We now come to the question of why pension plans do not pay their accumulated funds to retirees in a single sum. The main reason corporations and institutions throughout the country—whether commercial or nonprofit, public or private—do not make lump-sum settlements of accumulated benefits upon retirement is the conviction that their pension obligation to a retired employee lasts as long as the employee does, and that their pension plan must therefore pay the maximum possible *lifetime* income—an income that retired employees cannot outlive or lose through poor investments or incapacity in old age.

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Reference

1. *Standard and Poor's Trade and Securities Statistics, Security Price Index Record* (Standard and Poor, New York, 1970), pp. 167–184; *Standard and Poor's Trade and Securities Statistics, Security and Price Index Record, Current Statistics Supplement* (Standard and Poor, New York, Jan. 1972), pp. 50–51.

Ph.D.'s with Husbands

Susan M. Ervin-Tripp (Letters, 24 Dec., p. 1281) describes a recipe for determining whether the hiring of Ph.D.'s discriminates against women: ". . . multiply by .91 [the percentage of women with doctorates working in the last decade . . .] the percentage of Ph.D.'s that were given to women scientists in the top five departments in each field. . . ." Unless an institution or department has the resulting percentage of women at each rank it doesn't qualify as discrimination-free.

This hypothesis has much to recommend it, and Ervin-Tripp in all likelihood has stated a suitable zeroth-order approximation to the problem. Unfortunately she has neglected several first and second order corrections that may be comparable in magnitude to the term she cites. Her proposal actually gives an upper bound which may be several times larger than a true nondiscriminatory level of employment and which might mislead some into practices that discriminate seriously against men.

The corrections are necessary because Ervin-Tripp makes the implicit assumption that for the purposes of employment the mobility of women is identical to that of men. This may be reasonably valid for unmarried women. However, marriage places a constraint on the mobility of both men and women and limits their joint opportunities for careers, the limitation being more severe if they elect to live in a small, nondiversified community. For the sake of simplifying the analysis, let us divide the married women into two fractions, f_l and f_e, liberated and enslaved, depending on whether the woman or her husband determines where they both live.

In these terms, the percentage calculated by Ervin-Tripp's formula should be multiplied by the quantity

$$Q = [F_u + F_m (f_l + f_e P_h)] \quad (1)$$

in which F_u and F_m are the fractions of women with Ph.D.'s who are unmarried and married. The quantity P_h is the probability of an enslaved, married woman Ph.D. having a husband whose work takes them to a community that has an academic position suitable to her talents.

Insofar as I know, there have been no very detailed studies of what governs P_h, or of its impact upon the employment of women Ph.D.'s. However, for those in a large metropolitan area one would expect P_h to be larger than for a smaller, college-dominated town. My limited experience with employment of women Ph.D.'s on our faculty and with the placement of our own graduates leads me to believe that the effective value of P_h for an institution and community such as ours may be as small as ⅕ in some disciplines. In Eq. 1, F_u and f_l are substantially less than ¼, so to a good approximation, $Q \simeq P_h$. Therefore, the Ervin-Tripp approach overestimates the nondiscriminatory level of academic employment of women Ph.D.'s by the factor $1/P_h$, which may be severalfold.

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Special Virus Cancer Program

Nicholas Wade's report on the Special Virus Cancer Program (SVCP) at the National Cancer Institute (News and Comment, 24 Dec., p. 1306) needed to be written, was well researched, and represents a good overview. However, I reject criticism by "a virologist acquainted with NIH affairs," "a virologist under contract to the SVCP," "academic scien-

tists," "one well-known virologist," "many outside scientists," "a prominent critic of the SVCP," and other assorted virologists. I am distressed that so many of my colleagues are apprehensive about expressing their views. *Science* should thoroughly investigate and document this "fear." It may represent a problem of far greater significance than the SVCP and administrative aspects of science.

I share a number of the criticisms of the SVCP, but what is "worthless junk" to some represents an important attempt to others. For example, who was prepared some years ago to discount the possibility that material from human cancer patients might produce neoplasms in subhuman primates? This expensive undertaking was a failure, but even the nonelegant attempt represented an important extension of knowledge. The theater-like press conference to announce scientific findings and the concentration of large amounts of money in the hands of a few are certainly areas where constructive criticism by knowledgeable scientists may influence future events in a positive manner. However, the former is not restricted to scientists connected with the SVCP.

We should strive for high standards and maintain a posture that allows constructive criticism. I would urge my "hidden" scientific colleagues not to be so fearful. Some of us are friends.

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Wade's report on the SVCP is a poor example of scientific reporting on two grounds: extensive quotations of anonymous opinions and lack of objectivity.

It is inadmissible for a serious journal to accept the statements of scientists who, hiding behind anonymity, publicly express severe judgments toward their colleagues. Anonymous statements tend to be irresponsible; they cannot be evaluated by the public, who do not know how qualified the "eminent virologist" or the "distinguished cancer researcher" are. The lack of objectivity is evident in Wade's failure to report more favorable opinions and to compare the scientific output of the SVCP with other programs.

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. . . any scientist who expresses his opinions of a particular program should not be ashamed or afraid to have his name

published. If a particular scientist does not feel sufficiently competent in his research and is afraid criticism of the SVCP would affect his grant or the renewal of his contract, then he should honestly reply "No comment" to the interviewer's questions. However, if a scientist who is doing competent research and whose work is esteemed by others, were to have his research funds canceled by the National Cancer Institute (NCI) because he expressed his views, he would have an extremely good case in the scientific community for action against the NCI and the SVCP program. Grant programs have contributed greatly to cancer research but have not yet found the solutions. As urgent as the cancer problem is, other approaches are necessary to obtain rapid answers; the contract system is one such means. Under such a system, there will be deficiencies and duplications of effort in certain areas. This also occurs with grants, but I feel there is more sharing of ideas and more cooperation among contractees than there is among grantees.

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Accelerator at Argonne

In her report "High energy physics . . ." (News and Comment, 3 Sept., p. 897), Deborah Shapley suggests that the 12.5-Gev zero gradient proton synchrotron (ZGS) at the Argonne National Laboratory could be in line to be shut down. The arguments leading to that conclusion are derived from inadequate information and place the ZGS in an unfavorable light.

The highly competitive research program at the ZGS operates with the strong participation of university-based research teams. From the point of view of the Argonne Universities Association, which represents 30 major universities and is responsible for formulating, reviewing, and approving Argonne's policies and programs, the program is an excellent example of successful university participation in research at Argonne. Some 400 high energy physicists, mainly from midwestern universities, are actively participating in the ZGS program. In 1970, 20 experiments were completed at the ZGS, and 60 scientific papers were published based on the data taken in these or in previous experiments at the ZGS.

A severe shortage of funds has resulted in a substantial decrease in the operating budget of the ZGS for the past 4 years. As a result, the pace of research has had to be slowed, despite continuous pressure from ZGS users for more accelerator time. A steady flow of new proposals for topical experiments is received by the ZGS Program Committee. The schedule of approved experiments is crowded, and an 18-month backlog now exists.

Shapley reports that the phrase "useful but older" was used to describe several accelerators, including the ZGS. This epithet misses the mark widely in the case of ZGS, which has been in operation for only 8 years. No other accelerator has exceeded the ZGS in the simultaneous operation of many experiments. Beam handling and detector facilities at the ZGS are modern, and every known elementary particle can be produced. An ingenious, new, and far-reaching program to improve internal beam intensity is now well under way. Shapley does not mention the development at the ZGS of the largest bubble chamber in the world—12 feet in diameter; the unmatched pioneering work in the application of superconductivity to high energy physics research; and the exceptional versatility and cost-effectiveness of the accelerator, which allow many experiments to provide data simultaneously.

The ZGS and its related research program are making unique contributions to physics at the present time and give every promise of doing so for many years.

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Shapley reviews the serious problems now facing the national high energy physics program and performs a service in pointing out the need for new, more comprehensive procedures to assess priorities in science funding. However, we believe the opinions expressed in her report do not accurately represent the current feeling among high energy physicists. Although the laboratory administrators interviewed by Shapley are highly respected individuals within the field, a more balanced sampling of the entire high energy physics community would have been desirable. Unfortunately, the opinions of the many university physicists who have a