Breast cancer screening case–control study design: impact on breast cancer mortality

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Background: Recent case–control studies on the effectiveness of population-based breast cancer screening show differences in the magnitude of breast cancer mortality reduction. We investigated the role played by aspects of the case–control study design on these differences, e.g. the definition of cases and exposure to screening.

Material and methods: We investigated six case–control studies conducted in East Anglia (UK), Wales, Iceland, central and northern Italy, South Australia and The Netherlands.

Results: The breast cancer mortality reduction in the different case–control studies ranged from 38% to 70% in the screened versus the nonscreened women. We identified differences in design, e.g. the inclusion or exclusion of the first years of screening, and the correction factor for self-selection bias.

Conclusions: Overall, the design of the case–control studies was similar. The differences in the magnitude of breast cancer mortality reductions are therefore unlikely to be caused by variations in the design of the case–control studies. These differences must be due to other factors, like the organisation of the service screening programme and the attendance rate. The reduction in breast cancer mortality estimated in these case–control studies indicates that the impact of current mammographic screening is at least consistent with the effect reported by the former randomised screening trials.

Key words: breast cancer screening, case–control studies, design

introduction

Recently, several case–control studies have been conducted to estimate the effect of population-based service screening programmes [1–4]. Their results show a decrease in breast cancer mortality which is at least consistent with the results of the randomised controlled trials (RCTs) conducted in the 1970s and 1980s. However, the magnitude of the effect, expressed in the odds ratio (OR), varies substantially between the different studies. We investigated the role of variations in the design of case–control studies on the differences in estimated effects of population-based breast cancer screening on breast cancer mortality.

Population-based screening aims to identify each eligible woman in the target population in the area served by a screening programme and to personally invite them to each organisational round of screening [5]. Continuous monitoring of the programme is required to better understand the positive results of screening and the negative outcomes, e.g. the number of false-positive screening outcomes. In addition, changes over time in baseline risk for breast cancer, screening methods and therapy also necessitate a periodic reassessment of efficacy.

A case–control study is an efficient method for estimating the benefit of cancer screening [6]. In 1986, Sasco et al. [7] suggested that a routine case–control assessment of the functioning of a mass screening programme could be, or even should be, an integral part of ongoing evaluation. The use of a case–control study includes a number of methodological challenges, like the definition and selection of cases and controls, the definition of exposure to screening and self-selection bias [8].

Conducting a case–control study begins with defining the source population. A source population should include women who have died of breast cancer (cases), women who have not died of breast cancer (controls) and population-based screening must be available to all members of the source population [9].

The use of death certificates is a valid way of classifying breast cancer as the underlying cause of death for the cases [10–12]. Controls should be selected from the source population that generated the cases. Furthermore, they should be free of breast cancer at the time of death in the source population [9, 13]. Care must be taken to ensure that the cases and controls have an equal opportunity for screening [8]. After identifying the cases and controls, the screening history of the cases is compared with the screening
history of the controls. If screening is effective, the cases have had less exposure to screening than the controls.

Self-selection bias has received a great amount of attention when using case–control studies for measuring the effectiveness of screening. It refers to the possible difference in baseline risk for breast cancer death a priori for women who accept the invitation to screening compared with those who do not accept the invitation to screening [8]. In the literature, contradictory results have been noted with regard to the direction and magnitude of self-selection. Where Friedman and Dubin found that women who accepted screening were at higher baseline risk for breast cancer mortality compared with a control group, Moss et al. found the opposite [8, 14, 15].

**Material and Methods**

**Literature Search**

A PubMed search was carried out for the period 2000 to 2010 to identify recent publications written in English on case–control studies that assessed the effect of a breast cancer service screening programme in steady state on breast cancer mortality. A study was included if it fulfilled the following conditions: the case–control studies had to be based on population-based screening programmes and had to include breast cancer deaths occurring in the steady state of screening, which we defined as breast cancer deaths after the year 2000.

The following search strategy was used: ‘mass screening’ [mesh] and (‘case–control studies’ [mesh] or ‘case–referent’) and ‘breast neoplasms/epidemiology’ [mesh]. We manually searched the bibliographies of recent reviews of the evaluation of service screening on breast cancer mortality for additional references [16–18]. In total, 121 studies were retrieved. Titles and, if necessary, abstracts found through the search strategy were evaluated for potential relevance.

Six articles of recent case–control studies in population-based service screening countries were identified: East Anglian region (UK) (Allgood et al. [1]); Wales (Fielder et al. [19]); Iceland (Gabe et al. [2]); The Netherlands (Paap et al. [20]); central and northern Italy (Puliti et al. [3]); and South Australia (Roder et al. [4]). From this point on, we refer to these studies using the name of the first author.

**Design Aspects of Case–Control Studies**

We focused on five design aspects of the selected case–control studies, which could influence the magnitude of the estimated ORs: the source population, the selection of cases, the selection of controls, the definition of exposure to screening and correction for potential selection bias [8, 13, 21]. If information was not available in the paper, we contacted the first author. The design of the six case–control studies is presented in Figure 1. In Figure 1, a timeline represents the change from a not-invited population to an invited population (source population) after the start of the implementation of a screening programme. From this invited population, a case and control were selected who both have a screening history from which the effectiveness of breast cancer screening can be estimated.

**Source Population.** According to the source population, we looked at differences in the age of invitation, e.g. women aged 40–49 years are not included in every country. This can lead to a different estimate of the impact of the screening programme. It is likely that breast cancer screening has less impact on breast cancer mortality in women aged 40–49 years compared with those aged 50–69 years. Including women aged 40–49 years in the analysis will therefore result in an OR closer to 1.

**Selection of Cases.** A distinction is made between primary breast cancer deaths (breast cancer as underlying cause of death) and secondary breast cancer deaths (breast cancer present at death). Inclusion of primary breast cancer deaths alone can give different results than when secondary breast cancer deaths are included; in this last case, the effect of screening is probably diluted.

An optimal screening effect is not achieved in the first years of screening because prevalent screen-detected cases benefit less [22]. Prevalent screen-detected cases are women diagnosed in the first round of screening or in a woman’s first test at a subsequent round [23]. Therefore, they dominate the first years of the implementation of screening programmes. Excluding breast cancer diagnosis and breast cancer deaths during the first years of implementation from the case–control study results in an estimate of the steady state of the screening programme.

**Selection of Controls.** For the controls, we focused on the methods used to ensure that they had the same opportunity for screening as the cases.

**Definition of Exposure to Screening.** The time window for including screening history should be limited to the detectable preclinical period (DPCP) [8, 24]. Including screening examinations outside the DPCP can distort the effect of screening. Therefore, differences in the time window of screening exposure can lead to differences in outcome. Etzioni and Weiss [21] found that underestimation of the duration of the DPCP leads to greater bias than overestimation. Both are expected to bring the OR closer to 1 [21, 25].

**Correction for Selection Bias.** In the six case–control studies, we checked which method and correction factor was used to correct for self-selection.

![Figure 1](https://example.com/image1.png)

**Figure 1.** Design of the case–control studies for screening. Inv1, invitation 1 of case and control; Inv2, invitation 2 of case and control; N-Inv, not-invited women; Inv, invited women.
bias, e.g. the method and correction factor of Duffy et al. [26]. Duffy et al. calculated a correction factor of 1.36 based on the relative rate of breast cancer deaths in non-attenders in the Two-County, Malmo, Gothenburg, Stockholm and Canadian national breast screening RCTs compared with women in the control arm of these RCTs.

Adjustment for socioeconomic status (SES) can contribute partly to the amount of self-selection bias [19]. As there is a correlation between SES and breast cancer risk, differences in SES in screened and nonscreened women are likely to have an influence on the effect estimate [27, 28].

Results

The start of the service screening in the countries of the six identified case–control studies was between 1987 and 1990. Table 1 provides details of the relevant background information of the service screening in each country. Table 2 shows an overview of the results of the analysis of women who attended screening compared with those who did not attend screening. The mortality reduction ranged from a minimum of 38% in Wales [OR 0.62, 95% confidence interval (CI) 0.47–0.82] to a maximum of 70% in The Netherlands (OR 0.30, 95% CI 0.14–0.63). These results do not include any corrections for self-selection.

Source population

All programmes invite women aged 50–69 years, with the exception of Wales, which invites women aged 50–64 years (Table 1). The UK and Australia allow women over 70 years to come to the screening, whereas The Netherlands actively invites women aged up to 74 years. Women aged 40–49 years are actively invited in Iceland and are allowed to take part in the service screening in Australia.

Selection of cases

Four case–control studies included primary breast cancer deaths only, Allgood and Fielder also included secondary breast cancer deaths (Table 3). The ORs calculated by Allgood and Fielder, 0.35 and 0.62, respectively, were no different to the other four studies. Allgood, Paap and Roder restricted the years of breast cancer diagnosis and the time period of breast cancer deaths, thereby excluding the prevalent cases in the years of the implementation of the screening programme. Allgood and Paap showed the lowest ORs of all case–control studies.

Selection of controls

Five case–control studies used a pseudo diagnosis for each control, which was equal to the date of diagnosis of that control’s matched case (Allgood, Fielder, Gabe, Puliti and Roder). To ensure an equal opportunity for screening, in the design phase, Puliti postponed the pseudo diagnosis for the controls matched to screen-detected cases by 1 year to compensate for lead time in the screen-detected cases [30]. Fielder did the same, but then as a secondary analysis and with a postponement of 18 months. This had no effect on the OR. Paap corrected for opportunity for screening by making sure that the control was invited for the same screening round from which the index invitation of the case emerged. In Gabe’s study, the correction for opportunity was based on a calculation described by Duffy et al. [31].

Exposure to screening

Primary analysis of the case–control studies was based on the complete screening history before diagnosis (case)/pseudo diagnosis (control); the ever/never comparison (Figure 1). Paap used an index invitation for exposure, which was defined as the invitation date closest to the date of diagnosis of the case.

Correction for selection bias

In all articles, corrections for self-selection bias were made using the method described by Duffy et al. [26] (Table 4).

Table 1. Background of the service screening programmes in the case–control studies

<table>
<thead>
<tr>
<th>Country</th>
<th>UK</th>
<th>Wales</th>
<th>Iceland</th>
<th>Netherlands</th>
<th>Italy</th>
<th>Australia</th>
</tr>
</thead>
<tbody>
<tr>
<td>National screening</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Cancer registry</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Area of case–control study</td>
<td>East Anglian region</td>
<td>Wales</td>
<td>Iceland</td>
<td>Southeast Netherlands</td>
<td>Northern and central Italy</td>
<td>South Australia</td>
</tr>
<tr>
<td>Age invited</td>
<td>50–70 active, 70+ allowed</td>
<td>50–64</td>
<td>40–69</td>
<td>50–74</td>
<td>50–69</td>
<td>50–69 active, 40–49 and 70+ allowed</td>
</tr>
<tr>
<td>Screening interval (years)</td>
<td>3</td>
<td>3</td>
<td>2</td>
<td>2</td>
<td>2</td>
<td></td>
</tr>
<tr>
<td>Attendance rate</td>
<td>75%&lt;sup&gt;a&lt;/sup&gt;</td>
<td>77%</td>
<td>61%–62%</td>
<td>82%</td>
<td>65%</td>
<td>57%&lt;sup&gt;b&lt;/sup&gt;</td>
</tr>
</tbody>
</table>

<sup>a</sup>www.cancerscreening.nhs.uk
<sup>b</sup>www.cancerscreening.gov.au

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Allgood and Fielder used the same correction factor as Duffy et al. (1.36). Roder mentioned in the discussion that the adjusted OR for self-selection using Duffy’s formula et al. would be around 0.70, thus indicating a 30% mortality reduction instead of 41% reduction with no correction for self-selection.

Instead of using the correction factor of 1.36, Gabe, Puliti and Paap used an area-specific correction factor, which were 1.17, 1.11 and 0.84, respectively. Gabe (personal communication with first author) calculated their correction factor as being the relative risk of breast cancer death for non-attenders compared with the uninvited population from the RCTs as estimated in the paper by Duffy et al. [26]. Puliti used a group of not-yet-invited women, which was available because of the long implementation period of the service screening in Italy. They compared the OR in the never respondents with that in the not-yet-invited. Paap used the incidence-based mortality method as a tool to calculate a correction factor for self-selection [23]. They calculated the incidence-based mortality for a group of women not-yet-invited during the implementation period of screening and compared this with the incidence-based mortality for the not-screened women in the same period. Corrections for SES were carried out by Allgood, Fielder and Roder and showed no alterations to the ORs.

discussion

Our study focused on the impact of differences in the design of recent case–control studies on the effect of breast cancer mortality in population-based breast cancer screening programmes. Although we found many minor differences in the set up of the six case–control studies, the overall design was quite similar. However, the range of the mortality reductions in the different case–control studies was large: from 38% reduction in Wales to 70% reduction in The Netherlands.

Looking at the differences in invited age groups, women aged 40–49 years were only included in the studies by Gabe and Roder. Roder included a stratified analysis of the effect of different age groups, which showed an OR of 1.18 in the age group <50 years and an OR of 0.54 (Table 2) in the age group 50–69 years. The change from an OR of 0.59 for all ages to an OR of 0.54 for the age group 50–69 years indicates that age of invitation plays no key role in the difference of the estimated OR in Australia compared with the other countries. A reason for this could be that there are fewer cases in the 40–49 age group than in those aged 50–69 years. Therefore, the OR of 0.59 will mainly consist of results for the latter group. Gabe did not show any age-specific results, but a large change in OR is not expected for the proportionally small numbers found in the younger age group.

A difference between the screening case–control studies is the inclusion or exclusion of the first years of screening. Analysis of the steady state gives lower ORs than the studies which included the first rounds of screening. This is also demonstrated in Fielder’s study, where the OR including diagnosis in the early years of screening was 0.62 (95% CI 0.47–0.82) and the OR excluding diagnosis in the early years of

<table>
<thead>
<tr>
<th>Study</th>
<th>Country</th>
<th>Ever–never, OR (95% CI)</th>
<th>Index invitation</th>
<th>Number of screens</th>
<th>Time since last screen (years)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Allgood et al. [1]</td>
<td>UK (East Anglian region)</td>
<td>0.35 (0.24–0.51)</td>
<td>0.56–4.03</td>
<td>2.51</td>
<td>1.03–2.80</td>
</tr>
<tr>
<td>Fielder et al. [19]</td>
<td>Wales</td>
<td>0.62 (0.47–0.82)</td>
<td>0.48–0.77</td>
<td>0.43</td>
<td>0.28–0.81</td>
</tr>
<tr>
<td>Gabe et al. [2]</td>
<td>Iceland</td>
<td>0.59 (0.41–0.84)</td>
<td>0.48–0.88</td>
<td>0.65</td>
<td>0.92–2.70</td>
</tr>
<tr>
<td>Paap et al. [20]</td>
<td>Netherlands (IKL region)</td>
<td>0.30 (0.14–0.63)</td>
<td>0.38–0.56</td>
<td>0.60</td>
<td>0.63–0.91</td>
</tr>
<tr>
<td>Puliti et al. [3]</td>
<td>Italy</td>
<td>0.46 (0.38–0.56)</td>
<td>0.38–1.03</td>
<td>0.63</td>
<td>0.36–1.30</td>
</tr>
<tr>
<td>Roder et al. [4]</td>
<td>Australia</td>
<td>All ages: 0.59 (0.47–0.74)</td>
<td>0.22–0.80</td>
<td>0.42</td>
<td>0.18–0.96</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Age 50–69: 0.54 (0.41–0.72)</td>
<td>0.31–1.42</td>
<td>0.67</td>
<td>0.18–0.88</td>
</tr>
</tbody>
</table>

aNumber of screens and time since last screen corrected for SES and health service access.

bFrequency of recent screening: ≥3 screening rounds at ≤30-month intervals immediately preceding diagnosis.

CI, confidence interval; IKL, Comprehensive Cancer Centre Limburg; OR, odds ratio; SES, socioeconomic status.
screening was 0.49 (95% CI 0.36–0.66). Excluding the early years of screening in the studies by Gabe and Puliti will probably bring the effect on breast cancer mortality closer to the effect found by Allgood and Paap. Roder, however, did not find a large impact of the service screening programme compared with Allgood and Paap. This could perhaps be due to the lower attendance rate in Australia (57%) compared with the UK (75%) and The Netherlands (82%).

All case–control studies aimed to ensure an equal opportunity for screening for both the controls and the cases. Fielder showed no alteration in the OR. A sensitivity analysis carried out by Puliti for time lags of 6 months and 1.5 years

### Table 3. Design aspects of recent case–control studies

<table>
<thead>
<tr>
<th>Country</th>
<th>UK</th>
<th>Wales</th>
<th>Iceland</th>
<th>Netherlands</th>
<th>Italy</th>
<th>Australia</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cases</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number</td>
<td>284</td>
<td>419</td>
<td>226</td>
<td>118</td>
<td>1750</td>
<td>491</td>
</tr>
<tr>
<td>Breast cancer deaths</td>
<td>Primary or secondary</td>
<td>Primary or secondary</td>
<td>Primary</td>
<td>Primary</td>
<td>Primary</td>
<td>Primary</td>
</tr>
<tr>
<td>Death certificate</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Other</td>
</tr>
<tr>
<td>Restriction for years of diagnoses</td>
<td>From 1995</td>
<td>From 1991</td>
<td>From start service screening</td>
<td>From start service screening</td>
<td>From the year before start of service screening</td>
<td>From 1994</td>
</tr>
<tr>
<td>Controls</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number</td>
<td>568</td>
<td>717</td>
<td>902</td>
<td>118</td>
<td>7000</td>
<td>1473</td>
</tr>
<tr>
<td>Matching variables</td>
<td>Date of birth</td>
<td>Year of birth, one from same GP, one from other GP</td>
<td>Year of birth, area of residence</td>
<td>Year of birth, area of residence at index invitation of case</td>
<td>Date of birth, area of residence</td>
<td>Date of birth</td>
</tr>
<tr>
<td>Matching factor</td>
<td>1 : 2</td>
<td>1 : 2 or 1 : 1</td>
<td>1 : 3 or 1 : 4</td>
<td>1 : 1</td>
<td>1 : 4</td>
<td>1 : 3</td>
</tr>
<tr>
<td>Screening exposure</td>
<td>Ever/never attendance before (pseudo) diagnosis</td>
<td>Ever/never attendance before (pseudo) diagnosis</td>
<td>Ever/never attendance before (pseudo) diagnosis</td>
<td>Screened at index invitation</td>
<td>Ever/never attendance before (pseudo) diagnosis</td>
<td>Ever/never attendance before (pseudo) diagnosis</td>
</tr>
</tbody>
</table>

*aIn the paper of Allgood contributory is used instead of secondary.

*bPersonal communication with first author.

The decision on cause of death was made by cancer registry staff using death records, clinical extracts from hospital records, pathology records and occasionally consultation with treating clinicians.

*aIn the paper of Gabe screening area is used instead of area of residence.

GP = general practitioner

### Table 4. ORs corrected for self-selection bias

<table>
<thead>
<tr>
<th>Study</th>
<th>Country</th>
<th>Crude, OR (95% CI)</th>
<th>Factor Duffy 1.36</th>
<th>Using own correction factor</th>
<th>SES</th>
</tr>
</thead>
<tbody>
<tr>
<td>Allgood et al. [1]</td>
<td>UK (East Anglian region)</td>
<td>0.35 (0.24–0.51)</td>
<td>0.52 (0.32–0.84)</td>
<td>0.35 (0.23–0.51)</td>
<td></td>
</tr>
<tr>
<td>Fielder et al. [19]</td>
<td>Wales</td>
<td>0.62 (0.47–0.82)</td>
<td>0.75* (0.49–1.14)</td>
<td>No difference</td>
<td></td>
</tr>
<tr>
<td>Gabe et al. [2]</td>
<td>Iceland</td>
<td>0.59 (0.41–0.84)</td>
<td>0.65 (0.39–1.09) (Factor = 1.17, 95% CI 1.08–1.26), is adjusted for opportunity bias as well</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Paap et al. [21]</td>
<td>Netherlands (IKL region)</td>
<td>0.30 (0.14–0.63)</td>
<td>0.24 (0.10–0.58) (Factor = 0.84, 95% CI 0.58–1.21)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Puliti et al. [3]</td>
<td>Italy</td>
<td>0.46 (0.38–0.56)</td>
<td>0.55 (0.36–0.85) (Factor = 1.11, 95% CI 0.87–1.40)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Roder et al. [4]</td>
<td>Australia</td>
<td>0.59 (0.47–0.74)</td>
<td>0.70</td>
<td>0.59 (0.47–0.74), is adjusted for health service access as well</td>
<td></td>
</tr>
</tbody>
</table>

*Did only correct for self-selection on tumours diagnosed in 1995–2001, where the crude OR was 0.49 (0.36–0.66).

CI, confidence interval; OR, odds ratio; SES, socioeconomic status.
instead of 1 year showed only small alterations in the OR. Gabe showed a change in the OR from 0.59 (95% CI 0.41–0.84) to 0.51 (95% CI 0.31–0.86) after correction for opportunity for screening, thus only a small change in the OR. Gabe based his correction on a method developed by Duffy et al. [31]. Using the correction method of Duffy et al. will already partly adjust for the prevalent screens in the first years of screening. The study by Duffy et al. also corrected the OR of Fielder for opportunity for screening using the same method, which did not lead to a change in the OR. These results show that the impact of opportunity for screening on the magnitude of the OR is limited.

The analyses for exposure to screening are similar in all case–control studies except Paap, who used exposure to the index invitation as their primary analysis instead of using the ever/never comparison. The screening history should be limited to the period when the cancer is potentially detectable on the mammogram—in the DPCP. The exact duration of the DPCP is not known, but in the future, when service screening has been running for a longer period, the ever/never comparison will underestimate the length of the DPCP, which would cause a false reduction of the size of the benefit of screening [21, 25]. By only using the index invitation, Paap has probably underestimated the length of the DPCP. Extending the exposure to screening, to the invitation preceding the index invitation, resulted in a small change in mortality reduction from 70% to 73% (OR 0.27, 95% CI 0.12–0.62).

By correcting the OR for self-selection bias using the correction factor of 1.36 calculated by Duffy et al., two assumptions are made: the relative mortality in the noncompliers compared with the not-yet-invited is the same in the service screening programmes as it is in the RCTs and self-selection in Sweden and Canada is the same as in the countries where the case–control studies are carried out. This may not hold for most populations. For example, a high attendance rate, like in the two-county trial, will make the group of women not attending the screening more special [26]. The area-specific calculated correction factors did not indicate a very high impact of self-selection on service screening compared with no impact of self-selection (OR 1). The correction factor for Italy calculated from not-yet-invited cases and never-screened cases was 1.11 (95% CI 0.87–1.40), the correction factor of Gabe was 1.17 (95% CI 1.08–1.26) and the correction factor of Paap calculated from the incidence-based mortality method was 0.84 (95% CI 0.58–1.21).

The adjustments for SES had no effect on the ORs of the case–control studies, indicating a minor influence of self-selection bias on the effect estimate of screening on breast cancer mortality as well. Furthermore, to assist the interpretation of the case–control study, Roder also included a population survey to identify potential differences in risk profiles by screening participation. This survey showed that predictors for screening participation were a higher number of first-degree female relatives with a history of breast cancer, exposure to hormone replacement therapy and a history of breast surgery for any reason. This suggests that screening participants may have a higher background risk for breast cancer than non-participants. On the other hand, Lawrence et al. showed that the 10-year relative survival rate in never attenders (51.9%) was lower than the survival rate in women diagnosed before invitation (67.6%). This was in contrast with the survival rate of occasional non-attenders, who had a relative survival rate of 66.9% [32]. This indicates that the correction factor for self-selection is influenced by the type of non-attender as well.

So it seems that the correction factor of 1.36 is not valid in other screening programmes to correct for self-selection and should not be used in future case–control studies. Instead, region- or country-specific correction factors for self-selection should be estimated. More emphasis should be placed on exploring methods like the incidence-based mortality method to calculate differences in the underlying breast cancer mortality risk in the screened and not-screened populations to determine the impact of self-selection in the service screening.

Overall, the design of the six case–control studies was similar. However, inclusion of the first years of screening and the correction factor used to correct for self-selection bias can probably explain some of the differences in the magnitude of the effect in the recent population-based case–control studies. Other factors must influence the ORs of the case–control studies, e.g. differences in attendance rate, in the quality and organisation of the screening programme and in the quality of treatment. The range of obtained mortality reduction was 38%–70% in the six case–control studies (25%–76% after correction for self-selection). This indicates that the impact of current mammographic screening is at least consistent with effect (25%–30%) reported by the former randomised screening trials.

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disclosure
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references