Global Health Fund and Global Realities

I READ WITH INTEREST THE PROFILE OF Jeffrey Sach's efforts at organizing a global health fund with the purpose of combating malaria, tuberculosis, and HIV in the developing world (News Focus, "Dollars and cents vs. the AIDS epidemic," by G. Vogel, 29 Jun., p. 2420). Such a fund could undoubtedly save lives—but only if the programs that the fund will underwrite are carefully designed and implemented. Many of the countries hardest hit by the targeted diseases are in the throes of profound political and social instability, instability that could easily derail any attempts

The global health fund will provide developing countries with support for public health efforts.

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at distributing medicines and other medical technology. Determining ways to prevent theft, waste, and maldistribution of the fund's aid is of primary importance. During the Somalia famine, food aid was used as a political weapon and as an opportunity for profit by unscrupulous warlords; it is imperative that the same does not happen to the medical aid that the global health fund will provide, as this would cripple the fund's credibility and political support. I have witnessed waste and mismanagement of medical resources firsthand in my 12 years' experience with providing vaccines and medical technology to Eastern Europe and the Middle East.

Careful oversight of the fund's expenditures would be best undertaken by an experienced charity organization such as AmeriCares, with close coordination of its activities with the United Nations. This would eliminate the need to build an organizational structure from the ground up. Even these organizations, however, would be limited in their ability to affect the political realties in many countries, realities that must be addressed in the design of the fund's projects if they are to be successful.

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Science Lobbying Tactics

AS A SCIENTIST, I WOULD NOT CHOOSE TO highlight the blocking of the new U.S. Department of Agriculture (USDA) regulations regarding the care of laboratory mice and rats as a prime example of successful pro-science lobbying (News Focus, "Perfecting the art of the science deal," by D. Malakoff, 4 May, p. 830). That this "success" was achieved by what can only be described as backroom tactics is a minor issue. The deplorable aspect of this ostensible victory for biomedical research is that it is at the same time a resounding defeat for logical consistency, and hence for the rationality that forms the very basis of all scientific enquiry. If someone were to argue that mice, rats, and birds are not animals in the sense of the USDA rules regarding the use of experimental animals, he or she would be laughed at by any thinking member of the general public—and with good reason. This pyrrhic victory may well backfire by reinforcing the image of scientists as a bunch of self-serving sophists, thus ultimately contributing to the rising tide of antiscientific sentiment.

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Letters to the Editor

Letters (~300 words) discuss material published in *Science* in the previous 6 months or issues of general interest. They can be submitted by e-mail (science_letters@aaas.org), the Web (www.letter2science.org), or regular mail (1200 New York Ave., NW, Washington, DC 20005, USA). Letters are not acknowledged upon receipt, nor are authors generally consulted before publication. Whether published in full or in part, letters are subject to editing for clarity and space.

Dietary Fat: At the Heart of the Matter

THE ROLE OF DIETARY FAT IN THE CAUSATION of coronary heart disease (CHD) has long been a topic of interest and dispute. In his News Focus article, Gary Taubes discusses what he calls "The soft science of dietary fat" (30 Mar., p. 2536). He reviews the history of the diet-heart issue and concludes that public health recommendations regarding dietary fat have not been based on solid science. He is primarily critical of the "low-fat" recommendation that has long been made by authoritative bodies to the American public. Taubes covers many aspects of the diet-heart issue, but he focuses on the question of whether there has been an overemphasis on fat without sufficient evidence that dietary fat is a major cause of CHD. He points out that recent trends in heart disease mortality both in the United States and worldwide are not well correlated with changes in dietary fat intake. Certainly he makes several astute observations, but in some areas, particularly in cardiovascular epidemiology, he does not appropriately recognize several other factors that confound the role of certain dietary fats in causation of CHD.

In my view, Taubes does not rightly identify saturated fatty acids as the predominant dietary factor contributing to the development of CHD. The significance of saturated fatty acids has been demonstrated by an enormous number of high-quality studies carried out with dietary fat in the fields of animal research, epidemiology, metabolism, and clinical trials (*1*). Although all questions have not been answered, a clear picture of the metabolic and health effects of saturated fatty acids has emerged. One fact is incontrovertible. As shown in multiple metabolic studies in humans, saturated fatty acids as a class, compared with unsaturated fatty acids and carbohydrate, raise serum low-density

lipoprotein (LDL). Evidence is abundant that elevated LDL is a major cause of CHD and that lowering serum LDL levels reduces CHD risk (*2*). Even moderate reductions in LDL levels, such as those obtained by reducing dietary saturated fatty acids, are projected to substantially reduce risk of CHD in populations (*3*). Early prospective epidemiological studies gave results that are consistent with these projections (*4*). For example, in Northern and Eastern Europe, where intake of animal fats (mostly saturated fatty acids) previously was very high, serum LDL levels and CHD rates also were high. Conversely, in Southern Europe, where plant oils (mostly unsaturated fatty acids) are the predominant fat source, serum LDL levels and CHD rates were much lower. These relations were established more than 30 years ago, before increasing social and cultural homogenization in Europe partially obscured the relation of dietary fat to CHD (*4*). These population results, which in themselves were suggestive although perhaps not definitive, have been confirmed by results of controlled clinical trials. Several trials reveal that substitution of unsaturated fatty acids for saturated fatty acids lowers the incidence of CHD (*1*).

Although Taubes acknowledges the difference between saturated and unsaturated fatty acids, he does not draw a clear enough distinction in his discussion of dietary fats in general. Consequently, the article obscures the potential for public health benefits of substituting unsaturated for saturated fatty acids in the American diet. Such confusion does a disservice to the public health effort to further reduce the incidence of CHD through a reduction in intake of saturated fatty acids. On the other hand, Taubes does rightly note that other nutritional factors, for example energy imbalance leading to obesity, excessive carbohydrates, and insufficient intake of fruits and vegetables also influence population risk for CHD (*1, 2*).

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THE CASUAL READER OF TAUBES' ARTICLE might conclude that the only recommendation made by Senator George McGovern's committee in its Dietary Goals for the United States or in the Dietary Guidelines for Americans was to reduce dietary fat. We recommended reduced consumption of cholesterol, salt, and sugar but increased consumption of unsaturated fat, whole cereal grains, and fruits and vegetables. One could not recommend increasing intake of anything without reducing intake of something else. Saturated fat was, and is, the best thing to reduce. The recent report of the Nutrition Committee of the American Heart Association (*1*) has correctly emphasized "diet as a whole" rather than "segmented guidance on individual dietary components." In retrospect, we should have said, "eat less sugar and refined cereals." Nevertheless, one version of the total diet recommended would approximate the so-called Mediterranean diet.

"...the article obscures the potential for public health benefits of substituting unsaturated for saturated fatty acids in the American diet."

Taubes raises concerns about possible adverse effects of a low-fat diet, particularly upon brain function. However, 30% of calories as fat can scarcely be labeled a low-fat diet. One does not note mental incompetence in the Japanese, Chinese, and other populations who do, or did, consume a really low-fat diet. Such populations also demonstrate that low-fat diets do not necessarily cause obesity.

Ornish *et al.* (*2, 3*) have demonstrated that very low-fat diets combined with exercise actually reverse atherosclerosis. Although this is important in demonstrating the role of diet, there is little point in recommending such severe diets that few will follow.

Taubes calls a 10% lowering of cholesterol "trivial." That may be true for high-risk individuals, but not for a population. A modest reduction in risk may accomplish little for an individual, but a 10 or 20% reduction in risk represents a substantial gain for the population.

Taubes also says that Americans now consume 34% of calories as fat. A major problem is that we are uncertain exactly what Americans eat. It is clear that all methods of estimating food intake are grossly inaccurate and biased. People generally underestimate their energy intake,

and people who know, or think they know, what they should eat are likely to shade their reported intake toward what they consider desirable. Apparently, Americans have changed their diet little. Nevertheless, data from the Continuing Survey of Food Intake by Individuals by the U.S. Department of Agriculture indicate modest benefits by those who report eating lower fat diets and those who report consumption that approximates the Food Guide Pyramid (*4*). The inadequacies of food intake data also compromise the conclusions of studies like the Nurses' Health Study and the Women's Health Initiative.

> **"The article doesn't obscure the benefits; it simply questions whether they are sufficiently large for healthy individuals who eat reasonable diets...to be concerned."**

One expects research to modify dietary recommendations. In the last 25 years, we have learned more about the n-3 fatty acids, the glycemic index, and antioxidants, and there are additional data indicating the benefits of a high level of consumption of fruits and vegetables. The recent recommendations from the American Heart Association (*5*) include appropriate modifications. A reduction in saturated fat, however, remains at the core of an appropriate nutrition policy.

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IN HIS ARTICLE, TAUBES FOCUSES ON scientists who don't believe that dietary fat plays any important role for obesity and cardiovascular disease. The crucial evidence linking fat to obesity and coronary heart disease (CHD) is not discussed. The following are some examples.

The phenomenon that Americans have reduced dietary fat content slightly but are getting fatter is taken as evidence that it is easier to gain weight on low-fat, high-carbohydrate diets than on higher fat diets. What Taubes does not mention are the meta-analyses of intervention studies comparing ad libitum intakes of higher fat diets with low-fat diets that clearly show reduced caloric intake and weight loss on the low-fat diets (*1*). In addition, it is well known that people underreport their energy and especially fat intake, which makes the observed fat intake reduction questionable. The obesity epidemic in the United States and other countries is predominantly due to an inactive life-style, which reduces the metabolic demand for fat as fuel.

In his discussion about the importance of dietary fat and CHD, Taubes focuses on the effect of fat on fasting blood lipids, but low-fat diets with plenty of fruit, vegetables, and fish predominantly exert their cardioprotective effect through other mediators such as blood pressure, thrombotic, fibrinolytic, and arrhythmic factors. Accordingly, an increase in dietary fat of 1% of energy intake is associated with an 8% increase in CHD (*2*). A Mediterranean, fat-reduced diet includes plenty of fruits, vegetables, and fish, and such a diet has been shown to reduce mortality by 45 to 60% in individuals with CHD (*3*).

The message in the article is misleading and counterproductive for public health policy to reduce dietary fat content and increase the consumption of fruit, vegetables, and fish, and to increase physical activity, advice based on robust scientific evidence.

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Response

GRUNDY SAYS THAT MY ARTICLE OBSCURES the potential for public health benefits of substituting unsaturated fatty acids for saturated fatty acids in the American diet. The article doesn't obscure the benefits; it simply questions whether they are sufficiently large for healthy individuals who

eat reasonable diets (that is, sufficient fruits and vegetables) to be concerned. It also questions whether all Americans will benefit from a low-fat diet, or whether for some, at least, there are risks involved as well, which is still an unanswered question. Indeed, three decades of low-fat diet recommendations has led many Americans to replace saturated fats with carbohydrates, not unsaturated fats. Certainly, the food industry has responded by creating low-fat and no-fat products that do just that. Moreover, there is suggestive but not definitive evidence both for and against the benefits of a low-fat diet, and clinical trials have both succeeded and failed in confirming the benefits.

Grundy discusses the trials that show a positive benefit, implying that these are the important ones, which makes for a compelling argument but does so at the expense of good science. Hegsted and Astrup *et al.* also pay attention only to those data that support the benefits of low-fat diets and ignore or reject as irrelevant or flawed all the copious evidence to the contrary. Astrup *et al.* for example cite a case-control study with 108 patients (and 142 controls) as unequivocal evidence that increased dietary fat intake is associated with increased heart disease rates, although such case-control studies are virtually meaningless. They say that meta-analyses demonstrate that low-fat diets are efficient weight loss diets, yet there are trials and even meta-analyses—a controversial tool, in any event—that suggest the opposite. They say that the obesity epidemic in America and elsewhere is "predominantly due to an inactive life-style," which is a reasonable hypothesis but unproven. There remains the complicated question of why individuals would continue to consume more calories than they expend, despite enormous social pressure against obesity.

Hegsted suggests that small individual risk reductions represent substantial gains for a population, but individuals are not populations. And deaths are not prevented, as I pointed out in the article, they are only delayed. Low-fat diets might indeed delay them, but if they do, the effect is marginal. The key point, as Grundy says, is that "all questions have not been answered,"and so we might not want to act as though they have been.

—GARY TAUBES

CORRECTIONS AND CLARIFICATION

Reports: "A β_2 adrenergic receptor signaling complex assembled with the Ca^{2+} channel $Ca_v1.2$" by M. A. Davare *et al.* (6 Jul., p. 98). First, in the list of authors, two present addresses were indicated for Johannes W. Hell, the correct one being the Department of Pharmacology, University of Iowa, Iowa City, IA 52242, USA. Second, the designations for the open and closed circles in figure 2J were reversed. The legend should have read as follows: "(**J**) Current amplitudes measured from ensemble averages with albuterol either applied to the bath (n = 10; closed circles) or by pipette backfilling (n = 10; open circles)...." And third, panel D of figure 2 was incorrectly reproduced. The correct panel D appears here.