interesting contrast to Donald Black's *The Behavior of Law* (1976), in which more general propositions about the operation of law are advanced. It was necessary for Black, however, to rely on data collected by a variety of ethnographers representing many different theoretical approaches. Like many comparative theorists before him, he was unable to exert much control over the data that were available to him. As the comparative study of legal behavior advances in the years ahead, we can hope for a greater synthesis of the comparative and ethnographic approaches. Nader's students have demonstrated the utility of cumulative efforts, of putting similar questions to different societies, of collaboration in preparation, collection, and analysis of data. Yet at the same time the book shows us that we have only begun to move anthropology from its particularistic descriptive efforts (generously called at times "theories of particular societies") to general theory that is firmly grounded on carefully collected, systematic data.

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Children and Ethnographers

Transformations. The Anthropology of Children's Play. HELEN B. SCHWARTZMAN. Plenum, New York, 1978. xviii, 380 pp., illus. $29.50.

Question: what do children making mud-pies have in common with anthropologists doing ethnographic fieldwork? The answer, according to the author of this book, is that they are playing. If Helen Schwartzman's statement seems a bit strained and far-fetched, that is because we are not supposed to take it literally; we are, however, supposed to take it seriously. Schwartzman is dealing in metaphors, and what she hopes to emphasize with this one is that anthropologist and child alike are engaged in acts of invention and interpretation. Both of them, she says, are "continually constructing and transforming the contexts in which they exist in their efforts to make sense, and sometimes nonsense, of the worlds in which they find themselves" (p. 1). There is nothing either novel or startling about this observation (ethnographers, after all, have understood for quite some time that theirs is an interpretative, and, in this sense, an inventive craft), and I am not persuaded that equating fieldwork with play, even metaphorically, is an effective way of driving home the point or heightening its impact. But Schwartzman has no such qualms. Indeed, she builds upon her metaphor to make a considerably grander claim. To wit: just as the discipline of anthropology has transformed the study of play, so, too, the study of play can transform the discipline of anthropology.

To support this claim, Schwartzman embarks upon a lengthy survey of studies dealing with children's play. She has two main objectives: to describe in summary form the research that has already been accomplished, and to illustrate, by means of a historical analysis of this work, the manner in which anthropologists' conceptions of play have been shaped by different theories of culture and cultural development. On both counts, Schwartzman does a thoroughly commendable job. The body of writings on children's play is enormous, and she has organized it (geographically as well as chronologically) extremely well. Similarly, her comments about the effects of

"Children's play, Chicago." [From *Transformations*]

"English children playing fox and chickens." [Photo by A. D. Webb, from I. and P. Opie, *Children's Games in Street and Playground* (Oxford University Press, 1969), reproduced in *Transformations*]

changed views of culture on models for the analysis of play are, with very few exceptions, sensible and informative. I was surprised that she did not discuss the work of Alfred Schutz on mimetic forms of play, and I think her treatment of the linguistic component in children's games is oversimplified. But these are minor complaints, and they are not intended to detract from the many merits of Schwartzman's survey or the useful slant she provides on aspects of the history of anthropological thought.

Toward the end of *Transformations*, Schwartzman moves on to grapple with more abstract issues, including, most notably, difficulties that arise in trying to formulate adequate definitions of play. Dozens of definitions have been proposed and debated, but none of them, she finds, is wholly satisfactory. A basic problem, pointed out some years ago by Gregory Bateson, is that play is not merely a class of activity but also, and perhaps more important, a context for activity. In other words, we identify behavior as "playful" by virtue of a distinctive cognitive orientation toward events, a particular framing of social reality. And framings, as Erving Goffman has shown, can be easily superimposed, thus making it possible for persons to play at playing or even to play at playing at playing. Add to these complexities the well-documented fact that play frames may exhibit pronounced cross-cultural variation (they may vary strikingly within single cultures as well) and it is not surprising that operational definitions of play are in short supply.

These considerations lead Schwartzman to draw the following conclusions:

> The study of play, perhaps more than any other topic, requires that researchers adapt themselves to the character of their subject and not the reverse. Researchers who have a compulsion for organization, predictability, and exacting definitions and methodologies produce only illusory theories and explanations, which distort play and fool only researchers. On the other hand, investigators who are more tolerant of disorganization, unpredictability, and loose and fuzzy definitions are more likely to produce theories that allude to play (and that is the *best* we will ever do) and help to elucidate the nature of foolishness [p. 329].

Here, Schwartzman runs into trouble. "Fuzzy definitions" and "loose methodologies" may be useful for certain purposes, but theories that only "allude" to what they purport to be about do not constitute theories at all. Statements of this kind are usually the results of exploratory forays; they are preliminary characterizations, or descriptions, or simply working hunches. Schwartzman implies that a developed theory of play does not yet exist; but this does not imply in turn that such a theory is in principle beyond our reach, or that it must (or should) be grounded in allusion. One either has a theory *of* something, or one doesn't. And that applies to play as much as to anything else.

Which brings us back, albeit circuitously, to where we began—to mudpies, ethnographers, and metaphors. Schwartzman believes that anthropologists are well prepared to construct theories of play because their approach to the interpretation of cultural phenomena has always been "loose" and "flexible." Fair enough. But does this mean that ethnography itself is usefully likened to play? I think not, especially if it is true, as Schwartzman claims, that a basic ingredient in play is foolishness. There are those, no doubt, who regard ethnography as exactly that, but I, unabashedly chauvinistic in this regard, would disagree. Whatever ethnography is, it consists in a disciplined attempt to fathom other people's understandings of themselves, and to make explicit how it is that these understandings give shape, pattern, and meaning to their behavior. What is called for is a special kind of "translation," a sensitive but principled bridging of contrasting cultural worlds. For me, the "ethnography is play" metaphor fails entirely to capture this fundamental element, and that is one of the reasons I find it less than helpful. In science, as in literature, some metaphors work better than others. This one, I think, anthropology can do without—even if it means that the discipline must wait a while before being transformed.

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Processes of Science

Laboratory Life. The Social Construction of Scientific Facts. BRUNO LATOUR and STEVE WOOLGAR. Sage Publications, Beverly Hills, Calif., 1979. 272 pp., illus. Cloth, $15; paper, $7.95. Sage Library of Social Research, vol. 80.

Laboratory Life is a fresh, stimulating, and useful departure from the often rarefied study of the philosophy and sociology of science in favor of what the authors call an "anthropology of science." By refusing to accept the self-characterizations of scientists as more than another datum, the authors acquire insights available only to field observers in close daily contact with a tribe of scientists in their most sanctified workplace, the laboratory. They reveal the laboratory as an institution in which practitioners are constantly engaged in monitoring a variety of "inscription devices" to which they feed carefully prepared animal tissues, chemicals, or numerical data from other inscription devices and from which they derive charts, graphs, and pictures. Once obtained, these products replace all the carefully prepared offerings and are themselves studied in an effort to extract new information from a background of artifacts and previously identified patterns. The aim of the workers in the laboratory is to publish information thus filtered from their inscriptions in order to acquire credit, or credibility, with other practitioners and in order to reduce the mountains of data that constantly threaten to overwhelm and obscure the order painstakingly created from masses of noise.

Making statements about new information is a social endeavor in which the authors find that epistemological purity counts little, whereas anticipating objections and raising the costs of disagreement by carrying out further tests are highly valued. These additional tests provide supporting evidence that makes it possible to strip a statement of the circumstantial context in which it was created. Scientists, the authors find, assess the claims of colleagues according to the credibility built up by those colleagues, the reliability of the inscription devices employed, and the stakes involved in accepting a new statement as fact. Ironically, facts are accepted as such because of the specific conditions under which they were created, but in becoming facts they are stripped of reference to context and deemed equally true for all situations.

The scientists in Roger Guillemin's laboratory at the Salk Institute, which was the site of the anthropological fieldwork (conducted by Latour), were constantly endeavoring to propel their ideas and proposals toward the status of accepted facts. Their business was the "construction of reality." By monitoring the behavior and speech of these scientists the authors explore how facts are constructed in everyday work and demonstrate, by tracing the "deconstruction" of some facts, how reality is socially created and why it is misleading to speak of the "discovery" of scientific facts as if they are independent of the social conditions that generated them. The two chapters in which the authors explore the construction of the fact of the thyrotropin-releasing factor and the microprocess of fact construction in every-