LETTERS TO THE EDITOR

Relation between induced abortion and breast cancer

EDITOR,—In their review of the relation between induced abortion and risk of breast cancer, Brind et al speculate about discrepancies in two papers1 2 from a joint Swedish-Norwegian case-control study conducted in 1986–1988. Brind et al are concerned that the paper published in 1986 on oral contraceptives and breast cancer,1 which only used data on induced abortion; information on reproductive factors, including history of induced abortion, was collected, but the latter was not matched for the most important. Brind et al seem to believe that the Swedish fertility register referred to in our 1986 paper1 only includes women having had at least one child. The fertility register includes all women who have given birth in Sweden, whether or not they have given birth, and contain information on date(s) of birth(s). The speculation by Brind et al about differential recall bias according to which register was used for control selection, is therefore invalid. Brind et al refer to comments provided by Daling et al about “over reporting” of a history of induced abortion in our 1991 paper.1 Of 512 women interviewed face to face, eight women (1.6%) reported that they had had an induced abortion reported having had an induced abortion that was not recorded in the registry of legally induced abortions. In Sweden, induced abortion on request before the end of the twelfth week of pregnancy, became legal in 1975. Before 1975, induced abortion was permitted only after assessment by two physicians or by a social-psychiatric committee. The procedures to obtain abortion under this legislation were time consuming and perceived by many as stigmatising and paternalistic. Legally induced abortion in the first trimester became more easily accessible from the late 1960s, although accessibility varied between hospitals. Some women therefore had induced abortions abroad or unrecorded terminations of pregnancy. We are not surprised to find some Swedish women confidentially reporting having had abortions during the period 1966–1974 that are not recorded as legally induced abortions. It is plausible that such induced abortions are more susceptible to recall bias than induced abortions performed within the legal context in Sweden.

Also commented upon by Brind et al are the calculated odds ratios (ORs) in the study by Daling et al based on positive abortion statements from the interviews alone, and from data on positive abortion statements from interview or registry data taken from our 1991 publication. We demonstrated an apparent increase of risk attributable to differential recall by cases and controls. The calculations by Daling et al do not specifically consider the issue of recall bias but provide a “best estimate” on the association of risk of breast cancer and history of induced abortion using all available information on induced abortion from our data. Daling et al claim a statistically not significant effect of 16% “of the spurious increase in risk that arises from reporting differences between case patients and controls”, in contrast with our estimate that 50% of the increase of the OR is attributable to differential reporting from our analysis specifically considering the issue of recall bias. The data from a recent large historical cohort study based on register data in Denmark demonstrated no association between first trimester induced abortion and breast cancer, and give support to the notion that the small increase of OR reported from case-control studies on the association between breast cancer and history of induced abortion, and reflected in the review by Brind et al, is attributable to recall bias.

OLAY MIERIK
UNCH/WHO/World Bank Special Programme of Research, Development, and Research Training in Human Reproduction, Geneva, Switzerland

HANS-OLOV ADLO
Department of Medical Epidemiology, Karolinska Institute, Box 281, S-171 77, Stockholm, Sweden

GUNNAR EKLUND
Uppsala, Sweden

Correspondence to: Dr Meirik.


Reply

The letter from Meirik et al, which questions the concerns we expressed in our review and meta-analysis on induced abortion and breast cancer2 about irregularities in their own publications, raises more questions than it answers.

Indeed, we raised a number of concerns about their work, but curiously, the fact that their 1986 paper on oral contraceptives and breast cancer “did not contain data on induced abortion” was not one of them. We merely stated the fact that the paper “contained no abortion data”. Thus, the present letter of Meirik et al begins by answering a question we did not ask.

We did, however, ask questions about an extra group of young (<40 years old) control subjects drawn from fertility registers. Data from these subjects were used in the 1986 study,3 but omitted in the 1990 study,4 only to reappear in the 1991 study,5 in which retrospective interview data were compared with prospective, computer registry data. Lack of evidence of response (recall) bias was claimed. As we pointed out,6 “the deletion of the fertility register controls from the 1990 report was not explained.” Meirik et al now claim that because these controls were matched on age at first birth, which they call “the most important” of reproductive variables, this would “invalidate any attempt to analyse reproductive variables”. Now we are really at sea, and compelled to ask why, having matched for the most important control group in analysing the effects of other reproductive variables such as induced abortion, have they discarded the optimal control group? Yet Meirik et al introduce still more confusion regarding this very control group, claiming that we “appear (erroneously) to believe that the Swedish fertility register . . . only includes women having had at least one child”. This, they say, renders “invalid” our speculation “about differential recall bias according to which register was used for control selection.” We can only answer this charge by quoting the use of fertility register data on a given subject in their 1986 paper: “a continuously updated fertility register covering all Swedish women giving birth in 1960 or later.” If in fact, as is now claimed,7 “the fertility register includes all women in Sweden, whether or not they have given birth”, the error belongs to Meirik et al, and we appreciate their correction.

Even greater is our apparatus of their concerns to the point of making no serious error, to wit, the claim of “over reporting” of induced abortions. The very term “over reporting” was coined by this Swedish group in their 1986 paper3 and it uses the computer registry data on a given subject who had reported an induced abortion “from the years 1966–74 at interview, but none reported in the (prospective) abortion register.” In that paper, they reported (and still acknowledge) that seven cases and one control subject fit into this discordant category. Of critical importance is the fact that “over reporting” embodies the assumption that the abortions thus reported (as is, at interview only) had never actually taken place. Hence, the sevenfold excess of “over reported” abortions was used to calculate the “ratio of the ratios (22.4) of discordant cases regarding breast cancer patients and controls”. The fact that this ratio achieved statistical significance (p < 0.007) was the basis of their claim to having observed evidence of “this response bias.”

In our paper,1 to characterise the claim of “over reporting”, we echoed the eloquent and diplomatic words of Daling et al: “we believe it is reasonable to assume that virtually no women who truly did not have an abortion could claim to have had one”. In their
current letter, Meirik et al now say: “We are not surprised to find some Swedish women confidentially reporting having had induced abortions during the period 1966–74 that are not recorded as legally induced abortions.” In fact they mention that during this period, “Some women therefore had induced abortions abroad or unrecorded terminations of pregnancy.” This interpretation marks an about face; an acknowledgement that the pregnancy. “This interpretation marks an admissions abroad or unrecorded terminations of abortions during the period 1966–74 that are confidentially reporting having had induced abortions. Thus, based on discrepancies between the interview and computer registry data, the claim of “over reporting” is an acknowledged may not be the “gold standard” for assessing the occurrence of induced abortion. Therefore, the current letter, Meirik et al to be un-founded, and with that, significant evidence data, the claim of “over reporting” is not surprised to find some Swedish women had legal abortions on record, the current letter, Meirik et al. The use of combined induced abortion data for populations with such inordinately (5.5-fold) different exposure rates is entirely inappropriate, unless the individual odds ratios are homogeneous, which they are not (as shall be presently shown). Moreover, the induced abortion exposure rate is so much higher for Norwegian women, the use of the extra Norwegian control group in the combined calculation for Sweden and Norway (the latter only 0.2%) guarantees an underestimation of the odds ratio. As the 1990 paper does not, however, include the 195 extra Swedish controls, and as the Swedish controls under 40 years old are combined in the 1991 paper, it cannot be determined with precision how many of the controls reporting induced abortion in the 1990 study are Swedish versus Norwegian. Estimates may be made, however, of the numbers of exposed subjects and the limits of these estimates may be determined with precision, from the numbers that are reported. Specifically, it is known that the total number of Swedish and Norwegian controls reporting one or more induced abortions in the 1990 study is precisely 100. Of a certainty, 12 of these are from the group of Swedish controls age 40–44 (n = 121). It is also known that of the combined group of young Swedish controls (n = 391), 32 reported one or more induced abortions. If, as Meirik et al imply in their letter, the two young Swedish control groups are similar in their reported induced abortion exposure, we may allocate 16 exposed controls to each group. Thus, the estimated number of Swedish controls of all ages among the 317 in the 1990 study reporting one or more induced abortions is 12 + 16 = 28. As shown in table 1(B), this results in a crude odds ratio for the Swedish women of 0.92, slightly higher than that calculated for Sweden plus Norway from the original numbers as given in the 1990 study (0.89, table 1(A)). The effect on the Norwegian data is considerable, however, resulting in a crude odds ratio of 1.55 (table 1 (B)). To determine the limits of the odds ratios (table 1 (C, D)), it is alternately assumed that all the reported abortions among young Swedish controls are allocable to the main control group (the one used in the 1990 study), giving ORs for Sweden and Norway of 0.55 and 2.23, respectively; and then to the extra control group (the one omitted in the 1990 study), giving ORs of Sweden and Norway of 2.27 and 1.12, respectively. A further estimate may be made to arrive at a combined OR for Sweden and Norway, assuming it to be equal for women in both countries. This OR is 1.28 (table 1 (E)), which is the same as the weighted average we had calculated for worldwide data.

It is therefore inescapable that the inappropriate statistical analysis of the 1990 data resulted in an underestimation of the combined OR for women from Sweden and Norway, and the masking of a definitely positive association between induced abortion and breast cancer in Norwegian women. To determine the magnitude of underestimation (as well as to explain their deviations from epidemiological principles), Meirik et al will need to reveal all the raw data. It also would be prudent for them to explain the hard questions put to them, which they have yet to tackle at all, namely, (1) Why, in their 1989 computer registry study, did they compare women with abortions to general population statistics, with no adjustment for the substantial difference in the nulliparity rate (41% versus 49%, respectively), an adjustment that would surely have adjusted their OR upwards, and (2) Why, in the same study did they limit the age of abortion exposure to under 30 years? Considering the wide credence given this research group from the World Health Organisation, the high exposure rate to induced abortion, the high incidence rate of breast cancer, and most importantly, the overwhelmingly elective nature of induced abortion, Meirik et al must be forthcoming with more and better answers.

We have already noted the “deeply disturbing” trend—embodied in the words of Meirik et al—of researcher bias in the direction of minimizing the association between induced abortion and breast cancer.

### Table 1: Calculation of raw odds ratios for induced abortion and breast cancer incidence in Swedish and Norwegian women from interview data previously published by Meirik et al.

<table>
<thead>
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<th>Abortion</th>
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<th>Cases</th>
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<tr>
<td>A</td>
<td>Swedish and Norwegian women combined, from 1990 paper</td>
<td>+</td>
<td>73</td>
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<td>100</td>
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<td>349</td>
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<td>427</td>
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<td>B</td>
<td>Swedish and Norwegian data calculated separately, assuming equal induced abortion exposure in the two young control groups (see text for details)</td>
<td>+</td>
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<td>289</td>
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<td>-</td>
<td>OR=0.89</td>
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<td>138</td>
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<tr>
<td>C</td>
<td>Swedish and Norwegian data calculated separately, assuming no induced abortion exposure in young (&lt;40 y) Swedish control group deleted from 1990 paper</td>
<td>+</td>
<td>291</td>
<td>-</td>
<td>289</td>
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<td></td>
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<td>-</td>
<td>Swedish only, OR=0.92</td>
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<td></td>
<td>Norwegian only, OR=1.55</td>
<td>58</td>
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<td>154</td>
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<td>D</td>
<td>Swedish and Norwegian data calculated separately, assuming all exposure among young (&lt;40 y) Swedish controls was in group deleted from 1990 paper</td>
<td>+</td>
<td>305</td>
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<td>305</td>
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<td></td>
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<td>-</td>
<td>Swedish only, OR=2.27</td>
<td>20</td>
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<td></td>
<td>Norwegian only, OR=1.12</td>
<td>80</td>
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<td>122</td>
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<td>E</td>
<td>Swedish and Norwegian data calculated separately, assuming equal and independent in both nationalities (8 exposed controls in included group; 24 exposed young controls in group deleted from 1990 paper)</td>
<td>+</td>
<td>291</td>
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<td>Swedish only, OR=1.33</td>
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abortion and breast cancer," which reinforces the misconception that induced abortion is a safe procedure for women despite overwhelming evidence to the contrary.

JOEL BRIND
Department of Natural Sciences, Baruch College, The City University of New York, New York, NY 10010 USA

VERNON M CHINCHILLI
Biotascan Tion Section and Department of Health Education, Pennsylvania State University College of Medicine,The Milton S Hershey Medical Center, Hershey, PA 17033 USA

WALTER B SEVERS
JOAN SUMMY-LONG
Department of Pharmacology, Pennsylvania State University College of Medicine, Hershey, PA 17033 USA

Correspondence to: Professor Brind.

BOOK REVIEWS


This book describes a programme of work undertaken between 1994 and 1996 by the Health Education Authority and the Office For Public Management, entitled “The Joint Venture”. The work, involving a range of professionals and agencies, arose from the challenges posed by a major shift towards a price is a major factor inhibiting most personal purchasing, but the volume would be a useful addition to the libraries of large institutions.

JOHN YARNIELL
Epidemiology and Public Health, Queen’s University of Belfast


Will our government give the issue of homelessness priority?

In 1990, there were two government initiatives launched that attempted to resolve the problem. Despite these, the housing system continues to fail those who are least able to help themselves.

The title of this report published by Shelter forces one to consider what happens to the patient who has no home to go to. Of nearly 40 000 people who attended the accident and emergency department during 1992 at University College Hospital, London, almost 5% were homeless.

This large study gives detailed information about the illation of homeless people, and their use of accident and emergency. Improved access to GP services could reduce inappropriate use of accident and emergency and a model was described in some detail: in one hostel, a full time health worker was employed to ensure residents had access to primary health care. This led to significantly less attendances at accident and emergency.

The recommendations made for improving access seem sensible. Some of these, such as GPs in accident and emergency departments, are already being explored in more depth.

The conclusion drawn about access to healthcare and financial cost were not adequately supported by the data collected. However, the information required here was beyond the scope of this report.

The report attempts to cost treatment and suggests that substantial savings could be made by redirecting patients to primary care. The implication is that the way forward is for all homeless people to be registered with, and appropriately using, a GP, but this may not be the most appropriate solution for the home-less person.

I wonder if equal access to health care is an achievable situation or a hopeless ideal? To begin to look at alternative solutions must surely be a step in the right direction. However, the responsibility for the failed housing system remains with our government.

JENNY WAINWRIGHT
University of Glasgow


This book describes a programme of work undertaken between 1994 and 1996 by the Health Education Authority and the Office For Public Management, entitled “The Joint Venture”. The work, involving a range of professionals and agencies, arose from the challenges posed by a major shift towards a...
primary care-led NHS, brought about by the UK government’s rapid development of general practice fundholding. The Joint Venture aimed, through an evolutionary and creative process, to develop solutions to these challenges for both organisations and individuals.

The first stage was diagnostic and employed a “futures” simulation to identify problems and generate solutions. Following this, four developmental projects were launched in parallel: “Roundabout”, a behavioural simulation exploring how health gain and health promotion would fare in a primary care-led NHS. “A market research project”, to assess the future involvement of primary health care teams. “Action learning sets”, to explore the roles of managers and professionals in the new commissioning environment for health promotion. “The health gain consultancy programme”, a learning programme for senior health promotion specialists, exploring new ways of managing and influencing health promotion.

The book reports in detail the outcomes and experiences of these initiatives, devoting a chapter to each, together with a separate chapter entitled “Summary of the learning”. Anyone looking for a short list of simple solutions to the difficult challenges that lie ahead will be disappointed. The analysis and reporting of this qualitative work brings some insights and clarity, but on the whole does more to underline the sheer complexity of reorienting a large and established system towards new ways of thinking and working.

The book will be more of interest to those seeking to bring about such change than those immersed in it. It is too detailed an account for most practitioners and the jargon may be off putting. More important though, some will feel that the insights offered are too obvious, being after all, essentially the thoughts and observations of a group of practitioners.

MARTIN WHITE
Department of Epidemiology and Public Health, School of Health Sciences, The Medical School, Newcastle upon Tyne NE2 4HH


This book is a collection of essays that formed the basis of a conference, “Hard Choices in Health Care”. The first chapter explains “guaranteed entitlements”, including the guiding principles that should be in any health strategy. The issues are then explored by inviting experts from seven European countries to consider whether the development of a guaranteed entitlement to health care would better protect the rights of their citizens than the current system (Spain and Sweden already have some guaranteed entitlements). Each chapter includes the response from a different country, explaining their current health care system, the “rights” that their patients currently enjoy, and the feasibility of a guaranteed entitlement to health care. It includes some fascinating insights into different approaches—such as the waiting list initiative in Sweden and the development of citizens juries in the UK. It concludes with a comprehensive comparison of the different responses.

I was amazed how similar the issues faced by the different countries are despite their disparate health systems. They all seemed to have undergone radical health care system reforms, the majority perceived funding crises, all identified rising patient expectations as an issue, and there was a trend towards decentralisation. I was surprised that little attention was paid to public participation, with a sense that for many of the countries, it was a difficult issue that was not being tackled.

This book is described as “essential reading for all policy makers and health care workers interested in how we can guarantee the rights of patients in an era of change and uncertainty”. I am not sure it is essential but it is certainly very interesting and I do not think there is any other book that is as concise and readable on the subject; I wish I had been at the conference. By the way, the general consensus was that guaranteed entitlement to health care was not the way forward.

MELANIE JAYNE MAXWELL
Wirral Health Authority