The Harvard Review

VOLUME III
NUMBER 1
SPRING 1965

A New Psychology?

THE HARVARD REVIEW is published quarterly during the academic year; single copies 75 cents, subscriptions, $2 in Cambridge, $3 elsewhere. The views expressed in the articles that follow are those of the authors, and not necessarily those of the REVIEW staff or its sponsors. Designed and printed by the Capital City Press, Inc., Montpelier, Vermont.

Love in a Test Tube

JOEL E. COHEN

> It is indeed true, as we have said, that life brings absolutely no difference into the scientific experimental method which must be applied to the study of physiological phenomena, and that in this respect physiological science and physico-chemical science rest on exactly the same principles of investigation. But . . . phenomena merely express the relations of bodies, whence it follows that, by dissociating the parts of a whole, we must make phenomena cease if only because we destroy the relations. It follows also, in physiology, that analysis, which teaches us the properties of isolated elementary parts, can never give us more than a most incomplete ideal synthesis; just as knowing a solitary man would not bring us knowledge of all the institutions which result from man's association, and which can reveal themselves only through social life.
>
> —CLAUDE BERNARD, *An Introduction to the Study of Experimental Medicine* (1865)

Not long before she was married, my older sister, then a graduate student in cellular physiology at the University of Chicago, took me into her confidence concerning her relations with her husband-to-be. "There's one thing you should remember," she said, "love is just a state of your biochemistry."

Being at the time an impressionable youth, I resolved to keep the state of my biochemistry under careful surveillance. I have had no occasion since then to doubt the truth of my sister's revelation. However, I have been forced (by circumstances which need not be described here) to question the usefulness of her advice.

In the language of science, if my biochemistry and behavior are dependent variables, then the independent variables that produce love in me usually don't come in a test tube. The state of love seems at present to require something more than a purely biochemical description, and the independent variables that bring it about often have to be understood in other than biochemical terms.

I do not argue mystically that the pursuit of relations between biochemistry and behavior or of "biochemical explanations of mind" must be fruitless. The effectiveness of drugs in controlling the behavior (and, presumably, also the thoughts) of mentally healthy and ill people voids any such argument.

In fact, I make no claims about Truth, but only about present research strategies and standards. I claim that there are biological phenomena for which biochemical approaches are *not now* useful. New regularities, different in form from biochemical laws, usually emerge as the order of magnitude or level of complexity of a biological problem increases. Devotion to reducing biological phenomena to biochemical laws is no necessity of sound research; pursued exclusively, such devotion may even hinder scientific insight into the phenomena of life.

Because the elements of time and usefulness are central to them, these claims can only be supported by evidence, not proved by a priori logic. Optics is an example of a respectable physical science that uncovered new phenomena and invented new theories as it pushed into new domains. And psychology, sociology, and history (branches of biology not often recognized as such by biologists, let alone biochemists) provide examples of facts and theories unlikely to have been discovered through biochemistry and probably not best pursued through biochemistry.

And There Was Light

The science of light has available three theories—the theories of geometrical optics, physical optics, and quantum optics—for use in understanding various properties of light.

When the instruments used to measure light waves are insensitive to small amounts of light energy and are much larger than the wave length of visible light, the resulting science of light is known as geometrical optics.

The predictions of geometrical optics follow from three laws stated in high school physics courses: light in a uniform medium travels in a straight line (known before antiquity); light bouncing off a mirror is incident and reflected at equal angles (known to the Greeks); when light passes from one medium to another, the sine of its angle of incidence divided by the sine of its angle of refraction is a constant (discovered by Snell in 1621). These three laws follow from Fermat's principle: light traveling from one fixed point to another always follows a path which requires the greatest or least amount of time when compared with neighboring paths.

Soon after the three laws of geometrical optics were known, and all optical phenomena encountered by lens grinders could be explained, diffraction, interference, and polarization were discovered. Light diffracted by passage through a pinhole or narrow slit does not travel in a straight line. Light passed through thin films like soap bubbles produces complicated interference patterns of colors and intensities.

Here the relevant dimensions of the measuring instruments (e.g., the thickness of a soap bubble) are of the same order of magnitude as the wave length of visible light, though the instruments are still insensitive to low intensities of light. Not until a century ago did James Clerk Maxwell publish a unified theory, the electromagnetic wave theory, for these phenomena of "physical" optics.

Then, in 1897, using instruments sensitive to very small amounts of light, Hertz discovered the photoelectric effect commonly used in modern light sensors. This effect, the Compton scattering effect, and others had to be accounted for by an entirely new theory—that of quantum optics, created at the beginning of our century. It assumes that light energy must come in packets or quanta known as photons, and that it is impossible for a light source to emit anything but an integral number of light quanta.

As physicists extended the range of their measurements, new phenomena appeared, and new theories had to be invented. For a simple lens problem no physicist unzips his quantum theory: he uses different theories for different purposes, knowing that each is valid in its own domain.

Instead of optics, I could have picked thermodynamics, and shown that classical thermodynamics was a coherent theory of macroscopic variables like temperature and pressure before it was reduced to the kinetic molecular theory; I could have picked general relativity theory as an extension of Newtonian mechanics. All of these examples illustrate Bridgman's observation in *The Logic of Modern Physics*: "When an experiment is pushed into new domains, we must be prepared for new facts, of an entirely different character from those of our former experience."

Physicists would like to have one uniform theory for all physical phenomena. They will rightly point out, for example, that it is possible to derive physical optics from quantum optics, and geometrical optics from physical optics, by "passing to the limit," that is, by assuming that under the proper conditions certain effects become so small as to be negligible. Yet there do remain mutually irreducible domains in physics. Relativity theory and elementary particle theory are such domains today. Not only do these two fields differ in content, but no theory unites them in the way that electromagnetic theory unites light and the structure of matter.

More important than the theoretical pluralism of physics is the pragmatic truth exemplified by optics: in practice physicists use a whole quiver of locally effective theories.

Biologists, too, possess a quiver of locally effective theories, though they have considerably fewer than do the physicists. Moreover, the subject matter biologists deal with is much less readily encompassed by unifying theories. Thus, there is no obvious way to derive the following three examples each from the other or all from a common (say, biochemical) progenitor. Even if a derivation from biochemistry did miraculously appear, it would be neither necessary nor sufficient for future progress.

To Skinner a Cat

The first example is the analysis of some of the behavior of experimental animals using the concepts of operant conditioning. Suppose that when an animal does some particular thing or sequence of things without explicit prodding from its environment, the animal is regularly rewarded with something it has been deprived of, such as food. Then the rate or frequency at which the animal performs that thing will increase. The animal is said to be reinforced for performing the act, and the resultant shaping of behavior is known as operant conditioning. If an experimenter controls the animal's environment and the ways in which the animal is reinforced, he can also control very precisely the form and frequency of the animal's actions.

B. F. Skinner of Harvard and other behavioral psychologists have found striking regularities relating reinforcement by the environment to the behavior of an animal undergoing operant conditioning. The unexplained fact on which these laws rest is that reinforcement works: the environment can select, even create, an element of behavior and make it endure.

A little physiological evidence on the basis of operant conditioning does exist. Certain drugs, such as tranquilizers, are known to alter components of a pigeon's response to some schedules of reinforcement. Electrical stimulation to a "pleasure center" in the brain of a rat is a highly rewarding effect of the environment; a rat will work tirelessly to get such shocks. And recent work on classical conditioning in planarians may be relevant: it suggests that specific chemical changes in planarians may correspond to, and somehow represent, the animals' learning.

But these physiological and biochemical studies of the mechanisms of operant and classical conditioning could not have been conceived, and would have no reason for being, had not students of behavior, like Skinner, first demonstrated the behavioral laws. And investigations of biochemical mechanisms do not, at least now, produce new laws of behavior.[1]

New Kinetic Theory

A simple sociological law, whose relation to operant conditioning is not obvious to me, is a second example of an observed and partly rationalized regularity in biology that is not readily derived from biochemistry. The law is simply that freely-forming small groups

[1] While Skinner does not regard biochemical reductionists warmly, it is curious that he himself is a reductionist on a higher level. Implicit in his writing on the design of cultures and in his Utopian novel *Walden Two* is a belief that the principles of operant conditioning apply usefully far beyond the laboratory situations in which they have been validated; that they suffice for the efficient analysis and control of whole societies. Granting that extrapolation of regularities beyond their range of validation is often useful heuristically, I still find his confidence startling.

of human beings vary in size and relative frequency in a way that is predicted (described) by the Poisson distribution, a well-known probability law.

Sociologist John James recorded the numbers of single pedestrians, pairs of pedestrians, trios, and so forth, that passed by one morning on a sidewalk in Eugene, Oregon. He also recorded the frequency distribution of freely-forming small groups in the afternoon, in spring, in winter, in Portland, Oregon, in Seoul, Korea, in department stores, playgrounds, nursery schools, public picnic areas, and railroad depots.

Years after James published his data, James Coleman, trained as a chemist and now professor of social relations at Johns Hopkins University, observed that the data gathered in nearly all of these diverse circumstances closely matched the truncated Poisson function.

To explain this surprising regularity, Coleman, and later Harvard's Harrison White, set up simple mathematical models. (A mathematical model is a scientific theory that's just starting out.) These models treat people like point masses or atoms without internal structure; random impulses, specified as probabilities, push the atoms into and out of groups. At equilibrium, Coleman and White showed, the "atomic" systems corresponding to crowds of people have the distribution of single atoms, pairs, triples, etc. that James actually observed among real people. (See Coleman's *Introduction to Mathematical Sociology*, 1964, for details.)

A social fact is thus predicted, and given several alternate "explanations," by models that specify nothing about what goes on inside people. These models do, however, involve parameters like "probability of joining a group" and "probability of leaving a group," as well as assumptions about these parameters that are meaningful and measurable only on a social level. Further, only at the social level can the varied models for this one regularity be tested comparatively. Only at the social level, most likely, can the many isolated models of diverse social phenomena be related and integrated into coherent social theory.

The fundamental characteristic of innovations is that they are new. Hence it is tautological to say that, even if it were possible to derive this Poisson distribution from the biochemistry of the brain, it is hard at present to see the use of such a derivation. (Perhaps it would create great new possibilities for social control.) Even so, it is obvious that a biochemical derivation would not have much value for social theory per se.

Bygones

My last example comes from the domain where coherent social theories have been most often attempted and shortest lived: the study of history. I am even more an amateur in history than I am in the other fields I have mentioned, so I will simply cite one book which seems to respect the difficulty of its subject without indulging in mysticism or metaphysics: H. J. Muller's *Uses of the Past* (1952).

Trying to make explicit his assumptions in writing history, Muller says: "The scientific determinist himself must reckon with the power of beliefs, sacred traditions, new ideas, great leaders, simply because they are among the most recognizable, determinable causes in history. Otherwise he is forced back on a kind of mystical, inhuman fatalism that would be fatal to the historical sense. . . . even the determinists and fatalists are always implying that there were real alternatives, and that men made the wrong choice. Whatever we believe in theory, we continue in practice to think and act as if we were not puppets."

The argument for accepting human minds as causal factors in history is not that, in metaphysical Truth, they are, but that at present it seems useful to do so. If we knew every gene of George Washington, and every influence on him, we might or might not be able to account for everything he did. The speculation is idle because we can never know that detail about George Washington or any other historical figure. The available evidence, and the uses we wish to make of it, must channel the growth of our theories.

Conclusion

I argue for the legitimacy of many avenues to the understanding of mind and behavior. The value of biochemical understanding needs no defense because it is obvious and real. But a diversity of approaches needs encouragement. The scientific respectability of biochemical approaches could, if unduly exploited by those who adopt them, make it difficult for other approaches to attract support. Such a partial monopoly on resources would be inefficient because nature, in revealing her secrets, does not respect human standards of scientific fashion for long.

To say with Laplace and Einstein that the whole of history is a trajectory of a point in a higher-dimensional phase-space of physics hardly provides insight into history. To collapse sociology and psychology into biochemistry similarly would dispose us to overlook several interesting aspects of behavior.

Like the three examples above, most of the unanswered questions in biology, and nearly all of them in psychology, sociology, and history, are not merely unsolved problems in "mental biochemistry." Rather they are forests one does not see among the trees of biochemistry; they must, at least at first, be approached on their own, little known, terms.[2]

[2] I am grateful to Andreas Teuber, Mack Lipkin, Jr., and Paul Horowitz for criticizing drafts of this paper.