IS BEHAVIOR PROBABILISTIC?

by
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Many psychologists say that predicting actions with certainty cannot be done, because behavior is probabilistic. That is, given any set of conditions in which we find a person, we will be able to say only that there is a probability less than one that the person will do a particular thing. Therefore, the argument goes, any experiment that shows more people doing the predicted thing than you would expect from the base rate--enough more than you would expect from chance variation--shows that you have learned something about the causes of behavior.

Well, consider walking. For the purpose of argument, let's suppose your chance of taking another successful step is 999 in 1000 or .999. To say it another way, let's suppose you would expect to fall once in every 1000 steps. Suppose you walk fairly briskly along the street at 100 steps per minute. You would fall, on the average, once in 10 x 100 steps or once every 10 minutes. Other people walking on the street at that same rate would also be falling down every ten minutes. If ten people were walking near you, going in your direction, one of them would fall, on the average, every minute. But 100 steps per minute is a little fast for most people out on most errands. Fifty steps per minute, on the other hand, is a mere saunter. Let's suppose that 75 steps per minute is fairly close to the average on a city street where people are not just hanging out, but actually going someplace. You would fall, on the average, once in 13.3 x 75 = 1000 steps or once every 13.3 minutes. Among 13.3 other people, one of them would fall, on the average, every minute. Is this an accurate description of your experience when you walk on a city street? Among the people around you, do you see one falling, or even stumbling, every minute or so?

Well, you might say, taking a step could still be probabilistic, but the probability could be very, very high. All right, suppose the probability of a successful step to be 999,999 in a million. You would expect to fall only once in a million steps. At 75 steps per minute, a million steps would take 13,333 minutes or 222 hours. Suppose you are pretty sedentary and walk only one hour per day (counting <u>all</u> walking) on the average. Then you should expect to fall once in 222 days. Is that your experience? Do you fall--not from interferences such as an unseen object underfoot, but from inexplicable misfunctioning--once or twice a year? Do you know anyone--any physically normal adult--who does? I don't fall that often even from stumbling over something.

From my own experience (and yours, I believe) it is obvious to me that we walk with much better odds than a million to one that we will take the next step successfully. When the

probability is that great, it seems to me to be stretching things a great deal to claim that behavior is intrinsically shot through with only probabilistic regularities. It seems to me much simpler and more reasonable to suppose that walking is under very precise control and is not probabilistic at all. Environmental events that can interfere with walking, it is true, are probabilistic; we cannot know when a dog will dart between our feet or when we will step into an unseen hole in a meadow. But the management of walking, one foot after another, is highly controlled and successful almost without exception.

Outside psychophysics and other strongly physiological studies, the rates of success in psychological experiments are far below the rates of success in walking that I have been using here for illustration. In studies of acts of learning, of exhibiting memory, of showing emotion, of predispositions of personality, of presumed psychological disorders, even of the more complex perceptual phenomena, the rates of successful prediction of the next act are never as good as 999,999 in a million, never 999 in a thousand, never 99 in a hundred. Indeed, rates of successful prediction rarely reach 9 in 10, and when they do, they almost always fail to do so in the next similar study.

Some psychologists will say that I have pointed out the flaw in my own reasoning. I have said that actions heavily interlaced with bodily processes such as the psychophysics of perception, the control of muscular action, and so on, are indeed highly predictable in psychological laboratories the world over. Some psychologists, I suppose, would say that this more physiological kind of behavior is inherently more predictable and less probabilistic than the more mental kind of behavior--than behavior "mediated in the higher nervous centers," as they might put it. This view might be considered a modern version of the ancient claim that mind and body function by different laws. Books on psychology do not put this view into words ay more, but departments of psychology in every university hint at this kind of view when they establish subdisciplines of personality psychology, physiological psychology, and so on. My impression is strengthened when I discover that the research methods in those subdisciplines differ in ways easy to see (in the insistence on random sampling, for example) and when I notice that specialists in one subfield know little about the work in another and show little desire to know more--when they seem to believe that knowledge of a colleague's specialty will not help them in their own. In the departments with which I have been acquainted, the common attitude seems to be "If those guys don't interfere with me, I won't interfere with them." Furthermore, the professional journals of psychology are specialized by those same subdisciplines. The implication is strong that most psychologists believe that the laws applying to other subdisciplines can safely be ignored--that those laws have very little application to their own subdisciplines.

To say that nature has arranged for one kind of functioning of the human animal to proceed with a random or probabilistic admixture and another kind to proceed in a controlled manner--to say that, it seems to me, violates not only the principle of parsimony. It also violates the principle of evolution that later forms grow from earlier forms, that nothing grows out of the void, that the new builds on the old. If the neocortex is a late evolutionary development, it should use the principles than enabled the older brain to work. To claim that a new principle of functioning can appear in the brain at some point, a principle not built on an old principle, is to claim to construct a brain by supernatural means. It seems to me that we should demand of all varieties of psychology that if a law describes one kind of human behavior, the law should have its counterpart in other kinds of behavior. We should not have separate psychologies for walking, eating, naming colors, gambling, obeying, persuading, teaching, listening to Beethoven, and so on.

I would sympathize with specialists who choose to limit their research to the variables popular within their specialty, and who do not look for ways of functioning that cross boundaries of specialties, if no one had yet proposed principles of functioning to hold in all kinds of living functions--in all branches of psychology, so to speak. But the principle of control of perception is indeed meant to apply to all functions in which the nervous system sets off action.

I would sympathize with the specialists, too, if the principle of control of perception received no better experimental support than the kind of research results appearing regularly in journals of personality, clinical psychology, social psychology, and the like. But the fact is that the functioning of actual working models of perceptual control regularly show correlations better than .97 with actual human functioning. Furthermore, those correlations are not calculated by taking two (or more) data from this person, two data from a second person, two from a third, from a fourth, and so on, in a laboratory where environmental events are kept as unchanging as possible except for the variables being measured. Instead, the correlations are made by taking consecutive pairs of data from human and model; that is, each pair of data consists of one datum from the person and one from the working model. And the behavior occurs in an environment in which random changes in the environment disturb the effects of the acts of the person and the "acts" of the model. Nevertheless, the models typically behave almost exactly as the persons being modeled. Since the models are built on the principle of the control of perception, the inference is strong that humans function in that same way.

A considerable literature now reports the results of modeling the control of perception. The results--so far, at least--are uniform in their support of theory. By uniform, I mean that no single experimental subject has yet behaved with the

experimental apparatus in a way that the model cannot duplicate to a very high correlation. In my reading of this literature, I have found subjects whose correlations fall below .90 to be extremely rare. Subjects whose correlations exceed .97 and even .98 are common; almost all correlations are above .95. I am not saying that all research on the control of perception shows this degree of success; there are exploratory edges of the research that are not yet succeeding this well. But my statements apply to all research that models behavior by computer and compares it with actual human behavior.

This literature began to appear in the late 1950s. Powers's book appeared in 1973. Some of the easily available literature is Marken (1990, 1992), Powers (1971, 1973, 1978, 1989, 1992), Robertson and Powers (1990), and Runkel (1990). Beyond the research that tests perceptual control theory with working models, there is a further large body of literature that similarly postuates feedback loops; one part of it has been described by Smith and Smith (1988).

The literature on perceptual control is now sufficiently easy to come by that anyone capable of relinquishing some old ideas can seize the means of becoming informed. Because this literature does exist and because this experimentation does not require randomness to be a large component of all behavior, I do not sympathize with psychologists who claim that behavior is probabilistic.

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