# Letters

## Randomized Response Technique

The proposal to use the randomized response technique to elicit honest answers about sexual behavior (Research News, 24 April, p. 382) has several flaws.

1) If a respondent were very anxious to hide any hint of unacceptable behavior, he might respond "no" even when the coin toss resulted in heads simply because the "no" response involves no risk of detection. Randomized response may make it easier for the respondent to admit to unacceptable behavior, but it does not guarantee honesty.

2) The number of cases required to produce estimates having a specified level of reliability with the use of the randomized response technique would be much larger than the number required in surveys using conventional methods because the randomization procedure substantially increases sampling error.

3) In any survey intended to estimate the prevalence of behaviors that affect the risk of AIDS, it would be necessary to analyze the resulting data by a variety of social and demographic characteristics in order to identify high- and low-risk groups. For example, we would want to be able to estimate the use of prostitutes by men classified by age and marital status. In order to make reliable estimates for each cell in such a table, the number of cases in each cell would have to be much larger than the number required in a conventional survey in which responses can be associated directly with respondents having specified characteristics. This further multiplies the number of cases required.

4) Any satisfactory analysis of an AIDS-risk survey would require the cross-classification of two or more risk factors, derived from responses to two or more questions. If the responses to these questions were obtained by the randomized response technique, the cell size would have to be huge. For example, if one question asked a man whether he had had a sexual relationship with another man, we would surely want to know whether any prophylactic measures had been taken. Imagine that both questions are asked in the randomized response mode and further that the analyst wishes to classify the respondents by age and marital status. It is easy to see that the whole enterprise would become unmanageable.

There are alternatives to the randomized response technique that may overcome respondents' reluctance to provide truthful answers to sensitive questions. For example, the use of a self-administered questionnaire in a conventional interview survey may help. A technique that reduces the embarrassment in providing honest answers to sensitive questions is to hand the respondent a card showing response categories, each associated with a specific letter. This enables the respondent to respond to the question with a letter ("A," for example), rather than an explicit description of the behavior in question. This method has often been used to obtain answers to questions about methods of contraception. Another possibility is conducting the interview in a clinical setting, free from familiar distractions, in which the respondent may feel the urgent need for honest answers. In any effort to estimate the prevalence of high-risk behaviors, it will be necessary to impress potential respondents with the protection of confidentiality and the need for honesty. The AIDS epidemic has raised the need for accurate information on patterns of sexual behavior to the level of a national priority, and every effort must be made to assure results that are accurate and reliable and that can be analyzed in an appropriate and useful manner.

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## Risk Perception

The recent risk assessment issue (17 Apr.) contained a significant article by Paul Slovic on "Perception of risk" (p. 280). One point that he touched on deserves strong underlining. Slovic says "Risk concerns may provide a rationale for actions taken on other grounds. . . . [h]idden agendas need to be brought to the surface for discussion." This is especially true when one considers the media treatment of risk.

We in the scientific community must recognize the central role played by risk perception in the selection of television news broadcasts. The barely hidden agenda of every network or station is to sell advertising time, and the price is keyed to the ratings. Consequently the most important parameter in deciding what to cover and what to omit is the television channel's perception of what will boost the ratings.

Slovic's figure 1 is a scatter diagram of perception of familiar risks by the public, according to the parameters "unknown risk" and "dread risk." The upper right quadrant (representing "highly unknown" risk and "highly dreadful" risk) is a perfect menu for obtaining high television ratings. The public easily gets all worked up over the issues falling in that quadrant. Television coverage tends to emphasize both the unknown and the dread factors, thus pushing perception of risk further up. Several of the "risky" things in the upper right quadrant (notably nuclear power) seem to be there simply because television put them there.

It is fair to ask what things have been kept at low perception of risk by the influence of television. Smoking, alcohol, Valium, and several others in the low-dread category are historically associated with heavy television advertising budgets.

Scientific professionals need to be more active in drawing the attention of the public to the distortions of risk inherent in what passes for "news" on television.

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## Cancer Control

Barbara J. Culliton (News & Comment, 24 Apr., p. 380) quotes Vincent T. DeVita, Jr., as saying that I have "departed with reality" because I do not agree with his rosy assessment of recent progress against cancer. While one might ask whether any reality is left at the National Cancer Institute (NCI) if I "departed with" it, the remark seems to disparage my views and discourage healthy debate. Culliton's article focuses on the defensive reaction of cancer officials to a recent General Accounting Office (GAO) report (*1*), but does not really explain the positions of GAO and others (*2*) who disagree with extravagant claims of progress.

"Reality" includes the following facts, all taken from a recent publication of the NCI itself (*3*), except as noted:

- The age-adjusted U.S. cancer *mortality* rate (1970 standard) rose from 162.2 per 100,000 population in 1975 to 170.7 in 1984. The preliminary cancer death rate for 1985 (from the National Center for Health Statistics) is nearly identical to the 1984 figure. I see no reason to omit lung cancer, but NCI does; the death rate for non-lung cancer was 125.4 per 100,000 in 1975 and 125.1 in 1984. Not much progress there, even by NCI's reckoning.
- The age-adjusted cancer *incidence* rate (1970 standard) for the Surveillance, Epidemiology, and End-Result registry area, our closest thing to national cancer incidence data, was 330.5 per 100,000 in 1975 and 351.8 in 1984. There is again no reason to omit lung cancer, but if we follow NCI, the incidence rate for non-lung cancer rose from 285.3 per 100,000 in 1975 to 296.5 in

1984. No progress there, either.

■ The 5-year *relative case survival rate* ("relative" means adjusted for mortality in the general population) was 48.6% in 1974–1976 and 48.7% in 1977–1983. Rates for single years of diagnosis show no essential change since 1975.

■ These figures are as up-to-date as anything one could reasonably expect. Most cancer deaths occur within 2 years of diagnosis (although breast cancer is a notable exception). Had there been any substantial change by (say) mid-1983, it should be apparent in the 1985 rates. It is not there.

■ NCI points, correctly, to progress at younger ages, but does not seem to understand that large percentage improvements in the uncommon cancers at ages from infancy through the early middle years do not offset smaller percentage increases in the much more common cancers at older ages. From 1975 to 1984, the death rate for persons under 20 went from 4.9 per 100,000 to 3.7 (a 20% decline), and for those over 75 it went from 1212 to 1351 (a 9% increase). Which change is larger, in terms that matter most?

These points clearly show a failure to control cancer overall, despite undoubted successes in treating some uncommon forms of cancer, mitigating the harsh effects of treatment, and improving the quality of life of patients not cured. The implications are large, including those for research initiatives, demonstration programs, medical training, and clinical practice.

Our cancer program is in big trouble, and headed for bigger, when its most senior program officials themselves do not recognize reality. For the leader of a major public research agency to ignore such data, and instead to say that it is the skeptics who have "departed with reality," is more than a blatant attempt to deny the legitimacy of criticism. It is the ultimate self-indictment.

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1. *Cancer Patient Survival: What Progress Has Been Made?* (GAO Report PEMD-87-13, General Accounting Office, Washington, DC, 1987).
2. J. C. Bailar and E. M. Smith, *N. Engl. J. Med.* **314**, 1226 (1986); J. Cairns, *Sci. Am.* **253**, 51 (November 1985).
3. *1986 Annual Cancer Statistics Review* (National Cancer Institute, Bethesda, MD, December 1986).

I appreciate the coverage given GAO's report *Cancer Patient Survival: What Progress Has Been Made?* in the 24 April issue of *Science*, but feel that a number of clarifications should be made to place both the report and reactions to it in perspective.

My first concern relates to the objective of GAO's analysis and the research design we employed to achieve that objective. The article implies that our objective was to determine whether progress had been made against cancer *in general* and then incorrectly states that "GAO's evaluation is based solely on survival data." This characterization gives the impression that a single measure was examined to reach conclusions about a broad issue. In fact, the opposite is true: we were asked to examine what progress had been made in the particular area of cancer patient *survival*, and our evaluation involved consideration of statistical bias through a systematic examination of data on cancer incidence, mortality, survival, detection, and treatment, as well as disease symptomology and progression. We used *all* of these data to reach conclusions on real changes in cancer patient survival.

Later in the article is the statement that GAO's methodology "relied heavily on the opinions of groups of research physicians." This gives the impression that opinions of research physicians constituted the central focus of the study, when in fact they were a validation mechanism. While we did conduct sessions at comprehensive cancer centers, this does not mean that the information provided by the cancer experts carried more weight than any other data we collected. Instead, these sessions were conducted to validate or refine initial conclusions that were based entirely on our own research. By emphasizing these validation efforts and omitting discussion of our basic statistical work, the article could confuse readers with respect to the design of GAO's study and the extensiveness of our data collection efforts.

Selectivity of presentation is again the issue in another area. The article states: NCI "scientists protest charges that data on cancer survival rates are overstated." In fact, GAO's argument is that NCI has not drawn enough attention to the types of statistical bias inherent in survival rate measurement. The important point here is that NCI has *concurred* with this argument, but the article makes no mention of that fact. I think a balanced picture of NCI's reaction of GAO's report would have included the fact that, whatever its "anger," NCI has accepted GAO's recommendation.

In summary, then, I make four points of clarification. The GAO did not base its evaluation exclusively on survival rates, did not rely on expert opinion except as validation, did not take as its subject "the war on cancer," and received explicit, formal agreement from NCI to its recommendation.

Why is it important to be clear about matters such as these? Because GAO's study takes its place in the normal process of science by which data and their interpretations are independently scrutinized and rescrutinized. What we did was to objectively examine NCI's use of data. In our turn, we expect and hope that independent researchers will examine our report in the same manner. It is in this way that progress may be made, not only in the "war on cancer" and in the use of statistics, but also in the accountability of agencies (including GAO) to the public, and in the acceptance by agencies of the legitimacy, propriety, and usefulness of this normal scientific process.

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## Foreign Engineering Students

The article by Elinor G. Barber and Robert P. Morgan on the "Impact of foreign graduate students on engineering education in the United States" (3 Apr., p. 33) is a valuable complement to the studies of U.S. engineering faculty and graduate students conducted by the American Society for Engineering Education (ASEE). Results of the latter series of studies have been reported in 1982, 1983, 1984, 1986, and 1987 (*1*).

Where dealing with percentages of foreign citizens in the U.S. engineering graduate school population, ASEE data generally agree with those reported by Barber and Morgan. ASEE percentages of foreign citizens are consistently lower because our definition of "other" includes graduate students who are nonrecipients of financial aid. Moreover, the ASEE survey population embraced all engineering disciplines, rather than Barber and Morgan's four disciplines.

When our data are adjusted to exclude students who have not been aided financially, the comparisons made in Barber and Morgan's table 1 are generally confirmed. Readers of the *Science* article may be interested in noting that the ASEE data also are grouped to show regional differences as well as 4-year projections. We found that the percentage of foreign students enrolled for the first time in full-time graduate study in engineering had risen from 38% to 44% from 1983 to 1985. The projected percentage of foreign citizens among Ph.D. candidates is expected remain just above 50% through 1988–1989.

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