

tions, we could achieve a balanced budget as this asking too much? I consider one of the primary goals of the society to continue to publish high-quality journals.

The future of JACS could also be put on solid financial footing in a number of other ways. One is obviously to streamline ACS operating costs and decide on priorities, of which JACS seems to have presently a very low one. Orville Chapman suggested that we could consider incorporating JACS on its own and see whether it loses money, breaks even, or even makes money. (It seems that some private publishers can even achieve this miracle.) An increase in subscription rates may be necessary, and within limits, I'm sure acceptable if the quality of the journal is maintained. There are questions, however, to the latter. There seems to be recent bias to turn increasingly to theoretical papers and de-emphasize experimental chemistry, such as synthetic organic chemistry, with a still unclarified view on Communications. Surely this can result only in a decrease of subscriptions. Also, regardless of the editor's intentions to the contrary, after obligatory page charges, I predict Communications will become the major part of JACS.

I don't consider, however, publication of the society's major journal entirely a financial venture. If we want to make only money, I suggested in the past that we should turn to other types of more lucrative "publications" with greater appeal than a chemistry journal. I feel, however, that the society has a mandate to continue JACS, as much as possible, in its present highly successful and proven form. Imposing obligatory page charges indiscriminately will cause additional problems.

Eliel emphasizes the hardship put on some when so-called, well-supported authors do not pay page charges in full. I think this comment is quite unfair. I challenge seriously that any author deliberately would avoid helping the journal if he has sufficient funds. Eliel, further, said nothing about the productivity of authors. There is a considerable difference in paying page charges if someone publishes one or two papers a year, or when a productive group publishes a larger number of papers. The other side of the coin would be a negative page charge to reward those members who do not publish at all.

I would urge that by combining all financial resources available, we keep the journal alive in its present form. What we need on the part of ACS is a more energetic and dynamic effort to solve the financial problems and generate new funds. We should try to convince agencies to finance publications which are published under their sponsorship directly, as the Petroleum Research Fund does, and ask industry to voluntarily support the journal more generously. If Internal Revenue Service regulation would prevent higher industrial subscription fees,

JACS, in my view, still probably is the best asset the society has, and I don't think we should light-heartedly experiment with it, after a century of proven high standards and generally even successful financial performance. If some support is needed, such as a dollar contribution of membership fees for publications, it is a small price to save the journal in the form it served the whole of American and world chemistry so well for a century. Even docile taxpayers can eventually revolt if their views are far too long neglected (remember Proposition 13 in California?) Membership fees should be used with

some consideration of the contributors. As signing 2% of our fees to publications surely can not upset the ACS budget! I realize not all members feel so strongly about JACS as some of us in academic life do, but at the same time, we are not asking too much.

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Computing cancer rates

SIR: M. R. V. Sahyun (C&EN, March 20, page 43) states that 13% to 21% are the upper limit values "representative of the real environmental hazard owing to substances which are the subject of the 'chemicals and cancer' debate and which are within the scope of our control." This conclusion derives from finding out by how much (in per cent) the observed crude death rates (for all cancers) for the U.S. in the years after 1960 depart from the straight line purporting to relate the "all cancer" death rates to expectation of life during the 1900-80 period. For 1974, this divergence is found to be +13% and for 1995 (which requires extrapolating the observed trend in death rates) it is reported as +21%. These two figures are then taken as the limit to the contribution of environmental hazards to cancers.

Quite apart from the appropriateness of the age adjustment method used and the use of death rates rather than incidence rates, the fact that "all cancer" rates do, or do not, change with time bears little relevance to the issue of clarifying the etiology of cancers. The overall trend in cancer death rate is the sum of the individual trends for each cancer (lung, stomach, acute leukemia, etc.) and for each population subgroup, defined according to sex, age, place of residence, occupation, etc. Such group-specific time trends vary considerably both in direction and magnitude and this is completely lost when the trends are combined. For example, in the U.S., from 1950 to 1970, the stomach cancer rates have been falling markedly in both sexes and in blacks and whites, whereas lung cancer rates have very markedly increased, but more in blacks than in whites, and within each ethnic group, more in males than in females.

Examination of group-specific time trends may provide useful pointers, but it is no solution to the problem of the identification of etiologic agents nor to the quantification of their role. Moreover, even a group-specific time trend may provide misleading suggestions. This difficulty can be illustrated by an example. Suppose that some factor (increased use of refrigeration, for example) has contributed to a decline in the incidence rates of some specific cancer (say liver) over the period observed; and that some other factor (the widespread introduction of a new industrial chemical) has contributed to an increase in the incidence rate of this cancer over the same period. In this case the net effect on liver cancer incidence might be either a rise, a fall, or no discernible change. From this, it should be clear that inspection of the time trend in death rate, even for a specific cancer, cannot provide information as to the proportion of such cancers which are attributable to a specific cause, or group of causes.

It is a biological truism (sometimes neglected) that for the development of any disease, including cancer, both genetic and environmental components are necessary. In this general

sense it is legitimate to say that as many as 100% of cancers are "environmentally related," the concrete challenge for environmental research being to find out what the relevant environmental components are. At this point, the key practical issue becomes (as the author of the letter rightly stresses) whether these environmental agents depend on "controllable human activities." It is known today that in developed countries, for a substantial fraction of human cancers (see e.g. Higginson, J. and Muir, C. S., *Cancer Detection and Prevention*, 1, 79, 1976; Schneiderman, M. A., "Persons at high risk of Cancer," Fraumeni, J., ed., Academic Press, 1976) the environmental causes are well identified. Cigarette smoking leads the way, followed by excessive consumption of alcoholic beverages, and by a number of industrial (asbestos, vinyl chloride, etc.) and medicinal exposures (arsenic, diethylstilbestrol, etc.). Detailed information on these is provided in the International Agency for Research on Cancer series of monographs "Carcinogenic Risk of Chemicals to Man," which also indicates that, in addition, there are more than 100 substances present to a varying extent in the human environment for which there is clear-cut evidence of animal carcinogenicity with no (or no adequate) epidemiological data available. In this situation, the immediate and relevant question is not an estimate of the proportion of cancers which may be "environmentally related" (whatever this elusive expression may mean) or which may be ultimately preventable, but: "What actions can be implemented now to control those human activities impeding the reduction or removal from the environment of agents well identified or strongly suspected as carcinogens for man?"

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Phosphorus in our environs

SIR: I have several concerns relating to your article on phosphorus in the April 24 issue, page 11. It contains several misleading opinions as to the potential impact of phosphorus on the environment which I want to address.

Phosphorus has been established as the limiting nutrient since 1971 for all of the Great Lakes and most of the inland lakes within the Great Lakes Basin. In the last several years farsighted legislators in Indiana, New York, Canada, and the city of Chicago have implemented total detergent bans. These bans demonstrated that phosphorus enrichment of lakes can be a reversible process. Effects of a particular source of phosphorus can be reversed when the source is removed. As a result, Michigan moved in October of 1977 to a complete ban after five years of a partial ban. Minnesota has enacted a ban but has been enjoined from enforcement. Wisconsin has recently passed a detergent phosphate ban. Ohio is considering a bill to ban phosphorus in detergents.

There is little doubt that emphasis on dissolved rather than total phosphorus inputs to lakes represents an extremely important distinction. This must be kept in mind by anyone measuring phosphorus inputs, identifying

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