BOOK REVIEWS

Sandra D. Mitchell, ***Unsimple Truths: Science, Complexity, and Policy***. Chicago: University of Chicago Press (2009), 160 pp., $27.50 (cloth).

In *Unsimple Truths: Science, Complexity, and Policy*, Sandra Mitchell argues that philosophy of science should take better account of several kinds of complexity: "multilevel organization, multicomponent causal interactions, plasticity in relation to context variation, and evolved contingency" (21). The main conclusion about multilevel organization—that is, hierarchies of parts and wholes—is that the higher-level wholes usually cannot be reduced to the lowest-level parts. Mitchell links multicomponency and plasticity, by pointing out that interactions between multiple parts of a system often enable it to remain functional despite loss of particular parts. She contends that this fact should lead us to reject "modularity" as a criterion of causation. In response to evolutionary contingency, Mitchell urges us to think of natural necessity and contingency as matters of degree, rather than as an either-or dichotomy. Finally, she argues that in our complex world, certain ways of representing uncertainty and managing systems often work better than traditional methods: entertainment of "scenarios" rather than use of standard decision theory and "adaptive management" rather than once-and-for-all "solutions" to problems.

Mitchell introduces some interesting science in order to motivate her philosophical claims, such as recent work on causes of depression. And she does well to bring scenario analysis and adaptive management—important, recently conceptualized tools for understanding and responding to escalating ecological crisis—to the attention of philosophers of science. With regard to natural necessity and contingency, she offers a plausible diagnosis: philosophers of science, starstruck by achievements in formal logic, tried to import a dichotomy that works perfectly well in that context into the realm of scientific generalizations, where it turns out to be inappropriate. She also proffers an intriguing picture of nested relationships between logical, physical, chemical, and biological possibilities and actualities (fig. 3, 59). It made me wonder, though, why this figure has biological possibility nested not just within chemical possibility but also within chemical actuality. Many evolutionary novelties involve chemical novelties—for example, when plants evolve new classes of insecticidal compounds. This implies that unactualized chemical possibilities are involved in some biological phenomena that could have occurred but did not.

With regard to reductionism and modularity, I found the book's arguments unconvincing. And it does not engage with other well-known arguments against reductionism that, to me at least, are convincing or with the power dynamics that continue to sustain reductionist dogma despite its apparent lack of any decent intellectual or pragmatic rationale. Below, I shall focus on issues related to reductionism, that is, the view that higher levels of organization can and should be reduced to (i.e., explained solely in terms of) lower levels.

One target of Mitchell's critique is the attempt to reduce various aspects of human behavior and physiology to genetics. As she notes, mucking around for answers at the genetic level often turns up only "messy, murky causal relations" (3). Yet Mitchell fails to ask why billions upon billions of government dollars still rain down on genetic research, while inquiries, for example, into the environmental causes of disease—one kind of study at higher levels of organization—hobble along on starvation rations (S. Steingraber, *Living Downstream: An Ecologist Looks at Cancer and the Environment*, New York: Addison-Wesley, 1997). In the absence of any valid justification for such disparities, I have come to conclude that vested economic interests and political ideology underlie them.

Mitchell does assert that "a bet-hedging strategy might be appropriate for distributing funds to different areas of research. Given the state of uncertainty regarding genes, environmental influences, and other components of the complex etiology of adult depression . . . it is at least unwise to put all one's eggs in a single basket" (104). Yet while this truism is true, it is not very interesting without saying more about how many eggs are to go into the gene versus environment baskets. Are they roughly equal numbers or 11 for genetics and one token egg for the environment? The former would depart radically from the status quo, while something like the latter, or worse, probably already obtains. Mitchell's recommendation does not entail any change to current funding practices.

The book's more general antireductionist conclusion is equally tepid: "a nonreductionist approach (in the sense of not focusing exclusively on the most basic physical components of a system) . . . is necessary" (9). Even the most infinitesimal deviation from reductionism would conform to this call. Bolder stances are long overdue. For example, consider that for most systems under study, the environment (i.e., everything outside the system) is much vaster than the set of its parts (everything inside it). We might therefore expect that in general, the physical, biological, or social environment exerts a greater causal influence on the behavior of the system and, therefore, has greater explanatory import for that system than do its components. In other words, it may be not only that reductionism is false but that something like its opposite comes closer to the truth.

While Mitchell laments the failures of reductionism, her arguments miss important virtues of alternative approaches. For example, as the title of the book implies, she maintains that to get at the truth, we must formulate theories that are more complex than reductionistic theories are. Yet one important reason to invoke higher-level causes is that the resulting explanations often unify and simplify what seems like only messy murkiness at the lower level. For instance, Mitchell points out that the conjunction of a particular allele and stressful life events does better at explaining depression than any extant genetics-only stories do. But this explanation—one gene and one particular class of life events—seems much simpler than any purely genetic explanation involving even a moderate number of genes would be.

So far, I have mentioned two antireductionist arguments not entertained by Mitchell: a size-of-the-environment argument and a parsimony argument. (See G. M. Mikkelson, "Complexity and Verisimilitude: Realism for Ecology," *Biology and Philosophy* 16:540–42, for a more specific and fully developed version of the latter, as applied to the science of ecology.) But she also does not engage other, better-known arguments, like the one elaborated by Garfinkel (*Forms of Explanation*, New Haven, CT: Yale University Press, 1981): that due to multiple realizability, reductionistic explanations often miss the point, that is, answer the wrong question. Instead, she tries to refute Kim's proreduction argument, which she lauds for its "high standard of clarity" but in the same paragraph admits "may be somewhat challenging to follow" (26–27). Mitchell rebuts Kim by granting his metaphysics but pointing out that lower-level "descriptions are always partial" (33) and, therefore, cannot completely explain what happens in the world. Yet this point has no bearing on the reductionist ideal, which is always, as much as possible, to seek explanations of higher-level patterns in terms of lower-level properties, rather than vice versa. A dogmatic reductionist could insist that lower-level descriptions are less partial than are higher-level descriptions.

Overall, *Unsimple Truths* manifests a major strength, and a major weakness, of much recent philosophy of science. The strength is its engagement with real science—and not just physics—as an antidote to the ill-grounded speculation found in some earlier philosophy of science and much current work in other areas of philosophy. In this sense, philosophy of science has done well at the "naturalistic" part of "normative naturalism." However, I would argue that it often falls short on the "normative" side. For example, philosophers of science including Mitchell have not gone nearly far enough in challenging the reductionistic doctrines that still pervade science.

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Peter Dear, ***The Intelligibility of Nature: How Science Makes Sense of the World***. Chicago: University of Chicago Press (2006), xii+242 pp., $27.50 (cloth), $17.00 (paper).

This is a historian's book about what science is and does. Short and concise, and written at an introductory undergraduate level, it addresses broadly philosophical questions about the nature of science through an investigation of some key episodes in its past.

The book is divided into six short chapters, with an introduction, a conclusion, and a helpful bibliographical essay with suggestions for further reading. The chapters take us from the early seventeenth century up to the first part of the twentieth century, discussing examples taken from different sciences and different scientists. Chapter 1 ("The Mechanical Universe from Galileo to Newton") begins with the mechanist universe of Descartes, Huygens, and Newton. (Despite the title, Galileo has little role to play in the discussion.) Peter Dear contrasts the mechanist conception of the world, as found in Descartes and Huygens, modeled on the machine—where everything is explained in terms of size, shape, and motion—with the Aristotelian conception modeled on the organism. The mechanist model, in turn, is contrasted with Newton's world of active forces.

In chapter 2 ("A Place for Everything: The Classification of the World"), Dear turns to taxonomic approaches to nature in the late seventeenth and early eighteenth centuries. Unsurprisingly, he treats the conflicts among different kinds of classificatory schemes for plants and animals in scientists such as Ray, Linnaeus, Buffon, Jussieu, and Cuvier. But in addition, he also discusses a taxonomic scheme proposed by Étienne François Geoffroy in 1718 for representing certain observed chemical facts about the reactions of metals in various solutions and William Herschel's scheme for classifying different kinds of celestial objects. Chapter 3 ("The Chemical Revolution Thwarted by Atoms") deals with chemistry in the eighteenth century, focusing on the difference between the quantitative and phenomenological approach Lavoisier took, emphasizing exact observation and measurement, and the approach taken by Dalton, which emphasized physical models.