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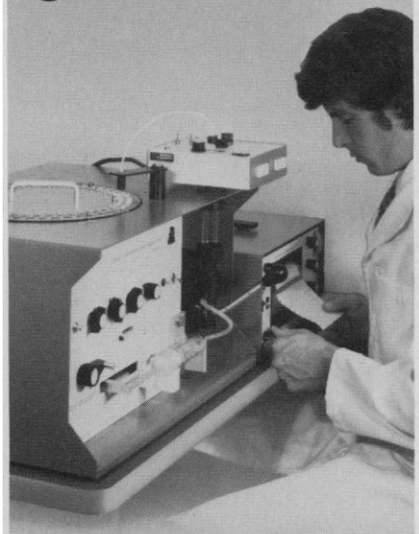

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West by certain circles interested in the frustration of the relaxation of international tension and in the revival of the cold war, and in seeking pretexts to defame—by any means—the noble aims and sincerity of the Soviet foreign policy that has gained unanimous gratitude and popularity throughout the world.

The decision of the Nobel committee to confer the peace prize on Sakharov—a decision that fundamentally contradicts the spirit and the letter of the basic provisions relating to this prize—is unacceptable to genuine champions of peace. Soviet scientists believe that the award of the Nobel prize to Sakharov is unworthy and provocative, and is a blasphemy against the noble ideas—dear to all of us—of humanism, peace, justice, and friendship among the peoples of all countries.

[Signed by] G. B. Abdullaev, G. A. Avsyuk, A. P. Aleksandrov, V. A. Ambartsumyan, M. S. Asimov, A. A. Baev, N. G. Basov, N. V. Belov, N. A. Borisevich, A. E. Braunshtein, A. P. Vanichev, I. N. Vekua, E. P. Velikhov, A. P. Vinogradov, S. I. Vol'fkovich, S. V. Vonsovskii, B. M. Vul, Ya. S. Grosul, N. P. Dubinin, N. M. Zhavoronkov, Yu. A. Zhdanov, A. A. Imshenetskii, A. Yu. Ishlinskii, A. P. Kapitsa, K. K. Karakeev, M. V. Keldysh, F. V. Konstantinov, V. A. Kotel'nikov, E. M. Kreps, A. M. Kunaev, G. V. Kurdyumov, A. L. Kursanov, M. A. Lavrent'ev, L. M. Leonov, A. A. Logunov, A. K. Malmeister, M. A. Markov, G. I. Marchuk, Yu. Yu. Matulis, N. V. Mel'nikov, I. I. Mints, E. N. Mishustin, A. N. Nesmeyanov, A. I. Oparin, B. E. Paton, B. N. Petrov, N. A. Pilyugin, B. B. Piotrovskii, P. N. Pospelov, A. M. Prokhorov, O. A. Reutov, A. M. Rumyantsev, K. M. Ryzhikov, B. A. Rybakov, A. S. Sadykov, N. N. Semenov, D. V. Skobel'tsyn, G. K. Skryabin, V. I. Smirnov, V. I. Spitsyn, V. D. Timakov, A. N. Tikhonov, A. A. Trofimuk, V. M. Tuchkevich, P. N. Fedoseev, N. P. Fedorenko, G. N. Flerov, A. V. Fokin, A. N. Frumkin, M. B. Khrapchenko, N. V. Tsitsin, V. A. Engel'gardt.

IRVING S. BENGELSDORF
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Notes

1. The nine academicians whose signatures appeared under the 1973 *Pravda* letter, but not under the 1975 *Izvestiya* letter, are N. N. Bogolyubov, B. M. Kedrov, A. M. Obukhov, Yu. A. Ovchinnikov, L. I. Sedov, S. L. Sobelev, I. M. Frank, Yu. B. Khariton, and P. A. Cherenkov.

Wood Versus Fossil Fuel for Excess Carbon Dioxide

Erik P. Eckholm's recent estimate, reported by Constance Holden (News and Comment, 3 Oct., p. 36), that "one-third of the world's population depends on wood for cooking (and, to a lesser extent, heating)" has interesting ramifications for detailed interpretations of the carbon dioxide buildup in the atmosphere in the past century. It has been estimated that half of the wood harvested each year is burned.

Lundell (*1*) has reviewed the box models of the carbon cycle proposed by Craig (*2*), Bolin (*3*), and others. Lundell has also calculated the boundary conditions for the shifting of exchange rates among the various carbon dioxide reservoirs. Wood-burning and deforestation have two additive effects. Wood-burning releases a large amount of carbon dioxide into the atmosphere, perhaps much more than has been previously estimated (for example, the estimate that 1.1 metric tons of wood are burned per capita per year in Thailand). Deforestation for lumber (and urbanization) has the additive effect of destroying the photosynthesizing organisms that transfer atmospheric carbon dioxide back into what we now propose as the "cellulose reservoir." The inflow and outflow into the cellulose reservoir during the last century is difficult to estimate, but a key and simple question is, Why hasn't photosynthesis prevented the 15 percent or so increase in atmospheric carbon dioxide in the last century? The current shortage of firewood suggests that part of the answer lies in a rapidly expanding human population burning cellulose much faster than it is being formed and held in living trees. Radiocarbon studies have documented the effects on the atmosphere of burning fossil fuel, but the wood-burning contribution to the atmospheric excess of carbon dioxide is more difficult to document because the cellulose reservoir has a radiocarbon/carbon ratio only a few percent different from that of the atmosphere.

It is possible that the biosphere could restore the cellulose reservoir in some decades, but only if the remaining parts of the reservoir (for example, the Amazon forest) are not depleted, if the population and per capita annual consumption are stabilized, and if, as Eckholm suggests, even more reforestation is undertaken.

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1. L. L. Lundell, thesis, Rice University (1973).
2. H. Craig, *Tellus* **9**, 1 (1957).
3. B. Bolin, *Sci. Am.* **233**, 124 (September 1970).

Sex Differentials in Academic Salaries

In Bayer and Astin's article (23 May, p. 796), the section dealing with salaries contains an error that vitiates a large part of their analysis. In August 1974 one of

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my students, Susan M. Speer, and I issued a press release about 1972–73 salary differentials (*1*) based on the same data as those used by Bayer and Astin—data from the faculty survey carried out by the American Council on Education. When we began our analysis we noticed immediately an unusual marginal distribution for the data on faculty salaries: implausibly large numbers of respondents were shown to have "current base institutional salary" on the order of $70,000, $80,000 or $90,000. On inspecting the questionnaire we found that respondents had been asked to round their salaries to the nearest $1000 by marking a number in each of two columns of numbers. We hypothesized that a significant proportion of respondents who received salaries of less than $10,000 had inadvertently rounded to the nearest $100. Inspection of the marginal distribution for salaries (*2*) showed that virtually no respondents reported salaries between $40,000 and $70,000 but approximately 1.5 percent reported salaries in excess of $70,000. This is a small proportion, yet these inaccurate responses deviate so extremely from central tendency that they can add substantially to the variance in salaries.

Accordingly, we eliminated from our analyses all respondents reporting base salaries of $50,000 or more. Some who received less than $5000 could have been coded as having salaries, say, just under $50,000, but since our analysis was confined to full-time faculty and few full-time faculty members made less than $5000 in 1972–73, we considered this risk acceptable. Before the respondents indicating salaries of $50,000 or more were eliminated the standard deviation for salaries was $10,601, an obviously anomalous datum to anybody familiar with faculty salary distributions and one that should have made Bayer and Astin aware that something was amiss. After those cases were eliminated, the standard deviation for males was $5769 and for females $4484.

In preparing this comment I decided to attempt a replication of Bayer and Astin's error. I ran a regression of females' salaries on several independent variables without using the $50,000 cutting point and obtained results comparable to Bayer and Astin's: the multiple correlation coefficient (R) turned out to be .38, slightly higher than Bayer and Astin's .29 primarily because of the introduction of a larger number of independent variables and perhaps because several dummy variables were calibrated differently. I know this result to be erroneous, because in our analyses of a year ago using the $50,000 cutting point we found that we could account for 66 percent of the variance in female salaries (R =

.82). This result is comparable to the one obtained from similar data in 1969, when R was .76 for females. It is therefore important to recognize the untenability of Bayer and Astin's speculation (*1*, p. 800) that their surprisingly small R for females

indicates substantially greater difficulty in predicting 1972–73 salaries of women than of men. The 1968–69 study showed higher multiple correlations and substantially greater similarity in the degree of predictability of men's and women's salaries (men, $R = .81$; women, $R = .76$). These shifts in results between 1968–69 and 1972–73 suggest that the traditional criteria used in the awarding of salaries may be in the process of being abandoned or reformulated, or at least are not being uniformly applied to women and men throughout the various sectors of academe.

Even on a strictly intuitive basis it seems inconceivable that the multiple correlation coefficient for females could have dropped from .76 to .29 in 4 years (*3*); such an erosion of a reward system obviously could occur only in a situation of virtual anarchy.

With the erroneous data eliminated, the multiple correlation coefficient for females appears to have increased slightly between 1969 and 1973 (.76 to .82), and that for males appears to have decreased slightly (.81 to .77). Such a change suggests as a sociological hypothesis that, in a conflict-laden environment where some factions raise questions about the legitimacy of reward processes, such processes tend to become more formula-dominated, particularly as applied to those who have raised questions of legitimacy. Administrators are increasingly pressed to justify decisions on females' salaries with reference precisely to those variables that Bayer, Astin, and I have introduced into our equations. I venture to predict that as we obtain more time-series data we shall find that "luck" becomes a progressively smaller factor in females' salaries, while such a trend may not exist at all, or to the same degree, for males. In any case, since the Bayer-Astin speculation about the "abandonment" or "reformulation" of salary determination processes for females implies that luck now plays a much larger role than formerly, it may create a dangerously erroneous impression in the minds of academicians who believe that the most effective manner of restoring legitimacy consists, in part, of leaving fewer matters to chance.

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References and Notes

1. See, for example, *Chron. Higher Educ.* **7**, 9 (5 August 1974); *Behav. Today* **5**, 210 (12 August 1974).
2. Findings cited in this comment are based on a 15 percent random sample from the American Council on Education file containing data on more than 50,000 faculty members.

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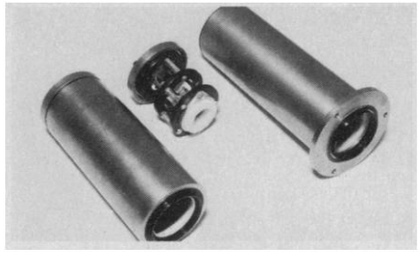

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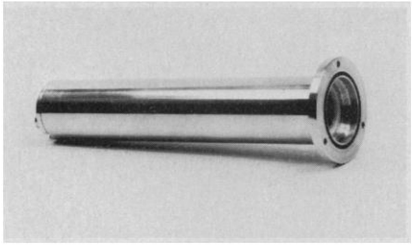

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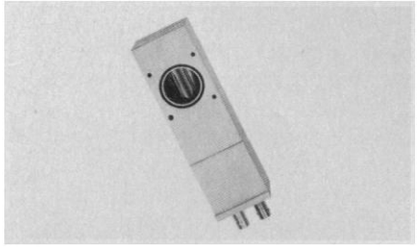

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3. Bayer and Astin's salary data for 1968–69 were based on a questionnaire item in which respondents were asked to indicate their institutional salaries by checking one of nine income categories ranging from "below $7,000" to "$30,000 and over." This item did not invite response error in the manner of the 1972–73 item.
4. The 1972–73 faculty data used as a basis for this comment were collected by the American Council on Education's former Office of Research under a grant from the Research Applied to National Needs (RANN) Division of the National Science Foundation. Access to the data was achieved through the Council's Division of Educational Statistics, Washington, D.C. I thank the Computer Center of the College of William and Mary for the use of data processing facilities.

Faia faults the article by Helen Astin and me for errors in procedure, results, and interpretation which are apparently his, not ours. He is, however, correct in that we did encounter some difficulty in our survey with a small number of the responses to the item requesting salary data from academic personnel. A few of the 53,000 respondents elected not to disclose their salaries in the survey, and 15 percent overlooked reporting whether their designated salaries were on an academic year (9 to 10 months) or a calendar year (11 to 12 months) basis. A trace, 409 individuals out of 53,000 respondents, or 0.77 percent, reported salaries in excess of $70,000, some of which were clearly spurious although others in this salary range can be presumed to be correct inasmuch as our sample included many chief administrative officers as well as eminent scholars. Faia's reported proportion of 1.5 percent above this salary level would appear to mistakenly include those who omitted the salary item.

The brevity required for our *Science* article precluded detailed description of the full procedure we employed to draw our subsample for the analyses. We were aware of the possible misreporting of salaries in the high ranges, however, and all salaries in excess of $40,000 were analyzed for their presumed consistency with the respondents' rank, degree level, length of service, publication productivity, or administrative responsibilities. In the subsample of 4998 cases used in our analysis, 12 men and 6 women who had incomes above this level were retained in the sample under the presumption that their incomes were reasonable, given their status and roles. All respondents who did not report their salary, or did not report the basis of their salary, were excluded from our subsample. The resulting subsample standard deviation on income was $6410, not Faia's erroneous $10,601 which apparently was derived by including coded omitted responses to the salary item (scored as "100" and thus possibly analyzed by him as $100,000 salaries).

We also did not interpret our findings with respect to the poor predictive results of salaries for women by adopting Christopher Jencks's "luck" explanation (*1*). Nor is a "situation of virtual anarchy" a necessary "only" feasible interpretation of such results, as Faia claims. An alternative explanation which we suggested is that a low multiple correlation coefficient could be obtained if some institutions had instituted broad corrective actions to adjust their women faculty members' salaries while others had done little or nothing by 1972–73 in response to recent antibias legislation.

It nevertheless remains perplexing as to why Faia's results with respect to one of the prediction equations would deviate so substantially from ours, particularly inasmuch as he has employed our data. Faia's attempt to replicate our earlier 1968–69 study with our 1972–73 data, collected by us for the same purpose, has until this time resulted in only an early press release, subsequently picked up by the semi-popular press, which he cites in his letter. Only if he reports his study in full in a refereed scholarly journal, where peer appraisal by the scientific community might take precedence over media publicity, might the discrepant results which he claims be more adequately understood.

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References

1. C. Jencks *et al.*, *Inequality: a Reassessment of the Effect of Family and Schooling in America* (Basic Books, New York, 1972).

Mixed Blessing

The *Science* cover of 12 September raises a curious coincidence. The caption for the distressing photograph of the dead chestnut reminds us that the tree's death was caused by the fungus *Endothia parasitica*, a species whose extracellular products enjoy the incongruous distinction of being the subject of a food additive regulation, under the Food, Drug, and Cosmetic Act. The federal regulation for fermentation-derived, milk-clotting enzymes [number 121.1199 (CFR 21)] provides for the use in cheese manufacture of the enzyme produced by pure culture fermentation of *E. parasitica*. Pathogenic for some trees, apparently, but beneficial to man.

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