Therefore, the following question remains: When there exists reproducible, biologically plausible evidence of a significant positive association, however modest, between a common elective exposure (i.e., induced abortion) and a common life-threatening illness (i.e., breast cancer), how can the public health possibly be well served by policymakers' steadfast adherence to the contrary presumption of harmlessness?

JOEL BRIND
VERNON M. CHINCHILLI
WALTER B. SEVERS
JOAN SUMMY-LONG

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Notes

Affiliations of authors: J. Brind, Department of Natural Sciences, Baruch College, The City University of New York; V. M. Chinchilli (Center for Biostatistics and Epidemiology and The Milton S. Hershey Medical Center), W. B. Severs, J. Summy-Long (Department of Pharmacology), Pennsylvania State University College of Medicine, Hershey.
Correspondence to: Joel Brind, Ph.D., Department of Natural Sciences, Box A0506, Baruch College-CUNY, 17 Lexington Ave., New York, NY 10010.

Responses

For controversial topics, a critical letter to the editor and its response can resemble a conversation between two brick walls. To avoid that perception, we begin with points made by Brind et al. with which we agree. First, we agree that the current state of the evidence does not provide definitive conclusions regarding induced abortion and breast cancer. Indeed, in our editorial (*1*), we argued that what was most needed were results from a large cohort study within which an independent assessment of a woman's history of induced abortion was made. A month later, just such a study was published (*2*), and it showed no overall effect and no dose–response effect. An accompanying editorial (*3*) declared ''a woman need not worry about the risk of breast cancer when facing the difficult decision of whether to terminate a pregnancy.''

We also agree that the public's health is a central concern. It can be best served by judicious assessments of evidence by decision-makers free of wish bias. How we go about those assessments (i.e., what criteria we use to make judgments, how we define them, and what rules we assign to them) is crucial. We agree that biologic plausibility is an important consideration and that it cannot stand alone. We agree that the extent to which the epidemiologic findings are consistent and statistically significant is also important in making causal assessments. And we agree that a thorough analysis of bias and its impact on the validity of epidemiologic studies is necessary.

Against this backdrop of consensus come the difficult judgments and the obvious disagreements. At the heart of the matter is the extent to which measurement bias explains the inconsistencies in the epidemiologic results. For us, the results of large cohort studies, which do not suffer from the inherent recall problems of case–control studies, provide additional important evidence that it is not time to make a causal claim. Nor is it time to make changes in recommendations to women. However, we make no ''steadfast . . . presumption of harmlessness'' as Brind et al. mistakenly claim, as if we could predict the course of scientific knowledge in all its evolutionary splendor. Brind et al., on the other hand, *have* claimed causation (*4*) and may therefore be making what some could consider an unnecessarily menacing false alarm (*5*). What the future holds remains a matter of careful investigation.

DOUGLAS L. WEED
BARNETT S. KRAMER

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Affiliations of authors: D. L. Weed, Preventive Oncology Branch, Division of Cancer Prevention and Control, National Cancer Institute, Bethesda, MD, and Kennedy Institute of Ethics, Georgetown University, Washington, DC; B. S. Kramer, Division of Cancer Prevention and Control, National Cancer Institute, and editor-in-chief, Journal of the National Cancer Institute.
Correspondence to: Douglas L. Weed, M.D., Ph.D., National Institutes of Health, Executive Plaza South, Rm. T-41, Bethesda, MD 20892.

Prompted by substantial regional differences in the association between induced abortion and risk of breast cancer, we attributed the overall 90% increased risk in our study (*1*) largely to underreporting of abortion by healthy subjects. Dr. Brind and colleagues argue that the small number of subjects exposed to induced abortion (12 of 225 case patients and one of 230 control subjects) in the

southeastern regions does not justify this conclusion.

We agree with them that subgroup analyses based on small numbers increase the probability of chance findings. However, the choice for comparing the two regions was not arbitrary. Rather, it was based on a sound hypothesis: Populations with different religions and attitudes toward induced abortion may differ in their willingness to report induced abortions. Indeed, we ended up with small numbers in the southeastern region, but precisely these numbers were found to have a large impact on the estimated relative risk (RR) of breast cancer after induced abortion (all regions RR = 1.9 and 95% confidence interval [CI] = 1.1-3.2, versus western regions RR = 1.3 and 95% CI = 0.7-2.6).

Furthermore, our conclusion in regard to the presence of reporting bias was also based on the much larger numbers of women who used oral contraceptives in each of the two regions. Since ''women who are reluctant to report induced abortions may also tend to slightly underreport their use of oral contraceptives,'' we investigated reporting bias in our data on oral contraceptive use from women and their prescribers. We compared the control groups from the two regions to investigate the difference in their tendency to underreport these ''sensitive issues.'' We found that control subjects in the southeastern region underreported 6.3 months (95% CI = 1.7-10.9) of oral contraceptive use more than control subjects in the western region. The difference between the case subjects in the two regions was 0.8 month (95% CI = −3.6-5.2); within the southeastern region, control subjects underreported 4.5 months (95% CI = −0.2-9.2) of oral contraceptive use more than case patients. As we understand it, Brind et al. argue against our reporting bias explanation just because this difference between case patients and control subjects is not statistically significant at the 5% level ($P = .06$). This is somewhat surprising to us, since it is proper epidemiologic practice *not* to rely on significance tests using the conventional alpha-level of .05 for confounder selection (*2*,*3*). With case–control analyses based on oral contraceptive data reported by the women only and, alternatively, combined oral contraceptive information from women and prescribers, we illustrated this in our report (*1*). Although the *P* value was greater than .05, the difference in reporting oral contraceptive use between case patients and control subjects clearly produced a bias in the expected direction. Since many women will be more willing to report their oral contraceptive use than their history of induced abortion, these results certainly are indirect evidence for reporting bias as an explanation for the regional differences in the association between induced abortion and risk of breast cancer.

The Swedish study by Lindefors-Harris et al. (*4*) is the only study so far in which reporting bias was directly evaluated. We agree with Brind et al. that it would be highly unlikely for women to report an induced abortion that never took place, which shows that the registry was not complete. Even so, however, the study does provide suggestive evidence that reporting bias was present, if we assume that the chance to be registered at the time of induced abortion was equal for women who would and would not develop breast cancer later on (case patients/control subjects). Within the group of women with a registered induced abortion, more control subjects (16/59 = 27.1%) than case patients (5/24 = 20.8%) did not report the induced abortion.

However much the possible biologic mechanisms underlying the abortion–breast cancer association may appeal to us and how large the public health issues may be in case of a true relationship, our first and foremost concern should be directed at the basic question of whether or not the epidemiologic data are unbiased. Therefore, in reply to the final question raised by Brind et al., we would like to comment that, in our view, public health and epidemiologic research are equally disserved by inferring a causal association when an obvious type of bias has not been ruled out convincingly.

MATTI A. ROOKUS
FLORA E. VAN LEEUWEN

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Affiliation of authors: Department of Epidemiology, The Netherlands Cancer Institute, Amsterdam.
Correspondence to: Matti A. Rookus, Ph.D., Department of Epidemiology, The Netherlands Cancer Institute, Plesmanlaan 121, 1066 CX Amsterdam, The Netherlands.

Erratum: ''Preliminary Results From the Cancer Research Campaign Trial Evaluating Tamoxifen Duration in Women Aged Fifty Years or Older With Breast Cancer,'' by the Current Trials Working Party of the Cancer Research Campaign Breast Cancer Trials Group [J Natl Cancer Inst 1996;88:1834-9 (Issue 24)]. In Table 1, the median age in the 5-year tamoxifen group should be 61 years, not 51. The Journal regrets the error.