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Behaviorism from the Psychiatric Perspective

2-1. Introduction

Biological psychiatry says there is a mind but, as a matter of contingent fact, it is reducible and ultimately identical with the brain. The very large group of theories lumped together as psychodynamics takes an opposing view. They say there is a causally significant mind that cannot be reduced to its material substrate yet, even though it is unobservable, we can still talk meaningfully about it. From the psychodynamic point of view, the nature of mind may be a metaphysical dilemma, but talking about the mind in action is not meaningless.

The simplest type of psychodynamic theory is what Dennett has called “folk psychology.” This is what you or I understood about the mind before we became sophisticates, or what our grandparents believed all their lives. Simply, it means a model of mind in which the non-stop, three-ring circus inside my head, where I can “see,” “feel” and “hear” all manner of delights and horrors, this internal video set is where I really live, where I experience and decide all that counts in my life. You might see my body, but you can’t see the real me. I have an accountable public life and a private, inner life where I can get away with all sorts of things up to and including the odd murder. “I” am an insubstantial creature inside my head.

But I am not just a passive observer of my body getting on with its business, I actually run my body. I decide things, I put my plans into action or change my mind, decide to be good or bad, do what feels good or (less often) what is honorable, work out what I should have said, plot vengeance, and so on. Actually, I have quite a full life here inside my head and even though I can’t see inside yours, I’m sure you do, too.

Unfortunately, what seems clear in daily life isn’t quite enough in the more extreme cases. Do we believe the man who says he didn’t know the gun was loaded? Or if he knew it was loaded, that he meant no harm? What should we say to the fellow who insists that Martians talk to him telepathically or the chap who goes to water every time he sees a cockroach? While we struggle with these and many similar cases, attempts to render my inner experience into a rational, non-question-begging account of “minds in general” quickly run into enormous difficulties. It soon becomes apparent that, as long as we talk in metaphor or in vague generalities, we can limp along, fairly sure that we understand each other, but anything more precise becomes mired in qualifications and counter-examples.

In the mid-nineteenth century, empirical enquiries into the nature and function of mind consisted mainly of introspection, attempts to sneak up on the mind and catch it in the process of “minding”, as it were. Highly intelligent people sat around, trying to apprehend the nature of the experience called

“seeing an orange patch” or “hearing middle C”. After this had been going on for quite some time, it became apparent that “armchair philosophizing” (actually armchair psychologizing) wasn’t going anywhere, and a few radicals started to look for something more reliable. They decided it was necessary to abandon all talk of minds as mere froth and bubble, as relics of the age of superstition whose only function is to distract serious researchers from their proper paths. This stern approach dominated psychology for the greater part of the twentieth century, only recently slipping into history. The principle underlying this approach is quite simple: Science is about observables. Any theory that goes beyond the directly observable has to be firmly anchored in careful, replicable measurements. But since the mind is unmeasurable in principle, then it is by definition excluded from the realm of scientific enquiry.

2-2. Early Behaviorism

Reliability was first claimed by Wilhelm Wundt, a nineteenth century German philosopher who is credited with laying the foundations of psychology as a separate, empirical discipline. Wundt first graduated in medicine but soon gravitated to physiology, studying and later lecturing under the great Helmholtz. From there, he became a professor of philosophy, which led to his interest in what became psychology. As a philosopher, he accepted that the immediate contents of consciousness comprised the subject matter of psychology but as a physiologist, he had a radically different approach to its study. Abandoning introspectionism, Wundt applied the conventional methods of physiology, as Helmholtz devised them, to the study of mental processes. This was strikingly original and he was largely responsible for establishing experimental psychology as a specialty in its own right. Later in his career, he published extensive researches into what are now called cognitive psychology and social psychology. In fact, Wundt was probably the first to use the term “folk psychology.”

Of course, Wundt did not live in an intellectual vacuum. By the mid-1870s, empirical psychology was waiting to happen. The physical and biological sciences were racing ahead and it was only a matter of time before the ethos that permitted them, and which they in turn validated, spread to encompass all rational enquiries. Schools of psychology were soon flourishing in major academic centers in the Western world. In Britain, under the influence of Francis Galton, a nephew of Charles Darwin, psychology was very mathematical, focusing on intellectual assessment, the genetics of personality and such like. In the United States, a similar revolution took place, profoundly influenced by Wundt’s students and later by the experimentalist John B. Watson, who publicized the work of the Russian researcher, Ivan Pavlov. But until Watson, consciousness was still very much the proper domain of psychology.

Watson objected very strongly to the mentalist element in the evolving German tradition. He was unrepentant on the direction he expected the new psychology to take: “I can state my position here no better than by saying that I should like to bring my students up in the same ignorance of (the mind-body problem) as one finds among the students of other branches of science” [1, p166]. All considerations of mind and mental contents, of consciousness and introspection, were impediments to progress that “bound (psychology) hand

and foot”. Observable behavior was all that counted; all talk of unobservable intervening variables, such as minds, was doomed to sterility. Very early, he adopted a rigid biologism: “The findings of psychology become the functional correlates of structure and lend themselves to explanation in physico-chemical terms.” And the goal of all this was “...to learn general and particular methods by which I may control behavior.” He believed it would be possible to write such a psychology in just a few years.

Watson had a particularly confident, even abrasive, style of writing which, coupled with the confused results of fifty years of introspectionist psychology, had a strong appeal in the newly assertive scientific atmosphere after the First World War. Introspectionist psychology abruptly collapsed, to be replaced by an aggressively objective, anti-mentalist science of behavior. Psychologists saw themselves as flag-bearers in the continuing scientific revolution aimed at dragging Man from his special seat just a little below the angels. They were content to use the mind to investigate behavior, much as chemists used it to investigate materials, but never to investigate itself. Consciousness, they believed, was a red herring, even a deadly trap, and all mention of it was soon swept away.

Watson was not entirely an intellectual Vandal; he knew that if he wanted to demolish a well-established academic field, he had to offer something else in its place. What he had in mind was building a new, general psychology based on Pavlov’s concept of the conditioned reflex. Whether this was his intention, or whether he conceived of conditioning as simply an investigatory method isn’t entirely clear but others certainly tried. Their efforts were not appreciated by the great Pavlov himself who argued against this ambition. Interestingly, just like Watson, Pavlov was an early and fervid supporter of Mind-Brain Identity Theory: “...uniting or identifying the physiological with the psychological, the subjective with the objective, which I am convinced comprises the most important present-day scientific undertaking” [2].

Twenty-five years into the new century, psychology had become behaviorism, the study of observable behavior, an idea that exerted a profound effect upon many areas of human endeavor. For the next sixty years, human studies were dominated by the notion that, if the mind exists, it is rendered irrelevant by the fact that it cannot be studied rationally. Now this opinion isn’t entirely correct. Strictly speaking, the early behaviorists were saying: “The mind can’t be studied rationally by our methods.” This is not the same thing as saying it can’t be studied by any methods at all. So for sixty years, human psychology painted itself into an unproductive corner, all the while spreading disdain for anything that did not reach its particular and largely self-proclaimed standards of science.

2–3. Skinner’s Radical Behaviorism

The most far-reaching and thorough-going attempt to construct a non-mentalist human psychology was Skinner’s Radical Behaviorism. Burrhus Frederic Skinner was an American psychologist raised in the “Brave New World” of early twentieth century scientific psychology. He had an exceptionally long career, published profusely, and was utterly dismissive of anybody’s efforts but his own. His disdain for his opponents (and there were many) was

so complete that he rarely if ever bothered to answer their objections to his theories. Late in his life, people started to wonder whether he was capable of considering that he could be wrong.

Skinner wrote in a particular, objective style, rigidly eliminating all mentalist references. He spoke of “organisms” that “emit behaviors” which are then “reinforced” by their “environmental consequences.” A behavior acting or operating upon the environment is an “operant.” Because operants can be reinforced, they can be made to dominate the organism’s behavior or eliminated, depending on their effects. Operants can be conditioned by their consequences, so that psychology’s ultimate goal of “shaping and maintaining behavior” (i.e. controlling it, in Watson’s blunt language) comes within reach.

To take a common example, a baby gurgles and grunts. By their happy responses, the doting parents encourage their infant to use closer and closer approximations of real words. Language is thus acquired without the aid of unseen and unknowable “intervening variables” (minds). Correct use of language is maintained by, essentially, the fact that speaking properly gets us what we want (i.e. it is positively reinforcing). For a radical behaviorist, the reinforcing environment (the verbal community) shapes and maintains language by contingent reinforcement of the organism’s verbal operants. A Skinnerian account of speech does not need a mind to learn the language.

Skinner’s psychology was profoundly influential, especially in the United States. Generations of psychologists were trained in the theory and practice of radical behaviorism. An enormous research program developed, and Skinnerian methods of behavior management were applied in fields ranging from schools to prisons. However, and despite his vast output, there are problems in pinning down his views as, in his successive publications, Skinner tended to supersede his work rather than to revise it. Thus, he could say at one stage that the mind was irrelevant, later that it existed but couldn’t be studied, and later still that it was quite relevant but was nothing special anyway. However, not long before he retired as one of America’s most distinguished scientists, he published several works that have to stand as the definitive statements of his position. In *Beyond Freedom and Dignity* [3], he wrote a popularized version of his otherwise fairly opaque theory, and in *About Behaviorism* [4], he outlined his philosophy of behaviorism.

From about the late 1950s onwards, his ideas were subjected to increasingly critical analysis. In 1957, Scriven [5] dissected Skinner’s assertion that a true account of human behavior must be essentially (or totally) atheoretical. This was the basis, Scriven argued, for Skinner’s intense opposition to Freudian psychoanalysis. Scriven concluded that Skinnerian radical behaviorism was itself definitely not atheoretical: “...Skinner has elevated the relatively atheoretical nature of his approach into a sterile purity that his (own) approach fortunately lacks” (p94). An essential element in the Skinnerian program had failed. Radical behaviorism was therefore open to attack on theoretical grounds, which Skinner had previously denied (and, typically, he continued to deny).

A more telling critique of the radical behaviorist program followed publication of Skinner’s *Verbal Behavior* in 1957. The psycholinguist and philosopher, Noam Chomsky [6], reviewed the psychologist’s account of language acquisition, including the claim that human verbal behavior can be

predicted and controlled “by observing and manipulating the physical environment of the speaker” (p 26). “(Skinner) confidently and repeatedly voices his claim to have demonstrated that the contribution of the speaker is quite trivial and elementary, and that precise prediction of verbal behavior involves only specification of the few external factors that he has isolated experimentally with lower organisms” (p 27-28).

Chomsky’s critique was brief (about thirty pages to review Skinner’s book of nearly 500 pages), precisely targeted and devastating. In a series of carefully-marshaled points, he showed that all the basic premises of the radical behaviorist approach to language were devoid of scientific content. One by one, he took Skinner’s major concepts, showing that “...if we take his terms in their literal meaning, the description covers almost no aspect of verbal behavior, and if we take them metaphorically, the description offers no improvement over various traditional (folk) formulations” (p 54).

Chomsky argued that the scientific ethos in radical behaviorism was illusory, that terms such as stimulus, response, operant, reinforcement, control, etc., were hollow, merely “...the illusion of a rigorous scientific theory with a very broad scope (p 30) ... Skinner’s claim that his system ... permits the practical control of verbal behavior is quite false” (p32). Skinner’s every effort failed. He could not define stimulus with any precision, “stimulus control” lacked the sense he claimed for it, his law of reinforcement was tautological, “...the term reinforcement has a purely ritual function” (p38), and so on. This led Chomsky to characterize the psychologist’s work as “hopelessly premature,” “empty”, etc. He concluded: “If it were true in any deep sense that the basic processes in language are well-understood and free of species restrictions, it would be extremely odd that language is limited to man” (p30).

His review was a small masterpiece; forty years on, nobody has improved on it and nor, for that matter, did Skinner. He never revised his book to take account of Chomsky’s objections. Since then, and despite intense efforts, the Skinnerian research program on “verbal behavior” has largely faded from view. Towards the end of the 1970s, a number of critiques of radical behaviorism appeared, including those by a psychological methodologist, Brian MacKenzie, and by a philosopher, Daniel Dennett.

MacKenzie’s work [7] was another small wonder, a precisely detailed analysis of psychological epistemology. After an exhaustive and demanding review, he concluded that behaviorism had given us only “...some portion of the tools appropriate for building a science—but not the science itself ...” (p 170). It would not be possible to summarize his work beyond this, his last sentence, but Skinnerian radical behaviorism was particularly criticized for its “systematic pretensions” (p 163) and its total failure to deliver on any of its promises. After the intellectual edifices (of Skinner’s psychology) had crumbled, all that was left was a set of skills that were not unique to radical behaviorism. Simply speaking, Skinnerians described and controlled animal behavior somewhat better than other psychologists and lion tamers, but not differently, and certainly not radically so.

Dennett’s criticism of Skinnerian radical behaviorism [8] is articulate and equally incisive. In his opinion, Skinner made several substantial mistakes in his theory. Firstly, he mistakenly supposed that all mentalism is necessarily supernatural and thus no better than superstition. He (Skinner) therefore de-

terminated to sweep all mentalist explanations from his theory, but on this crucial first step, he failed. We cannot translate mentalist accounts of human behavior into non-mentalist statements. Skinner's work is in fact a good source of failed attempts to do this. Dennett was able to show that a non-magical account of that most mentalist of concepts, intelligence, was indeed possible, meaning that a major plank in the rationale for radical behaviorism collapsed.

Secondly, Dennett argued that Skinner made a mistake in generalizing the results of his laboratory experiments on rats and pigeons to humans. It is one thing to place lower animals with limited means of dealing with the environment in a highly restricted environment, and then announce that their behavior is necessarily under the control of a few very simple principles. It is something else again to assume that those principles necessarily govern all human behavior under all possible circumstances: "Since all the explanations he has so far come up with have been of the unmasking variety (pigeons, it turns out, do not have either freedom or dignity), Skinner might be forgiven for supposing that all explanations in psychology, including all explanations of human behavior, must be similarly unmasking ... Pigeons do not exhibit very interesting or novel behavior, but human beings do" [8, p 66-7]. Dennett's argument can be summarized as saying that genuinely intelligent creatures can always mimic the behavior of less-endowed animals. Therefore, the necessary first step for the Skinnerian program was to prove that humans don't have genuine intelligence, rather than simply assuming it to be the case.

This leads to a well-known contradiction for behaviorists, which is that if they argue that humans don't have intelligence, or creativity, or motives, then they must also believe it of themselves. The philosopher Alfred Ayer once said that to be a behaviorist is to pretend to be anesthetized from the neck up. If, as Skinner argued, scientific creativity is just a matter of being in the right environment, why did he accept all his honors and awards? Kline [9] has summarized this criticism pungently: "If we only do what we have been reinforced to do, then presumably Skinner, also being subject to schedules of reinforcement, writes what he writes simply because he has been so reinforced. There is thus no reason to think that (Skinnerian psychology) is true, or ... that Skinner believes it to be true. Hence, why should we bother to examine it?" A genuinely non-mentalist theory of behavior cannot come to grips with such quintessentially mentalist concepts as truth and falsity. Only thinking creatures can appreciate errors—and falsehoods.

This leads to another of Dennett's criticisms, which is the notion of the "undischarged homunculus" in Skinner's theory. Skinner claimed he had eradicated the need for mentalist explanations but, as a matter of logic, he had not. All he had done was shift them around, from an homunculus or "little man" in the head to another man hidden in the environment. This led him to argue [3] that there is no such thing as a creative artist. What we think of as a creative artist, he insisted, is merely an artist skilled at arranging a "creativity-inducing environment." But who decides what constitutes a creativity-inducing environment? The artist, presumably, so Skinner merely shifted the problem from one of explaining creativity to one of explaining how people decide what will induce creativity (presumably a fairly creative exercise in its own right).

There is an “undischarged mentalist debt” or homunculus lurking in every one of Skinner’s allegedly neutral environments. In another context, Scriven noted the same “covert and unjustifiable substantive implications” buried in an avowedly non-mentalist behaviorism: “I remember the glee with which I discovered that nobody actually produces operational definitions, even when they say they do. (Cordell L.) Hull’s work is replete with examples of allegedly operational definitions. Within three lines of many of these, he will insert an ontological addendum but still insist that the defined term has no meaning except as an intervening variable” [7, p145].

As mentioned, Dennett is of the view that, at the beginning of his career, Skinner made a profound and far-reaching mistake by equating mentalism with the supernatural. The psychologist was determined to eradicate from his “science of general psychology” all mentalist concepts, and therefore never looked seriously at the age-old questions of whether a non-mentalist psychology is possible or, crucially, whether mentalism is genuinely beyond analysis. Thus, there was a great deal of circularity, even question-begging, in Skinner’s psychology, i.e. he frequently assumed the truth of that which required proof. For example, it is typical of humans that we “plan ahead,” which means just what everybody thinks it means. But Skinner didn’t allow any mentalist concepts, and planning ahead is entirely mentalist. Behavior is under the control of the environment, he believed, but since future events haven’t yet happened, they can’t control behavior. What appears to be a case of people planning ahead is actually a matter of their past history of reinforcing contingencies controlling their behavior. Given a detailed account of everything that has happened to them, we would be able to say just what compels them to act in a particular way right now such that, lo and behold, a few days or weeks down the track, they get whatever it is they said they wanted in the first place.

Unfortunately, in discarding mentalism as non-science, Skinner adopted another bit of non-science. As every psychologist knows, keeping track of the history of reinforcing contingencies of even a laboratory animal is difficult; working out what happened to a human years before, when no records were kept, is impossible. What he called a “proper behavioral analysis” is just magical thinking. Skinner was led to this error by his major assumptions:

1. Mentalism is necessarily supernatural;
2. Therefore, behavior must be under environmental control;
3. But future events can’t control behavior because they haven’t yet happened;
4. Therefore, the controlling element must lie in the past history of environmental contingencies.

The real question here is whether we can derive a natural or non-question-begging means by which future events can control behavior (equivalent to the mentalist or folk explanation, “If you want to pass your exams, you’d better study now”). I believe we can, and an everyday example will demonstrate the point. I ride my bicycle to the university, locking it in the rack before attending a lecture. After the lecture, I decide not to go straight home as I want to go to the library to collect a book the lecturer has mentioned. In order to get to the library, I need my bicycle but, for the life of me, I can’t recall where I left it. I stand for a few moments, carefully “tracing” my movements since leaving

home and then experience that flash of recollection that says: "It's outside the gymnasium."

It is true that I don't have any sort of "real" picture of all the university's bike racks in my head, nor is there a physical model of my pushbike rattling around in my head. In order to recall where I left my machine, I need to have a means of representing it in my head, and of coding that information in a system of memory. Since rats can easily recall how to get back into a house, and pigeons never forget where they have placed the first two sticks for a new nest, we can't argue that memory is not a natural mechanism. The whole process may in fact be biological for all that it matters. But if manipulating the internal representation can successfully locate my bicycle even when I can't see it, then I would say that, on first principles, the same or a similar mechanism should also be able to cope with organizing to go the library to collect a book I have never seen. There is no substantive difference between my behavior being controlled by a bicycle I can't see and a book I can't see. Both matters involve the manipulation of information coded in my head. If there is nothing supernatural about using this type of mechanism to explain the effect of past events on my behavior, then there is nothing supernatural about using it to plan ahead.

As it happened, the book didn't control my behavior as it was on loan. It was the (mental) expectation that I would find it that counted.

Strictly speaking, of course, my wish to borrow the book was a past event as soon as I had formulated it and, in that sense, Skinner couldn't object to it controlling my behavior. Collecting the book wasn't part of the wish, and that is true of all future events. I simply set up a behavioral program that should have ended with my leaving the library with a particular book under my arm. If future events genuinely controlled human behavior, then we would never make mistakes. In his fanatical opposition to the notion of internal control of behavior, Skinner was forced to confront the idea that future events could control behavior. His "reflex" rejection of that possibility led him to miss the point that current internal representations aren't in the future.

Why didn't Skinner think of these obvious objections? The answer is that only a full-blown, anti-mentalist Skinnerian system requires the abolition of plans as mental events and, almost certainly, he didn't believe there was any chance of error in his system. What seeps through his later works in particular is a profound satisfaction with his ideas. Everything he described seemed to fit his notions remarkably well, and he devoted a lot of space in *Beyond Freedom and Dignity* to showing just this point. I suggest the reason human behavior fitted his flawed theories so very well was because he didn't offer an explanation of human behavior at all, just a redescription from a novel point of view. Explanations can easily be proven wrong but descriptions, even in new languages, can never be wrong. Intellectually, descriptions take no chances and radical behaviorism fell straight into this trap.

In our old, poetic ways, we say that a if slave driver wants a slave to work harder, he will give him a good whipping to teach him a lesson; in turn, the slave quickly works out that, by appearing to be busy, he can avoid the lash. This is a fairly humdrum, mentalist "explanation" that involves motives, hopes, fears and other unobservables. Skinner rewrote this to read: "Thus, a slave driver induces a slave to work by whipping him when he stops; by re-

suming work, the slave escapes from the whipping (and incidentally reinforces the slave driver's behavior in using the whip)" [3, p26]. All traces of mentalism have been removed; we are left with a bare, environmentalist account (not quite: I think the word "induce" has a covert mentalist element). But the quote says no more than that which it was designed to replace; it can't say more, as it is just another way of saying the same thing, and it doesn't say less as it hides the mentalism in the environment. As an attempt to explain human behavior, radical behaviorism is mere description masquerading as explanation. This is not to suggest that mentalism itself is anything more than description because, until we have an explanatory account of mind-body interaction, mentalism is just another pseudo-explanation. Just like radical behaviorism.

Skinner believed that he had found the non-mentalist key to controlling and predicting behavior, and argued that all human behavior fitted his concepts. Now this is true, all human behavior can indeed be made to fit his devastatingly simple concepts. The real question is: should we do this? The history of ideas is littered with failed theories where somebody tried to reduce all human activity to the outcome of one or two fundamental principles. Thus, Skinner needed to show why our understanding of ourselves would necessarily be improved by his particular stance, how a non-mentalist theory (or description, as it really was) was an improvement over the mentalist. For him, of course, there was a very simple answer: by eradicating superstition, our self-understanding would automatically be improved. Is this true? Or can there be a naturalistic account of mentalism? It was one of the more maddening features of behaviorists that they never addressed this question, but simply assumed the answer to be in their favor. The fact that nobody had ever derived a natural theory of mentalism was enough to convince them that it could never happen, with the result that eschewing mentalism was the only rational path to follow.

In *Beyond Freedom and Dignity*, Skinner did not argue this point but outlined his new way of looking at age-old questions. Throughout this book, he insists that while we have long looked at human behavior through mentalist spectacles, we can in fact also look at it from an environmentalist stance. He simply translates the mentalist concepts used in ordinary language, re-writing them in terms that have the effect of showing humans as marionettes dangling from environmental threads. All behavior, he argues, can be seen as the outcome of a previous history of contingent reinforcement of essentially random actions or operants. So it can, but should it? Or does the effort destroy something vital of our understanding of behavior?

Skinner never proved that his view was the only one available, nor did he show that his had greater predictive value or any of the other features that distinguish science from faith. He simply said: "We can look at all human behavior from this point of view," as though the reason for doing so was self-evident. Despite the unwavering conviction that sustained him for the better part of sixty years, he did not explain human behavior just because he failed to address these critical questions. He never wrote his technology of behavioral control, because he didn't have one.

2-4. Pavlov's Conditioning Model

Skinner was not the only psychologist in the behaviorists' century. In Europe, the dominant behaviorist theory was based in the work of the Russian psychologist, Ivan Pavlov (1849-1936). Every first year psychology student knows that Pavlov discovered the process of conditioning by observing dogs salivating in response to the dinner bell. At the time of his momentous discovery, Pavlov was studying digestion and had exteriorized the salivary flow in dogs so that their response to different stimuli could be precisely measured. He found that putting meat powder into their mouths caused a copious flow of saliva but before long, the dogs started to salivate almost as much when they heard the bell announcing lunch.

Pavlov's legendary formulation of this event was to regard the meat powder as the unconditioned stimulus and the salivary flow it stimulated as the unconditioned response. During the process of conditioning, the lunch bell became the conditioned stimulus, and the secondary salivary flow the conditioned response. In due course, Pavlov extended his concept to cover all forms of human learning until conditioning became the atomic building block of human behavior. This is what all basic psychology textbooks state; it's what all psychology students believe. Such a pity, then, that like most legends, there's hardly a word of truth in it.

Some parts of this comfortable little story are true. Ivan Pavlov was indeed a Russian scientist who worked with salivating dogs, but that's about all. He was a physiologist and, to his death, denied that he was a psychologist. It is true that he was studying the process of digestion in dogs when he discovered something about saliva and lunch bells, but he never claimed to have discovered a universal process called conditioning. As far as he was concerned, the concept of artificially-controlled physiological responses was simply a means of laboratory investigation and probably nothing more. He never believed that it was the (or even a) basic building block of human behavior and was antagonistic to the hope held by many psychologists of using it to build a general psychology.

In one of the last papers of his very long career, which was also his first paper in the psychological literature, Pavlov [2] attacked many of the concepts on which behaviorist psychology was based: "The psychologist takes conditioning as the principle of learning, and accepting this principle as not subject to further analysis, not requiring ultimate investigation, he endeavors to apply it to everything and explains all the individual features of learning as one and the same process ... (the psychologist) takes one physiological fact and ... gives it a specific meaning in ... the learning process (but) does not seek an (empirical) confirmation of that meaning" (p 91). Psychologists, he insisted, were still too much influenced by their historical origins as philosophers to understand the scientific process. They were not empirical researchers, but theorists who ignored scientific facts as it suited them: "...a whole mass of concrete facts remain without the slightest attention on (the psychologist's) part" (p 100).

Pavlov did not trust psychologists. Despite their protestations of scientific determinism (which, in any case, he believed to be false), they simply "...disguised by various scientifically decent synonyms" the same "dualism

and animism” in which ordinary people believed. At no stage was he sympathetic to the idea that a behavioral analysis will allow control and prediction of behavior: “The variety and number of these (cortical) stimuli are countless, even in an animal like a dog.” All his research indicated that the cerebrum was more, not less, complex than anybody had previously thought. Simple reductionism in any form was anathema to him: “I reject point blank and have a strong dislike for any theory which claims a complete inclusion of all that makes up our subjective world” (p 122; bear in mind that he did not take psychology’s anti-mentalism seriously). Physiology would eventually explain what reductionist psychology couldn’t: “...it is clear to me that many psychologists jealously ... guard the behavior of animals and man from such physiological explanations, constantly ignoring (established physiological processes) and not attempting to apply any of them to any extent” (p 123).

This would seem to be a fairly clear and authoritative rejection of behaviorist psychology’s attempts to found a general psychology in the theories of Ivan Pavlov. On the other hand, of course, it could be that the great man was wrong, that psychologists correctly saw more in his work than he did himself. This is not the case: Pavlov was right and the psychologists were wrong, because there is no process of conditioning. Without conditioning, there is no modern theory of learning, meaning psychology loses a large part of its claim to a separate existence.

The case against the notion of conditioning has been argued by a number of different authors, including Popper [10]. A paper by Efron [11] appeared at a time when behaviorists saw little standing between them and complete domination of the field of human psychology. For example, in the introduction to his textbook, Yates [12] gloated over the impending collapse of psychoanalysis: “It will surely not be long before every Hollywood star has his or her behavior therapist.” In fact, Freudian narcissism powers the Hollywood scene today just as it always has done.

In the title to his paper, Efron asserted that the concept of the conditioned reflex was meaningless. He pointed out that, within psychology, different authors use the term “conditioned reflex” in a variety of totally different ways. He cited a dispute between two authors, one of whom argued that worms can be “conditioned” while the other insisted that the first did not know the difference between “true conditioning” and “pseudo-conditioning.” Efron made a number of points:

a) that these types of disputes were due to “epistemological chaos” rather than to disagreements over genuine scientific facts;

b) the chaos derives from the assumption that all human behavior can be explained by eliminative materialism, i.e. that all “concepts of consciousness, volition and the causal efficacy of mental processes” can and should be excluded from the field of science;

c) that in attempting to eliminate all mention of conscious mental processes, reductionist biologists (essentially psychologists) have degraded the narrow concept of the reflex by progressively broadening it to the extent that it has long since become meaningless;

d) all attempts to salvage a meaning for conditioning, such as operationalism, are doomed to failure because they necessarily enter an infinite regress.