51. F. Haurowitz, H. H. Reller, H. Walter, *J. Immunol.* **75**, 417 (1955).
52. J. S. Garvey and D. H. Campbell, *J. Exptl. Med.* **105**, 361 (1957).
53. M. Fishman and F. L. Adler, *ibid.* **117**, 595 (1963).
54. P. Maurer, *Proc. Soc. Exptl. Biol. Med.* **113**, 553 (1963); T. J. Gill, H. J. Gould, P. Doty, *Nature* **197**, 746 (1963).
55. M. D. Scharff, A. J. Shatkin, Jr., L. Levintow, *Proc. Natl. Acad. Sci. U.S.* **50**, 686 (1963).
56. R. E. Franzl, *Nature* **195**, 457 (1962).
57. G. J. V. Nossal, A. Szenberg, G. L. Ada, C. M. Austin, *J. Exptl. Med.* **119**, 485 (1964).
58. G. J. V. Nossal, *Brit. J. Exptl. Path.* **151**, 89 (1960); G. Attardi, M. Cohen, K. Horibata, E. S. Lennox, *Bacteriol Rev.* **23**, 213 (1959).
59. J. Robbins, in *Molecular and Cellular Basis of Antibody Formation* (Czechoslovak Academy of Science, Prague, in press).
60. P. Stelos and D. Talmage, *J. Infect. Diseases* **100**, 126 (1957).
61. T. B. Tomasi, Jr., and S. Zigelbaum, *J. Clin. Invest.* **42**, 1552 (1963).
62. Z. Ovary, *Prog. Allergy* **5**, 459 (1958).
63. L. Thomas, in *Cellular and Humoral Aspects of the Hypersensitive States*, H. S. Lawrence, Ed. (Hoeber-Harper, New York, 1959), p. 529.


64. Supported in part by USPHS grant AI-01821-07 and by the Commission on Immunization of the Armed Forces Epidemiological Board, and supported in part by the Office of the Surgeon General, Department of the Army, Washington.


# Empiricism in Latter-day Behavioral Science

Developments in this field, as in other sciences, invite critical review and corrective revision.


V. Edwin Bixenstine


The mood of behavioral science today is sometimes difficult to assess. There is a rising flow of research, as reported in journals, at regional and national meetings, and at a growing number of invitational conferences. We can only be impressed by the energy and the enthusiasm evinced by the participants. A great deal is being done.

On the other hand, there is an undercurrent of perplexity and doubt. While more persons register at our conventions than ever before, attendance at section meetings for the presentation of papers is embarrassingly thin, and little serious attention is given to the research reported. It is a safe bet that there is now a high inverse relationship between mass of reported works and the attention each receives.

There is a general feeling that we behavioral scientists have less confidence today about our grasp of the field than we had 20 years ago. A reviewer recently commented that the days of the grand theory, à la C. L. Hull or E. C. Tolman, are gone. We now bite off small chunks in specialized areas. Some groupings of behavioral scientists are characterized by inability or lack of desire to communicate with any but the insiders. Sometimes these groupings are established as a result of exclusion; for example, J. B. Rhine and his extrasensory perception group inaugurated a journal because editors refused to publish their work. On the other hand, B. F. Skinner's journal has the mark of aristocracy; it is a product of selective inbreeding.

If our work is often unattractive to all but a few of us, we find little to console us in the recurrently critical judgment of the laity. Public acceptance is far from crucial as regards the intrinsic merit of a research project. On the other hand, can we be sure that lack of lay enthusiasm for projects dear to the hearts of behavioral scientists is always a function of lack of public understanding? Might it be that we have psychic investments in our topics and methods quite different from the need to know, understand, and relate? I think that we do.

Events conspire today to impel the scientist into certain forms of research activity. We have had so few "breakthroughs" in behavioral science that we no longer approach research with the faint but uplifting hope that *this* time an important, vital insight will result. Number of published works has more to do with status than the importance of the work has. Journal editors have gradually altered their publication policy: reports must be brief, nontheoretical, and on empirical research that is simple both in design and results. Monetary support from granting agencies is likely to go to someone with a "program." This sounds fine, since it encourages systematic development of an inquiry, but inevitably it also means investing in certain kinds of apparatus and in certain procedural and methodological tools which tend to fix the approach and reduce receptivity to new possibilities. No one receives support who says, in effect, I will study $X$, using procedures $a$, $b$, or $c$, and if my interest in $X$ wanes, I will study $Z$, using procedures $d$, $e$, or $f$.

The value placed on publication, the editorial policies of journals, and the impact of granting institutions converge in effecting what I call "production-line research." This is research which revolves around a gimmick—a fixed procedural tool or method with which the researcher produces a series of studies, using first one set of variables and then another, systematically plotting some "behavioral space" as defined by the operational coordinates used.

## The Special Impact of Skinner

Woven through these developments in the practice of our science is a complementary philosophy and rationale. Disturbed as some of us may be about the way behavioral science is practiced, what is more disturbing is the fact that many others approve, and often talk as if we were approaching the ideal practice of our science. I believe this remarkable complacency can be traced largely to the impact of B. F. Skinner. I suspect that Skinner will emerge historically as one of the most influential behavioral scientists of the mid-20th century. His writings are clear, scholarly, persuasive. He has, as a teacher, great capacity to inspire a loyal following. Add to these attributes


The author is associate professor in the department of psychology, Kent State University, Kent, Ohio. This article is adapted from an address delivered in December 1963 before the psychology colloquium at Kent State University.

the fact that he is a paradox—a man whose preachments are at variance with his practices—and one begins to appreciate the reasons for the kinetic tension he generates in our field.

Let us examine Skinner the paradox. First we note that he *advocates* a "nose-first" style of research. "So far as I can see," he reflects, "I began simply by looking for lawful processes in the behavior of the intact organism" (*1*, p. 80). He proposes that certain unformalized principles actually operate in his, and in most, research. Let us call his first rule the "nose-following" principle: "When you run into something interesting, drop everything else and study it" (*1*, p. 81). Other principles Skinner proceeds to unveil are really subordinate to the first. They are, "Some ways of doing research are easier than others" (*1*, p. 82) (that is, there is no reason to depart from a line of least effort); "Some people are lucky" (*1*, p. 85) (have faith in nose-following); "Apparatuses sometimes break down" (*1*, p. 86) (there are all kinds of lucky accidents—another reason for relying on nose-following).

Conjecturing, conceptualizing, theorizing, in Skinner's view, are expendable if not harmful preoccupations. The scientist's job is to "smell out" the paths of order and lawfulness—he is obviously an explorer rather than a creator or inventor. Nature is most assuredly "out there," quite distinct from the nature of man the explorer. The model of science Skinner has in mind is clearly that of Newtonian physics. If recent post-Einsteinian developments in physics threaten older modes of thought for physicists, they are of no great moment to Skinner.

He sums up his evaluation of the place of theory as follows (*1*, p. 69).

> Perhaps to do without theories altogether is a *tour de force* which is too much to expect as a general practice. Theories are fun. But it is possible that the most rapid progress toward an understanding of learning may be made by research which is not designed to test theories. An adequate impetus is supplied by the inclination to obtain data showing orderly changes characteristic of the learning process. An acceptable scientific program is to collect data of this sort and to relate them to manipulable variables, selected for study through a common sense exploration of the field.

The foregoing passage brings us face-to-face with the Skinner paradox. His preachment is: do not theorize; rather observe, explore, follow your nose. The admission of a measure of "common sense" need not constitute a contradiction. However, in practice Skinner's "common sense" is far from common! Skinner's own temperament is much more inventive than it is curious. He is startlingly creative in applying the conceptual elements of his—let's be frank—*theory* to a wide variety of issues, ranging from training pigeons in the guidance of missiles, to developing teaching machines, to constructing a model society! For Skinner, not to theorize means not to explicitly define concepts apart from the methods and procedures of one's researches. From one point of view this is excellent. The primary purpose of experimentation is to help us *think* and think *clearly* about our universe, often by providng us with a new vocabulary. It seems to me, however, that to say that thought must be expressed only in terms of experimental procedures is to impose an unnecessary restriction.

That Skinner has operated so effectively under this handicap is a tribute to his talent. I wish we were all as capable. We are not. It is unfortunately much easier to do what Skinner says we ought to do than what he does. I suspect this partially explains the ardor and number of Skinner converts: one can apply Skinner's preachments and thereby gain vicariously a sense of participation in Skinner's practices and accomplishments. Only a few, however, can successfully emulate the master as he really is. My belief is that, while we cannot all think with equal brilliance, we can all endeavor to think to the best of our abilities and with the greatest possible freedom from unnecessary fetters. I believe, further, that such unbridled thinking is desirable and scientifically heuristic, and that Skinner's position handicaps his followers in their exercise of thought.

## The Necessity of Theory

You may ask, But why think, speculate, and theorize rather than merely search, observe, and catalog? Let us examine the answers that occur to us.

1) *The nature of man.* Skinner notes that "theories are fun," and so they are. They are valuable because we enjoy them and invest our time and identities in them, and because our commitment to research is often a function of our need to remove our doubts regarding these investments. I believe it is human nature to construe, to constantly push understanding in advance of knowledge. Now, with regard to following your nose, our biologist friends tell us that, in the course of evolution, the olfactory sense was undoubtedly the first significant *distance* receptor. But man seems constitutionally averse to depending on his nose. Recent evidence suggests that most of his "smell brain" is not employed in analyzing smells at all; rather, it seems to be involved in complex emotional and dispositional states. Man will never be content with following his nose; he is wholly oriented toward the farthest possible extension of his perception. To theorize is the logical fulfillment of his nature. It is his true "sixth sense."

2) *Minimization of the trivial.* One stands irresolute before the infinite possibilities for scientific observation: where to begin, what to include or exclude, what methods to use, what elements to study, and so on. Skinner would depend on "common sense" to insure that one chooses the important over the trivial. Sidman (*2*), who shares Skinner's views, appears to be much less sure of "common sense." Sidman concludes a chapter entitled "The scientific importance of experimental data" by apologizing for wandering "far from the topic under consideration," then explains (*2*, p. 40):

> I have discussed several types of data and several reasons for experimentation. The importance of data is usually judged on these bases, but I have tried (despite my undoubtedly apparent prejudices) to make the point that these bases are not in fact adequate foundations for judgment.
>
> What, then, are we to substitute? Science is supposed to be an orderly logical process, not subject to the whims of prejudice and other human frailties of its participants. If science is to use the importance of data as a criterion for accepting or rejecting an experiment, it must have a set of impartial rules within which the scientist can operate when he has to make evaluations. Do such rules actually exist? The answer is no.
>
> If I have led the student out on the end of a limb and left him to shift for himself, I have done so on purpose. For I cannot take him any further. Whether he likes it or not, he will be on that limb for the rest of his scientific lifetime.

So, we have left only the virtue of realism; for Sidman there just *is* no way to minimize the trivial—not even the way of "common sense." "The cumulative development of a science provides the only final answer to the importance of any particular data . . ." is his conclusion (*2*, p. 41). No doubt this fosters a kind of stoic optimism; after all, the future may prove the value of work entirely ignored by contemporaries. Conversely, a favorable contemporary evaluation in no way insures that a work will ultimately prevail. Clearly, this point of view is of great comfort to the "production-line" researcher. It enables him to persevere, grimly hopeful that the future will reach forth and touch him with greatness, transforming the trivial into the profound.

All research is a gamble, and we all hope for the "jackpot." Still, have we no better guide than a gambler's intuition—or "common sense"—regarding where we place our "bet"? I submit that it is an essential function of theory to help us do important, significant research to the greatest extent of which we are capable. We theorize in an effort to go beyond our present knowledge and to divine what we may of a distant future. True, the future alone will reveal what is important. But it behooves us to *pre*view that ultimate revelation insofar as we can.

3) *Interdependency of theory and "accident."* Sidman and Skinner make much of the accident—the lucky happening. Some wit has declared that the first law of science is that anything which can go wrong *will* go wrong. To be sure there never seems to be a dearth of the accidental, especially of the wrong kind. Certain classes of accident are so commonly encountered that we have evolved elaborate mathematical procedures to help us discriminate between the accidental, in the sense of randomness, and the lawful. It appears, then, that accidents happen frequently and to everyone, so the point is *not* just that "some people are lucky," as Skinner facetiously puts it. Rather, it appears that some people have the perceptiveness to see something worth while in "lucky" accidents, while remaining unconcerned with the many irrelevant, "unlucky" accidents.

Take, for example, the accidental discovery of the x-ray—quite similar to the equally accidental discovery of the radioactive nature of uranium. The story is familiar (*3, 4*): in 1896 Henri Becquerel laid a piece of uranium ore in a drawer containing an unexposed but sealed photographic plate and later was surprised to find a "picture" developed on the plate. Similarly, Wilhelm K. Roentgen a year earlier laid some barium platinocyanide crystals on a table near a vacuum tube which he had constructed. He turned on the current and noticed that those distant crystals were glowing! Like Becquerel, Roentgen knew that here was no accident. He proceeded with a number of experiments on the spot and concluded that the vacuum tube must radiate some kind of energy that was spanning the meter or so of space and penetrating glass, wood, metal, flesh, and other substances to cause a fluorescence in the barium platinocyanide.

Sidman holds (*2*, p. 10) that the investigator appreciates the importance of these fortunate accidents because he harbors no prior theoretical convictions to narrow his perceptions.

> When a hypothesis-bound investigator, after carefully designing his apparatus and experimental procedure to answer a specific question, finds that his equipment has broken down in the midst of the investigation, he is likely to consider the experiment a failure. On the other hand, the simple-minded curiosity tester is likely to look closely at the data produced by the apparatus breakdown.
>
> Here, perhaps, is the greatest virtue of the curiosity-testing school of experimentation. Those who have no hypothesis or who hold their hypothesis lightly are likely to be alert to the accidental discovery of new phenomena.

Was it because Becquerel and Roentgen had no hypotheses cluttering up their sensoria that they so quickly apprehended the significance of their accidents? Michael Faraday, as early as 1822, was working on the thesis that light was electromagnetic in character, and by 1864 James C. Maxwell had expressed this idea in mathematical form. Faraday had talked of "rays," and Roentgen immediately called his phenomenon "x-rays." Becquerel was also fully aware of the electromagnetic theory of light. These men can hardly be said to have been without hypothesis, theory, preconception. It was *because* of their concern with the Faraday-Maxwell hypothesis of a basic energy form—a hypothesis which reached fruition in 1905 with the work of Albert Einstein—that they quickly grasped the meaning of these "accidents."

It is curious that it is psychologists who would so misapply certain concepts basic to the psychology of perception. To be sure, a theory or hypothesis functions as a "set," and *wrong* sets can lead to false perceptions. But no perception psychologist advocates the eradication of sets in the interest of achieving freedom from misperceptions. The most unexceptional percept is really an integral mixture of expectancy formed, perhaps, mere moments previously and the always partial, incomplete, and momentary data of the senses. Furthermore, without expectancy or set at this elementary level, no perception would develop. In instances where congenitally sightless individuals achieve vision, they are at first, except for a crude response to differentials of illumination and color, functionally blind. As far as vision is concerned, they have yet to learn what follows what, or to form the elementary sets and expectancies through which the disparate, variable, often chaotic flow of visual sense data is integrated into perceptions.

I submit that there is no antagonism between hypothesis and "accident." On the contrary, it is those events which upend expectancy or which appear to confirm our hypotheses in *unexpected* ways which command our attention. Surely, an investigator without hypothesis would fail to be moved at all by such events. There is an epistemological dilemma at the heart of the rule of empiricism in science. The empiricist's mistrust of reason often takes the form of a constant attempt to reduce everything into elements of the "basic sense data." His guiding conviction is that verifiable or true knowledge resides in the raw quantum of sensation. But this is patently nonsensical. Such a reducton would render us as unknowing as the newly sighted person is unseeing! I doubt that there is any serious danger today of a renaissance of 17th-century rationalism. But we may be discovering that radical empiricism swings too far in another direction. Neither reason nor the senses alone provide a meaningful body of knowledge. Science is reason tempered by observation, and observation impregnated by thought; it is an orderly construction fitted to the world of the senses, an experiential search for a world of order.

## Summary and Conclusion

Let me recapitulate. I am not happy with developments in the behavioral sciences. I wonder whether science does actually stand divorced from the intention, motive, and character of the scientist. I suspect that some of our current philosophies in science, whether so conceived or not, encourage and abet a science of the trivial. I believe we should recognize that, as epistemology, the empirical rule cannot stand alone. The great advances in science are associated with its grand conceptions even more than with its discoveries.

We often mistakenly assume that the rule of objectivity has traditionally divided the character of science from that of the scientist. This is far from true. The objectivity of 18th- and 19th-century science was believed to be a function not so much of methodology and procedure as of the honesty and integrity of the scientist. Michael Faraday had this to say about the matter (*3*, p. 233):

> It puzzles me greatly to know what makes the successful philosopher [scientist]. Is it industry and perseverance, with a moderate proportion of good sense and intelligence? Is not a modest assurance or earnestness a requisite? Do not many fail because they look rather to the renown to be acquired than to the pure acquisition of knowledge, and the delight which the contented mind has in acquiring it for its own sake? I am sure I have seen many who would have been good and successful pursuers of science, and have gained themselves a high name, but that it was the name and the reward they were always looking forward to—the reward of the world's praise. In such there is always a shade of envy or regret over their minds, and I cannot imagine a man making discoveries in science under these feelings. As to Genius and its Power, there may be cases; I suppose there are. I have looked long and often for a genius for our own laboratory, but I have never found one. But I have seen many who would, I think, if they submitted themselves to a sound self-applied discipline of mind, have become successful experimental philosophers.

Are these issues native only to behavioral science? Earlier, I laid at Skinner's door much of the blame for an unreasonable complacency regarding the disposition of modern behavioral science. In all fairness to Skinner, however, it is only correct to acknowledge that the same problem is evident in other areas of science. Skinner appears to be a spokesman, in the behavioral sciences, for one of those famous "Zeitgeists." Let me quote a series of observations by Paul Weiss, a biologist at the Rockefeller Institute, New York City, from a paper entitled "Experience and experiment in biology" (*5*):

> Without imagination one can contrive infinite variations of experimental set-ups, all of them novel, yet utterly uninteresting, inconsequential, insignificant. The mere fact that something has not been done or tried before is not sufficient reason for doing or trying it. . . .
>
> But is not scientific history full of instances of accidental discovery of the unexpected? True again, but he who does expect something will be on the alert even for the unexpected, while he who just ambles, looking for nothing in particular, is prone to miss even the obvious. . . .
>
> We see instruments turning from servants into tyrants, forcing the captive scientist to mass-produce and market senseless data beyond the point of conceivable usefulness—a modern version of the Sorcerer's Apprentice. . . .

Finally, I recommend study of N. W. Storer's article "The coming changes in American science" (*6*). Storer, a sociologist at Harvard University, feels that prior to 1940 the basic currency in science was professional recognition.

This in turn reinforced certain fundamental values—a high emphasis on communication; dedication of the individual and a tendency to work in small, select groups; a spurning of worldly gain and a proclivity for *basic* research. Today, professional recognition is being replaced by more common currencies—money, power, and worldly prestige. The result is a shift of values in science: numbers are no longer small or groups select; communication and professional recognition are viewed as means to gain money, power, and prestige rather than as ends in themselves; basic research is giving way to a kind of imposter with a thinly disguised commercial goal.

I do not know how to resolve the issues in modern science. It seems to me we may be troubled by the embarrassments of too much success. Will we continue to succeed if we do not change our ways? Maybe, but not as magnificently I am sure as we could if we did change them. We cannot do anything about our numbers—nor would I want us to forswear all worldly possessions. We *can* examine critically and revise certain of our guiding philosophies. We can follow a suggestion of P. H. Abelson (*7*) and alter granting procedures so that institutions, as well as individuals, are given support. In the end, however, the major force for change resides in the minds and hearts of scientists who share a concern about science.

### References

1. B. F. Skinner, *Science and Human Behavior* (Macmillan, New York, 1953).
2. M. Sidman, *Tactics of Scientific Research* (Basic Books, New York, 1960).
3. B. Ginzburg, *The Adventure of Science* (Simon and Schuster, New York, 1930).
4. D. Q. Posin, *Dr. Posin's Giants: Men of Science* (Row Peterson, Evanston, Ill., 1961).
5. P. Weiss, *Science* **136**, 468 (1962).
6. N. W. Storer, *ibid.* **142**, 464 (1963).
7. P. H. Abelson, *ibid.*, p. 453.