relationship between experiments and simulations is murkier than one might think.

This is certainly true. Simulationists and experimentalists face equally relevant challenges when it comes to establishing that the results of their simulation or experiment are informative about the real world. But it is one thing to point this fact out, and it is another to understand how those challenges are overcome, under differing circumstances, and in varying contexts. It is here that Marcel Boumans' contribution becomes especially valuable. He presents an example from economics in which a mathematical model performs the role, not of a representational entity, but of a data sensor. Boumans argues, and I concur, that the manner in which such models are assessed is particularly interesting. They cannot be assessed merely by being confronted with facts about the world, since these models are themselves used in generating the relevant data about the phenomena in question. The relevant strategy for assessing these models is calibration. In other words, rather than being held side by side with the relevant bit of the world, the models are held up against other instruments that are antecedently believed to be reliable sources of data.

This is an area of research on models—looking at the variety of ways in which models are assessed—that deserves much more attention in philosophy of science. Philosophers rarely think the assessment of models is any different from the assessment of theories. But this is a provincial view of models. In a vast range of applications, models are created to represent phenomena for which data are conspicuously scarce—for example in the case of a model of the inner convective structure of a star—or where the model itself is central in creating the data, as in Boumans' example. In such cases, the standard story, which relies on concepts like similarity, goodness of fit or isomorphism, is inapplicable. It has nothing to say about the relevant criteria of assessment. Philosophers have much to learn about the various ways in which models are assessed under circumstances in which the traditional story does not apply. Some of the contributions to this volume offer a good start.

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Ilkka Niiniluoto, ***Critical Scientific Realism.*** Oxford: Oxford University Press (1999), xiv + 341 pp.

This book offers an accessible introduction into Niiniluoto's philosophy virtually devoid of the technicalities typical of his earlier work. It is impressive in many respects, particularly in its comprehensiveness and rigour of its arguments.

The first chapter sets the stage for the rest of the book. Niiniluoto announces that he will be defending a cluster of views that he calls 'Critical Scientific Realism' (CSR). These are: a) that there is a mind-independent world, b) that the notion of truth can best be captured by a Tarskian version of the correspondence theory, c) that both observational and theoretical statements have truth values, d) that the aim of science is truth plus some associated epistemic utilities, e) that it is possible to approach, and to assess our progress towards, the truth, and f) that the best explanation for the success of science is the approximate truth of our scientific theories. He, thus, methodically situates CSR in a broad range of philosophical debates, viz., ontology (ch.2), semantics (ch.3), epistemology (ch.4), theory construction (ch.5), and methodology (ch.6). The last four chapters provide illuminating discussions on related topics, viz., internal realism (ch.7), relativism (ch.8), social constructivism (ch.9), and on extrascientific reasons for and against realism (ch.10).

In the preface, Niiniluoto states his hope that this book can be received as a textbook, a monographic treatise, and a polemic tract. It fails to achieve the first of these. Most of the chapters are intimately intertwined with the author's overarching aim, i.e., defending CSR, and, as such, discussion tends to be skewed. Despite this, there are notable exceptions such as ch.1, which presents a neat overview on the varieties of realism, and Section 5.2, which presents a useful account of the relations between meaning variance, theory-ladenness and incommensurability.

Perhaps the most displeasing aspect of this book is that occasionally depth is sacrificed for comprehensiveness. Section 5.4, for example, offers a brief discussion of how the realist debate features in specific sciences but no serious attempt to illustrate how CSR weighs in. More alarmingly, scientific realists of all stripes will be taken aback by the scant coverage of such main topics as inference to the best explanation and the no-miracles argument. With this attitude, Niiniluoto shows the same irreverence that other scientific realists show to his formal accounts of truth-likeness and approximate truth.

Niiniluoto's hasty discussion and dismissal of two popular scientific realist views, viz., entity realism (ER) and structural realism (SR), is one issue that demands closer scrutiny (Section 5.3). He argues that CSR goes beyond both ER and SR in that it accepts theoretical sentences, "both universal ones expressing laws [contra ER] and existential ones expressing ontological claims [contra SR]," as perhaps false but nonetheless truthlike or approximately true (13). He also claims that neither position can account for the "interpenetration" of facts and theories (139). It is not entirely clear what he means by this latter claim. In the case of ER, he is objecting to Nancy Cartwright's idea that laws about fundamental particles are neither true nor reign supreme outside the laboratory. Yet Cartwright's point is that we have no *inductive* reasons to suppose that these laws are true and universal in force. Niiniluoto does not address this finer point. Furthermore, Cartwright (at least in her more recent work) does not reject theory altogether but rather supports a patchwork view of laws, where different laws, not necessarily related to each other, act in different domains. In any case, ER need not be held hostage to Cartwright's version. ER can be construed to accept that at least some theoretical laws may be truthlike or approximately true.

Contrary to Niiniluoto's allegations, SR—incidentally the term was coined by Grover Maxwell, not Jerzy Giedymin, as Niiniluoto implies—allows existential statements expressing ontological claims, provided that these are restricted to structure. Indeed, the epistemic version of SR expresses the structure of a theory via its Ramsey sentence, which, as we all know, makes such ontological claims. Here the interpenetration objection may just boil down to the position's reliance on an observational-theoretical distinction. In his critique of constructive empiricism's appeal to such a distinction, Niiniluoto argues that for a fallibilist, like himself, "even naked-eye observation reports are uncertain to some degree, and their epistemic status does not sharply differ from measurement results by scientific theories" (117). One need not draw the distinction as the constructive empiricist does, however. For example, one can appeal to a broader notion of observation sentences in the Quinean tradition, where there is no special status for naked-eye observation reports. Moreover, even if the distinction is problematic, not all versions of SR endorse it. Niiniluoto focuses on John Worrall's version, which holds that what survives scientific revolutions is the mathematical structure of a mature theory—not just the laws as Niiniluoto maintains. Against this, he argues that Worrall's example of the transition from Fresnel's theory of light to Maxwell's electromagnetic theory presupposes some ontological continuity, viz., light, and, hence, cannot show that continuity is present only at the structural level (133–134). But light is a phenomenon, not an entity inhabiting the physical world. Furthermore, even if we take light to be a physical entity, SR does not prohibit continuity at the ontological level. The claim is, rather, that ontological continuity is parasitic on structural continuity, i.e., if by 'light' we mean that which satisfies the given mathematical equations, then light is carried over through theory change with structural realist blessings. Finally, against Niiniluoto's claim that ER and SR are "diametrically opposed," we can point out that the above comments plus recent work by other authors suggest that they complement and, perhaps even necessitate, each other.

I think most of us would agree with Niiniluoto that, though we cannot prove realism in its various domains, there are plenty of lessons to be learned in debating it. Despite its faults, this book is a great exercise of philosophical acumen with innovative and exciting ideas aplenty.

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Naomi Zack, ***Philosophy of Science and Race.*** New York and London: Routledge (2002), 145 pp.

Does the concept of "race" find support in contemporary science, particularly in biology? No, says Naomi Zack, together with so many others who nowadays argue that human races lack biological reality. This claim is widely accepted in a number of fields (philosophy, biology, anthropology, and psychology), and Zack's book represents only the latest defense of social constructivism in this context. There are several reasons why she fails to make a convincing case.

Zack starts by arbitrarily ascribing an anachronistically essentialist connotation to the concept of race. After having made that everyday notion semantically so crude and outdated there is no wonder that she finds it quite easy to conclude that such an awkward category has no place in science. Her main rationale for seeing our race distinctions as being poorly matched to biological characteristics (e.g., population differences in gene frequencies) is that these biological characteristics do not fall into discrete and mutually exclusive categories as "required" by the common-sense taxonomy. This opposition between the continuity of variation found in biology and the alleged discreteness of common-sense "races" is repeated throughout the book, and it is presented as creating an unbridgeable gap between biology and the colloquial concept of race.

Contrary to what Zack says, however, today's common-sense ideas about race are not so radically disconnected from contemporary science. Rather, "race" in ordinary usage is informed by biological knowledge to a considerable extent. Most people no longer think about race in terms of pre-Darwinian racial "essences" and "mutually exclusive" ideal types. In fact, as pointed out by Anthony Appiah (whom Zack quotes on this matter but without taking him seriously enough), the discourse on race has long been characterized by a practice of "semantic deference," according to which people tend to use the word "race" assuming that the biologists could say more precisely than they could what it meant. So, in line with such cognitive division of labor, when typological thinking in biology was replaced by population thinking, common sense largely followed suit, readily deferring to the new consensus of experts and thus opening a path for smooth integration of biological knowledge.