Rational Thermodynamics by C. Truesdell
Review by: Mario Bunge

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either a bare conjecture (and so by Watkins's own admission no help) or else inductively supported (impossible according to Watkins). Watkins thinks that Hume would not object to E since it refers only to the past. However, the problem of induction is not that of inferring statements that refer to the future from statements that refer only to the past but that of going beyond available evidence.

Second, why is it reasonable to believe the no-change hypothesis? The principle that it is reasonable to act on the assumption that things will go on pretty much as they have so far looks suspiciously like one form of the principle of induction. So it does, but Watkins will deny that he simply asserts the principle. He derives it from the uncontroversial principle that it is reasonable, in practical contexts, not to make stronger assumptions than necessary and the claim that, given E, PP is a weaker claim than PP′.

But why is it weaker? Though Watkins claims that E generates a "formal" difference between PP and PP′ (p. 348), the difference clearly has to do with what would have to happen for one hypothesis rather than the other to come out true and so is not formal at all. In fact, PP′ strikes us as making a stronger claim because it contradicts our normal inductive expectations, postulating a wholly unaccounted for disruption of a lawlike statistical generalization: the natural projection of E. To see this, try to suppose that some Humean skeptic had succeeded in convincing us that we really had no idea whether or not nature operated uniformly so that, at any moment, anything might happen. Then how would we judge which hypothesis made the weaker claim?

Finally, what counts as no change? No hypothesis is intrinsically the null hypothesis. In a world governed by "gruish" laws, some gruish variant of PP′ will be the null hypothesis. Again, Watkins relies covertly on our normal inductive expectations. He has not solved the Popperian pragmatic problem of induction.

Watkins's book has many merits: its clarity and attention to argument for example. But it is flawed by its insularity. Watkins proceeds as though the terms of debate in epistemology had been finally fixed by Popper. As a result, he ignores the greater part of postwar philosophy. (Another example: Watkins insists that any adequate aim for science involve the notion of truth, yet he almost completely ignores the extensive recent literature on realism.) Those fascinated by Popperian arcana (for example, long and detailed reports on attempts to fix up Popper's proposed measure for the content of theories) will find plenty to interest them in this book. Others can afford to let it pass. *Michael Williams, Northwestern University*

C. TRUESDELL, ***Rational Thermodynamics.*** With an appendix by C.-C. Wang. Second edition corrected and enlarged. New York-Berlin: Springer-Verlag (1984), xvii + 578 pp. $69.00.

Classical or phenomenological thermodynamics passes generally for an exhausted field. According to most textbooks it is a complete theory and, moreover, one that has been reduced to statistical mechanics, which in turn would be nothing but an application of classical particle mechanics. Being a cut and dried theory, it is a subject fit for teaching, not for research. So far, the textbook story. For better or worse, this story is one of the many myths that the textbook industry has kept alive. The book under review is clear proof that thermodynamics is incomplete, and far from being in perfect health; and that it continues to be a research field that has attracted some of the most sophisticated scientists of the day.

For one thing, the thermodynamics most physicists and engineers still learn at school is not really thermo*dynamics* but thermo*statics*. Indeed, it concentrates on closed systems near equilibrium, where hardly anything happens. For another, the standard theory is so general that it pays hardly any attention to the peculiarities of the various kinds of material, that is, the so-called constitutive "relations" (equations or inequations). (The only notable exception is the ideal gas.) Such generality is of course a reason for having drawn the attention of some philosophers, but it is also a reason for having remained isolated from other branches of physics and for remaining at a safe distance from the real world. Finally, the mathematics involved in thermostatics is far from clean. Typically, thermodynamic "differentials" are mathematically spurious, and many a proof is verbal rather than mathematical.

In contrast to the typical textbook, this book is a rigorous and up-to-date exposition of thermodynamics proper. It concerns extended and deformable bodies modeled as continua, where processes of various kind may occur, such as wave propagation, diffusion, and chemical reactions. Unlike thermostatics, thermodynamics proper presupposes continuum mechanics. This viewpoint is responsible for a number of departures from tradition. For example, when viewing thermodynamics as the thermomechanics of extended deformable bodies in some process of change, one is forced to shed the restriction that temperatures are "definable" only in states near equilibrium. Second example: The kinetic theory is no longer seen as a model of a system of classical particles, but as a simplified model for irreversibility in gas flows. (Consequently the characteristic equation of the kinetic theory appears as a constitutive equation.) Third example: The new vantage point spares us a number of confusions, obscurities, and even "magical and mystic trips" characteristic of standard thermostatics. For instance, we are finally told that there are several different "Second Laws"—a gratifying finding for the present reviewer who, when a puzzled student, filled a notebook with twenty or so ostensibly inequivalent but equally vague formulations of "the" Second Law. (Regrettably, nobody seems to know which one of those equivalent formulas is true.)

The present volume is the second edition of a work originally published in 1968. It differs from the first mainly by a 57 page long Historical Introit, and by a large number of appendixes concerning the foundations of thermodynamics as well as a number of special problems. These appendixes summarize some of the research done in thermodynamics over the past two decades. They are clear proof that the field is alive and well—although in departments of mathematics and schools of engineering rather than in physics departments. And they refute the Kuhn-Feyerabend-Lakatos thesis that all scientific revolutions (such as the inception of statistical mechanics) consist in the "overthrowing" of earlier theories. Quite often the allegedly deposed monarchs continue to reign, although sharing power with the new ones. The new classical—or neoclassical—thermodynamics built by Truesdell and his associates is a case in point. And it should be a warning to philosophers of science who derive all their information from textbooks. *Mario Bunge, McGill University*