mal studies, and on the uncoupling of contraction and activation by disruption of the T-tubules. But even 600 pages of text do not allow a full treatment of such a vast subject. The remarkable thing is how much has been covered, how well, and how readably, of a many-splendored field which is notoriously difficult even for the expert.

One would wish the book to be widely available to graduate students and advanced workers alike. But here is a catch—the price.

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Statistical Inference

Likelihood. An Account of the Statistical Concept of Likelihood and Its Application to Scientific Inference. A. W. F. EDWARDS. Cambridge University Press, New York, 1972. xvi, 236 pp., illus. $13.50.

The technical construct *likelihood* was isolated and named 50 years ago by R. A. Fisher. Since then, many theories of statistical inference based on assumed parametric families of probability models have been elaborated, including Fisher's own theories of information and fiducial probability, the frequentist operating characteristic theories originating with J. Neyman, E. S. Pearson, and A. Wald, and the more recent resurgence of Bayesian theory. Likelihood can be recognized as playing a uniquely important and unifying role *inside* each of these theories. A small group of statisticians has maintained that likelihood is itself the *whole* of statistical inference, meaning that the sole task of the statistician is to compute and directly interpret likelihood functions determined by observed data, and implying that the working statistician has little need for the large corpus of theory which has occupied mathematical statisticians for many decades. Fisher advocated direct likelihood inference in circumstances where his fiducial methods could not be applied. G. A. Barnard in England and A. Birnbaum in the United States have ably defended inference from likelihood alone. A. W. F. Edwards gives an account of the exclusively likelihood viewpoint in the book under review, which is "aimed at the scientist rather than the statistician."

To fix ideas, consider a sequence of independent trials which can result in success (S) or failure (F) where the probability of success on a single trial is an unknown constant p. If a sequence of eight trials produces the data

$$SSFSFSSS,$$

then the statistician computes the likelihood function

$$L(p) = p\times p\times(1-p)\times p\times(1-p)\times p\times p\times p = p^6(1-p)^2.$$

Thus likelihood can be succinctly described as the *probability of what happened* considered after the fact. In general, a sequence of n trials including a successes and $n-a$ failures, in any order, yields

$$L(p) = p^a(1-p)^{n-a},$$

which is a likelihood function on $0 < p < 1$. This function is convex with a maximum at $p = a/n$, where the width of the peak is roughly proportional to $n^{-1/2}$. Edwards uses the term *support* for the natural logarithm of likelihood. Likelihood is regarded as defined only up to an undetermined scalar multiple, so that only relative values such as $L(p_1)/L(p_2)$ are meaningful. Correspondingly, only support differences are meaningful. Edwards introduces the handy concept of *m-unit support limits,* meaning the extremes of the range of parameter values such that the support is within m units of its maximum. It turns out that 2-unit support limits from reasonably large samples on many common models are similar to ± 2 (standard deviation) limits for parameter determinations, as given by Bayesian or frequentist theory, thus forming a bridge to more traditional methods of inference.

Edwards clearly and simply describes ideas extending those sketched above, using mainly examples of frequency counts drawn from genetic studies. The short and readable chapters follow a common format of introduction, technical discussion, and summary. After chapters presenting basic concepts, there are chapters which relate and contrast likelihood inference with familiar alternatives such as Bayesian inference, maximum likelihood, and significance testing. Other chapters explore technical matters such as the handling of several parameters, the relation of likelihood to information, and anomalous or pathological behavior of certain likelihood functions.

The tone of the book is good-naturedly polemical. Much space is devoted to oft-heard criticisms of different approaches to inference, especially those well represented at Cambridge University past and present, but virtually no self-critical judgment is directed at the difficulties of the exclusively likelihood school. This lack of balance is a serious defect in a book on a subject so controversial as statistical inference. Also, I believe that professional statisticians will find the treatment rather superficial. For example, the illustrations chosen by Edwards give no feeling for how likelihood ideas could possibly cope with real data sets of even moderate size and complexity, where 20 or 30 parameters are more typical than 2 or 3. For another example, I am unable to see what general principle is invoked to permit the use of likelihood based on the t-distribution as a device for eliminating σ when μ is the parameter of interest (pp. 116–17, 194–95).

Edwards ignores two fundamental difficulties of the likelihood school. First, any use of likelihood is predicated on an assumed family of precise parametric probability models, but Edwards does not discuss where these come from, how one assesses their fit to data, or what are the consequences of using false models. On the contrary, he criticizes as unnecessary and illogical the main tool of statistical inference which purports to assess fit between data and a family of models, namely significance tests. Instead, he offers *support tests* which are really estimation devices in disguise. It is one thing to warn the user of tests about their limitations, but an extreme step to rule out altogether the practice of retaining null hypotheses until they are overturned by empirical data.

Second, there is a major question of what likelihood means. Edwards asserts that the log likelihood of a hypothesis measures support for that hypothesis in the data, but how does this formal definition correspond to an intuitive concept of support? For an intuitive understanding, one must go back to the original definition of likelihood as the probability of what happened. It follows that likelihoods resemble the tail areas of significance testing, which Edwards derides, in the sense of being probabilities considered after the fact. As with significance tests, therefore, the direct interpretation of likelihood can only be to rule out certain hypotheses and not to offer positive support for the remaining hypotheses. If hypothesis H_0 has likelihood 1/20 or 1/100 of the likelihood of another contemplated hypothesis H_1, then I may feel that I can reject H_0 as implying that the observed data represent too improbable an occurrence. Since Edwards does not pursue this line of reasoning, he does not exhibit the notable distinctions between likelihood testing and tail area testing. For example, 2-unit support limits for a normal mean, while implying an approximate 5-percent significance level in terms of tail area, correspond to a likelihood improbability factor of only $1/e^2 = 1/7.4$ relative to the most likely value, thus showing that likelihood testing implies wider limits than tail area testing. This type of distinction is generally true, and may be the price to be paid for the use of the logically more satisfying concept of likelihood.

The direct interpretation of likelihood deserves more exploration and ultimately more use. Edwards's book, despite its limitations, is therefore welcome as part of a healthy movement in statistics.

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A Complex Sediment

Till. A symposium, Columbus, Ohio, May 1969. RICHARD P. GOLDTHWAIT, Ed., assisted by Jane L. Forsyth, David L. Gross, and Fred Pessl, Jr. Ohio State University Press, Columbus, 1972. xii, 402 pp., illus. $20.

When a symposium volume provides basic material suitable for students and interpretative articles that will provide for years of dispute, then it should have a wide audience. George W. White, a long-time till enthusiast and teacher of glacial geologists, to whom this volume is dedicated, sets the tone for the collection by warning us against the "naive assumption that till is till." The contributors to this volume are geologists, soil scientists, and agronomists and are from state, provincial, and national geological surveys, research councils, and academic institutions. Their combined work shows that till is a complex sediment whose niche in the glacial history of an area is not yet clearly understood everywhere.

An introductory chapter by R. P. Goldthwait is a fine summary of till, its origin, transport, and deposition; an excellent bibliography provides a guide to the scientific background. Goldthwait is particularly suited to write such an introduction because of his long association with field problems in glacial geology in many parts of the world. It is also appropriate that one of the most indefatigable field and laboratory workers in till studies, Alexis Dreimanis, should present a summary that details how a large number of geologists in North America classify and study till, and also a later paper on the distribution of rock and mineral fragments in till.

The disputes will arise from many articles. Under the general heading of Genesis, individual papers vary from a simple enlargement on previously published material (Stewart and MacClintock), which makes a simplistic, unsubstantiated explanation of till fabric preserved in an "ablation till," to the detailed, meticulous field and laboratory work of Boulton in Spitsbergen and Pessl in Connecticut. An article by Drake on the genesis of tills found in New Hampshire strikes a glancing blow at the problems encountered in New England, where the combination of topography and lithology makes it difficult to identify lodgment and ablation till. Indeed, the attachment of genetic names to till bodies without sufficient evidence may do more to block a real understanding of the origin of tills than any other act.

The section on Thickness and Structure includes studies on large-scale block inclusions in Saskatchewan, stacking of single sheets of till, and the succession of relatively thin sheets of till in northeastern Ohio and northwestern Pennsylvania and, curiously alone in this field-oriented symposium, a paper on theoretical rates of till deposition on irregular topography.

Several authors write on Stratigraphic Correlations, which are perhaps not as immediately useful in trying to understand the origin of till but which are certainly important as regional building blocks in the construction of Pleistocene history. Others contribute to a section on Composition, in which details of mineralogy and grain size are used to identify and differentiate till sheets.

J. T. Andrews, an exacting worker in the field of till fabrics, warns of the lack of reliability of ordinary till-fabric diagrams and touches on the many pitfalls in fabric studies. Yet it seems that some clear directional trends, which agree with other directional indicators, are shown in rose diagrams from relatively widespread single-sample localities (as in articles by Evenson and by Ramsden and Westgate). Although only four papers are listed in the section on Fabric, four other papers (under Genesis) use till fabric as the basis for their presentations.

A paper on a Pleistocene mudflow, by Hester and duMontelle, brings up a question I raised many years ago in mapping in New England: how much ground moraine truly is emplaced as subglacial till and how much identical-appearing till is superglacially derived flowtill deposited singly or in layers from the last large ice blocks to melt away. If both superglacial and subglacial tills can appear identical, or nearly so, as shown by studies in this symposium and elsewhere, then many parameters must be studied, both in the original environment (as by Boulton) or in the landscape abandoned by glaciers (as by Drake). Boulton unknowingly paraphrases a thought that T. C. Chamberlin wrote to N. S. Shaler of Harvard in 1885, one that applies not only to till fabric and till genesis, but also to the whole field of glacial geology—the truism that "so many different processes can produce similar results."

Goldthwait's summary introduction, the field data, and the inferences presented in this collection ought to be available to every glacial geologist, for much can be derived from the study of them. It is too bad the editors were not more exacting: "sheer planes," poorly reproduced and out-of-focus photos, references not cited or incorrect, typographical errors, and numerous misspellings mar the smooth reading. But these are really small complaints to set against the conception and usefulness of this symposium volume.

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Scattering Phenomena

Raman Spectra of Molecules and Crystals. M. M. SUSCHINSKII. Translated from the Russian edition (Moscow, 1969). Israel Program for Scientific Translations, New York, 1972 (distributor, Wiley, New York). x, 446 pp., illus. $34.

Most scientists working in the field of Raman spectroscopy no longer dwell on the physical nature of the scattering process, but instead consider Raman scattering as an analytical technique for investigating the structure of matter. Authors writing on the subject have neglected detailed discussions of the physical basis of Raman scattering in favor of more involved treatises on experimental techniques or chemical