CORRESPONDENCE

Tolerable Pollution

SIR,—I have read with great interest Dr Samuel S. Epstein's article entitled "Control of Chemical Pollutants" (*Nature*, **228**, 816; 1970). Many of the points made by Dr Epstein are very well taken and need emphasis. Unfortunately, however, some of the concepts that he expresses need to be examined more closely and carefully thought through.

For instance, in the determination of "efficacy" the practising physician may have entirely different concepts than the academician who is not dealing with patients on a daily basis. To the academician there is probably no place for a placebo. To the harassed physician trying to bring solace to the whining, complaining patient the placebo may have a most important, though limited, role. I remember clearly a female patient I once had of 70+ years who demanded of me a "tonic". "Tonics," it should be remembered, were practically a part of the culture of people in this age range. Finding no "tonics" in the formulary of the hospital where I was working, I prescribed an innocuous "bladder mixture" made up of potassium iodide and hyoscyamus. Much to my surprise, the patient returned at her next visit lauding the "efficacy" of this "tonic" to the skies and insisted that it be renewed. Obviously this was not a material that I should be permitted to market as "Dr E's famous tonic", but was there any harm in its use in these circumstances? Who, then, is to determine efficacy? Should flavours, colours, antirancidity materials or other such materials be permitted in foods? Do they have "efficacy"? Some might well argue that foods should be eaten only with their natural inherent flavours and colours and that the addition of artificial (or even natural) flavours or colours does not improve the food's efficacy. Rancid food should simply be thrown out. What about the present uproar over the use of tolbutamide in diabetics? Even cyclamates are thought by many physicians to have value in the treatment of obesity and diabetes. Who is to make these important determinations?

Turning to the concepts of carcinogenicity, teratogenicity, and mutagenicity, there are few people who do not believe that we should control them to the best of our ability. However, as I pointed out in my Ramazzini oration (*J. Occ. Med.*, **13**, 161; 1971), the concept of zero is a mathematical one. As our analytic techniques become more and more sensitive, it will ultimately be necessary for us to establish a "practical" zero. In essence this will mean that some finite, measurable quantity will be determined to be "tolerable". Thus a "tolerance" for carcinogens, teratogens, and mutagens will be established. Precedence for this approach has been established in the milk and water sanitation fields. Here a definite number of organisms are allowed, that is, are considered tolerable. It was recognized that to set the levels at zero organisms could well have eliminated these essentials from human consumption. Thus recognizing the impracticality of the so-called Delaney clause and devoting research efforts to a serious attempt to establish tolerances for carcinogens seems a more sensible approach than simply extending this concept to teratogens and mutagens. This approach has been taken by Truhaut and an international committee in attempting to establish a tolerance for aflatoxins in foods.

Lastly, the concept of "disinterested" scientists needs carefully to be examined. What, truly, do we mean by a "disinterested" scientist? Is he "disinterested" because he is an academician? After all, Pasteur made what is probably one of the most important discoveries to mankind when he was working for the French wine industry. Is the scientist directly engaged in or dependent for support from a federal bureaucracy "disinterested" in its perpetuation? Can a scientist's views be modified as he lives longer and becomes more mature and knowledgeable? Is this change in viewpoint due to his having lost his disinterested status?

The concepts I expressed in my editorial on environmental carcinogenesis (*Cancer Res.*, **22**, 395; 1962) seem today to need re-emphasis. We do need to control *significant* exposures of the general population to carcinogens, teratogens, and mutagens. After all, today we do not have any real comprehension of what the teratogenic or mutagenic rates are in the human race as a whole. For all we know, these rates may be on the downswing now because of man's ability to overcome some of the vicissitudes of his former existence. A rereading of the "Report on the Sanitary Condition of the Labouring Population of Great Britain" by Edwin Chadwick, 1842 (edit. by M. W. Flinn, Edinburgh University Press, 1965) should reassure us on the real progress we have made in just a little over 100 years. We still, nonetheless, live in a real world, fraught with real and imminent dangers which we must do our best to minimize in a real and practical way.

Yours faithfully,

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Obituary

Wendell Meredith Stanley

WENDELL MEREDITH STANLEY, Professor of Molecular Biology and of Biochemistry at the University of California, Berkeley, died suddenly, aged 66, on June 15 in Spain.

His career in biochemistry and molecular biology began at the University of Illinois in 1926 when, as a graduate student of the organic chemist, Roger Adams, he synthesized a series of fatty acids and correlated their structures and physical properties with effectiveness against the leprosy bacillus. After earning his PhD degree in 1929, Stanley went to Germany as a National Research Council Fellow in the laboratory of Professor Heinrich Wieland, where he worked on sterols of yeast. Returning to the United States in 1931, he worked for a year at the Rockefeller Institute for Medical Research in New York with cell physiologist W. J. V. Osterhout, on model chemical systems that simulated cell membranes.

In 1932, Stanley moved to the Rockefeller Institute Laboratories in Princeton, New Jersey. There he began his studies on the chemistry of viruses which culminated in 1935 with the crystallization of tobacco mosaic virus (TMV), the first of these disease agents to be obtained in such a form. Not content with the isolation and crystallization of TMV, he proceeded with a small group of colleagues to develop systematically and imaginatively an entirely new field of science: the biology, chemistry, and physics of viruses. From these efforts, and work elsewhere, the chemical composition and morphology of a variety of viruses were defined in molecular terms. Stanley's initial studies on the chemical modification of viruses later provided the foundation for inactivating viruses for vaccine production and for studies on molecular