding the subject of the operations involved in operational definition became typical of behavioristic psychology.

Stevens' reification of science is extreme. His proposal that science and scientists be subjected to scientific study is an excellent idea, aside from his rather grandiose title, the "Science of Science." The proposal is fundamentally flawed by Steven's plan to completely disregard any study of the activity, experience, thinking, and, in his more extreme statements, even the behavior of

scientists in his Science of Science.

> It is proposed that in our study of the science-maker we begin with the products of his activity--his finished propositions--rather than with his "experiences" or any other phase of his earlier behavior. This is a sensible place to begin. If we were to study the manufacture of any product, such as automobiles, we should probably find it useful first to ascertain what an automobile is. . . . Science manufactures sentences. (1939, p. 250)

While eventually it might prove interesting for a "behavioristic psychology . . . the only one that can be of much help in this enterprise" to examine "the scientist as a sign-using organism" (p. 250), even this limited examination of the activities of scientists would not be essential to the Science of Science, since the study of the product would be sufficient. "It is possible to include without remainder the study of science under the study of the language of science" (p. 244).

The analogy to automobiles, while interesting from the point of view of a sociology of knowledge, is logically superfluous, since Stevens has explained earlier what science is: "Science, as we find it, is a set of empirical propositions agreed upon by members of society" (p. 227). While Stevens reasons for concentrating the Science of Science strictly upon the products of science--sentences--and regarding even a behavioristic study of scientists as a secondary project are not entirely clear, it is very clear that describing science as a set of propositions which we can find is precisely what Wartofsky (1968) describes as the reification of science. Regardless of whether the individual or social aspects of science are emphasized,

science is surely a human activity (Brandt, 1967; Holzkamp, 1964; Wartofsky, 1968), and to regard science as a concrete object--a set of words or sentences--reifies science and forgets the activity which constitutes scientific work. Psychology is, of course, also reified as a subset of the collection of physical sentences which is part of the unity of science as conceived by logical positivists (especially Neurath, 1931, 1937). "Science is a thing agreed upon by members of society" (Stevens, 1935a, p. 327).

In addition to the normal reification which Ingleby (1968) has described as characteristic of behavioristic psychology, Stevens bluntly calls the subjects of psychological research "the thing observed" (1935a, p. 328, 1939, p. 228). Stevens also reifies the psychological experimenter, though less bluntly.

> A steadfastly objective outlook of this sort avoids the slough of subjectivity and makes possible a straightforward scientific epistemology according to which an independent experimenter, about whom we ask no questions, investigates the natural phenomena of knowing in "the other one." (1936, p. 96)

> Although a particular experimenter may himself become the object of study by another experimenter, and he in turn by still another, at some stage of such a regress an independent experimenter must be (i.e., is always) assumed. (1939, p. 228)

This rather mysterious independent experimenter is certainly nothing more than an abstract idea which Stevens hypostatizes.

Although he claims that operationism provides the only means by which we can assure that we shall never again think of "consciousness as a substance" (1935a, p. 330), Stevens implicitly treats consciousness as an unknowable substance which we cannot

penetrate. "Not even psychology knows anything about private experience, because an operation for penetrating privacy is self-contradictory" (1939, pp. 227-228). At one point Stevens comes explicitly to the conclusion that operationism reveals that experience is a thing. "Any attempt to define the term experience operationally . . . discloses at once that the discriminatory reaction is the only objective, verifiable thing denoted" (1936, p. 95). Stevens believes that his version of behaviorism does not need to exclude mentalistic concepts such as image, idea, etc., and gives the following example as an operational definition of immediate experience.

> An empirical (operational) definition of immediate experience is possible provided we note precisely what its advocates do when we ask them to indicate an example of it. Almost invariably they point to an elementary discrimination such as: "I see red." Elementary discriminations, then, are what is meant by the immediately given, and discriminatory reactions, of course, are public and communicable. (1939, p. 239)

Bridgman had discussed immediate experience in detail with considerable sensitivity, and I can only assume that such a gross reification by a "follower" of his might cause him to "see red."

Bridgman is not the only authority cited somewhat questionably by Stevens. Stevens (1939) cites William James several times, and mentions the first edition of Korzybski's (1958) major work, and strongly implies that these writers would support Stevens' conception of operationism. My own limited knowledge of Korzybski suggests that he would consider many of Steven's reifications as involving the semantic confusion which he strongly condemned as objectification.

> If, through lack of consciousness of abstracting we identify or confuse words with objects. . . . I call it objectification. . . . If we objectify, we forget . . . that words are not the objects or feelings themselves. (Korzybski, 1958, p. 417)

Korzybski might well include Stevens among "the behaviorists" who "try to be ultra 'scientific,' not realizing that their knowledge of scientific method . . . belongs . . . to the sixteenth century" (p. 303). Stevens cites James in support of the way in which the movement which Stevens is championing (operationism, behaviorism, logical positivism, and physicalism) is proving "disastrous for metaphysics" (1939, p. 223). It was James who said that "metaphysics means only the unusually obstinate attempt to think clearly and consistently" (1962, p. 457). For metaphysics in this sense, Stevens indeed proves disastrous.

Finally, I wish to mention that although Stevens was a respected scientist and the principal theoretician of operationism, whose views were extremely influential within neobehaviorism, his position was at times so extreme that I believe it deserves ethical as well as intellectual censure. Only a few years after Freud had been forced to watch his works being burned, Stevens (1939, p. 236) clearly and unequivocally advocates the burning of books which do not conform to Stevens' view of science. The censorship of ideas was all too common in 1939, and today (nach wie vor, now as before, as Adorno often put it) such censorship still takes place in many ways in many places; to advocate it is ethically and intellectually indefensible, particularly as part of a theory of science.

2.3 Operationism in practice

Stevens' theory of operationism, if not his moral judgements, were accepted by most major neobehaviorists, including Boring (1936, 1941), Hull (1937), Skinner (1938), and Tolman (1936). Some behaviorists proposed slightly different theoretical approaches to operationism (Kantor, 1938; McGeoch, 1935, 1937; Pratt, 1939), and a few psychologists openly criticized Stevens' theory (Crissman, 1939; Hart, 1940; Waters and Pennington, 1938). But by far the most important critique of operationism from this era, by Israel and Goldstein (1944), did not focus upon Stevens' theory, but upon the application of this theory within experimental psychology.

Firstly, Israel and Goldstein point out that Bridgman's technique of operational definition had become an almost mandatory methodological "ism" primarily within psychology.

> Operationism as a general methodological discipline, a professed scientific way of life, has attained much greater prominence in psychology than in any of the other sciences. The ordinary physicist, chemist, or biologist shows calm disinterest in operationism and operational definitions, if he has heard of these things at all, and his scientific journals contain almost no mention of these terms. Yet . . . an ordinary psychologist, can hardly remain unaware of the operational movement, nor can he remain indifferent to the insistent demand that every term which he uses be operationally defined. (Israel and Goldstein, 1944, p. 177)

They further point out that Bridgman had not proposed an "ism", but had simply offered a method of clarifying the experience of scientists which might avoid some errors such as the reification of absolute space and time.

> I believe that I myself have never talked of "operationalism" or "operationism." . . . no esoteric theory of the ultimate nature of concepts, nor a philosophic championing of the primacy of the "operation." . . . So far as it is anything definite at all, it is a technique of analysis which endeavours to attain the greatest possible awareness of . . . our activity or operations, whether the operations are manual in the laboratory or verbal or otherwise "mental." . . . The value of an operational analysis is often to be found in the fact that it allows us to profit more easily by our general experience. (Bridgman, 1938, pp. 114, 119, 130-131)

Israel and Goldstein not only point out the contrast between this modest view and the grandiose conceptions of Stevens, but, more importantly, they note that operationists within psychology frequently employ operational definitions in their experimental practice in a manner radically different from Bridgman's proposals, namely to simultaneously produce the phenomena which they define, i.e., to reify the term defined.

> The operations specified to define the meaning of psychological terms are operations involved in producing, in eliciting, the phenomena referred to, whereas in the case of Bridgman's definitions the operations involved are those of measuring the quantity or testing the identity of the phenomena. (Israel and Goldstein, 1944, p. 180)

Bridgman (1928) had specifically claimed that concepts such as electrical field, for which no operations other than those which enter into the definition of the concept can be found which verify that the concept refers to a real entity, must be considered to refer only to a mental invention.

> The operationists among psychologists trace their doctrine to Bridgman's original proposal of an operational technique of analysis in physics, and they represent their procedures as direct applications of the principles which Bridgman laid down. (Israel and Goldstein, 1944, p. 177)

In fact, the claims by operationists to be orthodox followers of the

scientific methods advocated by the Nobel prize winning Bridgman, can be based only on either sheer pretense or outright ignorance.

Israel and Goldstein present an extensive survey of the utilization of operational definitions within psychology, with many examples from the works of Boring, Pratt, Stevens, Skinner, and Tolman. With the exception of one minor subgroup of operational definitions used by Tolman, they conclude that the use of operational definitions by neobehaviorists are radically different from the techniques advocated by Bridgman. The operationists actually employ "a method which dispenses with the operations of defining concepts" and consequently "an operational concept in psychology may stand for a class of phenomena admittedly unidentifiable by any present operation" (p. 185).

> In the critical matter of definition, the operationists do not actually rely upon their functionally connected operations alone to identify and distinguish phonomena. They start with a heritage of already identified variables, entities, and events, and for these they adopt the established definitions or supply working definitions of the ordinary kind. (p. 187)

This lack of attention to linguistic definition has remained typical of more recent behavioristic research, often resulting in what Brandt (1970) calls "The Behaviorist's Leap," with considerable potential for reification. Israel and Goldstein conclude that when the implicit and unstated ordinary definitions of psychological terms fail to adequately identify the phenomena in question, the operationists "presume to establish the existence and determine the identity of hypothetical phenomena which are otherwise unidentifiable" (p. 187, my emphasis).

2.4 The liberalization of operationism

Although the Israel and Goldstein article was the "casus belli" (Boring, 1945, p. 278) of the 1945 Symposium on Operationism, which included papers and comments by Langfeld, Boring, Bridgman, Feigl, Israel, Pratt, and Skinner (1945), nevertheless it has been widely noted that the participants at this symposium failed to answer the major criticisms of operationism which Israel and Goldstein had put forward (Adler, 1947; Benjamin, 1955; Plutchnik, 1963; Wallach, 1971). Rather this symposium marks the beginning of what Wallach (1971) calls the "liberalization of operationism," with many of the participants indicating some degree of disagreement with Stevens' extreme position. (Stevens and Tolman did not participate due to wartime circumstances). Langfeld (1945) began his introduction with the remark that "it is obviously impossible to explicate an operational definition for every construct-term in scientific discussion" (p. 241), and Feigl went somewhat further:

> Just how precisely, completely or directly operational definitions enable us to identify objects is a matter of great methodological importance, but also surely a matter of degree. To demand definition of every term used in a piece of scientific discourse would not only be unduly pedantic (beside being incapable of practical fulfillment and thus utopian) but also quite unnecessary. (Feigl, 1945, p. 251)

However, both Boring and Skinner defend the more extreme position that all terms used in scientific psychology should be operationally defined. "Since science is empirical and excludes private data, all of its concepts must be capable of operational definition" (Boring, 1945, p. 244).

> There is no reason to restrict operational analysis to high-order constructs; the principle applies to all definitions. This means . . . that we must explicate an operational definition for every term. (Skinner, 1945, p. 271)

However, Skinner noted that "the confusion which seems to have arisen from a principle which is supposed to eliminate confusion is discouraging" (p. 294), and he later rejected operationism (in theory, if not in practice--see section 4.4, and Skinner, 1950, 1953, 1974).

A second aspect of the liberalization of operationism involves the legitimation of the use of hypothetical constructs, which were first distinguished from operationally defined intervening variables by MacCorquodale and Meehl (1948). Tolman (1936) had introduced the concept of intervening variables into psychological theory in a paper on "operational behaviorism," and although his complete formulation is quite complex (see Figure 3.3), the basic notion of intervening variable is fairly straightforward. Intervening variables are entities or processes which can be defined in terms of objective experimental manipulations, which intervene or mediate between the presentation of a stimulus to an organism, and the resulting behavior; they are "a set of intermediating functional processes which interconnect between the initiating causes of behavior . . . and the final resulting behavior" (Tolman, 1936, p. 117). They are specifically intended as an objective means of dealing with mental processes, and as an objective, behavioral alternative to physiological processes.

> It is the behavior of organisms . . . which I wish to predict and control. . . . And in these predictions, mental processes . . . will figure only in the guise of objectively definable intervening variables. (Tolman, 1936, p. 116)

Tolman (1936) specifically notes that "operational behaviorism finds . . . mental processes. . . . are objective entities" (p. 118),

which can be studied by objective, behavioristic methods, and should be defined "by going at them experimentally, i.e., operationally, from the two ends" (p. 127). Tolman concludes that

> operational behaviorism . . . asserts that psychological concepts, i.e., the mental capacities and mental events--may be conceived as objectively defined intervening variables. And it asserts that these variables are to be defined wholly operationally. (p. 129)

MacCorquodale and Meehl (1948) proposed that theoretical concepts which are presumed to refer to a real entity or process be termed hypothetical constructs, and that terms which merely abstract or summarize known empirical relationships be called intervening variables. The meaning of an intervening variable could be said to by exhausted by its operational definition, whereas a hypothetical construct has a surplus meaning involving "the supposition of entities or processes not among the observed" (p. 106-107). The latter must be handled with greater caution, and should be regarded as inadmissible unless "their actual existence" is "compatible with general knowledge and particularly with whatever relevant knowledge exists at the next lowest level in the explanatory hierarchy" (p. 107).

MacCorquodale and Meehl (1948) argue that "it seems clear from Tolman's description that" his intervening variables "are what we are calling abstractive rather than hypothetical" (p. 100). Since Tolman (1936) had specifically treated operationally defined intervening variables as concrete entities, it is not surprising that he did not accept the proposed interpretation of his work.

> To use Meehl and MacCorquodale's distinction, I would now abandon what they call pure "intervening variables" for what they call "hypothetical constructs," and insist that hypothetical constructs be parts of a more general hypothesized model or substrate. (Tolman, 1949, p. 49)

The distinction between intervening variables and hypothetical constructs, and the general trend toward liberalizing operationism have been the subject of continuing controversy. Bergmann (1953) called the distinction a "pseudodistinction" (p. 447), and Marx (1951, 1958, 1963) proposed that the distinction is not a sharp one and that a continuum between hypothetical speculation and operationally clear intervening variables replace the conceptual distinction. Marx (1951) however, insisted that "if psychological theories are to be placed on a sound scientific basis, logical constructs of the more distinctly operational type must . . . replace . . . the hypothetical construct" (p. 236). Marx (Marx and Hillix, 1973) continues to hold that the goal of operational definition of all terms is a requirement of scientific psychology, although Wallach (1971) has pointed out that few, if any, contemporary philosophers of science believe that such a goal is either possible or desirable.

Further liberalization of operationism has been put forward by various writers. Adler (1947) demonstrated conclusively that rigorous operational definition is no guarantee against nonsense in scientific theory, as Stevens had claimed. Similarly, Newbury (1953) showed that Stevens' claims that operationism avoided philosophic assumptions is not true. Pfannenstill (1951) discussed the inability of operationsim to deal with questions involving historical research. Ginsberg (1954)

showed how the requirements of strict operationism are not compatible with the importance of the role of theory in psychological science. (For a more complete survey, see Benjamin, 1955; Marx, 1963; Wallach, 1971). It is increased recognition of the importance of the role of theory which is the basis of Wallach's (1971) claim that even the liberalized use of operational definitions violates the standards of modern philosophy of science. However, the strictly operationally defined intervening variable is still in use in psychology, especially in the study of cognition, and I believe that its use is furthering rather than preventing reification.

2.5 Reification by operational definition

While Kant's transcendental illusion might be said to form the background upon which the entire project of operational behaviorism was conceived, it is possible to explicate a more precise relationship between the reification of a particular concept and its operational definition. Of course operational definition is not essential to reification; Berger and Luckmann (1966) provide numerous examples of the perception of human ideas or activities as concrete things which involve no relationship to operationism. However, I submit that reification is essential to operationism; the operational definition of a psychological concept, in the manner in which behaviorists normally utilize such definitions, necessarily treats the theoretical concept as a concrete object.

The classical example of an operational definition in

behavioristic psychology is Pratt's (1939) argument that "intelligence is what the intelligence tests test" (p. 79). Disregarding the question of the overall scientific tenability of such a definition, we may simply ask: can this definition be utilized without hypostatizing intelligence? Since Pratt (1939) extensively criticized reification, the question is not necessarily trivial, but I believe the answer must be negative.

Intelligence is an abstraction; it is a quality attributed to humans and other animals that act intelligently, and the quality "intelligence" is abstracted from such intelligent activity. In MacCorquodale and Meehl's terms it is basically an intervening variable; the concept merely summarizes the complexity of intelligent action in a shorthand form. Only within the context of a particular theory which hypothesized, e.g., that cortex size is the cause of intelligent action, would the concept intelligence assume the surplus meaning of a hypothetical construct.

If we take the proposed definition seriously, it is apparent that a great many aspects of intelligence must be neglected and forgotten. Many actions and thoughts which would generally be considered intelligent cannot be included in a standard test because they are simply too complicated. Thus, formulating the theory of relativity, designing the Taj Mahal, or, in fact, most of that which we would ordinarily describe as intelligent activity, is so complex as to be totally impossible to be literally included under "what the tests test." The issue here is not the experimental problem of whether the tests can successfully predict

quality work in physics or architecture, but simply that most intelligent action cannot be incorporated into a test. If the operational definition is considered seriously to be the definition of intelligence, then most intelligent activity must be omitted from the abstraction intelligence.

Furthermore, I do not believe that this is merely a specious or atypical operational definition. After considering the same example, Blumer (1969) notes:

> This observation applies equally and fully to all instances of so-called operational procedures. If the concept or proposition that is being operationalized is taken to something that is present in the empirical world, one cannot, as a true empirical scientist, escape the necessity of covering and studying representative forms of such empirical presence. To select (usually arbitrarily) some one form of empirical reference and to assume that the operationalized study of this one form catches the full empirical coverage of the concept or proposition is, or course, begging the question. It is this deficiency that runs so uniformly through operational procedure, that shows that operationalism falls far short of providing the empirical validation necessary to empirical science. (pp. 30-31)

Moreover, the operational definition of intelligence not only excludes most of that which it purports to define, it further posits the abstraction as something concrete. The operational behaviorist can consider the definiens, in the case of an adult pencil and paper intelligence test, only as either the physical behavioral movements of marking the paper, or, more commonly in the actual practice of using intelligence tests, only the physical marks produced on the paper. Consideration of the subject's experience of the meaning of the test items, the testing situation, his or her subjective intentions, etc., are not admissible data. Rigorous consideration of Pratt's definition leads directly to the conclusion that intelligence is physical marks

on a piece of paper. This is not only a reification of the abstraction intelligence, but is such an absurd definition that it is small wonder that operationists do not actually rely upon their operational definitions, but fall back upon their everyday knowledge of the meaning of the term (Brandt, 1970; Israel and Goldstein, 1944).

The participants at the 1945 Symposium on Operationism were all asked to comment on this particular example of an operational definition. Only Bridgman (1945) noted the reification involved.

> With regard to the intelligence test, the assertion as it stands begs the question. The question-begging word is the humble 'what.' The assertion that the intelligence test tests a 'what' implies . . . that the results of the test have the properties of a 'what.' (p. 249)

A further example of the way in which reifying may be considered related to forgetting involves the way in which operationists 'forget' to consider the question of who is performing the operations which are used in operational definitions. Bridgman (1928, 1936) was very clear that any operations made by the individual scientist could be used for the operational analysis of scientific concepts. The critical literature on operationism has not sufficiently stressed the fact that operationists invoke in their operational definitions, usually implicitly; operations which they cannot specify because they are performed by the subjects of their experimental research. This ambiguity concerning the performer of the operations involved in operational definition is clearly present in Tolman (1936, 1938), in Stevens (1939) and in Skinner's (1938) emphasis upon the animals' operations in operant conditioning. Thus Pratt's definition of intelligence clearly relies upon the mental operations

which subjects use to decide which marks to place upon their answer sheet, although traditional operational behaviorists ignore such mental operations and observe only the subjects' responses. The attempt to define a concept by reference to operations which one does not observe, understand, have any theories about, etc., cannot be said to offer definitional clarification, but only greater confusion. Operational behaviorists who presume that they have defined a concept with complete scientific rigor in a case like this have necessarily forgotten the unspecified operations of their subjects which are essential to the definition proposed. (The rest of this thesis will be concerned with recent efforts to include the subjects' cognitions within the scope of operational definitions. In these cases we will encounter operations which neither the experimenter nor the subject nor any living creature could possibly perform, namely operations performed by computers.)

Many other similar operational definitions could be considered. Holzkamp (1964) has analyzed the reification of anxiety by means of operational definitions, and Brandt (1970) has pointed out the reification involved in operationally defining and measuring attitudes. The reification is each case is further demonstrated by the assertion that such abstractions, once operationally defined, can be measured. Marx (1963) defines operationism as "a movement in science which insists that adequate definitions of terms are those in which meanings are synonomous with the operations involved in measuring" (p. 42). If the relationship between operational definition and measurement is even nearly as direct as Marx indicates, then it becomes very easy to believe that that which

has been operationally defined is a concrete thing, since one has also measured it. The measurement of intelligence is clearly implied in Pratt's definition, and the use of numbers to describe intelligence contributes to the illusion that a hidden reality is involved. Actual events are not uncommonly attributed to the causative power of such a hidden reality. If John gets higher grades at university than George, it is not uncommon to attribute the cause of this event to John's possession of greater intelligence. The logic is quite similar to attributing the cause of this event to George's possession of greater thickheadedness (Chapter 1; English and English, 1958).

The use of operationally defined intervening variables has been described as merely a shorthand summary of empirical facts (MacCorquodale and Meehl, 1948; Marx, 1951). However, Koch (1964) describes neobehaviorists as employing an "intervening variable paradigm" with the claim that exclusive reliance upon operationally defined intervening variables can "offer a guarantee of objectivism <u>at the level of theory</u>" (p. 15). Such an attempt reifies theory along with the psychological concepts involved in the theory. While some behaviorists now describe traditional areas of study such as conditioning, without reference to operational definitions (e.g., Rachlin, 1976), modern behaviorists interested in the study of cognition still rely heavily upon operationally defined intervening variables. Such studies define various aspects of cognition in much the same way as intelligence, and such variables are treated as objective entities which can intervene between the presentation of a stimulus and the consequent response, again, much like intelligence

in the example considered. The next chapter will discuss the continuity of methods used by operational neobehaviorists and modern cognitive behaviorism, after which I shall discuss reifying in a particular subfield of cognitive behaviorism, the information processing approach to thinking and problem solving.

3. COGNITIVE BEHAVIORISM: THE STIMULUS-COGNITION-RESPONSE (S-C-R) MODEL

"Cognitive theorists from Tolman (1932) to Irwin (1971) have had little trouble in operationalizing their theoretical constructs." (Bolles, 1975, p. 272)

3.1 Behaviorism and the cognitive revolution

I suspect that many behaviorists have not been completely comfortable with the dictum that "psychology must discard all reference to consciousness" (Watson, 1913b, p. 163). If my suspicion is correct, such discomfort would, at least in part, account not only for the revival of interest in cognition among current behaviorists, but also for the enthusiastic acceptance of operationism by behaviorists, since operationism has been widely interpreted as providing the means whereby "the behaviorist can eat the cake of consciousness and have it too" (Boring, 1950, p. 659). The following two chapters will examine the continuity between modern behavioristic approaches to the study of cognition and classical neobehaviorist methodology, with particular attention to modern efforts to treat mental phenomena as operationally defined intervening variables.

In his earlier work Watson (1907) had used analogies to human sensation, visual imagery, and feelings to explain the maze behavior

of white rats; even in his manifesto for behavioristic psychology Watson (1913b) noted that animal researchers "feel uneasy and unrestful . . . because we feel forced to say something about the possible mental processes of our animal" (p. 160). Watson proposed to alleviate such uneasiness, not only by eliminating anthropomorphic treatments of the consciousness of animals, but, by offering an interpretation of Darwinism which "recognizes no dividing line between man and brute" (p. 158), extending the ban on anthropomorphism to humans! Since "the behavior of man and the behavior of animals must be considered on the same plane," the behaviorist "can dispense with consciousness in a psychological sense" (p. 176).

> We might call this the return to a non-reflective and naive use of consciousness. In this sense consciousness may be said to be the instrument or tool with which all scientists work. Whether or not the tool is properly used at present by scientists is a problem for philosophy and not for psychology. (p. 176)

This reification of consciousness as a tool is considered a philosophical act in the same sense in which Skinner (1974) considers his recent book to be philosophy: behavioristic psychology cannot reflect upon the resolution not to use consciousness reflectively, which is itself a reflective conscious act (Corriveau, 1972; Kvale and Grenness, 1967). Although other early behaviorists (e.g., Lashley, 1923; Stevens, 1936) do consider conscious reflection as theoretically within the scope of behavioristic psychology, a complete history of the attempts by behaviorists to account for consciousness or to justify ignoring experience is beyond the scope of the present paper.

Boring's (1950, 1953) historical accounts do not credit Watson with much intellectual importance as either a psychologist or philosopher of psychology; he is credited with founding behaviorism, as an "ism," through his skills as a propagandist, polemicist, and dramatic and enthusiastic opponent of introspection (Boring, 1950, pp. 641-663; Flugel and West, 1964, are more complimentary to Watson).

Boring indicates that Watson's (1913a, 1916, 1924) attempts to translate the concepts of thinking, feeling, and mental association into subvocal speech, glandular activity, and conditioned reflexes were generally regarded as inadequate on both logical and epistemological grounds. What came to be known as Watson's "naive behaviorism" was rejected by more sophisticated intellectuals, who gradually developed behaviorism in a manner which culminated logically in an operationistic conception of objectivity within which consciousness could be studied as an operationally defined intervening variable.

> Watson ignored consciousness without denying it, but the behavioristic sophisticates do neither. Rather they keep consciousness, making it objective. They banish the mentalistic terminology and deal with objective date of social or physical entities, or (like Tolman) they introduce intervening variables which reduce to objective data when the operations of their observations are considered. One can eat his cake and have it too. Ingestion leads to absorption. (Boring, 1950, p. 649, my emphasis)

Although Boring prefers Stevens' (1939) term behavioristics to contrast the work of more sophisticated behaviorists with Watson's naive behaviorism, the label neobehaviorism is more common. The

latter term is associated with the works of Hull (1943), Tolman (1932), Boring (1933), and many others. In addition to generally accepting operationism (Boring, 1936, 1945; Hull, 1937; Tolman, 1936), neobehaviorists formulated more complex models of psychological functions than Watson's effort to "write a psychology....in terms of stimulus and response" (1913b, pp. 166-167). Tolman's intervening variables, Hull's mediating processes, and Boring's physiological speculations all point beyond Watson's S-R functionalism in the direction of what Woodworth (1929) first called a stimulus-organism-response (S-O-R) model of psychological functions. Hull's name in particular is associated with neobehaviorism, and his approach has been described as "a herculean elaboration of this S-O-R formula" (Hilgard, 1956, p. 122). Acceptance of methodological behaviorism, an S-O-R model of human psychological functioning, and operationism are virtually definitive of neobehaviorism. Although Skinner's (1931, 1938, 1945) early work, which also goes beyond Watsonian behaviorism, was initially associated with neobehaviorism, Skinner's consistent rejection of intervening processes and his more recent (1953; 1974) repudiation of operationism have led to the label of radical behaviorism rather than neobehaviorism for his position (see section 5.1).

By the early 1950's Boring (1953) regards it as generally established that "operational logic . . . shows that human consciousness is an inferred construct" (p. 187), which psychologists may employ

whenever it seems convenient, although most mid-century psychologists usually prefer to employ the term "under some other name" (p. 187), such as verbal report. Boring completely follows Stevens (1935b, 1939) in operationally defining conscious experience by the public discriminatory behavior from which the experience construct is inferred, and makes explicit Stevens' implication that operational definition can legitimate public discriminatory introspection (which is, however, no more sophisticated than Watson's acceptance of a language method in behavior.)

> As a practical matter in this age of functional psychology most psychologists use all available technics--introspective, verbal, behavioral--and forget about epistemology. Psychology has, however, only recently come to this stage.
> . . . there are relations between consciousness and behavior which make it possible at will, when information is sufficient, to transform the data of consciousness into the data of behavior. Introspection requires verbal report, but verbal report is behavior. The consciousness that the subject "has" is what he describes himself as having, and describing is behaving. Any experimenter who knows fully what went on in his introspective experiment can transform the data of consciousness into the data of behavior, a practice that has been called operational reduction, since it substitutes for the purported object of observation the observational operations themselves. . . . .
>
> There is no doubt that consciousness is going out of fashion in psychology at present, being replaced by these operational substitutes. The change is slow, however, because it is not inevitable. You can also make the reverse transformation, if you wish, transforming behavior into consciousness or its near equivalent, the unconscious. (Boring, 1950, p. 621)

Stevens (1935a) had also pointed out that the operationistic "concept of the conscious is like the concept of the 'unconscious' in that it may be inferred from behavior" (p. 327n.). In a later

paper on the history of introspection Boring (1953) makes it very clear that the introspected data of consciousness are legitimate only as an operationally defined intervening variable. Despite his admiration for Titchener, Boring asserted that Titchener's version of introspection had become extinct by mid-century, and moreover, in Boring's personal opinion "literally immediate observation, the introspection that cannot lie, does not exist" (1953, p. 187). However, historically

> introspection is still with us, doing its business under various aliases, of which verbal report is one. . . . camouflaged introspection is accepted by the modern positivists who hold that the concept of conscious experience has meaning only when it is defined operationally. (Boring, 1953, p. 169)

Despite the camouflaging of introspection, Boring finds consciousness present in psychology, during the era that is widely regarded as the zenith of neobehaviorism, in a variety of guises--as the intervening variables and mediating processes of behavioristic theory, in the data of verbal reports employed in psychophysics in particular and experimental psychology in general, in tests of mental abilities and attitudes, in clinical protocols, and negatively in various conceptions of unconsciousness. Despite the existence of obvious disagreements, Boring portrays the mainstream of mid-century American psychology--neobehavioristic operationism--as regarding even the latter remnants of introspection as, in principle, capable of being studied objectively as operationally defined intervening variables within a physicalistic conception of psychology.

The paradox of operationism--its anti-reifying intentions and reifying tendencies--and the thorough incorporation of this paradox into neobehaviorism, have been described in some detail because I believe that these issues constitute important aspects of the historical and intellectual background of modern cognitive psychology. It was precisely the methodological weapons forged by Watson, Tolman, Stevens, Hull, Boring, and even Skinner which Hebb (1960) advocated taking up in his call to arms for a cognitive revolution in American psychology. And, although the scope of obvious disagreement has not diminished, it has been primarily with the methodological weaponry which we have been examining, that the cognitive revolution has been waged. The result has been an unprecedented subjection of human cognition to reification.

Although important behavioristic treatments of cognition were formulated during the 1950's (e.g., Berlyne, 1954; Festinger, 1957; Hebb, 1953; Maltzman, 1955; Miller, 1953; Miller, Pribram and Galanter, 1960; Newell, Shaw and Simon, 1958; Osgood, 1957), it was not until Hebb's (1960) presidential address that a manifesto for a revolutionary new paradigm (although the latter term was not yet fashionable) involving a "thoroughgoing behavioristics of the thought process" (p. 738) was put forward. More precisely, Hebb called for an American "Civil War" to test the soundness of the "American Revolution" of behaviorism, by proving that the scientific study of mind, consciousness, cognition, etc.,

could not secede from behaviorism, and that the positivistic unity of science could not be broken.

Hebb (1960) seems at first to be proposing that "us cognitivists" (p. 737) rebel but quickly surrender to behaviorism, since he asserts "that 'mind,' 'consciousness,' and so on are references to crudely conceived intervening variables--no more, no less" (p. 739). He even cites Boring (1953) as having proven that "introspection, as immediate knowledge of conscious content, does not exist; consciousness is wholly a construct. . . . the introspector engages in inference, not observation" (Hebb, 1960, p. 739). Hebb further follows Stevens and Boring in concluding that mental constructs, once operationally defined, can be studied by behavioristic methods: "Mind and consciousness, sensations and perceptions, feelings and emotions, all are intervening variables or constructs and properly part of a behavioristic psychology" (p. 740). When Hebb goes so far as to assert that "physiologizing may become necessary" in order to complete "the analysis of thought, the inference from behavior to . . . mediating processes" (p. 744), it becomes difficult to distinguish the new cognitive psychology which he is calling for from the physiological speculations which Boring (1933) and Hebb (1949) had contributed to S-O-R neobehaviorism.

Yet there is a sense in which Hebb's (1960) speech is the manifesto for a new cognitive paradigm that many cognitive psychologists (e.g., Harper, Anderson, Christensen and Hunka, 1964)

have taken it to be. Hebb (1960, p. 744) does insist that the postulate of an ideational process which cannot be expressed in terms of the S-R formula has been experimentally demonstrated. "It is necessary to distinguish between sense-dominated behavior (comprised under the S-R formula) and a broad spectrum of behavior not so dominated" (p. 738), and the existence of the intervening variable cognition can be inferred from the latter type of behavior. In fact Hebb defines "the term cognitive. . . . as a reference to features of behavior that do not fit the S-R formula" (p. 737). In calling for the "serious, persistent, and if necessary daring, exploration of the thought process, by all available means" (p. 744) Hebb appears to be calling for something really new, but his identification of the thought process with an intervening variable (as first formulated by Tolman, 1932), and his insistance that "reference to mental processes" should not be inconsistent "with a fully behavioristic analysis" (p. 738), radically restricts the meaning of "all available means." I believe that Hebb is calling for something new, but in no sense revolutionary. He is calling for a new emphasis upon the intervening variable of cognition within neobehavioristic theory and research, or what I shall call the stimulus-cognition-response (S-C-R) variant of neobehaviorism.

3.2 The stimulus-cognition-response model: A new paradigm?

At the first of a series of symposia on cognitive psychology

which he organized, Solso (1973) simply announced that "a new paradigm has been born in psychology" (p. ix). The thesis that contemporary cognitive psychology represents a paradigm shift away from behaviorism has been enthusiastically proclaimed (Ausubel, 1965; Buss, 1978; Dember, 1974; Hayes, 1975; Jenkins, 1974; Palermo, 1971; Paivio, 1975; Powers, 1973; Reynolds and Flagg, 1977; Segal and Lachman, 1972; Solso, 1975; Weimer and Polermo, 1973), sternly opposed (Berlyne, 1975; Briskman, 1972; Dreyfus, 1972; Hebb, 1974; Libsey, 1974; Mackenzie, 1972; Skinner, 1977; Warren, 1971;), and vigorously waffled over (Boneau, 1974; Deese, 1969, 1972). Apparently the controversy has become so heated that one recent opponent prefers to remain anonymous ("Observer", 1978).

The question is complex and my classification of these authors as simply for or against the interpretation of cognitive psychology as a new paradigm is necessarily rather simplistic. There are widespread differences of opinion concerning the value of various approaches to the study of cognition and the relationship of modern cognitive psychology to the history of psychology. There are also quite divergent interpretations of the meaning of the term "paradigm" as it has evolved in Kuhn's (1962/1970, 1970, 1974) work on the history of science, and of the applicability of this analysis to psychology. Most of my discussion will concern the former set of problems (especially insofar as they relate to reification), and I shall not address the latter questions in any detail. My argument

will, however, question indirectly the assertion that cognitive psychology represents a paradigm shift in the sense of a revolutionary change in worldview.

There can be little doubt that a major revival of interest in cognition has taken place within American psychology since Hebb (1960) issued his challenge for a second phase of the behavioristic revolution. The number of new journals, texts, handbooks, and symposia devoted to cognitive psychology certainly justifies the conclusion that a "cognitive bandwagon" (Battig, 1976) exists in current American psychology. Even Dember's (1974) claim that a "cognitive revolution" in which "psychology has gone cognitive" (p. 161) may be a reasonable description of the phenomenon of greatly increased interest in cognitive topics. This phenomenon appears to be a healthy sign of interest in long neglected subject areas, and both the quantity and quality of work demonstrates that cognitive psychology is clearly more than a passing scientific fad. Nevertheless, I do not believe that it is a paradigm shift, especially since the mainstream of cognitive psychology is hardly the opposite of neobehaviorism that some have taken it to be. For example, Ausubel's (1965) claim that "the contrasting views of cognitive and neobehavioristic theorists" are as fundamentally different "as they can possibly be" (p. 3) has been shown by Moroz (1972) to rest upon a confusion of the meaning of "cognitive" and "phenomenological". Even the most sophisticated proponents of the paradigm shift interpretation of

cognitive psychology (e.g., Buss, 1978; Weimer and Polermo, 1973) make similar errors, assuming that cognitive psychologists generally must accept experience as data in opposition to neobehavioristic objective methodology. As I shall show, this is not the case.

Many leading cognitive psychologists have noted that it is precisely the unlikely combination of explicit interest in mental functioning and behavioral methodology which they consider definitive of contemporary cognitive psychology (Boneau, 1974; Newell and Simon, 1972; Reynolds and Flagg, 1977; Solso, 1973, 1974, 1975; Estes, 1975-1978). These cognitive behaviorists, employing operational definitions of cognitive intervening variables and behavioristic research methods, more or less in keeping with Hebb's (1960) proposals, make up the mainstream of current American cognitive psychology (the stimulus-cognition-response model), although Gestalt psychologists, phenomenologists, etc., are obviously also interested in cognition (see section 5.2).

I believe that it can be demonstrated that the S-C-R approach contains most of the fundamental assumptions of classical neobehaviorism, including a) the goal of predicting and controlling behavior, b) the rejection of experience and the reifying of behavior as data, c) the acceptance of hereditary variables as partial determinants of behavior, d) the positing of complex systems of variables intervening between stimuli and responses, and e) operationism. Since Watsonian behaviorism shares only the first two of these assumptions, I believe

that a compelling, although indirect, case against the paradigm shift interpretation of cognitive psychology can be established, since, to the best of my knowledge, no one has seriously entertained the extremely dubious claim that neobehaviorism constituted a paradigm shift away from Watson's naive behaviorism.

Most, although not quite all, of the neobehavioristic cognitive psychologists utilize information processing concepts in their explanations of cognition. Erickson and Jones (1978) point out that within cognitive psychology "the information processing language (metaphor?) is almost universal" (p. 61). The impression that cognitive psychology might constitute a new paradigm may, in part, have been generated by the erroneous belief that the new model of information processing computer programs could, ipso facto, provide a new paradigm. The most influential expositions of this approach have been advanced within the context of thinking and problem solving literature (e.g., Newell and Simon, 1972), which will be considered in detail in the next chapter. Since a few (Kreitler and Kreitler, 1972; Paivio, 1975) of those involved in the cognitive revolution in psychology have rejected information processing concepts while remaining strict behaviorists and operationists, and others (e.g., Johnson, 1972; Cohen, 1977) maintain that information processing theory is only one of several valid behavioristic approaches to cognition, I have termed the more general behavioristic approach to cognitive psychology the stimulus-

cognition-response (S-C-R) model, although many cognitive behaviorists identify cognition with information processing, and could be said to espouse a stimulus-processing-response (S-P-R) approach to psychology. While I shall concentrate my criticism of this approach on the reifying of thinking (and thinkers) committed by the leading proponents of information processing theory, the S-P-R approach has by no means been limited to the thinking and problem solving area, and is playing a major role in research and theories dealing with perception, memory, linguistics, learning, creativity, and cognitive psychology generally (Cohen, 1977; Estes, 1975-1978; Hayes, 1978; Lindsay and Norman, 1977; Massaro, 1975; Neisser, 1976a; Reynolds and Flagg, 1977).

Probably the best known candidate for status as a new cognitive paradigm among non-information processing approaches is the "neomentalism" of Paivio (1975). While Paivio vaguely associates his approach with a new paradigm, he claims to be a "tough minded behaviourist" and asserts that

> neomentalism is an objective science based on explicit operational procedures. Mentalistic concepts are defined by patterns of performance that permit strong inferences to be made about the nature of private events. The subjects' descriptions of their conscious experiences . . . are not essential to the inferential process. (p. 274)

Paivio is aware that informational processing concepts can be translated into S-R terminology (see below and Millenson, 1967; Suppes, 1969), and he nevertheless maintains that his own approach is "more behaviourally inclined" than S-P-R approaches. This puts him

in the company of radical behaviorism (see section 5.1), but Paivio distinguishes his approach from Skinner's and suggests that there are some (unspecified) associative mechanisms relating verbal symbols and visual images which are acquired in ways which neither classical nor operant conditioning can account for (p. 285).

I think that Paivio is best interpreted as exploring the intervening variable of imagery within the neobehaviorist paradigm. It is not even clear whether Paivio himself believes that his "behavioural mentalism" represents a new paradigm. He associates his investigation of "intervening mental entities" with what he calls Hebb's (1949) "neurobehavioural mentalism" (p. 264), and defends his use of "operational procedures" to link hypothetical inner events with observable behaviour as "the standard procedure for studying intervening variables and symbolic processes for at least 60 years" which differs today only "in the precision with which such procedures are being used" (p. 270). In reviewing Paivio's (1971) earlier book, Neisser (1972) notes that Paivio's experimental work is much clearer than his theorizing, and that his reliance upon associationism "ends by endorsing the orthodox behaviorist view" (p. 630).

Another factor which may have led some to conceive of cognitive psychology as a new paradigm was Ulric Neisser's (1967) initial textbook in the area, which played a significant role in defining the field. While Neisser himself is neither an operationist

nor a behaviorist, the same cannot be said about many other leading authors in the area of cognitive psychology. Neisser's (1967) text presents "the cognitive approach" as "essentially incompatible" (p. 5) with behaviorism; he criticizes the naive realism of many psychologists, and asserts that "the world of experience is produced by the man who experiences it" (p. 3).

Since Neisser (1963, 1967, 1976a, 1976b) has also published several lucid critiques of the adequacy of information processing theories in accounting for human cognition, I feel that it is unfortunate that in his initial text he made sufficient concessions to the latter approach to leave the door open for information processing theorists to dominate the field of cognitive psychology and to reify cognitive processes.

Neisser (1967) suggested that his text "might be called 'Stimulus Information and its Vicissitudes,'" and that "the term 'cognition' refers to all the processes by which sensory input is transformed, reduced, elaborated, and used" (p. 4). Furthermore, "the task of a psychologist trying to understand human cognition is analogous to that of a man trying to understand how a computer has been programmed. In particular, if the program seems to store and re-use information, he would like to know by what 'routines' or 'procedures' this is done" (p. 6). Finally, after noting that early attempts to measure the information bit rates in human cognition (Miller, 1953; Quastler, 1955) had not proven useful in psychological

theory, Neisser noted that:

> Although information measurement may be of little value to the cognitive psychologist. . . . computer programming has much more to offer. A program is not a device for measuring information, but a recipe for selecting, storing, recovering, outputting, and generally manipulating it. As pointed out by Newell, Shaw, and Simon (1958), this means that programs have much in common with theories of cognition. (p. 8)

Neisser quickly followed this statement with the observation that attempts to develop computer programs which were claimed to be realistic theories of cognition (such as Newell, Shaw, and Simon, 1958, had done), did not appear likely to do "even remote justice to the complexity of human mental processes" (p. 9). Neisser furthermore consistently used the term information in its everyday meaning of knowledge about the world, unlike information processing program theories of cognition employed by operational behaviorists, who leap back and forth between the everyday meaning and the technical communication theory meaning of information and its everyday meaning (Brandt, 1970), and reify the abstract technical processing of information as a hidden cause of actual human cognition.

Although Neisser (1967) made it clear that he regarded computer programs merely as heuristically useful analogies to human cognition and not as theories of cognition per se, his own concessions to the computer simulation approach may have contributed to what Neisser (1976a) was later to deplore as the domination of experimental cognitive psychology by "mechanistic information processing models, which treat the mind as a fixed capacity device

for converting discrete and meaningless inputs into conscious percepts" (p. 10, see also Neisser, 1976b). Neisser's more recent book is strongly concerned that cognitive psychology develop in a way which is relevant to human cognitive activity as it occurs in the complex everyday world. Although Neisser did, very briefly, mention such concerns in his earlier text (1967, p. 305), I would speculate that Neisser in 1967 was perhaps too closely involved in the limited realm of experimental laboratory studies or cognition to notice that his use of (what he regarded as) the analogy to computer programming would give a powerful legitimation to operational behavioristic approaches to cognitive psychology, which Neisser has otherwise consistently opposed. In any event, recent texts on cognitive psychology which explicitly endorse an information processing approach based upon operational definitions and methodological behaviorism are able to cite Neisser's (1967) text, selectively, in support of their approach to the field.

In a recent text which claims to present "an updated version of Neisser's excellent book" which provided "the model for our book, in both form and intent" (p. xii), Reynolds and Flagg (1977), a completely neobehavioristic information processing approach to the topics of perception, memory and language is put forward (curiously they do not consider thinking and problem solving within the scope of cognitive psychology). Other recent texts (Anderson, 1973; Cohen, 1977; Hayes, 1978) also are more behavioristic and favor

information processing models much more than Neisser's original cognitive psychology text, although these latter authors do not claim (falsely) to be following Neisser's lead to the extent that Reynolds and Flagg do. Despite their claim that cognitive psychology is a revolutionary new paradigm, Reynolds and Flagg admit that "the methodological aspects of neo-behaviorist psychology were retained" (p. 6). The methodology of neobehaviorism is described simply as "operationalism," while the methodology of cognitive psychology is referred to as "liberated operationalism" and "simulation" (p. 5). The precise nature of this liberation of operationism is not specified, although it presumably refers to a willingness to use operationally defined concepts which refer to mental as well as behavioral processes, since "liberated empiricism" is described as "the introduction of some nonobservable characteristics of the organism as long as these could be operationalized or directly tied to observable behavior" (p. 6). Since nearly all operationists since Stevens have advocated such procedures, it is difficult to accept this as a new, revolutionary, or liberated methodology. When Reynolds and Flagg conclude that cognitive psychology rests upon the "methods perfected by neo-behaviorists" (p. 15, my emphasis), it becomes clear that the new paradigm represents nothing new methodologically. Perfect methods require no liberation.

Like many others who support the interpretation of cognitive psychology as a revolutionary new paradigm, Reynolds and Flagg claim that recent cognitive psychology offers a completely new active

conception of the subject of psychological processes. I do not believe that this claim is justifiable.

> The new cognitive psychology . . . takes its . . . reliance upon operationalism from behaviorism and neo-behaviorism (as well as borrowing modeling and simulation techniques from computer science and information theory). The new cognitive psychology is an empirical mentalism.
>
> Active Man
> This new empirical mentalism we've described does not qualify the revolution that took place in psychology as a paradigm shift in Kuhn's terms. So far all we have described in an unlikely marriage between old methods and older content areas, with the addition of some extradisciplinary garnishings. What qualifies this as a revolution is not content or method, but rather the new terms of outlook, or pre-theoretical assumptions. All . . . theories . . . to this point see a person as a passive reactor or storer of information. . . . The cognitive view, on the other hand, assumes a constantly active organism that searches, filters, selectively acts on, reorganizes, and creates information. This view is totally new and sets the cognitive approach apart from all prior views of mind. A revolution has occurred. (Reynolds and Flagg, 1977, pp. 10-11)

The claim that no prior view has emphasized mind as an active agent is quite impossible to sustain. Without even considering philosophic approaches that emphasize the activity of mind (e.g., Kant), within experimental psychology, Gestalt theories of perception and thinking considered the subject as the active agency of mental acts (Kohler, 1929; Wertheimer, 1945/1959; Ellis, 1938). The thought-psychology of Otto Selz emphasized the active operations of the thinking subjects in opposition to the passive concept of the subject of classical associationism, and this view has influenced recent English language psychology through the works of Duncker (1945) and de Groot (1965, 1966). Reynolds and Flagg credit much of the "totally new" view of

the active, constructive subject of cognitive psychology directly to Neisser (1967), although he specifically credits Bartlett's (1932, 1958) concept of active mental schema as a decisive influence upon his view, along with nineteenth century "act psychology" (Neisser, 1967, p. 10). Neisser's approach has long historical roots as his references to Brentano and Gestalt psychology make clear; this approach certainly did not suddenly appear in 1967, as Reynolds and Flagg imply. The attempt to impose operational neobehaviorism and information processing theory upon Neisser's position is in keeping with the general direction of other recent cognitive psychology texts (Cohen, 1977; Anderson, 1973; Hayes, 1978), but it is surely not with Reynolds and Flagg's expressed purpose of following the intent of Neisser's text, although Reynolds and Flagg are apparently unaware of this.

A final illustration of the extent to which behavioristic and information processing approaches have come to dominate the field of cognitive psychology is provided by Estes' (1975) state of the field address in the introduction to his six volume Handbook of Learning and Cognitive Processes (1975-1978). Estes (1975) asserts that "the scientific study of the processing of information . . . defines the field . . . of cognitive psychology" (p. 1), and that "observed behavior of organisms must be our principal index concerning states of information." (p. 2). With this definition of cognitive psychology, some standard behavioristic assumptions, and a strong

belief that information processing models may provide the means for integrating an otherwise divergent collection of research, Estes' "State of the Field" of cognitive psychology argues in a reasonable though roundabout fashion, that behavioristic learning theorists both are, and should be, engaged in the difficult task of inferring "uniformities of processes that lie behind variations in behavior" (pp. 8-9). Estes seems to steer a middle course between the radical behaviorism of Skinner and the information processing neobehaviorism of Newell and Simon (see section 5.1), but he in no way avoids reification by attempting a liberal, moderate integration of disparate extremes within the reifying behavioristic paradigm. His position, is, I believe, no less reifying than the positions of his more extreme fellow behaviorists, but he does avoid some of the foolishness of boastfully proclaiming the reality of contingencies and information processing that we shall encounter in the next chapters.

Estes believes that Watson's original behavioristic revolution was correct in its decision to emulate other biological and physical sciences, and to strive for the prediction and control of behavior utilizing objective measurements of overt behavior as its only data (p. 4). Such a program for psychological science has been successful in the sense that:

> It seems clear that a full description of the behavior of any organism can, in principle, be given in stimulus-response terms. . . . If we can specify the environment sufficiently fully, there is little doubt that principles of operant conditioning built on this framework can enable us to predict and control the behavior of any organism to almost any desired

> degree. Demonstrations of this ability are seen routinely in the cage of the animal trainer, in custodial institutions for the mentally retarded, to some extent even in prisons and reformatories. (p. 8)

The reification in this statement is that of traditional behaviorism (Ingleby, 1968), and the statement is surely untrue (Breland and Breland, 1961, 1965; Weimer and Polermo, 1973, 1974). What is interesting is that someone believing these statements should show interest in the study of cognition. The reason for such interest is apparently that in order to advance "the progressive refinement of prediction and control. . . . to account for behavior of organisms outside of highly restricted environments. . . . we find ourselves forced to begin . . . the making of inferences . . . that lie behind . . . behavior" (pp. 8-9).

Estes claims that our power to predict and control behavior does not come from direct observation of behavior, which reveals only endless variety, but from explanatory mechanisms and general principles inferred from behavior, with the most abstract principles having the widest applicability (pp. 7-8). Principles of operant conditioning are generalizable from the laboratory to situations involving young children and the mentally retarded, while

> Evidence that the same mechanisms are at work can be detected in the adult by means of delicately controlled experiments, but in normal behavior these mechanisms operate only against the background of complex symbolic processes that must be understood in the context of broader theories of memory and cognition. (p. 22)

The goal of predicting and controlling normal adult behavior in unrestricted environments leads "straight into the psychology of

memory--the heart of cognitive psychology" (p. 9). Although both learning and memory are abstractions inferred from behavior, the principles of memory are more abstract and more widely applicable, as Estes explains from the operational definition of each term:

> Learning always refers to some systematic change in behavior or behavioral disposition that occurs as . . . a property of a system that comprises an organism together with some limited aspect of the environment. (p. 9)
>
> The distinguishing property of concepts of memory is that they refer not to any particular organism environment system but rather to some property or state of the organism which . . . resulted in the consequence of altering the organism's potentialities for a response in . . . new . . . situations. (p. 10)

The latter more general concepts are required in cases involving "'control processes' (Atkinson and Shiffrin, 1968) in human memory and the recognition that learned voluntary strategies play a major role in virtually all aspects of human learning" (p. 7). Prediction and control in such cases has been limited by the inability of operant conditioning conceptions to characterize a change in the organism which is independent of a particular environment. This means that conditioning theories cannot describe "what is learned or what is remembered" (p. 16). However, this "what" can be characterized by (reifying the abstraction) information as it is processed by a computer program, thus allowing the study of cognitive processes to "achieve both the appearance of greater objectivity and the fact of greater testibility" (p. 5).

> The new art of computer programming placed in the cognitive psychologist's hands a new tool of such unprecedented scope

> . . . as to sweep away all the technical hindrances to the construction of formal models for complex mental processes, not only those manifested in simplified laboratory tasks. . . . Even more important . . . is the role of the computer-based information-processing system as a source of . . . a new theoretical framework . . . of human memory. (p. 18)

Thus information processing theories offer the crucial possibility of explaining the acquisition of learned voluntary strategies as memory routines, and thus, the potential to predict and control the unconfined behavior of adult humans over long ranges of time and situation.

> It is essential to keep in mind that even the most sophisticated human behavior is more than a collection of voluntary strategies and that . . . we need also to understand how strategies and control processes are developed and maintained and how structural constraints impose limitations upon the possible output of control processes. (p. 7).

For Estes the entire purpose of studying cognitive psychology is the prediction and control of human behavior in situations where the direct exercise of stimulus control is not possible. Estes' conception of cognitive psychology is not only behavioristic in advocating the goals of prediction and control of behavior, but also in advocating that human and animal psychology should employ the same methods and goals. Estes claims that cognitive psychology, and especially "informational interpretations may help to maintain and extend lines of communication between : . . the fields of human and animal learning" (p. 20). The study of cognitive psychology, if it is interpreted as the inference of intervening variables from behavioral data according to operational neobehavioristic procedures,

is just as applicable to animals as it is to humans. The recent appearance of a major volume devoted to research on "cognitive processes in animal behavior" (Hulse, Fowler, and Honig, 1978) provides one more indication of the essential continuity of neobehaviorism and cognitive psychology.

This latter development in no sense shows psychology to have come full circle and returned to the early projections of introspected human consciousness onto animals of Watson and others (Watson, 1907; see especially Washburn, 1908). Rather the question of whether cognitive processes are conscious or not is essentially irrelevent to the mainstream of cognitive psychology, which accepts the neobehavioristic position described by Boring (1950, 1953) as the ingestion of consciousness, which becomes merely an epiphenomenon which may be treated as any other intervening variable inferred from behavior. In fact, many years after Hebb (1953) pleaded with Canadian psychology to take up the serious study of conscious human thought, after Burt (1960, 1962) asked British psychologists to consider consciousness and introspective data along with analyses of behavior, and after Hebb's (1960) famous challenge to American psychology to undertake "the behavioristic treatment of mind or consciousness" (p. 739), Mandler (1975) still found it necessary to make very similar pleas for the consideration of consciousness by cognitive psychologists! Greater interest in cognitive topics has not produced a revolutionary change in world view or a paradigm shift, especially since the

cognitive concepts are operationally defined in the same way that neobehaviorists had defined the intervening variables associated with the S-O-R model of psychological functioning.

3.3 The evolution of humans as flow charts

Any attempt to describe essential continuities between the classical formulations of neobehaviorism by Boring, Hull, and Tolman, and more recent information processing theories of cognitive psychology should recognize that certain clear changes have taken place. The use of mathematical communication theory to describe human functioning (Broadbent, 1958; Hovland, 1952; Miller, 1953, 1956; Quastler, 1955) obviously could proceed only after the development of that formal mathematical theory (Shannon, 1948; Shannon and Weaver, 1949). Similarly the evolution of theories of human cognition in the explicit form of computer programs (Newell, Shaw, and Simon, 1958; Feigenbaum, 1961; Reitman, 1965) or in the form of flow charts often written as the first stage or as summaries of computer programs was obviously dependent upon the development of modern computers and programming. Recent surveys of these approaches to cognitive psychology have been offered elsewhere (Estes, Vol. 5, 1978; Hayes, 1978; Lindsay and Norman, 1977), and I offer only a very general, almost caricatural, flow chart of the stimulus-processing-response model of human psychological functioning for the purpose of comparing this general conceptualization with the historical evolution of more realistically

intended diagrams. Figure 3.1 presents this general outline of the S-P-R model which I have freely adapted from an information processing flow chart of perception given by Neisser (1976a).

Neobehavioristic theories written before 1950 obviously could not take this form, and Hull's hypothetical-deductive system seems particularly difficult to describe in terms compatible with modern flow charts. However, when the complex system of postulates is expressed in the diagram of stimuli-intervening variables-responses that Hilgard (1956) has used to describe Hull's (1952) final systematic formulation (Figure 3.2), it appears to be at least potentially amenable to formulation in terms of information processing theory. The same is true of the complex diagrams of independent, intervening, and dependent variables that Tolman (1936) used to describe his purposive, operational, cognitive behaviorism (Figure 3.3). Moreover, while Tolman admits that an entire thick book would be required to work out the details of his (1936) original formulation of operational behaviorism, his summary seems eminently compatible with modern cognitive behaviorism.

> Operational behaviorism comprises two main principles: (a) It asserts that the ultimate interest of psychology is solely the prediction and control of behavior. (b) It asserts that psychological concepts, i.e., the mental capacities and mental events--may be conceived as objectively defined intervening variables. And it asserts that these intervening variables are to be defined wholly operationally. (Tolman, 1936, p. 129)

Even some proponents of the interpretation of cognitive psychology as a new paradigm have noted some significant similarities

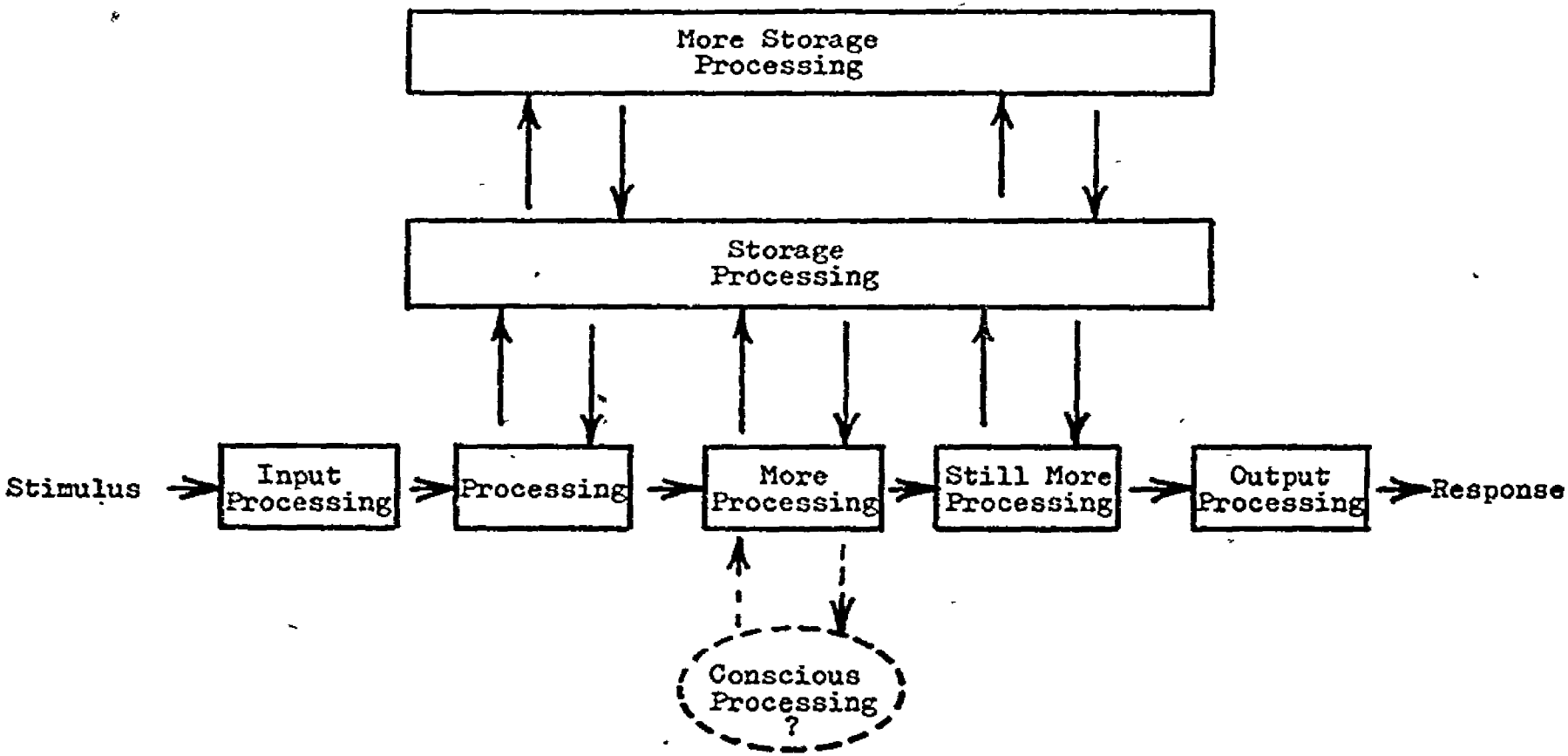
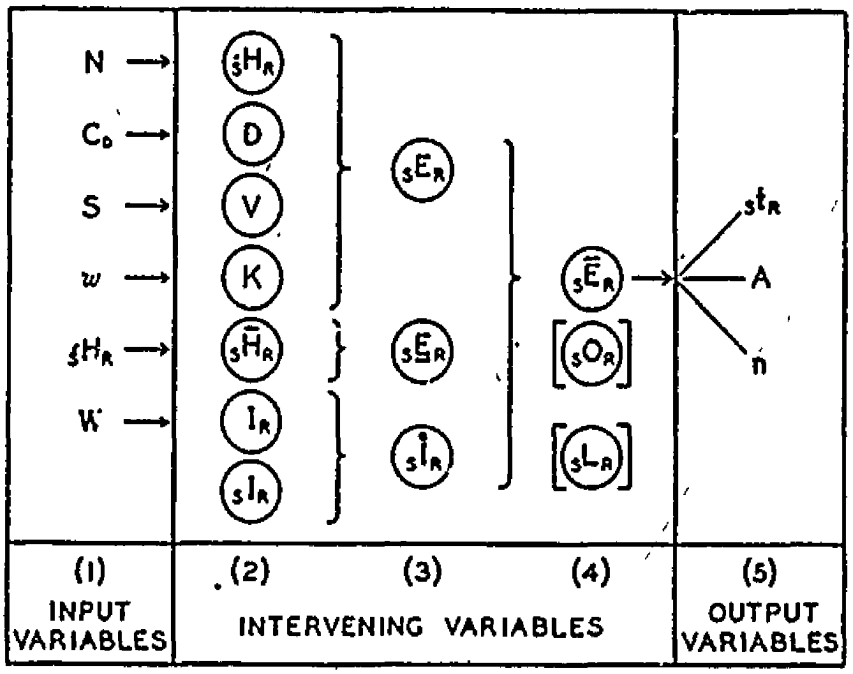


Figure 3.1. The information processing (S-P-R) model of cognition. Freely adapted from Neisser (1976a).



Column (1)

N, number of prior reinforcements
C_D, drive condition
S, stimulus intensity
w, amount (weight) of reward
$_sH_R$, strength of a habit based on same response conditioned to another stimulus
W, work required in responding

Column (2)

$_sH_R$, habit strength
D, drive
V, stimulus-intensity dynamism
K, incentive motivation
$_s\bar{H}_R$, generalized habit strength from related habit
I_R, reactive inhibition
$_sI_R$, conditioned inhibition

Column (3)

$_sE_R$, reaction potential
$_s\bar{E}_R$, generalized reaction potential
$_s\dot{I}_R$, aggregate inhibitory potential

Column (4)

$_s\bar{\bar{E}}_R$, net reaction potential
$_sO_R$, oscillation of reaction potential
$_sL_R$, reaction threshold

Column (5)

$_st_R$, reaction latency
A, reaction amplitude
n, number of non-reinforced responses to extinction

Figure 3.2. A diagrammatic summary of Hull's (1952) final system of behavior. From Hilgard (1956).

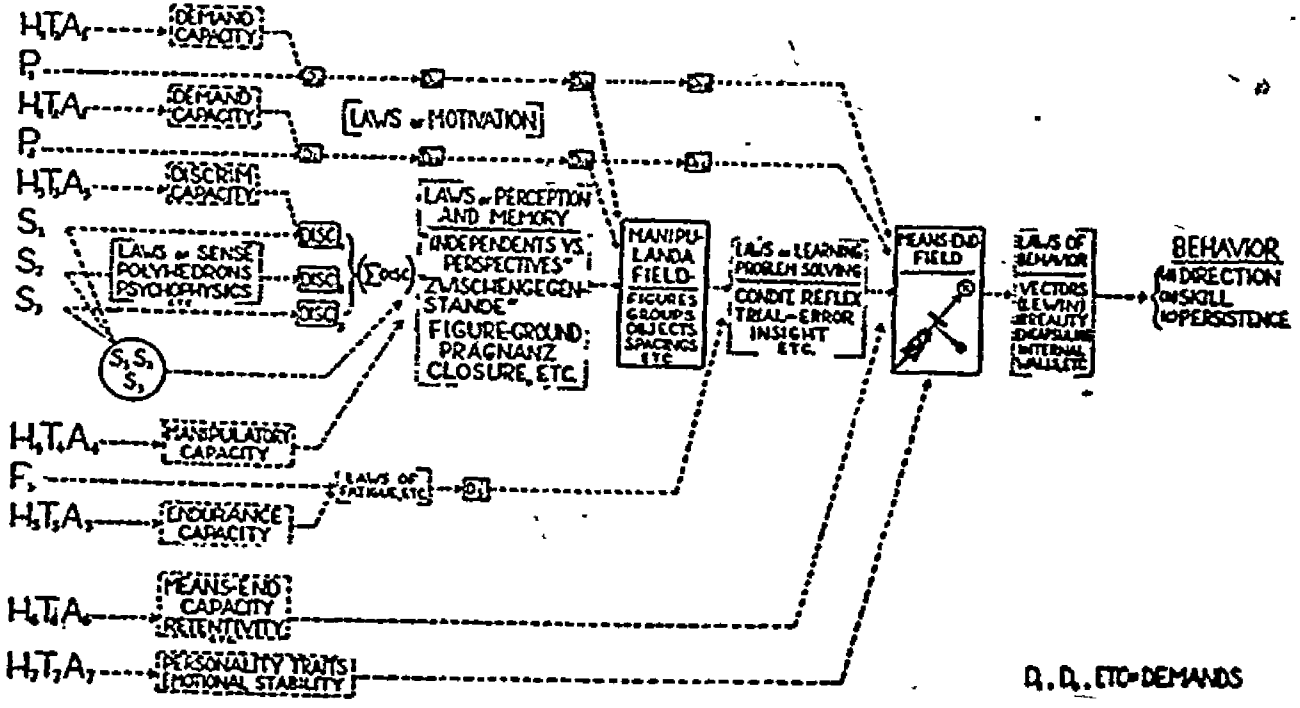


Figure 3.3. Tolman's original diagram describing operational behaviorism. Subscripts denote different measurements of the independent variables H (heredity), T (training), A (age), P (physiological drives), and S (stimuli). From Tolman (1936).

between modern information processing concepts and ideas of Hull and Tolman. In the closing essay in the Handbook of Learning and Cognitive Processes, Estes (1978b) lists nearly a dozen homologies between the concepts of learning theory and those of information processing. Although his purpose is not primarily historical, Estes points out "the close correspondence between . . . Hull's conception of stimulus trace and . . . primary memory. In fact . . . the concept of stimulus trace shades with no perceptible transition into . . . short term memory" (p. 279). Estes adds that "the central interpretive concept in information-processing models . . . of memory search or scanning" is not essentially different from "the concept of vicarious trial and error (VTE) developed many decades ago by Tolman (1938)" (p. 280). These are more than incidental anticipations of modern cognitive theory. Whether or not one agrees with Estes' assessment of memory as "the heart of cognitive psychology" (1975, p. 9), it is clear that the storage and retrieval of information is essential to the computer simulation of human cognition. Without the "memory processing" entities in Figure 3.1, the latter diagram is nearly identical to a diagram of the S-O-R model given in Figure 3.4.

Figure 3.4 is Boring's (1937) diagram of what he calls an "electrical analogy" which depicts "the organism as a series of open communicating systems, transmitting action in the direction of stimulus to response" (p. 447). When we consider that Boring intended memory to be included in his diagram "in the region 8-12

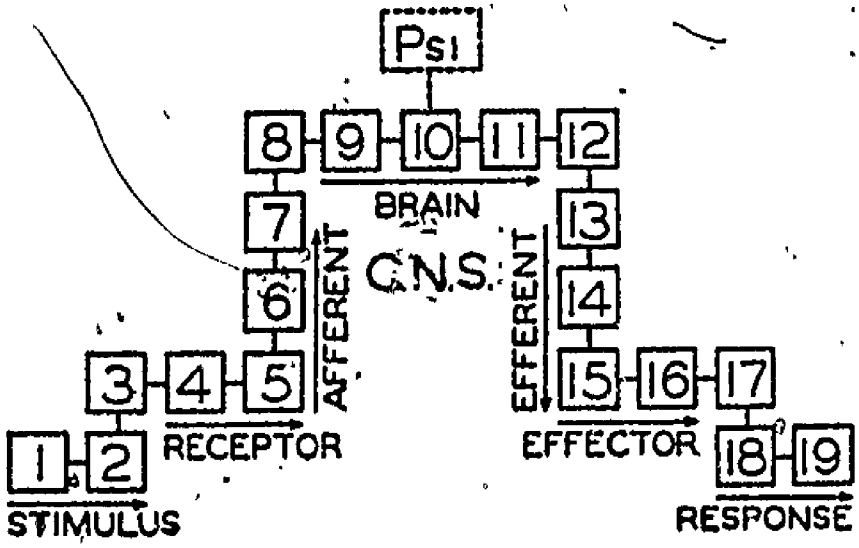


Figure 3.4. Boring's schematic diagram of the organism as a series of open communicating systems. Psi represents consciousness or phenomenal experience. From Boring (1937).

as a trace of the organism's past" (p. 449), then the anticipation of the modern information processing flow charts is indeed striking. If the general conceptualizations (though, of course, not the details) of modern cognitive psychology are so thoroughly anticipated by the ideas of popular and well known neobehaviorists, then the case for a new paradigm is greatly weakened. I believe there are two relevant objections to the claim that Boring's diagram anticipates information processing flow charts: (a) Boring's diagram does not provide for feedback (his arrows point in only one direction), (b) Boring said his diagram describes the transmission of "action," which is quite different from "information." I don't think either objection is telling; they can be reduced to, at most, a matter of relative emphasis. (An adequate answer to the final question of whether a "mere" change in emphasis could constitute a new scientific paradigm would require a lengthy discussion of the philosophic and historical criteria for Kuhn's term which would not be directly relevant to psychological theory and might well result in an answer qualified by "under some circumstances." It seems hardly possible to doubt that American psychologists today are emphasizing cognition more than they did 30 or 40 years ago; my point is that many prominent spokespersons for current cognitive psychology conceive cognition, and other concepts, in the same manner as earlier behaviorists and operationists, with the same tendencies to reify described in the previous chapters.)

Miller, Pribram, and Galanter (1960) put the concept of feedback at the center of cognitive, or as they called it, subjective behaviorism. They proposed the concept of a feedback loop, taken from physiology, replace another physiological concept, the reflex, as the basic unit for psychological analysis. Boring and the other neobehaviorists were elaborating systems of mediation between the basic unit of an S-R connection, while Miller et al. argued for completely replacing this unit with one they named the TOTE (for test-operate-test-exit). This new unit was readily describable in computer flow chart form, and since loops are an important element in many computer programs, Miller et al. argued by analogy that human plans might be made up of physiological feedback loops as computer programs are made of loops of operations and instructions.

Boring's diagram lacks not only any feedback loops, but it also fails to anticipate the theory of information and channels for processing information developed by Shannon (1948; Shannon and Weaver, 1949), which had been incorporated into psychology by Miller (1953, 1956) and others. Figure 3.5 is a diagram which is typical of the flow charts of the late 1950's (Chase, 1978), and which was published about halfway between the appearance of Boring's (1937) diagram and the present (Broadbent, 1958). Figure 3.5 illustrates both an information processing channel and feedback of information. Boring's diagram does seem old fashioned by comparison; the same is true of the text of his (1937) article describing his

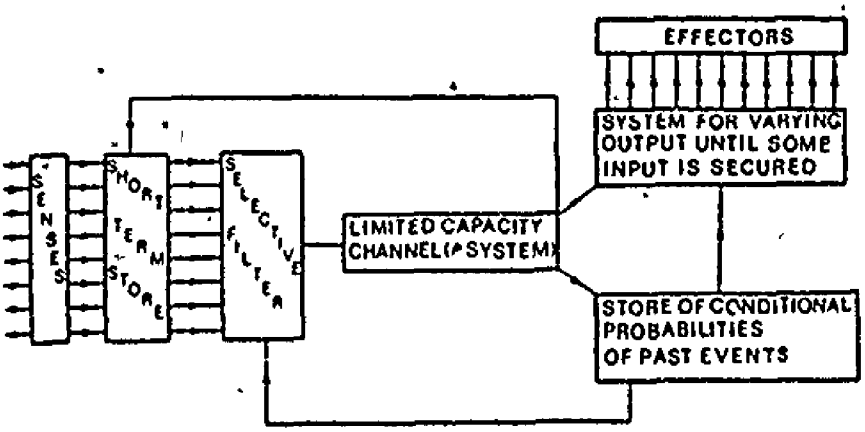


Figure 3.5. A flow diagram of a person as a communication channel. From Broadbent (1958).

diagram, which completely lacks the modern jargon of information channels, bits, and chunks, as well as TOTE feedback units and computer programs. However, what might be called the "substance" of Boring's thought stands up to comparison rather well.

Boring is keenly aware of the possibility of feedback, although he does not use the modern term. His essay opens with the observation that Newton's third law limits the concept of cause and effect so that we have "not a serial progressive action, but that mutual interdependence of two events which makes them into a system" (p. 445). Furthermore, Boring describes his diagram as "a series of open communicating systems" where a system is defined as "open" in that it communicates with antecedent or consequent systems" (p. 447). Certainly, as a matter of relative emphasis, Boring believes that cases where reaction, or communication with an antecedent system, is negligible, are fairly common in psychology, whereas Miller et al. consider the TOTE feedback loop as the essential new unit for all psychological analysis. Similarly, Boring does not emphasize his concep - of Aufgabe, attitude, and memory, to the extent that Miller et al. emphasize Plans, although the former concepts play a role in Boring's formulation similar to the concept of Plan, without, of course, the analogy to computer programs. Tolman's (1948) conception of a "cognitive-like map . . . indicating routes and paths" (p. 199) is an even clearer anticipation of Plan and Images, as Miller et al. (1960, pp. 8-9) acknowledge.

Furthermore, the use of feedback loops as the basic unit of analysis has been shown to be a much less radical departure from analysis into S-R units than was originally presumed. Boring (1937) is quite clear that he is describing an artifically isolated S-O-R unit which is actually connected, simultaneously and sequentially, with a complex pattern of other such units. A feedback loop, described in terms of test-operate-test-exit units, is really nothing more than a sequence of two S-R units, where the second stimulus is dependent upon the first response. In fact the two systems of notation have been shown to be mathematically isomorphic.

> We are led to conclude that Operations and R's are indistinguishable. We have already shown that tests and a certain S-R configuration called a discrimination are indistinguishable. . . . Any . . . behaviors . . . can be presented by the two notations. Both notations provide mechanisms for unit definition, sequential order, branching, testing loops, environmental changes, and behavioral operations. . . . the two languages are fundamentally the same, since the conceptual units of both are observed behaviors, and the facilities for testing-discriminating. In one notation the configurations of terms is called a program (plan); in the other it is a set of contingencies, but the referent is the same. What differences there are lie in emphasis. (Millenson, 1967, pp. 316, 319)

Since the mathematical identity of S-R and feedback notations has been verified by other mathematical psychologists (Suppes, 1969) and accepted by at least some leading cognitive theorists (Johnson, 1972; Simon, 1969a), it seems clear that Boring's classical neobehaviorism and some versions of information processing theory are not radically different. While in 1937, Boring did not express his theory in terms of communication theory or computer programs, he

did become an early advocate of the indistinguishability, for behavioristic operationists, of cybernetic robots and human subjects (Boring, 1946).

As his reference to Newton makes clear, Boring (1937) intends his diagram to describe the flow of some form of physical activity, rather than purely abstract information. But modern information processing theorists (see e.g., Newell and Simon, next chapter) also require some underlying physiological mechanisms which allow biological systems to process information (a situation in which hypostatization is an obvious danger). Boring's systems do not merely transmit random energy, but also the capacity to discriminate; the "independent variability" (p. 449) of the stimulus is transmitted as "successive differentiation in the nervous system" (p. 451). While Boring's concept is not so precise as Shannon's theory of information communication, capacity to discriminate by means of transmitting differentiation is certainly related to Shannon's notion of transmission of uncertainty reduction.

Boring's vague notion may even be more accurate, for Shannon's theory of communication channels has proven not to be a good model of human information processing. People process (read out loud, memorize) lists of common words, letters, decimal or binary numbers at about the same rates, even though the different terms contain very different amounts of information (Miller, 1956; Neisser, 1967).

There was no such thing as <u>the</u> rate of transmitting information. . . .

> Under some circumstances (for example, naming numbers, pointing at lights), reaction time does not even increase with the stimulus information, leading to the nonsensical prediction that the rate of information transmission is infinite. (Chase, 1978, p. 29)

Consequently, the emphasis within the information processing approach has shifted from formal measures of information bit rates to an emphasis upon processing and computer program models. Flow charts like Figure 3.5 which include formal information channels have been largely abandoned in favor of diagrams more like Figure 3.1, or a more specific example of the contemporary approach, Figure 3.6. Figure 3.6 has been called a typical second generation flow diagram of man (Chase, 1978), and diagrams of this type are now widely accepted in cognitive psychology.

> Though there are still many variations . . . a substantial consensus has emerged concerning the overall outline or flow diagram characterizing the normal adult . . . information processor. (Estes, 1978a, p. 11)

Figure 3.6, the final flow diagram that I wish to consider, illustrates several points. First, it shows that Figure 3.1 is not merely a caricature developed by opponents of the information processing approach (Neisser and myself), and that such flow charts are indeed seriously advocated by proponents of information processing models of cognition. Secondly, it illustrates the already mentioned abandonment of communication channels as described by Shannon in the charting of the flow of information in humans. Thirdly, this final diagram illustrates the extent to which the information processing approach is committed to methodological

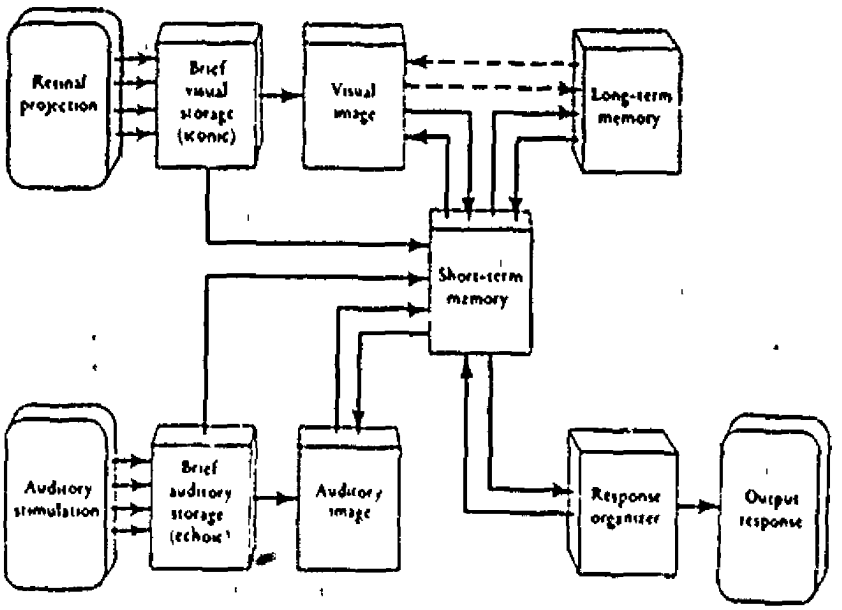


Figure 3.6. A modern flow diagram of a person as an information processor. From Haber and Hershenson (1973).

behaviorism, since iconic storage appears as a specific entity in flow charts only after Sperling's (1960) demonstration of the existence of the icon in terms of measurements of overt behavior, although the phenomenon of iconic storage had been demonstrated by introspective methods at least 30 years earlier (Bridgin, 1933; Woodworth and Schlosberg, 1954). In addition to the widespread interpretation of verbal reports as data (see e.g., next chapter), Figure 3.6 illustrates the further continuity of the information processing approach with the neobehavioristic tendency reported by Boring (1950, 1953) to characterize consciousness as an intervening variable, "usually under some other name" (1953, p. 187), in this case short term memory (STM).

Finally, Figure 3.6 and the entire phenomenon of flow chart diagrams of human information processing are illustrative of the reification of human beings by the cognitive variant of neobehaviorism. While I have usually tried to use a neutral (and non-sexist) terminology to describe these flow charts, such as "models of cognition" or "models of psychological functioning," they are, in fact, more often described as "models of man" (e.g., Chase, 1978, p. 30, describes Figure 3.6 in this way). Classical (S-O-R) neobehaviorism's organism, a thing-like mediator between stimuli and responses, has gradually evolved into a thing-like processor of inputs into outputs. Such "models of man" hypostatize information and systems for processing it, while reifying human beings and their cognitions.

Neisser (1976a) predicts that changes will need to occur

in the current S-P-R flow diagrams, since they have little ecological validity and are unrealistic and irrelevant to everyday life. I shall not attempt to guess what the flow diagrams of the future will be like. However, examination of the evolution of such flow charts over the past 40 years provides further evidence that no clear, revolutionary new paradigm of cognitive psychology has emerged. Certainly some clear changes have taken place; feedback loops have become more important and more explicit, communication channels have come and gone, and modern computer influenced models resemble Boring's diagram of a typical instance of information flow much more than Tolman's or Hull's attempts to list all relevant quantitative variables. However, I do not believe that these diagrams suggest any "graphic" evidence of a transition from a behavioristic to a cognitive worldview. Tolman's original conception of operational behaviorism, influenced by Gestalt theory, seems the most cognitively flavored of the entire group (Figure 3.3). I also feel that the general stimulus-interaction of intervening variables or processes-response pattern exhibited by all the diagrams reflects assumptions about the scientific primacy of the transphenomenal physical world and the necessity of relying upon behavioral data and operational definitions which have characterized the entire period.

The essential continuity of modern cognitive behaviorism and earlier neobehavioristic theories indicates that the fundamental

reifications of behavior, stimuli, responses, learning, etc. which have been described by Holzkamp (1964), Ingleby (1968), and Jacoby (1975) as characteristic of the behavioristic approach to psychology, have not been abandoned. I further believe that the greatly increased attention which neobehaviorists have directed at cognition has produced widespread additional reification within modern cognitive psychology. The sheer bulk of experimental and theoretical research in cognitive psychology would require that a reasonably comprehensive documentation of this latter charge covering all the various subfields of cognitive psychology would make up a very large work. I have chosen to concentrate the present critique of reification in the area of thinking and problem solving. Quite similar criticisms are applicable to information processing approaches to the study of perception, which Neisser (1976a) considers "the basic cognitive activity" (p. 9), or to the study of memory which Estes (1975) calls "the heart of cognitive psychology" (p. 9). However, the theories of information processing which have been most influential within the whole of modern cognitive psychology have been advanced primarily within the context of thinking and problem solving (Erickson and Jones, 1978), and it is to these which we now turn.

4. A "THINK" IS NOT A THING: THE INFORMATION PROCESSING APPROACH TO THINKING AND PROBLEM SOLVING

"The theory proclaims man to be an information processing system." (Newell and Simon, 1972, p. 9)

4.1 Introduction to this approach: Newell and Simon

The information processing approach is the most important addition to neobehavioristic operational methodology since World War II. This approach has been developed primarily by A. Newell and H. A. Simon and their co-workers, and it has come to occupy a central position within modern cognitive psychology. In the chapter on "Thinking" in Marx's text on psychological theory, Taylor (1963) observed that the approach "that regards the thinker as an information processing system and employs simulation is the one which appears to offer the most promise" (p. 490). The past fifteen years have certainly confirmed the prediction that this approach would increase in influence; the most recent review of the literature on thinking notes that "the information processing language (metaphor?) is almost universal" (Erickson and Jones, 1978, p. 61). Because this approach has become so important, the present chapter will discuss reifying in the work of the most prominent theorists of the information processing (S-P-R) model of human thinking.

Erickson and Jones (1978) have summarized the "paradigm" of

information processing theory:

> While . . . contemporary research continues to reflect the classic Gestalt view that problem solving, by virtue of its emphasis on response discovery, is something apart from learning. . . . the jargon has changed; instead of insight and restructuring, information processing language stresses search, discovery, and strategy shift. Within the latter paradigm the most influential exposition is that of Newell and Simon (1972; Simon and Newell, 1974). . . .
>
> The information processing approach offers a paradigm wherein a solver . . . is an information processing system (IPS). Knowledge, represented in long term store, consists of networks of ordered list structures which are serially searched and copied as chunks into a limited capacity short-term or working memory at appropriate times. The IPS paradigm offers a loosely structured metatheory; its specific assumptions often occur in computer programs that produce detailed performance output for a defined problem environment. This reflects the explicit stance of the IPS theorists (e.g., Newell and Simon, 1972; Simon, 1972) that only a few gross characteristics of the human IPS are invariant over tasks and problem solvers. . . . An important concept is the problem space (Newell and Simon, 1972), an internal problem representation that contains symbol structures representing possible knowledge states along with a set of operators for transforming knowledge states. . . . The activity of problem solving is a goal-directed search within the problem space and it is captured theoretically, in a problem behavior graph. . . . At the level of observable strategies, each knowledge state corresponds to a class of stimulus conditions which promote an action sequence, or a production system, which has the flavor of stimulus-response automaticity. (p. 62)

Newell and Simon and their co-workers (Newell, Shaw and Simon, 1958; Newell and Simon, 1972, Simon and Newell, 1974; etc.) consider the computer simulation of human thinking to represent a comprehensive theory of human cognitive processes including problem solving, perception, memory, concept formation, learning, etc. Newell and Simon specifically deny that their approach treats the information processing language of computer programs as a metaphor

for human thought; rather, they consider computer programs to be an actual description of the underlying hidden cause of human cognitive behavior.

> The present theory views a human as a processor of information. . . . the label could be thought vacuous unless the phrase information processing took on additional technical meaning.
>
> One may try to provide this meaning by saying that a computer is an instance of an information processor. This would seem to suggest that the phrase is a metaphor. . . . Something ceases to be a metaphor when detailed calculations can be made from it. . . .
>
> But an alternative to metaphor is at hand. An abstract concept of information processing has emerged. . . . to describe how man processes . . . symbolic information. (Newell and Simon, 1972, p. 5, my emphasis)

> A program is a very concrete specification of the processes, and permits us to see whether the processes . . . are sufficient to produce the phenomena. The vaguenesses that have plagued the theory of higher ment.. processes and other parts of pyschology disappear when the phenomena are described as programs. (Newell, et al., 1958, p. 355, my emphasis)

Newell and Simon's "description" of the hidden cause of human thinking and problem solving behavior consists of a variety of computer programs. The programs range from extremely specific ones which perform specific tasks such as finding a checkmate in chess by a series of successive checks (Baylor, 1965; Baylor and Simon, 1966), to the "general problem solver" (GPS) program, which performs a variety of logical tasks (Ernst and Newell, 1969).

The computer simulation approach is explicitly considered to be "a thoroughly operational theory" (Newell et al., 1958, p. 166). The approach is operational in the sense of accepting the standard behavioristic operational definitions of human behavior which it

purports to give a causal explanation for that is explicitly "reductionistic" (Newell and Simon, 1972, p. 9). Thus a protocol of a human solving a problem is taken to be "verbal behavior as data" (Newell and Simon, 1972, p. 12) and is usually summarized as a problem behavior graph (PBG). "Thinking aloud is just as truly behavior as is circling the correct answer on a pencil-and-paper test" (Newell, et al., 1958, p. 345), and the problem solving process is inferred from and defined by the verbal behavior in the same operational manner in which cognitive capacities or inclinations are inferred from and defined by paper marking behavior. Just as Pratt (1939) insisted that "intelligence is what the tests test" (p. 79), Newell and Simon operationally define problem solving as what the PBG records. Thinking and problem solving are treated as concrete things in much the same way that intelligence, anxiety, and attitudes are reified by their operational definition and measurement.

The goal of the theory is to predict performance, which is no more to be confused with understanding the process than is verbal behavior to be confused with introspection. Newell et al. (1958) consider information processing theory able to predict, at least under some conditions, the behavior of humans that is covered by such operationally defined terms as a) learning, defined as "any more or less lasting change . . . in response . . . to successive presentations of the same stimulus" (p. 354), b) set, "defined as 'a readiness to make a specified response to a specified stimulus' (Johnson, 1955,

p. 65)" (p. 348), c) insight, meaning "'suddenness' of discovery, and grasp . . . of the 'structure' of the problem, as evidenced by absence of trial and error" (p. 349), and d) a variety of other similarly reified operational concepts.

Newell et al. (1958) point out that information processing theory and associationism "share a generally behavioristic viewpoint and a committment to reducing mental functions to elementary, mechanistic . . . events" (p. 352), and information processing theory is even more successful in demonstrating "that the higher mental processes can be performed by mechanisms" since a computer's relatively "'active' response to stimuli. . . . has provided us with operational and unobjectionable interpretations of terms like 'purpose,' 'set,' and 'insight'" (pp. 352-353). The more recent work by Newell and Simon (1972) has lost none of this enthusiasm for and committment to mechanical explanations of human thinking defined by "operational specifications for the precise behavioral properties of the human" (p. 792).

Neither has this "most promising" of theoretical approaches to human thinking lost any of the familiar rhetoric of operationism. Although on purely rational grounds, it could be hoped that a more cognitively oriented psychology might retreat from the reifications of operationism toward Bridgman's conception of operational analysis rooted in human experience, this has not been the case. The claims that information processing theory will rid psychology of vague and

mentalistic concepts are asserted no less vigorously than Stevens (1936, 1939) and other earlier operational behaviorists made the same promise.

> Mechanisms . . . can in fact solve problems. This aspect of problem solving has been thought to be 'mysterious' and unexplained because it was not understood how sequences of simple processes could account for the successful solution of complex problems. The theory dissolves the mystery. . . . The specification of the program . . . clarifies . . . notions whose meaning are only vague. . . . The vaguenesses that have plagued the theory of higher mental processes and other parts of psychology disappear when the phenomena are described as programs. (Newell et al., 1958, pp. 341, 354-355)

Newell and his co-workers seem to feel that abstractions such as "information," "output," and "processing" are less vague and more concrete than "insight" or "purpose" in much the same way that Skinner (1971) feels that his concept of "contingencies" is more concrete and specific than mentalistic concepts such as "freedom" and "dignity." However, I believe there is a genuine sense in which Newell and Simon are better psychologists than Skinner; they seem interesting and probing when compared to the latter's grandiose mysticism. Because they are better psychologists, the influence and deleterious consequences of reifying tendencies in Newell and Simon's work is apt to be more profound; it is only superficially paradoxical that reification is a greater danger in better theory.

4.2 The value of information processing theory

In the historical addendum to the systematic statement of their theory of human problem solving, Newell and Simon (1972, pp. 873-889) trace the genesis of their theory to the application of

the "new operationality" (p. 877) of symbolic logic, as utilized and accelerated by cybernetics, linguistics, statistical decision theory, and human factors research, to the problems and shortcomings of traditional S-O-R psychology in dealing with cognition. "The real forerunners of the work, so far as American psychology is concerned" (p. 874) were Tolman (1932), Brunswick (Tolman and Brunswick, 1935), and, to a lesser extent, Boring (1933). Although the influence of European psychology, and the names of Bartlett, Selz, Duncker, Wertheimer, Piaget and de Groot, are mentioned briefly, both Newell and Simon's framing of the problem and their solution seem typically American. The problem, as they see it, stems from the inability of physiological psychology to sufficiently fill the gap in stimulus-response behaviorism, and Newell and Simon cite a formulation of this problem which is a classic statement of the inadequacies of two extreme operationistic psychologies feuding over whether consciousness is an epiphenomenon of environmental or physiological "reality."

> A fact is a relation, and the simple basic fact in psychology is a correlation of a dependent variable upon an independent one. Ernst Mach made this point and B. F. Skinner took it up about the same time this book was being written. He created the concept of "empty organism" (my phrase, not his), a system of correlation between stimulus and response with nothing (no C.N.S., the "Conceptual Nervous System"--his phrase, not mine) in between. This book. . . . undertook to assess the amount of neurological filling available in 1932--how much fact there was ready to relieve the psychophysiological vacuum--and it sought to formulate the requirements that more filling would have to meet. (Boring, 1963, pp. vi-vii)

Notice that Skinner correctly accuses Boring of reifying the

"nervous system" and Boring correctly accuses Skinner of reifying an "empty organism," while both agree about the reification of facts in psychology. Both these approaches, and that of Tolman which Newell and Simon support against Boring and Skinner, are forms of extreme operationism, before its liberalization. Almost unbelievably, the solution advanced by Newell et al. was more operationism and more reification, and even more unbelievably, this was a genuinely worthwhile addition. Newell and Simon are quite clear that their own appraisal of their contribution was "our operationalization of their vague (in our eyes) concepts. . . . The greatest weakness of the antecedents of information processing psychology was the lack of a language to make its concepts clear and operational" (p. 876).

The reason that I believe this step to have been a genuine contribution is that the information processing theorists added an operational and reified conception of thought to psychological models which had no concept of thought, and however we may appraise the overall result, this is an important improvement. A stimulus-response model describes only empty environment; a stimulus-organism-response model could describe some form of creature, and a stimulus-cognition-response model could describe a creature that cognizes, and the latter is a much better (thinglike) model of human being than either of the former.

Newell and Simon are among the best of the information processing behaviorists, and their model is comparatively intelligent.

For example, they observe that, "We, as experimenters have no privileged access to the real world that constitutes the external environment or another human" (p. 85). Behaviorists rarely recognize this; instead, they often assume that they can manipulate the external environment of their subjects.

> Even when we create a laboratory situation, we must still ascribe the aspects of that environment that are relevant for our subjects behavior. We saw, in effect, that our choice of representations amounts to a set of hypotheses about what encoding will be provided by the subject IPS. (p. 85)

Despite their good sense in recognizing the existence of subjectivity, I submit, in effect, that their choice of representations amounts to a set of reifications of whatever a human being might be doing while in their laboratory.

Another positive aspect of Newell and Simon's research is that they have, in a few cases, been able to include some of the thought processes of the experimenter within their computer simulation, along with the simulation of the subjects thinking (Simon and Newell, 1974, p. 108ff.). Although their efforts in this direction are quite modest, they represent a pleasant contrast to the practice of most other behaviorists, who often presume a sharp alienation between the thinking of the experimenters and that of the experimental subjects (Brandt and Brandt, 1974). In this context it should be pointed out that the claim of some reviewers of Newell and Simon's (1972) book to the effect that "data for select subjects are presented and analyzed . . . with something like Freudian thoroughness" (Neimark

and Santa, 1975, p. 177; see also Johnson, 1972) is greatly exaggerated. While Newell and Simon study some protocols at some length, they recognize their treatment of these protocols as sufficiently shallow and mechanical that at least part of the analysis of the protocols can itself be computerized, providing a further instance of the simulation of the thinking of the experimenters (Simon and Newell, 1974, pp. 118-119).

It is further to their credit that Newell and Simon insist that their model is to be judged as an abstract model; they have the sophistication to realize that the fact that their model can be run on a computer and is thus objective, is, for the purpose of evaluating their theory per se, simply an artifact, and not essential to the theory as theory. Naturally, it is extremely important for the practice of evaluating the theory that it does run on a computer so that this output can be compared to the verbal behavior of a human. They explain that it is a

> peculiarity that the theory performs the tasks it explains. That is, a good information processing theory of a good human chess player can play good chess; a good theory of how humans create novels will create novels; a good theory of how children read will likewise read and understand. (Newell and Simon, 1972, p. 10-11)

Thus in a unique sense this model is "operational;" it does literally produce phenomena which are the meaning of the model, which are then compared to human behavior. Newell and Simon correctly point out that this peculiarity does nothing to validate the model; the abstract model must be judged by the same criteria as any other model which is merely

produced by predictions in the mind of a theorist, which that theorist must write down rather than reading them from a computer printout. Information processing theory is a mathematical abstraction, and Newell and Simon do not wish to reify it as computer hardware. To do otherwise would be to indulge in unproductive comparisons of organisms and machines, and to once again completely neglect any conception of cognition.

Beyond mentioning that I personally enjoy some of Newell and Simon's extensive consideration of chess (although the psychology is de Groot's, 1965, and only the less interesting computer simulations are Newell and Simon's), I will cease praising information processing theory and attempt to bury it. A thinking person in the real world is more accurately and completely modeled by the information processing version of what I call the S-C-R model in psychological theory than by previous behavioristic models. However, in the manner of operational behaviorism, the consequence is that the thinking person and his/her ideas are also more thoroughly and completely reified than by earlier behavioristic theories. Human mental activity is now explicitly modeled as a thing, in addition to human behavior.

4.3 Reification in information processing theory

Although it is difficult to minimize the value of adding some conception of thought processes to a model of psychological functioning, Newell and Simon and their co-workers themselves minimize their contribution by insisting upon the extension of reifying operational

methodology into the realm of cognitive processes, resulting in a thorough hypostatization of the IPS, and reification of persons and their thinking, remembering, perceiving, etc. This theory completely accepts the usual behavioristic reification of human activity as behavioristic process (Ingleby, 1968), and further hypostatizes a "hidden reality" of an information processing program which lies "behind" and "explains" human behavior.

Although Newell and Simon sometimes characterize their approach as a conjecture or hypothesis, they believe that they have provided sufficient empirical verification to conclude explicitly that the abstract IPS is definitely real, and that "the individual human being . . . is a system consisting of parts: sensory subsystems, memory, effectors, arousal subsystems, and so on" (Newell and Simon, 1972, p. 3). The phrase "the human IPS" which permeates their work, indicates both the explicit positing of the theoretical abstraction as a concrete thing, and the complementary reifying of persons as this concretized abstraction. Parallel to the hypostatizing of the whole abstract system and the reifying of humans as systems, Newell and Simon (1972) hypostatize a set of subsystems and reify the "parts" of the human's cognitive activities: "The theory posits a set of processes or mechanisms that produce . . thinking" (p. 9).

As noted in section 4.1, Newell and Simon do not claim that humans are analogous to computers, but rather that humans <u>are</u> information processing systems. "Programmed computer and human problem solver are both species belonging to the genus IPS" (Newell and Simon,

1972, p. 870). This theory reifies the abstract IPS as a "hidden reality" which is the cause of human cognition and behavior. It is further asserted that humans are this reified abstraction; human beings are reified as thinglike IPS's. The latter assertation (that humans are IPS's) supports the former (that IPS's are real), by pointing out concrete instances of the abstraction, namely persons and their thinking and acting. This position can appear very reasonable, especially if the initial definition of IPS in abstract terms is forgotten.

By ignoring the formal, technical definition of an information processing system in mathematical or programming language, the theory can be made to appear extremely reasonable. Both personal experience and everyday language support the assertion that humans do process information, so to say that humans are information processing systems sounds like a truism rather than an erroneous reification. However, this apparent truth rests upon the same type of failure to distinguish between different definitions of the same term which Brandt (1970) has described as the behaviorist's leap, and which we have seen is typical of operationism.

Both Random House (1970) and English and English (1958) give the first, most common, definition of information as knowledge about facts, which is essentially a private event. The less common technical definition of information as "data that can be coded for processing by a computer" (Random House, 1970, p. 730), is an event defined by public operations. English and English note that the technical definition of

the term within information theory "is unhappily chosen for . . . the reader must remind himself that the author (probably?) did not mean by 'information' what the reader, in talking with himself, means by that term" (p. 261). By failing to clearly distinguish between knowledge about facts, and the codification of knowledge about facts in terms of binary numbers, Newell and Simon give a false sense of reasonableness to their theory that a human is an IPS.

Moreover, when we do not forget or ignore the technical definition of an IPS, it becomes far from intuitively reasonable to equate this abstraction with a thinking human.

> An information processing system (IPS) consists of a memory containing symbol structures, a processor, effectors, and receptors. Hence it incorporates (a) a set of elements, called symbols; (b) symbol structures, consisting of instances of symbols connected by relations; (c) memories capable of storing and retaining symbol structures; (d) information processes that take symbol structures as inputs and outputs; (e) a processor that includes a set of elementary information processes (eips), a short term memory that holds the inputs of the eips, and an interpreter that determines the order in which the eips will be executed; and (f) input and output channels. (Simon and Newell, 1974, p. 102)

It seems to me that such a system must be formally identical to the sum of its parts, and that the whole IPS, the various intermediate subsystems, and the smallest discrete units, eips, out of which "the entire behavior of the IPS is compounded" (Newell and Simon, 1972, p. 29), all must have the same ontological status. All these entities may be a physical sequence of positions of flip/flop switches or other computer hardware. We have already seen that Newell and Simon regard the objectivity of computer hardware as unimportant to their theory.

Simon and Newell (1974) prefer to discuss the system as a mathematical formalism; they point out that any computer program is formally identical (isomorphic) to a set of difference equations. Such a mathematical formalism, an ordered set of binary numbers as a function of discrete time intervals, is not a physical reality, but is an actual idea. It is quite clear that neither concrete computer operations nor ideas about a set of equations could reasonably be considered as the actual cause of the variety of "cognitive behavior" which Newell and Simon claim to explain. It is only by reifying the bits, eips, subsystems and total IPS as concrete entities existing in some unexplained hidden realm, that Newell and Simon (1972) could put forward the assertation that such processes "produce the behavior of the thinking human" (p. 9), as a serious scientific hypothesis.

I suspect that Newell and Simon actually believe the relatively small elements of the system, the eips, exist as the real cause of some small portion of human thinking. Once the eips are given the status of real causal agent for some portion of actual cognition, i.e., once the eips are reified, the nature of the IPS as a mechanical sequential system requires that the IPS, the bits of information, the memory subsystems, etc., all be reified as a mechanical sequence of the "real" eips. Of course Newell and Simon did not proceed to first reify the eips, and then notice that this required the reification of the other parts as well as the whole system; logically the system and all its parts are either real or not. However, it is possible that psychologically, the eips, the primitives of their system, are more

strongly believed to be real by Newell and Simon.

> The . . . contribution of formal logic to information processing psychology. . . . was to demonstrate by example that the manipulation of symbols (at least some manipulation of some symbols) could be described in terms of specific, concrete processes quite as readily as could the manipulation of pine boards in a carpenter shop. The formalization of logic showed that symbols can be copied, compared, rearranged, and concatenated with just as much definiteness of process as boards can be sawed, planed, measured, and glued. (Newell and Simon, 1972, p. 877)

Whether my attempt to read between the lines of passages such as this indicates that Newell and Simon somehow perceive eips as showing a greater sense of realness than other parts of the system, is obviously debatable. But, if the psychological process of reifying the eips has some sort of priority, it is certain that the dramatic and deleterious assertion that humans are thing-like systems would have to follow from the belief that elementary computer processes are the real cause of human cognition.

The above passage could be taken as indicating that Newell and Simon erroneously believe that specificity implies concreteness. This is disproven by much of modern mathematics, where, for example, a real number may be a well specified element in the well specified real number system, without any claim being made for the concreteness of either (Cohen and Ehrlich, 1963). It is not clear that Newell and Simon make such an extreme logical error, which would ignore the distinction between an analytic judgement of specificity and a synthetic judgement of concreteness. Such an extreme error would, however, account for their reifying the well specified eips as concrete entities. That Newell and Simon's logical error is extreme can be

seen by comparing their theory with English and English's (1958) attempt to define reification by offering an extreme, even caricatural example: the attribution of the production and causation of human thinking to entities called bits, eips, and IPS's, while more sophisticated, seems just as illogical and extreme as attributing the cause of human "thickheadedness" to an entity called "thickness" (English and English, 1958, p. 452).

Newell and Simon are relatively unconcerned with the logical foundation of their theory, even claiming that "the conjecture that a thinking human is . . . an IPS. . . ., of course, is an empirical hypothesis" (Simon and Newell, 1974, p. 102). I will examine some of their empirical evidence in the next chapter, but in other passages Newell and Simon seem clear that they regard this conjecture as already established empirically and believe that further research is indicated only in order to 1) refine computer programs so that their output more precisely resembles the verbal behavior of humans, and 2) compare the output of computers with that of humans under a wider variety of conditions. Further research of this kind would not address Newell and Simon's error in reifying their mathematical formalism, and would not really address the question of the validity of their conjecture.

The theoretical limit of Newell and Simon's research would be the exact reproduction of human protocols by computers operating according to their theory. While this would represent an impressive advance over present results (Newell and Simon admit that, at present, certain types of expressions are eliminated in making PBG summaries

of verbal behavior because current programs have no hope of simulating them), this would neither demonstrate that a human is an IPS nor eliminate the theory's reification of the mathematical formalism. The widening of computer simulation methods to more varied types of situations would be more impressive evidence than exactitude over the present range of experimental settings. The ultimate empirical evidence would be the construction of an automoton indistinguishable from a human, and while this is probably impossible in principle (Dreyfus, 1972,; Neisser, 1963, 1976a, 1976b; Newell, 1973), even such perfect simulation would not establish Newell and Simon's claims. Since "equivalent results do not guarantee equivalent procedures" (Cohen, 1977, p. 179), simulation cannot, in principle, prove the theory correct. Computer programs for playing chess can produce the same move using radically different methods, and humans can say the same things while thinking very differently.

Experimental data cannot correct logical errors in theorizing. I believe that Newell and Simon are in error in claiming 1) that there exist information processing mechanisms which cause human cognition, 2) that a human is an IPS, and 3) that these assertions are empirical questions. Information processing theory is systematically flawed by reification, and no experimental data can correct this error. I do not believe that this error renders the theory worthless, nonsensical or scientifically uninteresting.

I contend that the reification of human beings and human cognition has morally and ideologically objectionable consequences.

However, one need not agree with my speculation on the ideological and ethical results of reification, to agree that it is an error which scientists should strive to eliminate. Anyone sincerely interested in seeking knowledge which does not consist of "networks of ordered list structures" (Erickson and Jones, 1978, p. 62), can recognize that Newell and Simon have reified knowledge. Anyone who recognizes this should be critical of Simon and Newell's claim that "the information processing formalisms. . . . have provided us with the technical means to create a 'mental chemistry' of cognitive processes" (p. 142). Newell and Simon and their co-workers have indeed created such a chemistry, but both the "chemistry" and its "elements" are the creations of their imagination. Only by reifying both elements and chemistry, can they assert that this imaginary chemistry is the real cause of all actual human thinking, while it remains simply the actual result of their own imaginative thought.

4.4 Radical and cognitive behaviorism on thinking

Skinner (1953, 1966, 1974, 1977) has for many years objected to "mentalistic" conceptions of human behavior (including thinking) in both everyday language and in psychological theory. Skinner (1974, pp. 8-18) points out that no contemporary writer can claim to speak as the behaviorist (as Watson had done in 1913), and Skinner has extended his criticism of mentalism to other behaviorists, including information processing approaches to human behavior. While Skinner would not deny that many information processing theories fall within the scope of

methodological behaviorism, he has specifically declined joining the cognitive bandwagon (Skinner, 1977), and maintains that operant conditioning analyses can account for the phenomena of human thinking without the concessions to mentalism that he believes are made by information processing theorists. As with Boring's (1963; see above, p. 88) argument with Skinner over physiology, the argument between radical behaviorists and information processing theorists reveals reification on both sides.

Skinner's well known objections to mentalism include the charge that mentalistic theorists hypostatize a homunculus or some other unreal inner agency, and neglect the determination of behavior by the environment. His objections to information processing theories are similar:

> Information theory has . . . input and output, and the problem is to relate one to the other. Knowledge, thought, and cognition are therefore invoked. Someone or something (somewhere) processes the information received, stores it, and retrieves it on appropriate occasions. This is done out of sight, if not out of mind. An operant analysis . . . goes beyond input and output to the much more subtle and complex environmental arrangements. . . . As the analysis grows more powerful, and hence more successful, there is less and less need to assume such inner activities as the processing, storing and retrieving of information. Characteristics of behavior once traced to thought and to a memory store of knowledge, processed or not, are traced instead to environmental contingencies. (Skinner, 1966, pp. 256-257)

Thinking and problem solving need only be described with three terms and the relations among them: "a stimulus, a response, and a reinforcing consequence" (1966, p. 226). Human thought should be treated simply as behavior without invoking any consideration of an inner program of information processing (1974, pp. 102-118). Skinner seems to be unaware of the extent to which information processing

theorists are comparing humans (or human minds) with computer programs rather than with machines, and his attempt to extend his criticism of information processing theory to the problem of the extent to which people can be characterized as machines is quite inconsistent.

> The metaphor of storage in memory, which has seemed to be so dramatically confirmed by the computer, has caused a great deal of trouble. The computer is a bad model--as bad as the clay tablets on which the metaphor was probably first based. We do make external records for future use . . . but the assumption of a parallel inner record-keeping adds nothing to our understanding of this kind of thinking. (It is not the behaviorist, incidentally, but the cognitive psychologist, with his computer-model of the mind, who represents man as a machine.) (Skinner, 1974, p. 110)

But, in another discussion of men and computers, he asserts that

> there is no reason why a machine cannot be constructed so that it is altered by the consequences of earlier actions. Some have already been so constructed--we call them men. (Skinner, 1966, p. 246)

Skinner's contradictory remarks on the human-machine dichotomy are basically irrelevant as criticism of Newell and Simon's (1972) consideration of computer program models of human thinking and problem solving. That Skinner's own theory (or lack thereof, Skinner, 1950) contains considerable reification has been demonstrated by Ingleby (1968), and the latter's criticism by no means exhausts the reification within Skinner's approach. In addition to Ingleby's discussion of "stimulus," "response," and "reinforcement," it is relatively easy to document Skinner's reification of "science," "behavior," and "contingencies." Despite his supposed break with operationism, (Skinner, 1953, 1974), Skinner's distain for subjective meaning and the mentalism of everyday language, leaves him with no real method of defining his terms other

than the extreme operationism which he championed earlier (Skinner, 1945). Skinner (1974) asserts that "science carries him beyond personal experience" (p. 124), that "human behavior" has been more discussed than "any other thing" (p. 3, my emphasis), and that "the contingencies, not the mind, make discriminations" (p. 105). It is the reification of the omnipotent "contingencies" which gives Skinner's approach its sinister, totalitarian overtones.

As indicated above (section 4.2), Newell and Simon's approach to thinking and problem solving is a considerable improvement over Skinner's. In recent reviews of "thinking" (Erickson and Jones, 1978; Neimark and Santa, 1975) Skinner is not mentioned at all, while Newell and Simon are considered extensively. Since terms such as "knowledge, thought, and cognition . . . have no place in an operant analysis" (Skinner, 1966, p. 256), Skinner puts forward no model of the human mind at all, and a computer program model is certainly an improvement on nothing.

However, one portion of Skinner's criticism of information processing was discussed at a symposium on cognitive psychology (Voss, 1969), and the discussion of the problem of hypostatizing information processes gave some clarity to reification in Newell and Simon's approach. Bourne (1969) offered the strict behavioristic critique of information processing theory, and Newell (1969) replied for the information processing theorists. Bourne (1973) differs from Skinner (1977) in claiming that all behavior is cognitive and that all psychologists are cognitive psychologists, but that they should study

only behavior and not posit underlying processes. If the label "cognitive" is disregarded, Bourne's criticism of information processing approaches is similar to Skinner's (alternatively we might call Bourne's position "radical cognitive behaviorism").

Although Bourne (1969) does not use the term reification, this issue is clearly at the heart of his critique. He contends that conceptions of thinking processes and of information processes which underly cognitive behavior, both refer to entities whose reality is at best doubtful. Bourne is critical of all theorists who engage in "talking about thinking, or . . . positing 'thought processes' as the mechanisms which permit or enable organisms to do the complex things they do" (1969, p. 168). He prefers "S-R associational theories" which

> make no assumptions about processes within the organism, except for those which concern the establishment of associations and the possibility of persistent memory traces. Behavior is a consequence of external stimulus conditions and no assertions are made about intervening, autonomous thought processes. Concept formation, like other forms of learning, is the more or less mechanical connecting up of stimulus and response. (pp. 168-169)

Bourne thus accepts "behavior," "memory traces," and "S-R bonds" as real entities, while rejecting "information processes" as reification. Bourne contends that "<u>assertations</u> about underlying processes . . . <u>are not harmless</u>, if they are taken seriously and literally" (p. 173), and such assertions are questionable on at least three grounds.

> First, underlying processes are commonly described in behavioral terms; e.g., a process which selects and revises hypotheses or compares items of information. The process often sounds suspiciously like behavior gone underground. . . .

> Second, such a theory seems to impose the necessity to study invisible processes--processes which might not exist and for which there is no extra-behavioral description (certainly not physiological) which permits recognition when and if they did occur. . . . What counts is how well the theory describes the data. But, if that is so, where is the need for process-talk? . . .
>
> Third, the theoretical underlying process often seems to be an invention--an invented answer to the question, What has the subject learned that enabled him to respond as he did? The subject's responses are obviously systematic and organized. Something seems to be needed to do the coordinating. A mechanism working on those coordinating principles is invented, and the invention is than said to "explain" the subjects behavior. (pp. 173-174)

Bourne's proposed solution is the formulation of the rules which describe the process of behaving itself, and the insistence that "the rules are principles of behavior, not principles of a mechanism which in turn produces behavior" (p. 175; see also Skinner, 1974, for a similar account of rule following behavior). Both Skinner and Bourne claim that strictly behavioral descriptions can adequately deal with thinking, in the sense that "thought . . . is a change in the individual's possibilities (or potential) for behaving" (Bourne, 1969, p. 185).

> People do have thoughts. Moreover, people do engage in implicit activities, like subvocal speech. . . . What has been questioned is the notion that implicit action regulates, or that thinking 'explains' behavior. Both implicit and explicit behavior (and thoughts, too, for that matter) are conceived to be part of human skills and abilities, to be described and understood as performances. The assumption that one, as a hidden computing process, regulates and results in the other is doubtful. (p. 185)

Bourne objects to the explanation of behavioral processes in terms of physiological, psychological, or informational processes, and he asserts that information processing approaches add nothing to the ability to predict and control behavior. He especially objects to information

processing theorists who make the "assertion that the process, as an entity, separate and apart from the rules it assumably obeys, really exists" (p. 190).

Although I am personally more sympathetic to Newell and Simon's approach than to Bourne's or Skinner's criticisms, I think that Bourne's critique, and Newell's (1969) reply provide some additional evidence that Newell and Simon 1) remain within the paradigm of operational neobehaviorism, and 2) do indeed hypostatize information processes.

Newell (1969) replied that, in his opinion, psychological scientists should "push on to discover the molecular mechanism behind" (p. 199) regularities in behavior, and that failure to do so would be "a failure of nerve" (p. 208). Moreover, "the reasons one searches for the mechanisms behind a regularity is that they are usually there, waiting to be found" (p. 199).

> One can find a mechanism for producing the light that shines from stars. One can find a mechanism for photosynthesis. One can find a mechanism for sickle-cell anemia. One can find a mechanism for inflation.' And one can expect to find a mechanism that enables an organism to follow learned complex rules. (p. 200)

Newell argues that information processing mechanisms should exist because 1) "physiological mechanisms are there" (p. 200), and, 2) even if there are no known physiological mechanisms which can account for complex behavior, "some process must exist" (p. 202).

> For me, the attribution of a set of enabling psychological processes to the subject happens in terms of the theory that I build. . . . A process theory (and for the psychology at hand I want to talk about an information-process theory) has in it terms that refer to memories and their capacities, and to representations of information in these memories. It has terms

> that refer to processes for encoding and decoding, for storing and retrieving, for communication, and for interpreting structures representing conditional sequential actions. And so on. A theory that has such terms is a processing theory and attributes psychological processes to the subject. It does not ask for measurements of anything except behavior. . . .
>
> Furthermore, one cannot play the mathematicians' "symbol game" here, and say that this theory is simply a set of equations (or other symbolic expressions) combined according to some formal rules and applied to the empirical data by a set of formal coordinating definitions, without any imputation to the underlying structure of the subject being described. (p. 206)

Newell holds firmly to the position that the information processes are not metaphors and are not mathematical models, but are real concrete processes. In rebutting the claim that the assumption of underlying processes is excess theoretical baggage which is not warranted by the facts of behavior, Newell claims that

> the nonprocessing assumption is really excess baggage with no remaining operational content. . . .
>
> Bourne is logically justified; there is no necessity. Yet it is a failure to grasp an opportunity, for the processes are there. (p. 207, my emphasis)

Newell has a logical opportunity to distinguish between psychological processes that are experienced, and informational processes that are inferred, and he eschews the distinction, preferring to argue that both types of processes are real because they can be inferred from behavior and/or physiology. Both Bourne and Newell are so firm in their committment to methodological behaviorism, that the experience of a process must not be taken as even tentative evidence for its existence. In the next chapter I shall discuss some alternatives where this is not the case.

5. ALTERNATIVES TO REIFICATION

"I am not sure whether there can be a way of really understanding the miracle of thinking. Certainly you are right in trying." (Einstein to Wertheimer, quoted in Wertheimer, 1959, p. 227n.)

5.1 Summary and limitations of the critique

Behaviorism began with Watson's (1913b) call to "dispense with consciousness in a psychological sense" and to "return to a non-reflective and naive use of consciousness" (p. 176). Shortly thereafter, Watson (1913a) put forward the first specifically behavioristic reification of thinking, by proposing that the psychological process could be reduced to physical laryngeal movement, or subvocal speech. It is my contention in this thesis that behaviorists since Watson have frequently (although certainly not always) reified thinking, and thinkers, in their efforts to formulate a behavioristic psychology of thinking and problem solving. The leading cognitive neobehaviorist, Tolman, hypostatized cognition as an operationally defined "objective" intervening variable. Newell and Simon's information processing approach, continues to rely upon operational definitions, and furthermore, includes the allegation that computer program-like operations form a reality which underlies and causes human problem solving behavior. Nor do I believe that radical behaviorists (or "radical cognitive behaviorists") avoid reification by contending that environmental contingencies are the cause of human discrimination and thinking.

Lukacs (1971) regarded the lack of relevance to everyday life in much academic and scientific work as a sort of corollary of reification. Psychologists interested in thinking and problem solving could put more effort into research in which they ask people (including themselves) questions like: "What are the real problems in your life? What are you doing to try to solve them?"--rather than presenting puzzles to subjects for solution under experimental laboratory conditions. Such a step might go a long way toward ending the extreme lack of ecological validity and relevance to everyday life in the field of thinking and problem solving, which Neisser (1976a, 1976b) has pointed out prevails throughout contemporary cognitive psychology.

> The study of information processing has. . . . no account of how people act in or interact with the ordinary world. . . .
> In a recent article Allan Newell (1973) tabulates no fewer than 59 experimental paradigms of current interest. . . . Fifty-seven of the paradigms on Newell's list are based on artificial laboratory situations; the only ones with a shred of ecological validity are playing chess and looking at the moon.
> This trend can only be reversed, I think, if the study of cognition takes a more "realistic" turn, in several senses of that word. First, cognitive psychologists must make a greater effort to understand cognition as it occurs in the ordinary environment and in the context of natural purposive activity. . . . Second, it will be necessary to pay more attention to the details of the real world in which perceivers and thinkers live. . . . We may have been lavishing too much effort on hypothetical models of the mind and not enough on analyzing the environment that the mind has been shaped to meet. . . . A satisfactory theory of human cognition can hardly be established by experiments that provide inexperienced subjects with brief opportunities to perform novel and meaningless tasks. Finally, cognitive psychologists must examine their work for more fundamental questions: human nature is too important to be left to the behaviorists. (Neisser, 1976a, pp. 7-8)

The kinds of problems which Newell and Simon (1972) have studied the solution of, should according to Neisser (1976b) be called "'puzzles,'

because they are so different from the problems of ordinary human life" (p. 137). The puzzle solving studied by Newell and Simon involves tasks which

> are all set by other people, and accepted by subjects in an unemotional and conflict-free way. Most of them are puzzles, in which all the information needed for the solution is present from the first. "Intelligent performance in natural situations," in contrast, might be defined as "responding appropriately in terms of one's long-range and short-range goals, given the actual facts of the situation as one discovers them." (Neisser, 1976b, p. 137)

Neisser (1976b) goes on to compare puzzle solving as studied by Newell and Simon with intelligence testing which consists "largely of arbitrary problems of little intrinsic interest" which are "deliberately disembedded from the . . . testee's ordinary experience" (p. 137). Thus, if human problem solving is defined by the performance of humans on the type of puzzles utilized in the experimental work of Newell and Simon, human problem solving is reified in a way which is quite similar to defining intelligence as that which intelligence tests test.

Neisser (1963) had pointed out that "cognitive activities of machines and men are . . . substantially different" in that

> three fundamental and interrelated characteristics of human thought . . . are conspicuously absent from existing or contemplated computer programs. 1) Human thinking always takes place in, and contributes to, a cumulative process of growth and development. 2) Human thinking begins in an intimate association with emotions and feelings which is never entirely lost. 3) Almost all human activity, including thinking, serves not one but a multiplicity of motives at the same time. (p. 195)

In 1976 Neisser maintains that computer simulations still do not have

these three qualities.

> A computer program by contrast does not grow, has no emotional basis, and is monomaniacally single-minded. I see now that all three of these qualities arise because people develop and use their cognitive skills in a real environment; they are always in concrete situations with multiple opportunities. (Neisser, 1976b, p. 140)

These same three qualities are lacking, Neisser asserts, in intelligence testing and in laboratory studies of human cognition.

> The behavior of subjects in such studies often can be (and sometimes has been) successfully simulated. Behavior on some intelligence tests has also been simulated, as have solution patterns on a wide variety of puzzles. . . . As a duffer at chess and as one profoundly out of tune with the Towers of Hanoi, I am enormously impressed with what . . . Simon's programs can do. Nevertheless . . . development; emotionality, multiple motivation--are still missing. . . .
>
> There is a reason for this. The development of human intelligence occurs in a real environment with coherent properties of its own. . . . programs. . . . can only treat . . life as a series of puzzles, not as a sustained encounter with reality. By this measure, they must fall short of their goals. (Neisser, 1976b, pp. 140, 144)

Neisser is not alone in such criticism; Newell (1973) himself has voiced some similar concerns, and Allport (1975) has put forward a scathing critique of the empirical results obtained by information processing behaviorists. He concludes that they offer a "charade" and a "sort of make-believe science" which "will never cumulate" (p. 143) in knowledge relevant to human life.

By treating human problem solving behavior as thing-like, cognitive behaviorists tacitly assume that this "thing" can be studied by cutting it up into pieces--laboratory studies of puzzle solving behavior--and then examined as a whole by putting such pieces of behavior back together "with just as much definiteness . . . as

boards can be sawed . . . measured, and glued" (Newell and Simon, 1972, p. 877). Human thinking and problem solving is no such thing, and cannot be understood by the study of artificially isolated "pieces" of behavior.

Two questions seem to me to arise logically at this point. First, is it possible for behaviorists to adequately deal with any essentially psychological phenomena without reifying? Second, can non-behaviorists, who do not dispense with conscious reflection, scientifically study thinking and problem solving without reifying, or is reification necessary for the scientific study of this field?

It is beyond the scope of the present work to give a complete answer to the first question. If Waters (1958) and Ingleby (1968) are correct in their contention that the basic concepts employed by behaviorists--behavior, stimulus, response, and reinforcement--are reified in their normal usage within behavioristic theory, then it seems very difficult for any behavioristic theorist to explain adequately any psychological phenomena whatsoever without reification. Moreover, since psychology--the study of psyche--involves phenomena which are the private experiences of individuals, any behaviorist who is willing to accept only public (and often, replicable) phenomena as data for scientific study, must face a serious dilemma. Private events must be either ignored, or dealt with on the basis of inference from public phenomena. Few major behavioristic theorists, from Watson to Skinner, or Newell and Simon, have been content with the former option of ignoring private phenomena. Private events, whether or not they

are called "covert behavior," are generally considered to be aspects of the field of psychology. Ignoring private phenomena leads to an artificially superficial behavioristic psychology, and dealing with them exclusively on the basis of inferences or analogies from public events is an option with great potential for reification. The latter option too easily leads to the effort to translate "mentalistic things or events. . . . into behavior," and to the conclusion that "what is felt or introspectively observed is not . . . nonphysical" (Skinner, 1974, p. 17). I personally believe that the behaviorist's dilemma leads either to unacceptable superficiality or to erroneous reification. If it is accepted that all phenomena which are legitimately classified as psychological have a private, or subjective, aspect, then it follows that behaviorism necessarily leads to a reifying psychology.

The above thesis is stated in a speculative manner because I do not claim the more radical conclusion that behaviorism is a necessarily reifying psychology has been established by the present work. My present critique merely establishes that reification is common within the works of some leading behavioristic theorists concerned with thinking and problem solving.

In the next two sections, I shall attempt to answer the second question by demonstrating, briefly, that significant contributions to the scientific study of thinking and problem solving can be, and have been, made without reifying.

5.2 Alternatives: Preliminary considerations

It is fairly obvious that behaviorists have no monopoly on reification (Jacoby, 1975); it is clearly possible to misapprehend concepts such as "schemata," "transcendental ego," "the unconscious," or "Gestalten" as referring to substantive entities or processes. Thus, although the simple abandonment of behaviorism per se would provide no guarantee against reification, the abandonment of Watson's proscription on using consciousness reflectively might constitute a constructive first step. Husserl seems to have understood psychology in this way: "The psychologist . . . must begin with . . . himself" (Husserl, 1970, p. 253, see also pp. 397-400). By attempting to include private events, or phenomena from the individual life space of each scientist, phenomenological psychologists can seek to explore scientifically the totality of the experienced world, including both public and private phenomena, and need not come up against the behaviorist's dilemma, with the resulting potential for reification.

> What imposes itself here and must be considered before everything else is the correct comprehension of the essence of the life-world and the method of a "scientific" treatment appropriate to it, from which "objective" scientific treatment, however, is excluded. (Husserl, 1970, p. 123)

In this context Husserl (1970) considers "the objective sciences as subjective constructs" (p. 129), and calls it a

> thoughtless naivete. . . . that . . . merely subjective relativity is supposedly overcome by objective-logical theory, yet the latter belongs, as the theoretical praxis of human beings, to the merely subjective and relative and at the same time must have its premises, its sources of self-evidence, in the subjective and relative. (pp. 132-133)

By means of "the epoche of objective science" which involves "a withholding of natural, naive validities and in general of validities already in effect" (p. 135), the phenomenological psychologist assumes an attitude which at least allows the theoretical possibility of reflecting upon previously taken-for-granted reification, and understanding it as error. Whether such methods can in fact overcome reification is a complex question which I will address briefly in the concluding section.

Phenomenological psychologists are, of course, not the only psychologists who begin with, or at least utilize reflection upon their own consciousness. Such psychologies have been put forward in a number of different ways--as the introspection of psychical elements and compounds (e.g., Wundt, 1902), as phenomenological psychology (Giorgi, 1970; Gurwitsch, 1964; Husserl, 1970), as Gestalt theory (e.g., Ellis, 1938), as thought-psychology (e.g., de Groot, 1965), and in emerging conceptions of psychology as a hermeneutic (Gauld and Shotter, 1977; Radnitzky, 1973), or dialectical science (Riegel, 1975, 1978; Rychlak, 1968, 1976). There are, of course, serious and fundamental differences in the way in which human thinking would be understood by different interpreters of these various approaches, and I shall not attempt to offer any one proper approach to the scientific study of thinking, in the sense of a methodological dogma. However, these differing perspectives share the common position of either starting with, or at least not prohibiting, the psychologist's self-reflection upon her or his own consciousness. Such psychologists can thus accept

both public and private events as data, and do not face the same dilemma which behaviorists face concerning efforts to understand (or predict and control) those aspects of psychology which involve private phenomena. While this does not guarantee the absence of reifying in their psychological studies, it does provide an advantage (though not a "substantial" one). The use of reflection upon one's own consciousness within psychological methodology does not, of course, prohibit the attempt to infer aspects of thinking of which an individual may not be directly aware: psychologists remain able to attempt such inferences with respect to their own and others' thinking processes. However, by reflecting upon their own thinking, psychologists who devise explanatory constructs may remember these constructs as their ideas, and not regard them as an independent reality of things-in-themselves.

With reflection upon their own thinking and problem solving as part of their methodology, psychologists may, of course, ask others to do the same. The proper manner of interpreting what others say about their private experience is one of the most complex problems of psychological methodology which will not be resolved in the present work (see Brandt's 1970, comments on "judgements;" de Groot's, 1965, discussion of "thinking aloud" and "introspection;" Gauld and Schotter's, 1977, discussion of the "hermeneutic circle"). Minimally it seems clear that it is incorrect to regard what others say as "verbal behavior," "responses," or "output" emitted by "organisms," "black boxes," or "IPS's," governed by the "facts" of the external "stimulus conditions," the environmental "contingencies," or the internal "program." My own

reflecting upon the relationship between my private experience and my verbalizing is an essential first step in considering what similar relationship might exist for others. With regard to the problem of reifying, my reflection upon my own private experience and public activity (including verbal activity) does not reveal either to be essentially thing-like. It is not unreasonable to assume that the same is true for "the other one." Furthermore, by studying my own and other's thinking and problem solving in realistic situations, I can avoid the reification involved in cutting up this human activity into artificially isolated pieces

5.3 Alternatives: Some specific comparisons

Bridgman (1936; see above, section 2.1) considered it difficult to use language (at least English) without reifying. Whorf (1967) suggests that the same may be true of what he calls "Standard Average European" languages. In the next chapter, I will briefly discuss this problem, as well as some suggestions that reification is socially necessary and inevitable (Berger and Luckmann, 1966; Berger and Pullberg, 1966). The present section is intended to demonstrate by means of some specific examples that thinking and problem solving can be scientifically studied without reifying (to the extent that this is possible, as discussed below).

Duncker (1945), de Groot (1965), and Wertheimer (1959) made indubitable contributions to the scientific study of thinking and problem solving, without the extensive reifying evident in contemporary

cognitive behaviorism. Some of their examples of thinking and problem solving have been discussed recently by cognitive behaviorists. I shall compare the different approaches to these examples, the respective contributions to the scientific understanding of the examples, and the extent of reifying in the different approaches.

Duncker's (1945) monograph is a classic work in the field of problem solving. One example from this work, Duncker's candle problem, has been flow charted and researched in accordance with contemporary behavioristic methods by Weisberg and Suls (1973).

Duncker's work is basically Gestalt psychology, although he also makes some use of the thought-psychology of Selz and refers to Husserl extensively. Reflection upon his own experience is frequent and seems to be an integral part of Duncker's methodology. Ducker uses reflection upon his own everyday problems, and observations by colleagues, as part of his effort to fill the gap between the "notorious danger" (p. v) of oversimplification involved in laboratory experimentation and the construction of a general theory of problem solving. Duncker carefully describes his major theoretical constructs--resonance, restructuring, functional fixedness, etc.--as processes which seem to be involved in succeeding or failing to solve a problem. These constructs are derived from personal experience, logical requirements of a problem, and protocols of subjects "thinking aloud" while engaged in working on an experimental problem. Although Duncker's experimental problems are essentially puzzles, he is keenly aware of the limits of such experimentation, and strives to cover a wide variety of problems under

laboratory conditions and supplements these with numerous illustrations from everyday life.

The candle problem which Duncker called the box problem, is one of a series of physical and mathematical puzzles which he investigated in order to examine the hindrance of problem solving by functional fixedness. He describes the problem as follows:

> The "box problem:" On the door, at the height of the eyes, three small candles are to be put side by side ("for visual experiments"). On the table lie, among many other objects, a few tacks and the crucial objects: three little pasteboard boxes (about the size of an ordinary matchbox, differing somewhat in form and color and put in different places). Solution: with a tack apiece, the three boxes are fastened to the door, each to serve as platform for a candle. (Duncker, 1945, p. 87).

Duncker asked subjects to think aloud while working on this problem, and presented the problem in three different ways: with the boxes empty, with the candles, tacks and matches in the three boxes, and with the boxes filled with unrelated objects such as buttons. The first case was the easiest to solve since the empty boxes were more readily perceived as possible platforms. Unexpectedly, the third situation proved to be the most difficult for Duncker's subjects to solve. Duncker explained this deviation from his prediction, not on the basis of the experimental measurements collected, but on the basis of "certain qualitative remarks of the Ss" which "soon made us aware . . . that . . . we had obviously overlooked an important factor" (p. 89). The subjects explained that when the boxes were filled with material relevant to the problem, they were more aware of the boxes than when they were filled with irrelevant material. Duncker calls this greater "'Contact' between

S and object" (p. 90), and considers it a factor facilitating solution even though the boxes are initially "contacted" in their function as containers rather than in their potential new function as platforms for the candles. In this experiment, and in a series of others, Duncker discusses how the perceived "reality" of the initial function of the crucial objects hinders the restructuring of the situation that is required to perceive that the object could be used for quite a different function (as platform rather than container). "Often it is the unfolding of the" new functional "situation which destroys the 'reality' of the old situation and . . . function". (p. 93).

Duncker certainly tried hard to avoid the error of reification. He described thinking and problem solving as an active process, and often put nouns in quotation marks to emphasize that they refer to explanatory ideas and not to objects. He did not assume the reality of hidden processes which cause problem solving behavior. He tried to survey a wide range of problems relevant to everyday life, although some of his work dealt with what Neisser would call "puzzles."

Weisberg and Suls (1973) offer an information processing model of the candle problem (Figure 5.1), which apparently is not yet a working program. Their model of the candle problem is indirectly based on the work of Newell and Simon (1972) and Miller, Pribram, and Galanter (1960). Their experimental verification of the model is based primarily upon the presentation of the problem in pictorial form (Figure 5.2), and Weisberg and Suls (1973) do not discuss what effects this dramatic difference in procedure might have upon their investigation of "Duncker's candle

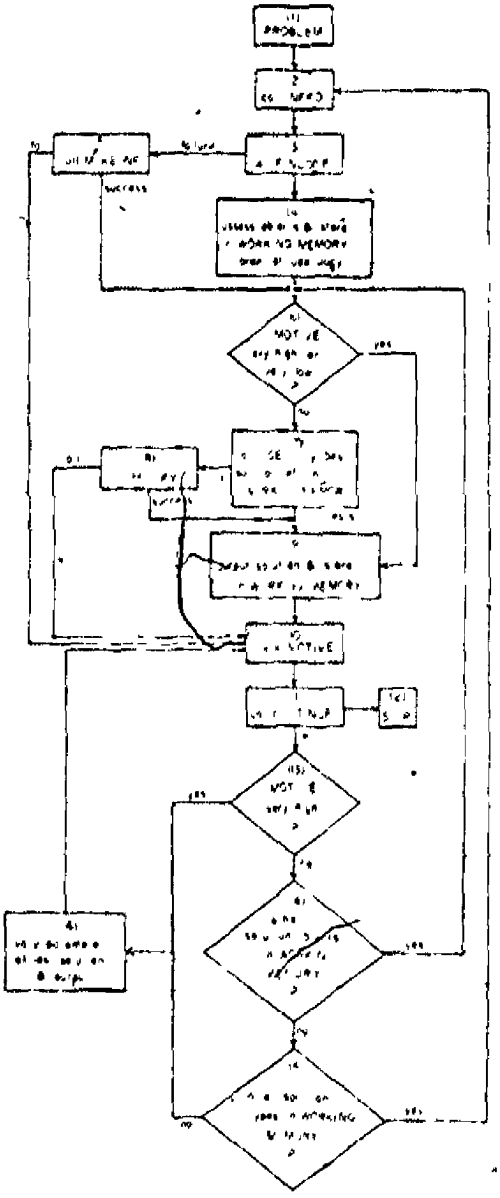


Figure 5.1. Weisberg and Suls (1973) model of Duncker's candle problem.

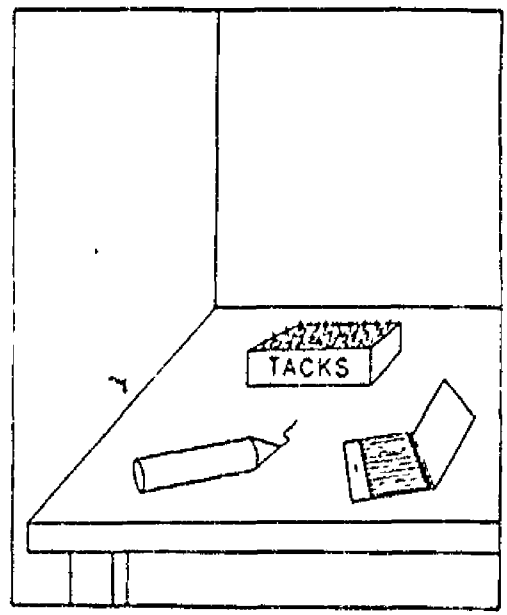


Figure 5.2. The pictorial representation of the candle problem used by Weisberg and Suls (1973).

problem." One relatively small group of subjects (25 of 409 total) was shown actual objects, but even this group was shown only the objects depicted in Figure 5.2, which is a much smaller collection of objects than Duncker used, and these subjects were told "to solve the problem mentally, without manipulating the objects" (Weisberg and Suls, 1973, p. 266).

On the basis of a series of experiments using the picture in Figure 5.2, Weisberg and Suls 1) conclude that Maltzman's (1955) behavioristic attempt to account for behavior in the candle problem on the basis of habit-family hierarchies cannot account for the latency of initial box solutions, 2) claim that the model in Figure 5.1 is supported, and 3) offer an extensive critique of Duncker's concept of "functional fixedness." Only the first of these points is justified.

Weisberg and Suls (p. 273) list ten behavioral differences which they claim their model accounts for. Some of their points are totally unrelated to their specific model. For example, they claim that "labelling the box as separate object will facilitate Box solutions" supports their model because it "disconfirms S's expectations" (Weisberg and Suls, 1973, p. 273). Since there is absolutely nothing in their model about S's expectations, this argument is completely out of place. (The result of labelling the box facilitating the solution of the problem seems to very clearly support Duncker's contention that any change which draws attention to the box, increasing "contact," will facilitate box solutions.)

Arguments which are at least relevant to their model take the

form. "Initial Box solutions take longer to produce than initial direct Tacks solutions," therefore "the S must use MODIFY and/or MAKEONE for Box solution" (p. 273). This is clearly a non sequitur; even statistically significant time differences do not imply the existence of a particular information processing subroutine which the subject "called" during the extra time. Weisberg and Suls do not even claim any evidence which would support the role of MOTIVE in their model, although they assume "motivational factors . . . depend upon the momentary value of the parameter MOTIVE" (p. 260). In the model MOTIVE apparently functions as a "counter" which regulates how many times certain subroutines are engaged. Such a device can be useful in computer programming, but it is hardly worthy of being called a "model" of human motivation.

The first part of Weisberg and Suls' criticism of Duncker's concept of functional fixedness does not hold up at all. Both the experiments reported in Weisberg and Suls (1973) and some of their previous work

> demonstrated . . . that the functional fixedness effect could be eliminated by simply pointing out the box to S through the use of a verbal label. Apparently, functional fixedness was not due to perceptual difficulties, as Duncker had believed. (p. 256)

Duncker (1945, e.g., pp. 97-99) discussed at some length a variety of ways in which changes in the instructions, coloring of the boxes, etc., could make the crucial objects more prägnant and facilitate the restructuring of the situation required for solution. Far from refuting Duncker's contentions, the results obtained by Weisberg and Suls actually confirm Duncker's theory that increasing the Prägnanz of the boxes (by

labelling them) will facilitate box solutions.

Weisberg and Suls' closing argument is that Duncker's demonstration of functional fixedness may, in part, be based upon experimenter-bias effects.

> Acceptance of the general accuracy of the model . . . brings with it some interesting conclusions concerning the concept of functional fixedness as exemplified by the candle problem. Functional fixedness in the candle problem arises because of E's misperception of S's orientation to the problem situation. That is, we present S with a problem which seems to have a straightforward solution (use the tacks or wax), and when he solves the problem in this way we congratulate ourselves on having discovered an important phenomenon in human thinking, viz., that prior use of an object can interfere with its use in a novel way. However, from S's point of view there is no reason to go further except . . . if he suspects that psychologists are devious people and therefore would not be interested in the obvious. . . .
>
> Box solutions should not be referred to as "correct," since from S's point of view every solution he tries is "correct." Also, the term functional fixedness should not be used to describe S's behavior on the candle problem, since most of the fixedness observed heretofore seems to have been on the part of E, the present authors included. (Weisberg and Suls, 1973, pp. 274-275)

There are some valid points in this argument. Psychologists have, at least at times, exhibited functional fixedness. Duncker (1945, p. 87) was rather rigid about his criteria for a "correct" solution, and, writing in 1935, he did not resolve all of the problems which have since become called experimenter bias effects. However, Duncker (1945, p. 89) did anticipate the problem of such effects, and gave some clear reasons why he believed his experimental subjects had not been biased by their anticipations of what the experimenter might want. Weisberg and Suls accuse Duncker of creating "functional fixedness" as an experimental artifact without even mentioning Duncker's (1945, p. 87ff.) cogent arguments against such an interpretation of his experiments. Finally,

it is very clear from the protocols published by Weisberg and Suls, that it is not the case that "from S's point of view, every solution he tries is 'correct'" (p. 275), especially when, as in their experiments, the subjects were asked to report as many solutions as possible.

Weisberg and Suls commit the basic error of assuming the reality of the information processing routines in their model as the hidden cause of behavior on the candle problem. They seem to combine this worst feature of Newell and Simon's approach with an even worse feature of more traditional behavioristic methods, the compiling of large quantities of data into averages, which are mathematical abstractions although often erroneously treated as though they referred to objective entities. The purpose of the latter reifications (which Newell and Simon do not utilize) is to demonstrate that certain null hypotheses, which could not be true, are improbable (Bakan, 1973; Lykken, 1968; Wallach, 1971).

I shall now consider another comparison, concerned with the problem of choosing a move in a game of chess. De Groot (1965) researched this problem extensively, and Newell and Simon and their co-workers (Baylor, 1965; Baylor and Simon, 1966; Chase and Simon, 1973a, 1973b; Newell, Shaw, and Simon, 1963; Newell and Simon, 1972; Simon and Chase, 1973; Simon and Gilmartin, 1973; etc.) have based much of their own research upon de Groot's findings.

De Groot's (1965, pp. 52-74) approach to the study of chess thinking was explicitly based on the Denkpsychologie of Selz, and to a

lesser degree upon Gestalt theory. His experimental methodology was taken from Duncker's method of asking subjects to "think aloud" while working on a problem. Reflecting upon his own thinking was essential in de Groot's work. The primary chess positions about which de Groot asked a number of subjects to think aloud were taken from his own games in chess tournaments. In many of the secondary research projects, de Groot frequently acted as a subject and asked someone else to write down de Groot's own thinking aloud (de Groot's research was done between 1938 and 1942, without the use of tape recorders). De Groot also made use of the self-reflection of his subjects, asking them about the adequacy of their protocols immediately after recording them, asking the subjects to what extent they felt the instructions to "think aloud" interfered with their normal thinking process, etc. (on the latter issue he found considerable individual variation).

De Groot's extensive analyses of the thinking processes involved in chess play can only be cursorily summarized here. His principle explanatory

> concept of 'progressive deepening' . . . denotes a remarkable phenomenon peculiar to rather lengthy thought processes that are needed for solving difficult choice problems. The analysis of a certain idea (plan), move, or variant proceeds in successive phases of (re-) investigation, either immediately or non-immediately. The investigation not only broadens itself progressively by growing new branches, countermoves, or considerable own-moves. But also literally deepens itself: the same variant is taken up anew and is calculated further than before. The term 'progressive deepening' is meant to include both aspects, which can only be artificially pried apart. (de Groot, 1965, p. 266)

This process is characteristic of the thinking of all reasonably good

chess players, and often leads to "redefinitions" or transformations of the total problem situation which are essential to solving the problem of choosing a chess move.

Among de Groot's more important specific findings are the facts that the very best chess players in the world neither consider more initial moves nor think more moves ahead than relatively weaker players. The best players simply examine better moves. The only other unusual skill de Groot found among his grandmaster subjects was the ability to reconstruct chess positions after seeing them for only a few seconds.

De Groot's analyses of the protocols of chess thinking (including his own thinking) are much more detailed than has been indicated here, and his work remains the standard against which other studies of chess related thinking are judged (Newell and Simon, 1972, p. 749).

Newell and Simon (1972) basically concede that their computer program studies of chess thinking have not achieved the level of psychological understanding reached by de Groot.

> What de Groot . . . calls progressive broadening and deepening. . . . is not prominent in computer chess programs. . . .
>
> Human chess players periodically attempt to redefine the problem. . . . Nothing of the process of redefinition occurs in current computer chess programs. . . .
>
> A major difference between human play and most chess programs lies in the evaluation of positions. The evaluations by de Groot's subjects were often rather elementary. . . . in contrast . . . to the rather elaborate polynomial evaluations that have been used in most chess . . . programs. . . . In human play there seldom occurs a balancing of many factors, some pro, some con, to arrive at an overall estimate. (pp. 752-753)
>
> De Groot shows that redefinitions often occur in chess play. . . . In these situations the dynamic analysis of moves appears more as

> an information-gathering process than simply a forward search through a branching space. (pp. 761-762)

In view of their own estimate of the inadequacies of chess programs (including the Newell, Shaw, and Simon, 1963, program which was specifically designed to simulate human chess play) in producing a "thinking" process similar to those revealed in human protocols, it is not clear why Newell and Simon conclude that it is a human "program" which plays human chess. "With due regard for the differences . . . from one chess program (human or computer) to another, we conclude nonetheless that there are strong generic similarities" (p. 751).

Efforts to simulate human chess play (such as the Newell, Shaw, and Simon, 1963, program) seem to have been abandoned as too difficult a task. Research on computer chess play has continued in two different directions. The primary effort has been in the area of artificial intelligence where attempts to construct programs which play better chess are being worked on by many programmers who are only secondarily interested in simulating human play if such methods will help to increase the playing strength of their programs (see Charness, 1976; Hearst, 1976; Kaplan, 1977, 1978; Levy, 1976, 1977, 1978). Even chess playing programs which "learn," (Michie, 1976; Zobrist and Carlson, 1973) are directed toward achieving better chess playing programs rather than toward simulating human learning. Explicit attempts to simulate human chess playing have focused upon "parts" of human chess thinking, such as simulating memory of chess positions, eye movements of human chess players, etc. (Chase and Simon, 1973a, 1973b; Simon and Chase, 1973; Simon and Gilmartin, 1973).

Since Newell and Simon concede that de Groot has made a greater contribution to the understanding of chess thinking than any other approach, and since reification in the works of Newell and Simon has already been discussed, it remains only to consider to what extent de Groot avoided reification in his achievements. De Groot's own approach has evolved from his initially Selzian thought-psychology toward a more behavioristically inclined approach sympathetic to information processing (de Groot, 1965, pp. 371-406, 1966, 1969). Even in his initial formulation, de Groot (1965) described "the theory of Selz" on which his own work was founded as "a machine-like conception of human thought" (p. 67). However, it seems clear that de Groot uses theoretical concepts to explain the thinking of chessplayers, which, while de Groot may consider them to be relatively mechanistic conceptions of thinking, do not refer to a "hidden mechanism" which is assumed as the real cause of human thought. The conception may be relatively "mechanical," but it is not a conception which refers to some "real" mechanisms. Thus de Groot concludes that, at most, he has advanced "a possible theory on the 'real processes' in the . . . mind" (p. 387). The theory, however mechanical, merely is a theory, and does not posit a real mechanism which the theoretical terms are alleged to refer to. In other words, the Selz-de Groot approach is a mechanistic theory, but not a reification of a hidden machine.

The final comparison that I wish to consider is between some examples of "good" and "ugly" procedures for proving geometry theorems given by Wertheimer (1959), and an attempt by Greeno (1976) to

formulate this distinction in information processing terms. The posthumously published book by Wertheimer, the founder of Gestalt psychology, is a brilliant study of the differences between productive and unproductive thinking. Unlike the work of his student, Duncker, Wertheimer uses few explicit experiments. Instead, he focuses his examination upon how thinking, in a variety of settings, occasionally forges ahead to the solution of a problem, and how such thinking processes are distinguished from thinking which either does not succeed or does so only by the rote application of previously learned rules. Wertheimer takes examples from pedagogy, everyday life, and scientific thinking, and applies the general principles of Gestalt psychology to try to find the common principles which operate in various examples of successful, productive thinking.

Wertheimer attempts to integrate the examples which he considers into his theoretical discussion, or, as he puts it, to "face the basic theoretical issues in direct contact with the concrete material" (p. 3). In discussing proof of the area enclosed in a rectangle, Wertheimer calls proofs "ugly"

> which clearly have no inner relation to the issue, which go in another direction, a direction alien to the problem. . . . The steps drop from the blue; their content, their direction, the whole process does not reasonably grow out of the inner requirements of the situation, appears arbitrary, blind to the issue of how the area is built up structurally out of the smaller units in just this form. In the end the steps do lead to a correct, or even proved answer. But the very result is seen in a way that gives no insight, no clarification. (Wertheimer, 1959, p. 33)

In contrast, "the positive, productive course of thinking" faces the

question "in the figure, with reference to its characteristic form. . . . None of the steps implied is in a direction blind . . . to the inner nature of the problem situation" (p. 33). After considering many other examples, Wertheimer summarizes the Gestalt theoretical concept of productive thinking, which he insists differs fundamentally from traditional logic and from any form of associationism.

> Thinking consists in
> envisaging, realizing structural features and structural requirements, proceeding in accordance with, and determined by, these requirements, thereby changing the situation in the direction of structural improvements, which involves:
>
> that gaps, trouble-regions, disturbances, superficialities, etc., be viewed and dealt with structurally;
> that inner structural relations--fitting or not fitting--be sought among such disturbances and the given situation as a whole and among its various parts;
> that there be operations of structural grouping and segregation, of centering, etc.;
> that operations be viewed and treated in their structural place, role, dynamic meaning, including realization of the changes which this involves;
>
> realizing structural transposability, structural hierarchy, and separating structurally peripheral from fundamental features--a special case of grouping;
> looking for structural rather than piecemeal truth.
>
> In human terms there is at bottom the desire, the craving to face the true issue, the structural core, the radix of the situation; to go on from an unclear, inadequate relation to a clear, transparent, direct confrontation--straight from the heart of the thinker to the heart of his object, of his problem. All the items hold also for real attitudes and for action, just as they do for thinking processes. (pp. 235-236)

Greeno (1976) took up not only the task of showing that computers can solve geometry problems (which had been demonstrated as early as 1959, Gerlernter, 1960), but the much more difficult task of showing that computer programs could simulate "good" and "ugly"

solutions in Wertheimer's terms. He began with the assumption that

> Knowledge that is required for geometry can be represented as a production system, including mechanisms that are found in current theories of problem solving (e.g., Newell and Simon, 1972) for setting goals and searching in a problem space. (Greeno, 1976, p. 124)

The example which Greeno deals with is called the problem of vertical angles (Wertheimer, 1959, pp. 97-107), namely proving that the opposing angles formed by the intersection of two straight lines are equal (proving $\underline{a} \cong$ in Figure 5.3). Greeno (1976, p. 146-147) gives two flow charts of solutions to the problem, one which primarily uses algebraic relations and one which primarily uses geometric relations. He proposes that the second meets Wertheimer's criteria for a meaningful solution since it is "slightly simpler" and "uses only geometric relations" (p. 147). Greeno concedes that simplicity in terms of number of steps is not what Wertheimer had in mind in his examples of good or meaningful proofs.

> The second feature . . . is the extent to which they use geometric relations, rather than algebraic relations. I think that this may have been what Wertheimer had in mind in referring to understanding structural relations in this problem, rather than applying an algorithm in a way that often might seem arbitrary in the sense of lacking motivation in the domain of the problem.
>
> This distinction can be made rigorous if we define two problem spaces, one having productions that we call geometric, the other having productions that we call algebraic. (Greeno, 1976, p. 148)

This distinction fails to satisfy Wertheimer's criteria, which, in this particular example he described as "grasping . . . the inner relatedness within the given structure of the needed grouping" (Wertheimer, 1959, p. 103). Furthermore, Wertheimer specifically noted

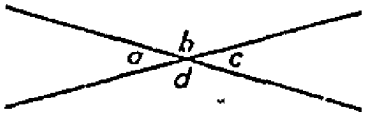


Figure 5.3. The vertical angles problem. The task is to prove a=c. From Wertheimer (1959).

that in "good" solutions, after grasping "the angle a 'as playing the same role in ab as c does in bc. . . . the procedure becomes easier by transition to algebra" (p. 105). Greeno thus clearly fails in his attempt to clarify Wertheimer's concept of meaningful solutions to this problem.

Finally, Greeno's use of the terms "production" and "problem space" are taken directly from Newell and Simon and are similarly reified. Both concepts are technical mathematical abstractions defined in computer programming terms (see Newell and Simon, 1972, pp. 33 and 810 for the technical definitions) which are treated as the hidden cause of a human "producing" a solution in the subjective "space" in which he or she is thinking.

Wertheimer's book, in my opinion, makes the greatest contribution to a scientific theory of thinking and problem solving of any work with which I am acquainted. He describes a vast range of human thinking in clear simple language, and yet distinguishes common features of productive thinking under this great variety of circumstances. Beyond an occasional lapse in which a noun is used to describe a process, where the context makes clear that Wertheimer did not intend to describe the process as a thing, I find no evidence of reifying in his work. Certainly there is nothing so extreme as positing a hidden realm of things-in-themselves which is the cause of "good Gestalten."

In all three examples, I find that the non-behavioristic theorists make a greater contribution to the scientific understanding of human thinking and problem solving than the behavioristic theorists,

and do so without reifying. When one remembers that reification is, after all, an error, this result is hardly surprising.

6. WHY REIFY?

"If men no longer had to equate themselves with things, they would need neither a superstructure of things nor an invariant picture of themselves, after the model of things." (Adorno, 1973b, p. 96)

6.1 Reification: The social context of the intellectual process

From the considerations of the previous chapters, two conclusions seem clear. First, over a period of many years, reification has remained a common error within behavioristic theories of thinking and problem solving. Second, it is possible to study thinking and problem solving without the extensive reification found in cognitive behavioristic studies. The majority of the present work has been confined to the field of psychological theory: to the demonstration of reifying within behavioristic studies of cognition, and briefly in the last chapter, to the demonstration that such reifying is not necessary for scientific study of human thinking and problem solving.

However, consideration of psychological literature alone does not yield an understanding of the reasons why the above conclusions are true. Psychological theorists need not ask why, or how, the reifying behavioristic approach has attained a dominant position within North American English-speaking psychological theory. Nor is there any clear indication within psychological theory, why less reifying approaches--dialectical, Gestalt, hermeneutic and phenomenological psychologies--have a relatively small following among scientific psychologists in

North America. Psychologists have occasionally discussed such problems, but in so doing they move out of the field of psychological theory per se, into the sociology of psychological knowledge. In order to "grasp the inner connectedness of the problem" (Wertheimer) of reification within psychological theory, we need to see the way in which this problem is structurally connected with reification in the society in which the psychological theory is produced.

Reification has been discussed primarily outside psychology. After all, Lukacs (1924/1971) put the problem of reification forward, not as an esoteric error of psychological theorists, but as "the central structural problem of capitalist society in all its aspects" (p. 83). Ingleby (1968) and Jacoby (1975) have provided the most extensive discussions to date of the ideological role of reification in psychology. Psychology "reflects" the social fact that a human's services for some period of time is a commodity, economically equal to other purchasable things. A psychology instructor for the next year can be purchased for about the same price as a good automobile. The fact that human labor is a commodity is "reflected" in psychological thought which considers humans as things.

However, this is only one aspect of the ideological role of reification in psychology. Sciences, like psychology, do not merely passively "reflect" the given social order, but also contribute to maintaining and changing the social order. Husserl (1970) has this aspect of science as ideology in mind when he says that "Merely fact-minded sciences make merely fact-minded people" (p. 6). Reifying

psychology actively helps to "shape" the behavior of persons, through clinical and industrial applications, and by helping to regulate the social "image of man." Reifying psychological theory contributes to the legitimation of the idea that people are, and should be treated as, things. Reification in psychology is not merely technically incorrect, but it also contributes to the use of electroshock treatment, advertising for overconsumption, and other harmful effects. This two-fold ideological function of psychology is, I believe, essential to understanding why reification prevails in psychology.

Among theorists who discuss reification as a problem in psychological theory, many note that it is an error which is difficult to overcome. English and English (1958) call the error "insidious" in psychology. Laing (1959) terms it an "inveterate tendency" (p. 9). Bridgman (1936) describes reification as so difficult to resist that it requires an effort similar to the way medieval monks used to resist the urges of the flesh. Husserl (1970) points out that thinking, without reifying in the manner typical of "objective" sciences, is extremely difficult. To perform the epoche of objective science demands "at first, a complete personal transformation, comparable in the beginning to a religious conversion" (p. 137).

Why is it so difficult to think without reifying? Ingleby (1968) accepts a rather dogmatic answer by Lukacs' student, Goldmann (1969):

> Now there are also elements of reality essential to the existence of a class which it is not in the interest of that class to have subjected to public, or even scientific scrutiny. Anyone seeking to study such elements will encounter powerful internal and external resistances. (p. 43)

Politically conservative thinkers have also noted that thinking without reifying is extremely hard. Berger and Pullberg (1966) consider "de-reification" essentially impossible for the vast majority of people within a stable culture. Their argument is related to the claim that alienation is inevitable (Hyppolite, 1969; Kaufmann, 1970). Berger and Pullberg define reification as the "alienated mode" of normal human thinking about the objective products of society. Both Lukacs' (1924/1971) "orthodox" Marxist view that the proletarian revolution will sweep away reification, and Berger's view that reification is inevitable seem overly dogmatic (see Meszaros, 1970).

The question of whether reification and alienation are socially necessary will not be resolved here. Both politically conservative pessimists and Marxist optimists consider theoretical reification, such as that which I have been examining within psychological theory, to be caused by, and to contribute to "pre-theoretical" reification. Hence the overall elimination of pre-theoretical reification would necessarily mean the elimination of the error in psychological theory. However, the fact that theoretical reification is situated in a social context allows some further consideration of the problem as it affects psychological theory, in a society where pre-theoretical reification has not been eliminated.

Berger and Pullberg consider language to be an essential means of implementing and transmitting reification from one generation to another. The particular form of the language may be more important than they indicate.

> Thus we are compelled in many cases to read into nature fictitious acting-entities simply because our sentence patterns require our verbs, when not imperative, to have substantives before them. We are obliged to say 'it flashed,' or 'a light flashed,' setting up an actor IT, or A LIGHT, to perform what we call an action, FLASH. But the flashing and the light are the same; there is no thing which does something, and no doing. . . . Scientific language, being founded on western Indo-European . . . sees sometimes actions and forces where there may be only states. . . . scientists . . . unknowingly project the linguistic pattern of a particular type of language upon the universe, and SEE them there, rendered visible on the very fact of nature. (Whorf, 1956, pp. 262-263)

The difficulty which many seem to have experienced in attempting to think without reifying is, in part, due to the way that "Standard Average European" (Whorf, 1956) languages utilize nouns, which compels us to think in terms of things rather than processes. Whorf and Lee (1959) suggest that we might reify less (or at least differently) if we thought in Hopi or the language of the Trobriand Islanders. If "reality" is socially constructed, the construction of language is part of this process, and the ideological function of reification within psychology may, to some extent, be corrected by utilizing care in scientific language.

Theoretical scientific systems of thought, like psychology, may be able to dispense with some reification without a major transformation of society. Intellectuals could play a more positive social role by attempting to purge their disciplines of reification, which might contribute to a relative de-reificatio in the everyday world. As I have already indicated (Section 5.2), I believe that the first important step toward such a de-reification in psychological theory is the explicit inclusion of self-reflection within the method of science.

A number of psychological methodologies do this, Husserl's phenomenological psychology offers particular promise. The epoche offers a possibility of de-reification at various levels of theory without waiting for a Communist utopia to make such thinking possible. Husserl's phenomenological psychology is not necessarily incompatible with Marxism as Paci (1970) has shown. Husserl's phenomenological epoche is one method which would restructure the whole problem of reification.

Two very different opponents of reification, Korzybski (1958) and Adorno (1973b) have both suggested that another manner of "restructuring the whole" would involve the use of a dialectical logic which allows A to be both B and not-B simultaneously. The "reality" of someone's "possession" of a certain "amount" of intelligence as measured by tests can become functionally fixated in a way that prevents our realizing that this person may act intelligently and stupidly under different circumstances, or even at the same time. The notion that the concepts with which we should work as scientific psychologists are so completely fluid is rather frightening. It is not certain that we can conduct science in such a manner, but to explore such possibilities would certainly result in more "realistic" psychology than the present mainstream system of reification. If there is no hidden reality of things-in-themselves which we are approximating in our scientific pursuits, but only an ever changing interaction between the present situation and the present individuals, a very different form of scientific psychology would result. We could not pretend to ourselves that there is a hidden realm which is neither body nor mind, where standard

information processing "routines" operate. To try to grasp the structure of a whole which is the interaction of a capricious person in an ever changing world, using a language which both distorts and reveals, would not lead to an ability to predict and control human activity.

Perhaps the goal of prediction and control is as much a delusion as reification. Certainly, it is not legitimate to claim that determinism must be the case because we cannot do science without it (Skinner). But to restructure the whole by abandoning Aristotelian logic, or by the phenomenological epoche is a radical step for the average working psychologist. Physicists have become aware that there are limits to their ability to know with certainty. The social scientist does not yet have a clear grasp of the limits of certainty in the human scientific fields. To abandon the "certainty" of positivistic progressive knowledge is to step into a world where very little is certain. To attempt the scientific study of psychology in a world where "'The truth is the whole,' and the whole is false" (Marcuse, 1960, p. xiv) is frighteningly difficult. However, this may be the world in which we live.

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SUMMARY OF THE THESIS

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THESIS

REIFYING THINKING A CRITIQUE OF COGNITIVE BEHAVIORISM AND THE STIMULUS-COGNITION-RESPONSE (S-C-R) MODEL IN PSCHOLOGICAL THEORY

Reification is the apprehension of human phenomena as if they were things (Berger and Luckmann, 1966) Within psychological theory, the error may involve both the treatment of persons as things and the hypostatization of particular psychological concepts. The process of reifying in psychological theorizing normally has aspects of abstracting, forgetting, and positing a concrete entity The error is considered common in psychological theory, and has been criticized by many writers, including Holzkamp (1964), Ingleby (1968), and Jacoby (1975)

The use of operational definitions by behaviorists has contributed significantly to the hypostatization of psychological concepts, despite the fact that such definitions were once regarded as a safeguard against reification Bridgman's (1928) original notions of operational analysis required considerable alteration in order to utilize them within the context of methodological behaviorism (Stevens, 1939, Tolman, 1936)

Contemporary cognitive behaviorists have continued to rely upon the methods of operational definition developed by neobehavioristic theorists The result has been the extension of reification to numerous concepts referring to private events, as well as the continuation of reification of public aspects of human activity Increased interest in cognition among behaviorists has not resulted in a new paradigm of psychological research but rather in an extension of the neobehavioristic stimulus-organism-response (S-O-R) model to what I call a stimulus-cognition-response (S-C-R) model of psychological functioning

The most highly regarded contemporary theorists concerned with adult thinking and problem solving, Newell and Simon (1972), utilize an information processing approach within the general scope of operationistic behaviorism Although they have contributed a computer program model of the mind which is a genuine scientific advance over previous behavioristic theory, they nevertheless hypostatize information processes as the hidden cause of actual human thinking, and reify human problem solvers as information processing systems

Some non-behavioristic approaches to the study of thinking and problem solving are briefly considered, and I conclude that significant contributions to the scientific study of this area have been made without reifying Gestalt theorists, thought-psyhologists, phenomenologists, dialectical psychologists, and others who explicitly utilize self-reflection by psychologists upon their own thinking offer a variety of non-reifying alternatives The phenomenological temporary suspension of belief in the objective world might offer a systematic antidote to reification The reasons why reification has been so common in behavioristic studies of adult thinking probably lie outside the area of psychological theory, in the sociology of psychological knowledge

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