wave of interviews took place during an interval between Apollo missions. The first wave took place just after Apollo 12 and the last after Apollo 15. The data obtained in these interviews included responses both to open-ended but focused questions and to highly structured psychological scales and semantic differentials. The scientists were asked questions about where they stood at the time of each interview on several controversial scientific questions being addressed by Apollo, such as the origin of the moon, its temperature history, and the origin of mascons and tektites. Another bank of questions was designed to discover the actual epistemologies-in-use within the group. Here the focus was on the beliefs each scientist had about such issues as the relationship between theory and data, whether the hypotheticodeductive method is actually used by scientists, and whether scientific hypotheses can ever be verified or falsified. There was quite a range of opinions on these latter issues, revealing that several epistemologies were actually in use within this group of Apollo scientists.

The data Mitroff obtained in his interviews are rich, but the small number of subjects in his study did not allow him to ask some important questions of his data, such as what explains the variation in epistemologies-in-use, in degrees of commitment to theories of one kind or another, in aggressiveness, and in hostility that he finds within his group of scientists. Mitroff's goal in the analysis of his interviews seems restricted to showing that scientists do not always, or even frequently, conform to what he calls the "Storybook image" of science, according to which scientists are rational, emotionally neutral, universalistic, willing to share ideas openly, disinterested, and impartial. In his study, and in the analyses of almost any sociologist of science I can think of, scientists are often emotionally committed to an idea or theory, particularistic, self-interested, secretive, partial, and biased. It is not new any more to say these things about scientists, and certainly Robert Merton is no believer in the Storybook image of science as Mitroff tries to suggest. In addition, while demolishing the straw man of Storybook science. Mitroff quotes from his interviews excessively, which makes parts of the text drag considerably.

Even though Mitroff's assertions that scientists neither are objective in the traditional sense nor share a common epistemology are not new, they are not trivial. It is essential to know those two facts in order to realize that a new philosophy of science must come into play if we are to agree that science can in some sense ever be objective. Mitroff suggests such a philosophy of science in chapter 7, a philosophy that combines elements of what Mitroff calls the Kantian and Hegelian "Inquiring Systems" (IS) with the traditional Lockean and Leibnizian IS's:

> Science advances through the process of scientists of widely differing persuasions (types and degrees of commitment) thrusting their opposing conceptions and commitments at one another. Through this process science not only subjects its results to severe (but not crucial) tests but also exposes the underlying commitments of its practitioners.
>
> It is important to emphasize that in this process commitments alone do not make for the objectivity of science. It is the presence of intense commitments coupled with experiments (Lockean IS), seemingly impersonal tests, arguments (Leibnizian IS), evidence, and general paradigms that make for the objectivity of science. Science, as opposed to other systems of knowledge, is distinguished by the fact that, if not in theory then in actual practice, it has learned how to make use of strong determinants of rationality (testing, evidence, etc.) plus strong emotional commitments [p. 249].

There is much more that is worthwhile in this book about the Apollo scientists, their personalities, their research roles, their views of the Apollo program, and their notions about the moon as a symbol than can be discussed in this review. On balance the book is worth the price.

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Galileo's Thought

The Natural Philosophy of Galileo. Essay on the Origins and Formation of Classical Mechanics. MAURICE CLAVELIN. Translated from the French edition (Paris, 1968) by A. J. Pomerans. MIT Press, Cambridge, Mass., 1974. xxvi, 498 pp., illus. $25.

Galileo. A Philosophical Study. DUDLEY SHAPERE. University of Chicago Press, Chicago, 1974. xii, 162 pp. Cloth, $9.75; paper, $2.95.

Although long a hero of modern science, Galileo became a touchstone for historians of science in 1939 when Alexandre Koyré's *Études galiléennes* showed how to analyze the Scientific Revolution philosophically and thereby how to treat science historically. In the 35 years since then, Galileo has become something of a minor historical industry. Almost all of his major and important minor works have appeared in English translations (the most recent example being Stillman Drake's translation of *Two New Sciences*, University of Wisconsin Press, 1974), and one can count on at least a few scholarly studies each year. It is a further mark of Koyré's influence that, with few exceptions, the secondary literature has followed his mode of analysis (if not always his specific conclusions) and addressed the issues he raised.

The two works under consideration here may serve as good examples. Though quite different in scope, depth, and quality, they agree with each other and with Koyré in viewing Galileo's role in the history of science as a conceptual one, to be examined and grasped within Galileo's works themselves by detailed textual and conceptual analysis and by reference to a similarly textual past. Despite the chronological ordering of the material discussed, structual patterns take precedence over developmental ones, as Galileo's science is reduced to its essentials and then ranged in place between Aristotle's and Newton's. Galileo the man, especially the struggling young professor who could seldom make ends meet and was always on the lookout for a better-paying position while he taught subjects he disliked, or the witty man of letters who enjoyed a good argument perhaps as much as the search for truth, plays no role in either Clavelin's or Shapere's Galilean world.

Nonetheless, if it is Galileo's thought that one is interested in, one can hardly find a better guide than Clavelin's study. In a full, at times even wearying, tour of Galileo's science and its philosophical environs, Clavelin begins with an unusually lucid and informative account of Aristotle's doctrine of motion, emphasizing its ontological dependence on the mover and its cosmological underpinnings in a hierarchically ordered universe. Against the backdrop of this account, Clavelin is able to show that, for all the technical and critical ingenuity displayed, the medieval science of motion as pursued at Oxford and Paris retained its full Aristotelian commitment.

Galileo abandoned that commitment. He did so, argues Clavelin, in chronological stages and at two philosophical levels. His *De motu* (1592) opened the search for a unified science of motion expressed in mathematical terms. The *Meccaniche* (about 1595–1600) found a way to do a mathematics of motion that combined Archimedes and Aristotle's *Mechanical Problems* and, in its analysis of the inclined plane, made a fundamental separation between the gravific and motor forces of weight. By seeking a single account of "natural" and "forced" motion, admitting motion in a void, and dissociating weight from natural place, Galileo had already begun to undermine the cosmological bases of the Aristotelian doctrine of motion. In the *Two World Systems* (1632), Galileo defended the Copernican system by setting out new cosmological principles that not only conceded but required a moving earth. By providing a new basis for cosmological order, namely, a uniform circular motion ontologically equivalent to rest, and by using a principle of conservation of that motion (and composition of motions superadded to it), Galileo freed motion from its ties to the moving body (or to the mover) and made it the subject of independent study.

The *Two New Sciences* (1637) represents that study. Since the notion of "change of motion" was no longer self-contradictory, Galileo could treat kinematics as the science of changes in velocity resting on a new definition of acceleration. Despite some interesting thoughts on the infinite and on indivisibles, however, Galileo lacked the mathematical tools to follow through on his essentially "differential" definition and had to turn to the methods (though not necessarily the underlying concepts) of his 14th-century predecessors. With an extensive theoretical kinematics at hand, he could then employ the earlier separation of weight and motive force to argue the equal acceleration of all bodies in a void and thus to link mathematical motion and real motion.

More than a technical achievement, Galileo's science of motion offered a new standard of scientific explanation. By seeking for observed data an abstract model from which the data followed as mathematical corollaries, Galileo found that middle road between pure reason and pure perception that has since become the foundation of modern science.

This summary hardly does justice to the complexity and subtlety of Clavelin's analysis, which is rewarding at every turn. Though it would be caviling to point out occasional errors in fact or questionable interpretations of details, it is worth noting that Clavelin at times, and especially toward the end of the book, undercuts the historical strength of his arguments by comparing Galileo's thought to some "right" or "normal" standard of scientific reasoning, as if Galileo were to be graded on his progress toward the present.

Shapere's book focuses on two aspects of Galileo's scientific thought: first, whether he had the principle of inertia and, second, his attitudes toward and uses of experiment, mathematics, and idealization as methods of scientific inquiry and exposition. None of these questions is new, nor do the mode of analysis and the conclusions reached seem to break any new ground. The discussion takes place on a restrictively conceptual level. In chapter 2, for example, on Galileo's intellectual background, such nonphilosophical determinants as the Renaissance engineering tradition are ignored in favor of a pure Platonism reduced to five basic propositions and an essential Aristotelianism condensed to three basic distinctions. To be a Platonist, Galileo will have to subscribe to the five propositions, both in thought and in literary deed; to be a true founder of classical mechanics and modern science, he will have to reject the three distinctions root and branch. Since we have been told at the outset that the principle of inertia was fundamental in the transition from medieval to modern science and that Galileo did not have that principle, we do not expect him to be freed of the distinctions. The very succinctness of the five propositions serves as an early warning that Galileo will be denied a place in the Academy.

So it turns out. By the end of chapter 4, Galileo has held too tightly to the medieval tradition outlined in chapter 3. Whatever the novel elements of the *Two World Systems* (reduced to five propositions), ultimately Galileo "shied away" (p. 121) from the modern principle in order to preserve the static, spatial order of an Aristotelian universe. And what of his method? Shapere is undecided about the importance of experiment (pp. 143–144) and shares the historian's disappointment over the "scanty and inconclusive" record (p. 86). The importance of mathematics to Galileo cannot be ignored, but it could not derive from Platonism as opposed to Aristotelianism, since Plato and Aristotle essentially agreed on the place of mathematics in natural philosophy (p. 138).

There is a petulance about this book which undercuts its historical, if not its philosophical, value. A tone of argument informs the text, but it is not always clear who is doing the arguing, and with whom. In chapter 3 especially, the passive voice combines with logical inference to compose theories, critiques, and rebuttals that possibly no one in fact held or presented, and the absence of documentation prevents recourse to the sources. At the level of more recent, identifiable controversy, Kuhn, Koyré, and Mach are the frequent targets of Shapere's historical and philosophical thrusts. But Mach seems by now a ragged ta.get, and Koyré's Platonist Galileo has long since become (at least for historians) a Renaissance eclectic. As for Kuhn, before one can rebut his arguments one must understand them, and Shapere does not.

On Galileo, read Clavelin.

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The Cavendish Laboratory, 1874–1974. J. G. CROWTHER. Science History Publications (Neale Watson), New York, 1974. xvi, 464 pp., illus. $50; prepaid, $35.

The subject of this book is a rich and fascinating one, for Cambridge University's Cavendish Laboratory has been a leading center of research during the past century. Construction of the Cavendish was made possible when the chancellor of the university, William Cavendish, Seventh Duke of Devonshire, generously provided the estimated £6300 required for the building and apparatus. Prior to its formal opening in 1874 the laboratory was referred to as the Devonshire Laboratory, but at that time it was given the name of the Cavendish family, which had counted among its members the celebrated 18th-century scientist Henry Cavendish, whose unpublished papers on electricity were edited and published in 1878 by the Cavendish's first professor of experimental physics, James Clerk Maxwell. The original building and the buildings of the New Cavendish Laboratory (in use since 1973) are described by Crowther.

Cavendish scientists have made momentous advances in such fields as