important. There are a few letters and some brief manuscripts, such as the one on quaternion integers.

The historical contributions are less impressive than those in the first two volumes. As Hamilton is still largely unstudied by historians of science, this book will certainly interest the scholar working on 19th- and early-20th-century science. But the historian needs not only the printed documents as a source for tracing the development of ideas but also careful annotations. Neither editor appears to have history as his primary interest, and their contributions are brief, consisting only of a short introduction, four short appendices, a brief index, and a few notes, most of them references to other papers in the volume. The comments in the appendices seem to be based primarily on present-day algebra. An interesting review of the applications (including recent ones) of quaternions to physics is brief and incomplete.

Most of the papers concern the theory of quaternions. Hamilton's initial discovery in 1843 established the basic algebraic relations. A quaternion is of the form $A + iB + jC + kD$, where A, B, C, and D are real numbers, and where i, j, and k obey these multiplication rules: $i^2 = j^2 = k^2 = -1$, $ij = -ji = k$, $jk = -kj = i$, and $ki = -ik = j$. Associative and distributive laws are assumed for multiplication of these quantities by numbers, and by each other, but quaternion multiplication is noncommutative. Hamilton called A the scalar part and $iB + jC + kD$ the vector part, using (later) the prefixes S and V before a quaternion to indicate these. The scalar part of the quaternion product of two vectors is the negative of the vector scalar product, and the vector part is the present cross product.

Quaternions contain all the vector mechanism used later, outside the quaternion context, by Heaviside and Gibbs. But other properties may seem strange today. It is proper with quaternions to add a scalar and vector, an operation not allowed in vector algebra. We can speak of the ratio of two quaternions or of the reciprocal of a quaternion, even if that quaternion is a vector. Thus when Hamilton writes Newton's second law for a central force in quaternion notation, the displacement vector α is in the denominator. Hamilton introduced the ∇ operator, a powerful mechanism in both quaternion and vector analysis, in 1846.

A collection of papers affords a perspective of motivational aspects missing from single documents. The genesis of quaternion ideas is of particular interest. Hamilton tells at least four times of arriving at quaternions, with some interesting differences between the tellings. The first (paper 3 in the present collection) is from Hamilton's notebook, 16 October 1843: "I, this morning, was led to what seems to me a theory of quaternions. . . ." The discovery is described in a letter written the next day to John T. Graves and in two later documents, the preface to the *Lectures on Quaternions* and a letter to his son, with the lines about carving the quaternion relations into a bridge.

From these accounts we can see some important sources for Hamilton's quaternion work. First, much of the initial impetus comes from the study of complex numbers, the theory of couples. Hamilton, like others, was dissatisfied with the common loose way of introducing and using negative and complex numbers, feeling that it should be possible to place algebra and analysis on a more secure basis. Hamilton was not a pure mathematician, however; reasoning from physical analogy was important to him. He first thought, under Kantian influence, that complex numbers represent "the algebra of pure time," but gradually mentioned this motivation less and less.

If complex numbers correspond to the plane, Hamilton argued, there should be a similar algebraic structure, the triplets, related to three-dimensional space. He sought ways of adding and multiplying triplets corresponding to geometric operations in three dimensions. Hamilton, along with others, is intuitively striving toward the concept of "an algebra," breaking away from the notion of "the" algebra and culminating in the 20th-century view of abstract algebras. In these discussions on triplet multiplication we see a factor very important in 20th-century physics and mathematics, the intuitive reliance on mathematical elegance coupled with a willingness to engage in algebraic "play" in manipulating symbols. Speaking of the quaternions, Hamilton says, ". . . whether the choice of the system . . . has been a judicious, or at least a happy one, will probably be judged by the event, that is, by trying whether these equations conduct to results of sufficient consistency and elegance."

So this volume, in spite of its limitations, will be useful to the historian of science. One hopes that the time that elapses before the final volume is published will be less than the 27 years separating volume 2 and volume 3.

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Unease about the New Physics

Letters on Wave Mechanics: Schrödinger, Planck, Einstein, Lorentz. K. PRZIBRAM, Ed. Translated from the German with an introduction by Martin J. Klein. Philosophical Library, New York, 1967. xx + 75 pp., illus. $6.

The volume of *Briefe zur Wellenmechanik* which was compiled in 1963 for the Austrian Academy of Sciences by Karl Przibram, and which now appears in this English translation with an introductory essay by Martin J. Klein, includes 21 letters exchanged between Schrödinger and three of the most distinguished scientists of his time: Planck, Einstein, and Lorentz. Fourteen of these letters were written between April and June of 1926, immediately after Schrödinger's discovery of the wave mechanics. An additional six letters between Schrödinger and Einstein were selected from the years 1928, 1939, and 1950 to illustrate the later, more philosophically oriented and more elaborate interpretations of quantum mechanics. The personal correspondence of these men uncovers the contemporary reactions and the inner conflicts, expectations, and disappointments associated with the realization of a major accomplishment in 20th-century physics.

Schrödinger wrote his six fundamental papers on wave mechanics within a period of six months in 1926, when he was 39 years of age. Clearly, these papers demonstrate Schrödinger's thorough mastery of Hamiltonian mechanics and the use of eigenvalues in the physics of continua. They also reveal the general influence of a Boltzmannian statistical interpretation of atomic events, as well as the more immediate stimulus of Einstein's studies of the wave-particle duality for radiation and de Broglie's statistical hypothesis (1923–25) of the wave characteristics of particles. But beyond this we sense that Schrödinger was troubled and therefore motivated by the loss of "physical intuition" suggested by the quantum concept of discontinuous energy jumps. Confronted by the growing inadequacies of the Bohr theory, and uneasy about the challenge which quantum theory presented to causality, continuity, and determinism, Schrödinger in 1926 explored the statistical side of atomic physics and thermodynamics in the hope of being able to establish a continuous transition from micromechanics to macromechanics, that is, to relate the quantum theory to classical mechanics in the same way that wave optics is related to geometrical optics. In particular, the energy concept, he believed, should not simply be transferred from macroscopic experience to micromechanics. His whole soul rebelled at the "bizarre discontinuity" of exchanges of energy—"this damned quantum-jumping," as he called it on another occasion.

To accomplish his objectives Schrödinger tried to show in his first paper that the usual rule of quantization, for the simplest case of the nonrelativistic, unperturbed hydrogen atom, could be "replaced by another requirement in which there is no longer any mention of *integers*." Treating quantization as "a proper value problem," he looked upon the integral properties of the hydrogen atom in the natural way in which the number of nodes of a vibrating string corresponds to an integer. He even went on to suggest that several different vibrations could take place simultaneously so that the different Bohr frequencies could be treated as acoustical beats between different proper vibrations. As Martin Klein indicates in his perceptive seven-page introduction to this volume, Schrödinger felt that his wave mechanics was applicable to a wide range of basic atomic problems. More important, it was also elegant and intuitively natural from the standpoint of classical mechanics when compared with the perplexities of the existing quantum theory.

In 1926 Schrödinger wrote to Planck: "It all resolved itself with unheard of simplicity and unheard of beauty; it all came out exactly as one would have it, quite straightforwardly, quite by itself and without forcing." Planck, who was conservative and traditional, wrote Schrödinger, in 1926 when he was 68, that he had read his article "the way an inquisitive child listens in suspense to the solution of a puzzle he has been bothered about for a long time." Eagerly awaiting Schrödinger's visit and lecture in Berlin, Planck wrote: "There are always many questions to be asked, for the appetite increases with eating."

A year later Schrödinger was eager to learn how the quantum theory was being judged in Berlin, and so he asked Planck:

> Is what the matrix-physicists and q-number-physicists say true—that the wave equation describes only the behavior of a statistical ensemble. . . ? I would willingly believe it since the interpretation is really much more convenient, if I could only pacify my conscience and convince it that it is not frivolous to get off so easily in overcoming the difficulties. . . .
>
> Well as God wills; I keep quiet. That is if one really *must*, I too will become accustomed to such things.

Lorentz, at age 72, responded to Schrödinger's work with a detailed and cautious, but friendly, analysis and critique of some of the difficulties and misgivings which Schrödinger already had puzzled about—notably the dispersion of wave packets, the question of waves for a many-bodied system, and the problem of radiation interpreted as a beat frequency phenomenon. Lorentz wrote:

> If I had to choose now between your wave mechanics and the matrix mechanics, I would give the preference to the former, because of its greater intuitive clarity, so long as one only has to deal with the three coordinates *x, y, z*. If, however, there are more degrees of freedom, then I cannot interpret the waves and vibration physically, and I must therefore decide in favor of matrix mechanics.

Again, in the same letter:

> If one wants to imagine that electrons are not always little planets that circle about the nucleus, and if one can accomplish something by such an idea, then I have nothing against it. But if we take a wave packet as model of the electron, then by doing so we block the way to restoring matters. Because it is indeed asking a lot to require that a wave packet should condense itself again once it has lost its shape.

As for the analogy with beats and combination tones Lorentz wrote:

> So I have lost my taste for explanations by means of sum and difference oscillations to some extent, but I can certainly reacquire it if your theory succeeds in other respects.

Schrödinger replied:

> Would you consider it a very weighty objection against the theory if it were to turn out that the electron is incapable of existing in a completely field-free space? Or perhaps even that "free" electrons do not permanently keep their identities at all in the usual sense?

Schrödinger to Lorentz:

> The frequency discrepancy in the Bohr model . . . seems to me, (and has indeed seemed to me since 1914), to be something so *monstrous*, that I should like to characterize the excitation of light in this way as really almost *inconceivable*.

The Einstein-Schrödinger letters on wave mechanics in this collection cover a longer time span. They therefore provide a deep insight into both men's aversion for the Copenhagen interpretation of quantum mechanics. In 1926 Schrödinger wrote to Einstein: "Your approval and Planck's mean more to me than that of half the world." Einstein replied: "I am convinced that you have made a decisive advance with your formulation of the quantum condition, just as I am equally convinced that the Heisenberg-Born route is off the track."

Within two months after the completion of his decisive papers on wave mechanics Schrödinger had demonstrated the logical equivalence between his wave mechanics and the 1925 matrix mechanics of Heisenberg, Born, and Jordan. The following year, in 1927, Schrödinger accepted the chair of theoretical physics in Berlin when Planck retired. There, within the sympathetic environment of Einstein, Planck, and von Laue, he continued to explore and criticize philosophically the uneasy dualistic features of the Copenhagen-Göttingen-Cambridge interpretation of the quantum theory of Bohr, Born, Heisenberg, Jordan, and Dirac.

By 1928, wave and matrix mechanics had acquired the essential characteristics of what Born already in 1924 had called "quantum mechanics." Einstein told Schrödinger in 1928:

> I think that you have hit the nail on the head. . . . The Heisenberg-Bohr tranquilizing philosophy—or religion?—is so delicately contrived that, for the time being, it provides a gentle pillow for the true

believer from which he cannot very easily be aroused. So let him lie there.

But this religion has so damned little effect on me. . . . I cannot make head or tail of it mathematically. My brain is also too worn out by this time.

Schrödinger wrote to Einstein in 1950:

It seems to me that the concept of probability is terribly mishandled these days. . . . the quantum mechanics people sometimes act as if probabilistic statements were to be applied *just* to events whose reality is vague. . . . [The] proper basis of reality is set aside as trivial by the positivists. . . . The present quantum mechanics supplies no equivalent. It is not conscious of the problem at all; it passes by with blithe disinterest.

Einstein answered:

You are the only contemporary physicist, besides Laue, who sees that one cannot get around the assumption of reality—if only one is honest. Most of them simply do not see what sort of risky game they are playing with reality—reality as something independent of what is experimentally established. . . . Only one of the tools of our trade remains—the field concept, but God knows whether this will stand firm. I think it is worthwhile to hold on to this, i.e. the continuum, as long as one has no really sound arguments against it.

This volume of letters is a crucial fragment of a critical chapter in the history of quantum mechanics, and incidentally provides substantial insight into the personal feelings and reactions of four great minds in action.

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A Closet Door Briefly Opened

The Nature of Time. Report of a meeting, Ithaca, N.Y., June 1963. T. GOLD and D. L. SCHUMACHER, Eds. Cornell University Press, Ithaca, 1967. xvi + 248 pp., illus. $8.75.

Physics is far from being a closed subject, so it is not very daring to suppose that some cherished physical concepts might be poorly understood, or even incorrect. Some such questions are popular subjects for research. Other questions people have tended to ignore, as skeletons in the closet, on the sound principle that it probably would be a waste of time to keep sorting through the skeletons until someone can come up with some more concrete starting points.

The present book contains some skeleton rattling by a group of scientists and philosophers who met to consider the merits or otherwise of time as we now think we understand it. The result is a lively discourse on some amusing and perhaps even serious questions.

One of the more prominent examples discussed in the book is the distinction between the roles of time in physics on the macroscopic level (thermodynamics) and on the microscopic level. With a minor exception, the microscopic laws of physics appear to be symmetric against time reversal, in the sense that one can derive from one physical situation another perfectly good one by reversing the direction in which time is supposed to flow. This is not true in thermodynamics, where a preferred direction of flow of time is defined by the second law, the statement that entropy increases with increasing time. The conventional resolution of this dilemma is to identify entropy as a measure of probability, so that the second law says only that systems evolve from states of lower probability to states of higher probability. This recourse to initial conditions works well enough in a closed system. It does leave open the amusing idea that there might be, somewhere else in the universe, a system set to evolve in the opposite sense in time.

In fact, it appears that the local laws of physics are not strictly invariant against time reversal, for people have observed elementary particle decays (of the K^0 meson) that should not have happened under complete symmetry. Unfortunately, this asymmetry was only discovered in 1964, a year after the conference, so we do not have a discussion of whether this small defect would be of moment to people set to live out their lives in the opposite sense of time.

There is yet the older problem of radiation. It is a common enough experience that, when a charge is accelerated, an electromagnetic wave propagates away from the charge, but no one has reported the time-reversed chain of events. Wheeler and Feynman gave in 1945 a beautiful scheme for preserving the symmetry of the local laws of physics, again assigning the time asymmetry to the initial conditions. However, the scheme works only inside a box with perfectly absorbing walls. Apparently if you are determined to preserve local time symmetry you have to make some strong statements about the global nature of the universe.

It is still not clear just how serious these problems are, or where they would lead us. It is, however, pleasant to have this new collected discussion of these and other aspects of time.

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Tribute to Bethe

Perspectives in Modern Physics. Essays in honor of Hans A. Bethe on the occasion of his 60th birthday, July 1966. R. E. MARSHAK, Ed. Interscience (Wiley), New York, 1966. xii + 673 pp., illus. $19.50.

The physicist I. I. Rabi is fond of recalling the good old days when there were not experimental physicists or theoretical physicists but just plain old physicists. Nowadays there are many subdivisions among theorists and experimentalists, whose individual specialties have developed so much that these groups have trouble communicating, to say nothing of being able to work in several areas. In this reviewer's experience, there are few physicists under the age of 40 who can discuss in any depth even one of the branches of physics outside their own specialty. No one is to blame for this—it is simply what happens when a large number of aggressive, ambitious, and intelligent people make the type of assault on a field that took place in physics after World War II.

One can only envy a man like Hans Bethe, who at one time not only knew essentially all of physics but worked actively in most of the important areas. Bethe himself has slowed down now—today he is an expert only in atomic physics, nuclear physics, and high energy physics. He feels that quantum field theory and elementary particle physics are for young people.

This volume, composed of articles from Bethe's students and friends, is almost overpowering, revealing as it does the truly monumental contributions to physics he has made. There are over 40 papers by prominent physicists of all varieties—even experimentalists—covering nuclear physics, solid state physics, particle accelerators, quantum electrodynamics, particle physics, astrophysics, quantum field theory, the theory of nuclear reactors, cosmic ray physics, thermonuclear weapons, and geophysics. Bethe's well-known impor-