speaks with authority. That is so in his account of the prophetic speech of 1929, given by Orso Mario Corbino, director of the Physics Institute of the University of Rome and Enrico Fermi's strong supporter, in which he emphasized the future importance, scientific and technical, of nuclear physics. When the subject is more remote or when failure, not success, is involved, the treatment is much less satisfactory. After a short discussion of the disintegration of light nuclei by alpha particles beginning in 1917, Segrè says, "Rutherford's experiments were repeated in Vienna, and Austrian scientists found more disintegrations than Rutherford did. A lively debate arose, but in the end it was found that Rutherford was right." To one who already knows of the controversy between the nuclear physics groups at Cambridge and Vienna who were using scintillation counting in 1926–27, this offhand remark is frustrating, because an examination of the conflict could reveal much of the nature of experimental physics. (Why did so "simple" a technique succeed at the Cavendish Laboratory but fail in Vienna?)

In painting with broad brushstrokes, it is clear that details must be suppressed. However, why does Segrè, the editor in chief of Fermi's *Collected Papers* and Fermi's collaborator and biographer, say (on p. 144) that Paul Dirac "put Fermi's statistics on a quantum mechanical basis," when the title of Fermi's paper is "On the *quantization* of the ideal monatomic gas" (my emphasis)? And I must include one other caveat against an oversimplification. Segrè states (on p. 245) that Hideki Yukawa's reasoning in proposing the meson theory of nuclear forces involved "little more than an application of the uncertainty principle and of relativity." The type of argument presented as Yukawa's is not to be found in Yukawa's papers, but was given first by Gian Carlo Wick in a letter to *Nature* in 1938, four years after Yukawa's theory was proposed.

There is a useful ten-page bibliography that emphasizes biography, and there are ten short appendixes, containing mathematical derivations. Seven of these deal with the thermodynamics of blackbody radiation and quantum statistics; the last appendix (two pages) is called "Quantum mechanics in a nutshell." The appendixes are too brief for anyone who is not already familiar with the subject and too elementary for anyone who is. The illustrations in the text, on the other hand, are very fine, being either reproductions from the original literature or photographs, some of them from Segrè's private collection. Together with his personal observations and attractive style, they help to make this book an appealing one, especially for physicists and students of physics.

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## Sources of Inspiration

**The Tragicomical History of Thermodynamics, 1822–1854.** C. TRUESDELL. Springer-Verlag, New York, 1980. xii, 372 pp. $48. Studies in the History of Mathematics and Physical Sciences, 4.

Clifford Truesdell, along with his distinguished career in the sciences of continuum mechanics and rational thermodynamics, has demonstrated an abiding penchant for the history of those sciences as well. In the 1960's he made significant contributions to the history of rational mechanics, with a special fervor for the work of Leonhard Euler. In the 1970's he turned his linguistic and logical skills to fathoming the major writings in 19th-century thermodynamics, with special favor for the almost unknown work of Ferdinand Reech.

A very short preliminary version of Truesdell's views appeared without his authorization in 1971 under the title *The Tragicomedy of Classical Thermodynamics*. The intervening years have not changed his judgment about thermodynamics as the peculiarly tragicomic science, the science that he says has no peers when it comes to the high "ratio of talk and excuse to reason and result" (p. 3). But he invites thermodynamicists to read his book and to share with him the discovery he unveils there: "*Thermodynamics need* never *have been the Dismal Swamp of Obscurity* that from the first it was" (p. 6).

To reveal his discovery Truesdell, at various stages in his chronological account of the classics in thermodynamics, steps back from the immediate history to cast his net of mathematical logic. He identifies a set of equations that he considers to be the correct mathematical expression of the thermodynamic ideas under discussion. He then proceeds to demonstrate various thermodynamic relations that might have been deduced thereby and asks why the 19th-century thermodynamicists let them slip by.

Truesdell keeps these logical commentaries separate from his historical accounts by placing them in sections labeled "critiques" or, when he cannot resist commenting during an account, by enclosing such paragraphs in brackets. He clearly does not expect that historians of science will approve of this practice and advises any such who might chance upon his work that "he would do well to omit all sections labelled 'critique' and all words confined between square brackets, for in that way he will save himself such pain as my 'ahistorical' approach might otherwise inflict" (p. 5). Actually Truesdell inflicts far more pain by explicitly ignoring almost all that historians of science have written. The real question, of course, is whether he promotes our historical understanding with his claims that thermodynamicists were blind to these supposed relations that they should have seen.

He does and he doesn't. He does give historians some good questions to mull over, but he doesn't give any fully satisfactory answers. He offers two main reasons to explain why thermodynamicists failed to explore fully the logical import of their theories: they did not express their ideas in rigorous mathematics, and they allowed physico-philosophical ideas to intrude into mathematical reasoning. Truesdell often makes additional appeal to the activities of a mythical figure that he variously calls the tragicomic muse or daemon or fury of thermodynamics.

Thus, although Truesdell admires Carnot's remarkable intuitive powers, he regrets Carnot's non-mathematical mode of analysis, chiding him for not seeing a certain simple argument and calling it a failure "typical of the theorist who tries to get along without mathematics" (p. 106). But why did Carnot ignore mathematics except in his footnotes? Truesdell appeals to the muse: "Carnot does not follow the tradition of eighteenth century rational mechanics. . . . Instead, the sardonic muse directs him to write in a medium that anybody can understand" (p. 80).

Truesdell might better have appealed to the fact that Carnot wrote in relation to the thinking of his day about steam engine technology, thinking that viewed pressure and not temperature as the key to power and toyed with ideas that other substances more volatile than water might provide more power. Carnot's axiom, therefore, that all substances would yield the same power between any given temperatures, would have had immediate relevance for practical engineers. Perhaps Truesdell would view that motive with disfavor, because he says, in one of his choice bracketed sentences, after quoting Kelvin's observation in support of the caloric theory in 1849 that practical engineers did not question Carnot's axiom: "[A fatal mistake, this, for 'practical engineers' are the last persons in the world from whom to expect searching questions!]" (p. 174).

The other reason that Truesdell frequently gives for failures to see the logical consequences of theory is that physico-philosophical ideas intruded on questions that should have been strictly mathematical. Thus, in his discussion of Reech's 1853 paper where Reech just missed pulling all the assorted strands of thermodynamic theory together because he could not divest himself of ideas derived from the caloric theory, Truesdell comments: "[Spectator, let the logical blunder which Reech here commits serve as a paradigm of the confusion physico-philosophical reasoning can produce when applied to a mathematical question!]" (p. 279). And the muse too may have been at work, for Truesdell has earlier observed: "Alas, here too the tragicomic fury casts her spell by making [Reech] attempt to prove everything by running engines backward and forward against each other" (p. 237).

But why did physico-philosophical reasoning persist in intruding on the development of the logic of thermodynamics? Truesdell never does say, but he suggests at one point the great difficulty of breaking free from tradition.

> We who have been brought up to take it for granted may have trouble seeing just what was the difficulty of first grasping it [the uniform interconvertibility of heat and work]. There should be no difficulty at all. We must not forget that every scientist is, like ourselves, brought up with a set of beliefs he has not been encouraged to question. Only the exceptional man knows how to ask an important question. Still more exceptional is the man who can answer one [p. 152].

Truesdell has done an exceptional job of raising questions and of summarizing the significant writings in thermodynamics and reducing all the various systems of notation to a single system. Anybody beginning to browse in the thermodynamic literature from 1822 to 1854 could well use this book as a guide. But two cautions are in order lest the reader take Truesdell's account as the final word: his omitting discussions of steam and vapors leaves out important attempts to confirm thermodynamic ideas; his logical analysis of ideas fails to call attention to the way certain ideas gave direction to the evolution of thermodynamic thought.

By excluding vapors and steam Truesdell eliminates the only empirical data that were available to test Carnot's axiom. Thus he offers no account of how Clapeyron derived an equation, now known as the Clausius-Clapeyron equation, so that he could use data on vapor pressure and heats of vaporization to determine the universal function of temperature that Carnot had proposed. Moreover, the behavior of saturated steam, which Truesdell calls "that nemesis of thermodynamics" (p. 175), played a key role in the transition from the caloric theory to the mechanical theory of heat. In 1850 both Rankine and Clausius adopted the new theory and argued that, contrary to the behavior accepted in the caloric theory, saturated steam liquefies during adiabatic expansion. This claim not only went contrary to the caloric theory, it also apparently contradicted the empirical evidence. It was well known that steam issues from a safety valve dry and not wet. In 1851 Clausius strengthened the case for the mechanical theory by explaining this apparent anomaly. Thus, thermodynamics had more empirical guidance in the supposed "Dismal Swamp of Obscurity" than Truesdell's logic with its restriction to gases can admit.

As for the evolution of ideas that pure logic cannot capture, let me mention the one that I consider the most important. Truesdell confesses that, after seven attempts in 30 years to understand the 1854 paper by Clausius, he still cannot. In that paper Clausius introduced the version of the second law that led later to his formulation of the entropy concept in 1865, namely, the statement that heat cannot pass from a colder to a warmer body without some related change also occurring. After subjecting that statement to logical scrutiny, he concludes: "All that remains is a Mosaic prohibition. A century of philosophers and journalists have acclaimed this commandment; a century of mathematicians have shuddered and averted their eyes from the unclean" (p. 333). But whatever Clausius's statement may have lacked in logic seems insignificant in comparison to how it finally contributed to the evolution of thermodynamic ideas. By expressing the second law in terms divorced from the steam engine and based upon a common spontaneous process, Clausius provided the direction that led to Gibbs and his thermodynamic explanation of spontaneous processes wholly removed from the steam engine. The muse of thermodynamics is not tragicomic.

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## Foundational Study

**Works on the Foundations of Statistical Physics.** NIKOLAI SERGEEVICH KRYLOV. Translated from the Russian edition (Moscow, 1950) by A. B. Migdal, Ya. G. Sinai, and Yu. L. Zeeman. Princeton University Press, Princeton, N.J., 1979. xxviii, 284 pp. Cloth, $19.50; paper, $7.50. Princeton Series in Physics.

It is good that Nikolai Sergeevich Krylov's collected works on the foundations of statistical physics have finally become available to the English-reading world. They consist of his Ph.D. thesis, "The Processes of Relaxation of Statistical Systems and the Criterion of Mechanical Instability," defended in July 1941 at Leningrad University, "On the description of inexhaustively complete experiments," which appeared in *Uchenye Zapiski* of Leningrad University in 1944, and "The Foundations of Statistical Mechanics." The last work was planned as a large monograph treating all aspects of this intricate subject, but only the first two of the planned six chapters were completed when Krylov died in 1947, at the age of 29.

When statistical mechanics was introduced at the end of the 19th century probability theory was little developed and was moreover quite foreign to the Laplacian classical physicist. As a consequence the advent of statistical mechanics led to many conceptual difficulties, some of which can still be found as "paradoxes" in modern textbooks. The basic physical issues were clarified—at least to the satisfaction of working physicists—by P. and T. Ehrenfest in their famous 1911 paper. By the time Krylov started to write his book probability theory and ergodic theory had flourished in the hands of Birkhoff, Hopf, Kolmogorov, von Neumann, Wiener, and others. Thus Krylov started to reexamine the foundations of statistical mechanics at a time when the basic mathematical tools were sufficiently secure and when statistical mechanics, now enriched by its quantum version, had proved to be *the* tool to explain the macroscopic behavior of matter on the basis of its microscopic constituents.

Krylov starts by carefully formulating the "classical" Ehrenfest picture. For a gas in a macroscopic container we expect the laws of hydrodynamics to be valid: concentration and temperature differences tend to level out in the course of time, and the system approaches thermal equilibrium. Microscopically the gas can be idealized and imagined to consist (at least if we forget about quantum me-