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,[1] Brind *et al* speculate about discrepancies in two papers[2 3] from a joint Swedish-Norwegian case-control study conducted in 1984–1986. Brind *et al* are concerned that the paper published in 1986 on oral contraceptive use and breast cancer did not contain data on induced abortion; information on reproductive factors, including history of induced abortion, was collected, but the latter was not among the variables a priori decided to be used in the analysis of the association between use of combined oral contraceptives and risk of breast cancer.[2]

Brind *et al* question why the controls selected from the Swedish fertility register were not included in the paper published in 1990 on reproductive variables and risk of breast cancer.[3] We did not include these controls because apart from being matched to their cases on exact age, they were also matched on age (± 2 months) at first birth for parous cases, as explained in our 1986 paper.[2] This latter matching criteria invalidate any attempt to analyse reproductive variables, having matched for the most important. Brind *et al* seem to believe that the Swedish fertility register referred to in our 1986 paper,[2] only includes women having had at least one child. The fertility register includes all women residing in Sweden, whether or not they have given birth, and contain information on date(s) of childbirth(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*[4] about "over reporting" of a history of induced abortion in our 1991 paper.[5] Of 512 women interviewed face to face, eight women (seven cases and one control) 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[6] or unrecorded terminations of pregnancy. We are not surprised to find some Swedish women confidentially reporting having had induced 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,[5] to demonstrate 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",[4] 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.[5] The data from a recent large historical cohort study based on register data in Denmark[7] 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 *el al*,[1] is attributable to recall bias.

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1 Brind J, Chinchilli VM, Severs WB, Summy-Long J. Induced abortion as an independent risk factor for breast cancer: a comprehensive review and meta-analysis. *J Epidemiol Community Health* 1996;**50**:481–96.
2 Meirik O, Lund E, Adami H-O, Bergstrom R, Christofferen T, Bergsjo P. Oral contraceptives and breast cancer in young women. A joint national case-control study in Sweden and Norway. *Lancet* 1986;ii:650–4.
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4 Daling JR, Malone KE, Voight LF, White E, Weiss NS. Risk of breast cancer among young women: relationship to induced abortion. *J Natl Cancer Inst* 1994;**86**:1584–92.
5 Lindefors-Harris B-M, Eklund G, Adami H-O, Meirik O. Response bias in a case-control study: analysis utilizing comparative data concerning legal abortions from two independent Swedish studies. *Am J Epidemiol* 1991;**134**:1003–8.
6 Meirik O. Birth rate and birth control in Sweden 1962–1976. *Lakartidningen* 1978;**75**:426–7.
7 Melbye M, Wohlfart J, Olsen JH, *et al*. Induced abortion and the risk of breast cancer. *N Engl J Med* 1997;**336**:81–5.

Reply

The letter from Meirik *et al*, which questions the concerns we expressed in our review and meta-analysis on induced abortion and breast cancer[1] 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[2] 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 a fertility register. Data from these subjects were used in the 1986 study,[2] but omitted in the 1990 study,[3] only to reappear in the 1991 study,[4] in which retrospective interview data were compared with prospective, computer registry data, and evidence of response (recall) bias was claimed. As we pointed out,[1] "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 confounder 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 unambiguous description of this fertility register given 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, "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 appreciation of their correction of a much more 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 1991 paper,[4] and it was used to refer to discordant 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 (that 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*[5]: "we believe it is reasonable to assume that virtually no women who truly did not have an abortion would 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 computer registry may not be the "gold standard" for assessing the occurrence of induced abortion. Thus, based on discrepancies between the interview and computer registry data, the claim of "over reporting" is acknowledged by Meirik *et al* to be unfounded, and with that, significant evidence of response bias evaporates (as also pointed out by Daling *et al*.[5]) However, it is troubling that this admission is made only obliquely in their present letter, and that they continue to cling to recall bias (although they have lowered its status to a "notion") as an explanation for the repeatedly observed positive association between induced abortion and breast cancer.

Now they look for support to a recent study by Melbye *et al*[6] from Denmark, as it was based on computer registry data and as it found no association, at least between first trimester induced abortion and breast cancer. We recently have published a brief commentary[7] on the Melbye *et al* study, which study embodies such substantial departures from proper statistical analysis as (1) the breast cancer (the outcome variable) registry's *antedating* the abortion (the exposure variable) registry by up to 5.5 years, and (2) the misclassification of some 60 000 women as not having had any abortions, who actually had legal abortions on record,[8] among other serious flaws. As we have stated,[7] "we believe that a proper analysis of the Danish cohort data will instead confirm a significant, positive overall association between induced abortion and breast cancer."

As to the previous work of Meirik *et al*, we included their 1990 study[3] in the meta-analysis on the basis of its being "better designed".[1] However, the present revisitation of their earlier work has uncovered flaws so substantial as to necessitate our retraction of the credence we had given it. As noted above, the 1990 paper of Meirik *et al*[3] did not include the extra group of 195 young Swedish controls from the fertility register. However, it did include an extra group of 105 young (<40 years old) Norwegian controls that had been selected for the earlier study. According to their 1986 paper,[2] "2 controls (for each of the 105 patients) were used to increase the statistical power", because "the prevalence of OC use is lower (in Norway) than in Sweden". In the 1990 paper,[3] separate data for Swedish and Norwegian women were not shown. At first glance, the addition of the extra, young Norwegian controls would not seem to matter much, out of a total control population of 527. However, as the 1991 study[4] revisited only the data for the Swedish women, the number of patients reporting induced abortions from each country in the 1990 study are easily calculated by subtraction. Thus, of the 73 cases with induced abortion reported in the 1990 study, only 26 were Swedish,[4] and 47 were Norwegian. Considering that only 105 of the total cases were Norwegian, the exposure rate to induced abortion among the Norwegian cases is seen to be an astonishing 44.8%, compared with only 26 of 317, or 8.2% among the Swedish cases! 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, as 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 only one we are given) 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,[4] 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), which gives ORs for 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 approximately 1.3 (table 1 (E)), which is the same as the weighted average we had calculated for worldwide data.[1]

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,[1] which they have yet to tackle at all, namely, (1)Why, in their 1989 computer registry study,[9] 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 upward, and nullified their claim of having observed a statistically significant negative association, 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 work of Meirik *et al*—of researcher bias in the direction of minimising the association between induced

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[3 4]

	Abortion	*Cases*	*Controls*	*Cases*	*Controls*
A Swedish and Norwegian women combined, from 1990 paper[3]	+	73	100		
	–	349	427		
		OR=0.89			
B Swedish and Norwegian data calculated separately, assuming equal induced abortion exposure in the two young Swedish control groups (see text for details)	+	26	28	47	72
	–	291	289	58	138
		Swedish only, OR=0.92		Norwegian only, OR=1.55	
C Swedish and Norwegian data calculated separately, assuming no induced abortion exposure in young (<40 y) Swedish control group deleted from 1990 paper[3]	+	26	44	47	56
	–	291	273	58	154
		Swedish only, OR=0.55		Norwegian only, OR=2.23	
D Swedish and Norwegian data calculated separately, assuming all exposure among young (<40 y) Swedish controls was in group deleted from 1990 paper[3]	+	26	12	47	88
	–	291	305	58	122
		Swedish only, OR=2.27		Norwegian only, OR=1.12	
E Swedish and Norwegian data calculated separately, assuming equal ORs for both nationalities (8 exposed controls in included group; 24 exposed young controls in group deleted from 1990 paper[3])	+	26	20	47	80
	–	291	297	58	130
		Swedish only, OR=1.33		Norwegian only, OR=1.32	

abortion and breast cancer,[10] which reinforces the misconception that induced abortion is a safe procedure for women despite overwhelming evidence to the contrary.

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BOOK REVIEWS

Coronary heart disease, prevention, management and rehabilitation. Colin Waine (Pp 52; £13.20). London: RCGP Clinical series, 1996. ISBN 0-85084-221-2.

Given the extensive literature on coronary heart disease (CHD), it is surprising that such a slim volume has made a useful new contribution. This book manages to condense current thinking on the prevention, management, and rehabilitation of CHD into five concise and readable chapters.

It begins by summarising the WHO 1982 guidelines on CHD prevention before describing how this strategy can be applied in the primary care setting. The first chapter outlines the various approaches to prevention, distinguishing between the population approach and the high risk approach. The second chapter discusses the ways in which general practitioners can be active in all levels of prevention and emphasises the importance of identifying those at risk, using both opportunistic testing and screening. The chapter includes guidelines for secondary prevention and concludes with the often neglected issue of preventative strategies starting in childhood.

Chapters 3 and 4 deal with the management of myocardial infarction and angina and emphasise the problem that despite public health efforts, members of the public are slow to recognise symptoms of myocardial infarction.

Chapter 5 serves as a reminder that the prognosis after myocardial infarction can be improved by a well planned rehabilitation programme and outlines a four stage programme to which hospital and primary care teams should aspire.

Although much of the contents of this book will already be familiar to general practitioners and public health doctors, its main value lies in its brevity and clarity. It covers the main issues and is supported throughout by epidemiological evidence. It will be useful to general practitioners and public health doctors who require a quick update in this area and it will serve as benchmark against which general practitioners can measure their preventative activities.

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Metabolic consequences of changing dietary patterns. Edited by A P Simopoulos. (Pp 241; 221US$). World Review of Nutrition and Dietetics Vol 79. Basel, Switzerland: Karger, 1996. ISBN 3-8055-6296-9.

This is a useful introduction to a topic of major public health importance to underdeveloped, transitional, and developed countries. Five of the chapters explore this topic directly and investigate the metabolic and health consequences of recent dietary and other lifestyle changes in populations in Australia, Asia, and Africa. There are useful reviews on the following; the Australian Aboriginal Diet (this, from the Menzies School of Health Research, Darwin, which specialises in Aboriginal health, incorporates a comprehensive review of descriptive and interventional research on the Aboriginal hunter-gatherer population); the food choices and availability of Chinese Australians; traditional and current diets and meal patterns in South Africa (a comprehensive review of rural and urban African populations—indigenous and immigrant—by D Labadarios, ARP Walker, *et al*); the traditional and changing Korean diet; the ancient and modern Chinese diet. The final two chapters review the evidence that: (a) tea is a (largely) protective agent against cancer and (b) coffee may have carcinogenic properties. Both reviews provide adequate summaries, as far as can be judged, of the current literature but are clearly out of keeping with the main theme of this volume.

This volume is likely to be of strong interest to nutritionists and medical and social anthropologists among several other public health related disciplines. Unfortunately the price is a major factor inhibiting most personal purchasing, but the volume would be a useful addition to the libraries of large institutions.

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Go home and rest? The use of an accident and emergency department by homeless people. Christopher North, Hannah Moore and Christopher Owens. (Pp 75; £10.50). Shelter, 1996. IBSN 1870767-46-2.

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 whose need is greatest.

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 illnesses 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 conclusions 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 homeless 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.

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Achieving health gain through health promotion in a primary care-led NHS. Health Education Authority and Office For Public Management. (Pp 80; £10.00, paperback). London: Health Education Authority, 1997. ISBN 0 7521 0719 4.

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 the 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.

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Hard choices in health care. Edited by Jo Lenaghan. (Pp 204, UK price £17.95, elsewhere £20.00). London: BMJ Publishing Group, 1996. ISBN 0 7279 1081 7.

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.

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