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 *Uchenÿe 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 mechanics) of very many (on the order of 10^{26}), very small (on the order of 10^{-8} centimeter) billiard balls. As Krylov emphasizes over and over again, the "classical theory" is consistent as long as the following three conditions are met:

1) Initially the system is in local equilibrium. (This is a restriction on the kind of things we find in nature or can prepare in the laboratory.)

2) The dynamics has a sufficiently strong mixing character, a property that expresses in a certain way the tendency to approach thermal equilibrium. This property must be derived from Newton's equation of motion, and one of Krylov's outstanding achievements was to locate what is nowadays believed to be its origin: Phase points that are initially close tend to separate exponentially in the course of time owing to collisions. Krylov even estimated on that basis the time a gas composed of hard spheres needs to establish its local equilibrium.

3) Only a certain class of physical quantities can be observed. (This is a restriction on what we can see and the kind of measurements we can perform.)

Krylov argues that under these conditions hydrodynamics can be derived from microscopic dynamics, a goal still far from being achieved.

After elucidating the "classical theory" Krylov asks the crucial question whether conditions 1 and 3 can be regarded as laws of nature consistent with or even derivable from classical mechanics. This is roughly what Krylov understands by laying the foundations of statistical physics. Going through a large variety of arguments he comes to the radical conclusion that there is no way of constructing statistical physics on the basis of classical mechanics. He reaches the same conclusion regarding quantum mechanics as the fundamental theory.

A sketch of Krylov's arguments would lead too far here. Still I am tempted to present at least one as an example. Under the mechanical evolution initial local equilibrium will not remain so, strictly speaking. Since the initial time is a matter of choice, condition 1 cannot be regarded as a law of nature in a strict sense.

Krylov's book should be read by anyone interested in the foundational and philosophical aspects of physics. It is of more than just historical interest. Krylov raises questions that even now have not really been answered. Why does a microscopically predetermined history appear on a macroscopic level as a sequence of random events? Do we understand, on a dynamical basis, why the statistical approach works so well? Why does the complicated microscopic motion express itself in "simple" macroscopic evolution laws (as for example in hydrodynamics)?

To a great extent the book is nontechnical and uses very few formulas. Nevertheless it is not easy reading. For an introduction it might be useful to consult the article "Modern ergodic theory" by J. L. Lebowitz and O. Penrose in *Physics Today*, February 1973. Krylov's Ph.D. thesis and the sketch by Sinai of the subsequent development of Krylov's ideas are more technical and require some background in dynamical systems.

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Mathematical Physics

Mathematical Problems in Theoretical Physics. Proceedings of a conference, Lausanne, Switzerland, Aug. 1979. K. OSTERWALDER, Ed. Springer-Verlag, New York, 1980. viii, 412 pp. Paper, $27.70. Lecture Notes in Physics, vol. 116.

A few years ago workers in mathematical physics began a series of biennial conferences on their subject (Moscow 1972, Kyoto 1975, Rome 1977, Lausanne 1979, Berlin 1981). Previously, mathematical physics had appeared as a small subsection of conferences on mathematics or high energy physics or as the subject of more specialized conferences. From the book under review, it is clear that the idea of a general conference was a good one. The book, the proceedings of the Lausanne conference, is a smörgasbord of lectures on Schrödinger operators, statistical mechanics, quantum field theory, gauge theory, dynamical systems, supersymmetry and group theory, and C*-algebras, reflecting a series of challenging problems on which interesting progress has been made.

There are 61 contributions, most of which are short (two to four pages) summaries of significant work. There are also roughly a dozen longer reviews, which can serve as introductions to subjects on which progress has recently been made. The book is recommended as a sampler for those who wish to acquaint themselves with what is going on in mathematical physics, as well as as a source book for the specialist. It has been produced in the usual excellent Springer style, photoreproduced from typescript in durable flexible binding.

The first hundred pages of the book are devoted to the theory of the Schrödinger equation, scattering theory, and the properties of bound-state wave functions and eigenvalues. The general theory of scattering has progressed considerably in recent years, and as a result mathematical techniques are much closer to the physical intuition and formal manipulation displayed in the traditional heuristic scattering theory. For example, one of the basic problems of scattering theory is to prove asymptotic completeness, that is, that any state is a superposition of bound states (belonging to the discrete spectrum of the Hamiltonian) and scattering states. As a result of work by D. B. Pearson one now has two systematic methods of finding interaction potentials for which asymptotic completeness fails, which for brevity I will refer to as Pearson's singular spectrum generator and Pearson's wave trap. The singular spectrum generator (described by Pearson in a contribution to this book) causes waves to vacillate; they cannot decide whether to stay localized and be bound states or to run away and become scattering states. On the other hand, in the wave trap a wave traveling in from infinity is made, by multiple reflections, to linger an infinitely long time. Clearly, if asymptotic completeness is to hold for an interaction potential, the potential must not behave like a singular spectrum generator or a wave trap. New geometrical methods in scattering theory, described in a contribution by V. Enss, make it possible to bring out this point very clearly and lead to a beautiful and simple proof of asymptotic completeness for the two-body problem. So far the geometrical method has not been used to produce a similar proof for the n-body problem with $n > 3$, but there has been considerable general progress, which is reviewed by J.-M. Combes.

Most of the remaining contributions in this part of the book deal with properties of eigenvalues and eigenfunctions for special systems. W. Hunziker reviews the recent work on the Schrödinger Hamiltonian in the presence of electric and magnetic fields, which includes the Stark and Zeemann effects. This is one of the oldest applications of quantum mechanics, but it has not been given a mathematically rigorous treatment until recently. In the course of this work on foundations a number of beautiful results have been proved. For example, the ground state energy of a system of spinless Schrödinger particles is always larger in the presence of a static magnetic