Teaching Dogma in Psychology

By Richard Marken

Dr. Richard Marken, Associate Professor of Psychology, joined the Augsburg College faculty in 1974.
Lecture date: May 15, 1985.

I HAVE COME HERE to come out of the closet—I am not a straight psychologist. I have been convinced for at least five years now that the foundations of my discipline are wrong. I feel like the little boy who noticed that the emperor was not wearing any clothes. All the people who would like to be considered smart are saying that behavior is controlled by environmental events.

This is the central dogma of scientific psychology and of the social sciences in general. It is the basis on which all research is conducted in these disciplines.

Things look quite different to me. It looks to me as if behavior controls the environment—not vice versa. Behavior is the process by which we control the things that matter to us—to behave is to control.

The difference between the conventional view of behavior and my own is fundamental. From my point of view the introductory psychology texts are wrong from the preface on. There are irreconcilable differences which I will try to make clear. As you can imagine, given what I have just said. It has been terribly difficult to teach some of the standard psychology courses, notably the intro course and the research methods course. It is not a problem that can be cured by putting a little section on "my point of view" in these courses. It would be like having to teach a whole course on creationism and then having a "by the way, this is the evolutionary perspective" section. Why waste time on non-science? From my point of view, most of what is done in the social sciences is scientific posturing and verbalizing.

First, let me tell you a little about how I came to this revolutionary position. I did not set out to be in this boat; I am not a revolutionary by temperament, and I have not been brainwashed by some weird cult.

I was trained as a standard experimental psychologist. My specialty was auditory perception. I did my thesis research on an esoteric but conventional topic—auditory signal detection. I knew my stuff—I became an expert in experimental design and some of the more powerful aspects of statistical analysis.

Shortly before coming to Augsburg, in 1974, I was browsing through the library at UCSB and noticed a new book with the intriguing title: *Behavior: The Control of Perception*, by William T. Powers. I was curious, because I was a student of perception and interested in behavior. But I couldn't imagine what this book might be about. I looked through it briefly. My impression was that the author knew what he was talking about. I, however, did not. The book, it turns out, was about control theory as a model of behavior. I had no idea, at the time, that control theory would eventually turn my professional life into agony and my intellectual life into bliss.

During my second year here I discovered that Powers' book was in our library. I went back to take a look at it. I had an idea that it might help me in a talk I was preparing, at the time, on the control of behavior. This talk was to be sort of a rebuttal to one given earlier by Dr. Ferguson on the glories of behavior control. I was trained at a school that was very oriented toward cognitive psychology, bristling

with the then new computer-oriented approach to behavior. I thought Skinnerian behaviorism a dinosaur that had been comfortably interred so I was surprised to find so many people here who not only admitted but were proud of their adherence to Skinnerism. I was going to present the enlightened cognitive view. I know now that the differences between cognitive, behaviorist, and other approaches to psychology are matters of form more than substance—different verbalisms for the same basic model.

I tried formulating the talk on the basis of concepts from cognitive psychology—along with some of the stuff I was learning from Powers' book. But as I read and re-read Powers, he seemed to make more sense than anything I was reading in the cognition texts. Powers spoke directly and clearly to the fundamental problems that I had only intuitions about. I realized that cognitive psychology was trying to differ from behaviorism by talking bravely about mind, but the basic approach was the same: behavior is caused by inputs into the system; the inputs just swirl around more inside the system before coming out as behavior. I eventually based the entire talk on Powers' book, which I really didn't fully understand at the time.

After the talk, my interest in challenging Skinner diminished, but my interest in control theory continued to grow. I was still a conventional psychologist. I was even trying to do some perceptual research—based on the standard model. But control theory kept bugging me. I wanted to do research based on control theory. I tried to graft control theory into some of my research projects. This really didn't work; Control theory implies such a fundamentally different orientation to behavior that attempts to apply control theory to the results of most conventional research will be fruitless—I will explain why in a moment.

This was about 1978, and I was starting to see the beauty of control theory. My faith in conventional psychology was waning, and this was very troubling. I read all I could find on control theory. I started to realize that much of what was said about control theory or feedback theory in the behavioral science literature was wrong.

In 1978, Powers came out with an excellent article in *Psychological Review*. This was a significant event, because it was the first new publication I knew of, since his book, and it described some actual experiments demonstrating some of the basic principles of control theory. The article was rough going—mathematically and conceptually. But I set up the experiments on my computer and started really to understand what was going on—and what was going on was downright amazing. The process of behaving is a truly remarkable phenomenon; I began to understand what the title of Powers' book meant: To behave is to control, and what control systems control is not their actions but the perceptual consequences of their actions.

My understanding was further expanded by a series of four articles Powers published in *Byte* magazine in 1979. The experiments I was doing (and still do) look pretty simple. They involve controlling events on a computer screen. Though simple, the experiments demonstrate the way control systems work—and the results are completely inconsistent with all current models in psychology. Control systems behave in ways that are quite counter-intuitive. The experiments are simple for the same reason that the experiments in physics labs are simple—we know what results we're going to get. The results are perfectly repeatable. They show how control works. Once you know the principles and can repeatedly demonstrate them, you have a solid foundation for going on to more complex phenomena. The experiments I do are of a type completely alien to conventional "Psychology Today" mentality, so they are sometimes dismissed as trivial. To my mind, one quality fact is worth all the statistical generalities in all the social sciences.

In 1980 I began my own little research program on control theory. I designed a number of studies that were aimed at showing how the behavior of a control system (like a person) differs from that of the kind of system that psychology currently imagines people to be. I have had little difficulty publishing these reports, and the reception of my work at meetings has been positive—probably because no one really understood what I was talking about.

By 1981 I had become a complete prodigal. I now understood control theory rather well and knew precisely why it was usually a waste of time to try to interpret existing research findings in terms of control theory. This is the usual challenge I get—how does control theory explain this or that "fact"? My first answer is that the statistical results you find in the social sciences do not, for me, constitute meaningful facts. But the real problem is that facts obtained in the context of the wrong model are simply misleading and worthless.

Once you get to a certain point in your understanding of control theory, you realize that almost all of traditional psychology can be ignored. This is a rather sickening experience at first, and everyone I know who gets excited about control theory eventually encounters the problem. A clinician friend of mine in New Jersey, an avid control theorist, just isn't willing to cross the line and ignore what deserves to be ignored—yet. I sympathize. It's not easy to ignore everything you were once taught to take very seriously. But this is what had to be done in physics after Galileo. You just have to take off in the right direction. Physics doesn't need to spend a lot of time explaining why pre-Galilean physics is wrong. Revolutions are revolutionary—you don't gain anything by clinging to old ideas that are wrong, no matter how much you used to love them.

Current approaches to psychology and the social sciences are based on an input-output model of behavior. In every methods class you learn that the proper way to study behavior is to manipulate independent variables (environmental input, such as room temperature or reinforcement schedule) to determine their effects on dependent variables (behavioral outputs that you have carefully operationally defined so as to be measureable). This should all be done under controlled conditions, so that you can correctly infer causality—that is, if there is a change in behavior, this change can be attributed to variation of the independent variable.

In some social sciences manipulation and control is impossible, but the approach is the same: look for correlations between input and output variables, between environment and behavior. This is bread-and-butter psychology and sociology and economics and political science. It's easy to do once you get used to it.

This method of doing research will give you good results only if the objects of study are input-output devices. Whatever the verbalisms used to describe different theories, the model of research in the social sciences assumes that organisms are some type of input-output device—arguments concern only what type (computer, conditioning machine, etc.).

The social sciences have persisted in using this model in spite of the fact that it *clearly does not work*. The results of research in the social sciences are a mess by any reasonable scientific standard. They are extremely noisy. Statistics must be used to determine whether anything happened at all in most studies. The reason for all this variability in the data is usually attributed to random stimuli flying around in the environment. But after 100 years of doing this kind of research, using more and more sophisticated apparatus and control, the variability is still there and it is still large.

Nowadays the variability of data in the social sciences is attributed to the inherent variability of behavior. Besides being unscientific by blaming the failure to understand a phenomenon on the objects of study, this posture can be seen as ridiculous just by looking around. If the behavior of the architects, engineers and workers who built the buildings in this city were as variable as social scientists imagine it to be, few of these structures would still be standing.

In fact, behavior is variable only when looked at from the wrong point of view—the point of view of the input-output model. What's wrong with the model can be seen by considering the output side of the model in more detail: Just what is behavior? The textbooks say that it is anything that organisms do—but we know that's not so. Psychologists don't study the acceleration of animals as they are accelerated to earth by the force of gravity, but the animal is behaving.

The behavior we are interested in is the kind that is generated by the organism itself—not only generated by the organism itself, but consistently so. If organisms never did anything more than once, we would see chaos. Instead, we see regularity—pressing a bar, getting dressed, having a conversation, making love.

The events that we recognize as behavior are named for the uniform results produced by organism actions, not for any particular pattern of the actions themselves. Thus we see an animal pressing a bar, but fail to note that the result (the lever going down) is always produced by a different pattern of actions. In fact, the detailed actions that produce any behavior are always different and must be different if the result is to repeat. The appropriateness of this variability cannot be understood in terms of the input-output model, so it is ignored.

Students of behavior have noticed that organisms use variable acts to produce consistent results, but few have noticed that these variations are necessary. Skinner, for example, considered the different ways the rat gets the lever down to be arbitrary—one way is just as good as another. In fact, if the rat pressed in the same way each time, the lever would not go down

on each occasion. The apparently random variability is really not random at all. But this causes a problem, because it then appears that the organism is varying its actions in just the right way to produce a consistent result. It looks like the animal is trying to get the lever down. This implies internal purposes, and there is no room for such things in an input-output model.

E.C. Tolman was on the right track. He showed that rats who could run a maze to a goal could still get to the goal when the maze was filled with water. Tolman correctly concluded that the rat had the purpose of getting to the goal and was using whatever means necessary to produce that result. But this was in the 1930s, before control theory and hence the tools to explain how purpose could be carried out. So everyone said, "response generalization" and went back to the labs with the input-output model intact (in their heads, if not in reality).

However, if one thinks about it for a moment, it is clear that Tolman's phenomenon—together with many everyday examples of the same thing—is completely inconsistent with the notion that behavior is the last step in a causal chain, as the input-output model implies. There is no way for any input-output system, however smart, to produce actions that will always have the same result in an unpredictably changing world. The straight-through causal model breaks down completely.

When we do anything we are adjusting our actions, usually without even being aware of it, to produce the intended result, regardless of the prevailing environmental circumstances. The rat pressing a bar is not just emitting this result—it is producing forces which, when combined with all other forces acting on the bar, produces the result "lever press." These "other forces," which I call disturbances, are always present when we do anything. We usually don't notice their contribution to behavior because their effects are usually precisely canceled by the actions of the organism. If I pressed a bit on the other end of the rat's lever, the lever would still go down because the rat would increase the forces it exerts in just the right way to produce the intended result. If I block a route you usually take to get to the store, you will get there by another route: the same result produced by different means. Thus, the effects of disturbances are not noticed, and behavior seems to just pop out of animals.

The process of producing consistent results in an unpredictable environment is called control. To behave is to control. The only system known that can do what organisms do every instant of the day is the negative feedback control system. A control system produces the consistent results we call behavior by producing pre-selected perceptions, not outputs. Control theory consists of the equations describing how closed loop control works. Control is not explained by muttering words like "feedback" and "error correction." I have never seen a correct treatment of control in the behavioral literature.

To the extent that behavioral scientists have dealt with it at all (and they have really tried), control theory has been twisted into what is really a disguised version of the old input-output model. This is usually done by imagining that closed loop control systems can be broken up into an alternating sequence of inputs and outputs. What you get is a sequential model where a person makes a response which produces a new input, which produces a new response. Input and output are preserved, alternating in time. In fact, such a system would not control anything. Real control systems work much more beautifully—there is no alternation in time. Input and output are joined in a continuous wheel of causation. The system is a wholly different thing from that which psychologists imagine it to be.

One reason psychologists have not learned control theory is that they think that they already know it. They don't—they just know terminology. When they get close to understanding it, they realize that it is completely different from their beliefs—so they redesign it to be consistent with their preconceptions.

Now I can try to explain why the results of behavioral research based on an input-output model is bound to be largely useless. According to control theory, when we are watching behavior we are watching a control system from the outside. This system will be controlling many different results of its actions (actually the perception of those results), some of which will correspond to very complex functions of the events that are part of the observer's perceptual experience. To control these results, which are almost certainly going to be quite abstract and, thus, hard for an outsider to notice, we will see the system doing many things in the process of protecting these results from the effects of disturbance. We might want to find the "cause" of one of these actions. So we do an experiment in which we manipulate stimuli to see if there is some effect on the action. Some effect is almost certain, although it will be only statistical. Almost anything you do is bound to disturb, in some

way, some controlled result of actions. The behavior you are studying may be only incidentally related to the means used to protect against the disturbance you have created. Hence we get statistical relationships —usually by averaging over several subjects.

If you had a better idea of what the subject was trying to control, you could get more precise results. This is what happens in operant conditioning experiments. Of course, the experimenters would never consider reinforcement a controlled result of actions, but it is. In operant situations you create disturbances to the rat's ability to control the reinforcement rate. This leads to precise and dramatic corrective actions by the rat. For example, if you require more bar presses per reinforcer, the rat presses faster, preserving the rate of reinforcement. Of course, to the experimenter it appears that the change in reinforcement schedule is controlling the rat's bar pressing. But this is an unfortunate illusion that has prevented psychology from progressing beyond the input-output conception. This illusion of stimulus control (a well understood property of control system behavior) is just as compelling as the illusion that the sun goes around a stationary earth—just as wrong and just as difficult to dispel.

What you get by studying control systems as input-output systems is exactly what you have in the social sciences—a confusing and often inconsistent array of findings, only weakly reproducible and little more than verbal models to account for them, models with virtually no predictive or explanatory power. If you knew what the subject was controlling, you would not have to do such experiments any more. You would know how the system would respond to any disturbance. This is one goal of research based on control theory: to discover the kinds of things that can be or are controlled. Then you can ask how they are controlled, and why. The "how" question will take you to lower-order control systems (What results are controlled in order to control this result?). The "why" question will take you to higher-order control systems (What higher-order result is being controlled by controlling this result?).

Control theory is revolutionary, and the revolution is going to be tough. One reason is that most social scientists see no problem with the status quo. People will continue to do bread-and-butter social science because it's what they know how to do—they know what kinds of questions to ask and what kind of results to expert. Social scientists are experts at having an explanation for the results, no matter how they come out, so long as they are statistically significant. It is easy to turn the statistical crank. With sufficiently powerful statistical tools, you can find a significant statistical relationship between just about anything and anything else.

Psychologists see no real problem with the current dogma. They are used to getting messy results that can be dealt with only by statistics. In fact, I have now detected a positive suspicion of quality results amongst psychologists. In my experiments I get relationships between variables that are predictable to within 1 percent accuracy. The response to this level of perfection has been that the results must be trivial! It was even suggested to me that I use procedures that would reduce the quality of the results, the implication being that noisier data would mean more.

After some recovery period I realized that this attitude is to be expected from anyone trying to see the failure of the input-output model as a success. Social scientists are used to accounting for perhaps 80% (at most) of the variance in their data. They then look for other variables that will account for more variance. This is what gives them future research studies. The premise is that behavior is caused by many variables. If I account for all the variance with just one variable, it's no fun and seems trivial.

If psychologists had been around at the time that physics was getting started, we'd still be Aristotelian, or worse. There would be many studies looking for relationships between one physical variable and another—e.g., between ball color and rate of fall, or between type of surface and the amount of snow in the driveway. Some of these relationships would prove statistically significant. Then when some guy comes along and shows that there is a nearly perfect linear relationship between distance traveled and acceleration, there would be a big heave of "trivial" or "too limited"—what does this have to do with the problems we have keeping snow out of the driveway?

Few psychologists recognize that, whatever their theory, it is based on the open-loop input-output model. There is no realization that the very methods by which data are collected imply that you are dealing with an open-loop system. To most psychologists, the methods of doing research are simply the scientific method—the only alternative is superstition. There is certainly no realization that the input-output model is testable and could be shown to be false. In fact, the methods are borrowed, in caricature, from the natural

sciences, where the open-loop model works very well, thank you. Progress in the natural sciences began dramatically when it was realized that the inanimate world is not purposive.

Psychologists have mistakenly applied this model of the inanimate world to the animate world, where it simply does not apply.

This was a forgivable mistake in the days before control theory, because before 1948 there was no understanding of how purposive behavior could work. Now we know, but the social sciences have their feet sunk in conceptual concrete. They simply won't give up what, to them, simply means science.

It is not, however, science, and the input-output framework is not the way to study closed-loop systems. There is a methodology for studying purposive systems; I have written a little about this. It is quite objective and experimental, and it gives results that are completely precise—and without statistics. But it is based on the rigorous laws of control, not on loose verbal, or mistaken quantitative, treatments of behavior.

I am not here seeking converts. I do not expect a social scientist to become a control theorist. Control theory requires a great deal of work; it is a lonely enterprise, and involves a painful change. But I hope that you can see why I can no longer teach the dogma.

I love psychology, and I consider it potentially the most exciting field left to explore. That is because it is basically virgin territory. All the attempts to understand behavior up to this point have been well-intentioned stabs in the dark. They have been based on the only tools available and on an allergic fear of committing metaphysics.

One might well ask. "Why should I believe you?" Well, you shouldn't. Understanding human nature is not a matter of finding the right words to use to describe a phenomenon, although one might easily get that idea by spending enough time in the social sciences. The only way to become convinced about the value of control theory is to learn it, to test it, to try to understand it. And then see if you can still buy the old approach. But learning control theory takes time, in my case at least two years—really four years before I was really comfortable with it.

I don't have a private pipeline to truth, and control theory is the beginning of a search, not the end. It won't solve all your problems. But it will, once you really begin to understand it, give you the extremely satisfying experience of finally knowing a little part of one of nature's secrets: the secret of purposive behavior. Then you can start looking at how learning, memory, consciousness, individual differences, and so on, enter the picture. But at least you will know that you are on the right track, proceeding from a solid foundation of replicable facts rather than from a trembling network of unreliable statistical generalizations.

Control theory has made me a revolutionary, not against psychology, but against the current dogma that passes for scientific psychology. If you are happy with the dogma, then go with it. If you want to understand human nature, then try control theory.

So my problem is what I, as a teacher, should do. I consider myself a highly qualified psychology professor. I want to teach psychology. But I don't want to teach the dogma, which, as I have argued, is a waste of time. So, do I leave teaching and wait for the revolution to happen? I'm sure that won't be for several decades. Thus I have a dilemma—the best thing for me to do is to teach, but I can't, because what I teach doesn't fit the dogma. Any suggestions?