dent enough to run the risks of being appraised in terms of their own contribution to knowledge and society."

Social science needs a constitutency, and it will get one only when people become aware of the contribution it can make to the solution of their problems. We cannot expect that awareness to come so long as federal support for the social sciences is confined to mission-oriented federal agencies or the natural-sciences-oriented National Science Foundation.

Innovative Thinking

The establishment of a National Social Science Foundation will permit the kind of innovative thinking which modern problems demand. Innovative and original thinking, when it deals with people rather than things—people who are also constituents and voters—is likely to be controversial. Some argue that increased federal support for research and scholarship in the social sciences should come through expansion of the present effort of the National Science Foundation in these fields, rather than through establishment of a National Social Science Foundation—and this is the official view of the present Administration. I do not agree, for a number of reasons, most of which I have already stated. Almost reason enough, however, is the fact that the National Science Foundation can obviously ill afford to foster the innovative and original—and therefore controversial—thinking about modern problems which is needed, if that kind of social science research may put in jeopardy the nine-tenths of its budget which is spent in the relatively noncontroversial natural and physical sciences.

Mission-oriented federal agencies are even more restricted to noncontroversial social science research. Thomas L. Hughes, again, put it very well when he testified before our Subcommittee:

> As the social sciences develop, it is particularly important that government support not force them into an inflexible system inhibiting a variety of public and private initiatives. This can be avoided by deliberately fostering innovation, a function with high risk but one which a foundation can better run than can an operating agency which must always keep its program supportive of its mission.

Secretary of Labor Wirtz made the same point about social science research funded by the Department of Labor:

> Our capabilities are such that we have to limit ourselves to those things that we are surest about, as far as their relevance and as far as their results are concerned. There is not risk research in what we are doing.

Secretary Wirtz went on to say, quite rightly in my view, that whether or not establishment of a new agency, such as a National Social Science Foundation, is warranted depends upon whether the government is willing to sponsor research in the social sciences on broader, more innovative lines. He added:

> If it is, such inquiry cannot be expected to come from the established departments or agencies of the government. If more conventional research is contemplated, the present structure probably permits it.
>
> The more we look into our human and social problems, the more we recognize need for innovative, disciplined scholars who can act as engineers in the social change our society needs.
>
> If we are ready to look at those things, then it seems to me we have to find a new approach to social science research, and if we are not, my testimony to you would be that the present situation is not so very bad.

I think our government must be willing to foster such new thinking and research if we are to meet the new problems of our changing times. I believe this can best be done by giving the social sciences separate recognition and responsibility through the establishment of a new federal agency, the National Social Science Foundation.

I hope that we will be successful in this effort to increase our knowledge of man so as to better serve the cause of mankind.

Note added in proof. Since this article was prepared, more than 50 additional witnesses primarily social scientists, have testified on this subject before the Subcommittee on Government Research; the overwhelming majority of them favor the creation of a separate National Foundation for the Social Sciences.

Thermodynamics in Einstein's Thought

Thermodynamics played a special role in Einstein's early search for a unified foundation of physics.

Martin J. Klein

Albert Einstein's "Autobiographical Notes" (*1*, p. 32) contain a striking passage that expresses his views on thermodynamics. "A theory is the more impressive," he wrote, "the greater the simplicity of its premises is, the more different kinds of things it relates, and the more extended is its area of applicability. Therefore the deep impression that classical thermodynamics made upon me. It is the only physical theory of universal content concerning which I am convinced that, within the framework of applicability of its basic concepts, it will never be overthrown." This last remark, he added, was "for the special attention of those who are skeptics on principle."

In this article I analyze the nature of that "deep impression" made by thermodynamics on Einstein's mind and trace the role that thermodynamics played in the development of his early work. This role was a major one: all of Einstein's boldly original attacks on what he saw as the critical problems of early-20th-century physics are intimately related to his understanding of thermodynamics. His early papers, which deal with what appear to be a wide variety of problems, are actually

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tied together by the one concern that characterizes his entire career—the search for a unified foundation for all of physics. From the beginning Einstein realized that there were inadequacies and inconsistencies in the principles upon which physicists were attempting to build satisfactory theories of matter and radiation, and he set as his goal the determination of a more adequate foundation for his science.

In this pursuit of basic principles thermodynamics was especially qualified to serve Einstein's purposes. For thermodynamics differs in an essential way from other general physical theories. It is not a "constructive theory," attempting, in Einstein's words, "to build a picture of complex phenomena out of some relatively simple propositions" (*2*). It is not like the kinetic theory of gases, for example, which offers an explanation of the observable properties of gases which is based upon assumptions concerning their fundamental structure. Thermodynamics is, rather, what Einstein called a "theory of principle," which starts from "empirically observed general properties of phenomena," such as the nonexistence of perpetual motion, and deduces from them results "of such a kind that they apply to every case which presents itself," without making any assumptions on "hypothetical constituents." Precisely because of this nonconstructive nature of thermodynamics—its independence of particular models—it could serve Einstein as an absolutely sure guide in dealing with the otherwise inexplicable difficulties of the physics of 1900. And this success of thermodynamics as a "theory of principle" suggested the possibility of other such theories, with consequences that we shall see later on.

But even in his very early work Einstein was not content to take thermodynamics only on its own terms, so to speak—to take it as a given, closed system. As a "theory of principle" it had to be intelligible from a more basic point of view. In other words, Einstein also concerned himself with statistical mechanics as a way of providing that deeper understanding of the laws of thermodynamics. He did more: he took seriously, as no one had before him, the insight that statistical mechanics offered into the limits of validity of the thermodynamic laws. Einstein was the first to investigate the fluctuation phenomena which must exist if the statistical interpretation of the second law of thermodynamics is sound, and to use them as a uniquely powerful method of probing the molecular world. When he expressed his conviction, in the passage quoted above, that thermodynamics would "never be overthrown," he knew exactly what he meant when he added the qualifying phrase "within the framework of applicability of its basic concepts," since he himself had had the major share in establishing the limits of that framework.

Thermodynamics and Fluctuations

During the autumn of 1900, directly after his graduation from the Swiss Federal Institute of Technology at Zürich, Albert Einstein completed his first scientific paper. This work, "Consequences of the phenomena of capillarity" (*3*), was an attempt to obtain information on the nature of intermolecular forces, the attractive forces that hold molecules together in the condensed states of matter. These forces manifest themselves rather directly in surface phenomena, and Einstein tried to explain the dependence of the surface energy of a liquid on its chemical structure.

Einstein's methods were those of thermodynamics, and the new graduate showed himself to be in full command of this subject. Thermodynamics alone was, of course, powerless to deal with the problem, since the validity of thermodynamic relationships does not even require the molecular structure of matter. Einstein had to work from a hypothesis about intermolecular forces, and in formulating this hypothesis he had let himself "be guided by the analogy of the gravitational force." He assumed, that is, that there is a universal function of the intermolecular distance which determines the potential energy of attraction between *any* two molecules, and that this function is multiplied by two constants, one characteristic of each type of molecule involved. Each molecular constant, in turn, is the sum of constants characteristic of its constituent atoms. This "simplest assumption" on the nature of the intermolecular attraction, together with the thermodynamic relationships among the measurable properties of the liquid and its surface, allowed him to give a reasonable accounting for the experimental material at his disposal. Einstein's calculations could not, however, determine the form of the postulated universal potential function or throw any light on how or whether it was connected with gravitation.

Despite these limitations, which were intrinsic to his approach, Einstein continued to work on this problem. Several months later, in April 1901, he wrote to his Zürich classmate Marcel Grossmann (*4*, p. 53), "I am now almost sure that my theory of the power of attraction of atoms can be extended to gases and that the characteristic constants for nearly all elements could be specified without undue difficulty." He was also still hopeful of establishing the connection between gravitation and intermolecular attraction—and even of using his work on molecular forces as a doctoral thesis. What moved him most, however, appears in his next sentence: "It is a magnificent feeling to recognize the unity of a complex of phenomena which appear to be things quite apart from the direct visible truth."

After completing a second paper (*5*) on intermolecular forces, in April 1902, Einstein dropped this approach once and for all. In just 5 years the mature physicist of 28 would look back on these papers and refer to them (in a letter to Johannes Stark) as "my two worthless beginner's works" (*6*). He had wrought wonders in the intervening years and could afford to scorn his maiden efforts. They had, however, underlined for him what could and could not be done by purely thermodynamic methods, and it is no accident that in the spring of 1902 Einstein turned his attention to statistical mechanics.

The problem he set himself in the first (*7*) of a series of three papers that appeared between 1902 and 1904 was to provide an adequate foundation for thermodynamics—that is, to derive the laws describing equilibrium and irreversibility from the general equations of mechanics and the theory of probability. The problem was not a new one: Ludwig Boltzmann had wrestled with it throughout the previous quarter of a century, and J. Willard Gibbs's treatise on the subject appeared in the same year as Einstein's paper. Einstein was, of course, unaware of Gibbs's work; indeed, he remarked some years later (*8*) that, had he known of it, he would not have published his own work. It would also appear that Einstein knew Boltzmann's ideas only through the Viennese master's *Lectures on Gas Theory* (*9*), which are a good deal less explicit on these matters than his memoirs of the 1870's (*10*). In any case Einstein developed the subject of statistical mechanics for himself, obtaining the principal features of the theory in this first paper: the canonical distribution, the equipartition theorem, and the physical interpretations of entropy and temperature.

Although his discussion was based directly on the mechanical equations of motion for the particles constituting a physical system, Einstein remarked at several points that only a few very general features of these equations really entered the argument. This suggested to him the possibility that thermodynamics might rest on statistical foundations more general than any previously considered. He returned to this suggestion the following year (*11*), redeveloping the theory on the basis of these more general assumptions. One especially noteworthy feature of this 1903 paper is Einstein's interpretation of the probability of a macroscopic state. Instead of trying to introduce equally probable configurations on a priori grounds, Einstein let the natural motion of the system determine the probabilities of its various states. The probability of a state is thus defined as the fraction of any long time interval that the system spends in that state, in the course of its evolution in time. This interpretation, due originally to Boltzmann (*12*), was one that Einstein would make peculiarly his own.

The last paper (*13*) of this series of three on statistical mechanics struck a new note as Einstein tried to find the significance of the fundamental constant of the theory. This constant κ, later known as Boltzmann's constant, appears in the typical factor of the distribution law, exp $(-E/\kappa T)$, where E is the energy of the system and T is its absolute temperature. It appears too in the relation between the entropy S and the probability W of a state,

$$S = \kappa \ln W. \quad (1)$$

Now it had been known for many years that the average kinetic energy of the translational motion of a gas molecule was $(3/2)\kappa T$, and that as a consequence κ could be expressed as the ratio of the gas constant R to Avogadro's number N_0, the number of molecules in a gram molecule of the gas,

$$\kappa = R/N_0. \quad (2)$$

Einstein found another way of looking at this fundamental constant. From the basic distribution law he could calculate not only the average value $\langle E \rangle$ of the energy of a system, which represented the thermodynamic internal energy of the system, but also the fluctuations about that average. He showed that this mean square fluctuation $\langle \Delta^2 \rangle$, defined as $\langle (E-\langle E \rangle)^2 \rangle$, satisfies the equation

$$\langle \Delta^2 \rangle = \kappa T^2 \frac{d\langle E \rangle}{dT}. \quad (3)$$

Einstein remarked about this equation: "The absolute constant κ therefore determines the thermal stability of the system. The relationship just found is particularly interesting because it no longer contains any quantity that calls to mind the assumptions underlying the theory." The true significance of κ for Einstein was that it determined thermal stability, or, in other words, that it set the scale on which fluctuations occur.

Now Einstein was not the first to derive this equation for energy fluctuations. Precisely the same result appears in Gibbs's book (*14*) (along with many other theorems about fluctuations), and Boltzmann was also familiar with the idea of fluctuations. But Gibbs immediately pointed out that the relative fluctuations in any system of ordinary size would be vanishingly small under normal circumstances, since "experience would not be wide enough to embrace the more considerable divergencies from the mean values," and "not nice enough to distinguish the ordinary divergencies." In a similar vein, Boltzmann remarked (*9*, p. 318) that "it seems futile to hope for any observable deviation, even in a very small time, from the limits that the phenomena would approach in the case of an infinite number of molecules."

Einstein also recognized that fluctuations would normally be small, but his first concern was to search for a case where they would not be; measurable fluctuations in energy for any system could serve to determine κ, and with it Avogadro's number and the whole molecular scale of magnitudes. [It is probably worth stressing the fact that these numbers were far from well known at the turn of the century (*15*).] The system Einstein turned to was black-body radiation—that is, the equilibrium radiation in an evacuated enclosure whose walls are maintained at some definite temperature. It was not that there were any measurements of energy fluctuations in radiation, but rather that the mechanism of these fluctuations seemed more evident. That is to say, if one asked for the linear dimensions of a volume of such a size that the root-mean-square fluctuation in energy is of the same order of magnitude as the energy itself in this volume, then that linear dimension clearly ought to be of the order of a wavelength of the radiation. Using the Stefan-Boltzmann law in combination with his fluctuation equation, Einstein could easily show that this linear dimension should be inversely proportional to the temperature, precisely the behavior of the characteristic wavelength at which the black-body spectrum has its peak. Even the value of the constant of proportionality was reasonably well reproduced, in view of the approximate nature of the calculation.

To Einstein this result meant that fluctuations would have to be taken seriously; he closed his paper with the words, "I believe that because of the great generality of our assumptions this agreement ought not to be ascribed to chance." Fluctuations would therefore provide a suitable way of determining Avogadro's number and molecular masses, and this explains Einstein's remark in a letter to his friend Conrad Habicht (*4*, p. 62), written 2 weeks after he had sent this paper off to the *Annalen der Physik*: "I have discovered in the simplest possible way the relationship between the size of the elementary units of matter and the wavelengths of radiation."

A Heuristic Viewpoint

An independent treatment of the principles of statistical mechanics emphasizing for the first time the importance of fluctuations may seem a major achievement for a physicist who had just turned 25. But for Einstein this work was only preparatory to his first attempts to deal with his fundamental concern—the foundations of physics. On 17 March 1905 he sent to the *Annalen* a paper entitled "On a heuristic viewpoint concerning the emission and absorption of light" (*16*). This is the work that physicists always refer to, inadequately, as Einstein's explanation of the photoelectric effect. Its author characterized it more sharply when he referred to it as "very revolutionary," in a letter written a month or so later (*4*, p. 74). For the "heuristic viewpoint" (*17*) of the title was nothing less than the suggestion that light be considered a collection of independent particles, or quanta, of energy, behaving like the particles of a gas—a suggestion that seemed a wanton dismissal of a century of evidence for the wave theory of light. On what basis did Einstein make such an audacious proposal, and what brought him to such an extreme view?

In his 1904 paper that dealt with fluctuations Einstein had referred to the expression for the entropy of a system "found by Boltzmann for ideal gases and assumed by Planck in his theory of radiation." This passing reference to Max Planck is our first indication that Einstein was already aware of Planck's work on the problem of the black-body radiation spectrum, published in 1901 (*18*). Einstein did not cite any of Planck's papers, but his emphasis on the importance of determining κ could well be a consequence of the heavy weight assigned to the fundamental constants in many of Planck's own writings on the radiation problem (*19*). By the spring of 1905, then, Einstein had been familiar with Planck's work for more than a year, a claim that very few indeed could have made at that time. His reflections on that very puzzling theory of Planck's were the origin of his first 1905 paper.

Please note that I am *not* saying that Einstein's idea of quanta, his "very revolutionary" proposal, was a consequence or development of Planck's quantum theory. What Planck had done, after several years of trying to develop a theory of the black-body spectrum within the framework of electrodynamics and thermodynamics, was to introduce the hypothesis that the energy of charged harmonic oscillators which interact with the radiation field can assume only discrete values, integral multiples of $h\nu$. Here ν is the frequency of the oscillator and h is a new fundamental constant of whose ultimate importance to physics Planck was immediately convinced. What apparently struck Einstein most forcibly is what Planck did not do in his theory. Planck had shown that the average energy of one of his oscillators had to be proportional to the spectral density of the radiation at the same frequency. He had derived (*20*) the equation

$$\rho(\nu, T) = (8\pi\nu^2/^2c^3)\bar{E}_\nu(T), \qquad (4)$$

where $\bar{E}_\nu(T)$ is the average energy of an oscillator of frequency ν at temperature T, c is the speed of light, and $\rho(\nu,T)$ is the energy per unit volume and per unit frequency interval of the black-body radiation of frequency ν, at temperature T. This equation is a consequence of classical electrodynamics. What Planck had not noticed is that the average energy of the oscillator, $\bar{E}_\nu(T)$ is also fixed by classical theory, in this case by kinetic theory or statistical mechanics; it must have the value κT, where κ is the constant discussed above.

The combination of this result with Eq. 4 leads to a definite prediction for the radiation spectrum,

$$\rho(\nu,T) = (8\pi\nu^2/c^3)\kappa T. \qquad (5)$$

This spectrum, however, was not only in obvious contradiction with experimental results but it meant that the combination of classical electrodynamics and the kinetic-molecular theory of matter was essentially incapable of dealing with the problem. For Eq. 5 implies an infinite amount of radiation energy per unit volume when the contributions of all frequencies are combined, a result that Paul Ehrenfest later (in 1911) dubbed "the ultra violet catastrophe" (*21*). This catastrophe, hinted at in 1900 by Lord Rayleigh (*22*), was first explicitly discussed by Einstein in the paper under consideration. It prompted some hard thinking, which he summarized in the introductory section of his paper.

"There is a profound formal distinction," he began, "between the theoretical ideas which physicists have formed concerning gases and other ponderable bodies and the Maxwell theory of electromagnetic processes in so-called empty space." The distinction he was concerned with was that between particle and field, between the discrete particles of matter and the continuously distributed field. The existence of this distinction marked a fundamental inhomogeneity in the foundations of physics: the electromagnetic field of Faraday and Maxwell and the material particle of Newtonian mechanics were both essential to the conceptual structure of physics, but there was an unreconciled tension in their coexistence. The problem of the black-body spectrum struck Einstein as an illustration of the need to unify the foundations.

He could not offer a solution to this difficulty on the level at which he would have liked to proceed, but he was not convinced in 1905 that Planck's theory of quantized oscillators provided much of an advance. At any rate Einstein took the trouble to point out that Planck's calculation of the fundamental constants (*23*), including Avogadro's number, from radiation data by means of his own theory could be accomplished just as well by using only the low-frequency limit of Planck's spectrum—namely, Eq. 5. Since Planck's determination of the constants was the only corroborative evidence for the correctness of his work, this argument served to assure Einstein that he could proceed independently.

But if electrodynamics and the kinetic-molecular theory could not be combined to obtain an adequate constructive theory of the radiation spectrum, what line could be followed with any assurance? The answer was evident to Einstein: thermodynamics, a theoretical structure independent of special assumptions, was the only safe guide, and the general statistical interpretation of the second law, which Einstein had made so much his own, could extend the insights offered by thermodynamics itself. Einstein proceeded, then, to take the radiation spectrum in the form first given by Wilhelm Wien (*24*), a form known to be inadequate for long wavelengths but well confirmed by experiment in the short-wavelength region. (He deliberately did not use Planck's form of the spectral distribution despite its greater power to account for experiments over the whole measured range of wavelengths.) By purely thermodynamic arguments Einstein showed that the entropy of black-body radiation in a given frequency interval depended upon the volume of the enclosure in exactly the same way that the entropy of an ideal gas depends upon its volume. Now, so far as one could tell from thermodynamics alone, this result might or might not have any real significance. But, interpreting entropy statistically, Einstein recognized that it was simply the independence of the motions of the gas molecules that produced this form for the entropy. This gave him the assurance to take the next step, the great leap: *if* the entropy of radiation has the same form as that of a gas, and *if* the entropy of a gas has that form *because* it consists of independent particles, *then* radiation too must consist of independent particles. Identifying the appropriate coefficient in the radiation entropy with the number of these particles or quanta gave Einstein the relationship for the energy ϵ of such a particle,

$$\epsilon = \kappa\beta\nu, \qquad (6)$$

where κ is the constant discussed above and β is one of the two constants in Wien's distribution law. Once again Einstein deliberately did *not* relate these light quanta of his to Planck's energy units.

I need not discuss the uses Einstein immediately made of this hypothesis of light quanta—his explanations of the principles of photochemistry and photoelectricity, which would eventually receive complete and quantitative experimental confirmation. It is worth emphasizing, however, that it was not until a year later, in a paper written in March 1906 (*25*), that Einstein recognized the connections between Planck's ideas and his own. He now saw that Planck too had departed from classical ideas and that Planck's theory rested on two apparently incompatible foundations: electromagnetic theory, from which Eq. 4 was derived, and the assumption of discrete energies for the oscillators. Planck's theory was not to be rejected for this reason, but one would have to recognize that Planck had "introduced a new hypothetical principle into physics."

Search for a Theory

Just 2 months after writing his paper proposing the hypothesis of light quanta, in May 1905, Einstein completed another remarkable work, "On the motion of small particles suspended in a stationary liquid required by the kinetic-molecular theory of heat" (*26*). This paper, which explains the Brownian motion, had as its purpose a critical test of statistical mechanics (or the kinetic-molecular theory of heat), since it discussed an observable phenomenon whose very existence depended upon fluctuations from the average thermodynamic behavior. In other words it was a development of the key idea of Einstein's 1904 paper, the search for observable fluctuation effects that could be used to fix precisely the molecular scale of magnitudes. This study of the limits of the range of validity of thermodynamics was at the same time a study of the generality of statistical mechanics: a key idea of Einstein's paper is that the theorems of statistical mechanics must apply as well to microscopically visible colloidal particles as they do to molecules; a colloidal particle has the same average kinetic energy and contributes just as much to the pressure as a molecule does.

Einstein soon found that the methods he developed for studying the fluctuations of suspended particles could also be applied in his continued probings into the significance of Planck's distribution law. In the 1905 paper on quanta he had based his bold hypothesis on an analysis of the distribution law for high frequencies, the Wien distribution. During the next few years he searched for the implications of Planck's distribution law, valid for all frequencies. An analysis of the energy fluctuations of radiation obeying the Planck law showed him that neither the classical field theory of radiation nor the corpuscular theory could account for the situation by itself. His equations indicated that there were two seemingly independent sources of fluctuations, one readily intelligible as due to interfering waves and the other equally readily intelligible as due to density fluctuations in the number of particles. A completely independent analysis of the Brownian motion that a mirror would have to undergo if it were suspended in an enclosure containing a gas and black-body radiation produced exactly parallel results. Both wave and particle "mechanisms" seemed to be demanded by the observed (Planck) radiation spectrum.

These analyses gave Einstein a unique insight into the magnitude and depth of the problems posed by the black-body radiation spectrum and associated phenomena. The real question was how to make a constructive theory from which these strange properties of radiation would follow. To this question Einstein devoted an extraordinary amount of effort over a period of years. We know very little about his many attempts at a theory, but his correspondence leaves no doubt about how hard he tried, and how alone he was in these efforts. In 1908 he wrote to his collaborator J. J. Laub (*4*, p. 87), "I am ceaselessly occupied with the question of the constitution of radiation and am in correspondence on this question with H. A. Lorentz and Planck. The former is an astonishingly profound and at the same time amiable man. Planck is also very pleasant in his correspondence. He has, however, one fault: that he is clumsy in finding his way about in foreign trains of thought. It is therefore understandable when he makes quite faulty objections to my latest work on radiation. He has not, however, said anything against my criticisms. I hope that he has read them and recognized them. This quantum question is so incredibly important and difficult that everyone should busy himself on it. I have already succeeded in working out something which may be related to it, but I have serious reasons for still thinking that it is rubbish."

In July of the following year he wrote to Johannes Stark, who was at that time one of the few physicists who put any faith in the idea that radiation was composed of light quanta (*6*, p. 279): "I am carrying on a lively correspondence with Planck on this subject; he is still stubbornly opposed to corpuscular (localized) quanta. You can hardly imagine what pains I have taken to devise a satisfactory mathematical way of working out the quantum theory. But up to now I have had no success with it."

One has the decided impression that Einstein was working harder at this problem than he ever had before. The carefree tone of his letters of 1905 is now replaced by a sense of pressure and lack of time to work out his ideas. [It may be noted that this was his last year at the patent office in Berne and, contrary to what is sometimes suggested, this job kept him busy—"eight hours of exacting work every day" (*6*, p. 277), as he described it to Stark.]

The kind of radiation theory Einstein was trying to construct in these years is indicated by a number of comments and suggestive hints in his two remarkable papers of 1909 in the *Physikalische Zeitschrift* (*27, 28*). In these papers Einstein outlined the two independent fluctuation arguments, referred to above, which had convinced him "that the next phase of the development of theoretical physics will bring us a theory of light that can be interpreted as a kind of fusion of the wave and emission [particle] theories." He pointed out the insuperable difficulties faced by a wave theory in accounting for phenomena like the photoelectric effect, in which a large amount of energy suddenly appears at one point, even though the intensity of the light wave may be very low. Einstein found the root of these difficulties in the lack of symmetry between emission and absorption: one could hardly conceive of the absorption of a contracting spherical wave by a charge as an elementary process inverse to the emission of an expanding spherical wave, even though both are acceptable solutions of Maxwell's equations. In this respect, at least, a corpuscular theory had formal advantages over a field theory.

The basic problem was to find that modification of the two fundamental theories, particle mechanics and the Maxwell-Lorentz electrodynamics, which would account for light quanta. In the earlier of the two papers (*27*) Einstein suggested one clue in the dimensional equivalence of Planck's radiation constant h and the quantity e^2/c, where e is the electronic charge, the natural unit of electric charge. Now e itself is "a stranger in the Maxwell-Lorentz electrodynamics," since this theory would allow a continuously varying charge and since one has to assume unknown forces holding the electron together; that is, the structure of a charged particle is unintelligible within electrodynamics. Therefore, Einstein surmised, "the same modification of the theory which contains the elementary charge as one of its consequences will also contain the quantum structure of radiation." The wave equation of optics would have to be replaced by an appropriate nonlinear equation in which e^2 would probably appear as a coefficient. The equation would have to be invariant to Lorentz transformations and would have to reduce to the ordinary wave equation for large amplitudes. "I have not yet succeeded in finding a system of equations meeting these conditions which I could see as appropriate for constructing the elementary electric charge and the light quanta." But in January 1909 he was still able to conclude with the words, "The variety of possibilities does not, however, seem to be so great that one has to be scared off by the problem."

In September Einstein's view had changed a bit (*28*). Once again he remarked on his lack of success in finding a theory that could exhibit both wave and particle structures for radiation. The trouble was that all one really knew were the fluctuation properties and these "present small foothold for setting up a theory." After all, he went on, if none of the usual wave properties of light had been known, but only the "wave term" in the energy fluctuations, "who would have enough imagination to construct the wave theory of light on this foundation?" This time he suggested that it might be most natural to consider the appearance of the electromagnetic fields of light as bound to singular points in the same manner as electrostatic fields. Perhaps it was not impossible that all the energy of the field could be considered as localized at these singularities. Such a singular point might be imagined as surrounded by a field of force, something like a plane wave whose amplitude fell off with distance from the singularity. Many such singularities sufficiently close together would produce overlapping fields that might resemble an ordinary wave field. Of course, Einstein said, one should not attribute any value to such a picture of radiation until it could be developed into an exact theory; it was only meant to show that wave and quantum structures need not be considered incompatible.

Einstein's efforts to construct a new fundamental theory that would unify the previously separate concepts of particle and wave, or, more basically, particle and field, had already changed emphasis at least once. In 1905 he had sounded as though he thought the particle might still be the single unifying concept, but by 1909 the field and its singularities had already taken precedence, as we have just seen. These efforts continued for some time: in 1910 he wrote to Laub from Zürich, where he now had a post at the University, that he had "the greatest hopes of solving the radiation problem actually without light quanta," and that he was "incredibly curious how the thing will turn out." But a few days later he reported "another failure in the solution of the radiation problem. The devil played a wicked trick on me" (*4*, p. 116).

Return to Thermodynamics

This period of intense striving toward a constructive theory of radiation seems to have come to an end, at least temporarily, at about the time Einstein moved to Prague, in the spring of 1911, to take up the duties of his first full professorship. His attitude at that time is characterized by a story Philipp Frank has told of his own first visit to Einstein in Prague. Einstein showed him the view from his office window at the University, which overlooked the garden of the neighboring asylum whose unfortunate inmates could be seen strolling about or in heated conversations. Einstein's comment was, "Those are the madmen who do not occupy themselves with the quantum theory" (*29*).

He put the matter more explicitly in a letter to his close friend and favorite "sounding-board" Michele Besso in May 1911. "I no longer ask whether these quanta really exist. Nor am I trying any longer to construct them, because I now know that my brain is incapable of accomplishing such a thing. But," he went on, "I am searching through the consequences [of the quantum hypothesis] as carefully as possible, in order to learn the domain of applicability of this concept" (*30*). Searching through the consequences rather than trying to construct a theory of quanta meant for Einstein a return to thermodynamic mehods, the one sure guide at a time when, to quote the words he wrote many years later (*1*, p. 45), "it was as if the ground had been pulled out from under one with no firm foundation to be seen anywhere upon which one could have built."

He applied his thermodynamic methods to give a new understanding of the photochemical equivalence law (*31*) and to develop further the quantum theory of the specific heat of solids which he had begun in 1907 (*32*). Although I do not want to discuss this work here, I must mention that it was the impressive success of Einstein's theory of specific heat in accounting both qualitatively and quantitatively for Nernst's measurements and in producing unexpected relations among the thermal, optical, and elastic properties of solids which did much to attract the attention of physicists to the quantum theory (*33*).

These thermodynamic studies reached a peak in Einstein's paper "Contributions to the quantum theory" (*34*), read to the German Physical Society in July 1914. In this work Einstein showed that the Planck radiation law could be derived by an argument that made no use at all of the statistical interpretation of the second law. The essential idea of his method was as follows. He considered a gas each of whose molecules had a vibrational mode such that the internal states of a molecule were those of a Planck oscillator with possible energies $nh\nu$. He then proceeded to treat this gas as if it were a mixture of chemical components, each component consisting of the molecules in a particular vibrational state. This meant assuming that these components could, in principle, be separated with the aid of semipermeable membranes. A thermodynamic calculation of the average internal energy of this mixture at equilibrium led directly to Planck's expression for the average energy of an oscillator.

Einstein's discussion also threw new light on Nernst's heat theorem, showing how it required that the lowest energy state of the system be unique (nondegenerate). At an even more profound level it indicated that "the concepts of physical and chemical change of a molecule seem to lose their distinction. A quantum type of change in the physical state of a molecule seems to be no different in principle from a chemical change." Further, because the theory of Brownian motion had obliterated the distinction between a molecule and an arbitrarily extended system, and because Debye had shown that one could successfully ascribe quantized states to extended systems, this statement could be generalized. "A quantum type of change in the state of an extended physical system is to be understood as analogous in kind to the chemical change of a molecule."

Another Theory of Principle

I suppose that my discussion of Einstein's ideas up to this point must seem a little like a discussion of *Hamlet* without mention of the Prince of Denmark, since I have not yet said anything about the theory of relativity. It might appear as though Einstein's most famous work lies outside the limits of any analysis of the role of thermodynamics in his thought. This is not the case.

The special theory of relativity was published in the third of his masterworks of 1905, under the title "On the electrodynamics of moving bodies" (*35*). The concern that prompted this work was the same as that which prompted the theory of light quanta: the difficulties arising from the incompatible features of the two disparate conceptual structures fundamental to physics—Newtonian mechanics and Maxwellian electrodynamics. The difficulty that proved to be the key to the situation, "the germ of the special relativity theory," had struck Einstein as a paradox when he was 16 years old. In the words of his "Autobiographical Notes" (*1*, p. 53): "If I pursue a beam of light with the velocity *c*, I should observe such a beam of light as a spatially oscillatory electromagnetic field at rest. However, there seems to be no such thing—neither on the basis of experience nor according to Maxwell's equations. From the very beginning it appeared to me intuitively clear that, judged from the standpoint of such an observer, everything would have to happen according to the same laws as for an observer who was at rest relative to the earth. For how is the first observer to know, i.e. be able to establish, that he is in a state of rapid uniform motion?" This "first, childish conceptual experiment" (*36*) posed essentially the same problem as the null result of the Michelson-Morley experiment. Mechanics satisfied a principle of relativity, so that all observers in uniform motion with respect to each other are equivalent, whereas the "luminiferous aether," the fundamental medium of electrodynamics, would provide a uniquely preferred frame of reference—undetectable according to the famous experiment of 1887.

This was by no means the only difficulty produced by the juxtaposition of mechanics and electrodynamics, of particle and field, as the various attempts to construct a theory of electrons or to explain mass electromagnetically indicated. Einstein would have liked to attack these difficulties directly by finding the unified theory that must replace the incompatible ones already in existence. As we have already seen, this task was too hard, even for him. "By and by I despaired of the possibility of discovering the true laws by means of constructive efforts based on known facts. The longer and the more desperately I tried, the more I came to the conviction that only the discovery of a universal formal principle could lead us to assured results" (*1*, p. 53).

The model that Einstein had ready to hand for such a universal formal principle was the laws of thermodynamics. Thermodynamics made no direct statements at all about the structure of matter but rather gave a systematic answer to the question "What must the laws of nature be like so that it is impossible to construct a perpetual motion machine of either the first or second kind?" (*37*). In the same way Einstein raised the question "What must the laws of nature be like so that there are no specially privileged observers?" Given, in other words, that Maxwell's equations do hold, what follows from also imposing the equivalence of observers in uniform relative motion? The analysis of simultaneity, the Lorentz transformation, and the whole structure of special relativity form the answer to that question.

This view of special relativity as a theory of principle analogous to thermodynamics, and not a constructive theory, can be found in many places in Einstein's later writings (*1*; *2*, pp. 54, 77, 101). But that does not mean that it is just an interpretation of his early work that he found convenient many years and many theories later. On the contrary, it can be found precisely stated in the context of a peculiarly relevant exchange that Einstein had with Paul Ehrenfest in the spring of 1907.

Ehrenfest (*38*) raised a question about some special features of the motion of a nonrigid and nonspherical electron, a question that had arisen from his reflections of Max Abraham's rigid, ellipsoidal model of the electron. Ehrenfest thought that it should be possible to give a "purely deductive" answer to his question, since Einstein's paper had made the Maxwell-Lorentz electrodynamics into a "closed system." This systematic theory ought then to provide the answers to questions about the forces involved in the motion of an electron with any given assumed structure.

Einstein's answer was immediate (*39*). He remarked first: "The principle of relativity or, more precisely stated, the principle of relativity together with the principle of the constancy of the velocity of light, is not to be interpreted as a 'closed system,' not really as a system at all, but rather merely as a heuristic principle which, considered by itself, contains only statements about rigid bodies, clocks and light signals. Anything beyond that that the theory of relativity supplies is in the connections it requires between laws that would otherwise appear to be independent of one another." He went on to illustrate this by explaining how one could discuss the motion of fast electrons by using the known laws for slow electrons together with the relativistic transformation laws for both kinematical quantities and electromagnetic fields. "Thus," he concluded, "we are by no means dealing with a 'system' here, a 'system' in which the individual laws would implicitly be contained and from which they could be obtained just by deduction, but rather only with a principle that allows one to reduce certain laws to others, analogously to the second law of thermodynamics."

Perpetual Motion Machines

Every reader of Niels Bohr's description (*40*) of his discussions with Einstein on the basic issues of quantum physics must have been struck by Einstein's genius for inventing crucial conceptual experiments. Bohr's responses to these over the years led to a steady development of his own interpretation of what quantum mechanics actually says. Einstein's attempts to capture some paradoxical feature of the quantum theory in a simple experimental situation may be related to his years as a patent examiner, but I think they also express another way in which he brought the spirit of thermodynamics into so much of his thinking. The problem of finding exactly how and where some particular proposal for a perpetual motion machine violates the laws of thermodynamics can often call for considerable insight and ingenuity, even though one is certain that the flaw in the argument is there to be found. Einstein's conceptual experiments, designed to display the contradictions he saw in the quantum theory, sometimes appeared to his colleagues like 20th-century versions of the perpetual motion machine.

There was one difference, however, pointed out by Einstein himself. It is best given in the enthusiastic tones of Paul Ehrenfest, writing to Niels Bohr in 1922 (*41*) and describing the current ideas of his house guest, Albert Einstein. Einstein was concerned once more with the disturbingly contradictory properties of radiation. "Einstein will either go mad over these questions or he will discover something else very deep. He is unbelieveably clever at thinking up such newer and newer crucial experiments. In a very gay public discussion in Amsterdam [a discussion with Einstein on the quantum theory] I put him down as a constructor of perpetual motion machines. He laughingly admitted it, just saying, 'The only thing missing [in the analogy] is the insight that no future construction can lead to an anticlassical result.' "

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41. Unpublished letter from P. Ehrenfest to N. Bohr dated 8 May 1922 [Bohr Archive, Institute for Theoretical Physics, Copenhagen]. This letter is quoted with the permission of the late Mrs. T. Ehrenfest-Afanassjewa and the late Professor Niels Bohr.