technology. If it had not been for Gay-Lussac's own acknowledgment of Charles's dubious priority, we should have "Gay-Lussac's law" of the constancy of gaseous expansion with temperature. We do unequivocally recognize "Gay-Lussac's law" of combining volumes of gases. Working alone and with Louis-Jacques Thenard, he isolated boron and helped to clarify the nature of the alkali metals and the halogens (his paper on iodine is a masterpiece of systematic and thorough chemical investigation). Here, incidentally, Crosland very sensibly and without partisanship balances the value of the French chemists' work against that of Humphry Davy of overinflated historical reputation. Alcoholometry, analytic technique, methods of assaying, the Gay-Lussac tower: all these and many more contributions are examined and placed within their respective lines of scientific and technical development.

The idea of a "contribution" to science, however, suggests a theory that regards scientific advance as analogous in the intellectual realm to the additive accumulation of capital in the economic. Crosland has always insisted (in this and in previous writings) that Gay-Lussac's primary purpose as a scientist was to discover "laws." Leaving aside the question whether this is or is not what every scientist would like to do, we may ask: is that all he wanted to do? His mentors, Berthollet and Laplace, hoped to reduce molecular phenomena to Newtonian physics. They thought they fully understood celestial dynamics, and in their view (and in Napoleon's view as well) it was time to understand the dynamics of small particles and the agencies—heat, light, electricity, magnetism, and capillarity—with which they interact. The task was to make the second Scientific Revolution, that is, not to add to our knowledge but to transform it. Did Gay-Lussac ever share these hopes and this program? If he did, when and why did he give them up? If he did not, how does one explain his "positivism," his humble satisfaction with enunciating laws rather than exploring mechanisms? Was there no sense of loss, no bitterness (as in Dulong's case), no conflict in this descent from the grand style of his teachers? Do we dare to suggest that perhaps the analogy between career, as accumulation of posts and property, and contribution, as cumulative advance of narrow knowledge, cuts deeper than we at first thought?

On these and related questions—those for example raised recently by J. W. Herivel, Eugene Frankel, and Robert Fox regarding the decline of scientific culture in France during precisely the years (1818–1830) when Gay-Lussac turned from chemical science to chemical technology—Crosland is silent. This patient and thoroughgoing biography—Crosland spent years negotiating with the Gay-Lussac family and various curators and archivists to get access to Gay-Lussac's papers—will likely remain the standard one. It is very informative. It is, however, not the last word.

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A Test of the Kuhnian Theory

Chemistry Transformed. The Paradigmatic Shift from Phlogiston to Oxygen. H. GILMAN McCANN. Ablex, Norwood, N.J., 1978. x, 180 pp. $14.95. Modern Sociology.

Since its appearance in 1962, T. S. Kuhn's *The Structure of Scientific Revolutions* has been at the center of a vigorous and often vacuous debate. In this brief monograph McCann, a recent Princeton Ph.D. in sociology, seeks to pin down Kuhn's protean theory of scientific revolutions. First he "operationalizes" the theory; then he tests his version against Lavoisier's chemical revolution.

In specifying the theory of scientific revolutions, McCann makes two sorts of claims—some about the broad developments that are likely to accompany a scientific revolution and some about the scientists who are likely to be recruited to a revolutionary paradigm. During a scientific revolution, he proposes, the pertinent specialty community's size and productivity will increase faster than usual because new opportunities for recognition will stimulate immigration and publication. Likewise, so long as the revolution's success is in doubt, the community's output will be more theoretical than usual because the conflict is essentially over interpretative principles. Once the revolutionaries feel assured of victory, however, they will abandon polemics and resume the practice of normal science. Meanwhile, in sciences amenable to quantification, the community's standards of exactitude will rise in consequence of attempts by the contestants to use quantitative evidence to buttress their positions. Finally, as the revolution succeeds, its triumph will be mirrored in the growth of the revolutionaries' share of the community's publications and citations.

Regarding the recruits to the revolutionary faction, McCann postulates that the more theoretical, quantitative, and youthful members of the community will tend to embrace the new paradigm more readily than their colleagues. Those with theoretical propensities will tend to join the vanguard because their approach obliges them to consider any theory that promises to resolve alleged anomalies. Those with quantitative propensities will tend to join the vanguard because their penchant for precision makes them susceptible to any theory that challenges orthodoxy on quantitative grounds. And the youthful will tend to join the vanguard because, lacking a prolonged involvement with and investment in the prevailing paradigm, they are more likely to perceive scientific and personal advantages in rallying to the revolution.

As a test for his propositions, McCann chose the late-18th-century revolution in chemistry, partly because Kuhn made frequent reference to this case. Limiting himself to the periodical literature in Britain and France, McCann sought out and coded all the articles bearing on chemistry published by authors in these countries between 1760 and 1795. From this collection of 868 articles by 207 authors, he drew a "cited sample" consisting of the 717 articles written by the 129 authors whose work was of sufficient interest to garner them one or more references in the original collection. This sample serves as McCann's data base for most of his testing.

Dividing the sample along national lines, McCann finds impressive confirmation for long-surmised differences in the size and productivity of the British and French chemical communities of the late 18th century. While 27 British chemists published 101 articles between 1760 and 1795, 102 French chemists published 616 articles! Struck by this disparity, McCann draws upon the historical and biographical literature to argue that the comparative weakness of the chemical community in Britain was due to the relative lack of opportunities there both for learning chemistry and for making a career in the science. For all its interest, however, this argument is but a digression. Indeed, McCann's subsequent statistical analysis of the British case turns out to be another digression. As he admits, this analysis is inconclusive, partly because membership in the British chemical community never exceeded ten intermittently productive chemists and partly because Lavoisier's theory never made much headway in the British periodical literature prior to 1795.

Unlike the British case, the French

case provides an adequate basis for testing hypotheses about scientific revolutions. The French chemical community was fairly large and tight-knit and it did swing over to Lavoisier's theory between 1785 and 1789. To judge from McCann's statistics, the French case furnishes substantial support for his claims about the developments accompanying a scientific revolution. Somewhat contrary to prediction, the most rapid growth in the community's size and productivity occurred between 1772 and 1777, *before* Lavoisier managed to convince his fellow chemists that their science was in a state of crisis. Just as predicted, however, the level of theory in the chemical literature rose to a peak in the mid-1780's, then declined; the fraction of articles reporting quantitative results climbed from 25 percent for 1760–1777 to 36 percent for 1778–1795; and the swift triumph of Lavoisier's theory within the French chemical community between 1785 and 1789 was reflected in a rapid rise in the revolutionaries' share of the community's publications and citations.

While McCann marshalls considerable support for his trend hypotheses, he fails to make an effective case for his propositions regarding recruitment. The trouble is that he takes articles, rather than chemists, as the basic units of analysis. As a result a few prolific revolutionaries dominate his statistics, obscuring the details of the recruitment process. For instance, from the evidence presented, it is not at all clear that younger chemists tended to embrace Lavoisier's theory sooner than older chemists in France. (D. L. Hull *et al.*, *Science* **202**, 717 [1978], have recently shown that such a hypothesis is not supported by evidence from the Darwinian revolution in Britain.)

Many historians of science will boggle at McCann's unbridled enthusiasm for statistics, his somewhat cavalier attitude toward historical research, his failure to provide such basic information as lists of cited chemists and dates of conversion, and so on. This reviewer, however, welcomes his attempt to subject the theory of scientific revolutions to the bar of quantitative evidence. Although his conclusion that the outcome is almost entirely favorable to the theory is unwarranted because of the flaws in his testing procedures, his goal is laudable and his version of the theory suggestive. It is to be hoped that similar, more sophisticated studies will soon be forthcoming.

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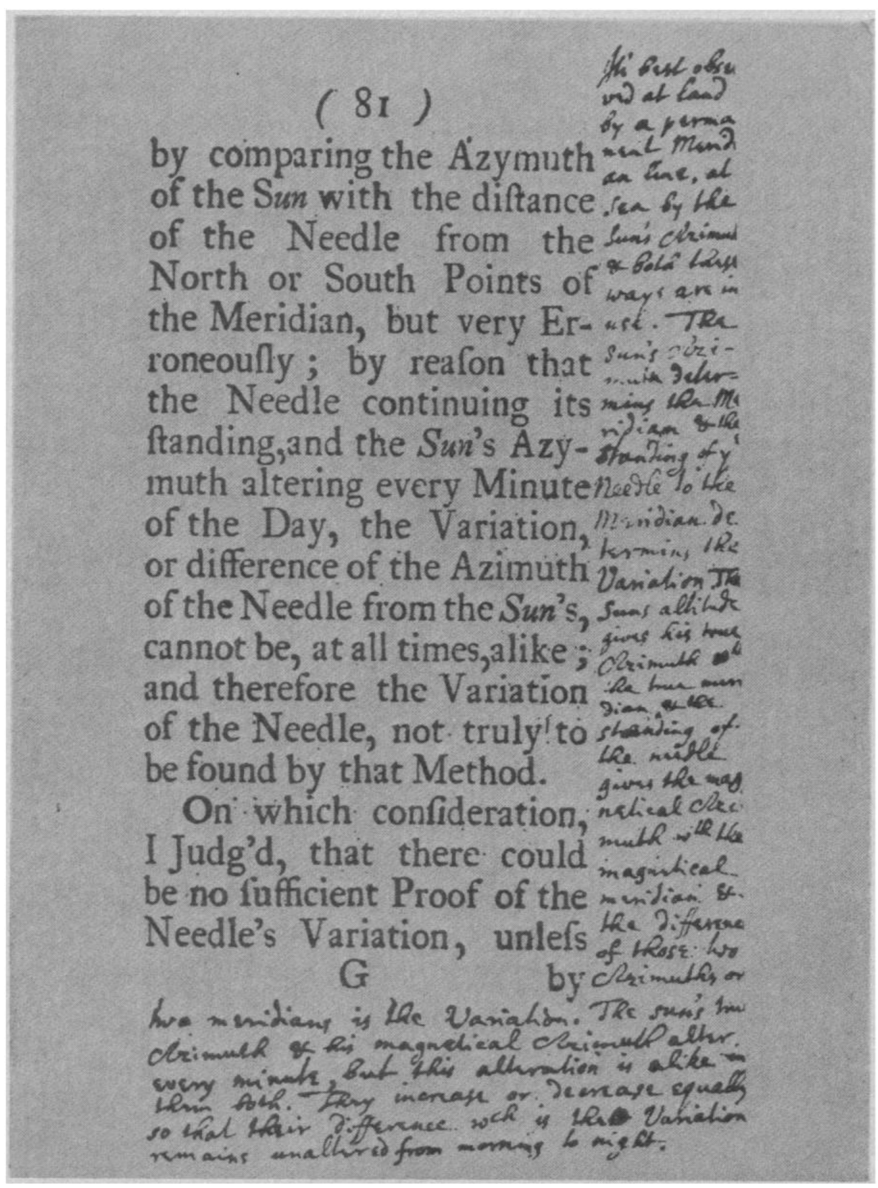
Newton's Reading

The Library of Isaac Newton. JOHN HARRISON. Cambridge University Press, New York, 1978. xiv, 286 pp. $62.50.

I cannot think of enough good things to say about John Harrison's book on Newton's library. It is a book aimed at Newtonian scholars, and as such a scholar I can affirm that Harrison has given us just what we wanted. The heart of the book, filling two-thirds of its pages, is a catalogue of 1763 works for which there is evidence that Newton owned them. Two catalogues made by later owners of the library, which Richard de Villamil published in 1931, furnish the basis of the present catalogue. Those two 18th-century catalogues were mere lists, with entries so abbreviated they were frequently difficult to interpret. Harrison's entries provide full bibliographic references for all the books he has been able to identify (the overwhelming majority), and he has further supplied numerous cross-references. He has gone far beyond this. Nearly half of the library, kept intact for more than two centuries, is now in Trinity College, Cambridge, and quite a few other volumes once in it have surfaced in various other collections. Harrison has inspected all the books in Trinity and many of the others; in his catalogue he supplies a history of references to each volume, its present location if known, any inscriptions Newton or others inserted, and a resume of notations Newton made in it. In a large number of cases the inscriptions of others were written by authors in copies they were giving to Newton, providing in the early books a precious record of Newton's range of acquaintance and in later ones testimony to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by comparing the Azymuth of the *Sun* with the diſtance of the Needle from the North or South Points of the Meridian, but very Erroneouſly; by reaſon that the Needle continuing its ſtanding, and the *Sun's* Azymuth altering every Minute of the Day, the Variation, or difference of the Azimuth of the Needle from the *Sun's*, cannot be, at all times, alike; and therefore the Variation of the Needle, not truly to be found by that Method.

On which conſideration, I Judg'd, that there could be no ſufficient Proof of the Needle's Variation, unleſs

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Newton's notes in Edward Howard's *Copernicans of all sorts, convicted: by proving that the earth hath no diurnal or annual motion* (London, 1705). [From *The Library of Isaac Newton*]