

THEY SHOULD HAVE THOUGHT ABOUT THE CONSEQUENCES: THE CRISIS OF COGNITIVISM AND A SECOND CHANCE FOR BEHAVIOR ANALYSIS

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Cognitivism has dominated mainstream psychology for decades, but lately its position has been weakened. It has been criticized not only by behavior analysts but also by social and evolutionary psychologists, neuroscientists, and authors who are or used to be cognitivists themselves. At the same time, psychologists and neuroscientists in several fields are now rediscovering the necessity of understanding behavior in light of its functional relations. Hence a need for relevant knowledge is felt. Cognitive psychology has little to offer in this regard. Behavior analysis, on the other hand, possesses a wealth of relevant facts and theory. Though chances seem slight that mainstream psychology will return to behaviorism, a window of opportunity now exists for the reintegration of behavior-analytic research into the mainstream.

When psychology was being changed and redefined in the late 1950s and the 1960s, many psychologists saw new possibilities arising from the “cognitive revolution” and started practicing cognitive psychology. Others thought the science had little to gain from the study of unobservable mental phenomena—not behavior, its antecedents, and consequences. This latter group of people often consisted of behaviorists, and guided by B. F. Skinner’s example (e.g., 1977, 1990), they have regularly attacked the cognitivist majority camp. These proponents of the mainstream’s return to behaviorism have pointed out what they see as fundamental weaknesses in the thinking of cognitive psychologists, such as the assumption that mental processes may be measured. Indeed, they have pointed out the problems created by claiming that such processes exist at all (e.g., Uttal, 2000, 2004). And perhaps their most important point: By overlooking the contingencies of reinforcement, cognitive psychologists fail to gain an understanding of the consequences that strengthen, maintain, or weaken behavior (see, e.g., Pierce & Cheney, 2004).

Despite their consistent and often well-reasoned criticism of cognitivism, behaviorists have long been crying in the wilderness. By the 1990s, cognitive psychologists believed their revolution had brought about important and lasting changes. “The cognitive revolution leads to a combined ideological

revolution,” said Sperry (1993, p. 879). “Alternative beliefs emerge about the ultimate nature of things, and a changed cosmology brings a new set of answers to some of humanity’s deepest questions,” he claimed. Such perspectives may not bring about a need for discussion with infidels. Little wonder, then, that no debate seemed to be going on between behaviorism and cognitivism (see Hardcastle, 1994). Behaviorism was in decline, said some (Robins & Craik, 1994), or was simply dead and irrelevant (e. g., Medin & Ross, 1992). Such responses from cognitive psychology were those of a giant feeling secure, choosing to overlook a minor irritant.

But times have changed. For more than thirty years, cognitivism—with its core assumptions that people have mental states, the manipulation of which can be described in terms of rules or algorithms (see e.g., Marr, 1982)—has been the defining paradigm of psychology. Now it is under renewed attack. There are new groups of attackers, and there are more of them than there used to be.

What has made cognitive psychology vulnerable, I shall argue, are some but not all the weaknesses typically ascribed to it by behaviorists. Whether the cognitive psychology we know is thrown out in a new revolution or it just withers away less dramatically, there is no question that mainstream psychology is changing. This change opens an opportunity for behaviorists to affect psychology as a whole to a greater extent than has been possible for some time. However, being able to influence the process of change presupposes a notion of what can and should be changed. The present article is an attempt to have some bearing on that notion.

Functionalism, Behaviorism, Cognitivism

Functionalism was the first American paradigm to dominate psychology. A school of thought with strong roots in biology, it focused on the interaction between organism and environment. This focus led to certain assumptions: Behavior was seen as adaptive, ultimately as a way of furthering the organism’s survival and its chance of spreading its genes. Mental processes were there to serve behavior. Indeed, functionalists held that understanding mental acts cannot be separated from understanding the context and consequences of those acts. Therefore, questions regarding the *function* of perception, thought, emotion, and overt behavior were central. Any field of research and all sound methods were acceptable if they could aid in answering such questions (see Carr, 1925; Wagner & Owens, 1992).

In 1913 John B. Watson published his article “Psychology as the Behaviorist Views It,” signaling a rebellion against functionalism and its forerunner, the structural psychology of E. B. Titchener. Watson’s article started a process that led the hegemony of psychology into behaviorist hands, where it resided for decades.

Behaviorism was never just one thing. It encompassed views from Watson’s (1930) description of thinking as a purely muscular process to Tolman’s (1949) beliefs, expectancies, and field-cognition modes. Still Watson and Skinner, the two main thinkers of this school of thought, both refrained from theoretical speculation regarding “private” causes of human action and wanted empirical research to be about the effect of external stimuli on behavior. In the present article I shall primarily deal with behavior analysis—the movement that does, after all, represent the clear majority of behaviorists today.

Behaviorists aim to predict and control behavior (Skinner, 1972; Watson, 1913), though Skinner (1947) underscores that understanding is the goal, for which prediction and control are the means and the test. Like the functionalists, behaviorists were interested in the relation between behavior and environment. In the main, their interest was limited to this relationship. The behaviorists had little use for concepts they deemed useless, as well as meaningless, such as unobservable way stations between behavior and its consequences. As a result, the thought that causes of behavior could be found within an organism became anathema (see Overskeid, 2006; Pierce & Cheney, 2004).

The cognitive revolution ensued when an increasing number of researchers came to see the behaviorist ideology and research focus as inadequate (e. g., Miller, Galanter, & Pribram, 1960). In his pioneering book, aptly titled *Cognitive Psychology*, Ulric Neisser described the subject matter of the field. It is, he says, “all processes by which the sensory input is transformed, reduced, elaborated, stored, recovered, and used” (Neisser, 1967, p. 4). This is, as Neisser acknowledges, a sweeping definition, possibly including every psychological phenomenon. Hence he stresses that the concern of cognitive psychology is from a particular point of view, that beginning with sensory input. Studying goals, needs, or instincts is also important, says Neisser, but these are the tasks of dynamic, as opposed to cognitive, psychology. Cognitive psychologists have since proceeded along the lines drawn by Neisser, with most researchers going about their task as though cognitive capacities are relatively autonomous and separate from noncognitive capacities (see Braisby & Gellatly, 2005; von Eckardt, 1993).

The cognitive revolution was not a counterrevolution in the sense that its goal was a return to what used to be—certainly not to structuralism but also not to functionalism. Indeed, even if functionalist attitudes and practices seem to have permeated much of American psychology, the one exception may be cognitive psychology (see Owens & Wagner, 1992; Wilcox, 1992).

Behaviorism had abandoned some elements of functionalism and kept others. The cognitivists focused once again on mental phenomena, and in that sense they brought back one of functionalism’s essential constituents. Functionalism also had other important elements, however. Among the most important was the assumption that behavior and mental acts may be understood only in the light of their context and consequences. Starting with the early functionalist writings of William James (1890/1950), this assumption was always taken to include context and consequences in the external world, as well as thoughts, feelings, and motives within the acting and thinking person.

Historically, the empirical foci of behaviorist researchers were narrow, with the study of classical and instrumental conditioning at the center of interest (see Hergenhahn, 1982). The hegemony passing to cognitive psychology meant a widening of the area of interest for mainstream experimental psychologists, but historically speaking, it was still narrow. Since at least the German faculty psychologists of the eighteenth century, mental activities had generally been taken to be of three kinds—not only cognition but also emotion and will, sometimes spoken of as affection and conation (or other more or less overlapping terms; see Hilgard, 1980). Among the three, it was traditionally far from clear that cognition should be allowed any form of primacy. Wilhelm Wundt (1893) thought of emotion as preceding cognition. Likewise, Alexander Bain (1888), the leading figure in late-nineteenth-century British psychology, saw feeling as the primary mental activity, followed by

volition and thought. Bain was a strong influence on William James, who is, of course, often seen as the father of functionalism.

The past fifteen years have seen a revival of the study of affect, and modern authors have faulted cognitive psychologists for not giving sufficient attention to feelings and related fields, such as motivation (see Overskeid, 2002). Furthermore, critics have argued forcefully that core areas of cognitive psychology cannot be properly understood without taking into account, among other things, evolution (e.g., Sugiyama, Tooby, & Cosmides, 2002), social psychology (e.g., Lerner & Tetlock, 1999), development (e.g., Vosniadou, 1996), the brain (e.g., Bechara & Damasio, 2005), and individual differences (e.g., Stanovich & West, 2000). Neisser, too, has dissociated himself from the cognitive psychology he helped to pioneer but later found “too narrow and too vague” (Neisser, 1994, p. 226). Like others, Neisser now wants a cognitive psychology that includes social interaction and the brain, among other things.

Cognitivism and Some of Its Critics

Attacks on cognitivist assumptions are not new, and they include important criticism by people based far from the behaviorist camp (e.g., Penrose, 1989; Searle, 1980; but see Overskeid, 2005). Still, why has cognitive psychology, after playing a leading role for decades, gone from being questioned by only a few to being attacked from many different sides?

Despite the wide definition given in early works like Neisser’s textbook (1967), cognitive psychology in practice has had a research focus too narrow to allow a full understanding of even phenomena commonly regarded as central parts of the discipline’s subject matter. As knowledge has continued to grow in psychology and neuroscience, this narrow focus has become increasingly clear.

The study of choice can serve to illustrate this point. An act of choosing is the pivotal element of processes, such as reasoning, decision making, and problem solving, that are central to cognitive psychology. Yet there are problems important to the understanding of human choice that have never been seriously pursued by cognitive psychologists.

Though we may be aware that we are choosing, even in the part of the process we regard as a conscious choice, there are many aspects we are never aware of. For instance, why do we consciously consider the alternatives we do, instead of the other possibilities that almost always exist? And what does “value” really mean, a question that must be answered if we want to move beyond heuristics and biases in our attempts at understanding why people regard alternative A as more valuable than alternative B?

It is now clear that simple verbal priming procedures can strongly and unconsciously affect people’s values and choices, and hence cast light on these two questions. John Bargh, a leading researcher in the field, started searching for information that could help him understand these phenomena. “When I looked into the traditional cognitive-science literature . . . I found no help at all,” writes Bargh (2006, p. 150), who believes the relevant research must focus more on behavior and its functions. Important, in his view, will be more knowledge of “the functions . . . of language, how people use it to get things done in their daily lives” (Bargh, 2006, p. 150).

Thagard (2004) offers a list of challenges to the cognitivist position. Though these challenges are problems that have been raised by nonbehaviorists, several are interesting from a behavior-analytic point of view. For instance

“the emotion challenge” is the charge that cognitive science neglects the important role of emotions. The same, it seems, can be said about behavior analysis. However, some of the phenomena often grouped under “emotion” have to do with motivation and the effects of punishment and reinforcement, about which behavior analysis has more to say. We shall delve a little deeper into this later.

Evolutionary psychologists are also among those who criticize cognitive psychology. These researchers have gathered evidence that choices in traditionally “cognitive” areas of psychology differ in ways they claim can be predicted, not by means of traditional cognitive reasoning but through an evolutionary analysis (e.g., Fiddick, 2004; Platt & Griggs, 1993).

Furthermore, strong evidence exists that all men and women are not created equal in this domain; the choices people make when they reason are affected significantly by individual differences, a field that cognitive psychology has also tended to overlook (see Stanovich & West, 2000).

Another of Thagard’s (2004) challenges is one he calls “the social challenge,” the claim that cognitivists have ignored the importance of social factors. And, indeed, there is much, and increasing, evidence that social stimuli change people’s methods of making judgments and decisions in ways that were not predicted from cognitive theory (e.g., Lerner & Tetlock, 1999).

Social psychology is among the subdisciplines of psychology that have grown increasingly cognitive since the late 1970s. Yet here, too, the shortcomings of the cognitive approach are becoming clear, primarily because psychology is left drifting in a vacuum when reference to functional relations is lacking. There is little reason to assume that most social psychologists know behavior analysis or research about learning very well. For instance, Cesario, Plaks, and Higgins (2006, p. 907) use words such as *context* and *motivation* in a discussion of what many would call contingencies of reinforcement. Yet their message is clear: “a purely cognitive approach may be insufficient. It is difficult for perception and cognition to be of much assistance to behavior if they are not context sensitive and not in the service of motivation.” This statement, of course, is equally true whether the context in question refers to the social or to the physical world. Thagard (2004) calls this “the world challenge”: the charge that cognitivists disregard the effects of the physical environment.

Also, cognitive models of choice-related processes have traditionally failed to take the nervous system into account. It is becoming increasingly clear, however, that the human brain works in ways that make some types of choice probable in certain situations, leaving others much less likely (e. g., Bechara & Damasio, 2005). As pointed out by Bechara, Damasio, and others, when people choose, different parts of the brain function in ways that strongly influence the outcome. Very important among these functions is responding to stimuli in ways that create and register emotion. Indeed, the new science of emotion constitutes one of the strongest challenges to business as usual for cognitive psychology (see, e.g., Kahneman, Diener, & Schwartz, 1999; Overskeid, 2000).

Neuroscience is not the only area outside cognitive psychology to have mapped the influence of emotion on choice. Clinical studies have yielded important knowledge regarding the way anxiety and mood disorders can bias people’s choices (e.g., Prokopcakova, 2004). Social psychologists have pointed to the role of affect in many, perhaps all, of the choice behaviors typically

studied in their field of research (e.g., Harmon-Jones, 2000; Winkielman, Berridge, & Wilbarger, 2005). The same goes for research in personality, as well as in other subspecialties, such as organizational psychology (e.g., Moxnes, 2005). Still emotion, affect, or feelings have tended not to be among the central elements in cognitive theories of choice (see Baron, 2000).

The cognitive tradition has also been faulted in other areas of research, often on grounds similar to those we have just seen in the study of choice, that is, a narrowness that excludes relevant data and theory from other fields (e.g., Neisser, 1994; Wilcox, 1992). For instance, cognitivists have traditionally studied perception with little or no mention of emotion. There is now evidence, however, that is interpreted by nonbehaviorists to mean that perception is often affected by emotion. For instance, in brain-damaged patients, visual extinction occurs less frequently for faces with angry or happy expressions than for faces with neutral expressions (Vuilleumier & Schwartz, 2001). In participants with normal vision, a fear-related stimulus appears to enhance contrast sensitivity (Phelps, Ling, & Carrasco, 2006), and stimuli that elicit positive or negative emotion are identified more easily than others (Zeelenberg, Wagenmakers, & Rotteveel, 2006).

The Behavior-Analytic Critique of Cognitive Psychology

It seems clear, then, that during the past decade or so, behaviorists have increasingly been getting company in the camp of cognitive psychology's critics. Some of the new critics share certain points of view with behavior analysts. It should still be clear, however, that the new groups that disapprove of standard cognitive psychology are not always attacking the same things that behaviorists are, or for the same reasons.

Behaviorists have directed several types of criticism against cognitive psychology (e.g., Staddon, 2001). The three points below are important and are probably those most often emphasized when behavior analysts discuss cognitivism.

1. **Mentalism.** If mentalism presupposes dualism, that is, if "the mental" is a name for something nonphysical, cognitivists holding this view have left the realm of science. This renders difficult a scientific discussion of the mental.
2. **Private causation.** Most behavior analysts doubt that private events can be full and real causes (see, e.g., Skinner, 1988).
3. **Functional relations.** Why does an organism behave the way it does? Cognitive psychologists take little interest in the processes that shape and maintain behavior.

For many years, behavior-analytic criticism and arguments have mainly fallen on deaf ears within mainstream psychology. Has anything changed? Is there any chance today that people might listen? Let us see.

Influence Versus Principles

In a free market of ideas, any person or organization that seeks customers will encounter the dilemma of attaining influence versus retaining the integrity of one's ideology. This problem becomes especially acute if one's ideas are more extreme than those of most potential consumers.

Among psychologists, behaviorists are commonly seen as extremists, and

for a long time, behaviorists have chosen ideological wholeness over influence. Given the values and priorities of behaviorism, however, it is not self-evident that this choice is the right one. Behaviorism is not an introverted philosophy. On the contrary, it has a tradition of wanting to change the world (see Skinner, 1987; Staddon, 2001). Human behavior, without doubt, is the direct cause of most of humanity's ills. Still there has been no breakthrough in attempts to solve these problems with behavior-analytic methods. This may be, of course, because the evidence behind the claims made for behavior-analytic methods is deemed insufficient. But then behaviorists' policy of ideological purity has not made communication with the outside world any easier.

I am not advocating that behaviorists give up their beliefs to become more palatable. Empirical facts and rational argument alone should decide beliefs. However, people tend to want others to believe the same things they do themselves, which is often the reason we engage in discussion. Let us assume behaviorists also want others to think more like they do. Now that cognitivism is weakened, how much can behaviorists accomplish in this direction and what can they do to achieve it?

A Hard Sell

Let us concentrate on the basic issues. Cognitive psychologists believe they conduct their research and draw conclusions in accordance with scientific standards, but radical behaviorists have radically different ideas about what it means to practice science. To convert mainstream psychologists, these behaviorists need to explain why their view of theory and empirical research is better. This could be a hard sell. Good reasons may exist, for instance, for other researchers to employ more often the within-subject methodology associated with the Skinnerian tradition (Sidman, 1960). It may be more difficult to persuade mainstream psychologists that the behavior-analytic view of the role of theory is better.

Some cognitive psychologists seem to view "mind" or "the mental" as something nonphysical. Cognitive psychology, however, has never been based on that view (see Catania, 1994). Uttal (2004) has argued, however, that much of cognitive psychology is built on *cryptodualism*, the idea that mind and brain can be thought of as independent entities. He may well be right. It is still not entirely clear, however, that getting rid of all traces of dualism will in itself affect the scientific behavior of mainstream cognitive researchers in any important way. An experimental researcher may be Muslim, Jew, or Sikh without this influencing his or her choice of problem and experimental method. By the same token, as long as one is not hypothesizing immaterial causes, a belief in dualism (or cryptodualism) will not necessarily affect one's experimental or theoretical work in psychology. Other psychologists differ in their views on these matters and still manage to work together. Most behavior analysts have preferred to stay apart.

An important related point is the extent to which the inference of cognitive processes from behavior is possible. Following the Skinnerian tradition, behavior analysts are skeptical of theory that "appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions" (Skinner, 1950, p. 193). However, as pointed out by Staddon (2004), among others, some of the most important advances in science have taken place by postulating "events

taking place somewhere else [or] at some other level of observation.” Hence whatever validity the Skinnerian view of theory may have, it seems unlikely that mainstream psychologists can be easily persuaded to stop referring to events that cannot be readily observed. This is especially so, as there is no clear evidence that prediction and control of behavior is made easier by abstaining from theorizing about processes that are difficult to observe (see Overskeid, 2006).

Whatever their private metaphysical assumptions, no mainstream psychologist who wants to be taken seriously will claim that psychological phenomena are caused by nonphysical events. Hence very few mainstream psychologists are mentalists in this sense. And though behaviorists may call them cryptodualists, it is difficult to attack cognitive psychologists on this point, for they will tend to agree with the attacker.

A Cognitive Taboo

Revolutions can lead to good, as well as bad. Among the less fortunate consequences is often a rejection of the old ways, even if they might have been of help in advancing the goals of the new rulers. Getting rid of behaviorist assumptions and practices was an avowed goal of the cognitive revolution. Cognitivism never had a rule against the study of functional relations of an organism's behavior. Yet in psychology, as in other fields that have undergone revolutions, certain prerevolutionary practices too strongly connected to the ancien régime also became taboo, even if good arguments for scrapping them were lacking. Many former colonies abolished free markets, the Islamic revolutionaries of Iran stopped wearing neckties,* and cognitive psychologists stopped asking why people behave.

The 1990s were in many ways the final golden years of cognitive psychology. Still traces of self-criticism could now be found in textbooks. Medin and Ross (1992, p. 44) wrote, “Cognitive psychologists have been criticized (perhaps quite properly). . . . Often this criticism has taken the form of pointing to the need to consider our human environment . . . in conducting and evaluating experiments.” Whatever effect such gentle criticism may have had, it appears not to have resulted in a wish to understand the effects of the environment on behavior. After discussing an experiment that indicated the behavior of human beings is sometimes rule-governed and hence not controlled by contingencies directly, Medin and Ross (p. 83) state unequivocally, “This experiment is another source of evidence that reinforcement does not operate in a direct and automatic manner to strengthen response tendencies.” As Medin and Ross were writing, clear evidence existed, however, that this is exactly how reinforcement operates—in humans, too—when behavior is contingency-shaped (e.g., Baxter & Schlinger, 1990; Svartdal, 1989). Though neuroscience has added to this evidence (see Montague, 2006), it has not been incorporated into more recent textbooks of cognitive psychology. Instead, such books often do not discuss reinforcement at all (e.g., Braisby & Gellatly, 2005; Matlin, 2005).

Cognitive psychologists should not have gone about their work as if the contingencies of reinforcement do not exist. The readers of this journal can no doubt think of many good reasons why. Of chief importance to cognitivists themselves, however, should be the fact that not studying functional relations

* A case may indeed exist for getting rid of neckties.

has made it harder to reach the goal most cognitive psychologists can agree on: to understand “all processes by which the sensory input is transformed, reduced, elaborated, stored, recovered, and used” (Neisser, 1967, p. 4). Let us see why.

To begin with, reinforcers and discriminative stimuli are sensory input—the input that shapes, occasions, or elicits behavior. This input thus guides the actions of a behaving organism, and according to cognitive theory, the sensory input is transformed and elaborated, before being used in behavior. Hence by not studying functional relations, cognitivists have shirked an important part of the job they gave themselves: They have not tried to understand “all processes by which the sensory input is transformed . . . and used.” What is more, however, sensory input from the contingencies of reinforcement is arguably the most important input an organism receives. Without feedback resulting from its own responding, without discriminative and eliciting stimuli, no organism can solve problems or, indeed, survive. Hence it seems that few, if any, of cognitive psychology’s main areas can be clearly elucidated without an analysis of functional relations.

From a behavior-analytic point of view, cognition is behavior. Most human behavior is assumed to be amenable to operant conditioning. Seen from this vantage point, it is not surprising that cognitive processes seem sensitive to the effects of reinforcement (see Overskeid, 2000). Behavior analysts often say cognition cannot explain behavior; it is just more behavior to be explained. However, with some exceptions (see, e.g., Chase & Watson, 2004; McHugh, Barnes-Holmes, Barnes-Holmes, & Stewart, 2006; Skinner, 1969; Valdivia, Luciano, & Molina, 2006; White, McCarthy, & Fantino, 1989) and perhaps to their detriment, behavior analysts have not put much work into explaining the behavior known as cognition. Cognitive psychologists, on the other hand, have made a living doing so, but as we have seen, their explanations have suffered from a fundamental weakness: They have lacked reference to the functional relations of which cognition is a part.

Cognitivism and Neuroscience

Behavior analysts have been uneasy about explanations in terms of neuroscience, though never with the same amount of skepticism as that displayed toward cognitive theory. Skinner (1963, p. 953) concluded, “An adequate science of behavior must consider events taking place within the skin of the organism, not as physiological mediators of behavior, but as part of behavior itself.”

Skinner criticized authors who developed conceptual and, he felt, premature theories of nervous activity to explain behavior (see, e.g., Delprato & Midgley, 1992). Skinner seems nevertheless to have held two assumptions quite consistently: (a) Behavior can in principle be explained by means of an elucidation of nervous processes, although (b) such an explanation will not replace behavior analysis but will instead fill out the picture painted by that science. In the words of Skinner (1974, pp. 236-237),

Something is done today which affects the behavior of the organism tomorrow. No matter how clearly that fact can be established, a step is missing, and we must wait for the physiologist to supply it. He will be able to show how an organism is changed when exposed to contingencies of reinforcement and why the changed

organism then behaves in a different way, possibly at a much later date. What he discovers cannot invalidate the laws of a science of behavior, but it will make the picture of human action more nearly complete.

It seems pretty clear that neuroscientists today are in the process of supplying Skinner's missing step. For instance, it has long been recognized that reinforcement may affect people's reports of perception and, some authors have assumed, perception itself (e.g., Solley & Murphy, 1960). Today, behavioral and neural effects of reinforcement can be measured simultaneously during performance of perceptual tasks (e.g., Seifert, Naumann, Hewig, Hagemann, & Bartussek, 2006).

Another type of research that should interest cognitivists, as well as behavior analysts, is about the correlation of brain events with the response selection process that results from operant learning (e.g., Koene & Hasselmo, 2005). Perhaps even more interesting, however, is that researchers such as Foster and Wilson (2006) and Loewenstein and Seung (2006) may be getting close to showing exactly "how an organism is changed when exposed to contingencies of reinforcement." Loewenstein and Seung have come a long way in describing the neural processes of which operant matching is an outcome. By measuring activity in rat neurons, Foster and Wilson have demonstrated how hippocampal activity during a learning episode is replayed backwards soon after learning. The hippocampus has long been known to play an important role in both human and rodent learning. Foster and Wilson speculate that reverse replay of learning episodes is central to remembering a connection between response and consequence.

I could continue, but suffice it to say that work of this kind constitutes one of the clearest threats to business as usual for cognitivism. In many important areas of cognitive psychology, neuroscientists are showing the importance of consequences in shaping the behavior that takes place within the skin, which, thanks to today's technology, is more easily measurable than ever. In related areas such as psychopharmacology, of course, researchers have long made use of operant methods to measure the effects of drugs on motor behavior (e.g., Arntzen, Sagvolden, & Slåtta 1993; Skinner & Heron, 1937). Any cognitivist willing to see will notice that today's neuroscience is vigorously calling attention to the importance of functional relations.

Movement and Change

Is there any reason, one might ask, for moving behavior analysis closer to the mainstream? Why not leave things the way they are and continue working in splendid isolation? When all is said and done, this is an ethical as well as a practical question. Influencing the scientific community and the world at large depends of course on the will to do so, as well as the means to do it effectively.

Ethics first: We noted that behaviorism is a school of thought with a tradition of wanting to change the world (see Skinner, 1987; Staddon, 2001). Let us assume, then, that *ceteris paribus*, many behavior analysts would like their school of thought to be less isolated, presumably because more people might then have assumptions that are more often true and lives that are better.

Then the practical side: Many authors have discussed the relationship

between behavior analysis and mainstream psychology (e.g., Henriques, 2003; Kimble, 1989; Lee, 1989; Staats, 1980). The points of view have varied, and proposals and plans for greater unity in psychology have been presented. Nothing very much seems to have happened. If coverage in textbooks is anything to go by, behavior analysis today is seen as irrelevant to other areas of psychology (e.g., Braisby & Gellatly, 2005; Brown, 2006; Matlin, 2005). Furthermore, the impact factors of behavior-analytic flagship journals such as the *Journal of the Experimental Analysis of Behavior* and the *Journal of Applied Behavior Analysis* are relatively low. During the last five years for which data are available at the time of writing (2001–2005), they have been unchanged or descending (ISI Web of Knowledge, 2006). Is there any reason to think that concrete integration attempts, were they to take place, would yield results?

Indeed, there are at least three reasons to think that they might. First, in an intriguing article, Morris (2003) discusses several “programs of direct action,” that is, approaches to various domains of psychology that do not constitute a unified body of work but still have important things in common: They are nonmediational and nonrepresentational, they focus on functional relations, and they exist in areas that are traditionally thought of as “cognitive,” such as perception (Gibson, 1979) and memory (Watkins, 1990), as well as in other fields. Behavior analysis is of course also such an approach, and Morris convincingly shows that though the other approaches he discusses are different in many ways, clear possibilities exist for integration. Morris’s article illustrates that a psychology studying cognition need not be based on the tenets of cognitivism, foremost among them the claim that people have mental states, the manipulation of which can be described in terms of rules or algorithms (see Still & Costall, 1991, for further illustration, and Marr, 1982, for a paradigmatic example of cognitivist explanation).

Furthermore, a movement toward integration has demonstrably been going on for some time in psychology, most notably among the domains of mainstream cognitive psychology, neuroscience, and social psychology. Cognitive psychology used to produce theories of cognition that were independent of any knowledge of brain functioning, often because the pertinent knowledge did not exist. Today, many cognitive psychologists see the relevance of neuroscience, and increasingly, neuroscience creates constraints that cognitive psychologists operate within. There is some amount of symmetry in this relation, as neuroscientists often test hypotheses based on cognitive research, and the field of cognitive neuroscience is thriving (see Gazzaniga, Ivry, & Mangun, 2002; Matlin, 2005).

Finally, research in several fields, such as reasoning, judgment, and decision making, are published in cognitive journals, as well as in those specializing in social psychology (e.g., Mussweiler, 2001; Teigen & Jørgensen, 2005). At the same time, social psychology inspires an increasing amount of research in neuroscience and vice versa (e.g., Dunbar, 2003; Norris, Chen, Zhu, Small, & Cacioppo, 2004). Indeed, “social neuroscience” is the name of a new and rapidly growing field (Decety & Keenan, 2005).

It seems clear, in other words, that previously important demarcation lines between psychological subdisciplines have become increasingly permeable, resulting in increased integration and new possibilities. As we have seen, this state of flux exists while the position of cognitivism is weakened, and consensus grows that cognitive psychology must change to be relevant.

Knowledge Lost

When cognitivists revolted against behaviorism, they threw out the behaviorists' knowledge of the functional relations of behavior: the way the activity of humans and animals is shaped by its consequences and occasioned or elicited by antecedent stimuli. With this knowledge went a way of thinking that always asks why a given behavior occurs, that is, how it was selected and maintained.

We also saw that understanding an organism's interaction with its environment was central to functionalism. Though the ideals of functionalism have in many ways had a renaissance in modern psychology, the dominant position of cognitive psychology seems to have precluded a rediscovery of functional relations.

Nevertheless, even if mainstream psychology has been blind to these relations for a long time, an increasing number of findings are forcing researchers to start looking for them. Above we saw some of Thagard's (2004) challenges to cognitive psychology. We saw Bargh finding the traditional cognitive literature unhelpful, wanting instead to study "the functions . . . of language, how people use it to get things done in their daily lives" (Bargh, 2006, p. 150). We also saw Cesario et al. (2006) suspecting that a purely cognitive approach is insufficient, pointing out that to be of any use, perception and cognition must be in service to motivation and sensitive to context.

It is a significant development when researchers with no background in behaviorist thinking rediscover the interplay between behavior and its context. Equally important, however, though perhaps less obviously welcome for orthodox behavior analysts, is another rediscovery, namely that of feelings and emotions. The growth of this field has been driven by groups such as social psychologists (e.g., Winkielman et al., 2005), neuroscientists (e.g., Bechara & Damasio, 2005), and even economists (e.g., Thaler, 1992). The research on affect is very relevant to understanding cognition (see, e.g., Dunn, Dalgleish, & Lawrence, 2006). Still cognitive psychologists have not been pioneering this area of investigation, in spite of interesting and relatively early contributions from researchers such as Kahneman (see, e.g., Kahneman et al., 1999).

Why, then, is affect relevant to the present discussion? The answer, of course, lies in the close connections among affect, reinforcement, and punishment (see Skinner, 1986) and in the renewed interest in affect that has led many mainstream psychologists and neuroscientists to start studying events that elicit affective responding. This is not the place to debate whether consequences and discriminative stimuli are indeed dependent on affect to have an effect on behavior. Many researchers, however, are beginning to think that this dependence may often, or perhaps always, be the case and that the mediation of reinforcement, punishment, and motivation may be the primary role of affect (e.g., Bechara, 2005; Overskeid, 2000; Panksepp, 2004). As we have seen, mainstream researchers seek knowledge about motivation and the effect of context and the environment, as well as the functions of language. Yet the literature suggests that the detailed knowledge of learning that exists among behavior analysts is virtually unknown among most of those in the mainstream.

A Hungarian Doctor

Ignaz Philipp Semmelweis (1818–1865) was a pioneer in introducing aseptic techniques, thus saving many lives in the hospitals where he worked. Still it seems he lacked the will to publish his findings at first, and when he did publish them, he did so in a way that unnecessarily provoked the medical establishment. Semmelweis is described as being dogmatic and arrogant (see Nuland, 2003).

Semmelweis's lack of interest in scientific communication and his ineffectual way of practicing it must have cost lives, because patients had to wait for more efficient communicators, such as Pasteur and Lister, before asepsis—the creation and preservation of a bacterium-free environment—became a universal principle of surgery. Semmelweis's inefficient communication style probably also carried costs for himself. He died in a mental asylum, apparently after being beaten up by guards (Nuland, 2003).

Differences exist between Ignaz Semmelweis and the behavior-analytic movement. However, there are also similarities: Semmelweis communicated well enough with those closest to him, and in his own country of Hungary, his ideas were accepted. Behavior analysts also seem to be communicating well among themselves but may perhaps lack the necessary will and means to reach others. Some would say that part of the reason lies in dogmatic views. (Can we be absolutely certain, for instance, that regarding private events as initiators of behavior could never do any good? Overskeid, 2006, discusses this question.)

Still, after being declared dead, Semmelweis's ideas came back to win the day. They did so, no doubt, because other authors with similar views communicated more efficiently than did Semmelweis—but also because medicine had developed. Other researchers had made observations similar to Semmelweis's, Pasteur had developed a relevant theory, and the medical community was thus more receptive to the thought of aseptic practice than it had earlier been.

By the same token, we have seen that an increasing number of mainstream psychologists appreciate the necessity of understanding the organism's interplay with its environment. Developments in evolutionary psychology, neuroscience, social psychology, emotion research, and other fields, as well as criticism from within, have made today's mainstream psychology ready for this line of thought; thus cognitive psychology is under pressure.

Where to Start

Today, then, seems a better day than most for behavior analysts to start finding their way back into psychology's mainstream. The weaknesses of cognitivism are being exposed, and nonbehaviorists are beginning to see that functional relations matter.

There are few indications, however, that mainstream psychology is ripe for the basic assumptions of behavior analysis in the same way that it is becoming ready for behavior analysts' empirical knowledge. It seems behavior analysts must decide, then, whether it is ethically and practically right for them to offer their knowledge to mainstream psychology without having a chance to sell their ideology.

The knowledge of functional relations has potential in today's psychology. This is not to say, however, that empirical behavior-analytic research in other areas could not contribute usefully to general psychology. Fields such as rule-governed behavior and instructional control (e.g., Martinez & Tamayo, 2005; Podlesnik & Chase, 2006), equivalence classes (e.g., Dixon, Rehfeldt, Zlomke, & Robinson, 2006; Smeets, Barnes-Holmes, & Striefel, 2006), and relational frame theory (Hayes, Barnes-Holmes, & Roche, 2001) are examples that easily come to mind. Authors working within the framework of relational frame theory are among those already making contact with the mainstream literature in interesting ways (e.g., McHugh et al., 2006).

Twenty years ago, Catania (1987) reviewed Bowler's (1983) book *The Eclipse of Darwinism*. Bowler describes the challenges Darwin's theory faced from other accounts of evolution a century ago, challenges so serious that Darwinism was said to be dying. At the time of Catania's writing, he believed it was behavior analysis that was being eclipsed. He expected this decline to be temporary, however, and his tone was hopeful. Catania asked two questions: first, "What kind of events will lead to the reemergence [of behavior analysis]?" (p. 255).

In pointing out opportunities for a reemergence of core behavior-analytic knowledge, the present article has given one possible answer to this question while describing a compromise that may be as good as it gets: No current trend indicates that mainstream psychologists are moving in a behaviorist direction. This observation should not be overly surprising. Behavior analysts have, after all, never shown convincingly that following their ideology will lead to more successful science. They have documented in detail, however, many possible relations among behavior, its antecedents, and its consequences. Now the world may be ready to rediscover that knowledge.

"When it begins to happen, how will we know?" was Catania's (1987, p. 255) second question regarding the reemergence of behavior analysis. The answer to that question, I think, is up to behavior analysts themselves, since to a large extent they must be the ones to decide whether and when it happens. We will know when behavior analysts see that mainstream psychologists are now feeling a need for something behavior analysts can offer. Seeing this is not sufficient, however: Behavior analysts must communicate with mainstream researchers. They must tell them why behavior-analytic knowledge is relevant to the concerns of people outside the behavior-analytic community.

An important way of communicating with mainstream psychologists is of course to publish in their journals, which can be achieved by performing research in fields deemed important by nonbehaviorists. A good way to start is to build on Skinner's theoretical work and interpretations (e.g., Skinner, 1953, 1957, 1969). In this gold mine of conjectures and observations, behavior analysts and mainstream psychologists should have no problem finding theoretical conjectures worthy of investigation. Indeed, there is evidence that when behavior-analytic and cognitivist researchers work on the same problems, they may have more in common than they tend to see themselves. This seems to be the case in research about contingency-shaped versus rule-governed behavior (or nonarticulable versus articulable skills, which is one of many sets of terms in the cognitive literature; see Overskeid, 1994, 1995). Furthermore, it has long ago been pointed out from the cognitive camp that Skinner's (1969) operant analysis of problem solving was interesting, relevant, and well ahead of its time (see Hunt, 1984).

To take just one more example, Skinner claims (1969, p. 125) that a man stating his purpose in acting in a given way may be constructing a “contemporary surrogate of future consequences” that will affect subsequent behavior. The hypothesis that people construct contemporary surrogates of future consequences is central to influential theories of human choice, theories developed more by neuroscientists perhaps than by pure psychologists (e.g., Bechara & Damasio, 2005; Nauta, 1971). If behavior analysts were to build on Skinner’s assumption and do research in this field, they would be working in an area seen as central to today’s neuroscience and cognitive psychology.

As Skinner’s work so clearly illustrates, all psychological phenomena are the subject matter of behavior analysis. Hence all problems in psychology are the problems of behavior analysis, and many of them would probably move closer to a solution if they were subjected to an operant analysis, theoretically as well as empirically.

Working on problems that other psychologists recognize as important is a good start. Cooperating across ideological border lines would be even smarter. Behaviorists and cognitivists have been bickering for a long time. Kahneman (2002) says he has been appalled by “the absurdly adversarial nature” of some scientific exchanges, “in which hardly anyone ever admits an error or acknowledges learning anything from the other.” Instead, Kahneman suggests the use of “adversarial collaboration,” which

involves a good-faith effort to conduct debates by carrying out joint research—in some cases there may be a need for an agreed arbiter to lead the project and collect the data. Because there is no expectation of the contestants reaching complete agreement at the end of the exercise, adversarial collaborations will usually lead to an unusual type of joint publication, in which disagreements are laid out as part of a jointly authored paper.

Kahneman, as well as others (Latham, Erez, & Locke, 1988), have initiated such collaborations and found them much more fruitful than traditional forms of debate. There is no reason why behavior analysts cannot do the same.

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The position of cognitivism is weaker than it used to be. Also, many researchers now see the need to understand functional relations between organisms and their environment. Not in a long time have behavior analysts had so clear an opportunity to influence psychology at large.

We communicate to influence the behavior of others. Effective communication depends on the ability to predict the response of those one is trying to influence: What will affect their behavior, what will not? I have argued that a message based on empirical knowledge will attain more than one based on ideology, but whether this is correct is in itself an empirical question. The point now should be to act, ultimately because a world that knows behavior analysis could be a better world.

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