

Context in Context

BY KENNETH KAYE



The Family (1962), by Marisol Escobar. Painted wood and other materials in 3 sections, 82 5/8" x 63 1/2". Collection, The Museum of Modern Art, N.Y. Advisory Committee Fund.

THE ECOLOGY OF HUMAN DEVELOPMENT: EXPERIMENTS BY NATURE AND DESIGN

by Urie Bronfenbrenner

Harvard University Press, 1979, \$16.50

In January 1971, I was called up for a pre-induction physical. My behavior at the examining center was soon noticed by the sergeant, who provided me with the opportunity to interview a lieutenant, then his captain, and finally by the end of the day—after my fellow pre-inductees had been processed and sent home—I was invited to meet with the major. To our mutual satisfaction, these conversations resulted in an agreement between the Armed Forces and myself to have nothing further to do with one another. We got along fine, though, on that one occasion. The way we behaved together comes back to mind in connection with Urie Bronfenbrenner's new book, *The Ecology of Human Development*.

In order to understand behavior, Bronfenbrenner says, we must know the "setting" in which it occurs. What he means by "setting" is not the situation as described by an observer, but the meaning it has to the participants. That meaning comes to them from experience in settings which may be similar to the immediate one for any of a thousand reasons. It is a consequence of interactions with other individuals who have played roles similar to those of the individuals in this setting. It is affected by their personal goals in other contexts which may be intricately or only tangentially related to this one, and also by the work, goals and values of significant others, present and past, in settings the individuals themselves have only imagined.

An "Ecological" Situation

My army experience illustrates the gist of the "ecological" view that Bronfenbrenner upholds, and it sup-

ports the view. It also illustrates, at a deeper level, what a limited view that is. Each decision I made that day was in accord with the *Handbook for Conscientious Objectors* provided by the American Friends Service Committee. It was a systematic strategy of non-cooperation. Yet I found it impossible to be antagonistic or even rude. The captain and the major seemed to be decent, not violent, men. I quickly realized that they had memorized handbooks of their own ("Section IV-C: Processing examinees who refuse to sign Form 138-L"). I remember sensing two powerful forces: the *institutional* pressure to conform to the values and procedures laid down in my handbook, and the *interpersonal* pressure to communicate as one human being to another.

Unquestionably, the latter was the stronger force. I could see that the captain and the major felt this conflict as well, that each in turn found it difficult to be menacing.

In a different context, hostility would have been easier: If they and a group of fellow soldiers had been dealing with a squadron of pacifists as militant as I knew myself to be, there would certainly have been an exchange of epithets, if not bloodshed. But in a one-to-one situation the captain and I, then the major and I, felt primarily the impulse to be civil, mutually respectful, even cordial.

In my encounter with the Armed Forces, our behavior could not have been predicted by observing any of us in a different context. It was not our "personalities" that made us interact with one another as we did. Nor was our behavior determined by the "stimuli" present—from the memorized handbooks to the G.I. pencils. No psychological laws governed our behavior. Nonetheless, the incident could be interpreted by working backward from it. *A posteriori*, one who had observed each of us and others in similar situations could construct a plausible interpretation of why we behaved as we did.

The problem is that such interpretations are never more than plausible. They restate what happened, they assert that such things generally happen, but they do not tell

us why: by what mechanisms our histories establish our values and our behavioral repertoires, by what mechanisms the social context controls our actions.

So the principle of "ecological validity" this book defends, while reasonable, is not necessarily profound or even helpful. Political philosopher Hannah Arendt stated it more eloquently in *The Human Condition*: "The reason why we are never able to foretell with certainty the outcome and end of any single action is simply that action has no end . . . Men have always known . . . that he who acts never quite knows what he is doing, that he always becomes 'guilty' of consequences he never intended or even foresaw, that no matter how disastrous and unexpected the consequences of his deed he can never undo it, that the process he starts is never consummated unequivocally in one single deed or event, and that its very meaning never discloses itself to the actor but only to the backward glance of the historian."

Hindsight vs. Foresight

Bronfenbrenner, however, is a psychologist, and his avowed goal is not merely glancing backward. His unflagging concern has been public policy in the fields of child care, education and family services: which means *predicting* the effects of practical programs. Here he is looking for ways of making that an easier task by generalizing about the effects of various kinds of environmental manipulations upon human development.

Unfortunately, Bronfenbrenner runs into trouble when he attempts to carve out a more specific argument. Take the important question of child development in divorced families. Reviewing a number of surveys and observational studies comparing children and mothers in intact families with families in which the mother is divorced and has custody of the child, we learn that the latter group of children have more behavioral problems, emotional disturbances and do worse in school; that their mothers are less affectionate, communicate less well with their children, are less consistent in

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their demands and (not surprisingly) are more often tense and depressed. Why? Obviously, Bronfenbrenner concludes, these problems result from the strain of the divorce itself and from the father's absence from the home. He agrees with the author of one of the studies, Mavis Hetherington: "It is critical to develop social policies and intervention procedures that will reduce stresses and develop new support systems for single-parent families in order to offer these families [a] more constructive and fulfilling life style."

Without denying any family the pursuit of a more fulfilling life style, let us examine an alternative hypothesis. Might it not be that tense, depressed, unaffectionate, poorly communicating and inconsistent parents are more likely to get divorced in the first place? In that case their problems do not stem from the divorce at all: they may have been worse before the divorce. Perhaps federal resources would be more wisely spent at the premarital than at the postmarital stage. There is evidence (which, to Bronfenbrenner's credit, but to the detriment of his argument, he also reviews) to support this alternative hypothesis. Unhappily married mothers seem to have all the problems of divorced ones, while widowed mothers do not.

This is not an area in which one can do an experiment, assigning families randomly to stay married or to get divorced. But if one is limited to after-the-fact description, one has to live with those limits. We can use Tolstoy's truths about happy and unhappy families when they seem to be applicable; they are presented as description. On the other hand, quasi-science, including correlational psychology (limited to the statistical analysis of multiply confounded variables uncontrolled by experimental design) contains far less truth than fiction does because it masquerades as explanation.

Generalization without Prediction?

The book draws upon published research, mainly by others, in the fields of social psychology and early education. It is a selective review, clearly written, often interesting. But what

the review generates is a set of 50 "hypotheses" worded in the following manner: "*The developmental potential of a setting is enhanced to the extent that there exist direct and indirect links to power settings through which participants in the original setting can influence allocation of resources and the making of decisions that are responsive to the needs of the developing person and the efforts of those who act in his behalf.*"

This is not what the opening pages have promised us, "a new theoretical perspective for research in human development." A theory must be able to generate testable hypotheses: specific predictions, not vague circularities.

The enterprise most of this book commends to us is a hopeless, self-contradictory pursuit. Bronfenbrenner recommends a quest for generalization, but denies the validity of general laws which are based on specific facts. Despite disclaimers, the book attaches virtue to broad, all-inclusive accounts of phenomena and suggests that one cannot usefully zoom in on particular aspects while ignoring others. While granting the need for both kinds of research, Bronfenbrenner is as quick to condemn the "strange situations" constructed by experimenters as he is to extol and to base policy recommendations directly upon quasi-scientific "ecologically valid" studies that tell us only what seems to have happened, under apparent conditions.

Social Relevance vs. Scientific Rigor

Social relevance gets higher marks, in Bronfenbrenner's book, than scientific rigor. And he unfairly suggests that rigorous psychological research is molded after the physical sciences; by implication, experimentalists are depicted as believing that men, women and children can be studied like machines or that they interact according to the laws of planetary motion. Like many of our colleagues, he would reject my earlier use of the word "mechanisms": It has become fashionable instead to assume that there are unfathomable, magic forces whose outcome under various conditions can be guessed from experi-

ence, but whose actual workings are outside the realm of science.

Meanwhile, the reality is that our scientific methods in psychology are modeled after the microscope and the catheter, not the analytic balance or the cyclotron. Slowly, the biological model is advancing our understanding of our nervous systems and our social systems. We do not deny the specialness of being human when we attempt to turn upon our own species the instruments and the methods of logical inquiry that have yielded an understanding of other organisms.

Furthermore, it is not true that scientists perform experiments because they believe the organisms they study will always behave in their natural habitats as they do in the laboratory. We do so in order to isolate features of those natural habitats one by one. Newborns fed under standardized conditions before a video close-up lens allow me to analyze the effects of particular kinds of maternal behavior upon their sucking pattern. The original intuitions about what aspects of the feeding situation might be significant for the developing mother-infant relationship—in this case, the built-in turn-taking between sucking and jiggling which prefigures later forms of dialogue—come from observations of natural feedings in all their diversity: breast and bottle, in public and at home, in modern as well as traditional cultures. But those intuitions would remain just intuitions if we could not isolate the phenomenon from contextual factors.

Whatever translates easily, and soon, into real-life applications is not automatically valid research. It is often invalid just because it does so: because it contributes nothing to our understanding. Worse, it competes against more basic research for limited government and private funds, and its undeserved advantage in this competition is fueled by the kind of rhetoric found in this book. The great danger is that methodological principles become dogmatic, and research proposals are judged for their avowed allegiance to those principles rather than for the appropriateness of their methods to the theories they are testing. □