

Psychiatry, the state of the art:  
vol 1. clinical psychopathology:  
nomenclature and classification  
(p. 311-316)

Faculté de Psychologie et des  
Sciences de l'Éducation  
UNITE DE DOCUMENTATION  
Université de Liège, B-32  
B-4000 LIÈGE  
Tél. 041/56 20 27 - Fax 041/56 29 44

IS BEHAVIOR STILL A RELEVANT VARIABLE ?

Marc N. Richelle

Psychological Laboratory

University of Liege, Belgium

The title of the present paper, being in the interrogative form, suggests that those currently doing psychiatric research sometimes dispense themselves with any reference to behavior. I shall contend that such a trend, if it really exists, might, among other consequences, result in not giving the recent achievements in various fields all the impact they could have. The question raised bears on clinical research as well as on fundamental or preclinical research, though I shall refer only to the latter, and more specifically to experimental psychopharmacology, for illustrative examples.

BEHAVIOR AND PSYCHIATRY : A BRIEF RETROSPECT

It might seem advisable first to agree on some definition of behavior. It turns out that this is not as simple as might seem, because of semantic drifts and because the term has been contaminated by the theories with which it has been associated, or by the (mis)interpretations of these theories. To state it briefly, let us admit that behavior covers active interactions in which an individual organism engages with his environment.

The place given to behavior by psychiatrists, be it in their descriptions of mental diseases or in their attempts to explain them, has changed, throughout the history of the field, with changes of emphasis in psychopathological theories on one hand, and with the vicissitudes of the notion of behavior in psychology on the other hand.

It will be reminded that early in the century psychologists, almost unanimously if not always enthusiastically, adopted behavior

as the subject matter of their science, instead of consciousness or mental life. Indeed, this revolution bore on method rather than on subject matter. Psychologists, aware of the many inadequacies of introspection, turned to observable events as providing safer ground for scientific inquiry; and, as scientists in other fields traditionally do without any frustration, they postponed the exploration of many areas until they become accessible to observation.

Nineteen century psychiatry, through its remarkable effort in describing and classifying psychiatric symptoms, had in some way anticipated behaviorism, in giving observable events obvious priority, and in distrusting introspection, the limits of which clearly appear to those who study organisms with whom communication is not always easy to establish.

In a quite different orientation, none more radically than Freud has contributed to a general distrust of introspection as a valid way to know human beings, by disclosing the totally unconscious factors that determine many of their actions.

However, despite its attention to the analysis of behavior, psychiatry has looked at it mainly in terms of symptoms, along the traditional lines of clinical medicine : symptoms are interesting, essentially, in so far as they reveal an underlying pathological state, the origin of which is to be found at another level. In the organicist tradition, some anatomic-physiological anomaly is assumed, while in the psychoanalytic tradition, the dysfunctioning is to be traced to the psychic apparatus. In both cases, behavior has but an accessory status. Explanation, and eventually intervention, are located at another level.

This still prevalent view has not favoured a behavioral approach to psychiatry. Behaviorism logically leads to consider behavior in its own right, but attempts to reformulate psychopathological disorders were slow to emerge. With the exception of a few forerunners, it was not before the nineteen fifties that proponents of behavior therapy or modification developed techniques based on a conception of symptoms, not as revealing some deeper problem, but as the very locus of the disorder. This by no means implies discarding a search for the variables responsible for the behavior being considered. But priority will be given to the interaction of the individual with his/her environment, that is, to a functional analysis that does not take for granted that the final explanation is to be found at another level, be it material or conceptual.

It is difficult to measure the influence of behavioral concepts on psychiatry through the therapeutic practices. Admittedly, these have been, at least in their earliest phase, oversimplifying and, occasionally, triumphalistic. They undoubtedly have underestimated developmental factors in the understanding of mental disorders,

they have ignored structures, as well as the distinctive features and consequences of symbolic behavior, they have, more often than not, indulged into inconsistent eclecticism. They did not adequately reflect the more subtle views of a modern science of behavior.

#### COGNITIVISM AND BEHAVIOR

Two currently expanding trends tend to present behavior analysis as an outdated stage of psychology and psychopathology. One has developed within psychology itself, under the label cognitivism. The other, biological psychiatry, is flourishing within psychiatry, as a modern form of organicism, enriched with the impressive achievements of basic biological sciences. Much could be said on the cognitivist paradigm, as some authors call it, in its relation with psychopathology. The word cognitivism has many different meanings in contemporary psychology, somewhat artificially confused by the use of a common unifying term. To many psychologists, it points to a real change of the subject matter of their science, behavior making the stage free for a new version of mental life, made of information processing, strategies, cognitions, competences, and the like. In its most coherent aspects, cognitivism aims at inferring properties of the mechanisms involved in processing sensory, perceptual, mnesic or linguistic information from experimental operations which, unavoidably, bear on behavioral data. It defines, in a purely abstract way (not by referring to specified neural mechanisms in the C.N.S.) the conditions to be fulfilled by the mental or neural device if it is to generate or control the observed behaviors. It covers a number of research areas traditionally concerned with higher levels of conducts, grasping and processing of information, problem solving, knowing, symbolic representations, and the like. Many cognitivists would also include, as a logical extension of their territory, at least to their own eyes, supposedly cognitive mechanisms which, by inference, are said to underly as well any complex regulation of behavior, such as an athlete adjusting his arm movement toward a target or migrating birds finding their way from one continent to another.

By claiming to offer models of processes located beyond observable behaviors or identifiable stimulations, cognitivism seems to present itself as a kind of psychology better prepared to meet the concern of CNS specialists. It emphasizes internal processes, possible candidates for a neurophysiological or neurochemical redefinition - through it should be emphasized again that cognitivist models are, as a rule, purely abstract, and have, until now, hardly produced any really valuable neurophysiological counterpart (whatever their heuristic significance for psychological research proper).

There is no doubt that cognitivism has reinstated terms and explanations very close to mentalistic formulations that were strongly rejected by a functional analysis of behavior. A famous

example is the notion of linguistic competence as developed by Chomsky and, after him, by a generation of psycholinguists. Inferred from the formal properties of a speaker's utterances, this mental entity has turned out to be of no great help in psycholinguistic research and has been progressively left aside, especially by specialists of language development. What is wrong with such notions is not, as Skinner observed, that they are mental (as such they would simply raise a problem of accessibility that psychology, cognitive or not, is currently solving step by step) but that they are oversimplifying and offer the illusion of explanation. We shall come back to this point in a moment.

#### BIOLOGICAL PSYCHIATRY AND BEHAVIOR

The attitude of biological psychiatry towards the analysis of behavior is of a more subtle and, sometimes, paradoxical nature. Let us first state that, in our view, there can be no real antagonism between the two approaches; there is on the contrary, continuity and complementarity since the behavioral approach unambiguously inscribes the analysis of behavior within biology. This implies a biological theory decisively open to successive levels of analysis and prepared to recognize its specificity to each level in an integrating hierarchy quite alien to crude reductionism. This view has been endorsed by most great biologists today but it is not, by far, part of the thinking habits of all those who practice biological research.

Biological psychiatry, being heir to the organicist tradition, is not totally free from a reductionist conception of mental disorders. Be it in neurochemistry and neuropharmacology, or in genetic studies, the search for univocal organic causal factors is often implicit, if not overtly stated. Pathological behavior is viewed as the by-product of neural dysfunction or of genic disorder. It is given its status of symptoms, in the most classical sense, the origins of which are not to be looked for, and not to be found anywhere else. The success of psychopharmacological treatment gives some support to this reductionism, at least at first sight.

As far as methods are concerned, one would expect that biological psychiatry would resort to the same degree of sophistication when it deals with behavior as it does when it deals with neurochemical or neurophysiological aspects of research. One is struck, however, by the discrepancy between exceedingly refined neurochemical techniques and, for instance, the uncritical use of categories in the description of clinical symptoms, or the choice of crude experimental tools for animal behavior studies, an admittedly necessary link between preclinical research and therapeutic essays on psychotropic compounds.

Let us mention a few examples of such oversimplification in

current psychopharmacological research. A unique test, such as passive avoidance, is said to measure memory, so that any compound that will alter performance in that test will be said to act upon memory capacities. A simple motor test, such as the duration of immobility of rats in water or the locomotor hyperactivity of rats raised in isolation, is taken as valid in checking for antidepressive effects in experimental psychopharmacology. In the first case, one is, curiously enough, back to an old psychology of mental faculties, that looked definitely abandoned years ago. This anachronism might result from two sources. First, the cognitivist trend mentioned above has reinstated terms that behaviorists had learned to avoid, because they are misleading. If it is fair to say that cognitivist psychologists do not use terms such as memory, attention, anxiety, as they were used in the nineteenth century, it is true also that the neurochemist, the neuroendocrinologist, the pharmacologist, when they talk about behavior, make a second hand use of that vocabulary, not distinguishable from prescientific use. Secondly, a central notion that emerged from behavioral studies in pharmacology and eventually became familiar to all psychopharmacologists in the sixties, the notion of drug-behavior interaction, seems to be overlooked by many contemporary researchers. It essentially refers to the fact that behavior is not only a dependent variable the variations of which are to be recorded as a function of pharmacological agents, but that it has also the status of independent variable; pharmacological action is to be described as a function of the behavior being studied, i.e. as a function of a set of environmental contingencies and of modalities of interactions between the organism and its environment. This notion could not possibly have emerged if behavioral psychopharmacologists in the fifties and sixties had not explored a wide variety of such modalities (under the label of schedules of reinforcement for instance). It is easily neglected when one single behavior is taken as a valid "model" of anxiety, depression, memory, etc.. or when behavioral research attempts to keep up with the rapid progresses of neurochemical studies. It is, of course, incompatible with one-way causal explanations, since it implies that external variables are taken into account. At the clinical level, it gives social, historical and cultural factors the place they deserve in the analysis of mental disorders, besides organic factors, and it precludes any a priori anticipations as to the causal relations involved. Curiously, enough in this respect, the cognitivist approach, essentially concerned with disclosing the mental structures of the subject, favours the reductionist revival typical of certain trends in biological psychiatry.

In the second example mentioned above, namely the locomotor hyperactivity of rats raised in isolation, a simple behavior is taken as reference for characterizing drug effects. But this is done in a patent circularity, where the relations between neurochemical, behavioral and clinical levels are juggled away. The be-

haviour used as reference in animal experimentation has been chosen only on the ground that it is altered by a category of compounds that are known to exert a given therapeutic action. More often than not, it is easy to show that another behavior might be used just as legitimately, that would not be modified in the same way by the category of drug being considered.

Coming back to behavior would also dispense experimental research in psychiatry with all the detours and blind-alleys in which it is periodically led by more ambitious "models" such as the learned helplessness model of Seligman, offered as a model of depression. It could be shown easily, after several authors, that interpretations of experimental data are in cases like this exceedingly frail, and far from legitimating extension to human pathology. The drift from experimental data, certainly not without interest in themselves, to more and more farfetched interpretations is often done by resorting to cognitivist concepts.

Finally, a precise analysis of behavior is still strongly desirable in the description and classification of clinical data. In spite of more than a century of symptoms description, and of recent attempts to refine and harmonize the inventory of patients behavior, nosological categories still lack precision in many respects. Some categories retain the imperfections of knowledge in the days when they first emerged, and have not really been critically revised since them. The classical distinction between endogenous and exogenous depression is a case in point. Though not all psychiatrist agree about the distinction, or, if they do, about the criteria on which to ground it, it is so familiar that it is used as a reference, without any questioning, in many pharmacological and genetic researches. These terms, loaded as they are with etiological connotations, implicitly bring support to causal hypotheses adopted in these fields of biological psychiatry. Looking for other possibilities is discarded, or left to approaches totally alien to scientific rules, with little chance for a really integrative synthesis. The contribution of biological psychiatry, in the long run, will find its true significance if it gives behavior the place it deserves and if it faces its complexities, as they appear to both the experimenter and the clinician.