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The Mathematical Theory of Black Holes. S. Chandrasekhar. 646 pp. Clarendon, Oxford, England and Oxford University Press, New York, 1983. Price: $110.00 (cloth). (Reviewed by Kip S. Thorne.)

It is appropriate that I and my research group have owned two copies of this book for 18 months, and only now am I writing this review. Eighteen months is hardly enough time for even an expert in the subject to attempt taking the measure of this book. It is one of those exceedingly rare books that will have a useful lifetime of at least 50 years. It is a book filled with new approaches to old subjects and old approaches to new subjects; it completes the unfinished researches of other physicists; and, maddeningly (and perhaps for the first time in the author's career), it leaves unfinished the researches of the author. It is filled with nuggets of mathematical insight. It leads the serious reader, in the words of the author, into "a realm of the rococo: splendorous, joyful, and immensely ornate." And it leaves the casual reader cold, bored, repelled by page upon page of mathematical analysis with only a rare passage of motivational text.

Chandrasekhar is an inspiration to aging physicists like me. Now in his seventy-fifth year, he continues to produce absolutely first-rate research at a pace that has slowed only modestly. This book continues a pattern that he adopted early in his career: He enters a new topic of research, works on that topic with single-minded intensity for a number of years, then wraps up his research by writing a definitive treatise for the benefit of posterity. With treatise finished, he then turns his back on the old topic, enters a new one, and repeats the process. In this manner he has produced a monumental series of treatises: *An Introduction to the Study of Stellar Structure* (1939); *Principles of Stellar Dynamics* (1942); *Radiative Transfer* (1950); *Hydrodynamic and Hydromagnetic Stability* (1961); *Ellipsoidal Figures of Equilibrium* (1969); and finally, now, *The Mathematical Theory of Black Holes* (1983).

Chandrasekhar begins his book with a passage "To the Reader: ... Since the entire subject matter (including the mathematical developments) has been written (or, worked out) *ab initio*, independently of the origins, the author has not made any serious search of the literature. The bibliographical notes at the end of each chapter provide no more than the sources of his information. The book is an expression of the author's perspective with the limitations which that implies." He might have added, but modesty forbade, "and with the power which that implies."

To understand the limitations and the power of Chandrasekhar's perspective, one must know some of the history of research on the mathematical theory of black holes.

The decade 1965–1975 was the Golden Age of black-hole research. During that period the foundations of the subject were laid by several dozen physicists, almost all of them under the age of 35 and most of them graduate students under the age of 27—people like Stephen Hawking and Brandon Carter at the University of Cambridge, Saul Teukolsky at Caltech, and Aleksandr Starobinsky in Moscow. Near the end of this Golden Age, just when these young and brilliant researchers, thinking that the cream had been skimmed off the subject, were moving on to greener pastures in quantum gravity and gutsy astrophysics, Chandrasekhar entered the field.

Now, Chandrasekhar thoroughly dislikes incompleteness in mathematical physics. For example, in the 1960's he was horrified to discover how incomplete was the classic research of Dirichlet, Dedekind, Riemann, and others on the equilibrium structures of a rotating, gravitating, incompressible fluid. Driven by the aesthetic ugliness of this incompleteness, Chandrasekhar devoted several years of his life to cleaning up the nineteenth-century theory and bringing it into a complete and elegant form in his *Ellipsoidal Figures of Equilibrium* (1969)—a treatise that has gone on to impact considerably the theoretical astrophysics of the 1970's and 1980's.

In their exodus from black-hole research in 1973–1975, Chandrasekhar's young and brilliant colleagues left behind a body of highly incomplete theory. This annoyed Chandrasekhar even more than had his discovery of the incompleteness in Dirichlet, Dedekind, and Riemann.

Chandrasekhar's attitude toward the situation actually was rather complex, as one might infer from the following remarks that he made to me in 1983 in a slightly different context: "Thomas Huxley is supposed to have said, 'A man of science past sixty does more harm than good'; and in Lord Rayleigh's biography by his son, the son asked the father, who was 67 at the time, what he thought about Huxley's view. Rayleigh thought for awhile and said, 'I don't know why it should be so, provided you stick to what you know and do *not* enter into controversy with younger people.' That is what Rayleigh said. Put beside it what Thomson said about Rayleigh. Thomson had to give the memorial address for Lord Rayleigh at Westminster Abbey; and he classified scientists into two categories, those who get the first idea, and those who get the last idea. Well, Rayleigh belongs to the latter category. It's cruel to say these things; but perhaps I can say them because I essentially fall into this category myself, to the extent I can judge... ."

Chandrasekhar has had the first idea often enough (maximum mass of white dwarf stars, 1931; general relativistic instabilities in compact stars, 1964; gravitational-radiation-reaction-induced instabilities in stars, 1970) to win a Nobel Prize; but in *The Mathematical Theory of Black Holes*, he concentrates on the last idea and the last word: He cleans up and completes, in a thorough manner, the body of incomplete theory that his younger colleagues left behind. And he does it without entering into controversy with them—at least not on the surface. However, if one knows something of the literature and reads beneath the surface, one sees Chandrasekhar riding smoothshod over the works of his younger colleagues—smoothshod on an elegant steed with velvet-covered hooves.

As always, Chandrasekhar insists on presenting his subject in his own way, and in this case that way differs in major respects from the approaches of his younger colleagues. Ever intellectually honest, Chandrasekhar warns of this in his "To The Reader" remarks—but the reader and history may soon forget those remarks and assume that Chandrasekhar's way is canonical. For example, a large portion of the book is devoted to the problem of reconstructing, from a radiative component of the Riemann tensor (the "Teukolsky function"), the metric for a gravitationally perturbed black hole. This problem was first solved by Paul Chrzanowski, a graduate student of Charles Misner at Maryland, by a reconstruction method that required making an unproved anszatz. Chandrasekhar found the lack of rigor in Chrzanowski's reconstruction method aesthetically and mathematically disturbing; so he embarked on a long and arduous quest for a more fully rigorous method. And when he had finally succeeded, his method was so alien in spirit and appearance to that of Chrzanowski that he could not see any way to relate the two to each other. As a result, and despite the fact that Chrzanowski's method was subsequently made rigorous by other young physicists (Robert Wald, Jeffrey Cohen, and Lawrence Kegeles), there is no reference whatsoever to Chrzanowski's work in Chandrasekhar's book.

The impact of this was brought home to me recently when one of my own graduate students, wishing to reconstruct the metric for a particular black-hole perturbation problem, went to Chandrasekhar's book to find the method of solution, and then struggled for months trying to apply it to his own problem. I have had real difficulty getting him to look at Chrzanowski's method, which might be easier to apply, because Chandrasekhar's method has now been canonized. In another ten or twenty years Chandrasekhar may have had the truly last word: The method of Chrzanowski and many other insights tied up in the incomplete, non-Chandrasekhar versions of black-hole mathematics may be lost to researchers.

But in return for losing other viewpoints, we get from Chandrasekhar's book a monumental and almost complete body of mathematical theory, presented in a totally coherent and aesthetically pleasing way. We are struck by the splendor of the theory, by the intricacies of its interconnections, by the mysterious amenability of black holes to totally analytical analysis.

Chandrasekhar's legendary analytical genius shows up brilliantly throughout the book, but most especially in Chap. 9 on "The Gravitational Perturbations of the Kerr Black Hole." In concluding this chapter, he himself looks back in amazement: "The treatment of the perturbations of the Kerr space-time in this chapter has been prolixious in its complexity," he says. "Perhaps at a later time, the complexity will be unravelled by deeper insights. But mean time, the analysis has led us into a realm of the rococo: splendorous, joyful, and immensely ornate." Then, in the bibliographical notes to the chapter, he remarks: "But the nature of the developments simply does not allow a presentation that can be followed in detail with modest effort: the reductions that are necessary to go from one step to another are often very elaborate and, on occasion, may require as many as ten, twenty, or even fifty pages. In the event that some reader may wish to undertake a careful scrutiny of the entire development, the author's derivations (in some 600 legal-size pages and in six additional notebooks) have been deposited in the Joseph Regenstein Library of the University of Chicago."

In attempting to follow the mathematical development as presented in the book, I am struck time and again by Chandrasekhar's style—a style at which I have often marveled when discussing physics with him and watching him do research: his development is motivated by deep insight into the forms and symmetries of the differential equations with which he struggles. The equations speak to him in a tongue that I will never master. In places where I, not knowing how to proceed, would go back to the physical origins of the equations and seek guidance from physical intuition, Chandrasekhar flies nimbly forward, guided uncannily by the equations themselves.

Chandrasekhar takes a long-term view of physics. The things he values are those which will last for decades or longer. In commenting on his presentation of the Dirac equation in the curved space-time of a black hole (bibliographic notes to Chap. 10), he says that "The account of spinor analysis in the text is largely based on the author's notes of lectures given by Dirac in the spring of 1932 on spinor analysis and the relativistic theory of the electron. The style and content of the lectures do not seem to have faded in the intervening fifty years." It is clear that Chandrasekhar intends for his book to have a similarly long life. He assiduously avoids topics that will make the book seem dated fifty, twenty, or even ten years from now. There is absolutely no mention of observational searches for black holes, or of the roles that black holes might play in quasars, galactic nuclei, or compact x-ray sources. Black holes are presented as an elegant topic in mathematical physics, totally divorced from their ill-understood astronomical implications.

The Golden Age of black-hole research produced two major branches of the subject: Stephen Hawking and Roger Penrose in England (among others) used techniques of differential topology to prove elegant theorems about highly dynamical holes, while Teukolsky and Chandrasekhar in America, Carter in England, and Starobinsky in Russia (among others) used techniques of differential geometry and analysis to probe the details of equilibrium holes and small perturbations of them. A definitive treatise on the first branch (dynamical holes; differential topology) was written in 1973 by Hawking and George Ellis [*The Large Scale Structure of Space-Time* (Cambridge University, London)]; but the second branch (equilibrium holes; differential geometry and analysis) has had no comparable treatise until now. Chandrasekhar's book fills the void, beautifully.

The book is fully self-contained, including introductions (concise but complete) to its mathematical underpinnings: differential geometry, the Newman–Penrose formalism, and the mathematics of spinors in curved space-time. Although the notation is not to my liking, these introductions are so nicely, concisely, yet thoroughly done that they are a place of preference to which I now send advanced students to learn the Newman–Penrose formalism and curved-space spinors. On the other hand, the conciseness and lack of motivational prose make it a hopeless place for the novice to learn any aspect of mathematical relativity.

Having laid its mathematical foundations, the book goes on to treat in enormous and elegant depth the Schwarzschild metric which describes a nonrotating, equilibrium hole; geodesics and gravitational perturbations of a Schwarzschild hole; the Reissner–Nordstrom solution which describes an electrically charged, nonrotating, equilibrium hole; test-particle orbits and coupled gravitational and electromagnetic perturbations of a Reissner–Nordstrom hole; the Kerr metric which describes a rotating, equilibrium hole; geodesics, electromagnetic waves, spin-$\frac{1}{2}$ fields, and gravitational perturbations of a Kerr black hole; and the Kerr–Newman solution which describes an electrically charged, rotating, equilibrium hole. In each of these the treatment is Chandrasekhar's own—sometimes relying heavily on previous work of others, but frequently not. A large fraction of the formalism is due fully to Chandrasekhar.

Much to Chandrasekhar's annoyance, the book's mathematical formalism is not wholly complete: it is marred by two major gaps.

The first gap is the absence of any treatment of the coupled gravitational and electromagnetic perturbations of a (charged, rotating) Kerr–Newman black hole. Chandrasekhar struggled for many months trying to bring the mathematics of such perturbations into tractable form—to no avail. The anguish that this incompleteness has given him is hidden beneath the words of defeat on p. 562: "It does not appear that the methods developed in Chap. 9 for the treatment of the gravitational perturbations of the Kerr black-hole can be extended in any natural way to the Kerr–Newman black-hole."

Of greater astrophysical import is the other gap. In Chandrasekhar's own words (p. 567): "The equations of the Newman–Penrose formalism have proved singularly inept at addressing...the important physical problem of the stability of the Kerr space-time." When I asked Chandrasekhar, shortly after publication of his book, whether he believed that the Kerr space-time (i.e., rotating black holes) might, in fact, be unstable, he replied "No."—There is a proof of stability based on a combination of numerical solutions of the black-hole perturbation equations by Bill Press and Saul Teukolsky, and an elegant analytical analysis by James Hartle and Daniel Wilkins; and Chandrasekhar admits, privately, that he finds this proof rather convincing. However, because it relies so heavily on numerical work, he regards the proof as unsatisfying—so unsatisfying, in fact, that he does not even mention it in his book. Instead, he poses as a crucial unsolved problem the *analytic* proof of stability; and in the last section of his book he presents a variational-principal technique (due to himself, John Friedman, and Bernard Schutz) which analytically proves stability against axisymmetric perturbations and might one day succeed in proving complete stability. Chandrasekhar's anguish that it has not yet succeeded peeks through at us in his book's final sentence: "This might be the subject of a new story; but our present story has ended."

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The Ideas of Particle Physics: An Introduction for Scientists. J. E. Dodd. 202 pp. Cambridge University Press, New York, 1984. Price: $44.50 (cloth); $17.95 (paper). (Reviewed by Nilotpal Mitra.)

At the time of this writing, there is a brief lull in the dramatic progress of particle physics. Present-day accelerators are at their limits of resolution and the debut of the super colliders is several years away. A very plausible scheme for understanding elementary particle data is in place, and experiments during the coming decade can either confirm current expectations of grand unification and supersymmetry or reveal completely unexpected phenomena.

It seems appropriate that this should be the time to review the progress of the subject, the better to understand why, given the experimental facts, our present paradigms are as they are, and what one may expect in the future. Dodd's book admirably suits this task. It is a sweeping review of particle physics, starting from Planck's hesitant introduction of the quantum hypothesis in 1900 to the discovery of the *W* and *Z* particles in 1983. In between, he has described, in chronological order, the marriage of special relativity and quantum mechanics; the birth of nuclear physics; Dirac's proposal and the subsequent discovery of anti-particles; the sudden glut of "elementary" particles in the sixties as the construction of accelerators opened new energy frontiers; their classification as quark composites; and the current synthesis of all known elementary particle phenomena in a unified gauge-theoretical framework. This is not a history book, though. Only the winning hypotheses, so to speak, are described, and so the book tends to portray scientific development as a linear progression. Nor is it a textbook. It uses fairly simple equations, but there are no derivations. Under the circumstances, the author manages remarkably well to explain abstract concepts but the reader will appreciate it better if he has had some previous exposure to physics up to the sophomore level. I found one inaccuracy: In Sec. 6.3.1 on parity, the author writes that atomic transitions occur between states of the same parity. It is just the opposite, because of the parity of the photon. There is a list of books and review articles of varying depth of content for further reading. One unfortunate journalistic gimmick occurs in the epilogue where the author tabulates forthcoming experimental searches, their significance, and, as he puts it, their "Nobel rating" because "the next few years are about discoveries and Nobel prizes." Until the Nobel committee can see their way to awarding the prize to the large teams that are essential in high-energy experiments, crediting one experimenter for a discovery will often be very misleading.

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