proach based on (i) and (ii) above and tied it to the von Neumann and Morgenstern theory of games. A third theme of Neyman and Pearson was to stress the long-run frequency interpretation of operating characteristics, at the expense of, almost to the point of denial of, the interpretation of individual bodies of statistical data.

To accept the relevance of operating characteristics is to accept the theory on its own terms. At this level, criticism of the theory becomes a technical subject largely beyond the scope of this review. Perhaps the chief weakness of the theory is that of inconclusiveness, for, if such conditions as unbiasedness of estimators or fixed size of tests are set aside as artificial, then Wald's theory of admissible decision functions reaches the conclusion that some Bayes rule must be used, thus leading full circle to the central Bayesian difficulty of choosing a prior distribution. On the other hand, if fixed-size tests are to be evaluated in terms of power functions, inconclusiveness again appears because optimum tests in general vary greatly with the choice of alternative hypotheses, and it may be no more plausible to pick out a specific alternative hypothesis than to pick out an intuitively desirable test statistic. Of course, any scientific theory must contend with internal difficulties, so that judgment and good sense play important parts in its use. It seems probable that many statisticians will long continue to accept the Neyman-Pearson theories in such terms, and the future role of these theories in teaching and practice is thus assured.

The deeper difficulties of the theory appear when it is recognized that the interpretation of individual sets of data is after all a necessity. A theory which deals only in long-run frequencies does not possess in itself the resources to deal with such interpretation. Consider for example the comments of Bowley on Neyman's initial and lucid exposition of the theory of confidence intervals in 1934. Regarding such limits for an unknown population proportion, Neyman quotes Bowley's essential comments as:

a) Does it really lead us towards what we need—the chance that, in the universe which we are sampling, the proportion is within these certain limits? I think it does not;

b) I think that we are in the position of knowing that *either* an improbable event has occurred, *or* the proportion in the population is within the limits.

Neyman essentially agreed with Bowley. [In a paper included in his *Selected Papers* Pearson had made a point similar to (*a*) some five years before.] But Neyman went on to identify the confidence method with long-run frequency interpretations. Perhaps this is too great a jump, for if (*b*) holds then it is logically necessary and not just a convenience to restrict confidence coefficients to fractions close to unity. Moreover, if (*a*) and (*b*) hold, then it need trouble no one that a 95-percent confidence interval might, conditional on some observable aspect of the data, have confidence only 80 percent or even 100 percent, where 100 percent obtains if a confidence procedure should produce an interval containing all possible values of the parameter. But most users of confidence regions are quite disturbed by such properties, their uneasiness being a sign of the difficulty of not reading a posterior probability interpretation into confidence coefficients. A similar point may be made regarding hypothesis testing, conveniently in terms of Pearson's hypothetical example of King Hiero's crown (*Selected Papers,* pp. 279–83). The king is represented as being happy with a decision procedure which offers chance .01 of hanging an innocent man and chance .01 of failing to hang a guilty man, under plausible assumptions. Would the king feel happy about applying the rule to hang a man about whom he had some extra, perhaps intangible, information pointing towards innocence? The existence of such information need in no way alter the long-run objective operating characteristic .01, but it could certainly affect one's feeling about the relevance of the operating characteristic to a specific real situation.

The criticisms just made are very much in the spirit of Fisher, who did believe in the necessity of being concerned with individual circumstances and did believe that his fiducial argument, suitably hedged, was protected in a way that the confidence argument is not. Moreover, the Bayesian view is very close to Fisher in this regard. I believe that Neyman and Pearson have never met the criticisms head on, nor indeed is it possible to do so satisfactorily. The best defense is perhaps an appeal to pragmatism and common sense; after all, when confidence coefficients or significance levels are obviously not relevant this fact will usually be recognized and a new, more suitable procedure constructed. In view of the counterbalancing difficulties in applying reasonable Bayesian methods, the pragmatic defense is probably acceptable for most widely used procedures; but constant care is required.

We are in little danger, I think, of rejecting any true theory of statistical inference in the near future, and must be satisfied with partially accepting each of two false, or at least oversimplified, theories. An uneasy truce between the posthumous contributions of Bernoulli and Bayes seems likely to go on for some time. But we have Neyman and Pearson to thank for their important role in lifting the debate to new levels of theoretical sophistication where the issues can be seen with great clarity.

A. P. DEMPSTER
Department of Statistics,
Harvard University,
Cambridge, Massachusetts

Quaternions and Other Topics

The Mathematical Papers of Sir William Rowan Hamilton. Vol. 3, Algebra. H. HALBERSTAM and R. E. INGRAM, Eds. Cambridge University Press, New York, 1967. xxiv + 672 pp., illus. $37.50. Cunningham Memoir No. 15.

The first two volumes of *The Mathematical Papers of Sir William Rowan Hamilton,* edited by A. W. Conway and J. L. Synge under the auspices of the Royal Irish Academy, appeared in 1931 and 1940. Now the work is to be brought to completion by H. Halberstam and R. E. Ingram. Volumes 1 and 2 dealt with geometrical optics and dynamics, respectively. The fourth and final volume, promised in a few years, will be on analysis and geometry. The main topic of volume 3 is the algebra of quaternions. This volume also includes early work on complex numbers and papers on the icosian calculus.

Almost all the articles in volume 3 are published papers. The editors have included the preface to *Lectures on Quaternions* because it "lends cohesion and perspective" to other material in the volume. They reprint the (previously published) manuscript pages written on the day when quaternions were discovered. Several other manuscripts are also included; the major document on the icosian calculus is perhaps the most 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.

ALFRED M. BORK
*Harvard Project Physics,
Cambridge, Massachusetts*

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 thor-