

decrease in the dissolved phosphate supply than in that of nitrate, carbon dioxide, or alkalinity. The abundance of algae has decreased in close proportion to the concentration of phosphate.

The real question that should be debated is not what is the key element that regulates algal blooms, but what should we do to limit most effectively the input of concentrated sources of nutrients to lakes? Elimination of phosphorus-containing detergents would make a great difference in the amount and concentration of phosphorus entering lakes. While it would leave important sources of concentrated sewage phosphate available, detergents have made sewage very much more effective than it was formerly.

Ideally, all sewage entering lakes would be treated in such a way as to remove phosphorus in those large geographical regions where phosphorus is not naturally present in excess. But even if the technology were adequately developed, it would take a very long time to finance and build the facilities and they would be costly to operate. In the meantime, lakes would continue to deteriorate.

It was possible to divert the sewage from Lake Washington without creating a similar problem elsewhere, an option not open to many communities. Where such diversion is not possible, lake eutrophication problems will have to be solved by controlling the character of sewage effluent, either by treatment or by controlling what goes into the sewage. Even if control of detergents by itself is not enough to solve all eutrophication problems, it could be a very helpful part of a control program.

The decisions to be made must balance a complicated set of short- and long-term effects. If the disadvantages of the phosphate ban outweigh the advantages for the reasons outlined in Abelson's editorial, the matter should be decided on that basis, not by obscuring the real effectiveness of detergent phosphates.

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Reference

1. W. T. Edmondson, *Science* 169, 690 (1970).

... There are three specific reasons for attacking phosphates in detergents as part of an overall program of phosphorus control. In the first place it would achieve a more rapid removal of

50 to 70 percent of the phosphorus now present in sewage effluents than would be possible by any other means. (As Congressman Reuss put it, it is easier to do something about three major detergent manufacturers than about the 200 million manufacturers of phosphates in physiological wastes.) Second, there would be a substantial and *permanent* reduction of overall treatment costs for phosphate removal at sewage treatment plants (an estimated saving of \$22 million per year calculated on the basis of alum treatment in the basins of lakes Erie and Ontario alone). Third, it would eliminate 50 to 70 percent of the phosphate generated in isolated dwellings and small communities, situations in which any other kind of control would be difficult if not impossible to achieve.

Abelson says that the elimination of phosphates from detergents would not "solve" the eutrophication problem. I agree, but there is no factual basis for anyone suggesting that it would not help. In a like manner, I doubt that the goal of 80 percent removal of phosphates at sewage treatment plants without detergent control would "solve" the eutrophication problem; nevertheless, it also would help. The effective cure for cultural eutrophication requires the combined removal of phosphates from detergents and as much of the remainder in municipal sewage as it is technologically feasible to remove. If we call these three solutions A, B, and C, the effect would be to reduce a city of one million inhabitants to about 400,000, 200,000 and less than 50,000, respectively, in terms of the phosphate generated. In areas with animal feedlots or intensive farming, additional controls, in part educational, may be required.

By reciting some of the unusual limnological claims made in the article in *Canadian Research & Development* Abelson more confused than advanced learning. A half century of experience with a large number of waters throughout the world has shown that carbon rarely limits the overall extent of plant growth in the aquatic environment. In saying that massive algal blooms have occurred in lakes containing very little phosphate one should realize that, like the empty dish after a good meal, lakes are low in dissolved phosphate when algae bloom because the phosphate is in the algae. Also, when the concentration is low it is the rate of supply that counts. With a continued influx of phosphorus and nitrogen compounds

from human wastes an algal bloom will be perpetuated; but with phosphates removed this would in most instances no longer be the case.

In terms of the phosphate question there is merit in treating drainage basins separately as Abelson suggests; but the overall policy in regard to detergent phosphates may be more dictated by industry and the ease of governmental control. A similar question arose 6 years ago in relation to non-biodegradable surfactants in detergents. The solution adopted by industry was international.

Finally, problems of eutrophication have been increasing exponentially in recent decades. Although we do not have exact information on how many waters may enter a critical phase during the next 5 or 20 years, we do know that with continued growth of human populations and associated technology we cannot extrapolate linearly from the past and present. If steps are not taken soon to alleviate the problem, we may find ourselves by the year 2000 in the middle of an algal bowl, with effects on water use comparable to those of the dust bowl on land use in the first third of the century.

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Molecular Biology:

Peroration or Obituary

Although my wife has often pointed out that my speeches might improve if they were set to music, I have yet to act on her suggestion. Hence, I was rather puzzled at first by C. G. Kurland's opinion ("Ribosome structure and function emergent," 18 Sept., p. 1171) that the "peroration that Stent [*Science* 160, 390 (1968)] has recently intoned for molecular biology may seem somewhat premature." Kurland, I thought, must be confusing my article with Joel Herskowitz's "Double talking helix blues" (recording issued by Vertebral Disk Co., Chicago, Ill.), although I wondered why he should find that long-overdue art form "premature." But on reading further his remarks concerning my views I realized that Kurland probably meant that I delivered a premature *obituary* for molecular biology.

Well, now that I learn how Kurland's

studies of ribosome structure have turned up deep epistemological and cosmological problems—such as (i) a “function emergent” of ribosomes (thus freeing us at last from pedestrian Watson-Crick reductionism, which “involves a deep faith in the overworked hydrogen bond and a considerable amount of hand waving,” and allowing molecular biology to become airborne, Teilhard-bound); (ii) the possibility that protein synthesis might not even obey “the first and second laws of thermodynamics (or their statistical analogs)”; and (iii) the even more revolutionary notion that the “physical laws governing the domain of molecular genetics” might not even possess the quality of “uniqueness”—I must admit that 3 years ago molecular biology could not have been all that dead. But now, when Kurland is at last able to “describe current work on the ribosomal proteins, work which may eventually provide the key to the mechanism of protein synthesis,” I fear that time is running out once more. No, hold it! If protein synthesis does not obey the second law, then time cannot run out, and molecular biology marches on forever.

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The Drinking Driver

I strongly protest the decidedly unscientific viewpoint reflected in the editorial “Agnew, alcohol, automobiles, and assessment” (28 Aug., p. 819) by Robert S. Morison, and I am disappointed that *Science* failed to detect the editorial for what it is—a sweeping generalization.

Morison failed to recognize the Department of Transportation’s new alcohol safety countermeasures program, a realistic effort to restrict the driving privileges of alcoholics who demonstrably constitute the actual menace to highway safety. Normal social drinking is not a major cause of highway accidents.

Moreover, the writer of the editorial apparently was unfamiliar with authoritative reports by the DOT on the subject of Highway Safety; by HEW on the subject of alcohol use and alcoholism; with the 5-year study completed by Rutgers University’s Center of Alcohol Studies; and with the published research of independent investigators

such as Ira H. Cisin, Julian Waller, and Selden Bacon (1–8).

Further, the editorial failed to reflect any knowledge of medical and scientific findings that alcoholism is an illness. Alcohol abuse is known to be symptomatic of a deeper underlying disorder. The distilling industry has never condoned excessive drinking and has long supported basic research into the causation and possible methods of treatment of alcoholism.

Also, I am somewhat dismayed that you would permit an apparent political attack on the Vice President of the United States. There are other media available to Morison where he can pursue his point of view without subjecting the scientific community to an unnecessary aggravation.

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References

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2. R. A. McFarland, *Police* 10, 66 (March–April 1966).
3. ———, *Traffic Safety Res. Rev.* 12, 71 (1968).
4. *On the Level: Traffic Safety Monog. No. 1* (National Safety Council, Chicago, Ill., 1969).
5. *Report of the Secretary’s Advisory Committee on Traffic Safety*, Dept. of Health, Education and Welfare (Government Printing Office, Washington, D.C., 1968).
6. J. A. Waller, in *The Prevention of Highway Injury: Proceedings of a Symposium 19–21 April 1967* (Highway Safety Research Institute, Univ. of Michigan Press, Ann Arbor, 1967), pp. 30–37.
7. J. A. Waller, *Quart. J. Stud. Alc.*, Suppl. No. 4, 118 (1968).
8. *Alcohol and Highway Safety Report*, Dept. of Transportation (Government Printing Office, Washington, D.C., 1968).

I am sorry that I did not know of the Department of Transportation’s new alcohol safety countermeasures program at the time I wrote the editorial. It may be worth more extensive publicity than it has received.

Careful examination of five of the eight references mentioned by Packowski (the others were not immediately obtainable) does not reveal anything that seems in conflict with my editorial. All of them stress the very high correlation between drinking and serious automobile accidents and recommend various courses of action to reduce the driving of automobiles by persons with blood alcohol above stated minimums.

In the short space available, I did not think it necessary to discuss the etiology and pathogenesis of alcoholism. Indeed, I assumed that most readers would already know that problem drinking is widely regarded as an ill-

ness. My purpose was to direct attention to the evidence that methods have been found to reduce some of the most serious effects of the disease both on the sufferer himself and the public generally. Rereading of the editorial reveals nothing that suggests that the distilling industry ever “condoned excessive drinking.”

In a year in which so much attention has been directed to the importance of science and scientists taking more responsibility for the secondary and tertiary effects of advanced technology, I find nothing inappropriate in an article which calls attention to the difficulties such scientists will encounter if the political leadership of the country persists in basing its recommendations on conventional wisdom rather than on scientific evidence.

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Vietnam Defoliation Saves Lives

The article “Ecological effects of the war in Vietnam” (1 May, p. 544) and the letter (11 Sept.), both by Orians and Pfeiffer, do not constitute good science. Not even bad science! A more revealing position can be found in the report to the Subcommittee on Science, Research, and Development of the House Committee on Science and Astronautics dated 8 August 1969 and entitled “A technology assessment of the Vietnam defoliant matter—a case history” (p. 60):

In the press interview in New York upon his return, Professor Pfeiffer expanded somewhat on this report. He observed that it was “completely unrealistic” to expect military commanders to abstain from defoliation actions. “There is no question about it,” he said. “They save American lives.” On a 65-mile journey by armed boat from Saigon to the sea, he said, “We scarcely saw a living plant.” However, he added that had the vegetation not been destroyed, he and his companion would probably not have returned alive.

When it comes right down to it, given a choice between the life of a tree and the life of an American soldier, we must choose in favor of the life of an American soldier. . . .

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