the most comprehensive account yet available of the conditions and practice of French science on the eve of the Revolution. Written in stately style and rich in archival and documentary evidence, it will be a source of pleasure and profit to students of French science for many years to come. For all its authority, it nevertheless remains a very personal book. It is not inappropriate, then, to ask why Gillispie wrote it. He offers several answers to such a question. The first is that "I have thought to explain the vitality of all French science at the end of the Old Regime through exhibiting the extent of its involvement in affairs" (p. ix). There can be little doubt that Gillispie demonstrates both the vitality of French science and the extent of its involvement in affairs at the end of the Old Regime, but it is not clear that this in itself constitutes an explanation of the former in terms of the latter. Gillispie himself seems to suggest that it was not the extent but the nature of this involvement in affairs—the nature of the links between science and the state—that was most crucial in this respect. In the course of the 18th century, the scientific initiative shifted from the Royal Society in London to the Académie des Sciences in Paris, from a liberal society of amateurs to a government-supported central academy of specialized scientists, a model imitated by absolutist regimes elsewhere in Europe.

> It is a liberal's vanity to imagine that freedom, for him the better cause, has been an historical concomitant of science, either as its condition or consequence. Neither a Bacon nor a Descartes ever prophesied that it would be, and the technical accomplishments of the Soviet Union are immense evidence that it need not be. Scientists, like most men, may enjoy freedom but seldom require it in the way that writers do. Instead they need standards. . . . They need support. . . . They need motivation. . . . All this was afforded by the prospect for election to the Academy in Paris [pp. 80–81].

This conclusion brings us to Gillispie's second aim in writing this book, namely to show that it was during this period in France that the relationship between science and government "began to assume a form characteristic of the modern state and of modern science" (p. ix). The form of that relationship, Gillispie concludes, was (and is) one of "partnership" rather than "partisanship." From science, governments have wanted expertise and instrumentalities, "powers but not power"; from governments, scientists have wanted "support, in the obvious form of funds, but also in the shape of institutionalization and in the provision of authority for the legitimation of their community in its existence and in its activities" (p. 549). Accordingly in 18th-century France "science was not the source of a reform movement or liberalism. Its role was to provide the monarchy with the services and knowledge of experts and in return to draw advantages from the state for the furthering of science" (p. 550). For students of 18th-century France, this will be one of the more controversial claims of Gillispie's book since it suggests a radical distinction between the norms and practice of science itself and the reforming spirit of an Enlightenment that looked to science for its model of rational knowledge and public action. Is this distinction justified? It seems to be problematic in the persons of *philosophes* such as d'Alembert and Condorcet, who were both practicing scientists and proponents of political and social reform (this may be one reason why Gillispie finds Condorcet so troublesome a figure); and it neglects the fact that in 18th-century France (as perhaps in all societies) the line of demarcation between means and policies was by no means clear. Lavoisier discovered as much when his scientific consideration of agricultural productivity brought him to the conclusion that the entire tax structure of the Old Regime needed reformation. It would be interesting in this respect to look more systematically at the lines of tension between science and government in 18th-century France, to identify those areas in which "polity" gave way to "politics."

Gillispie's final purpose in presenting this book—less an initial aim of the project than a reflection on its implications—concerns the most appropriate way of integrating science into history, which "is to be attempted with better prospects through the medium of events and institutions than through configurations of ideas or culture" (p. 549). As the author of *The Edge of Objectivity*, Gillispie can reasonably claim to have contributed to the practice of both intellectual and (now) institutional approaches to the history of science. But the dichotomy between "events and institutions" and "ideas or culture" seems to me to be an altogether artificial one which Gillispie's own practice clearly transcends. I would prefer to conclude that *Science and Polity* encourages us to move beyond the distinction between an external and internal history of science to an integral understanding of that activity in all its dimensions.

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Influences on Galileo

Prelude to Galileo. Essays on Medieval and Sixteenth-Century Sources of Galileo's Thought. WILLIAM A. WALLACE. Reidel, Boston, 1981 (distributor, Kluwer Boston, Hingham, Mass.). xvi, 372 pp. Cloth, $49.95; paper, $23.50.

More than anyone else, William A. Wallace has illuminated our understanding of 16th-century scholastic natural philosophy for its own sake and as it may have influenced the thought of Galileo, who, sometime around 1590, wrote three notebooks in Latin in the form of scholastic questions. In *Prelude to Galileo*, 16 of Wallace's articles ranging from the Middle Ages to Galileo have been reprinted. They are grouped under four subdivisions: Medieval Prologue (two articles), The Sixteenth-Century Achievement (five articles), Galileo in the Sixteenth-Century Context (six articles), and From Medieval to Early Modern Science (three articles), which describes and evaluates the different interpretations of the relationship between medieval and early modern science espoused by three great historians of medieval science, Pierre Duhem, Anneliese Maier, and Ernest Moody. Although there is some repetition among the articles because the same theme is considered from different standpoints, significant changes have been made, including the addition of numerous introductions, two appendixes, and useful cross-references.

There is an abundance of intellectual riches in this volume. Well-conceived interpretations and insights are intermingled with a mass of carefully organized detail. Although by the author's own admission the section on Galileo is the core of the volume, the articles in it are placed in a context that deepens our understanding of his relationship to his medieval and early modern scholastic predecessors. But Wallace's major contribution in these articles is undoubtedly his new interpretation of Galileo's handwritten early scholastic notebooks that were previously thought to have been "trite scholastic exercises, copied from another source, probably a professor's notes transcribed by Galileo in 1584 while still a student at the University of Pisa," and thus to be "his 'youthful writings,' or *Juvenilia*, not his own work, material for which he had no real interest and indeed failed to comprehend, and so could have exerted no influence on his subsequent writings" (p. 137). It is this long-standing interpretation that is challenged by Wallace, who convincingly demonstrates that the two notebooks, one concerned with questions on Aristotle's *On the Heavens*, the other with questions on Aristotle's *On Generation and Corruption*, represent Galileo's own lectures, which were, however, derived directly from lectures by Jesuit professors who taught at the Collegio Romano between approximately 1577 and 1592 and from the famous *Commentary on the Sphere of Sacrobosco* by the Jesuit astronomer Christopher Clavius. Moreover, Galileo probably wrote these lectures around 1590 rather than in 1584, as previously believed. Instead of being trite exercises of a 20-year-old student, the lectures are probably the product of a 26- or 27-year-old professor at the University of Pisa. In his essay on Duhem, Wallace argues further that in *De motu*, the third unpublished notebook written around 1590, Galileo may also have derived his key ideas from unpublished lectures of the Jesuit professors of the Collegio Romano (pp. 310–315).

But what can be made of our new knowledge of Galileo's earliest extant lectures? Will it, as Wallace believes, furnish insight into Galileo's "intellectual formation" and enable us to identify "the philosophy with which he operated during the first stages of his teaching career" (p. 228)? It will—provided the lectures really reflect Galileo's genuine opinions arrived at by serious reflection. If, however, they are mere rearrangements of the lectures of others made—or perhaps compiled—for the sole purpose of meeting the teaching requirements of the University of Pisa, or any other university, then our confidence in them would seriously diminish. It is not farfetched to suppose that Galileo would have prepared university lectures that contained basic ideas to which he did not personally subscribe. He seems to have done this very thing in 1599 and 1603, when he taught a course at Padua that was "little more than a popular summary of the main points in Clavius's commentary on Sacrobosco" (p. 137). Since Galileo was already a convinced Copernican in 1597, any lectures based on Clavius, who was a resolute geocentrist, could not have reflected Galileo's true beliefs in 1599 and 1603. In Wallace's favor, however, is the *De motu*, which truly reflected Galileo's beliefs. If its major ideas were primarily derived from Jesuits at the Collegio Romano, as Wallace suggests, then perhaps the other two notebooks, filled with concepts also derived from the same group of Jesuits, represent Galileo's genuine opinions at the time he wrote them.

In these articles, Wallace has presented much that is new and of great importance and has done so with profound scholarship. He has raised issues that will be pursued for some time to come on the always fascinating problem of Galileo's relationship to his predecessors.

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Galactic Astronomy

The Structure and Evolution of Normal Galaxies. Papers from a NATO Advanced Study Institute, Cambridge, England, Aug. 1980. S. M. FALL and D. LYNDEN-BELL, Eds. Cambridge University Press, New York, 1981. xiv, 272 pp., illus. $29.95.

Normal galaxies are the subject of much activity, both theoretical and observational, as witness at least a dozen other conference proceedings published on related aspects of the same subject over the last decade. Our picture of the structure and composition of galaxies is changing rapidly, and the 16 brief review papers in these latest conference proceedings are useful summaries of the current status of the field.

The emphasis of work on galaxies has changed dramatically as new and unexpected observational and theoretical results have become available. Thus, the discovery that elliptical galaxies rotate more slowly than their ellipticities suggest has led to the view that these systems are not oblate spheroids but rather triaxial bodies slowly turning end over end. Aspects of this view, as well as of structurally similar components of disk systems, namely bulges and bars, are discussed in a series of papers by F. Bertola, J. J. Binney, G. Illingworth, J. Kormendy, and M. Schwarzschild. These five papers, together with S. Tremaine's description of galaxy mergers, cannibalism on a galactic scale, give an excellent summary of the observational and theoretical aspects of the subject. Although I have grouped elliptical galaxies together with parts of disk (spiral) systems, kinematically they are quite different. The relatively low rotational velocity of many elliptical galaxies is a property not shared by the bulges of disk systems. Rather, the kinematic data for bulges are consistent with their being oblate spheroids flattened by their own rotation. Illingworth, who makes this point in his review, is careful to note that the bulges he has studied are all intrinsically fainter than his sample of elliptical galaxies. Objects with similar luminosities are, at present, too few to test for similarities in dynamics.

Another kinematic finding, that of a constant rotational velocity at the outer regions of spiral galaxies, is also an underlying theme in several of the papers. This result, first described nearly a decade ago in 21-centimeter studies of spirals, has been repeatedly confirmed by optical and more extensive 21-centimeter measurements. Previously it was thought that the rotational velocities decreased well within the optical image, reflecting the decrease of luminosity and its implied mass. An appropriate question today would invert the situation: Are there any isolated galaxies that do show a systematic decrease of rotational velocities at large radii? R. Sancisi, in a concise review of the distribution and kinematics of the neutral hydrogen component of galaxies, cautiously suggests at least two such examples. He notes, however, that the presence of noncircular motions in the plane or large-scale motions perpendicular to the plane of these systems could also account for the inferred decrease in rotational velocity.

The resultant greater gravitational attraction in the outermost parts of a spiral, much greater than implied by the luminosity distribution, requires a drastic change in the mix of the mass responsible for this gravitational attraction compared to the mass (stars) responsible for the luminosity; that is, material with a high mass-to-luminosity ratio is required. This has led many astronomers to believe that most spirals are surrounded by halos of optically invisible matter. Has our view of spirals, their composition and evolution, been based on only a few percent of their total mass, that part that shines so brightly at optical wavelengths? The "ghost" component, if it really exists, is perhaps the major unsolved problem in the study of spiral galaxies.

The spiral pattern found in disk systems has long been a complex riddle. The observed spiral shapes should be quickly smeared over because of differential rotation. Why then are spirals so common? A number of elegant phenomena have been invoked to solve this riddle, for example, stochastic spirals and shock patterns from density waves, each continually generating new spiral features. A. Toomre focuses on yet another view, "a neat old phenomenon" that he calls swing amplification, "a strong cooperative effect that inhibits interarm travel and encourages gravitational bunching."