Letters

Breeder Reactor Debate

The readers of *Science* would have been better served by Robert Gillette's article (News and Comment, 10 May, p. 650) on the response of the Scientists' Institute for Public Information (SIPI) to the AEC's breeder environmental impact statement if he had paid more attention to the facts and less to his own guesses about SIPI's motivation. The facts are fairly simple. As a result of a successful suit by SIPI the AEC was ordered to produce, under the requirements of the National Environmental Policy Act (NEPA), an environmental statement relative to the entire breeder program. According to the AEC, the breeder program is designed to meet a substantial portion of the nation's future demand for electricity—about 23 percent of the demand by the year 2000. According to Section 102(C) and (D) of NEPA the AEC was required to describe the environmental effects of the breeder program and to compare its costs and benefits with those of alternative means of meeting this need that have lesser effects on the environment.

SIPI's purpose in responding to the impact statement was to comment on the degree to which it met these requirements of NEPA. Since it is widely acknowledged (even in the AEC statement) that the environmental impact of solar energy is considerably smaller than that of the breeder program, and that conservation reduces the environmental impact of the power saved to zero, it was necessary for the breeder environmental statement to consider the degree to which such alternatives might provide for the electricity that the breeder program is intended to supply. The SIPI statement pointed out in some detail that the AEC statement was wholly inadequate in this regard, that it failed to disclose that the breeder's purpose could readily be met by adopting energy conservation measures and by potential means of producing power from solar energy, as described in several government reports. SIPI discussed the issue of whether or not the breeder is needed and pointed out that the combined effects of energy conservation and solar and other alternative sources of energy could supplant some, and ultimately all, existing fossil and nuclear plants by 2000. This was the appropriate response to procedures established by NEPA.

These considerations explain why the SIPI statement paid so much attention to the capability of alternative sources —such as solar energy—to replace the breeder program in a national energy system. It is unfortunate that Gillette chose to ignore these fairly obvious facts about the NEPA process and instead, in order to explain why we compared the breeder program with other power sources, constructed an imaginary reluctance on my part to take "a risky position . . . open to accusations of blind opposition to progress in the manner of latter-day Luddities."

Gillette's discussion of my views on the public availability of the Subpanel IX report is also rather deficient in newsworthy fact. It is true that the AEC claimed that the report has been in its public documents room, and it is also true that shortly thereafter I repeated my claim that the report was not accessible to the public (in an address before the American Chemical Society's meeting on 3 April). Again, Gillette might have informed the readers of *Science* why I repeated that statement. The reason was, as I said in that address, that "Thus, all three attempts known to me of serious efforts to obtain a copy of the Subpanel report, *after* the date at which, according to the AEC, the report was available to the public, December 1, 1973—by Senator Abourezk, Senator Jackson and Mr. Flanigan, the President's Assistant —failed."

I am aware that the full disclosure of the facts is more difficult and perhaps less entertaining than commenting on certain selected ones. Nevertheless, I think that more emphasis should have been given to the news and less to comments on some of it.

BARRY COMMONER
Scientists' Institute for Public Information, 30 East 68 Street, New York 10021

The information Commoner contends was ignored in the 10 May article is reported explicitly there and, in varying extent, in *Science* news reports of 29 March 1974 and 29 June 1973. Commoner neglects to credit the Natural Resources Defense Council for its pivotal role in the breeder case.

ROBERT GILLETTE

Unfettered Mother

As a scientist who returned to work when my baby was 1 month old, I can assure Daniel Roth (Letters, 31 May, p. 937) that breast-feeding does not mean one is "fettered to a squalling infant 24 hours a day." Both my son and my milk supply adapted quite happily to a somewhat erratic schedule whereby I fed him around the clock over weekends, his baby-sitter gave him one bottle at midday during the week, and he had bottles all day when I was out of town. He slept for 12 hours at night from the age of 6 weeks which, in my experience, is common among breast-fed babies and vanishingly rare among the bottle-fed. Breast-feeding is healthier for the baby and cheaper, sexier, and more convenient for the mother. And if anyone doubts that pediatricians are influential in controlling fashions in feeding, let him question any mother on the extent of her pediatrician's gratuitous advice on the subject and her reactions to that advice.

KAREN ARMS
Section of Neurobiology and Behavior, Langmuir Laboratory, Cornell University, Ithaca, New York 14850

Medical School Recommendations

Each year I write dozens of letters of reference for students applying to medical school. Many of these recommendations are to be typed on printed forms from the schools which declare that the information about to be given will be kept confidential. Unfortunately, I have found that this promise is not kept. Frequently, students drop by to say they have been accepted into medical school, and they thank me specifically for the nice letter I have written in their behalf. When I ask if they have seen the letter, they reply, Yes. Further

discussion invariably reveals that most medical schools either deliberately or through negligence allow prospective medical students to see all of the confidential information accumulated about them. This occurs during the interview procedure.

Most commonly, the student is given his confidential file to carry into the interview. He has many opportunities to browse through its contents during the day as he passes from one interviewer to the next; few students resist the temptation to look in their file. Those that do resist often hear the interviewer comment upon the letters, read sections aloud, or the student himself is allowed to see the letters.

It should be unnecessary for me to point out the problems that are created by this sloppy procedure, not to mention the questionable ethics involved. A faculty member writing a less-than-glowing letter is himself subject to student criticism. More important, medical schools depend upon honest, candid evaluations. A faculty member is much less likely to write such an open appraisal of the student if he knows that the information will not be kept confidential. Thus, the letters are less meaningful in sorting out applicants. There are, of course, some people who argue that all letters should be available to the student. Whatever the merits of this argument may be, it is clear that the medical schools should either tidy up their security or make a general announcement that all letters of recommendation are open to student perusal.

CLYDE F. HERREID II
Department of Biology, State University of New York at Buffalo, Buffalo 14214

Cancer Detection

To those of us who examine specimens submitted to a cytology laboratory for the diagnosis of cancer, it is always a bit startling and discouraging when it is brought home to us anew that apparently there are still many who believe that the Papanicolaou method of detecting cancer is limited to neoplasms of the uterus.

This was again made evident in a paragraph of Thomas H. Maugh's report "Fetal antigens: A biochemical assay for cancer?" (Research News, 12 Apr., p. 147). He briefly discusses the ineffectiveness to date of most attempts to diagnose cancer sufficiently early to effect cures, and then states: "The principal exception to this rule is the Papanicolaou stain (Pap smear), in which cells sloughed from the lining of the uterus are examined for abnormalities indicative of cancer. . . . But the Pap smear represents a unique case in which the sloughed off cells are readily accessible, *and it is unlikely that comparable cytologic assays will be developed for cancers of other internal organs* [italics added]."

For years it has been routine in many laboratories of diagnostic cytology to examine appropriate specimens for the detection of cancers of the nasopharynx, trachea, bronchus, lungs, stomach, esophagus, colon, urinary bladder, ureter, renal pelvis, renal parenchyma, prostate, breast, and central nervous system. Papanicolaou (*1*), himself, extended the application of the cytologic diagnosis of cancer to include the above-mentioned cancers. In the Papanicolaou Cytology Laboratory of the New York Hospital–Cornell Medical Center, for instance, a total of 37,437 specimens were examined during the year ending 30 June 1973. Most of these (26,572) were from the female genital tract, but the remaining 10,865 specimens were from nongynecological areas.

This diversity of specimens is not by any means peculiar to this laboratory. Most cytology laboratories receive a similar assortment for evaluation. Pathologists are expected to be trained in the interpretation of cytologic material, and the more than 100 approved schools of cytotechnology in this country (which train technologists to perform the preliminary microscopic examinations) must of necessity instruct their students in both the gynecological and nongynecological aspects of cytologic diagnosis.

It is unfortunate that the term "Pap test" has become, by virtue of common usage, synonymous with the cytologic detection of cancer of the uterine cervix alone. The term should properly include the cytologic detection of many other cancers of the body, both of males and females. Another example of incorrect terminology is the reference to the Pap stain as the equivalent of the Pap smear or test. The stain is actually a modified hematoxylin and eosin stain, utilizing several counterstains, which was developed by Papanicolaou over many years of exhaustive trials of different combinations and which is still used in many, if not most, diagnostic cytology laboratories. By no means is it a specific stain for malignant cells, but rather it is a very effective means of accentuating the morphologic details of cellular nuclei which are so important in this technique of diagnosis.

JOHN F. SEYBOLT
Papanicolaou Cytology Laboratory, New York Hospital, New York 10021

References

1. G. N. Papanicolaou, *Atlas of Exfoliative Cytology* (Harvard Univ. Press, Cambridge, Mass., 1954).

Certainly no one will dispute with Maugh that the most critical deficiency of modern cancer therapy is the lack of a means of detecting the onset of malignant neoplasias. Determinations of increased or decreased enzyme levels in the blood are merely indirect biopsies of already well-established primary or metastatic tumors. Use of α-fetoprotein for a serologic test is not only theoretically incorrect, it has proved worthless as a reliable and simple means of detection of cancer in our experience as well as in that of the Mayo Clinic group and others. The question can be raised of whether a successful chemical or serologic test for early cancer diagnoses will result as a by-product of cancer research per se, as for example the α-fetoprotein test, or from a concerted effort to pinpoint some elusive, unique property of neoplasia or a pathophysiologic state that appears with the onset of cancer. It is now almost 20 years since the 1945–1955 period of intensive search for a biochemical and serologic "screening test" for cancer by investigators with imagination and courage to attempt it. It appears that Jesse Greenstein's laconic comment that "Cancer tests can be the graveyard for many a reputation" may have kept competent investigators from this area of inquiry. However, in view of the enormous technological and biochemical information that has been acquired in the years since 1955, this moribund state of affairs should not be allowed to continue as a glaring deficiency in the overall Conquest of Cancer Program.

GREGORY S. DUBOFF
Thomas Henry Simpson Memorial Institute for Medical Research, University of Michigan, Ann Arbor 48104