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978-0-521-13099-8 - The Problem of the Earth's Shape from Newton to Clairaut: The Rise of Mathematical Science in Eighteenth-Century Paris and the Fall of "Normal" Science

John L. Greenberg

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*The problem of the earth's shape
From Newton to Clairaut*

This book investigates, through the problem of the earth's shape, part of the development of post-Newtonian mechanics by the Parisian scientific community during the first half of the eighteenth century. In the *Principia* Newton first raised the question of the earth's shape. John Greenberg shows how continental scholars outside France influenced efforts in Paris to solve the problem, and he also demonstrates that Parisian scholars, including Bouguer and Fontaine, did work that Alexis-Claude Clairaut used in developing his mature theory of the earth's shape. This evolution of Parisian mechanics proved not to be the replacement of a Cartesian paradigm by a Newtonian one, a replacement that might be expected from Thomas Kuhn's formulations about scientific revolutions, but a complex process instead involving many areas of research and contributions of different kinds from the entire scientific world. For example, Newtonian "normal" science does not take into account crucial developments in continental mathematics used to tackle the problem at issue.

Greenberg both explores the myriad of technical problems that underlie the historical development of part of post-Newtonian mechanics, which have been analyzed only rarely by Western scholars, and embeds his technical discussion in a framework that involves social and institutional history, politics, and biography. Instead of focusing exclusively on the historiographical problem, Greenberg shows as well that international scientific communication was as much a vital part of the scientific progress of individual nations during the first half of the eighteenth century as it is today.

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THE PROBLEM OF THE EARTH'S SHAPE FROM NEWTON TO CLAIRAUT

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Paris and the fall of "normal" science*

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To my wife Maité
without whose loving care, encouragement, and
help this book would never have been written

and

to my children Philippe and Sylvie

and

to the memory of my parents

Leonard S. Greenberg

(1913–1987)

and

Margaret O'Briant Greenberg

(1911–1992)

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Preface

The story recounted in the following pages is a chapter in the history of physical sciences in France. During the period in which a large part of this story takes place, the physical sciences in France have usually been represented in ways that accord with the following scheme: the publication of Isaac Newton's *Principia* in 1687 rounds off a seventeenth-century European scientific revolution. Now, the seventeenth-century scientific revolution scarcely began with Newton, but an irreconcilable conflict between the Newtonian world system and the preceding "paradigm," the Cartesian world system, means that Newton's *Principia* was certainly a revolutionary work. Although "crucial experiments" need not enter into the genesis of a scientific revolution (Copernicus, for example, simply reinterpreted old data; it was not until much later that any new observations began to play a role in determining the course of the "Copernican Revolution"), they did function decisively in giving Newton the victory over Descartes. "Normal science" was the "mopping up" of Newton's system by the French, beginning in the 1730s and 40s. The story ends with the death of Descartes's last advocate. Cartesianism becomes a dead issue. Any reader of Kuhn (1962 or 1970) should recognize the jargon I have used here and have no trouble imagining the scheme. Moreover, the plan may well strike Kuhn's reader as thoroughly plausible. Thus before beginning my story, I offer the following words of forethought. The story may be viewed as simply depicting an inevitable mopping up of a section of Newton's *Principia* – the articulation of a "Newtonian paradigm," and its extension to cover anomalies, by means of "puzzle solving" within the "paradigm," to use Kuhn's language. But it might also involve considerations that go beyond the confines of Newtonianism alone. If so, the problem of what makes up normal science, which Kuhn contrasts with "revolutionary" science, is raised. In that case, deciding which of two interpretations of the story that follows makes the most sense requires dealing with current issues in the history and philosophy of science.

I shall not sketch here the conclusions that I have drawn from the following story. If I did I could risk reducing the story's verve. I have tried to write a narrative that keeps the reader wondering.

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I will say, however, that problems of figures of equilibrium is a theme that reappears throughout the story. The story takes place in Paris during the first half of the eighteenth century. Today the investigation of problems of figures of equilibrium includes dynamical issues that were not dealt with during that time. The kinds of questions asked today involve solving functional equations and integral equations. Since the eighteenth century very powerful, highly ingenious methods have been developed to tackle these questions. Despite the invention of such mathematical tools, however, it is still not known to this day whether all figures of equilibrium of a rotating fluid mass that attracts in accordance with the universal inverse-square law of gravitation have been found. This gives an idea of just how difficult problems of equilibrium have become since the first half of the eighteenth century. The little concerning the technical aspects of problems of figures of equilibrium which appear in this story, including the advances that were made during the period that the story covers, could probably be summarized in a page or two. Even so, these few achievements were arrived at only with great effort, because the problems that were solved were still very difficult for the time. One of our aims is to understand how the problems were solved at all and the larger implications that such a success has.

Consequently, treating the questions that underlie the historiographic issue raised above necessitates that technical matters be examined in some depth and detail. However, it would be a mistake to think that the story that follows consists of nothing but the study of technical concerns. The technical discussion is embedded in a framework that involves social history, institutional history, politics, and biography that I interpret in ways that, to the best of my knowledge, cannot be found elsewhere. Remarks concerning social history, institutional history, politics, and biography are dispersed throughout the narrative, but there are also entire sections of the story that should in principle appeal to the historian who is not "internalist." The story's meaning goes beyond what could possibly emerge from a study confined solely to the technical side of the particular problems that make up the recurrent theme that I have already mentioned. If I manage to hold the reader's interest and attention, then I may succeed in transporting him to Paris as it existed during the first half of the eighteenth century.

In addition to answering this historiographic question, which involves current issues in the history and philosophy of science, the story has a moral that concerns the growth of science in a sense that differs from the one that is at issue in the historiographic problem. We shall find that international scientific communication was as much a vital part of the scientific progress of individual nations during the first half of the eighteenth century as it is today.

Research for this book was made possible by a University of Wisconsin travel grant in 1977, a National Science Foundation national needs postdoctoral fellowship in 1979–80, a Fulbright research scholarship in 1984–5, a National

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Science Foundation scholar's award in 1984–5, the U.S.–France Exchange of Scientists in 1984–6, a visiting research associateship ("poste rouge") with the Centre National de la Recherche Scientifique in 1986, an American Council of Learned Societies fellowship in 1987–8, a National Science Foundation scholar's award in 1990–1, and the French Société de Secours des Amis des Sciences. I am grateful to the officials in charge of the Archives de l'Académie Royale des Sciences (Paris), Bibliothèque de l'Institut (Paris), Observatoire de Paris, Bibliothèque Nationale (Paris), Archives Nationales (Paris), Archives de l'École Polytechnique (Palaiseau), Library of the Royal Society (London), and the Bernoulli Archives at the Universitätsbibliothek, Basel, for allowing me to examine their collections.

I thank Dan Siegel for accepting in 1979 an early version of this work as a Ph.D. "thesis." I profited greatly from René Taton's seminars at the Centre Alexandre Koyré in Paris in January and February of 1977, in 1979–81, and in 1984–5. I owe special thanks to Monsieur Taton who sponsored my National Science Foundation national needs postdoctoral fellowship in 1979–80 and my Fulbright research scholarship in 1984–5 and to Maurice Caveing who made me visiting research associate with the Centre National de la Recherche Scientifique in 1986.

I thank Bob Weinstock for helping me understand extremely difficult parts of Isaac Newton's *Principia* and for having saved me from making some serious errors pertaining to nomenclature. I also thank Clifford Truesdell for patiently answering my idiotic questions about elementary fluid mechanics. I thank John Pappas whose knowledge of the eighteenth-century French Enlightenment is vast and who taught me much about that Enlightenment. He helped me to portray the French men of science who appear in this book not simply as scientists but as *French* scientists. I thank Jim Casey for having saved me from making some great blunders concerning the application of the principle of solidification.

I also thank Dan Siegel, George Downs, and Bob Weinstock for providing the subsidy needed to publish this book.

I call attention to the fact that I only discovered David Beeson's *Maupertuis: An Intellectual Biography* while my own manuscript was being copy-edited and that I only discovered Rob Iliffe's article of 1993 on Maupertuis, Jesper Lützen's article of 1994 on Liouville, and John Heilbron's *Weighing the Imponderables and Other Quantitative Science around 1800* while the page proofs were undergoing final correction.

This book was written in very difficult circumstances. Since October of 1983 I have been afflicted with spastic paraparesis, an insidiously progressive form of multiple sclerosis. I have been ill the whole time that I did much of the research and all of the writing of this book. I apologize for not having included a subject index. By the time that I received proofs I was too physically handicapped to handle the preparation of a subject index for such an intricate work. I am

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I thank Helen Wheeler for cheerfully assisting in the production of this book. She had to put up with *a lot*.

I thank my son Philippe and daughter Sylvie whom I often neglected while working on the contents of this book, which means ever since they were born. Finally I thank Maité, my wife and guardian angel for more than twenty-two years. She was a constant source of moral support in my struggle to write this book and is a constant source of moral support in my battle against my illness.