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Background ‘Mammography’ (screening for breast cancer by x-ray examination) came to be widely—although not universally—accepted in the 1980s, when a number of clinical trials demonstrated a substantial reduction in risk. Early detection, before the disease spread, permitted therapy that was simultaneously less invasive and more effective. Questions that remained were largely about efficacy for younger women and optimal frequency for older women. The consensus was challenged in a series of papers by two researchers at the Nordic branch of the Cochrane collaboration, Gøtzsche and Olsen, who concluded that mammography does not save lives: instead, it exposes women to unnecessary surgical procedures.

Methods Qualitative review.

Results The basis for the Gøtzsche-Olsen critique turns out to be simple. Studies that found a benefit from mammography were discounted as being of poor quality; remaining negative studies were combined by meta-analysis. The critique therefore rests on judgments of study quality, but these judgments are based on misreadings of the data and the literature.

Conclusion The prior consensus on mammography was correct.

Keywords Mammography, breast cancer, screening, meta-analysis

The first large-scale clinical trial to demonstrate the efficacy of mammography was HIP (‘Health Insurance Plan’) in New York,^{1–7} followed by the Two-County study in Sweden.^{8–22} There were about half a dozen other trials as well, some negative but most positive. In theory, if breast cancer begins as a local disease, then early diagnosis—before the disease spreads—should allow treatment that is less invasive and more effective;²³ there may also be a biological rationale from the perspective of systemic disease.²⁴ After an initial period of controversy, mammography gained general acceptance.^{23–29} Some doubts remained, especially for younger women.^{30–32} There were also questions about optimal schedules for screening and cost effectiveness,^{33,34} but our focus is efficacy.

The consensus opinion was challenged by two researchers at the Nordic branch of the Cochrane collaboration, Gøtzsche and Olsen, who concluded that mammography does not save lives: instead, it exposes women to unnecessary diagnostic and surgical procedures.^{35–38} This opinion was based on a meta-analysis of the existing trials, where positive studies were eliminated as being of poor quality; the remaining 2 studies found negative effects. Thus, the critique hinges on the decision to exclude positive studies like HIP and Two-County. That decision was justified in turn by a literature review.

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In this paper, we discuss HIP, Two-County, and the best-known of the negative studies, CNBSS (‘Canadian National Breast Screening Study’).^{39–45} We summarize the trials and the critique. We briefly discuss other work faulting the positive studies.^{46,47} Our paper addresses the major points raised by the critics, and a few of the minor ones. We find that the quality judgments behind the critique—and hence the meta-analytic results—are based on misunderstandings of the data. We see no reason to believe that CNBSS was superior in quality to HIP or Two-County. In our opinion, therefore, the critique has little merit. We conclude that the prior consensus on mammography was correct: screening does save lives. Others have reached similar conclusions on the central points,^{26–29,48–53} although the critique has attracted some support.^{46,47,54–58} The Swedish trials (including Göteborg, Malmö, and Stockholm) have been reviewed by Nyström et al,^{53,59–61} with commentary^{35–38,62–66} and responses.^{53,67} The Edinburgh trial and the Finnish National Screening Program have been reviewed elsewhere, with comparison to the Swedish trials.^{27–29}

The HIP Trial

HIP is the Health Insurance Plan of Greater New York, a group medical practice which had in the 1960s some 700,000 members in 31 medical groups. Medical records consisted largely of paper, with computerized summaries. Subjects in the experiment were 62 000 women age 40–64, members of HIP, who were assigned to treatment or control by systematic list sampling. ‘Treatment’ consisted of invitation to 4 rounds of annual screening—a clinical exam and mammography (2 views, cephalocaudal and lateral). The control group continued to receive usual health care. Subjects were recruited during the period 1963–66. There were 18 years of followup.

The analysis was by intention to treat rather than treatment received. This is conservative, and measures the effect of the invitation to screening rather than the effect of screening itself. (Biases in treatment-received analyses are discussed by Shapiro et al.⁶) The effect of screening is diluted because there were only 4 rounds of screening, and some women in the treatment group declined to be screened: 67% were screened at least once, 40% were screened 4 times (Table 3.1 in Shapiro et al⁶). Because of this crossover, Shapiro et al⁶ refer to the treatment group as the ‘study group.’

Results from the first 5 years of followup are shown in Table 1 below.^{4,6,68} The effect of the invitation is small in absolute terms: $63 - 39 = 24$ lives are saved. Since the absolute risk from breast cancer is small, no intervention can have a large effect in absolute terms. On the other hand, in relative terms, the 5-year death rates from breast cancer are in the ratio $39/63 = 62\%$. The absolute differential persists throughout the 18-year followup period, and is perhaps more marked if we take cases incident during the first 7 years of followup, rather than 5.

The effect of screening on the screened can be estimated⁶⁹ when—as in the HIP trial—there is crossover only from the treatment arm to the control arm: some women invited to screening refuse, while control women do not seek out screening. In Table 1, there were 23 deaths from breast cancer among women who were screened, 16 among women who were offered screening but declined, and 63 in the control group. We estimate that the control group includes 16 women who would have refused screening and who died of breast cancer. The effect of screening on the women who accepted it was therefore to cut the 5-year death rate in half:

$$\frac{23}{63 - 16} = .49$$

Numerator and denominator are unbiased estimates, but there is considerable statistical uncertainty due to the limited sample size. There is also a tacit assumption that the invitation to screening

has no effect unless the woman takes it up. Similar estimates have been reported, based on data from the Two-County trial and population screening in Sweden,^{19,21} with discussion.^{20,23,70–74} Stronger assumptions are needed to analyze these data, which are partly experimental and partly observational; somewhat lower estimates have recently been suggested.²²

Table 1 HIP data. Group sizes (rounded), deaths in 5 years of followup, and death rates per 1000 women randomized

Study	Group size	Breast cancer		All other	
		No.	Rate	No.	Rate
Screened	20 200	23	1.1	428	21
Refused	10 800	16	1.5	409	38
Total	31 000	39	1.3	837	27
Control	31 000	63	2.0	879	28

Data from p. 20 in Freedman et al;⁶⁸ also see Tables 4.3, 6.2, and 6.3 in Shapiro et al.⁶ The numbers used in Freedman et al were provided by Sam Shapiro (personal communication, around 1988). Counts differ slightly from those in earlier publications, like Table 2 in Shapiro,⁴ presumably due to data editing. Screened means, accepted 1 or more screens.

We turn now to the critique. Gøtzsche and Olsen^{35–38} have 3 main arguments against HIP, which we discuss in turn.

- [1] Women with breast cancer diagnosed before randomization were differentially excluded from the screening group.
- [2] There was an imbalance in baseline characteristics.
- [3] There was differential bias in death certification.

Differential exclusion of breast cancer cases

Women were assigned to study or control in alternation, so the 2 groups should be equal in size. However, the study group is a bit smaller and the differential changes a little from one report to another.³⁵ On p. 18 of Shapiro et al,⁶ the difference is

$$\text{Study} - \text{Control} = 30\,131 - 30\,565 = -434.$$

According to Olsen and Gøtzsche,³⁷

‘This [differential] would be expected to create bias. If only 10% of these excluded breast cancer cases are added as breast cancer deaths after 18 years of follow-up, the breast cancer mortality becomes higher in the screened group than in the control group, since the difference in breast cancer mortality at that time was 44 deaths.’ [p. 6.]

In essence, women with a prior diagnosis of breast cancer are at higher risk of death from that disease: differentially excluding them from the screening group therefore creates a bias favoring mammography. To assess this criticism, we consider the exclusion criteria in HIP, which can be summarized as follows: (i) change in medical coverage between randomization and first screen, or (ii) pregnancy at screening, or (iii) diagnosis of breast cancer prior to entry.

The first criterion excluded few women; bias from this source must be small. Furthermore, reasons for changing medical coverage (moving, changing jobs) seem at best weakly related to breast cancer risk, so the sign of the bias is uncertain. With respect to the second, pregnant women are also few in number (we estimate 100 in each arm, as does Raymond Fink, personal communication). Such women must generally have been in their early 40s, and therefore at lower-than-average risk of breast cancer. Perhaps two-thirds of them were excluded from the study group at screening, which creates a (small) bias against mammography. These two criteria operate only on the screening group, accounting for some of the difference between the study and control groups.

In both arms of the trial, women with a diagnosis of breast cancer prior to randomization were excluded from counts of breast cancer cases or deaths. In the screening group, exclusions were mainly done at first screen; date of diagnosis was determined from medical records. For women in the study group who refused screening, and for women in the control group, exclusions were made when there was a recurrence of breast cancer or death; again, date of diagnosis was determined from medical records. (With paper records, exclusion prior to randomization would have been expensive because it would have been necessary to go through all 62 000 medical files.) This is not the most elegant of designs but it does not introduce bias in the counts—if followup is good and exclusions are done correctly. There is a small upward bias in determining person years at risk in the control arm and refused-screening group, which on balance works against mammography.

The design implies unequal group sizes after exclusions unless followup continues until the last breast cancer case has died. By way of illustration, suppose there were 1000 women with prior diagnosis of breast cancer in each group at baseline. If 80% of those in the study group accept screening and there is no detection in the control group, the initial imbalance would be 800. Over the next 18 years, perhaps half of the $1000 - 800 = 200$ cases in the refused-screening group and a similar percentage of the 1000 cases in the control group would have a recurrence of cancer or die, and then be excluded. On this basis, the difference between the study and control groups would drop to something like 400. Although this result depends on parameters subject to considerable uncertainty, the calculation shows that the difference of 434 (cited above) between the groups at year 18 of followup is not evidence of bias.⁷⁵

Table 2 HIP data. Incident cases during 5, 7 and 12 years of followup.

	Study	Control
5 years	304	295
7 years	426	439
12 years	767	740

Data from Table 5.1 in Shapiro et al,⁶ which has results for years 1–12. With histologic confirmation.

Screening does not prevent breast cancer but only speeds up detection. The study group should start with a higher incidence of breast cancer (‘lead time bias’), but the control group will catch up a year or so after screening stops. That is what happened (Table 2). Screening was finished (or nearly so) in 4 years, and lead time is on the order of 1 year: in other words, screening picks up a cancer roughly a year before it would become clinically manifest (pp. 43–44, 105–6 in Shapiro et

al⁶). Thus, the incidence of breast cancers in the two arms should equalize between years 5 and 7, and it does.⁷⁶ The differences in the table, 304 – 295, 426 – 439, and 767 – 740, are well within the range of chance variation. If high-risk women were differentially excluded from the study group, the count of incident cases in the study group would be lower than the control count, which is not the case.

The number of differential exclusions changes from one HIP paper to another, because there are women in the control group and the refused-screening group who had a diagnosis of breast cancer prior to randomization. As followup goes on, some of these women suffer a recurrence or die, and are then excluded from the counts (see above). Thus, differentials depend on length of followup period and, perhaps, on data editing. GO seem to be aware of these facts.³⁵ [We abbreviate ‘Gøtzsche and Olsen’ to ‘GO’; the quote is from p. 131; our comments are in square brackets.]

‘Deaths from breast cancer diagnosed before entry to the trial were generally excluded from analysis. [Done in HIP and Two-County.] Such exclusions can lead to bias when the first round of screening identifies cancer in women who have already noted a tumour in their breast if these women are subsequently excluded. [But exclusions were not made that way.] The New York trial excluded more cancers in the screening group than in the control group.’ [This is true at first screen but false when tables were compiled, because exclusions depend only on events prior to first screen, as discussed above—therefore irrelevant.]

Design issues have been explored⁷⁵ and Gøtzsche has responded:⁷⁷

‘We furthermore doubt that retrospective exclusion of women after 18 years of followup, as in the New York study, is reliable.’ [p. 2168.]

Here, Gøtzsche implicitly withdraws previous arguments^{35–38} about differential exclusions, claiming instead that procedures were retrospective and therefore unreliable. However, the exclusions were done as the study progressed. They were not based on participants’ memories but on diagnosis of breast cancer prior to baseline, as documented in the medical records. HIP surveillance of vital records, hospitals, and health insurance reports was designed to pick up all incident breast cancer cases, recurrences, and deaths, in the study group and in the control group. The evidence in Shapiro et al⁶ (pp. 3–4, 17–18, 22–24) shows that exclusions were made in a balanced, comprehensive manner. Ascertainment was nearly perfect through year 10 [p. 24], and not much worse in years 11–18. Although bias due to differential exclusion of breast cancer cases is possible, significant bias seems unlikely; Table 2 supports the data in Shapiro et al.⁶ On the other side, no tangible evidence has been produced to support claims of differential exclusions.

Of course, another interpretation may be offered for Table 2. Some large number of invasive breast cancers were excluded from the study group, balanced by the inclusion of a very similar number of DCIS (‘Ductal Carcinoma in Situ’) cases detected by screening, the corresponding cases on the control side remaining occult. This scenario seems far-fetched, for two reasons (apart from the nicety of the requisite balancing): (i) in the 1960s, DCIS might have accounted for 5% of breast cancers in the study group (compare Table 4 in Tabár et al¹⁵ with Table 1 in Rosner et al⁷⁸) and (ii) roughly half of untreated DCIS cases become clinically manifest (see Health Council of the Netherlands,⁴⁸ pp. 45–47). The rate of screen-detected DCIS increased rapidly in the 1980s,^{79,80} but this is some 20 years after HIP.

Imbalance of baseline characteristics

The next point in the critique is the alleged imbalance of baseline characteristics between the study and control groups in HIP. The discussion is based on Table 4.1 in Shapiro et al.⁶ Part of the table—with header and footnotes—is shown here as Table 3. ‘College’ means the respondent had some college; ‘menopause’ means the respondent had or was now having menopause; ‘lump’ means the respondent had a lump in the breast at some point. According to GO,³⁵

‘... in the table of seven selected characteristics ... we calculated imbalances for previous lump in the breast ($p < 0.0001$), menopause ($p < 0.0001$), and education ($p = 0.05$); there were no differences for age, religion, marital status, or pregnancies. These findings are incompatible with an adequate randomisation.’ [p. 130.]

Table 3 Characteristics of Women Entering HIP Project During 1964 (Percent)

Characteristic ^a	Study Group			Control ^c
	Total ^b	Examined	Refused	
Age 40–44	24.2	25.3	22.3	24.5
⋮	⋮	⋮	⋮	⋮
College	30.9	33.7	25.8	32.9
⋮	⋮	⋮	⋮	⋮
Menopause	70.9	66.6	78.8	74.1
Lump	9.5	10.9	7.0	11.8

a Not stated categories [non-response] range from less than 1% to 4% of total and are distributed in the same manner as knowns.

b Data for age are based on totals. For all other characteristics, data are based on a 10% sample of the examined group and a 20% sample of the nonexamined group.

c Based on a 20% sample of the control group.

We found no details of the calculation in any published paper, and believe GO misunderstood the sample sizes in the table header and footnotes. The table reports not on the whole cohort, but only on a sample of those recruited during 1964. (Samples were taken to reduce costs.) With allowance for non-response, the sample sizes for the examined, refused, and control groups are about 700, 600, and 1800 respectively. (Raymond Fink has contemporaneous documentation for planned sample sizes, personal communication.) On this basis, differences in education and menopause are insignificant. For lumps—the worst of the comparisons—we compute $z = 2$, $p = .05$ (two-sided), using the correct sample sizes. Table 2 in Shapiro et al² has more complete data for the examined group; the percentage with lumps may be computed from those data as 11.7%, which is much closer to the control figure of 11.8% in Table 3, confirming that the discrepancy noted by GO is just due to chance; also see Table 8 in Fink et al.³ We conclude there was no imbalance at baseline in HIP.

Bias in death certification

We turn now to the third element of the critique, namely, differential bias in death certification; the idea here is that breast cancer is less likely to be recognized as the cause of death in the screening group, due perhaps to a belief in the efficacy of screening. GO make the following claims.

[1] ‘Knowledge of screening status may affect the judgment of cause of death. Masked assessment of cause of death was used only in the trials from Canada and Malmö’³⁵ [p. 131.]

[2] ‘The review provided evidence that assessment of cause of death is unreliable and biased in favour of screening. Even when endpoint committees masked to group assignment were used, uncertain causes of death were significantly more commonly ascribed to breast cancer than to other causes in the control group.’³⁶ [p. 1340.]

[3] ‘Cause of death assessments [for 71 women from the HIP study group and 73 from the control group] were considered dubious and were reviewed blindly This review appears to be biased with two to seven times . . . more deaths classified as caused by breast cancer in the control group than in the screened group . . . ($p = 0.0003$).’³⁷ [p. 6.]

Determining cause of death can be difficult, and bias is possible even in a blind review. (For instance, women in the screening group will often have been treated earlier in the course of the disease, which may influence judgments as to whether cancer at another site is primary or secondary.) In short, the accusation is plausible. However, the evidence presented by GO is flimsy, and there is good evidence against their position. If bias in classification of deaths exists for HIP or the Two-County trial, it is not large.

Table 4 Deaths through end of followup (year 18), among breast cancer cases diagnosed in years 1–7.

Classification by HIP			
	Cases	Breast cancer deaths	Deaths from other causes
Study	431	180	58
Control	448	236	38

Death certification (inferred)			
	Cases	Breast cancer deaths	Deaths from other causes
Study	431	167	71
Control	448	201	73

Data from Tables 3.5, 5.1, 6.3, 6.5 Shapiro et al.⁶ We take Table 3.5 as reporting review of all breast cancer cases assigned to another cause of death on the death certificate, with results shown in Tables 6.3 and 6.5. Cases and deaths include diagnosis without histologic confirmation (5 in study group, 9 in control). Diagnoses reported in Table 2 above had histologic confirmation. HIP review data were published for year 7, but other years were similar (Shapiro et al.,⁶ p. 32).

With respect to [1], HIP used blind review on all breast cancer cases assigned to another cause of death on the death certificate (Shapiro et al,⁶ pp. 29–33). Moreover, there was extensive blind review on the Two-County data (Nyström et al^{59,60}). The second sentence in [1] is simply wrong. With respect to [2] and [3], GO’s statistics ignore the possibility that ambiguous deaths in the screening group are indeed less likely to be breast cancers, because screening and early therapy help to prevent death from that disease.^{50,75} By our calculations, the HIP review process moved 13 out of 71 deaths from other causes to breast cancer in the study group, and 35 out of 73 in the control group (last column of Table 4). Details of the GO calculation in [3] remain a little hazy, but they seem to be looking at an odds ratio like

$$\frac{(73 - 38)/38}{(71 - 58)/58} = 4.1,$$

which is significantly different from 1.0. Their procedure tests a composite null hypothesis that (i) screening has no effect, and (ii) there is no bias in the death certificates, and (iii) there is no bias in the HIP review. Rejecting the composite null provides little evidence about the particular hypothesis of concern—bias in the HIP review—unless hypotheses (i) and (ii) can be taken as true. Hypothesis (i) is the chief point at issue. Assuming that mammography has no effect in order to prove bias in the HIP review seems perverse.

Another way to handle the possibility of errors in determining the cause of death is to use death itself as the endpoint. The total mortality rate among incident breast cancer cases has less statistical power than mortality from breast cancer, but is unaffected by cause-of-death classifications. That endpoint too favors mammography (Table 5), although the difference is only borderline significant: $\chi^2 = 3.2$ on 1 degree of freedom, $p = .07$. The test is two-sided, and does not consider time of death. All screening was done during the first 5 years after entry, so there can be no benefit for women with cancers detected in years 6 and 7. Moreover, as time goes on, the number of deaths from causes other than breast cancer will increase, further reducing power. If we shorten the followup time to 10 years (Table 6), power will be better: $\chi^2 = 5.8$, $p = .02$. As the examples show, significance levels will be different for different time periods; Tables 5 and 6 seem representative of the HIP data. At year 7, the numbers of incident cases in the two arms have equalized, so the comparison of mortality rates is fair.⁷⁶ This idea has been developed on data from the Swedish trials, where sample sizes are much larger and results are highly significant.⁸¹

Table 5 Number of breast cancer cases diagnosed in years 1–7.
Percentage that died through end of followup (year 18).

	Cases	Deaths
Study	431	55%
Control	448	61%

Source of data is Table 4; cause of death is not material; includes diagnosis without histological confirmation..

Table 6 Number of breast cancer cases diagnosed in years 1–7.
Percentage that died in 10 years of followup.

	Cases	Deaths
Study	431	34%
Control	448	42%

Data sources as in Table 4; includes diagnosis without histological confirmation.

All-cause mortality

We have discussed two endpoints so far: (i) breast cancer mortality, and (ii) total mortality amongst breast cancers. However, GO propose all-cause mortality as the definitive endpoint in trials of screening:³⁶

‘The main outcome measure in the screening trials was breast-cancer mortality. This choice seems rational, since larger trials would be needed to show an effect on overall mortality. However, we showed that the assumption that a demonstrated effect on breast-cancer mortality can be translated into a reduction in overall mortality rests on suppositions that are not correct. . . . The only reliable mortality estimates are therefore those for overall mortality. . . . Thus, although the trials were underpowered for all-cause mortality, the reliable evidence does not indicate any survival benefit of mass screening for breast cancer.’ [p. 1341.]

To clarify the power issue, we sketch a hypothetical clinical trial. Randomize 200,000 women, half to mammography and half to control. Follow the women for 10 years. Assume 100% compliance in both arms, no loss to followup, 50% reduction in risk of death from breast cancer in the screening arm, and no other effects. Assume baseline mortality rates as in HIP. A trial of this size would be extraordinarily difficult to implement, and the power to detect a significant reduction in total mortality (at the .05 significance level) is barely .80. With more realistic compliance parameters and contemporary death rates, power would be even lower. Power is limited because death from breast cancer is a rare event. The conclusion is equally obvious—all-cause mortality is impractical as the defining endpoint for any single trial of mammography.⁵¹ (Pooling the trials is discussed later.)

The Two-County Trial: Sweden

Study design

The trial was done in two Swedish counties, Kopparberg and Östergötland. (Kopparberg is later called Dalarna, and in some publications, Kopparberg is labelled ‘W’ while Östergötland is labelled ‘E.’) The study population consisted of all women age 40+ in the two counties. Each county was divided into ‘blocks,’ and the blocks further subdivided into ‘clusters,’ these being small geographical areas with administrative identities of their own. The objective was to make the clusters similar within blocks, in terms of demographics and socio-economics. Different blocks, however, were allowed to be quite different.

Kopparberg had 7 blocks, each subdivided into 3 clusters. Some of the blocks were small cities, the clusters being parishes or tax districts; other blocks were more rural, although clusters were

called ‘municipalities.’ Within each block, 2 clusters were chosen at random for the ASP (Active Study Population, intervention); the remaining cluster went into the PSP (Passive Study Population, control). In Kopparberg, the ASP is therefore about double the size of the PSP. Östergötland had 12 blocks, each subdivided into 2 clusters. Within each block, 1 cluster was chosen at random for the ASP, and the other went into the PSP: here, the ASP and PSP are nearly equal in size. In this fashion, the entire population of both counties is randomized.

Randomization began block by block in 1977 in Kopparberg and in 1978 in Östergötland. After randomization, the ASP in a block was invited to screening (mediolateral oblique-view mammography only). There were 2 to 4 (occasionally 5) rounds of screening, with more for the younger women and less for the older. In 1984–86, the PSP was invited to screening, and then the trial was closed. Subsequently, all women in the 2 counties were invited to screening on a ‘service’ basis (as part of their routine health care). Compliance among women age 75+ was poor, so this group was dropped in the analysis phase. Compliance for women age 70–74 was also not so good: those in the ASP were therefore invited to 2 rounds of screening only.

Incident cases are counted for the period 1977/78–1986, in both arms of the trial, based on Swedish cancer registry data. More specifically, the incidence period is from the randomization of a block until closure of the trial, that is, completion of the first PSP screen in the block. Women with a diagnosis of breast cancer prior to randomization are excluded, using registry data. There are 2468 incident cases. Followup of these cases to determine mortality continues indefinitely, and the bulk of the reports on the Two-County trial focus on the experience of these cases. (This design is called ‘the evaluation model’ by Nyström et al,^{53,59–61} although no model is involved.) No one source fully describes the Two-County study. Details are in various publications by the investigators.^{8–13,17} Further clarification was provided by Duffy and Tabár (personal communication).

Results

Tables 7 and 8 show virtual equality of breast cancer incidence rates in the ASP and PSP over the period of the trial. The tables also show that death rates from breast cancer are significantly lower in the ASP. The reduction in death rates due to screening—more precisely, due to assignment to ASP—is $(6.5 - 3.9)/6.5 = 40\%$ in Kopparberg, and $(5.7 - 4.3)/5.7 = 25\%$ in Östergötland. This intention-to-treat analysis suffers from dilution effects. For one thing, there was a 10%–20% crossover rate in the treatment arm; there was a 10%–15% crossover in control, although some of this may reflect diagnostics rather than screening.^{9,11} And, if we were to extend the incidence period

Table 7 Two-County data. Counts.

	Kopparberg			Östergötland		
	Cases	Deaths	N	Cases	Deaths	N
ASP	694	152	38 589	732	167	38 491
PSP	359	121	18 582	683	213	37 403

N’s from Table 2 in Tabár et al.¹² Breast cancer cases and deaths from Table 1 in Tabár et al,¹⁷ with about 18 years of followup (to 1998). Incident cases during the period of the trial, approximately 1977–86, as explained in the text. Deaths during the trial or afterward, among incident cases (evaluation model).

Table 8 Two-County data. Incidence rates of breast cancer. Death rates from breast cancer. Per 1000 women randomized

	Kopparberg		Östergötland	
	Cases	Deaths	Cases	Deaths
ASP	18.0	3.9	19.0	4.3
PSP	19.3	6.5	18.3	5.7

Data sources as in Table 7.

beyond the period of the trial, as in the ‘followup model’ of Nyström et al,⁵⁹ service screening would play a major role.

The critique

The major points are as follows [Gøtzsche and Olsen,³⁵ p. 130].

[1] Cluster randomization is biased.

This is false, with a minor exception—ratio estimator bias,⁸² which affects all rates whose denominators are random (a typical denominator being person-years at risk). Numerators and denominators in the Two-County study are unbiased, because sample averages are unbiased. With a study of this size, ratio estimator bias is likely to be negligible. Of course, variances may be larger with cluster randomization, and this needs to be taken into account when analyzing the data.

[2] The ASP is older than the PSP, ‘ $p < 0.0001$,’ demonstrating the failure of the randomization.

The difference was discussed by Tabár et al.^{12,83} It amounts to a few months, and (if anything) dilutes the effect of screening.⁸⁴ GO exaggerated the statistical significance of this difference by ignoring the cluster randomization when computing p .^{85–90} Furthermore, there is good evidence to show that randomization was successful, producing comparable groups of women in the ASP and PSP along several important dimensions. For instance, there is near-equality of breast cancer incidence rates before the study began (Figure 1 in Nyström et al⁵³). Likewise, death rates from other causes are nearly equal.^{11,61}

[3] There is inconsistent reporting of population size: for instance, 134 867 in 1985⁹ and 133 065 in 1989.¹²

GO cite the 1989 paper¹² but miss some crucial details. The Two-County investigators linked their database to the Swedish cancer registry and cleaned the data by eliminating women with diagnosis of breast cancer prior to randomization. Before linkage, such women were excluded at recurrence of disease or death. The 1985 paper⁹ was written before linkage; the 1989 paper,¹² after linkage—explaining the difference in reported population size.^{49,83}

[4] There is inconsistent reporting of deaths from breast cancer. For example, take women age 40–49 in Kopparberg. Are the ASP:PSP counts 22:16 or 26:18? [Olsen and Gøtzsche,³⁷ p. 16.]

Table 2 in Tabár et al¹⁵ has 22:16 at 12.5 years of average followup, whereas Table II in Tabár et al¹⁶ has 26:18 at 15.5 years (average followup for all subjects, our calculation). The difference in

counts is due to longer followup, which GO ignore.⁹⁰ We resolved other ‘discrepancies’ in a similar fashion.

[5] There is bias in assigning cause of death.

This is the most disturbing of the arguments, and we take it up in some detail.

[5.1] ‘The decrease in breast-cancer mortality with screening in the Two-County study when the endpoint committee did not know status was similar to that when cause of death was assessed openly (and where we found bias in the classification process). Therefore, our findings that masked endpoint committees make biased assessments are supported.’ [Gøtzsche,⁷⁷ p. 2167.]

[5.2] ‘We found data from the Two-County trial [Tabár et al¹¹] that could illustrate this possible misclassification directly. . . . Among women with a diagnosis of breast cancer, mortality for other cancers was significantly higher (RR=2.42, 1.00–5.85) ($p = 0.05$); mortality from all other causes was also higher, although not significantly (RR=1.37, 0.93–2.04) ($p = 0.11$).’ [Olsen and Gøtzsche,³⁷ p. 16.]

With respect to [5.1], the agreement between endpoint committees shows if anything that bias is unlikely. GO would have an argument only if they had evidence to demonstrate bias in open reading; but they do not, as will be seen by examining [5.2]. Most of the data in Tabár et al¹¹ show near-equality of death rates from other causes among the ASP and PSP, which makes bias in death classification seem unlikely. Tables 5 and 6 in Tabár et al¹¹ report deaths by cause among the breast cancer cases, with 25/1295 deaths from other cancers in the ASP and 6/768 in the PSP. That is the probable source for the claimed $p = .05$, although we cannot quite replicate the calculation. What should we make of this finding? Adjustment for time on risk would increase p , since the ASP cases live longer than the PSP cases. Adjustment for multiple comparisons—and GO have clearly made many comparisons—would also have a substantial effect on p . This is not good evidence.

Longer followup confirms the view that GO have capitalized on an artifact. For instance, with 8 years of followup, Table 11 in Tabár et al¹² shows that risk of death from other causes among breast cancer cases is similar in the ASP and PSP ($p = .7$). Using data with 11 years of followup in Table 9 of Tabár et al,¹³ we consider deaths from other causes among the breast cancer cases, comparing the observed number of events in the ASP to an expected number computed from the PSP: observed – expected = 7 ± 22 , $p = .8$, taking into account age and county. (Deaths from other causes were allocated proportional to time on risk, which is conservative because hazard rates increase with age.) Tabár et al⁵¹ and Duffy et al⁹¹ analyze more recent data with similar conclusions; in particular, there is significant reduction in all-cause mortality among breast cancer cases, which cannot be explained by errors in death classification.⁸¹

We turn now to total mortality in the whole ASP and PSP. Of the endpoints under consideration, this one has the least statistical power, but is the most robust against classification error. Nyström et al⁵³ report on a pooled analysis of the Swedish trials: in combination, nearly 250 000 women were randomized and followed for 16 years. There is a 2% reduction in overall mortality among those invited to screening (RR=.98; 95% CI, .96–1.00). The standard error computed by Nyström et al⁵³ does not take clustering into account; however, sample design has relatively little effect on standard errors for death rates.^{18,53,91} By contrast, the effect of clustering on standard errors for average ages remains to be studied. The results of Nyström et al⁵³ support the following conclusions: (i) screening has an impact on total mortality, and (ii) bias in death certification plays little role in explaining the results of the clinical trials.

In summary, the Swedish data demonstrate a protective effect from screening if breast cancer deaths are determined from death certificate data or by either of 2 independent endpoint committees; screening has an effect whether breast cancer is the underlying cause of death, or present at death.^{53,59–61} Further support for the Two-County data comes from the statistical analyses described above, using deaths among breast cancer cases as the endpoint,^{12,51,91} or deaths in the whole study population.⁵³ GO have not made a case for differential bias in death classification. Their other points are even less convincing. Reviewers have concurred with this assessment: see §§ 4.2, 5 in Health Council of the Netherlands.⁴⁸

Other work

Statistician Donald Berry has the following points.

[1] ‘The presence of a number of well-known biases (including “lead-time bias” and “length bias”) make it difficult to ascertain the benefits of screening.’⁴⁶

‘Lead-time bias’ means that screening speeds up detection, so incidence is higher in the study group at the beginning of followup. ‘Length bias’ means that screening is likely to pick up slower-rather than faster-growing tumors. However, clinical trials like HIP or Two-County are skewed by neither bias, because (i) they use death from breast cancer as the endpoint not detection rates or lifespan after detection, and (ii) they use intention-to-treat analyses. Indeed, that is why attention is restricted to evidence from clinical trials.⁴⁶

[2] ‘A major issue in randomized studies—including screening trials—is lack of compliance with the study protocols participants skipped their assigned mammograms about 20% of the time. In addition some participants assigned to be control subjects opted to have screening mammograms. The extent of the bias caused by lack of compliance is not known.’⁴⁶

Generally, however, crossover dilutes the effect of screening: intention-to-treat measures the effect of assignment not the effect of treatment.

[3] ‘Women with pre-existing breast cancer were preferentially excluded from the screening group. The problem was most severe in the New York trial. . . .’⁴⁷

As shown above, the evidence for preferential exclusions in HIP or Two-County is speculative at best. This point gets a different twist in an interview (*New York Science Times* 9 April 2002 p. D4).

“‘Only the screening group had mammograms,” Dr. Berry said. “On second look at a woman’s first mammogram, one might find that breast cancer was present at the time but it had been missed,” he said. So more women might have been excluded from the mammography group after they developed breast cancer.”’ [But women were excluded if their breast cancer was diagnosed before randomization; what was found or missed on mammography is irrelevant to exclusion.]

[4] ‘. . .the scheduled control mammogram slipped in all three [Swedish] trials, allowing for more time to detect cancers in the control group [after last ASP screen until completion of first PSP screen].’⁴⁷

The Two-County trial followed its timetable as well as could be expected, and obtained near-equality of incidence rates in the ASP and PSP due to its design. In theory, however, time elapsed between the last screen of ASP and first screen of PSP could create a bias in favor of screening

(‘time-lapse bias’). The argument is not entirely straightforward. The evaluation model⁵⁹ counts only cases incident during the period of the trial; however, all deaths among these cases are counted, including those occurring after the trial closes. Screening the PSP when closing the trial advances the time of diagnosis for some breast cancers; subsequent deaths are then counted against the PSP. That is what creates a possible bias in favor of mammography. However, Tabár et al⁹ showed a significant effect from screening the ASP, at a time when only 5% of the PSP had been screened.⁹¹ These results cannot be affected by ‘time-lapse bias.’

The followup model⁵⁹ looks at all cancers incident after the trial starts, including cancers incident after the PSP is screened, and is also immune to time-lapse bias. The protective effect of mammography is significant according to the followup model (Table II in Nyström et al,⁵⁹ after pooling the two counties). Here, dilution is the problem: the effect of mammography is understated, because the PSP was screened at the end of trial, and continues to receive service screening after the trial is over. The theoretical bias created by the time lapse has no practical relevance.

Canadian National Breast Screening Study

This negative study is judged to have acceptable quality by GO and Berry. The study covered two age groups, 40–49 and 50–59. Women were recruited in the period 1980 to 1985, and followed to 1988; average followup was 8.5 years.^{40,41} There was late followup for the younger women, to 1993; and for the older women, to 1996.^{42–45} Two-view mammography was used, initially craniocaudal and mediolateral, but the latter was changed to mediolateral oblique in 1985.⁹²

To describe the designs, the following abbreviations will be useful:

MA = mammography, PE = physical exam, BSE = breast self exam, UC = usual care.

In both age groups, subjects were volunteers who turned up at a screening center and signed the consent form. PE was done mainly by highly-trained nurses. In both age groups and both arms of the trial, participants were shown how to do BSE. All participants randomized to treatment were invited to 4 screens, and 62% of them were invited to a 5th screen. The treatment screens in both age groups comprised MA and PE. The control condition, however, was different in the 2 age groups. In the group aged 40–49, the control was PE at first screen then UC only, that is, only 1 round of screening; 50 430 women were randomized. In the group aged 50–59, control women were offered 4 or 5 rounds of screening by PE; 39 405 women were randomized.

Table 9. Comparing CNBSS, Two-County, and HIP.

	Screening		Control
	Modality	Number	
CNBSS1: 40–49	MA+PE	4–5	PE at 1st screen then UC
CNBSS2: 50–59	MA+PE	4–5	4–5 rounds of PE
Two-County	MA	2–5	UC then 1 round of MA
HIP	MA+PE	4	UC

CNBSS differed from HIP (Table 9). The HIP trial measured the impact of screening by MA and PE compared to usual care, whereas CNBSS2 measured the impact of screening by MA and PE

relative to screening by PE only. (More precisely, the trials are measuring the impact of invitations to be screened.) Furthermore, Two-County differs from HIP and CNBSS. The different trials are measuring different things.

Power is an issue. CNBSS had low power because there were few deaths from breast cancer—66 in CNBSS1 (Table 9, Miller et al⁴⁰) and 77 in CNBSS2 (Table 9, Miller et al⁴¹). Any effect, or lack of effect, can be only be demonstrated with poor precision. Moreover, CNBSS has been dogged from the beginning by accusations of (i) poor radiology, and (ii) ‘steering’ high-risk women to MA; the trialists and others have responded to the accusations.^{92–103} We consider the points in turn.

According to Baines, McFarlane, and Miller,⁹³ center radiologists only agreed with the reference radiologist 30%–50% of the time. ‘Observer error and technical problems’ led to delayed detection in 22%–35% of cancers. Suggestions—for instance, don’t mark up the film with a grease pencil—‘were sometimes resisted by center radiologists.’ Baines and Miller were the two lead investigators on CNBSS, and McFarlane was the reference radiologist. Their report is not reassuring about the quality of the radiology.

The evidence on steering is generally anecdotal but should not be dismissed—or accepted—for that reason. There is one statistical analysis to report.^{94,100} In CNBSS1 (the 40–49 age group), 22 advanced breast cancers (4+ nodes involved) were detected by PE at first screen: 17 in the treatment arm and 5 in the control arm, $p = .017$. In the treatment group, there were 2 additional cancers detected by MA only, which are irrelevant for present purposes.

Responses

Bailar and MacMahon¹⁰² (with commentary¹⁰³) assessed the randomization and found it acceptable. Bailar and MacMahon did not consider the radiology or followup procedures. They did not look at CNBSS2 (women age 50–59). They did not follow their own plan for the review: among other things, they did not interview field staff. They acknowledge the 17:5 imbalance.^{94,100} They acknowledge that the protocol for the trial was not followed, with the result that (i) steering would have been easy to do, and (ii) there could have been some motivation for steering. Indeed, assignment to treatment or control was generally done locally, *after results of physical examination were known*. Nurses may have wanted high-risk subjects to get what seemed to be the better treatment. The CNBSS log books were altered, but ‘document experts found no evidence of a deliberate attempt to conceal the alterations.’ That seems weak: among other things, randomization could have been subverted simply by changing the order in which names were entered into the log books.

Baines⁹⁹ [p. 329] notes that a comparison of advanced cancers detected by MA+PE in treatment to those detected by PE in control (19 to 5) is biased. Such a comparison might indeed be biased, but it is not the comparison that was made.^{94,100} Baines also addresses questions about followup, radiology, and steering, as do other papers.^{42–45,101} CNBSS remains controversial in some quarters^{20,23,103–105} but approved in others.^{28–29}

GO on CNBSS

GO have not addressed the radiology, except to say that CNBSS was the only trial to have assessed the mammograms (p. 5 in Olsen and Götzsche³⁷), and CNBSS found small tumors.³⁵ But that does not address questions about which tumors were missed, which were found, and when they

were found. Baines, McFarlane, and Miller⁹³ are not reassuring about study quality. GO cite this paper without discussion.

With respect to the statistical evidence on steering, say GO,³⁷

the 17:5 imbalance in advanced cancers detected by PE at first screen of the younger women ‘is a post-hoc subgroup finding which is probably a result of the intervention, and exclusion of the deaths caused by these cancers does not change the result. . . .[p. 10]’

(i) GO are not in the strongest of positions to complain about ‘post hoc subgroup findings,’ since most of their analysis is post hoc. (ii) This particular finding is hardly post hoc, being mentioned in the original report of the trial (Miller et al,⁴⁰ pp. 1470, 1473). (iii) The 22 cancers detected by PE cannot be ‘a result of the intervention,’ since PE was done at first screen in both arms of the trial. (iv) These 22 cases may only be the tip of the iceberg. We cannot know how many other high-risk women were steered to treatment. The answer may be 0. But this is the number in question, and until this number can be estimated, adjustment for steering is impossible. GO also say:³⁵

‘A persistent criticism has been that an effect would be difficult to find because the breasts of all women in the age-group 50–59 years were physically examined regularly. This criticism is unwarranted because mammography will identify many tumours that are too small to be detected on physical examination alone. Furthermore, any effect of physical examination is likely to be small. A study of 122 471 women found no effect of regular self-examination of the breast on breast-cancer mortality after 9 years of followup, even though twice as many of the intervention group consulted an oncologist.’ [p. 132.]

This response to criticism of CNBSS is irrelevant, because breast self examination is not the same as breast physical examination. Breast self examination is done by the woman herself: breast physical examination is done to the woman by a trained professional. Breast self examination may be of little value,¹⁰⁶ whereas breast physical examination is effective at cancer detection. That seems to be the case in CNBSS, especially among the younger women (Table 10). PE even detects many cancers missed by MA.

Table 10 CNBSS data. Treatment arm.
Cancers detected by various modalities.

	Cancers detected by		
	MA only	PE only	Both
Age 40–49	105	81	73
Age 50–59	180	64	89

Computed by us from Table 5 in Miller et al⁴⁰ and Table 5 in Miller et al.⁴¹

CNBSS1: Invasive breast cancer

Table 11 summarizes the incidence of invasive breast cancer and deaths after 8.5 years of followup in CNBSS1:⁴⁰ there is no saving in lives from mammography. On the other hand, Figure 3 in Miller et al⁴⁵ suggests a beneficial effect at the end of late followup: the cumulative mortality curves cross. If advanced breast cancers detected by PE at baseline are excluded from the study population,

the effect will be about 20%, although statistical significance is not achieved. Table 11 and results from late followup support the idea that something went awry in the randomization: indeed, at 8.5 years, there is a highly significant excess of invasive cancers in the screening group. The excess persists through late followup,⁴⁵ and there is a similar (less significant) excess in CNBSS2.⁴⁴ These differentials are hard to explain on the basis of lead time, unless many cancers classified as ‘invasive’ do not progress to clinical significance, raising other questions about CNBSS. The differential in CNBSS1 is not significant at the end of late followup, and strikingly good balance is achieved between MA and UC groups in terms of risk factors and referral for surgical evaluation, observations which reduce the force of Table 11. Considering all these factors, we see no reason to judge CNBSS as higher in quality than HIP or Two-County.

Table 11 Cumulative number of invasive breast cancer to end of followup (mean 8.5 years). Deaths from breast cancer through 7th year after entry. CNBSS1.

	Number	Deaths
MA	331	38
UC	272	28
Difference	59 ± 25	10 ± 8
z	2.38	1.17

Computed by us from Tables 7 and 9 in Miller et al.⁴⁰

Conclusion

In their meta-analysis, Gøtzsche and Olsen^{35–38} excluded positive studies on mammography, like HIP and Two-County. Consequently, there was no benefit from screening. Were the exclusions justified? To answer this question, we had to examine in detail the various studies and the criticisms raised by GO. We focused on HIP, Two-County, and CNBSS. The chief criticisms leveled at HIP were (i) differential exclusion of high-risk women, and (ii) imbalance of risk factors at baseline. The chief criticisms of the Two-County trial were (i) use of cluster randomization, (ii) imbalance at baseline, and (iii) inconsistent reporting of data. Both trials were criticized for bias in determining cause of death.

With respect to HIP, point (i) reflects a misunderstanding of the design. Point (ii) reflects a misunderstanding of the table that was analyzed. This is bothersome, because the design features relevant to (i) are discussed in the reports GO cite, within a few pages of the numbers they use. Similarly, the table cited for (ii) contains most of the relevant information in headnotes and footnotes. For Two-County, points (i) and (ii) show some misunderstanding of basic statistical concepts like bias, variance, and clustering. Point (iii) depends on lack of care in reading tables, or lack of attention to explanatory material presented within a few pages of the tables used. Bias in determining cause of death remains a possibility. However, evidence cited to demonstrate this bias evaporates when examined, and there is compelling evidence on the other side, including comparability of death rates in treatment and control from causes other than breast cancer, reduction of total mortality among breast cancer cases, and a reduction in total mortality in the whole intervention group when the Swedish trials are pooled.

CNBSS has been criticized for (i) failures in randomization, and (ii) poor mammography. It has also been observed that (iii) CNBSS compared mammography to physical breast examination by trained personnel, rather than comparing mammography to usual care. GO's defense of the randomization mischaracterizes the evidence.^{94,100} The comparison was not post hoc; nor could the finding possibly have resulted from the intervention, because the comparison was of tumors discovered by physical examination at baseline in each arm of the trial. With respect to (ii), Baines, McFarlane, and Miller⁹³—the two principal investigators and the reference radiologist—are not reassuring about study quality, and GO have not discussed this paper. GO's response to point (iii) involves a confusion between breast examination by a practitioner and self examination; it also ignores CNBSS data on the efficacy of breast examination by a practitioner (Table 10).

GO's critique of the positive studies (HIP and Two-County), like their defense of CNBSS, is careless at best. Rather than clarifying the issues, their papers have instead generated much confusion. Clinical trials of mammography have led to substantial advances in understanding breast cancer, and a substantial reduction in mortality from this disease. It is time to move on,^{107–109} although some questions may remain.^{110,111}

Key messages

- There is good evidence from clinical trials that mammographic screening reduces the death rate from breast cancer.
- The critique by Gøtzsche and Olsen has little merit and has generated much confusion.

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