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## behaviorism behaviorism vs. cognitive approach

Posted by mascolo - 2010/02/17 23:17

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Concerning the debate about behaviorist and cognitive approaches to psychology: I believe that both approaches are inadequate. The behaviorist approach

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## behaviorism behaviorism vs. cognitive approach

Posted by Jon Krueger - 2010/02/17 23:17

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The behaviorist approach

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## behaviorism behaviorism vs. cognitive approach

Posted by Russ Poldrack - 2010/02/17 23:17

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It can only strengthen behavior to the extent that it is evaluated positively by the organism or has some relation to the organism's motives, goals, beliefs, values, etc. No, we can describe, predict, and control behavior without knowing what's happening inside the skin. We don't need to know how the organism feels about the reinforcer. Which is handy, because in general we don't know. We predict behavior (at least myself and Jerry Fodor do) in terms of propositional attitudes such as belief and desire. Whether this can serve as an adequate scientific account of behavior is disputed, but no other framework comes near to allowing us to explain and predict the behavior of others. Knowing that a person wants to kill me will be a lot more useful than to me than knowing that person's reinforcement history or brain-state. In general we DO know how people feel about reinforcers - we just have to ask them. Russ rpold...@s.psych.uiuc.edu

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Posted by Eric Lee Green - 2010/02/17 23:17

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Concerning the debate about behaviorist and cognitive approaches to psychology: I believe that both approaches are inadequate. The behaviorist approach

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Posted by Douglas Fitts - 2010/02/17 23:17

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understanding of behavior unless we know the \*meaning\* of the reinforcement to the individual or organism. meaning ? What is that? In behavioral terms, a reinforcement is simply that which causes a behavior to increase in frequency. That is the sole meaning of reinforcer . Perhaps you meant meaning of the 'stimulus' to the organism , except that in behavioristic terms that meaning is solely as a reinforcer or punisher (i.e., as encouragement or dissuasion). Now, I agree with you that any reinforcer is actually reinforcing a number of behaviors and that pure external behavior isn't adequate to explain why certain behaviors are being reinforced that we can't see being reinforced. Thus thinking , which Skinner explained in terms of internal behaviors of perception and re-perception and verbal behavior. Or, as he put it, human thought is a product of a verbal society . How can a reinforcement reinforce behavior? It can only strengthen behavior to the extent that it is evaluated positively by the organism or has some relation to the organism's motives, goals, beliefs, values, etc. But this implies that one must go inside of the organism to build an explanation of behavior. No. One merely sees whether the stimulus increases or decreases the behavior's occurrence. If it increases the behavior, it is a reinforcer. If it decreases the behavior, it is a punisher. No need to refer to motives , goals , beliefs , or values . Now, motives , goals , beliefs , and values are good catch-all words for describing complex sets of conditioned behaviors (some of which are undoubtedly covert or internal behaviors, cognitive behaviors so to speak). But they can all, in the end, be explained in terms of behavior

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Posted by Jon Krueger - 2010/02/17 23:17

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We don't need to know how the organism feels about the reinforcer. Which is handy, because in general we don't know. Neither statement contradicts the other. The interesting fact is we can learn much about how people feel about reinforcers without learning much about their behavior or its causes. People's feelings about reinforcers are not their behavior. The study of the former is not the study of the latter.

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Posted by Lee Lady - 2010/02/17 23:17

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The following is part two of a series of articles originally posted in September, 1991.

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Posted by Jonathan Barrett - 2010/02/17 23:17

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If learning can be understood independently of the internal structure of the organism and quite general laws of learning can be specified, then it must be that all actual organisms implement the same learning algorithm (at least at some appropriate level of abstraction). But, if that's true, it's miraculous, given the enormously different selective pressures that different species are subjected to. For example, look at all the different ways in which different species have solved the problem of obtaining energy. Shouldn't we expect a similar multitude of learning algorithms? Where have I gone wrong here? Also, I'm surprised that there is so much energy spent here defending behaviorist learning theory. Are there actually any real behaviorists left? All the modern textbooks on learning theory I've ever read (Dickinson, Contemp. Animal Learning Theory; Mackintosh, Cond. & Assoc. Learning) are quite explicit that learning theory has evolved into a COGNITIVE theory of how animals learn about correlations. Note, these are books written by hardcore animal learning researchers and not by softies. If anyone knows of a real behaviorist currently studying animal learning, could they please let me know who they are and maybe give me a list of some of their recent publications. Thanks. Jon

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Posted by Jon Krueger - 2010/02/17 23:17

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This is a problem of operant level

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Posted by Jon Krueger - 2010/02/17 23:17

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Are there actually any real behaviorists left? Sure. See JEAB, JABA.

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Posted by Anthony R. McIntosh - 2010/02/17 23:17

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Why is it that pigeons in a Skinner box are asked to peck a key or push a treadle instead of pressing a bar; why are the rats not asked to peck a key? Because it doesn't make any difference. You could have either do either, it's not out of their physical competence. The particular operant chosen is not critical. As has been demonstrated thousands of times. You really have to be careful when making such huge generalizations like this. You can get an animal to do almost anything in an operant chamber, but how well the operant response you \*force\* the animal to perform can be used to research operant conditioning is questionable. Often this entails punishing the animal for an incorrect response (e.g. Lashley jumping-stand). You can get a rat to bar-press in one or two training sessions (about 1 hr), but you cannot get a pigeon to do it in that amount of time

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The particular operant chosen is not critical. As has been demonstrated thousands of times. You really have to be careful when making such huge generalizations like this. Well, I should qualify not critical to what

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Posted by Anthony R. McIntosh - 2010/02/17 23:17

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Most cases? Hardly. The majority of operants can be brought under control of previously neutral stimuli. Of course there is species specific behavior: Rats naturally run around in burrows, make use of auditory and olfactory cues and manipulate food with their forepaws, pigeons tend to peck for food and are highly visual. But it's neither the majority nor the most interesting behavior. Then I would suggest you take a look at the more recent work on operant conditioning, starting with Tinbergen (for the history), Garcia, Shettleworth, Hearst, Seligman, Holland, Bolles, Timberlake, Colwill & Rescorla, and Gallistel. Ignoring the contribution of the organism unnecessarily narrows the focus of behavioral psych

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Often this entail having to punish the animal for an incorrect response (e.g. Lashley jumping-stand). No, punishment is the usual first sign of an incompetent trainer. Having to usually best described as so incompetent he couldn't figure out any other way to . Perhaps you should take a closer look at some of these operant paradigms before making these generalizations. The consequences of an incorrect response in different operant paradigms vary and often it's the paradigms that require responses that are not part of the subject's natural repertoire that have the most severe consequences. In a simple bar-press, the consequence is no food pellet, in the Jumping-stand the rat falls into a net; in passive avoidance there's a shock, in a water-maze the water used is cold. No, I stand by what I said. For instance take the example of the jumping stand. If the behavior is maintained solely by avoiding falling into the net, indeed it's an aversive method (although negative reinforcement, not punishment)

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It looks like that we are again getting into territory where we can argue indefinitely about who is right and who is wrong, but these point-counter point arguments tend to get hard to follow for everyone after a while, including the participants. The idea I get from your last post is that we probably agree on many of the global points but not the specifics. We can argue the specifics never accomplish anything, or we can get back to the point. Let me restate what I hoped I was saying and see if we can come to an agreement. The original idea in the experimental analysis of behavior was to identify general principles of behavioral change/learning that would apply across all species and across all behaviors. To do this, behaviorists sought to create an experimental environment where they could exert control over the behavior of the subject and assess the factors that led to behavioral change. This was the contention since Thorndike, and is the implicit assumption for anyone who studies learning to look at general principles independent of the species (this includes myself). Parallel to this was the study of the organism itself in ethology (Tinbergen for example) where species-specific behavior was the focus. Here the idea was to observe the behavior to determine the environmental factors that influenced these behaviors and if to see if there were commonalities between species. A big difference in this approach is that much of it was done in the field so experimental control was not as strong (there are studies which simulate field conditions in the lab to get more control). Both of these approaches can quite easily exist on their own, but some researchers have found that there is a profound interaction between them. There is no doubt that the principles identified in behavioral psych have a great degree of generalizability, but it's also the case that the principles can appear to be false when you require a response from the animal that is incompatible with its natural behavior

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Eclecticism is never a flag that attracts many followers, but scientific chauvinism is likely to win the wrong battles. (p 414 Weiskrantz, Analysis of Behavioral Change, 1968) Cute and relevant. But it seems to me that the battle isn't between people. It's between the best people you've got and Nature, fighting her tooth and nail to learn some of her secrets.

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Oh, no argument! I suspect that the knowledge gained about behavior (in my lifetime, at any rate) will be gained only by using both sorts of analysis. For example I have a hunch that social behavior will only be understood with both analysis of mutual contingencies and observation of highly social species in their natural habitat. Actually, I don't see an argument here. Surprisingly enough it seems that we've come to an agreement, and I thought this was doomed to mimic a Presidential debate :-) Anyone else have any thoughts on this subject? Randy

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Posted by John C Moore - 2010/02/17 23:17

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Anyone else have any thoughts on this subject? Randy

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Posted by Anthony R. McIntosh - 2010/02/17 23:17

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I look forward to reading more on this thread. Beats neuro-linguistic programming. amen Randy

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Posted by Lee Lady - 2010/02/17 23:17

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I look forward to reading more on this thread. Beats neuro-linguistic programming. Why do I think that this stimulus was inserted here to elicit a particular type of response? Well, I don't mind confirming your paradigm: I'll respond! If in fact it the techniques developed by NLP are effective in producing very specific and readily testable changes in people, then any theory of psychology that is unable to explain these results is clearly inadequate. This would be so even if it were true that NLP only provides a quick fix or for other reasons has limited clinical value. I realize that it can be very satisfying to confine your research to experiments with laboratory animals where you know that your theory works. Some people consider this being scientific. I'm a mathematician, not an empirical scientist. But in my opinion, in order to really be doing science one has to look for all those cases where one's theory doesn't seem to fit what actually happens, or where other explanations produce better results. The attitude of scientific psychologists Well, we don't need to know about NLP reminds me of what Poincare said when Lebesgue first produced examples of functions with counter-intuitive properties (such as being nowhere differentiable): Well, even if such functions exist, the proper thing for mathematicians to do is to just ignore them. The history of twentieth century mathematics shows that those who ignored Poincare's advice produced interesting and valuable mathematics. I believe that those scientific psychologists who examine NLP as a possible source of ideas, hypotheses, questions, and phenomena to be explained will produce interesting and scientifically valuable psychology.

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