bottom up rather than the top down—Middleton is able to give insight into such matters as the morale of the Division at various stages.

The National Research Council is Canada's leading scientific institution, and from these two studies of it Americans can learn much about the development of national science in their northern neighbor. The story is an interesting contrast to that of this country as presented, for example, in Hunter Dupree's *Science in the Federal Government* (1957) or Daniel Kevles's recent book, *The Physicists* (1978).

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A View of the Soviet Scene

Manipulated Science. The Crisis of Science and Scientists in the Soviet Union Today. MARK POPOVSKY. Translated from the Russian by Paul S. Falla. Doubleday, Garden City, N.Y., 1979. xviii, 244 pp. + plates. $10.95.

Before coming to the West from the U.S.S.R. in 1977 the author of this book was a prominent scientific journalist and biographer with a wealth of acquaintances within the Soviet scientific community. He has drawn deeply upon his personal experiences in order to write a polemical account of how the Soviet scientific system works or, to be more accurate, fails to work. Unlike Western accounts of the economics and administration of Soviet science, which are dependent upon open sources, impressions from study visits to the U.S.S.R., and selected interviews with Soviet research administrators, Popovsky's seeks to convey by wealth of anecdote and rumor the "real" flavor of the Soviet scientists' working life. Popovsky is remarkably uninhibited about naming the major culprits in high official positions in the Soviet scientific establishment, so that by the end of the book the reader is provided with a kind of rogues' gallery complete with identikit photographs. The book is written in a lively and persuasive style and is clearly motivated by a passionate desire to expose "the truth" that lies behind appearances and misleading official representations. Thus the critical Western reader, although he or she will read Popovsky's book with interest, is faced with the problem of deciding how much of it is typical or true, what relative weights should be attached to anecdotal compared with published sources (often critical but invariably restrained), and whether such a damning indictment of the political leaders' intentions is plausible in view of their expressed desire for rapid technical progress. In other words, is the case overstated?

The picture of Soviet scientists drawn by Popovsky is the exact opposite of Merton's classic view of the scientific community as a self-regulating group of independent scholars whose internalized norms of open-mindedness and peer evaluation are central to the promotion of scientific advance. According to Popovsky the political offensive against the technical intelligentsia began not in the late 1920's, as most Western historians would have it, but abruptly after the Bolshevik seizure of power in 1917. The political authorities made war on the old scientific establishment by a policy of deliberate starvation and intimidation and subsequently replaced them by politically loyal but often poorly trained cadres. All this served their primary objective of *political control* over opposition groups, real or potential. The unfortunate combination of obsessive control with long-standing Russian traditions of hierarchy and rank served to displace collegial relationships in science. A new generation of institute directors grew up eager to do the bidding of their political masters, unscrupulous in the pursuit of their own careers, and ruthless in their control over subordinate staff. Even those of genuine ability and conscience were able and are able, in various convoluted ways, to rationalize the compromises of their scholarly integrity that the Soviet system imposes; the rank-and-file scientific workers, on the other hand, derive what enjoyment they can from their scientific work but otherwise react to their administrative superiors with tact, private cynicism, and calculated apathy.

There are several factual errors in Popovsky's account, but much more important than these are the glaring omissions of counterevidence that would have necessitated some modification of the central message. For example, it is difficult to take seriously a history of Soviet science and technology that gives the government *no* credit for the industrialization of a backward country, the promotion of widespread literacy, the improvement in health and living standards of ordinary people, or the funding of science on such a generous scale. If this sounds like the insistence of the censor that the positive side should outweigh the negative it is certainly not intended as such. The technological and economic achievements of the Soviet regime can still be reconciled with the predicament of the individual scientist, but the interpretation needs to be based on a much more subtle understanding of the trade-off between economic and political goals. Moreover, the assertions that the authorities are willing to sacrifice efficiency in order to achieve maximum control (p. 49) and are hostile to good ideas because they highlight their own mediocrity (p. 142) do not allow for the frantic and evidently sincere concern the leadership has shown about the current slowdown in the rate of economic growth in the U.S.S.R. and about the wide technology gap that has opened up with the West in many key sectors.

Notwithstanding some doubts about its objectivity, this book maintains the interest of the reader throughout and has many new things to say. The chapters on research institutes in the defense sector and on the deterioration of the working atmosphere in the new science cities find no equivalent in other works on the subject, and Popovsky's classification of the different psychologies of research workers in the face of political interference is illuminating. Zhores Medvedev's recent book *Soviet Science*, which covers much the same ground as Popovsky's, is a cooler and more thoughtful book than Popovsky's but less rich in anecdotal material. In this sense, the two books are complementary. The restrained systemic analysis of the former tempers the fiery journalism of the latter.

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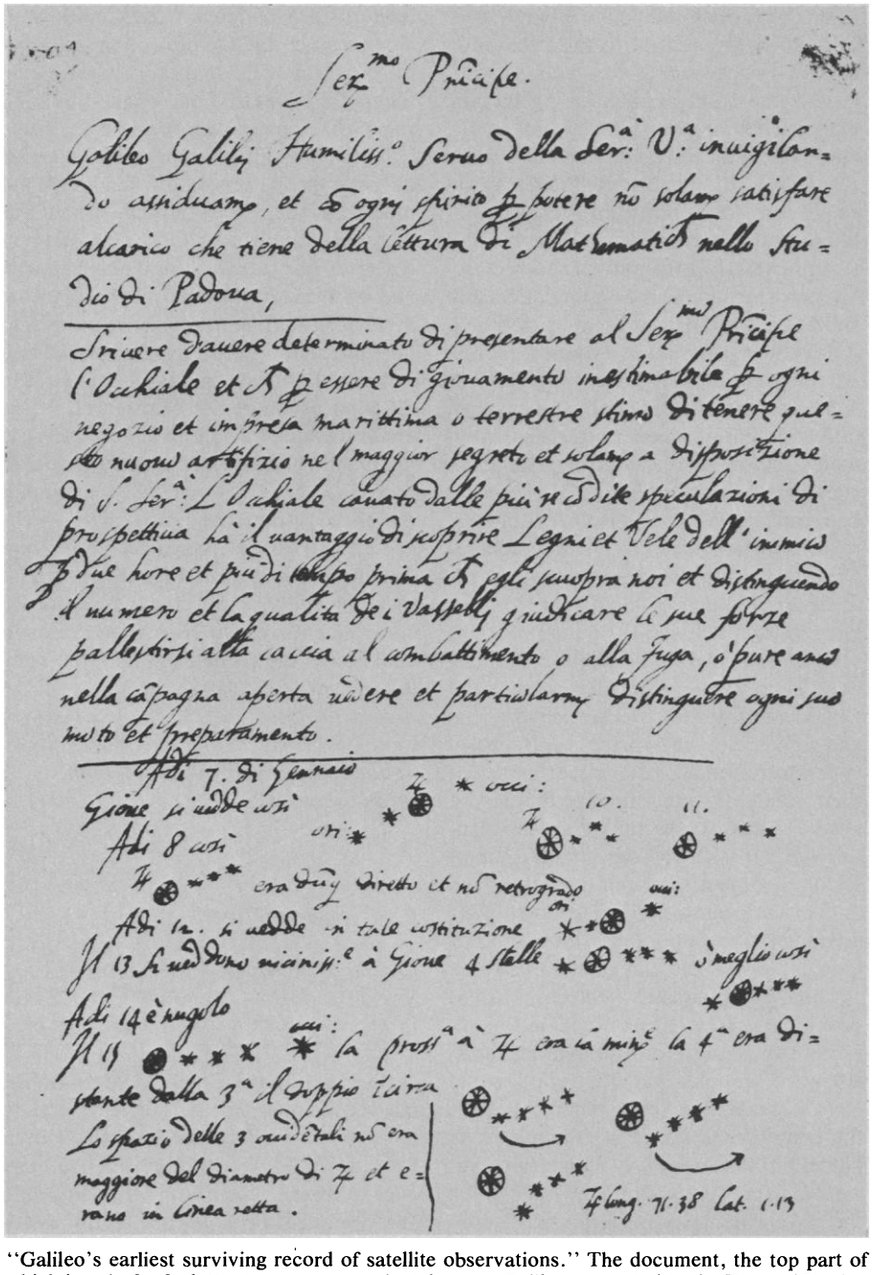
Galileo as Scientist

Galileo at Work. His Scientific Biography. STILLMAN DRAKE. University of Chicago Press, Chicago, 1978. xxiv, 536 pp., illus. $25.

Scientists have found Galileo Galilei most congenial, in the sense that they have been inclined to find their own "roots" in his work. The practice goes back at least to Isaac Newton, who in his *Mathematical Principles of Natural Philosophy* credited Galileo with knowledge or discovery of several concepts used in his own system of the world, specifically the law of inertia, the law of force, the principle of superposition of motion, the times-squared law of fall, and the parabolic path of projectiles. The list also includes Albert Einstein, who wrote a foreword to Stillman Drake's English translation of Galileo's *Dialogue Concerning the Two Chief World Systems* (1953) in which he summarized the main

point of the book as demonstrating the uselessness of an abstract center of the universe for explaining the fall of heavy bodies, thus drawing an analogy between Galileo's approach and his own general-relativistic rejection of an inertial system to explain the inertial behavior of matter.

To this traditional scientific appreciation of Galileo contrast modern interpretations put forth by historians and philosophers of science. Early in the 20th century Pierre Duhem, in *Studies on Leonardo da Vinci* (1913), argued that, historically, Galileo's important ideas derived from the medieval scholastics of Paris and that, philosophically, he was a reactionary for upholding a realistic interpretation of scientific knowledge against what Duhem regarded as the more progressive positivist instrumentalist view allegedly held by the Catholic Church. Then in 1924 there was Edwin A. Burtt's *Metaphysical Foundations of Modern Physical Science* arguing that there was no place for human values in the world of facts and figures created by Galileo's science. In 1939 Alexandre Koyré's *Galilean Studies* argued that on the one hand Galileo's work shows that good physics is made a priori and on the other his physics was not that good; for example, his conception of inertia was that of natural circular motion, not rectilinear motion (the latter conception being allegedly a contribution made by Descartes).



"Galileo's earliest surviving record of satellite observations." The document, the top part of which is a draft of a letter to accompany the telescope Galileo presented to the Doge of Venice four months earlier, shows observations of Jupiter made 7–15 January 1610. The diagrams in the lower right-hand corner "show that Galileo did not at once jump to the conclusion that the stars were literally orbiting around Jupiter. The minimum assumption was that they moved with respect to Jupiter, and in appearance they moved back and forth along a straight line. . . . At first there was no way of identifying a particular satellite among the three originally discerned. When four appeared, on 13 January, a problem arose how one star might pass another, if that was what they did. [Galileo's] arrows indicated a theory irreconcilable with true orbiting around Jupiter. That again is an indication that he was wedded neither to the Copernican system nor to the philosophical idea that all heavenly motions must necessarily be circular in form. When the latter became the simplest hypothesis for explaining what was actually seen, on the night of 15 January 1610, Galileo adopted it." [Department of Rare Books and Special Collections, University of Michigan Library; reproduced in *Galileo at Work*]

The trend toward such historical-philosophical interpretation has continued to the present, and in the last quarter-century it has been criticized by Stillman Drake, whose efforts began and have continued with the much-needed translation into English of all of Galileo's major works and whose interpretations are incomparably superior to those of his predecessors in scientific understanding and biographical information. The present work is Drake's most ambitious undertaking and most substantial accomplishment to date.

In a sense, the book is a well-documented vindication of the traditional scientific appreciation of Galileo. Moreover, the documentation is so extensive (including almost everything Galileo said or did scientifically) and includes so much new material (Galileo's unpublished and previously mostly unexamined manuscript notes on motion) that it may be some time before historians and philosophers can assimilate the information found in the book. In calling it a vindication I do not want to give the impression that the book is an explicit polemic. On the contrary, Drake's concern is with the systematic chronological arrangement and explanation of all documentary evidence relating to Galileo's scientific career; in this project Drake's approach could be described as a painstaking and unswerving adherence to two principles: to describe the facts as they really happened and to understand or explain, not judge or evaluate, what Galileo did. This approach is reflected not only in the content of Drake's accounts but in their arrangement: each of the book's 22 chapters bears for its title merely the years of Galileo's life under consideration and a list of scientific topics he studied in that period. For example, chapter 7 is headed "1606–8: The nova again; beam strength;

hydrostatics; speed paradox resolved; inertial experiment and trajectory."

Perhaps the most important result of Drake's examination is his proof that virtually all of Galileo's work in kinematics, published in 1638 in Days III and IV of *Two New Sciences*, had been completed by 1609, when his telescopic discoveries got him involved in astronomy, and that most of that part of the book had been written by the time the *Dialogue* of 1632 was published. The significance of this is that the latter book is thus shown to be Galileo's synthesis of physics and astronomy, and a defense of Copernicanism grounded on the second of his "two new sciences," rather than an incompletely thought-out espousal of Copernicanism. With respect to methodology, Drake documents at least two important qualities that augment the traditional view of Galileo as a keen observer, ingenious experimenter, and mathematical interpreter of nature: one is interest in and success with predictions, and the other is an engineer mentality. Galileo's predictive prowess showed itself primarily in connection with positions of Jupiter's satellites and the appearance and disappearance of Saturn's rings, predictions concerning which were sent by letter to acquaintances and were usually confirmed. His engineering frame of mind becomes evident from Drake's discussion of Galileo's construction of such instruments as the calculating "sector," the microscope, and the thermoscope (besides, of course, the telescope) and of the reports he frequently was asked to make to his employer (the Grand Duke of Tuscany) concerning various projects and problems of civil engineering.

The most controversial issue the book is likely to raise concerns what I shall call the philosophical question. Drake explicitly asserts in the preface that he wants to avoid discussion of the "philosophical implications" of Galileo's scientific work. This he does partly for temperamental reasons and partly for what I cannot refrain from calling philosophical reasons. He justifies his approach by reference to Galileo's view (with which he expresses agreement) that science and philosophy are distinct, and that hence one may engage in the former without engaging in the latter. The case, of course, depends on what is meant by the terms.

By "science" Drake usually means the study of questions that can be decided by "sense experiences and necessary demonstrations," and by "philosophy" the study of those questions that cannot be so decided. This view provides Drake's rationale for avoiding the philosophical question in favor of staying as close as possible to the texts, documents, and evidence of Galileo's scientific work and developing the theme of "Galileo at work" to which the book's title calls attention. Other times in Drake's usage "science" refers to the investigation of physical problems and "philosophy" to speculation on metaphysical topics. Here I think Drake is completely right when he emphasizes that Galileo did not engage in metaphysics.

A qualification is in order concerning the type of "philosophy" that is most likely to come to mind in a scientific context, namely philosophy of science, meaning considerations about the nature, aims, and methods of scientific knowledge. Drake's book documents that Galileo frequently engaged in such considerations, mostly in the process of explaining and justifying his scientific ideas in the face of opposition. Thus we have Galileo portrayed as both a scientist and a philosopher in this sense.

In summary, this is a timely book for scientists interested in their roots, an epoch-making book for the quality and thoroughness of the documentation, and a provocative one concerning the "philosophical implications" which Drake refrains from discussing but which others inevitably will.

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Sex Roles in the Kibbutz

Gender and Culture. Kibbutz Women Revisited. MELFORD E. SPIRO. Duke University Press, Durham, N.C., 1979. xx, 116 pp. $9.75.

This book originated as the Howard Eikenberry Jensen Lectures presented at Duke University. The published version provides a concise and readable account of the changes that have occurred among women of the kibbutz since Spiro conducted his well-known research of the '50's (see his *Kibbutz: Venture in Utopia* and *Children of the Kibbutz*, Harvard University Press, 1956 and 1958). He describes the erosion of the early ideals of sexual equality and notes that the hard-won actuality had given way, in 1975, to a division of labor in which most women are engaged in service jobs such as laundry and child care. Men hold the prestigious farm labor jobs. Today few women are engaged in kibbutz governance. Marriage and the family have increased in importance, although both institutions had been regarded by the "pioneers" as obstacles to the collectivist spirit and to the emancipation of women. There has been a "return from radical feminism to femininity" (p. 44). A beauty parlor now operates within the kibbutz studied by Spiro. Spiro describes these changes in some detail, using data from his fieldwork of 1950 and 1975. Has this sexual counter-revolution been imposed on the young "sabra" women of today? Quite the contrary, according to Spiro. Not only has the definition of equality been changed from one of "identity" to one of "equivalence," the women themselves are content to view their present condition as more "natural." The problem of female discontent, which Spiro identified in 1950, remains not because women are denied equal access to all jobs within the collective but because of the restricted opportunities available to both sexes in any small, rural community.

Unlike similar reports by Talmon-Garber (*Family and Community in the Kibbutz*, Harvard University Press, 1972) and Tiger and Shepher (*Women in the Kibbutz*, Harcourt Brace Jovanovich, 1975), Spiro's report examines the roots of this counter-revolution with reference to his own longitudinal data. Many of the individuals he observed and interviewed in 1975 were the children he and his wife studied in 1950. We learn that, although socialized so as to minimize all sex differences except those in dress and personal names, these preschoolers (now grown to adulthood) demonstrated significant sex differences in their behavior, particularly their play behavior.

Unfortunately this important portion of the book is a frustrating mixture of interpretation and data. A lecture format does not allow for digression, but the published version should have included an appendix giving the definition of a "play sequence," telling how many were recorded for each child and for each age group, and giving some information on how the behavior was transcribed and the reliability of the data. No actual frequencies of behavior are recorded in any of the tables; only percentages are given. For example, we are told the girls pretended to be animals in 23 percent of their fantasy play. But how many acts were scored? Is this percentage unique to girls in 1950, reared communally in a setting that minimizes sex differences? No data for any other group but the boys are provided for comparison. In short, the material is not presented with the clarity needed to understand what the scores for each sex mean.

Spiro provides a simplistic five-part division into which "determinants of hu-