Correspondence

The Editorial Board will be pleased to receive and consider for publication correspondence containing information of interest to physicians or commenting on issues of the day. Letters ordinarily should not exceed 600 words, and must be typewritten, double-spaced and submitted in duplicate (the original typescript and one copy). Authors will be given an opportunity to review any substantial editing or abridgment before publication.

Benefits and Risks of Screening Mammography

TO THE EDITOR: Breslow's critique [Breslow L: Radiation risks of mammography—Another view (Editorial Comment). West J Med 125:495-497, Dec 1976] of my editorial [Sickles EA: Radiation risks of mammography (Editorial). West J Med 125:493-495, Dec 1976] makes three major points. (1) He admits that there is no hard evidence of breast cancer induction by mammography as it is practiced today and apparently recognizes the fact that informed scientific opinion is just about equally divided for and against the linear extrapolation and no threshold hypotheses. Armed with this information he concludes that the "prudent" approach is to assume an unspecified but finite oncogenic risk, even though this risk may be immeasurably small. (2) He chooses to limit acceptable evidence of the benefits of mammography to the HIP [Health Insurance Plan of Greater New York] study,[1] since this is the only *controlled* study so far published which directly indicates data on patient survival. (The HIP study found that screening mammography significantly prolonged survival but that its beneficial effects were limited to women age 50 and above). (3) From these points he concludes that screening mammography is inadvisable for women under 50, because of a finite risk and no proved benefit.

This simple and cogent analysis would seem to have considerable merit, except for one fatal flaw: point (2) is incorrect, and therefore point (3) does not logically follow.

It is as clear to me as it is to every other working mammographer that the practice of mammography is currently far more advanced than it was as done by the HIP radiologists in 1963. Evidence of this abounds; the largest and therefore most meaningful experience is that of the Breast Cancer Detection Demonstration Project (BCDDP). One *can* derive much valuable information from these data, even though there is no matched control group. For example, the rate of detection of clinically occult cancer (cancer found by mammography but missed on physical examination) in the BCDDP is substantially higher than that of the HIP study (46 percent versus 19 percent of all cancers discovered by screening).[2] Furthermore, this interval improvement in yield applies almost equally to women under and above age 50. Lundgren, in screening the entire female population over age 35 of a Swedish community, showed even higher yields for mammography.[3] I interpret these findings as indicating a definite interval improvement in the ability of mammography to detect clinically occult cancer, for women both under and above age 50.

To disregard this evidence totally (I cite only the tip of the iceberg) and demand instead a randomized clinical trial is hardly "prudent." Rather, it is shortsighted and I believe unwise. Although a randomized clinical trial is the best way to demonstrate the efficacy of a diagnostic modality, it is not the only way, especially if the diagnostic modality is as accurate and productive as mammography. Would it really be "prudent" to ask 50 percent of the women in a clinical trial to wait until their breast cancers become palpable before receiving treatment when we know that in about half of them a cancer will be discovered earlier with the addition of screening mammography? I think not, especially since we also know that clinically occult cancers are associated with a much lower incidence of positive axillary nodes than palpable cancers, and that the presence of positive axillary nodes is an indicator of increased morbidity and mortality.

Even granting Breslow's "prudent" approach to the potential risks of mammography (that is, assuming that there are risks, although these may indeed be minimal or nonexistent), recognition of the recent advances in mammography and the implied benefits to women both under and over

age 50 prompts a completely different solution to the question of whether screening mammography should be done in women 35 to 50 years old. Rather than halting such screening and placing the burden of proof on mammographers to justify the success of their technique with a randomized clinical trial, I propose to continue reaping the benefits of screening and place the burden of proof on radiobiologists to *unequivocally* define and quantitate the risks. Mammography has been done for well over 20 years, and for the first 15 years at radiation doses 25 to 50 times greater than now possible. Yet I am not aware of, and I challenge Breslow to produce, even one proved case of breast cancer resulting from mammography. When dealing with a cancer detection technique even more accurate than the Pap smear, it is inconceivable to abandon that technique on the basis of unproved risk.

Mammography is indeed one of the triumphs of modern radiology. To attempt to discredit it on the basis of insufficient evidence of risk and to also ignore the great majority of evidence demonstrating its utility is overly cautious, misleading and incorrect. I again urge that screening mammography of women age 35 to 50 be continued in the BCDDP.

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1. Strax P: Control of breast cancer through mass screening. JAMA 235:1600-1602, Apr 12, 1976
2. Preliminary (current) statistics. Breast Cancer Detection Demonstration Project, Bethesda, MD, Jul 19, 1976
3. Lundgren B, Jakobsson S: Single view mammography—A simple and efficient approach to breast cancer screening. Cancer 38:1124-1129, 1976

Preventive Medicine and Behavior Change

TO THE EDITOR: Congratulations to Dr. Breitman for his clear, good sense letter in the September 1976 issue [Breitman G: Preventive medicine—A myth. West J Med 125:236, Sep 1976], from one who is actually in the field and not in the dream world.

It occurs to me that perhaps public misunderstanding (intentionally or unintentionally promoted) is due to misuse of the term "preventive medicine." The public may rightfully associate preventive medicine with the great advances in public health due to the work of physicians and medical scientists which controlled the great epidemic and infectious diseases. In that situation the physician was actually able to provide the drug or immunization or sanitation or other factor which resulted in the longevity and freedom from disease we all enjoy. That is real "preventive medicine."

But the "preventive medicine" that the sociologists and politicians are referring to should better be called "behavioral change medicine." The difference is fundamental because the result in this situation isn't ultimately under the control of the physician but depends on the actions of the patient, the public; depends on a change in long-established habits, personalities, environments and a myriad of other factors over which the physician has *no* control.

Our medical-social organizations in dealing with the promotors of this fallacy should insist on a stricter definition. The planners' grand proposal ought be called by something other than the honored term "preventive medicine."

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Physician's Assistants in California

TO THE EDITOR: In the December issue was a letter [Reade FZ: More on physician's assistants. West J Med 125:498, Dec 1976] by Frank Z. Reade, MD, regarding physician's assistants (PA) misrepresenting themselves as physicians.

I appreciate the concern expressed by Dr. Reade regarding an incident where a physician's assistant identified himself as a physician during a telephone conversation. In no way will an attempt be made to justify the action of any person, PA or not, who intentionally misrepresents himself as a physician.

A physician's assistant is a particularly vulnerable professional—we represent a new and basically unknown concept to the general public. A patient automatically assumes that anyone in a white jacket who does the work of a physician must indeed be a physician, thus he calls this practitioner "doctor" out of respect and tradition. Certainly, to encourage this facade is wrong, but to advocate that every time a PA is called a "doctor" that he immediately retorts with a lengthy dissertation of the role and evolution of midlevel health practitioners is equally unjustified. The classic "What's a PA?" bombards us daily and sometimes we have a tendency not to overempha-