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-

day discourse are particularly rich in observations that establish the power of their interpretation and the opportunities presented by this approach.

Unfortunately the work as a whole will be difficult for outsiders to the sociology or philosophy of science to penetrate. While it makes a real contribution toward the demystification of science, extended passages of sociological shop talk, containing frequently telling and usually judicious critiques of the ideas of other sociologists of knowledge, clutter the work. As a result, a major contribution to the specialty literature that deserves and could reward a broader reading audience will, I'm afraid, fail to attract or excite it as it could.

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The Work of Davy and Prout

The Transcendental Part of Chemistry. DAVID M. KNIGHT. Dawson, Folkestone, Kent, England, 1979 (U.S. distributor, Archon [Shoestring Press], Hamden, Conn.). viii, 290 pp. $19.

Despite the title, this is not the chemical equivalent of one of those recently fashionable books on the affinities between contemporary physics and eastern philosophy: nor is it a study of Kant's influence on chemical theory, although the latter topic is worthy of serious historical consideration. Rather, Knight has presented us with an essay on some of the more philosophical and speculative aspects of chemical theory in the 19th century. These latter include the possible varieties of the states of matter, the number and the relationship of the chemical elements, and the existence and status of atoms. The genre is intellectual history; the accent and emphasis are distinctly British.

In one respect, Knight's book can be read as a skeptical and slightly insular commentary on what are generally seen as the progressive triumphs of continental European chemistry in the epoch between Lavoisier and Mendeleev. Not surprisingly, its heroes are Humphry Davy (to whose work the title alludes) and William Prout (of hypothesis fame). The career of the former is portrayed, not incorrectly, as a succession of sorties against the weaker parts of the structure of Lavoisier's chemistry. Beginning with Davy's early speculations on those contradictory and ambiguous Lavoisierian elements heat and light, Knight shows in an excellent second chapter how the discovery of voltaic electricity at once complicated the problem of the "imponderables" and provided chemistry with a powerful new tool of analysis. It was as the most successful exploiter of this analytical instrument that Davy was to earn enduring fame for the isolation of the first alkali and alkaline earth metals. Paradoxically, it was not Davy's ambition to add to the number of known elemental substances in this way; rather, he had hoped to reduce drastically the number of Lavoisier's "simple substances" by means of electrolytic analysis. Knight also chronicles Davy's assault on Lavoisier's oxygen theory of acids in his investigations of "oxymuriatic acid," which Davy renamed chlorine, having demonstrated conclusively that the gas contained no oxygen. This potentially threatened the collapse of Lavoisier's whole theoretical structure, since it bore also on the theory of base and salt formation; but Davy's Swedish contemporary Berzelius saved the day by transforming the latent dualism of this aspect of Lavoisier's chemistry into the overt dualism of his own electrochemical theory.

The new ontological status that Dalton's atomic theory postulated for Lavoisier's provisional and operationally defined elements did not succeed in diminishing the faith of those who believed in a much less prodigal description of the material universe. Now, however, the faith took on new quantitative forms. In the wake of the first calculations of elemental atomic weights, the English physician William Prout developed his hypothesis that all of the elements were multiples of hydrogen. The Scottish chemist Thomas Thomson staked a professional reputation by endorsing the speculation with new analyses, but lost. Again the indefatigable Berzelius proved nemesis to simplifying British assumptions. Prout's chimera was not so readily dismissed, however: more primitive forms of matter than hydrogen could be, and were, postulated to account for the inconvenience of fractional atomic weights. Also, as Knight demonstrates, contemporaneous developments in organic chemistry like the radical theory, the concept of homology, and the discovery of isomerism helped fuel the hope that an irreducible stoichiometry was not the last word on the structure and properties of matter. The final chapter shows how an evolutionary interpretation of the periodic table reconciled some to the apparently unending proliferation of chemical species.

Knight's own book prompts one further speculation: perhaps it was the relatively slow professionalization of chemistry in Britain that allowed the speculative or transcendental part of chemistry to persist there while in Europe pressures of academic advancement and the development of research schools helped foster a more cautious empirical and instrumental approach. To test this we need more detailed studies of Continental chemistry in this period. But this book should reach beyond the specialist audience in the history of science; it should also prove useful to adventurous teachers of basic chemistry who wish something more than the historical platitudes characteristic of introductory texts. In this latter regard, Knight's essay has one small flaw: although he rehearses a full cast of characters who challenged the orthodoxies of 19th-century chemistry, he has a tendency to turn the script into a prologue for 20th-century developments. He could well have taken a cue from W. S. Gilbert, who realized that Whigs and Tories both must learn to dim their glories.

Readers are advised that this "book" is produced photographically from typescript and contains an unusual number of typographical errors. These do not detract unduly from the author's very readable style.

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