Africa. Neither article is particularly accessible to nonspecialists, although the fact that such transmission of Islamic astronomy even existed is interesting and suggestive. Finally, Y. T. Langermann discusses Averroes' criticism of Kindi's attempt to create a mathematical model for the action of compound drugs.

Leaving aside my illiberal complaints about what it is not, the book is handsomely and appropriately produced: good printing; minimal typographical errors; clearly produced, or reproduced, diagrams and illustrations; and a well-edited index—the last being a feature that is often skipped in edited volumes.

JOHN WALBRIDGE, INDIANA UNIVERSITY

Joseph LaPorte, ***Natural Kinds and Conceptual Change***. Cambridge: Cambridge University Press (2003), 232 pp., $70.00 (cloth).

Joseph LaPorte has taken a fresh look at a familiar set of issues: the naturalness of scientific kinds, the linguistic meanings of kind terms, and the stability of those meanings across theoretical change. The positions he adopts do not lend themselves to easy summary and can appear incompatible at first sight. He is an essentialist, but he also holds that individuals do not belong to their kinds essentially, that essences can be historical in nature, and that scientific kinds are stipulated rather than discovered. He believes in natural kinds, but he also thinks that naturalness is contextually determined and is a matter of degree. Moreover, he thinks that the causal theory of reference is a good account of the reference-fixing of scientific terms, but maintains that it fails to deliver referential stability across theory change. Similarly, he believes that the meanings of scientific terms often change with major theoretical changes (a vindication of Kuhnian incommensurability), but he does not regard this as an impediment to scientific progress. Finally, he endorses the Kripkean claim that there are necessary a posteriori truths in science, but takes this as a reason to rehabilitate analyticity. Part of LaPorte's considerable achievement consists in arguing that these apparently irreconcilable claims can be harmonized. It would take too long to explore them all, so I will restrict myself to examining just three of his most striking assertions, concerning the naturalness of kinds, the adherence to essentialism, and the claim that scientific kinds are stipulated rather than discovered.

LaPorte proposes that a natural kind is one with explanatory value in a particular explanatory context, which leads him to say that the naturalness of a kind is a matter of degree, varying with the explanatory value of the kind in question, as well as with the context of explanation (19–23). This position would not satisfy those philosophers who think of natural kinds in essentialist terms, for whom naturalness is an all-or-nothing affair. By contrast with such philosophers, LaPorte allows that a kind like *green* does figure in some explanations (e.g., camouflage) and that it can therefore be considered natural in certain contexts; similarly, "it is permissible to call *toothpaste* and *trash* 'natural' in [some] contexts" (26). However, a tension emerges when he nevertheless insists that "*toothpaste* and *trash* are not properly called 'natural kinds' . . ." in the "fairly strict context" of a philosophical discussion (26). By his own lights, it would seem as though such judgments can only be made in actual scientific contexts where these kinds figure as explanatory categories. Likewise, LaPorte's pragmatic account of what makes a kind natural does not sit well with his view that vernacular kinds merely conform to scientifically determined natural kinds. He claims that the connection between scientific findings and vernacular categorization is very tight, with ordinary speakers regularly deferring to scientists (25–26, 31). Yet, he later acknowledges that while ordinary parlance is sometimes adjusted to conform to scientific usage (e.g., whales are not generally considered fish in ordinary usage (68)), this is sometimes not the case (e.g., birds are not considered dinosaurs in ordinary usage (88)). His conclusion that "refinement does, nevertheless, seem to be the rule" (90) might have been modified in light of his account of the naturalness of kinds. That account would suggest that adjustment or refinement of ordinary usage takes place when the explanatory and other purposes of the folk coincide with those of the experts; when they do not, we should not expect the vernacular to copy scientific usage.

Similar remarks apply to LaPorte's attitude to essentialism. He shows quite decisively that the species concepts employed by the major taxonomic schools create serious problems for essentialists who believe that an individual belongs to its species essentially. For example, cladists hold that a species becomes extinct whenever it sends forth a new side species. Thus, the species *Panthera tigris* would become extinct (and an individual tiger would cease to belong to it) if a branching event were to occur that produces a new species (54–55). Since an individual tiger can actually cease to be a tiger in such circumstances without undergoing intrinsic change, such an individual cannot belong to the species essentially (i.e., belong to it in all possible worlds in which that individual exists). But LaPorte maintains that a species itself can nevertheless have an essence, even though individuals belonging to that species do not belong to it essentially. He states that it is essential for *Panthera tigris* to be the lineage that descends from some ancestral population P and that terminates in speciation or extinction (61). However, one might raise similar problems for the essence of tigerhood. Is it essential for tigers to be mammals? It seems as though one could argue (by analogy with the case of the individual) that the species *Panthera tigris* might not have belonged to the taxon Mammalia had there been some branching event further up in the phylogenetic tree. LaPorte might agree, adding that two phylogenetic trees need to have exactly the same history of branching to produce the same taxa, and that there is no other way of individuating taxa (by contrast with individuals). This would imply that no two branches of two phylogenetic trees can be considered the same unless every other branch of those trees coincides. But this judgment depends on one's views about the identity of species and higher taxa, and should at least have been situated in the context of the purposes relative to which we may want to individuate them. The claim that individuals do not have an essence but that species and higher taxa do seems out of step with LaPorte's sensitivity to the contexts in which we need to single them out.

On the matter of stipulation rather than discovery, there is a similar need to situate the issue within its broader context. Why are scientific categories stipulated rather than discovered? LaPorte argues the point in greatest detail with reference to the Darwinian revolution in biology. According to his account, after Darwin, we could make one of the following three claims, depending on how we understand the term 'species':

1. New species evolve from primitive ones.
2. Species do not exist; rather, the different alleged species are related by evolution.
3. All animals belong to the same species, having descended from a single ancestor. (125)

What Darwin actually said (and what the scientific community accepted) was Claim 1, but LaPorte's point is that Claim 2 or Claim 3 would have been quite consistent with his findings. The reason is that the term 'species' was formerly associated with two implicit criteria for its application, which pick out different extensions: (*a*) the descendants of a single pair of organisms, and (*b*) the varieties like polar bears and radishes that naturalists had identified. Darwin's findings showed that these two did not coincide; in other words, that polar bears and radishes had evolved and were not all and only those creatures descended from a single ancestor. When (*a*) and (*b*) are found to diverge, we have three options. On Claim 1, we reserve the term 'species' for (*b*), leading us to say that species evolve. On Claim 2, we keep using 'species' to stand for the two inconsistent criteria, forcing us to say that species do not exist. On Claim 3, we retain the term 'species' for (*a*), which leads us to say that all animals belong to the same species. Whichever of these three routes we take, there is linguistic change (on Claim 2 'species' means the same but must be discarded, so we will need a new term to stand in for the varieties radish, polar bear, etc.). Moreover, the fact that we have these three options shows (à la Quine) that meaning change and theory change are of a piece and that kinds are stipulated rather than discovered. Thus, LaPorte sets himself two challenges: first to show that despite the linguistic change there can be communication across the revolutionary divide, and second to show that meaning change can be distinguished from theory change. It is difficult to do justice to his arguments in the space of this review. Therefore, I will confine myself to one aspect of his response, namely his argument that meaning change can be distinguished from theory change.

LaPorte holds that theory change can be distinguished from meaning change, in part, because scientists "study the same entities" before and after, concluding that "there is no worry that there can be no truth about one shared world" (129). But the claim that we are dealing with the same entities begs the question, since the terms that pick out those entities are said to have shifted in meaning. There seems to be a more natural solution to this problem, which emerges when it is situated in the context of our explanatory interests. There is clearly a sense in which all of Claims 1, 2, and 3 are *possible* ways to go. At the same time, there is an equally clear sense in which Claim 1 is virtually forced upon us in this situation (LaPorte presents some interesting historical evidence that at least one of Darwin's contemporaries advocated Claim 3, but it is hardly a coincidence that this was a rabid anti-Darwinian). Despite the fact that, *in principle*, there may be nothing to distinguish meaning change from theory change, in practice the choice is usually quite clear. Thus, the challenge is to say what in actual circumstances leads us to jump one way rather than another, choosing Claim 1 over Claim 2 and Claim 3. Not surprisingly, the answer is: pragmatic considerations—on Claim 2 'species' must be discarded (a waste of a perfectly good concept), and on Claim 3 it is otiose (it has no explanatory value in biological science). Indeed, LaPorte himself clearly makes such judgments with respect to other cases (see, e.g., his comments on Galen and Harvey), thus showing that we have implicit ways of prizing apart meaning and theory, which are governed by our practical interests in explanation, prediction, and in going about the business of science.

I have tried to argue that there is a tension in LaPorte's position between a naturalistic account that privileges explanatory purposes on the one hand, and an adherence to a context-insensitive brand of metaphysical realism on the other. But notwithstanding this criticism, this book provides a very welcome new approach to a well-known set of philosophical problems, and it does so by using real examples from scientific practice and the history of science, rather than the toy examples that are common in the philosophical literature.

MUHAMMAD ALI KHALIDI, AMERICAN UNIVERSITY OF BEIRUT

Keith Parsons (ed.), ***The Science Wars: Debating Scientific Knowledge and Technology***. Amherst, NY: Prometheus Books (2003), 300 pp., $21 (paper).

The science wars are fought on many fronts. First and foremost is the battle over objectivity. Do scientists make genuine discoveries (which is what they claim to be doing), or do they construct theories that reflect their various (nonscientific) interests? The commonsense view, which is held by most scientists, most philosophers, and most of the general public, is that science is indeed objective. But the case against objectivity has in some instances been impressive. Theories of race and of gender have much too easily been used to support the socially dominant position of the theories' proponents. They were rubbish at the time and are now clearly seen by all to be rubbish. More contentious are the claims about current theories, though the case varies with the discipline. Many will concede some version of social constructivism when it comes to economics, say, but vociferously deny it in physics.

Keith Parsons has put together a valuable collection on the science wars, well suited for introducing students and others to this important debate. He is not new to this topic; earlier he wrote a very interesting work, *Drawing Out Leviathan: Dinosaurs and the Science Wars* (2001). The book under review, *The Science Wars: Debating Scientific Knowledge and Technology*, consists of four parts, each with three or four essays. All are reprinted from classic statements, each quite well chosen. The first part, for instance, is called "The Constructivist Challenge" and contains pieces by Latour and Woolgar, and by Shapen and Schaffer offering constructivist accounts of science. These are followed by articles by Klee, and by Gross and Levitt directly rebutting them. Subsequent parts are on feminism, postmodernism, and conservative critiques with pieces by Sandra Harding, Donna Haraway, and Steven Weinberg, among others. As well as the essays, each part has a brief introduction by Parsons and some short study questions.

Though the battle is chiefly about the objectivity of the sciences, there are related issues and motivating factors in the science wars that are of crucial importance. Let me mention two.

I suspect that most readers of this journal know about the Sokal Hoax and know that it was aimed at postmodern accounts of science. But do they know *why* Sokal did it? Sokal reports that left-wing political goals