B

CENTURY PSYCHOLOGY SERIES

Cognitive Psychophysiology: Principles of Covert Behavior

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Cognitive Psychophysiology

principles of covert behavior

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Preface

The major problem in writing a book on cognitive psychophysiology is how to cover the area adequately. The task is enormous, and obviously impossible for one person to accomplish with thoroughness. A truly complete coverage would include summaries of all psychophysiological measures (GSR, EEG, EMG, etc.) of activated neuromuscular circuits during numerous mental processes, as well as relevant aspects of behavioral cognition (linguistics, information processing systems, etc.). To this is added the technical procedures of measuring covert processes that involve gathering and processing data; this gets us into electronics, computer technology, statistics, neuromuscular anatomy, physiology, psychophysiology, as well as psychology.

One approach to a problem of this magnitude is that of the symposium in which there are cooperative efforts of many. Collaboration perhaps allows more complete coverage with more varied, stimulating ideas but typically lacks uniformity in approach and data bases. The alternative of the individual approach, as in the present instance, leads to restrictions and incompleteness of areas of coverage. (A symposium would not, I think, have come out with Newton's *Principia*, though on the other hand, one person could not have built the atom bomb.) With an apology that I know will be readily accepted by anyone who tries to keep up with the exponentially increasing literature in cognitive psychophysiology, I have restricted the empirical coverage to four data fields: electrooculography (Chapter 5), speech muscle electromyography (Chapter 6), somatic electromyography (Chapter 7), and electroencephalography (Chapter 8). Even so, it was physically (including psychologically) impossible to include the most recent research, and the coverage of these four areas is chronologically uneven since I could not write all four chapters simultaneously, as well as get along with the rest of the book. Without these sacrifices, this book would never have been published; as it is, I feel as though I have just stumbled over the finish line that I set out for in 1960.

Another constraint in covering the topic is book length, and at the sound suggestions of the editor and of Kenneth MacCorquodale (to whom also I owe a special debt for encouragement of this project over many years), I have noticeably shortened the original version sent to the publisher in December 1976—principally, I have removed the technical laboratory chapters on how to measure covert processes, and am publishing them as a separate manual elsewhere. In these days of rapidly rising prices the publisher and author have tried to price the book so that it is especially available to students.

My special hopes for this book are two: For lay thinking, above anything else, that it will help replace the naive Cartesian notion of a cerebral homunculus, of a Donovan's brain model of the mind being solely within the skull. The data to substantiate that we think with our entire body appear to me to be overwhelming, and the numerous extensive measurements of autonomic, cerebral, and muscular events during all cognitive activities fit well with a neuromuscular circuit model of the mind in which there is complex linguistic and nonlinguistic information processing (Chapter 9). What *is* incredible is that anyone ever did hold the notion that cognition is exclusively a brain function. A close examination of the issue shows that even such staunch "centralists" as Lashley, Hebb, and Osgood did not espouse a strict centralism in which peripheral mechanisms do not serve some function (such as in feedback circuits) during thought. My second hope for the book is a scientific one. We have many varied efforts at studying covert processes (Chapter 2), both theoretically and empirically, but they form such a hodgepodge in our history. I hope that this book will provide some unification and thus a sound foundation for a science of covert behavior which should be at least as scientifically productive as our traditional science of overt behavior. Principally, our effort is to achieve a wedding of behavioral mediation models with empirical psychophysiological efforts to measure directly those hypothetical constructs.

My indebtedness over the many years is to so many that they simply cannot all be named. Collectively, my dedicated and inspiring students, both graduate and undergraduate, deserve the highest ranking, and I think I remember them all starting with the small group of four who met evenings in 1959. My second debt must also be collectively to our colleagues who went before, and I think principally to John Watson (whose great insight on this issue I did not adequately appreciate until many years after graduate school, though I had memorized him thoroughly), and to Edmund Jacobson (who, with a different framework, independently confirmed many of my thoughts about research and about the

mind, as well as having shown me the way in many respects). My colleagues (Black, Chapman, Grings, Hefferline, Jacobson, MacNeilage, Mulholland, Osgood, Paivio, Rechtschaffen, Sperry, and Stoyva) in our Psychophysiology of Thinking Symposium (Academic Press, 1973) were most helpful for our field, and for giving useful direction to my ideas since then.

Of specific individuals, I cannot adequately express the value of my conversations and written interchanges with Charles Osgood, nor to his student, Meredith Richards, for the back-breaking task of improving the verbal presentation of the book. Finally, those who labored so effectively on the typing, clerical, and data-organizing tasks have well earned special thanks. Foremost is Charlotte Collings, whose outstanding help was given for well onto two decades, and to Betty Loving, Bernelle Rich, Fern Greenway, and Elke Thompson for the indices. Others are acknowledged as appropriate within the text.

Finally, I should note that when I first started in this field, having decided that the areas of secondary reinforcement, incidental learning, and knowledge of results in which I was then working showed limited promise, I met with some considerable resistance from some of our colleagues. These notions of directly measuring mediational constructs, of the importance of feedback functions within neuromuscular circuits, of directly explicating mental processes through psychophysiological measures, and in fact of seeking a systematic data base for the entire area of covert behavior evoked little sympathy of understanding some two decades ago. Usually the harshest criticism of a scientific approach to cognitive psychophysiology came from scientific materialists more often than from the mentalists, which has always been surprising to me. Since then, the development of a firm data base and of some primitive guiding theoretical notions have changed all that. I am especially heartened by the enthusiasm about the developing science of covert processes expressed by students and young scientists throughout the world, and I look forward to hearing further from that group over the years. As has been said by a number of individuals, what I refer to as my "science fiction chapter" (Chapter 11) is really not that far away.

F. J. M.

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part one

Introduction

1

The Task of Psychology

I. The Problem of "Mind," and the Development of a Natural Science Approach to "Mental Processes"
II. Bodily Locus of Cognitive Activities
III. From Localized Center Models to Circuit Concepts

The Problem of "Mind," and the Development of a Natural Science Approach to "Mental Processes"

If one observes our science in historical and contemporary perspective, one might well wonder how people developed the notion of nonphysical mental processes in the first place. Perhaps early humans invented mentalistic concepts about nonmaterial phenomena and agents in an effort to help them survive in a strange and hostile world. One can, for example, imagine a primitive human being dreaming of the dead and, on awakening, referring to those visited in the dream as "spirits." When faced with threatening events, humans may have postulated mystical gods as causal agents; they could then achieve security by placating the gods with worship and sacrifices. The personal experiences of "self awareness" and of "silently talking to oneself," having no apparent physical origin, might have lead primitive humans to such notions as the homunculus—or a separate entity within oneself capable of monitoring perceptions and thoughts. Eventually, such presumed phenomena became reified when languages incorporated words like "consciousness," "mind," "thoughts," and "ideas" into the vernacular, with no reference to corresponding physicalistic events.

Mentalistic concepts became firmly implanted in Western culture when they were formally developed by the early mental philosophers. The flowering of science in the seventeenth and eighteenth centuries guided some mental philosophers toward the empirical study of mind. Most prominent among these were the British Associationists, from Thomas Hobbes in the early seventeenth century to Alexander Bain into the twentieth. John Locke's reasoning about atoms of the mind, analogous to early models in chemistry, is a good example of the influence of science on the mental philosophy of that period. Scientists, too, started applying observational methods to "mental processes." In particular, physicists began to study the role of sense receptors as necessary instruments of scientific observations in the study of the nonliving world. A classic example is the minute time differences which existed between the astronomical observations reported by Maskelyne and those reported by his assistant Kinnebrook, whom he dismissed; that difference inspired the psychological concept of the "personal equation." At about the same time, physiologists were asking questions about sensation, perception, and the mind itself, particularly when they sought to understand the functioning of the receptor systems. The commonality in the endeavors of such diverse academicians as philosophers, physicists, and physiologists were symptomatic of the *zeitgeist* which, in 1879, led Wundt to found the school of "Structural Psychology"—the first science devoted exclusively to the study of mental phenomena.

Thus, the original task of psychology was that of understanding the mind. However, statements made then about "mind" were untestable (technically meaningless, cf. McGuigan, 1978a). As a requisite for progress psychology had to make a transition from nonmaterial conceptions of mental phenomena to strictly physicalistic ones. The transition was slow and, in many instances, extremely painful. A materialistic conception of mind has historically been unpopular, as it frequently still is. Gall, who held that the brain was the organ of mind, suffered great personal abuse and was even denied a religious burial because of his efforts to advance materialism. Nonmaterialistic conceptions of mind still abound, amazingly, even among scientists. The great Sherrington, in the early part of the century, sought in vain for a special nonmaterial energy of mind. As late as the 1950's, Lashley still found it necessary to vigorously attack nonmaterialistic conceptions held by contemporary neurologists (cf. especially Eccles, 1966).

There were both negative and positive reasons for the eventual transition of scientific psychology to materialism. On the negative side, decades of vigorous introspective investigations followed Wundt's founding of psychology; with these studies the Structuralists and other introspectionists hastened their own demise by accumulating evidence of the sterility of their efforts to introspect on a nonmaterial consciousness. The coupe de grace to Structuralism was delivered none too gently by the classical behaviorists, led by John Watson (1913). The critical argument in their attack was that direct observation of a nonmaterial consciousness through introspection did not satisfy a principle of intersubjective reliability—one person's observation of his or her own consciousness is necessarily private, and only publicly observable events can be scientifically studied. Behavior can be observed by more than one person, and as a phenomenon *does* satisfy a criterion of intersubjective reliability. As a result of the influence of the early behaviorists, psychology ceased to be the introspective study of "oneself" and became the behavioral study of "the other one."

While it gradually became apparent to psychological scientists that nonmaterialism was a blind alley, on the positive side materialistic approaches became increasingly productive in advancing our understanding of the higher mental processes. In proclaiming behavior to be the subject matter of psychology, the early behaviorists still maintained many of the old mentalistic terms. This point requires special attention, for it is often said that theirs was a complete rejection of "mentalistic notions." What the behaviorists actually did was abandon introspectionist definitions (in terms of "nonmaterial stuff") and redefined mental terms according to the principles of natural science. Hence, "emotion" ceased to be referred to as "affective quality" and was defined instead as visceral responding. "Consciousness" was redefined as the objectively observable behavior of a person describing the internal and external world (a process denoted by Skinner [1957] as "internal" and "external tacts"). And "thinking" was redefined primarily as implicit language behavior. Though Lashley eventually diverged theoretically from his teacher, Watson, his brain research on the higher mental processes was instrumental in advancing a staunch psychological materialism.

In short, the combination of (1) the growing awareness of a lack of scientific respectability of nonmaterial conceptions of mind, and (2) the fruitful contributions that followed from materialism, have led to the present renewed interest in the scientific study of cognitive processes.

Even so, an aura of mysticism about the behaviorist's view of higher mental processes persists—but for pragmatic rather than scientific reasons. Essentially, the problem has been one of smallness: the hypothesized implicit reactions equated with thought are minute, and for decades the young behavioral science was not technologically equipped to observe such events directly. The hypothesis was for long untestable. With the development of laboratories for making extremely sensitive psychophysiological measurements, we have acquired the technical capabilities to directly observe small scale responses and thus make suitable tests of early behavioristic theories. By amplifying and displaying minute muscular and glandular events on cathode-ray oscilloscopes in the psychophysiological laboratory, we succeed in dissolving the mysticism about small scale behavior. A response, in short, is activity of muscles and glands; it is irrelevent whether the response is overt (large) or covert (small). It is irrelevant whether a response can be observed with the naked eye (i.e., is overt) or whether specialized laboratory techniques are necessary for its observation (i.e., it is covert). In either case, the behavioral phenomenon can be objectively studied.

Psychology has made considerable progress by means of the classical S-R model. But from the early days behaviorists have been aware of gaps in the statements that can be made with this single stage model. Those gaps have been apparent when dealing with complex behavioral processes, and multiple hypothetical constructs have been proposed to bridge between directly observed external *S*s and overt *R*s. The variety of proposed *r*s and *s*s (inferred covert responses and internal stimuli) in the literature is a reaction to the shortcomings of the classical S-R model and our need to talk about unobserved behavior through traditional methods. One example of a hypothetical construct that intervenes between external stimuli and overt responses is Watson's implicit language habit, i.e., S ⟶ (implicit language responses). .⟶ resultant internal stimuli. .⟶R.

More recent hypothetical constructs are the fractional anticipatory goal response (r_G—s_G) of Hull (1943), the mediational response of Kendler and Kendler (1969), the mediating reaction (r_M) of Osgood (1953), the perceptual response of Schoenfeld and Cumming (1963), and the representational and implicit associative responses of Bousfield, Whitmarsh, and Dannick (1958; also studied by Underwood, 1965). Though such constructs have firm empirical anchorings on the antecedent (stimulus) and consequent (response) sides of the paradigm, their "reality status" is more convincing if they can be directly observed rather than inferred indirectly. By directly (psychophysiologically) measuring the covert bodily events that intervene between external stimuli and overt responses, we should be able to reduce the number and kinds of postulated logical constructs (some will probably turn out to be psychophysiologically impossible) and thus advance more parsimonious theories. One can envision an increasing degree of mutual facilitation between the psychophysiologist and the behavior theorist, in which each guides the other in both empirical research and theory construction.

R. C. Davis and his colleagues have indicated yet another reason for studying covert behavior, namely that overt responses are numerically small in proportion to covert responses: "One has but to observe them on a set of recording instruments to believe that they are by far the most numerous responses of the organism. It is clear that any overt response . . . is surrounded by a wide penumbra of them. . . . In this sea of somatic response an occasional wave breaks into an external response" (Davis, Buchwald, and Frankmann, 1955, p. 1). Related to this fact that covert responses are more numerous than overt responses is the point that covert responses underlie and thus determine overt behavior.

Two points emerge from this brief history: how the notion of "mind" arose in the first place, and how we laboriously arrived at a natural science approach to mental processes. The stage has thus been set for a scientific explication of "mind"—psychologists have provided the principles for explicating "higher mental events" in terms of covert processes, and psychophysiologists have developed suitable laboratory techniques for measuring these previously unobserved reactions. In short, contemporary study of covert processes can now be related to the original task of psychology, that of understanding the nature of the higher mental processes. Now, however, we are studying publicly observable events and thus proceeding on the natural science basis called for by Watson and his colleagues.

With this general understanding, we shall face some more specific questions about how this study should proceed. These involve the bodily locus of thought and the processes that constitute cognitive activities.

Bodily Locus of Cognitive Activities

Some theorists have considered thought to be strictly a function of brain activity, while others have asserted that thought involves other bodily systems as well. The former position has been referred to as the "centralist" and the latter as the "peripheralist" position. Two representations of the peripheral and central models were presented by Dashiell (1949), based on his 1925 article (Fig. 1.1). The peripheral model most closely corresponds to the principles of classical behaviorism, which stresses primarily response activity.[1] Behaviorists probably overemphasized the importance of peripheral events (as when they regarded thinking as only responding)—an understandable excess given the context of psychology in the early part of this century. At that time, behaviorists were scrupulously endeavoring to avoid the pitfalls of mentalism by limiting their statements to objectively observable behavior that could provide a sound data base for the development of a truly scientific psychology. Unfortunately, in emphasizing peripheral events, the classical behaviorist frequently paid only lip service to brain phenomena. The brain was little understood, and it was often said that "CNS" stood for the "conceptual nervous system." By concentrating on peripheral systems, behaviorists thus attempted to avoid the pitfalls of merely transferring unseen mental events to unseen brain events. As has often happened in science, positions have been oversimplified and "oversold" in an effort to introduce unpopular ideas. At a time when there was no question about the brain being important for cognition, behaviorists were trying to account for the higher mental processes within the constraints of their science of observable behavior by calling attention to the role of muscular and glandular events.

[1] One common error in understanding the peripheral model results from an oversimplification in Fig. 1.1. As Dashiell pointed out, motor representation A erroneously represents thought as a simple serial order process. Probably, though, no serious theorist ever conceived of thought as a single channel linear process—certainly not Watson who took pains to represent thinking as complex multichannel interaction (see especially our Fig 2.2 taken from page 266 of his 1930 edition of *Behaviorism*).

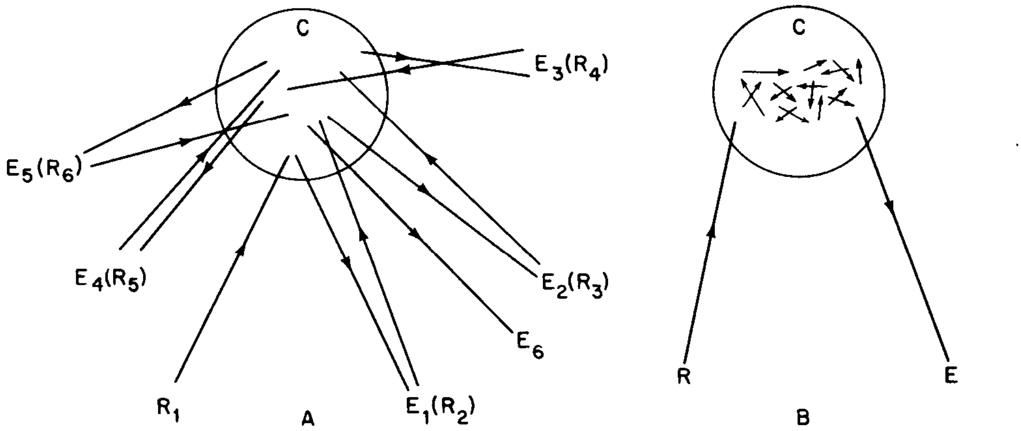

FIG. 1.1 Two extreme views of the role of the brain in thinking. In an extreme motor (peripheral) theory (A) "the original stimulations from a problem situation playing upon receptor R_1 evoke an abbreviated response at effector E_1. This in turn serves to excite receptor R_2 (kinesthetic or other) which evokes a response at effector E_2. And so the nascent abbreviated symbolic responses continue until the thinking eventuates in an overt act, as performed by effector E_6 . . . The neural counterpart of each idea was an excitation in a local spot (cell-cluster) in the brain [B]; and the transitions from idea to idea were referable physiologically to the passage of a neural impulse or train of impulses from cell-cluster to cell-cluster. Now this shooting around of neural currents within the cerebrum is as grossly oversimplified an account as is the story of receptor-effector arcs told above and suggested in part A of the figure" (Dashiell, 1949, pp. 588–89).

If the behaviorists overemphasized responses, the classical centralist neglected them. Extreme centralism has been referred to as "Donovan's Brain Theory," named after the science fiction story in which Donovan arranged to have his brain preserved so that he could continue thinking after his normal life ceased (McGuigan, 1973a). One obvious difficulty with this theory is that, if such an isolated preparation *could* think, without input from the external and internal environment it probably couldn't think very well, and most assuredly it wouldn't have much to think about. On the other hand, the term "peripheralism" is equally unfortunate in that it conjures up an image of thought occurring in a brainless body. One who thinks of thought as only behavior (muscle and gland activity) neglects critical nervous processes. A "peripheral, brainless person," and Donovan's brain model seem equally restrictive in accounting for cognitive processes.

In considering the question of where in the body thoughts occur, an objective scientist would eschew any predisposing biases, considering it possible that any or all bodily systems might serve some function during cognition. Actually, a variety of bodily systems have been empirically implicated in cognitive activities. Excellent accounts of brain functioning during thought may be found in Delafresnaye (1954), Eccles (1966), and more recently Young (1970). The eye has ranked high in importance among bodily organs implicated during cognition. Hebb (1968) held that