in 1966 to live in Britain, and even before that his experiences and contacts with the events and developments he describes were very limited; but because of the secrecy surrounding the Soviet space program a Westerner welcomes any glimpse of the Soviet viewpoint and of Soviet options during the early years of the space age.

The hero of Vladimirov's epic is Sergei Korolyov, the chief designer of spaceships, and the villains are the Soviet leaders and their political system. Vladimirov tells how Korolyov carefully studied the well-publicized American schedules of space missions and, by choosing projects accordingly, was able to achieve many "firsts" in space, including the launching of the first satellite in 1957, and to hold many space records until the Gemini program came into full swing. This successful strategy was, of course, popular with the Soviet leaders, so that Korolyov was able to obtain and keep political support and high priority for his programs. In Vladimirov's opinion this led to the illusion that the Soviets were far ahead of the United States, that they were indeed the number-one space power; hence the title, *The Russian Space Bluff*. I do not like the title; it is not descriptive of the book's content, and in fact there was no bluff in the usual sense of the word.

To me the greatest disappointment with the book lies in the author's failure to analyze the launch requirements for going to the moon. Although he argues that the Russians did not have any capability to compete with Apollo in a race to land a man on the moon, he does not mention the need for new large space launchers or any Soviet work on them. Clearly the booster was the key; it must be very large, it takes years to develop and test, and it requires large industrial facilities. Without a new launcher the Soviets were obviously not going to the moon.

The first Soviet test of a new large launcher was the orbiting of the Proton 1 satellite in July 1965. This launcher was in the same class as the U.S. Saturn 1, which had orbited the Pegasus 1 satellite in February 1965. However, a still larger launcher was needed for a manned landing on the moon. The Saturn 5 was first tested by the launch of Apollo 4 into earth orbit in November 1967. Although there have been occasional rumors that the Soviets were developing a comparable launcher, none has emerged.

The Zond 4 (March 1968), Zond 5 (September 1968), and Zond 6 (November 1968) were unmanned tests of circumlunar spacecraft of the Soyuz class that used the Proton launchers, and were obviously intended to lead up to a manned flyby and return mission. After the Apollo 8 circumlunar flight of Borman, Lovell, and Anders in December 1968, this Soviet manned circumlunar program was apparently abandoned; the last two missions, Zond 7 and 8, were flown unmanned in August 1969 and October 1970, yielding results of only minor significance.

The Luna 15 spacecraft was launched by a Proton booster and went into lunar orbit three days before Apollo 11. Although the mission failed, it was clearly a sample-return mission like the later successful Luna 16, and was hastily flown in an effort to detract from the dramatic success of the Apollo program. Vladimirov correctly analyzes this second type of "Apollo spoiler" mission, but he fails to appreciate the importance of the new space launcher. After all, this new Proton launcher soft-landed 1880 kilograms (Luna 16) on the moon, compared with 100 kilograms (Luna 9) for the standard launcher. Even this was far short of that required for a manned landing.

Vladimirov writes of Korolyov's early life and his years in prison, as well as of his rise to fame in the space age as the leader of the most successful Soviet rocket design team. Many of the incidents he relates have not been told before, certainly not by the official biographers. Vladimirov feels that it was most unfair to defer recognition of Korolyov and his achievements until after his death. This kind of secretiveness is indeed a curious practice, since the living leaders of the Soviet aircraft design teams are well known to the world.

Vladimirov is convinced that the effectiveness of Soviet space research is on the decline, and he enumerates four main defects:

> The first is the continual and invariably harmful interference in scientific affairs on the part of political leaders with no understanding of science; the second is the necessity under which scientists work to try and fit all their scientific conclusions—no matter what their branch of science—into the prevailing ideological framework of Marxism-Leninism; the third is the unbelievable conservatism and sluggishness inherent in the country's economic structure which results in a general fear of everything novel or of taking responsibility for possible failure; the fourth is the all-pervading secrecy.

The portrait of scientific research in the Soviet Union is most interesting, although I should have liked to read something about the Soviet attitudes toward their planetary programs, which, as with most of the unmanned missions, are not mentioned.

At first blush it would appear that Vladimirov's statements regarding the unique and dynamic role played by the genius Korolyov in the Soviet space program exaggerate a bit; however, the large number of major failures in the program since Korolyov's death—most recently Salyut 2—lend credence to his argument.

Despite many limitations, this book is important to all who are interested in the Soviet space program, the persons involved, and their motives. It constitutes a valuable and in some ways unique source of information about the Soviet side of the Space Race—one of the principal technological and political themes of the 1960's.

MERTON E. DAVIES
Physical Sciences Department,
Rand Corporation,
Santa Monica, California

Supply Equations

The Energy Crisis. LAWRENCE ROCKS and RICHARD P. RUNYON. Crown, New York, 1972. xviii, 190 pp., illus. Cloth, $5.95; paper, $2.95.

This is a good summary of the current alarm over alleged energy and mineral shortages. The thesis is simple: exponential growth depleting fixed resources equals catastrophe. Probably nothing in it was not said a hundred years ago by W. S. Jevons in *The Coal Question* (1865). Yet known coal resources today are larger by one or two orders of magnitude than they were then. The energy resource in shortest supply, crude oil, was being depleted in 1938 at a higher percentage of proved reserves than in 1972; also iron ore, aluminum, and copper. Prices of "nonrenewable" minerals have tended more to decline in this century than to rise. Obviously something is wrong with the theory. I suggest two defects, each fatal. First, reserves are only the ready shelf inventory of a mineral industry, only a small fraction of a much larger amount known to be in existence, which in turn is only a portion of the unknown and basically unknowable amount in the earth's crust. Second, the idea of exponential growth in the face of a fixed supply—assuming the supply is really fixed—is self-contradictory. For as the supply diminishes, the costs of working it rise and growth is slowed or stopped. If growth does not cease, it is because the mineral continues in abundant supply or because substitutes are found.

Limits to growth must exist, and we may for all I know be close to them. I am not sure how much punishment earth, air, and water can take, and we may be running up the cost of cleanliness so fast that unless growth is curbed man's life will soon again become nasty, brutish, and short. These subjects deserve careful examination. It is a great pity that the current excited clamor about nonexistent energy crises and mineral shortages diverts attention and resources from what may be the most serious problem of this century or the next.

If one really believed that mineral resources were becoming increasingly scarce, there would be grounds for austere optimism. Pollution would of itself become increasingly difficult and expensive. Providence would have put a brake on the ability of mankind to poison itself. But there is no sign that we are being let off that easily.

M. A. ADELMAN
Department of Economics, Massachusetts Institute of Technology, Cambridge

The Prince of Amateurs

The Mathematical Career of Pierre de Fermat (1601–1665). MICHAEL SEAN MAHONEY. Princeton University Press, Princeton, N.J., 1973. xx, 420 pp., illus. $20.

A quarter of a century ago J. L. Coolidge published *The Mathematics of Great Amateurs* (Oxford, 1949). Explaining the failure to include Fermat, whom E. T. Bell had called the Prince of Amateurs, he wrote (p. vi), "He was so really great that he should count as a professional." Coolidge's decision was, at the time, regretted, for no substantial account of Fermat's contributions was available. Grounds for that regret vanish with the publication of this volume, the first full-length serious study of Fermat's mathematics. It is an altogether exemplary account, for it goes well beyond a catalog of what was done and when, to provide a critical analysis of major aspects in the search for a leitmotif. This could be no casual undertaking, for Fermat was working on the frontiers of his subject at a critical stage in its development. Cartesian geometry bears the name of his chief rival, yet the precepts of the art had first come to the attention of Parisian mathematicians through manuscript copies of Fermat's *Introduction to Loci.* Mahoney's account (pp. 76–142) of this striking case of simultaneity of discovery is excellent. Eschewing a facile comparison of Fermat's analytic geometry with that of Descartes, or with modern views, he has afforded us a penetrating view of its relationship to the earlier Greek geometric analysis. There is also a discussion in detail of technical points, such as Fermat's criticism of Descartes's rules on the simplest curves required to solve geometrical problems.

An outstanding characteristic of this volume is its evenhanded evaluation of Fermat's discoveries. The author does not play the role of a protagonist arguing the case of his hero. This comes out, for example, in the discussion of the method of maxima and minima, for which Laplace had hailed Fermat as the discoverer of the differential calculus. Here Mahoney raises a point which modifies a view widely held by historians of mathematics. Against the customary notion that Fermat's algorithm arose from infinitesimal or limit considerations, he argues very plausibly (p. 148) that it rested instead on "Viète's brilliantly original, but (as befits a professional lawyer) frustratingly casuistic theory of equations." He holds (p. 164) that Fermat's use of the term *adaequalitas*

> . . . has certainly led historians of mathematics astray. For into it they have read the pseudo-equality of the differential calculus. . . . It cannot, however, provide that service. Fermat's method was finitistic, and so too was his use of the term *adequality*.

The word later took on, in the *Treatise on Quadrature* (about 1658) and the *Treatise on Rectification* (1660), a meaning closer to the concepts of the infinitesimal calculus, yet a preoccupation with problems kept him from seeing the fundamental theorem of the calculus (p. 279).

> Fermat found the answers to the problems he had posed. He did not invent the calculus.

In the wealth and beauty of the problems he proposed, Fermat's greatest claim to fame lies in the theory of numbers, where Mahoney's task of analysis was magnified by Fermat's failure to provide completed proofs. The well-known "Last" or "Great Theorem" sums up Fermat's work in that field.

> It is shrouded in mystery because Fermat could not or would not find the time to record his "proof" for posterity, or even for himself. The "proof" probably was no proof, because Fermat could not be bothered by detailed demonstration of theorems his superb mathematical intuition told him were true.

On one minor point (p. 289) the author gives credit to Fermat for a formula for amicable numbers known eight centuries earlier to Thabit ibn Qurra; but this does no harm to the well-substantiated evaluation (p. 280) that

> In number theory especially, one sees the paradox of Fermat's mathematical career: in seeking to renew and continue old, classical traditions, he unconsciously shattered them to lay the foundations of a new modern tradition.

This view, permeating the volume, is later (p. 352) expressed still more sharply:

> In a very real sense, Fermat presided over the death of classical Greek tradition in mathematics.

Earlier one has read (p. 66), concerning Huygens, Newton, and Leibniz, that "they, their colleagues, and their followers could learn little or nothing from [Fermat's] analytic treatises." That this may be too categorical an assertion is suggested by J. E. Hofmann's paper, "Über die ersten infinitesimal, mathematischen Studien von Johann Bernoulli," presented at the Twelfth International Congress of the History of Science at Paris in 1968, in which Fermat's influence on Bernoulli is indicated. Toward the close of the book Mahoney has given a more judicious statement on this problem (p. 353):

> Fermat's failure to publish did not preclude his influence in the development of mathematics in his age and later. It meant, rather, that the influence would be severed from his name. . . . Only number theory would remain Fermat's undisputed province; it would do so, ironically, because Fermat could interest none of his contemporaries in it.

In order to focus attention on the analytic transformation in mathematics wrought by Fermat's problems in the calculus, his contributions to coordinate geometry, and his theorems in the theory of numbers, Mahoney has made these three aspects the chief concern of the 30 sections making up this book. In an epilogue, "Fermat in retrospect," and in two appendices will be found