more. The discussion is very thorough. All the information is given to allow a reader to draw his own conclusions independent of theirs. Their conclusion is that there is no need for ABM deployment now because there is no practicable way for the U.S.S.R. to reduce our forces below the strength required for a retaliatory attack. They are very convincing, but then I was convinced before I read the book.

The only discussion of need for a Minuteman defense in *Why ABM?* is in chapter 2, by Herzberg. It is not really a discussion but rather an assertion of need in two paragraphs. Herzberg agrees that Safeguard is not the most effective way of defending Minuteman, but he thinks that the need is real and urgent, and favors Safeguard deployment as the only technology available now. If I thought a defense was required, I should agree; but I do not see that he or any other contributor to *Why ABM?* has demonstrated an urgent need.

There are several chapters in *ABM: An Evaluation* that deal with both the effectiveness and the cost of defending Minuteman assuming the defensive system works, and with the question of whether the system will work at all. In particular, Weinberg's chapter on cost and effectiveness is quite well done and concludes that we get very little defense for the cost with Safeguard. The argument made in other chapters that Safeguard cannot work reliably is not impressive. In *Why ABM?* the point is made by several contributors that it doesn't really matter very much if the system won't work well in a war; the name of the game is deterrence of war, and what really matters, therefore, is whether a potential enemy can dare to believe it won't work at all. Questions of cost-effectiveness and reliability are really relevant only to a discussion of alternative means of accomplishing the same ends.

The most important questions raised in both books concern arms limitation. We and the Soviet Union find ourselves with huge stocks of strategic arms that are of no use to either. The two sides agree that neither's security is enhanced by the possession of excessive numbers of nuclear weapons. In spite of this, we and the Soviet Union have been unable to come to an agreement on limiting the production and deployment of strategic arms. Both of these books emphasize again and again the importance of an arms control agreement with the Soviet Union. The evaluations of the effects of an ABM deployment, however, are nearly mirror images.

In *ABM: An Evaluation*, the contributors argue that deployment of the Safeguard system will make arms control much more difficult and will lead to another round in the arms race. Their argument rests primarily on the characteristics of the Safeguard system. Its components are those that were originally designed for a defense of cities, and city defense upsets the strategic balance. Since the longest lead time in building a defensive system is that required for setting up production facilities and testing and modifying the operating system, they argue that the Soviet Union must start now to develop a counter to a city defense even though we now say we have no intention of deploying one. Thus Safeguard must inevitably provoke another round in the arms race.

The conclusion reached on this question in *Why ABM?* is that an ABM system will not necessarily cause further buildup in offensive armaments. The contributors argue that the Soviet Union has always been defense-oriented, that the leaders of the Soviet Union have publicly favored defensive weapons systems, and that therefore a defensive weapons system will not provoke a response.

If the Safeguard system were built of components useful to defend only our retaliatory forces, I might believe that deployment would have little if any effect on the arms race or on the prospects of negotiating an agreement. However, since deployment of this system would decrease the lead time for development of a heavy defense by three to five years (Kahn's estimate in *Why ABM?*), and many prominent proponents of the ABM are still arguing for a heavy city defense (see Brennan's chapter in *Why ABM?*), I cannot see how the U.S.S.R. could allow this system to be deployed without responding.

There are many parts of both books that I have not discussed—on China, nonproliferation, effects on Europe and Asia, the history of arms control negotiations, and others. These are all questions involving national policy and its international implications, as are virtually all the *important* questions in the ABM debate. Having read both books, I'm left with an impression that perhaps the authors didn't intend. I find the technical differences between the two sets of experts to be minor in spite of the enormous amount of heat and smoke generated about them in the public debate. All of the important differences are matters of political judgment.

Both books are important reading: *ABM: An Evaluation* because, regardless of the outcome of the present debate in the Senate, this issue will come up again, and the public debate has been about the wrong things; and *Why ABM?* because of the insight it gives into the general problem of strategic policy. The people who come out badly in these books are those who are not mentioned in either—the political leaders in the administration and in Congress who have allowed the pursuit of technical superiority to dominate the pursuit of national and world security.

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Fluxions and Optics

The Mathematical Papers of Isaac Newton. Vol. 3, 1670–1673. Edited by D. T. WHITESIDE, with the assistance in publication of M. A. Hoskin and A. Prag. Cambridge University Press, New York, 1969. xl + 576 pp. + plates. $32.50.

In recent years funds and dedicated scholarship have been found to start the work that eventually will lead to the long-delayed complete edition of all of Newton's works, published and unpublished. Four volumes of *Correspondence* have so far appeared, covering the years 1669–1701. Two volumes of *Mathematical Works* have gathered together published English versions of Newton's mathematical tracts, all from the 18th century. Now, again under the competent editorial guidance of D. T. Whiteside (who is also responsible for the *Mathematical Works*), are appearing the *Mathematical Papers* (mark the somewhat confusing difference between "Works" and "Papers"). They will bring us, in eight volumes, all Newton's extant notes and manuscripts, accompanied by English translations. In 1967 and 1968 the first two volumes appeared, covering the years 1664–1670, which comprise Newton's "golden age" of discovery. The third volume brings us up to 1673.

In 1669 the 26-year-old Newton, whose discoveries in analysis and optics, as yet unpublished, had impressed, among others, his teacher Isaac Barrow, had succeeded Barrow in the Lucasian mathematical chair at Cambridge. During the ensuing years Newton continued his sheltered life at Trinity College, leaving Cambridge only a few times. He worked assiduously on his researches in mathematics, chemistry, and optics. From these years dates his construction of the reflecting telescope. He corresponded with John Collins in London, a man of wide scientific connections, eager to assist him in making his discoveries known, and with Henry Oldenburg, secretary of the Royal Society. Oldenburg, a man of even wider scientific connections, sponsored Newton's entry into the Royal Society after Newton had presented a communication on his telescope. Shortly after having become a Fellow of the Society, in 1672, Newton published in its *Philosophical Transactions* the "New theory about light and colours," his first printed paper.

Most of Newton's mathematical work in the period 1670–1673 dealt with the preparation of his Lucasian lectures on optics and with the casting of his discoveries in the theory of infinite series and fluxions into publishable form. The manuscripts on these subjects form the bulk of this third volume, and the greater part consists in the majestic tract "De methodis serierum et fluxionum," on the methods of series and fluxions, the first ample exposition of what we now call the calculus. The market for a book on such an abstruse subject being severely limited, Newton first expected it to be published as an appendix to a Latin version of an "Algebra" written by the Dutchman Kinckhuysen. Nothing came of this or of any modified plan of publication, and the manuscript rarely left Newton's files until it was published in 1736, after his death, by John Colson in an English translation from a copy made by William Jones. The original Latin text appeared only in 1773. If this manuscript, a labor of reworking and amplifying older notes, composed in the winter of 1670–71, had been published at the time, the history of the calculus would have run a different course. As it was, the calculus came to published light only in 1684 by means of a small paper by Leibniz (who did not mention Newton), and Leibniz's version won the day, but only after a bitter fight of gigantic pettiness.

Perhaps the most interesting sections of the remaining part of this third volume are the extant texts of Newton's optical lectures. They can now also be compared with the published text, in this case the posthumous *Lectiones opticae* of 1729. Another interesting paper, unpublished so far, is a study on the harmonic motion of a point in a cycloidal arc under gravity. Here Newton finds its tautochronic character, and this independently of Huygens, who published his famous results on the pendulum in 1673. Here, happily, there was no priority fight.

We can only have the greatest admiration for the patient, painstaking, and dedicated scholarship which the editor and his colleagues have bestowed on their task. Studying, understanding, dating, and collating the Newton manuscripts is an arduous task. An added task was the English translation and the preparation of the introduction and the generous notes, which by themselves almost constitute an introduction into late-17th-century mathematics. Beautifully presented by the publisher, this is a work which will be eagerly consulted for many years to come, a veritable (we blush for the cliché) *momentum aere perennius.*

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NMR in Organic Chemistry

Nuclear Magnetic Resonance Spectroscopy. Frank A. Bovey. Academic Press, New York, 1969. x + 398 pp., illus. $16.50.

The book under review is a brief introduction to nuclear magnetic resonance spectroscopy in the tradition of the now classic monograph by Pople, Bernstein, and Schneider (1959). Since the book is actually an introduction to applications of high-resolution NMR to organic chemistry, the title is somewhat too broad.

The sections the reviewer found best are those that reflect the research of the author: the part dealing with various aspects of molecular shielding, the chapter on coupling of nuclear spins and their dependence on molecular structure and geometry, and the section dealing with symmetry and polymer configuration. Polymer chemists in particular will benefit from the treatment of problems related to molecular symmetry and polymer configuration, because coverage of NMR applications to the study of polymers is usually missing in comparable monographs. The actual text is only 242 pages long (the rest are various appendices), and many important topics are treated only briefly. But even the sketchy outline of some recent developments is helpful; on the basis of a few key references the reader can choose several literature sources from which to learn in detail about the subject in question.

For a chemist dealing with organic structural problems acquaintance at least with some of the rules and approaches used in the analysis of high-resolution NMR spectra of strongly coupled spin systems has become a necessity. The author introduces in an empirical way the analysis of simple spin systems such as AB, ABX, and AB_2. The reader who attempts to recognize a typical spectral pattern of a simple, strongly coupled spin system often occurring in organic compounds is also aided by a rather extensive appendix in which about 300 computer-generated line spectra are given.

In the section dealing with chemical exchange processes a more extended discussion of complete line-shape analysis and of the relative merits of various approximative methods still often used by organic chemists might have been helpful.

As the author points out, the book is an extension of a review article on NMR which he published in *Chemical and Engineering News* in 1965. Owing to its nonmathematical, empirical approach and condensed size, the monograph would be useful to chemists and workers in other fields who want to be introduced to the basic ideas of high-resolution NMR spectroscopy and who also are trying to develop some working knowledge in solving problems related to organic chemistry.

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Antibodies, DNA, and RNA

Nucleic Acids in Immunology. Proceedings of a symposium, New Brunswick, N.J., Oct. 1967. Otto J. Plescia and Werner Braun, Eds. Springer-Verlag, New York, 1968. xviii + 726 pp., illus. $22.

This book begins with a discussion of methods for obtaining antibodies to nucleic acids. Antigens can be obtained either by complexing oligo- or polynucleotides with methylated serum albumin or by the covalent bonding of