his current view really is. He seems to want to say that there are paradigm-independent considerations which constitute rational bases for introducing and accepting new paradigms; but his use of the term "reasons" is vitiated by his considering them to be "values," so that he seems not to have gotten beyond his former view after all. He seems to want to say that there is progress in science; but all grounds of assessment again apparently turn out to be "values," and we are left with the same old relativism. And he seems unwilling to abandon "incommensurability," while trying, unsuccessfully, to assert that communication and comparison are possible.

These issues come to a head in Kuhn's proposals as to what must be done if a complete understanding of science is to be obtained, and what the character of that understanding will be once obtained. For the fundamental question is, Do scientists (at least sometimes, even in "revolutionary" episodes) proceed as they do because there are objective reasons for doing so, or do we call those procedures "reasonable" merely because a certain group sanctions them? Despite the ambiguities and inconsistencies of many of his remarks, Kuhn's tendency is clearly toward the latter alternative. Though occasionally tentative ("Some of the principles deployed in my explanation of science are irreducibly sociological, at least at this time"—*Criticism*, p. 237), in most passages he asserts his view categorically: "The explanation [of scientific progress] must, in the final analysis, be psychological or sociological. . . . I doubt that there is another sort of answer to be found" (*Criticism*, p. 21). "Whatever scientific progress may be, we must account for it by examining the nature of the scientific group, discovering what it values, what it tolerates, and what it disdains. That position is intrinsically sociological" (*Criticism*, p. 238). We must study scientific communities not as one of several steps in clarifying the nature of science (in attempting, say, to separate the irrational from the rational components as a prelude to analyzing the latter); it is the *only* step. What the community says is rational, scientific, is so; beyond this, there is no answer to be found. An alternative to this view is to think of sociology as able to bring to our attention the kinds of biases which scientists should learn to avoid, as interferences, hindrances to good scientific judgment. For Kuhn, however, such biases are an integral, and indeed the central, aspect of science. The point I have tried to make is not merely that Kuhn's is a view which denies the objectivity and rationality of the scientific enterprise; I have tried to show that the arguments by which Kuhn arrives at his conclusion are unclear and unsatisfactory.

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The Underpinnings of the Chemical Revolution

Atoms and Powers. An Essay on Newtonian Matter-Theory and the Development of Chemistry. ARNOLD THACKRAY. Harvard University Press, Cambridge, Mass., 1970. xxvi, 326 pp., illus. $12. Harvard Monographs in the History of Science.

This stimulating and suggestive essay ranges over that most important century of chemical history, the 18th. Most of the significant and familiar figures are discussed—Stahl, Boerhaave, Hales, Black, Priestley, Maquer, Lavoisier, Dalton—but in what will be an unfamilar setting to most readers. The trials of phlogiston theory and the triumphs of pneumatic chemistry do not loom large in this book. Rather, the author has sought to delve into the more fundamental presuppositions about matter theory which 18th-century natural philosophers and chemists debated and which formed part of the theoretical underpinning for the more spectacular episodes of the chemical revolution. This then is not a positivistic account of chemical discovery, but an essay that seeks to illuminate the place of chemistry in some of the major scientific and intellectual currents of the 18th century. Such approaches to the history of chemistry have been and still are exceedingly rare, and one must therefore accord a special welcome to Thackray's book.

The aspect of 18th-century chemical theory that Thackray has sought to explore is the impact and influence of Sir Isaac Newton's speculations on the nature of matter. It is no easy task to unravel the strands of Newtonian matter theory, for in this area the master's legacy was far from definitive or even consistent. Newton's own views were subject to some modifications throughout his lifetime, dependent upon such varied factors as the state of his researches on light and colors, the Cartesian and Leibnitzian criticisms of his natural philosophy, and not least his own heterodox theological beliefs. This provided his faithful disciples with ample scope for individual interpretation of the canonical corpus. Out of their mélange of text and gloss, Thackray has isolated three beliefs as fundamental to orthodox Newtonian matter theory in the 18th century: first, that matter was inertially homogeneous and internally structured (that is, that matter was ultimately composed of particles of identical solid matter defined inertially and that the qualitative differences of bulk matter were due to the different spatial arrangement of these fundamental particles or atoms); second, the acceptance of attractive and repulsive forces as the proper categories of explanation in a discussion of chemical change; and third, a belief in an all-pervading ether. Although Thackray points up the importance of ethereal concepts in 18th-century chemistry from Boerhaave's "matter of fire" through Hales's "air" to Dalton's "caloric," it is the aspect of the Newtonian legacy he explores least, thereby depriving his book (and his reader) of a full discussion of one of the most important themes of 18th-century chemistry.

By contrast, Thackray devotes much of his book to the influence of the other aspects of Newton's matter theory, namely the belief in an atomic structure of matter and the acceptance of interparticle forces. He follows the fate of these views from the early eagerness of Newton's immediate disciples, most notably the Keill brothers and John Freind, to reduce chemistry to a set of laws for the short-range forces operative between the constituent particles of matter, to the much later attempts to quantify the forces of chemical affinity by a more empirical approach as exemplified in the work of such later French chemists as Macquer, Guyton de Morveau, Fourcroy, and Berthollet, perhaps the last of great Newtonian visionaries in chemistry. Thackray has some interesting suggestions as to why the quest for the laws of affinity was abandoned in mid-century by British natural philosophers and chemists only to be picked up with increased vigor by the French in the latter half of the century. In a chapter devoted to the more speculative British natural philosophers such as Bryan Robinson, Robert Green, and Gowin Knight, he points to the negative influence of Boscovitch on British attempts to quantify the laws of chemistry. Irony indeed! For although Boscovitch's system was predicated upon the view that matter is internally structured and composed of homogeneous (though dimensionless) units associated with quantifiable forces of attraction and repulsion, he maintained that the determination of particular chemical events from the general theory was quite beyond the capacity of the human mind. By contrast we have Buffon's bold profession, in 1765, of Newtonian faith that the law of chemical affinity was identical with the inverse-square law of gravitation, and that "this law seems to admit of no variation in particular attractions, but what arises from the figure of the constituent particles of each substances . . . ." The invitation of the influential Buffon to chemists was clear: determine by experiment the forces of affinity between different chemical species, and the departure of these force relationships from an inverse-square law will reveal the shape of the constituent particles.

One factor served to modify the thoroughgoing Newtonian program of Buffon for chemistry, even in France. This was the Stahlian influence, which called for a chemistry based upon quality-endowed elements or principles related to laboratory chemical experience and not upon physical atoms or principles which lay well below the level of observation. This view found much favor with practicing laboratory chemists such as Macquer and Rouelle and led to a certain agnosticism about the ultimate physical constituents of matter; it did not, however, preclude research on or consideration of the problem of affinity. This tension in late-18th-century French chemistry is most clearly seen in Lavoisier, who expresses agnosticism about atoms and bases his chemistry on empirically defined elements but at the same time expresses a faith in the ultimate success of such researches as Guyton de Morveau's on the quantification of the laws of affinity.

Ultimately this tension was to be relieved by John Dalton, who quantified chemistry not in terms of force laws but in terms of the macro-determined *weights* of the *atoms* of *elements*. In the last and best chapter of the book, Thackray places Dalton in the context of British popular Newtonianism of the 18th century, which followed the paths of the itinerant lecturers through dissenting rural England. Although Dalton's *New System of Chemical Philosophy* struck a fatal blow to the Newtonian dream of chemistry by its basic assertion of the heterogeneity of matter, it is testimony to the influence of Newton on chemistry that Dalton felt the necessity of doctoring a quotation from the 31st Query of the *Optics* to justify his position.

This essay offers many other provocative insights into 18-century chemistry, although the author shows a disconcerting reluctance to follow up many of his suggestions. Nevertheless, he has given the specialist much to ponder and provided the general reader with a very readable account of an important episode in chemical history.

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Galileo's Innovations

Galileo Studies. Personality, Tradition, and Revolution. STILLMAN DRAKE. University of Michigan Press, Ann Arbor, 1970. x, 290 pp., illus. $8.50.

The 13 essays collected in *Galileo Studies* elucidate the work of Galileo "as he himself approached it, performed it, regarded it, and evaluated it." Selecting from articles he has published earlier, Drake combines, modifies, and edits those selected to illustrate a major theme set forth in his introductory chapter. He remarks that intellectual historians are at present bent on attributing as much of Galileo's thought as possible to his predecessors and as little as possible to his own originality. Granting that valuable clues to the path an individual thinker is likely to pursue may be had from reconstructed trends and patterns of ideas, Drake insists effectively on the indispensability of a complementary biographical or "internal" investigation of Galileo's own writings. Only by following out the steps Galileo took in his work can one do justice to this innovating thinker and correct mistaken attributions regarding the sources, burden, and contexts of his ideas. Did a concept of inertia first emerge in Galileo's mind in connection with projectile motion? Did Galileo believe circularity to be an essential component in inertial motions? Was Galileo indebted to, and building on, the Merton rule (mean-degree theorem of uniform motion) in his analysis of falling bodies? Affirmative answers to all three questions have become generally accepted by historians of ideas. In three essays ("Galileo and the concept of inertia," "Free fall and uniform acceleration," and "The case against 'circular inertia'"), Drake submits that the evidence afforded by strict attention to relevant texts accurately rendered does not warrant the affirmative answers.

To understand and appreciate Galileo's contributions to the advancement of science, one must indeed be informed about prior developments and traditions. Drake supplies an excellent portrait of physical thought in 16th-century Italy in his first essay, "Physics and tradition before Galileo." In his fifth essay, "The effectiveness of Galileo's work," he relates Galileo to his contemporaries as follows: Galileo was born into a world that already had a highly developed and technically advanced mathematical astronomy, but it had no coherent mathematical physics and no physical astronomy at all. It was Galileo who, by consistently applying mathematics to physics and physics to astronomy, first brought mathematics, physics, and astronomy together in a truly significant and fruitful way. The three disciplines had always been looked upon as essentially separate; Galileo revealed their triply paired relationships and thereby opened new fields of investigation to men of widely divergent interests and abilities. Mathematical astronomy, mathematical physics, and physical astronomy have ever since constituted an inseparable triad of science at the very base of modern physical science. Therein, I think, lies the primary explanation of Galileo's effectiveness [p. 97].

In setting forth Galileo's program for the reform of physical science and in showing how his work contrasted with that of his predecessors Drake ably defends the claims of priority that have been made for Galileo. Also, in several essays, he is concerned with personal factors—with facets and traits of Galileo's character—as these contribute to