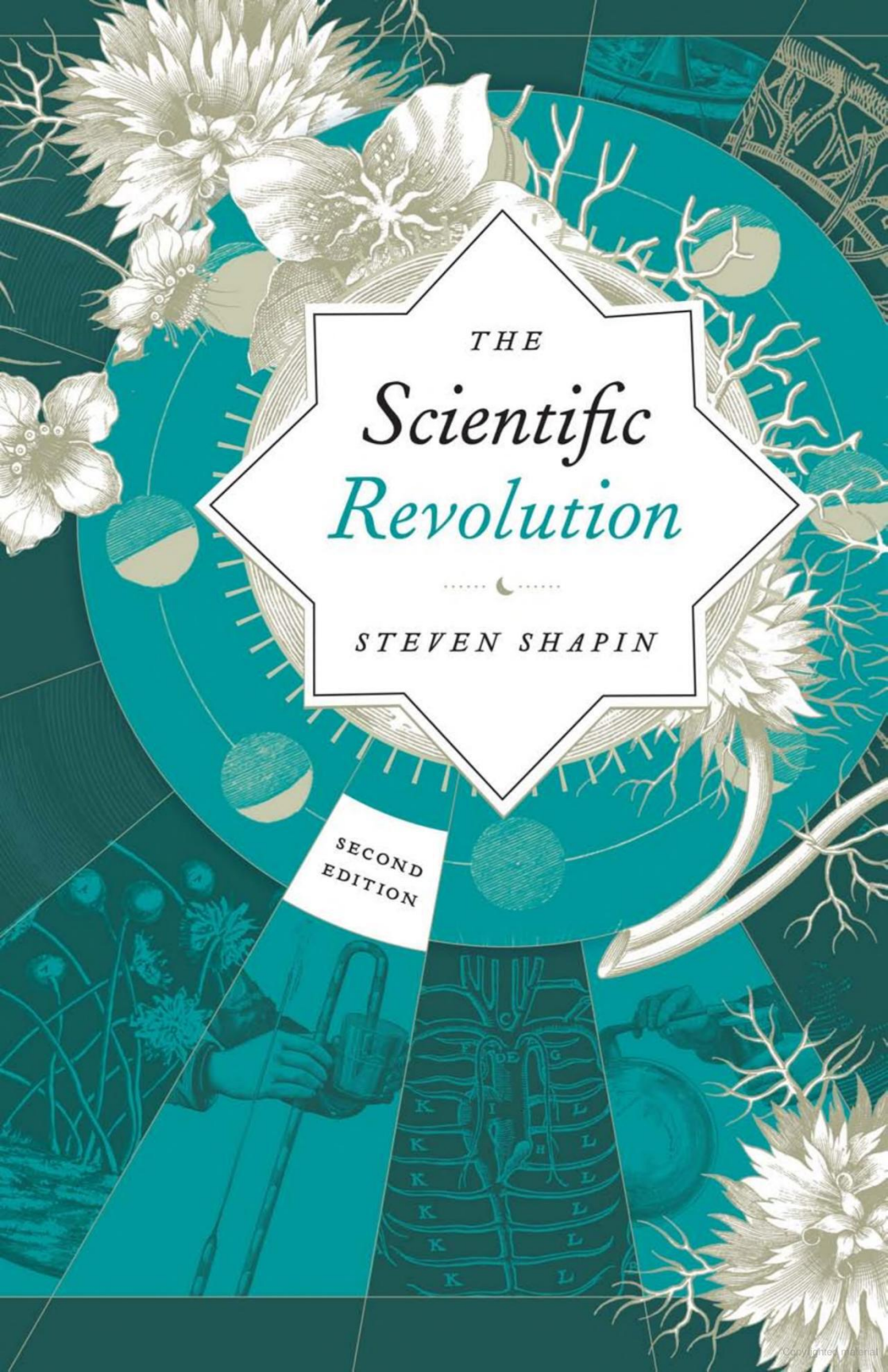
THE
Scientific
Revolution
STEVEN SHAPIN
SECOND
EDITION

The

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STEVEN SHAPIN

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Contents

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Acknowledgments

This is a work of critical synthesis, not of original scholarship. Although this book's aim is to give an up-to-date interpretation of the Scientific Revolution, taking account of much historical research produced over recent decades, it nevertheless draws on the efforts of generations of scholars. Accordingly, my greatest debts are to the many other historians whose work I use so freely and whose books and papers are listed in the accompanying bibliographic essay. There should be no doubt about the legitimate sense in which this is as much their book as mine, yet the interpretations I put on their work and the way in which I organize their disparate findings and claims reflect my own point of view.

To enable this book to most effectively address a general readership and to make the exposition flow as smoothly as possible, I chose not to burden the text with dense citations of relevant secondary literature. Moreover, direct quotations from modern historians were reserved for rare occasions when I judged that their particular ways of putting things were uniquely informative or revealing or when their precise formulations had attained something like "proprietary" status.

I was fortunate in having a few academic colleagues and friends who told me that such a book might be useful, encouraged me through some troublesome passages in its career, and read earlier versions,

making valuable suggestions about many aspects of its content, organization, and presentation. For these, it is a pleasure to acknowledge the special contributions of Peter Dear and Simon Schaffer. No one familiar with their work could possibly associate them with this book's remaining faults. For the first edition in 1996, two anonymous readers for the University of Chicago Press wrote constructive and detailed reports far beyond the usual call of duty. For assistance in locating several of the illustrations, I thank Paula Findlen, Karl Hufbauer, Christine Ruggere, Simon Schaffer, and Deborah Warner. I thank Alice Bennett, then a senior manuscript editor at the University of Chicago Press, whose diligent and dedicated copyediting did much to make the text more clear. Susan Abrams, my original editor, gave me the warm support and advice for which she was so highly respected, and I thank Christie Henry, then at the University of Chicago Press, for encouragement to produce this second edition.

The text for this edition has not changed. However, the extensive bibliographic essay has been almost entirely rewritten to reflect scholarship on early modern science that has appeared over the past twenty years or so. For suggestions about the shape and content of that revised essay, I thank Adrian Johns, Jan Golinski, and Peter Dear.

Introduction

The Scientific Revolution: The History of a Term

There was no such thing as the Scientific Revolution, and this is a book about it. Some time ago, when the academic world offered more certainty and more comforts, historians announced the real existence of a coherent, cataclysmic, and climactic event that fundamentally and irrevocably changed what people knew about the natural world and how they secured proper knowledge of that world. It was the moment at which the world was made modern, it was a Good Thing, and it happened sometime during the period from the late sixteenth to the early eighteenth century. In 1943 the French historian Alexandre Koyré celebrated the conceptual changes at the heart of the Scientific Revolution as "the most profound revolution achieved or suffered by the human mind" since Greek antiquity. It was a revolution so profound that human culture "for centuries did not grasp its bearing or meaning; which, even now, is often misvalued and misunderstood." A few years later the English historian Herbert Butterfield famously judged that the Scientific Revolution "outshines everything since the rise of Christianity and reduces the Renaissance and Reformation to the rank of mere episodes. . . . [It is] the real origin both of the modern world and of the

modern mentality." It was, moreover, construed as a conceptual revolution, a fundamental reordering of our ways of *thinking* about the natural. In this respect, a story about the Scientific Revolution might be adequately told through an account of radical changes in the fundamental categories of thought. To Butterfield, the mental changes making up the Scientific Revolution were equivalent to "putting on a new pair of spectacles." And to A. Rupert Hall it was nothing less than "an *a priori* redefinition of the objects of philosophical and scientific inquiry."

This conception of the Scientific Revolution is now encrusted with tradition. Few historical episodes present themselves as more substantial or more self-evidently worthy of study. There is an established place for accounts of the Scientific Revolution in the Western liberal curriculum, and this book is an attempt to fill that space economically and to invite further curiosity about the making of early modern science.[1] Nevertheless, like many twentieth-century "traditions," that contained in the notion of the Scientific Revolution is not nearly as old as we might think. The phrase "the Scientific Revolution" was not in common use before Alexandre Koyré gave it wider currency in 1939. And it was not until 1954 that two books—written from opposite ends of the historiographic spectrum—used it as a main title: A. Rupert Hall's Koyré-influenced *The Scientific Revolution*[2] and a volume of J. D. Bernal's Marxist *Science in History* called *The Scientific and Industrial Revolutions.* Although many seventeenth-century practitioners expressed their intention to bring about radical intellectual change, they used no such term to refer to what they were doing.

1. "Early modern," in historians' usage, generally refers to the period in European history from roughly 1550 to 1800. I shall be using the term in a slightly more restrictive sense, to denote the period ending about 1700–1730. Later I will use the terms "modern" and "modernist" to designate some specific reforms of knowledge and practice set on foot in the seventeenth century.

2. In the 1930s the French philosopher Gaston Bachelard referred to "mutations" (or large-scale discontinuities) in the development of the conceptual structure of science, a usage Koyré soon developed: "The scientific revolution of the seventeenth century was without doubt such a mutation. . . .It was a profound intellectual transformation of which modern physics . . . was both the expression and the fruit."

From antiquity through the early modern period, a "revolution" invoked the idea of a periodically recurring cycle. In Copernicus's new astronomy of the mid-sixteenth century, for example, the planets completed their revolutions round the sun, while references to political revolutions gestured at the notion of ebbs and flows or cycles—fortune's wheel—in human affairs. The idea of revolution as a radical and irreversible reordering developed together with linear, unidirectional conceptions of time. In this newer conception revolution was not recurrence but its reverse, the bringing about of a new state of affairs that the world had never witnessed before and might never witness again. Not only this notion of revolution but also the beginnings of an idea of revolution in science date from the eighteenth-century writings of French Enlightenment philosophes who liked to portray themselves, and their disciplines, as radical subverters of ancien régime culture. (Some of the seventeenth-century writers this book is concerned with saw themselves not as bringing about totally new states of affairs but as restoring or purifying old ones.) The notion of a revolution as epochal and irreversible change, it is possible, was first applied in a systematic way to events in science and only later to political events. In just this sense, the first revolutions may have been scientific, and the "American," "French," and "Russian Revolutions" are its progeny.

As our understanding of science in the seventeenth century has changed in recent years, so historians have become increasingly uneasy with the very idea of "the Scientific Revolution." Even the legitimacy of each word making up that phrase has been individually contested. Many historians are now no longer satisfied that there was any singular and discrete event, localized in time and space, that can be pointed to as "the" Scientific Revolution. Such historians now reject even the notion that there was any single coherent cultural entity called "science" in the seventeenth century to undergo revolutionary change. There was, rather, a diverse array of cultural practices aimed at understanding, explaining, and controlling the natural world, each with different characteristics and each experiencing different modes of change. We are now much more dubious of claims that there is

anything like "a scientific method"—a coherent, universal, and efficacious set of procedures for making scientific knowledge—and still more skeptical of stories that locate its origin in the seventeenth century, from which time it has been unproblematically passed on to us. And many historians do not now accept that the changes wrought on scientific beliefs and practices during the seventeenth century were as "revolutionary" as has been widely portrayed. The continuity of seventeenth-century natural philosophy with its medieval past is now routinely asserted, while talk of "delayed" eighteenth- and nineteenth-century revolutions in chemistry and biology followed hard upon historians' identification of "the" original Scientific Revolution.

Why Write about the Scientific Revolution?

There are still other reasons for historians' present uneasiness with the category of the Scientific Revolution as it has been customarily construed. First, historians have in recent years become dissatisfied with the traditional manner of treating ideas as if they floated freely in conceptual space. Although previous accounts framed the Scientific Revolution in terms of autonomous ideas or disembodied mentalities, more recent versions have insisted on the importance of situating ideas in their wider cultural and social context. We now hear more than we used to about the relations between the scientific changes of the seventeenth century and changes in religious, political, and economic patterns. More fundamentally, some historians now wish to understand the concrete human *practices* by which ideas or concepts are made. What did people *do* when they made or confirmed an observation, proved a theorem, performed an experiment? An account of the Scientific Revolution as a history of free-floating concepts is a very different animal from a history of concept-making practices. Finally, historians have become much more interested in the "who" of the Scientific Revolution. What kinds of people wrought such changes? Did everyone believe as they did, or only a

very few? And if only a very few took part in these changes, in what sense, if at all, can we speak of the Scientific Revolution as effecting massive changes in how "we" view the world, as the moment when modernity was made, for "us"? The cogency of such questions makes for problems in writing as unreflectively as we used to about the Scientific Revolution. Responding to them means that we need an account of changes in early modern science appropriate for our less confident, but perhaps more intellectually curious, times.

Yet despite these legitimate doubts and uncertainties there remains a sense in which it is possible to write about the Scientific Revolution unapologetically and in good faith. There are two major considerations to bear in mind here. The first is that many key figures in the late sixteenth and seventeenth centuries vigorously expressed *their* view that they were proposing some very new and very important changes in knowledge of natural reality and in the practices by which legitimate knowledge was to be secured, assessed, and communicated. They identified *themselves* as "moderns" set against "ancient" modes of thought and practice. Our sense of radical change afoot comes substantially from them (and those who were the object of their attacks), and is not simply the creation of mid-twentieth-century historians. So we can say that the seventeenth century witnessed some self-conscious and large-scale attempts to change belief, and ways of securing belief, about the natural world. And a book about the Scientific Revolution can legitimately tell a story about those attempts, whether or not they succeeded, whether or not they were contested in the local culture, whether or not they were wholly coherent.

But why do we tell *these* stories instead of others? If different sorts of seventeenth-century people believed different things about the world, how do we assemble our cast of characters and associated beliefs? Some "natural philosophers," for example, advocated rational theorizing, while others pushed a program of relatively atheoretical fact collecting and experimentation.[3] Mathematical physics was,

3. In the seventeenth century the word "science" (from the Latin *scientia,* meaning knowledge or wisdom) tended to designate any body of properly constituted

for example, a very different sort of practice from botany. There were importantly different versions of what it was to do astronomy and believe as an astronomer believed; the relations between the "proper sciences" of astronomy and chemistry and the "pseudosciences" of astrology and alchemy were intensely problematic; and even the category of "nature" as the object of inquiry was understood in radically different ways by different sorts of practitioners. This point cannot be stressed too strongly. The cultural practices subsumed in the category of the Scientific Revolution—however it has been construed—are not coextensive with early modern, or seventeenth-century, science. Historians differ about which practices were "central" to the Scientific Revolution, and participants themselves argued about which practices produced genuine knowledge and which had been fundamentally reformed.

More fundamentally for criteria of selection, it ought to be understood that "most people"—even most educated people—in the seventeenth century did not believe what expert scientific practitioners believed, and the sense in which "people's" thought about the world was revolutionized at that time is very limited. There should be no doubt whatever that one could write a convincing history of seventeenth-century thought about nature without even *mentioning* the Scientific Revolution as traditionally construed.

The very idea of the Scientific Revolution, therefore, is at least partly an expression of "our" interest in our ancestors, where "we" are late twentieth-century scientists and those for whom what they believe counts as truth about the natural world. And this interest provides the second legitimate justification for writing about the Scien-

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knowledge (that is, knowledge of necessary universal truths), while inquiries into what sorts of things existed in nature and into the causal structure of the natural world were referred to, respectively, as "natural history" and "natural philosophy." In the main, this book will follow early modern usage, including the designation of relevant practitioners as natural philosophers, natural historians, mathematicians, astronomers, chemists, and so forth. The term "scientist" was invented only in the nineteenth century and was not in routine use until the early twentieth.

tific Revolution. Historians of science have now grown used to condemning "present-oriented" history, rightly saying that it often distorts our understanding of what the past was like in its own terms. Yet there is absolutely no reason we should not want to know how we got from there to here, who the ancestors were, and what the lineage is that connects us to the past. In this sense a story about the seventeenth-century Scientific Revolution can be an account of those changes that we think led on—never directly or simply, to be sure—to certain features of the present in which, for certain purposes, we happen to be interested. To do this would be an expression of just the same sort of legitimate historical interest displayed by Darwinian evolutionists telling stories about those branches of the tree of life that led to human beings—without assuming in any way that such stories are adequate accounts of what life was like hundreds of thousands of years ago. There is nothing at all wrong about telling such stories, though one must always be careful not to claim too much scope for them. Stories about the ancestors as ancestors are not likely to be sensitive accounts of how it was in the past: the lives and thoughts of Galileo, Descartes, or Boyle were hardly typical of seventeenth-century Italians, Frenchmen, or Englishmen, and telling stories about them geared solely to their ancestral role in formulating the currently accepted law of free fall, the optics of the rainbow, or the ideal gas law is not likely to capture very much about the meaning and significance of their own careers and projects in the seventeenth century.

The past is not transformed into the "modern world" at any single moment: we should never be surprised to find that seventeenth-century scientific practitioners often had about them as much of the ancient as the modern; their notions had to be successively transformed and redefined by generations of thinkers to become "ours." And finally, the people, the thoughts, and the practices we tell stories about as "ancestors," or as the beginnings of our lineage, always reflect some present-day interest. That we tell stories about Galileo, Boyle, Descartes, and Newton reflects something about our late

twentieth-century scientific beliefs and what we value about those beliefs. For different purposes we could trace aspects of the modern world back to philosophers "vanquished" by Galileo, Boyle, Descartes, and Newton, and to views of nature and knowledge very different from those elaborated by our officially sanctioned scientific ancestors. For still other purposes we could make much of the fact that most seventeenth-century people had never heard of our scientific ancestors and probably entertained beliefs about the natural world very different from those of our chosen forebears. Indeed, the overwhelming majority of seventeenth-century people did not live in Europe, did not know that they lived in "the seventeenth century," and were not aware that a Scientific Revolution was happening. The half of the European population that was female was in a position to participate in scientific culture scarcely at all, as was that overwhelming majority—of men and women—who were illiterate or otherwise disqualified from entering the venues of formal learning.

Some Historiographical Issues

I mean this book to be historiographically up to date—drawing on some of the most recent historical, sociological, and philosophical engagements with the Scientific Revolution. On the other hand, I do not mean to trouble readers with repeated references to methodological and conceptual debates among academics. This book is not written for professional specialized scholars, and readers who develop an interest in the academic state of play will find guidance in the accompanying bibliographic essay. There is no reason to deny that this story about the Scientific Revolution represents a particular point of view, and that, although I help myself freely to the work of many distinguished scholars, its point of view is my own. Other specialists will doubtless disagree with my approach—some vehemently—and a large number of existing accounts do offer a quite different perspective on what is worth telling about the Scientific

Revolution. The positions represented here on some recent historiographic issues can be briefly summarized:

1. I *take for granted* that science is a historically situated and social activity and that it is to be understood in relation to the *contexts* in which it occurs. Historians have long argued whether science relates to its historical and social contexts or whether it should be treated in isolation. I shall simply write about seventeenth-century science as if it were a collectively practiced, historically embedded phenomenon, inviting readers to see whether the account is plausible, coherent, and interesting.

2. For a long time, historians' debates over the propriety of a sociological and a historically "contextual" approach to science seemed to divide practitioners between those who drew attention to what were called "intellectual factors"—ideas, concepts, methods, evidence—and those who stressed "social factors"—forms of organization, political and economic influences on science, and social uses or consequences of science. That now seems to many historians, as it does to me, a rather silly demarcation, and I shall not waste readers' time here in reviewing why those disputes figured so largely in past approaches to the history of early modern science. If science is to be understood as historically situated and in its collective aspect (i.e., sociologically), then that understanding should encompass all aspects of science, its ideas and practices no less than its institutional forms and social uses. Anyone who wants to represent science sociologically cannot simply set aside the body of what the relevant practitioners *knew* and how they went about obtaining that knowledge. Rather, the task for the sociologically minded historian is to display knowledge making and knowledge holding *as social processes.*

3. A traditional construal of "social factors" (or what is sociological about science) has focused on considerations taken to be "external" to science proper—for example, the use of metaphors from the economy in the development of scientific knowledge or the ideological uses of science in justifying certain sorts of political arrangements. Much fine historical work has been done based on such a construal. However, the identification of what is sociological about science with

what is external to science appears to me a curious and a limited way of going on. There is as much society inside the scientist's laboratory, and internal to the development of scientific knowledge, as there is outside. And in fact the very distinction between the social and the political, on the one hand, and "scientific truth," on the other, is partly a cultural product of the period this book discusses. What is commonsensically thought of as science in the late twentieth century is in some measure a product of the historical episodes we want to understand here. Far from matter-of-factly treating the distinction between the social and the scientific as a resource in telling a historical story, I mean to make it into a topic of inquiry. How and why did we come to think that such a distinction is a matter *of course*?

4. I do not consider that there is anything like an "essence" of seventeenth-century science or indeed of seventeenth-century reforms in science. Consequently there is no single coherent story that could possibly capture all the aspects of science or its changes in which we late twentieth-century moderns might happen to be interested. I can think of no feature of early modern science that has been traditionally identified as its revolutionary essence that did not have significantly variant contemporary forms or that was not subjected to contemporary criticism by practitioners who have also been accounted revolutionary "moderns." Since in my view there is no essence of the Scientific Revolution, a multiplicity of stories can legitimately be told, each aiming to draw attention to some real feature of that past culture. This means that selection is a necessary feature of *any* historical story, and there can be no such thing as definitive or exhaustive history, however much space the historian takes to write about any passage of the past. What we select inevitably represents our interests, even if we aim all the while to "tell it like it really was." That is to say, there is inevitably something of "us" in the stories we tell about the past. This is the historian's predicament, and it is foolish to think there is some method, however well intentioned, that can extricate us from this predicament.

The interpretations of professional historians respect the vast body of factual knowledge we now have about the past. Such respect

rightly counts as a measure of intellectual honesty, and all historians wishing to be honest will feel the desire to make endless qualifications to *any* generalization about past science. It is a pull I feel as strongly as any other historian: in the pages that follow there are many summaries I wish I had space to make more nuanced and more qualified. Yet succumbing to that pull has its costs. Stories of endless complexity, endlessly qualified, hedged about with modifications and surrounded by a moat of literature citations, are unlikely to be read by any but specialists. And though such accounts can further our stock of factual knowledge about the past, they are less likely to be coherent enough to advance our overall understanding. Part of my brief, to be sure, is to draw attention to the cultural heterogeneity of seventeenth-century science, but I have elected to do so by following a relatively small number of issues and themes through the period of interest.

I am content to accept that this account of the Scientific Revolution is selective and partial. There is a moderate bias toward the empirical and experimental sciences and toward English materials. This is partly due to my own historical interests and partly the consequence of my judgment that many previous historical surveys have been excessively skewed toward mathematical physics and Continental settings.[4] This concentration was justified by the view that what was "really new" and "really important" in the seventeenth century was the mathematization of the study of motion and the destruction of the Aristotelian cosmos—hence a tight focus upon such figures as Galileo, Descartes, Huygens, and Newton. The pride of place accorded in some traditional stories to mathematical physics and astronomy has tended to give an impression that these practices solely constituted the Scientific Revolution, or even that an account of them counts as what deserves telling about important novelty in early modern science. In weakened form, there is much about these as-

4. In many cases I use English materials not to imply or assert the centrality of developments particular to England but as a way of locally illustrating tendencies that were, in general form, widely distributed in Europe.

sumptions that is worth retaining, but this book will intermittently draw attention to the significance of reformed practices of making observations and constituting *experience* in a wider range of sciences. Indeed, some recent historical work has claimed that the seventeenth century, and especially the English setting, witnessed remarkable innovations in the modes of identifying, securing, validating, organizing, and communicating experience, and I want this survey to reflect the significance of those claims. Nor, despite the fact that this book devotes much attention to what have been called the "mechanical," the "experimental," and the "corpuscular" philosophies, do I simply equate these practices with the Scientific Revolution. Not all seventeenth-century natural philosophy was mechanical or experimental, and among those versions that did embrace mechanism and experimentation, their proper scope and role were disputed. Nevertheless, I think that attempts to "mechanize" not only nature but the means of knowing about nature, as well as *conflicts* over the propriety of mechanical and experimental modes, do capture quite a lot that is worth understanding about cultural change in this period.

If there is any originality about the conception of this book, it possibly flows from its basic organization. The three chapters deal sequentially with what was known about the natural world, how that knowledge was secured, and what purposes the knowledge served. What, how, and why. Some existing surveys have focused almost exclusively on what, while accounts of how have tended to suffer from idealization and why has scarcely been addressed at all, and then in relative isolation from the what and the how.

I want to engage with and to summarize a more-or-less canonical account of changes in belief widely said to be characteristic of the Scientific Revolution, while giving some indication that relevant beliefs varied and were even strongly contested. I start by picking up a number of strands in changing patterns of belief about nature that have routinely been treated by previous historians. I have claimed that there is no essence of the Scientific Revolution, yet pragmatic criteria push me at times toward an artificially coherent account of distinctive changes in natural knowledge. (When that artificial co-

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