calculate the form factors is the second major topic. The conclusion is drawn that adequate fits to the experimental results have been obtained in terms of the known vector mesons ρ, ω and φ, but these fits are not displayed.

The final article, by Paul T. Matthews, discusses unitary symmetry. Here the elements of the theory of Lie groups are neatly discussed. One can learn "all about" the groups U(1), SU(2), SU(3), SU(4), SU(6), SL(2,C), U(2,2) and U(6,6). Matthews presents the important physical conclusions related to invariance of physical theories under these transformations.

The authors of these progress reports have made major contributions to their subjects, of which they speak with authority. The writing is uniformly good, but only those possessed of unusual gifts should expect to read all parts of these articles without difficulty. This volume and presumably the remaining volumes will be especially useful as reference works to those interested in following developments in these fields. An author and a subject index add to the value of the book.

At least one common thread runs through these articles. They all show how it is that when so much is known, so much remains to be known. In this time of rising costs, it appears useless to deplore it, but volume I is not inexpensive.

* * *

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The entire physics

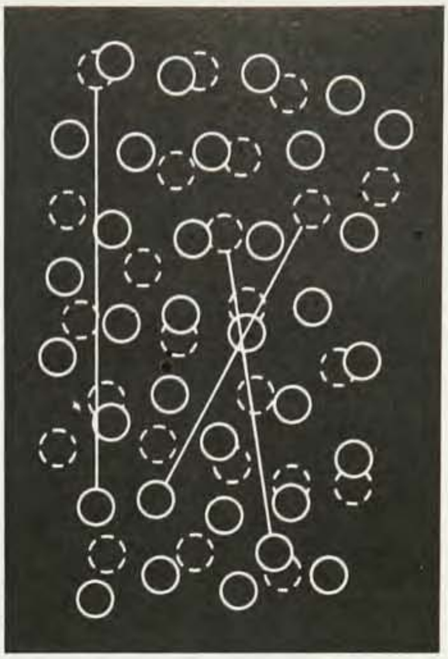
THE MANY-BODY PROBLEM IN QUANTUM MECHANICS. By N. H. March, W. H. Young, S. Sampanthar. 459 pp. Cambridge U. Press, London, 1967. $14.50

by John L. Gammel

The authors' aim is to provide an account of the methods used in the many-body problem and the physics that emerges within a single cover. It is difficult to believe that they will succeed absolutely because, after all, the many-body problem = λ (physics), where $0 \leqslant \lambda \leqslant 1$; but I suspect that $\lambda \approx 1$. It would be very difficult indeed to include all of physics in a single cover!

It does not take much looking to find one method not contained in the book. If one looks through the author index he does not find the names of Cyril Domb, Michael Fisher and Stanley Rushbrooke. Surely these people have contributed to the many-body problem. What is missing? Well, what is missing is a reference to Ising (Lenz cannot be found either). Nothing is said about the Ising model. Is nothing to be learned about the many-body problem from the Ising model? The answer is, Yes: There is much to be learned. In particular it is to be learned (as George Baker has recently) that the statement on page 207 is wrong. Namely, "we can conclude that the Goldstone approach, if rearranged in a series in powers of the density (or the number of interacting particles), appears to be rapidly convergent. Thus the Brueckner–Gammel theory can be considered reasonably well established. . ." The series is not only not rapidly convergent; it diverges. The source of the trouble is easy to locate. Hans Bethe and others have calculated the three-body cluster terms, and these appear small. But the four-body cluster terms have not been calculated, and just as the binding energy of He^4 is large compared with the binding energy of T or He^3, so the four-body cluster terms may be large compared with the three-body cluster terms. Baker tells me there is only one way to save the theory; namely, Brueckner was correct in the beginning; one must expand in powers of the K matrix for fixed density. One needs several terms in the expansion, and he must also do Padé approximants.

However, it is not really fair to judge the completeness or accuracy of the book in this way. Concerning completeness, the term "many-body problem" has come to stand for a certain body of knowledge that for some strange reason excludes considerations based on the Ising model, which is relegated to statistical physics. Concerning accuracy, Baker's conclusions are controversial (it is somewhat unfortunate that the appearance of these books, however valuable, on the many-body problem at a rate approaching one per month tends to obscure the fact that much



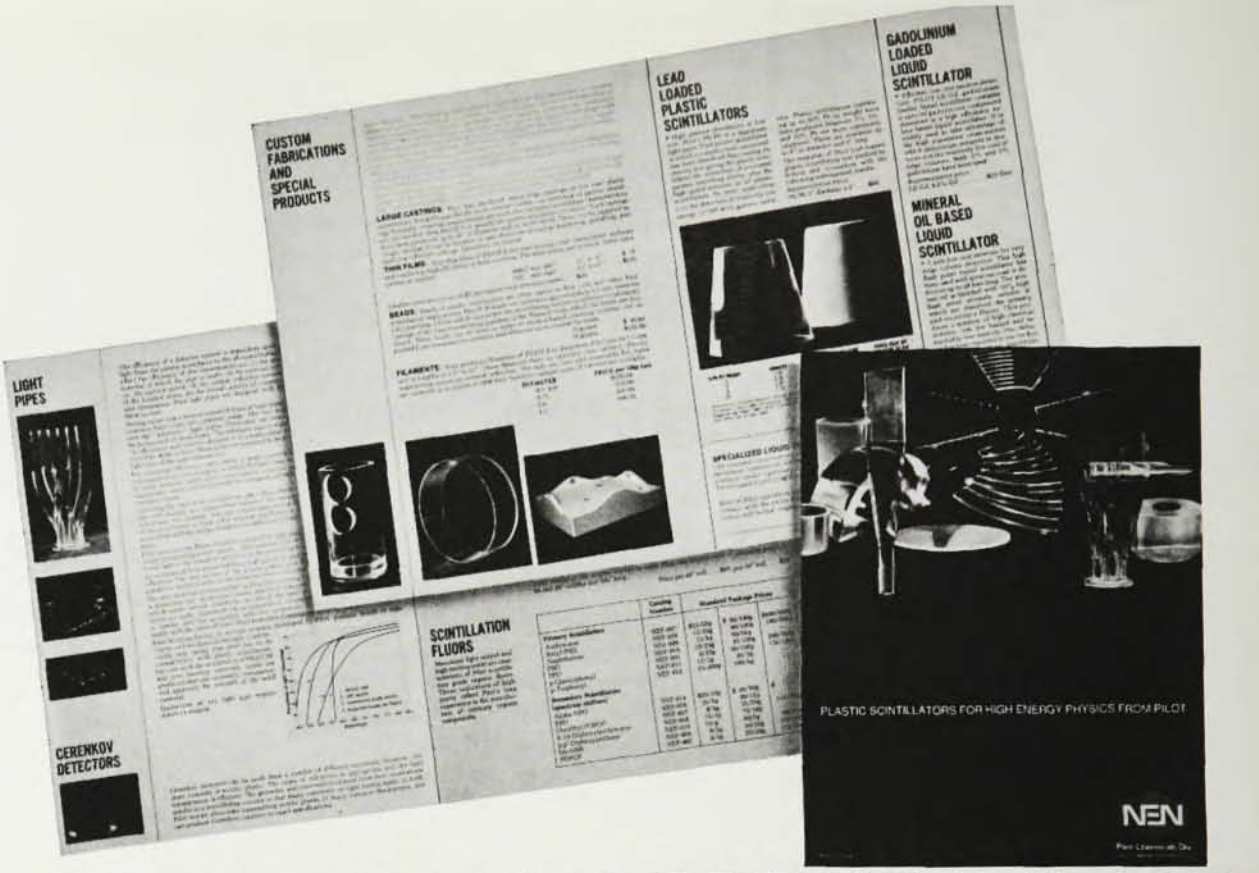
MANY-BOSON SYSTEM in Feynman theory. The possible sites for maxima and minima in the probability amplitude are shown with three typical correlations existing between them.

remains to be done about the many-body problem).

To form a fairer notion of the completeness, we may compare it to other similar books such as D. A. Kirzhnits's *Field Theoretical Methods in Many Body Systems* (Pergamon Press, Long Island City, New York, 1967). Green functions do not dominate the material in this book to the same extent that they do in Kirzhnits's book. The authors state that Green functions are "the most powerful many-body method yet devised," but devote only 67 of 410 pages to the method. The reviewer finds it a relief that the authors establish contact between the Green-functions method and the perturbation methods initiated by Brueckner.

In some respect the book is similar to Kirzhnits's: Everything is developed from elementary quantum mechanics. Although the Rayleigh–Schrödinger and Brillouin–Wigner perturbation methods are discussed, the main treatment is based on the time-dependent approach, and so inevitably the apparatus of quantum field theory appears. The analogy with quantum field theory is not pushed to quite the limit reached in Kirzhnits's book.

In distinct contrast to Kirzhnits (who omits these topics), the authors discuss superconductivity and many-



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boson systems. Thus this book is much more complete than Kirzhnits's. However, the application of these methods to atomic problems (with quite a few electrons but maybe not 238 and certainly not infinitely many) is better done in Kirzhnits's book.

In connection with finite systems, one has come again to the difficulty about $\lambda \approx 1$. Finite atomic systems are discussed in chapter 2. At the end of the chapter it is mentioned that the many-body methods developed in chapter 4 (many-body perturbation theory) hold out much promise for advances in atomic-physics calculations in the next decade. But this work is already begun, and nothing is done to guide the reader to the literature. A reference to H. P. Kelly, Phys. Rev. **136** B896 (1964), would have helped. Finite nuclei are not discussed at all. There are no references to Eden's and Emery's work (Sampanthar's colleagues at Cambridge), or Brueckner's, Rotenberg's, and Lockett's work, or Migdal's work (based on Green functions). I suppose that these things may be excluded quite properly on the grounds that they are not altogether definitive and belong anyhow to nuclear physics rather than the many-body problem. And yet references might have been provided.

Perhaps some idea of what the book does contain as opposed to what it does not contain may be conveyed by noting that the contents remind one of the contents of the 1958 Les Houches lectures (Wiley, New York, 1959). Since the 1958 Les Houches lectures may be accepted as a satisfactory historical basis for a definition of the many-body problem, there is, in fact, a basis for the authors' claims of completeness.

As a standard test of accuracy, the reviewer uses the discussion of the Hugenholtz–Van Hove theorem. This book passes the test very well: The authors reach the correct conclusion that Brueckner's and Gammel's $V(p)$ has little in common with the potential felt by a nucleon added to nuclear matter.

Many teachers will want to consider the adoption of this book as a text. It has problems at the end of the chapters.

I like this book very much, mainly, I think, because of the contact established between the perturbation methods and Green-function methods. The authors achieve their aim of including both methods in a single cover.

* * *

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Galileo on the Copernican system

GALILEO GALILEI: DIALOGUE CONCERNING THE TWO CHIEF WORLD SYSTEMS—PTOLEMAIC & COPERNICAN. (2nd edition) Trans. from Italian. 496 pp. U. of California Press, Berkeley, 1967. $12.50

by David C. Lindberg

Dialogue Concerning the Two Chief World Systems is Galileo's major defense of the Copernican system. Published in 1632, it was the culmination of Galileo's long and vigorous campaign to persuade both the scholarly community and the literate public of the truth of Copernican cosmology. Written in colloquial Italian and in popular dialogue form, it embodies all of Galileo's consummate wit, sarcasm and polemical skill. The personal consequences for Galileo of its publication require little elaboration. Although the book had been officially licensed by the Congregation of the Index, Galileo was brought to trial before the Inquisition, forced to recant, and sentenced to house arrest until his death in 1642.

Nevertheless, Galileo's book must not be regarded as a narrowly Copernican or astronomical tract. Galileo conceived his task very broadly: He must deal with the full spectrum of objections to the Copernican system—metaphysical, cosmological, astronomical and physical. Indeed, one might even argue that the main thesis is physical. There is a revealing phrase put into the mouth of one of the interlocutors, Simplicio: "The crucial thing is being able to move the earth without causing a thousand inconveniences." (page 122) If not his chief aim, it was at least one of Galileo's aims to demonstrate that from the standpoint of physics it is *not* inconvenient to move the earth; that is, it was incumbent on him to formulate new physical conceptions consistent with a moving earth. Thus we find Galileo analyzing, more or less comprehensively as the occasion allows, such physical problems as centrifugal tendency, projectiles, freely falling bodies, the motion of pendula and inertial motion. The book is therefore close to the origins of modern physics on a number of fronts, and the physicist who would learn something of the historical roots of his discipline could do worse than to read Galileo's *Dialogue*.

Nobody is better qualified to translate Galileo's works than Stillman Drake, who has also given us translations of Galileo's *Starry Messenger*, *Letters on Sunspots*, *Letter to the Grand Duchess*, *The Assayer* and *On Mechanics* . The result in this case—to waste no words—is superb. Drake's translation reflects scholarship without pedantry, a goal set by Galileo himself as revealed by a short passage quoted in the translator's preface. Drake has made Galileo speak in modern English without sacrificing the intent or flavor of the Italian original; nor is the translation ever marred by infelicities of phrasing.

About 25 pages of notes appear at the back of the book thus providing biographical and bibliographical information and clarifying doubtful points in the interpretation of Galileo's ideas. Here (as elsewhere) Drake has revealed his unrivaled command of the facts surrounding Galileo's life and work. And yet, if there is a flaw in the book, it is in these notes. Although enthusiastically calling attention to Galileo's every anticipation of a later conception (see, for example, the notes to pages 199, 213–217, 234), the notes provide no hint of Galileo's extensive debt to his scientific forebears. The erroneous impression is too readily communicated that Galileo's scientific conceptions were formulated *de novo*. For the reader who wishes to pursue the matter, may I suggest the following works, which will provide a particularly thorough view of Galileo's debt to ancient and medieval science: Marshall Clagett, *The Science of Mechanics in the Middle Ages* (Madison, Wisconsin, 1959); Ernest A. Moody, "Galileo and Avempace: The Dynamics of the Leaning Tower Experiment," *Journal of the History of Ideas*, volume 12 (1951), pages 163–193, 375–422; Alexandre Koyré, *Études Galiléennes* (Paris, 1939); also the works of Anneliese Maier, listed in Calgett's bibliography.

Finally, since this is the second edition of Drake's translation, I must say a word about the alterations that have been made to the first edition (1953). Although the preface asserts that