chromatography and found that more than 80% of the total PSA is free PSA (Fig. 1).

These data lead us to the following conclusions: 1) The serum PSA of some women with fibroadenomas or breast cysts could reach levels higher than those seen in serum of male patients with prostate cancer. The PSA values presented here are the highest ever reported for female serum. We speculate that this PSA is produced and released by the hyperplastic breast tissue, since this tissue contains more PSA than normal or cancerous breast tissue (5). We further speculate that, in fibroadenomas or cysts, there may be increased leakage of PSA from the tissue to the serum, a situation that is similar to that seen in prostate cancer. 2) The molecular forms of PSA in the serum of female patients with fibroadenomas or cysts are very different from those in the serum of male patients with prostate cancer. In the latter, PSA-ACT predominates; in the former, PSA-ACT is a minor component (Fig. 1). 3) Female serum should not be regarded as a PSA-free biologic fluid anymore. PSA levels higher than those seen in serum of prostate cancer patients can be seen in some female patients with fibroadenomas or cysts.

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Re: Induced Abortion and Risk for Breast Cancer: Reporting (Recall) Bias in a Dutch Case-Control Study

Rookus and van Leeuwen (1) have recently reported, as have many others, a statistically significant, increased risk of breast cancer among women exposed to induced abortion, a finding they “largely attribute” to “reporting bias.” To support this interpretation, they cited a much greater relative risk (RR) of 14.6 among women from the Roman Catholic southeastern region of The Netherlands, compared with the more secular western region (RR = 1.3). However, this apparently huge difference was obtained by limiting the analysis to parous women only under 45 years old, a subset containing only 13 subjects exposed to induced abortion in the southeast. It is not prudent to make such a strong claim based on such a small sample, regardless of statistical significance.

To bolster the claim, Rookus and van Leeuwen also compared self-reports with prescribers’ records of oral contraceptive use in the two regions. They found a slight but significant tendency for southeastern control subjects, compared with western control subjects, to underreport the duration of their oral contraceptive use. However, since the authors found no evidence of reporting bias between case patients and control subjects (who had been matched for region), reporting bias could not logically be held accountable for the observed positive association between induced abortion and breast cancer.

Nevertheless, Weed and Kramer (2) in their accompanying editorial chami-
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?

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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.

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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