the technical work Fredkin did and elaborates on thinking that lies at the base of Wolfram's world view. Fredkin especially stressed the idea that everything is a computation, and that the universe is a digital computer. Wolfram is mentioned only once in Wright's book, when disagreements between the two scientists are emphasized. Thus, neither Gleick's nor Wright's independent studies supports possible claims that Wolfram played a major role in making any "new kind of science" in the early 1980s or before.

In his preface, Wolfram says that the new kind of science was discovered in the period since 1991 and brought together in this volume. So we must look for it in the concepts, calculations, and theorems described here for the first time. I found in Wolfram's book interesting things that were new to me. Wolfram mentions (but does not display) a 1994 proof by Matthew Cook that one-dimensional automaton number 110 is a universal computer: It could do any calculation that could be performed by a Turing machine. This unpublished proof identifies a particularly simple automaton example of a universal Turing machine, which I think is the simplest example of such a machine identified so far. Some data included in *A New Kind of Science* were also new to me, especially the counts of the proportion of automata of various kinds that fall into each of the four classes. It is interesting to see how the simplest systems are capable of generating chaos and universal behavior and to see how variation of a parameter could give rise to a transition from mostly repetitive to mostly chaotic behavior. But these data mostly serve to illustrate well-known ideas.

Chapter 9 of *A New Kind of Science*, "Fundamental Physics," contains provocative speculations related to the way automaton models might incorporate quantum theory and gravity—via random network models and path independence. These speculations are, I think, new. The view that the universe is an automaton is due to Fredkin. But, the specific elements in Wolfram's speculation emulate previous two-dimensional quantum gravity theories and earlier work on integrable systems. This chapter describes a partially formed idea—exciting, but not yet science.

The book's longest discussion, in chapter 12, "The Principle of Computational Equivalence," roughly puts all chaotic calculations in the same category. This is a restatement and extension of Wolfram's 1980s idea that classifies automaton outputs into four categories. So far the classification has proved neither subtle nor fruitful.

The remaining apparently new material in the book is speculative and appears to be even less worked out than the examples just mentioned.

From my reading, I cannot support the view that any "new kind of science" is displayed in Wolfram's new book. I see no new kinds of calculations, no new analytic theory, and no comparison with experiment.

Per Bak's book, *How Nature Works* (Copernicus, 1997), covers subjects similar to those of Gleick and Wolfram and looks specifically at automaton studies and at Wolfram. Bak's judgment is that "more than anyone, Stephen Wolfram . . . pointed out that these simple devices could be used as a laboratory for studying complex phenomena." But he also said that "Wolfram never produced any theory of cellular automata." And that is where the subject stands to this day.

Facing Up: Science and Its Cultural Adversaries

Steven Weinberg
Harvard U. Press, Cambridge, Mass., 2001. $26.00 (283 pp.).
ISBN 0-674-00647-X

It is almost unfair for me to be reviewing *Facing Up*, a collection of essays by Steven Weinberg, since I consider its author a very important scientist–intellectual. Furthermore, I admire his writing and agree with 99% of his opinions. Happily for this review, the 1% on which we differ is central to this collection and will be discussed at length later.

Facing Up consists of about 20 essays composed over a decade and a half for various occasions, each with a brief preface in which Weinberg describes the inception of the essay and occasionally the response that followed it. More than half of this 250-page book was written for *The New York Review of Books*, with the luxurious length and full airing of controversies typical of that publication. In fact, some of the chapters here are answers to published responses in that journal.

Aside from a few very brief personal essays (Weinberg's opinions on Zionism, an account of the circumstances surrounding his greatest discovery), and one longish discussion of the present state of the science of cosmology, the topics upon which he chooses to divagate are fairly closely related. They define the progressive development of his own scientific philosophy—which for any theoretical scientist must relate to his personal philosophy as well.

Three main themes appear again and again: his own variety of reductionism (where I seem to have been nominated as a "cultural adversary"), his opposition to those who insist that science is socially constructed, and his insistence (to use his words upon being asked to join a dialogue on science and religion) that he is "all in favor of a dialogue . . . but not a constructive dialogue."

In several of the essays Weinberg elaborates on a theme that was central to his earlier book, *Dreams of a Final Theory* (Pantheon, 1992). That book was written as an apologia—in terms of the centrality of elementary particle physics in the scientific world picture—for the enormous expense of the Superconducting Super Collider (SSC).

In one of the essays in *Facing Up*, written for a Newton tricentenary event, he celebrates the standard model (and its possible successors) in physics as embodying the Newtonian ideal of discovering universal laws that apply always and everywhere. Here he sees the progress of science since Newton's time as extending unification to more and more of the world around us, until, at present, we can segue into scientific explanations of all of the facts of nature. So far, one can hardly differ with him. In the earlier book, *Dreams*, he developed the scheme of imagining a chain of "why" questions that would lead from any phenomenon in the observable universe, and end up eventually at the universal laws that reveal the dynamics of the elementary quantum fields composing the universe. "Therefore," he insists, the search for the laws of those elementary fields has some greater intellectual urgency (and may cost more money) than other sciences.

Another essay in *Facing Up*, "Nature Itself," was written as his contribution to the final, overview chapter of the three-volume history, *Twentieth Century Physics* (AIP Press, 1995), edited by Laurie Brown, Abraham Pais, and Brian Pippard. The other contributors to that chapter were John Ziman and myself. In this concise essay he views the century as essentially a grand progress toward the standard model and the Big Bang cosmology, thus ignoring much of the historical material. To my surprise, I learned from the introductory remarks to that essay that he had asked to write it in case the uncontradicted opinions in my contribution might corrupt future readers. For my part, I attempted to make up for what I saw as a weakness of the history by tracing what had happened to the profession of physics in the century: status, funding, public acceptance, and comprehension. This is the part, I suppose, to which Weinberg refers in the above-mentioned remarks as "much good sense." But a brief passage in my contribution pointed out that a problem with public comprehension is that the world we actually see, of human relations, stars, storms, and rocks, resembles not at all the quarks and leptons from which it is made, and, therefore, that there must be many layers of emergence of novel properties for which the standard model alone doesn't afford an explanation. Perhaps provocatively, I called emergence the "God Principle," in contrast to the phrase "God Particle" used by a particle colleague.

While Weinberg's reductive arrows are very comforting, in that they give us confidence in our explanations, the structure of science is more web than tree. There are broad generalizations—irreversibility, broken symmetry, adaptive evolution are a few—that cross-connect the sciences and don't necessarily involve the detailed nature of the underlying fields. Thus I don't accept the opinion expressed by Weinberg in another essay, "Night Thoughts of a Quantum Physicist," that there are "no freestanding principles of . . . biology or economics." My view is closer to E. O. Wilson's consilience than to Weinberg's reductionism.

Another article discusses Weinberg's testimony on the SSC before a Senate committee, ending with an amusing exchange with a committee member that illustrates the futility of such testimony. In a similar instance, when both of us testified on the SSC, my testimony criticized the claims of spinoff from particle research, such as magnetic resonance imaging. One of the senators came down among us later and thanked Steve profusely for the good that MRI diagnosis had done for a close relative.

With the essay titled "Night Thoughts . . .," the author begins the assault on the idea that science is a social construction, not a description of reality. This is a second major theme of the book. In the course of covering it, he discusses science historian Thomas Kuhn's seminal contribution to this aberration and comments on Alan Sokal's famous hoax. Weinberg's discussion of historical issues in science is a major value of this book, particularly with respect to his contention that Kuhn's version of a scientific revolution is too extreme and too special even to describe the birth of quantum theory. There is by no means the complete disjunction of attitudes before and after a revolution that would allow the sociologists to claim that the two contradictory sets of scientific views are both right, each from its own internal points of view. This is an attitude that Kuhn did little to dispel, although privately he might disavow it. (Nonetheless, Kuhn was capable of "denying the existence of objective reality.")

What Kuhn did was helpful to scientists themselves in understanding what they do and the role of the paradigmatic instance in changing the community's mind. But I don't think the radical extensions of his ideas have been of any value. These take many forms and are often disguised by protestations of belief in scientific fact. But all such protestors in the end insist that science is not an interconnected unity but a collection of separate fantasies produced by the canonical groups of DWM's (dead white males). I would add, for my own part, that the admission that the sciences are interconnected at every level, not just through the underlying equations of physics, is a strong defense against such nonsense.

Weinberg's discussion of Sokal's send-up of sociological pretensions was characteristically eloquent and insightful. One piece ends with a typically pointed remark: "It seems to me that Derrida in context is even worse than Derrida out of context." Enough said! (Jacques Derrida is credited with originating the theory of "deconstruction".)

Building on a brief remark in his *The First Three Minutes* (Basic Books, 1977), Weinberg here continues in several articles the discussion of the role of religion in science and vice versa. In *Facing Up*, he calls "the preconceptions of philosophy" (it is clear from context that he includes religion as well) ". . . modern science's chief adversary." He makes it clear, in his lucid prose, that anyone, either as a Weinbergian reductionist or a Wilsonian consilient, who believes in modern science as a unified structure will be hard put to allow for revealed religion in his philosophy. However, there are scientists who maintain belief in a god who "set the dials" of the standard model so that we can exist: Wheeler's "Anthropic Principle." As Weinberg and other cosmologists remark, the reasoning behind even that is shaky, and such a god seems unlikely to have strong opinions about the behavior of beings very recently evolved on a tiny planet on the edge of one of the hundreds of billions of galaxies.

Facing Up was, minor plaints aside, a delight to read. It is satisfying to find one's beliefs about the nature of science, and indeed of life, expressed with such clarity and eloquence.

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Crystals, Defects and Microstructures: Modeling across Scales

Rob Phillips
Cambridge U. Press, New York, 2001. $130.00, $47.95 paper (780 pp.). ISBN 0-521-79005-0, ISBN 0-521-79357-2 paper

Although nanoscience gets all the publicity, many of its thornier intellectual issues actually concern multiscale modeling. Indeed, this is one of the foci of the recent NSF program solicitation for nanoscale science and engineering. Modeling across scales goes to the heart of how we do physics, connecting different levels of description economically, faithfully, and above all, predictably. Some concrete examples: Can we make continuum models of real materials, simultaneously capturing the elastic properties at long scales and the atomic forces that seem to govern the motion of crack tips? Can we make predictive models for microstructure formation that take into account mechanisms of heat transport on long length scales and the details of atomic attachment kinetics at short scales? And how does microscopic disorder determine the large-scale mechanical properties of materials? These are all difficult questions, because many different length scales need to be treated or resolved simultaneously. In the ideal world, there would be a single protocol capable of incorporating the level of description and degree of resolution appropriate for each point or region in space. Hence, almost any theoretical framework hinges on the notion of adaptivity: In the context of numerical calculations, this means adaptive mesh refinement of one sort or another.

It was thus with some trepidation that I opened *Crystals, Defects and Microstructures: Modeling Across Scales*, by Rob Phillips. The last I had heard, the title topics were still open problems. What, I wondered, had I overlooked during the last few years, while I was somewhat preoccupied with changing my children's diapers? Had the dust settled to such an extent that people were now writing textbooks about these subjects? Eager to find the answer, I flipped to the last page—page 755, no less. There my eye lit upon the following sentence: "On the other hand, after the long journey of writing a book such as this, I am also struck at the mismatch between what I had hoped this book would become and what it actually is." Phew! In fact, only in the last 107-page chapter does Phillips actually discuss ways to bridge length scales. This discussion is an exercise in consciousness-raising, which many readers will find rather thought-provoking. It is packed with examples of multiscale modeling at work, ranging from Newton's realization that the gravitational field of Earth can be treated as originating from a point, to modern research topics, including hyperdynamics methods for accelerating time-dependent simulations, mesoscopic dislocation dynamics, solidification processes, and the author's own pioneering work on linking atomistic simulations to finite element simulations. One of the nice things about this chapter is that, in places, it acts as a sort of reinterpretation of much that has gone before it—for example, the discussion on linear elasticity theory and density functional theory.

Unfortunately, I finished this chapter wishing that the author had provided much more detail about the techniques. After all, one wants the readers to come away being able to do computations that they couldn't previously do; having the right philosophy is only half the battle. In particular, I would have wished to see a more complete description of the different techniques for implementing adaptive mesh refinement ideas (disclosure: I do indeed work on this topic, and my work is appropriately reproduced and cited) and perhaps a more practical description of renormalization group ideas, and their relation to effective theory construction, with actual examples.

The preceding 648 pages present a nicely-written, self-contained course on materials physics. There is a tasteful blend of the theoretical physicist's view of the material and the material scientist's more empirical, data-informed view. Many courses on solid-state physics are closely based around the classic *Solid State Physics* by Neil W. Ashcroft and N. David Mermin (Holt, Rinehart, and Winston, 1976), which mainly focuses on the properties of ideal materials, an appropriate