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ORIGINAL PAPER

The effects of organized screening programs on the demand for mammography in Switzerland

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Abstract The objective of this study is to estimate the causal effect of organized mammography screening programs on the proportion of women between 50 and 69 years of age who have ever used mammography. We exploit the gradual implementation of organized screening programs in nine Swiss cantons using a difference-in-difference approach. An analysis of four waves of the Swiss Health Survey shows that 3.5–5.4% points of the 87.9% utilization rate in cantons with screening programs in 2012 can be attributed to these organized programs. This effect indicates that organized programs can motivate women who have never done mammography to initiate screening.

Keywords Mammography · Screening · Switzerland · Difference-in-difference

JEL Classification I11 · I18

Introduction

Breast cancer is the most frequent cancer affecting women in Switzerland and the greatest cause of death in women under 70 years old [1]. Because the tumor stage at diagnosis is a relevant predictor of overall survival [2], many European countries have implemented organized mammography screening programs for the early detection of neoplasms. Mammography is a type of low-energy X-ray that examines the human breast, and it is currently the most common technique for the detection of breast cancer in industrialized countries [3]. Although regular mammography screening has been shown to decrease cancer-related mortality [4–6], it has come under increased criticism because it produces a considerable number of false-positive and false-negative diagnoses and detects neoplasms that would never have caused problems (overdiagnosis). The consequences of false-positive diagnoses and overdiagnoses are anxiety, higher costs, and damage from interventions for benign and non-obligate precursor lesions [7–9]. False-negative diagnoses, by contrast, lead to undertreatment of neoplasms associated with higher mortality and treatment costs. A recent systematic review reported a rate of sensitivity (true-positive rate) between 64 and 67% and specificity (true-negative rate) between 85 and 97% [10]. Over a 10-year period, 49% of all screened women receive a false-positive diagnosis [11]. In their recent systematic review, Gøtzsche et al. [4] conclude that for every life saved, ten healthy women will be treated unnecessarily, and more than 20% of the screened women experience psychological distress. Based on this evidence, a health technology assessment published by the Swiss Medical Board in 2013 [12] recommended to suspend the cantonal organized screening programs in Switzerland.

However, an assessment of organized mammography screening programs should not only consider the clinical efficacy of the procedure itself but also the effects of the organized programs on screening uptake. Screening uptake is important because it can be correlated with socioeconomic status, and organized programs have the potential to reduce socioeconomic inequality in screening uptake and premature mortality. Knowledge of the characteristics of women who respond more strongly to organized screening programs can also help to make

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them more effective. A systematic review by Schueler et al. [13] showed that women with a low educational level, low income, limited access to care, no insurance, and poor knowledge of screening were less likely to use mammography. Similarly, Wübker [14] found that 50- to 69-year-old women with higher education, a good-quality family physician, and more previous physician visits were more likely to have undergone mammography. A Danish study [15] also identified previous contact with a physician as an important predictor of mammography screening uptake, and a recent study by Bouckaert and Schokkaert [16] documented a significant income-related gradient in breast cancer screening uptake. The reported effects of income, health information, and access to care on mammography use suggest that the provision of cost-free access to mammography, information about the benefits of screening, and a list of preferred providers can increase screening uptake. This hypothesis was confirmed in a study investigating the effects of organized screening programs on the proportion of regularly screened women in 13 European countries on the basis of the Survey of Health, Ageing and Retirement in Europe (SHARE). Wübker [17] showed that screening programs explained a large proportion of the variation in screening rates between countries and concluded that organized programs are effective in increasing the screening rate. This result was echoed by a Spanish study reporting higher breast cancer screening participation for women in the target population of a regional screening program [18]. Moreover, organized screening programs can reduce socio-economic inequalities in mammography uptake. Carrieri and Wübker [19] found that regional organized screening programs in European countries reduced education-related inequalities in mammography uptake, and an evaluation of the 2006 Massachusetts Health Care Insurance Reform showed that universal coverage can increase mammography utilization particularly among women with low household incomes [20].

Previous studies mainly focus on mammography use at a given point in time and use cross-sectional data. The objective of our study is to investigate the effects of organized screening programs on screening initiation using repeated cross-sectional data and a quasi-experimental design. The main contribution lies in the analysis of screening initiation as a measure of the demand for mammography and in the identification of the causal effects of the organized programs. The analysis exploits the gradual implementation of organized mammography screening programs in nine Swiss cantons using a difference-in-difference framework. In the sensitivity analysis, we relax the assumption of a common trend in the participation rate between screening and non-screening cantons and include women between 40 and 49 years of age as an additional comparison group in a difference-in-difference-in-difference specification.

The remainder of this article is organized as follows: Sect. 2 describes the characteristics of the cantonal organized programs and the circumstances of their introduction. Sects. 3 and 4 describe the empirical strategy and the analyzed data. The results of the base-case and sensitivity analyses are described in Sect. 5 followed by a discussion of the results in Sect. 6.

Policy background

Breast cancer accounts for approximately one-third of all newly detected neoplasms among women in Switzerland (Fig. 1a) [21]. The incidence rate in the general female population increased continuously between 1988 and 2012 (Fig. 1b). One reason for this trend was the pronounced increase in the age group of 60- to 69-year-old women between the years 1988 and 2000. The Italian- and French-speaking (Latin) parts of Switzerland exhibited higher cancer incidence rates than the German-speaking part throughout the entire period of 1988–2012. An estimated 32,643 patients lived with an up to 10-year-old diagnosis of breast cancer in 2015 [21]. The prevalence rate among 50- to 59-year-old women decreased over the 2005–2015 period while it increased markedly in women over 60 years between 2000 and 2005 and continued to increase in 70- to 79-year-old women until 2010 (Fig. 1c). The incidence rates are unlikely to be affected by advances in cancer therapy or by the implementation of organized mammography screening programs. The general increase in the prevalence rate and the shift towards older patients, however, could be the consequence of reduced mortality rates due to more effective therapies or early detection.

Organized mammography screening programs were gradually implemented by the health administrations of nine cantons, hereafter called screening cantons (Fig. 2). During the observation period of this study, all women between 50 and 69 years of age who lived in a canton with an active screening program received biennial invitation letters with a medical questionnaire that they must bring to the consultation with the radiologist (see Table 1).

These organized programs also affected the costs of screening mammography for women in the target group. In Switzerland, payment for mammography is regulated by the TARMED fee-for-service tariff system [23]. At an average value of CHF 0.90 per TARMED tariff point, a mammogram with an evaluation of the images by one physician costs CHF 147. Each additional assessment by another physician costs an extra CHF 35. Within organized screening programs, each mammogram must be assessed

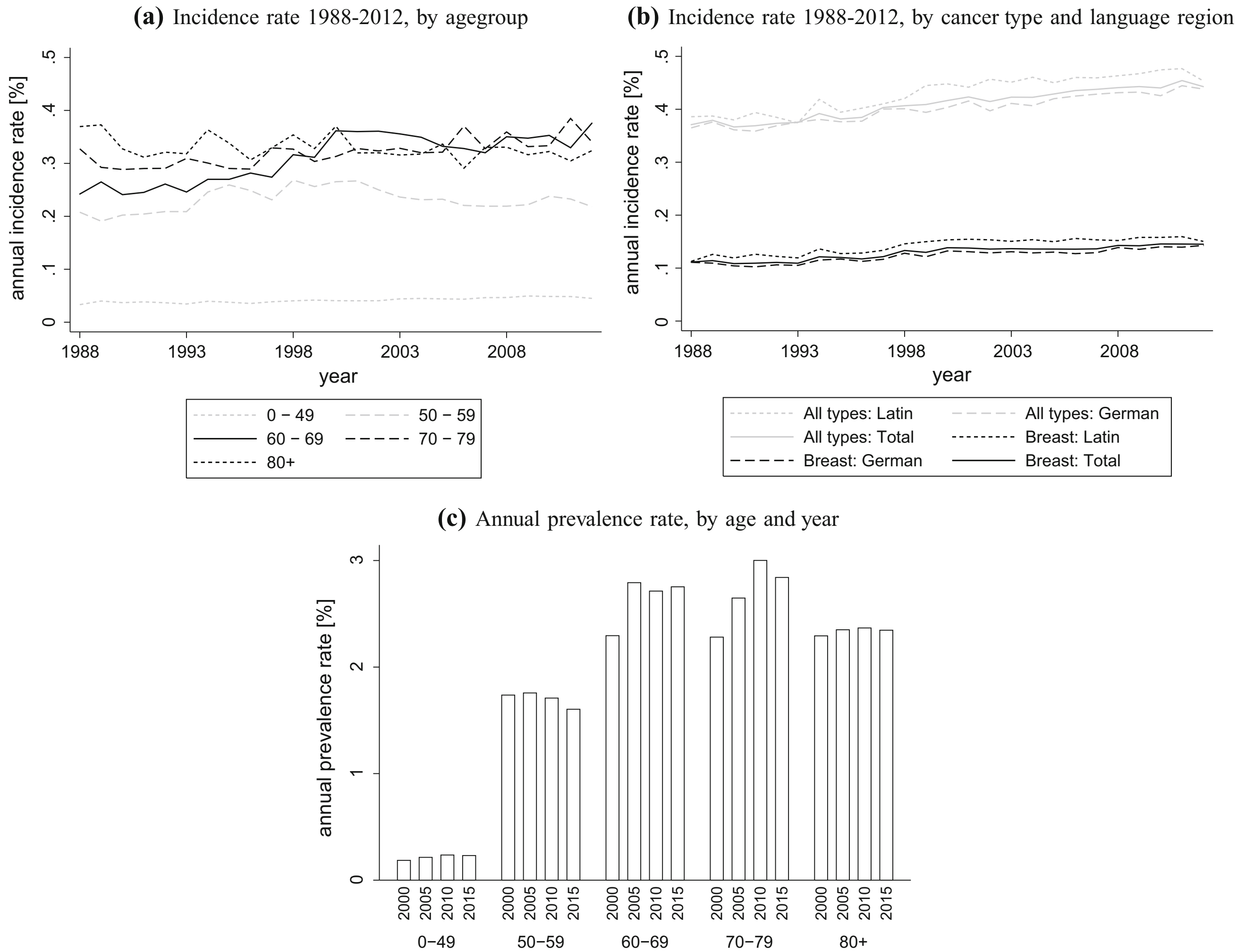

Fig. 1 Breast cancer incidence and prevalence rates in the Swiss female population. (source: National Institute for Cancer Epidemiology and Registration [22], own presentation)

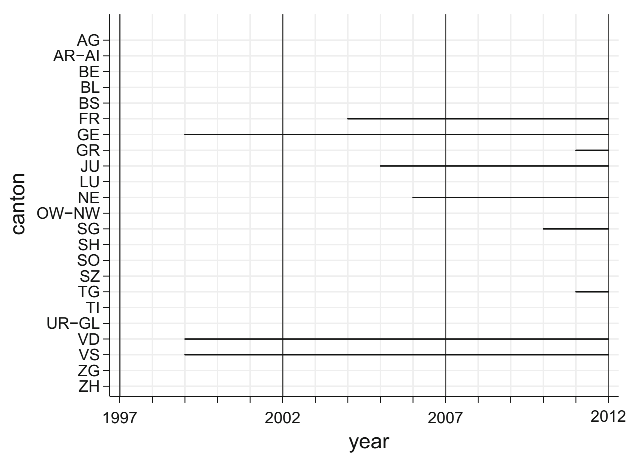

Fig. 2 Introduction of organized screening programs by canton. (source: Own presentation, http://www.swisscancerscreening.ch, State Council of the Canton of Basel-Stadt [33])

by at least two physicians, sometimes even three, which leads to an average cost of approximately CHF 200 per screening mammogram [12].

Before 1999, mammography was reimbursed by statutory health insurance only if it was for diagnostic purposes or if a woman had a family history of breast cancer. Beginning in 1999, screening mammograms were covered by statutory health insurance if they were conducted in a screening canton [24]. Beginning in 2001, screening mammography became exempt from a deductible in screening cantons. Thus, women living in screening cantons had to pay an out-of-pocket expense of only 10% of the costs, whereas women in non-screening cantons had to bear the full costs of screening themselves [25]. In the canton of Valais (VS) and Genève (GE), even out-of-pocket expenses were covered by the government or the local chapter of the Swiss Cancer League [24].

Table 1 Characteristics of Swiss organized mammography screening programs

Target population	All women between 50 and 69 years of age
Invitation letters	All women in the target population receive biennial invitation letters followed by reminder letters. In some screening cantons, the invitation letters contain a predetermined appointment with a radiologist or a list of preferred radiologists. Certain programs provide a Web- or phone-based booking system
Health questionnaire	With the invitation letter, the women receive a medical questionnaire that they must bring to the consultation with the radiologist
Information campaign	All programs include information flyers and brochures that are distributed among the population or in medical practices. All organized programs have Web pages that are listed on http://www.swisscancerscreening.ch
Preferred providers	Most screening cantons publish a list of preferred providers
Quality assurance	The certified providers receive special training and are regularly monitored for quality
Electronic patient data	The medical files created in the consultations with the radiologists must be stored electronically
Evaluation	The screening programs were evaluated by Swiss Cancer Screening [34]. The outcomes included the number of screenings, the diagnostic accuracy, the characteristics of the detected carcinomas and the subsequent service utilization for cancer treatment

Although the organized programs were designed to send invitation letters to all women in the target population, some programs did not reach full coverage in their first year or were preceded by a pilot project. In the canton of Vaud, a pilot project was initiated in three regions in 1993, 6 years before the actual cantonal program was established [24]. In the program of the canton of Jura, the coverage rate was not 100% when it was established in 2005 because the Jura region of the canton of Bern, which we coded as part of the canton of Jura only joined in 2009. In many cantons, the coverage rate increased gradually because the delivery of invitation letters was staggered for logistic reasons. Most screening programs published or sent a list of preferred providers, some provide a Web- or phone-based booking system, and in the cantons of Graubünden (GR) and St. Gallen (SG), the invitation letter even included a predetermined appointment with a radiologist.

The introduction of the cantonal screening programs followed a distinct regional pattern. The first six programs were introduced in the French-speaking part of Switzerland (Genêve, GE; Vaud, VD; Valais, VS; Fribourg, FR; Jura, JU; Neuchâtel, NE) [24], while the last three programs (St. Gallen, SG; Graubünden, GR; Thurgau, TG) were introduced in the eastern (German-speaking) part of the country. These regions differ from each other and the rest of the country in several respects. First, the French- and Italian-speaking cantons exhibited higher breast cancer incidence rates throughout the observation period of this study (Fig. 1) [1]. Second, breast cancer care varies significantly across regions with a pronounced difference between the eastern and the French-speaking part of the country. A study investigating regional variation in breast cancer care in Switzerland found that the cantons of St. Gallen and Graunbünden exhibited higher mastectomy rates, lower reconstruction rates, and less frequent use of a sentinel node procedure than five comparison regions [26]. The study also showed regional variation in the prescription of endocrine therapy and chemotherapy. A study comparing eight cantons of Switzerland reported higher overall survival rates of breast cancer patients in the cantons of Genêve and Valais even before the introduction of the first programs [27]. Third, the density of radiologists per resident differs significantly across screening cantons and the timing of the introduction seems to be related to this provider density. The first three cantons of Genêve, Vaud, and Valais belong to the four cantons with the highest density of radiologists, the three following French-speaking cantons can be found in the mid-range, and the three late adopters appear at the bottom of the ranking [28]. Fourth, voters in the French-speaking part regularly reveal different preferences on the organization of the health care system than the rest of the country. The approval rates for a law that stipulated managed care insurance plans in mandatory health insurance in 2012 were much lower [29], and the support for a unified health insurance fund instead of the private providers of mandatory health insurance has been higher in the French-speaking part of Switzerland [30]. Fifth, health care expenditures in the French- and Italian-speaking part of Switzerland are higher than in the rest of the country even when demand-side and supple-side factors are controlled for [31, 32]. The systematic differences across Swiss cantons in breast cancer incidence rates, breast cancer care, provider density, voting behavior and health care expenditures underline the need for controlling for the particularities of Swiss cantons in a regression analysis.

Methods

The objective of this study is to estimate the effect of organized mammography screening programs on screening initiation in screening cantons. The dependent variable y_i indicates whether a woman has ever undergone a mammography and thus captures the decision to begin using mammography. Economic theory suggests that patients decide when to seek medical care, whereas physicians influence treatment decisions such as the number and interval of follow-up visits [35, 36]. The unobserved latent propensity y_i^* to have ever used mammography can therefore be interpreted as a function of the demand for mammography. A woman chooses to undergo her first mammogram ($y_i = 1$) if her net benefit from undergoing the procedure is positive ($y_i^* > 0$), and she will choose no mammography ($y_i = 0$) otherwise.

$$y_i^* = x\beta + u \tag{1}$$

$$y_i = \begin{cases} 1 & \text{if } y_i^* > 0 \\ 0 & \text{if } y_i^* \leq 0 \end{cases} \tag{2}$$

Model specification

The empirical approach of this study explores the gradual implementation of organized screening programs in nine Swiss cantons using a difference-in-difference model in a repeated cross-section of four survey waves (1997, 2002, 2007, 2012). The policy effect is identified by the change in the proportion of 50- to 69-year-old women who have ever done mammography after the introduction of the organized programs.

The variable s_i indicates whether an organized mammography screening program was active in a respondent's canton at the time of the interview. To control for canton and time fixed effects, we include binary indicators of the canton of residence c_i and the year of the survey wave t_i. Individual characteristics x_i are used to adjust for compositional changes in the populations of screening and non-screening cantons, and standard errors were clustered at the canton level. This model identifies the average effect of all organized screening programs on the screening initiation rate under the assumption that the screening and non-screening cantons shared the same general trend described by β_t.

Because a woman who became a user of mammography cannot reverse this decision, the effect of an organized program on the probability to have ever used mammography should increase over time. We test for this trend by introducing a variable d_i to indicate the number of years that an organized program has been active in the canton of residence at the time of the interview. Using this specification, we divide the effect of the organized programs into an immediate effect β_s and a long-term trend β_d. As the number of women who have not yet undergone their first mammogram decreases, the increase in the policy effect is expected to diminish. We therefore assess the fit of non-linear specifications of d_i using the modified Hosmer–Lemeshow test.

$$P[y_i = 1 | x_i, c_i, t_i, s_i, d_i] = \alpha + x_i\beta_x + c_i\beta_c + t_i\beta_t + s_i\beta_s + d_i\beta_d + u_i \tag{3}$$

We use this difference-in-difference model to estimate the proportion of the utilization rate in screening cantons in 2012 that can be attributed to the organized screening programs or, in other words, the average treatment effect on the treated (*ATET*). The *ATET* is the average difference between the predicted probability of women living in screening cantons in 2012 to have ever used mammography and the counterfactual probability of these women choosing to undergo mammography in the absence of the organized program. The year 2012 was used to hold the incremental effect of the time trend constant in this exercise.

$$\begin{aligned}\text{ATET} = \mathbb{E}\{&\hat{P}^{s_i=1,t_i=2012}[y_{ict} = 1 | x_i, c_i, t_i, s_i, d_i] \\ &- \hat{P}^{s_i=1,t_i=2012}[y_{ict} = 1 | x_i, c_i, t_i, s_i = 0, d_i = 0]\}\end{aligned} \tag{4}$$

The choice of the binary indicator of having ever used mammography as the dependent variable has important implications for the interpretation of β_s and β_d. The coefficients β_s and β_d only measure the effect of the organized programs on screening initiation of women who would never have undergone a mammography without the program. The binary dependent variable y_{ict} does not measure whether the programs motivated women to have more than one mammogram. Hence, our study only informs policymakers about the effects of organized programs on screening initiation among women who have never undergone mammography but not on the proportion of regularly screened women or on the frequency of mammography screening in the target population.

Functional form

The estimation of a difference-in-difference specification with a binary dependent variable poses certain difficulties when choosing an appropriate empirical model. Popular candidates are the linear probability model and the unconditional fixed-effects logit model. The linear probability model is easy to interpret, fulfills the standard difference-in-difference assumptions, and allows the computation of the *ATET* as $\hat{\beta}_s + \bar{d}\hat{\beta}_d$ [37]. A disadvantage of the linear probability model is that it is inefficient when the errors are non-normal or heteroskedastic, and it may yield biased and inconsistent parameter estimates when the model makes out-of-range predictions [38]. The logit model is a possible solution to the problem of out-of-range predictions because it accounts for the bounded nature of the dependent binary variable. The regression coefficients β of non-linear models cannot be interpreted in terms of probabilities but can be converted into average marginal effects, B_k.

$$B_k = \frac{\Delta\mathbb{E}\left[logit(\alpha + \sum_{k=1}^{K} x_{ik}\beta)\right]}{\Delta x_k} \tag{5}$$

We chose the fixed-effects logit model over the linear probability model based on the Pregibon link test, the Ramsey RESET test and the modified Hosmer–Lemeshow test. The linear probability model was rejected in all three tests while the logit model passed them well. The modified Hosmer–Lemeshow tests of the linearity of responses over quantiles of continuous covariates (age, income, alcohol, duration) favored a quadratic specification of age and a cubic specification of income. Based on these tests, we determined that the fixed-effects logit model with higher-order age and income terms fits our data best.

Non-linear binary choice models have two major limitations when estimating difference-in-difference specifications. First, the causal effect of the policy intervention is not identified by the coefficients β_s and β_d because their incremental effects depend on the expected outcomes of individuals which vary across the population [37, 39]. The average marginal effects of β_s and β_d do not capture the true policy effects because the marginal effects assess the incremental effects in the entire population instead of the treatment group only, and β_d only indicates the effect of a unit change in the duration of the programs instead of the observed durations. The ATET is more meaningful than the average marginal effects because it represents the estimated change in the average outcome among those individuals who were actually exposed to the intervention and it considers the observed changes in the treatment variables. Because the ATET is always positive when both β_s and β_d are positive, the signs of these coefficients can be interpreted if they are same direction, and the standard errors can be computed by the delta method [40].

$$\begin{aligned} &\widehat{ATET}(s=1, t=2012) \\ &= \mathbb{E}\left[logit(\hat{\alpha} + x_i\hat{\beta_x} + c_i\hat{\beta}_c + \hat{\beta}_{2012} + \hat{\beta}_s + d_i\beta_d)\right] \\ &\quad - \mathbb{E}\left[logit(\hat{\alpha} + x_i\hat{\beta_x} + c_i\hat{\beta}_c + \hat{\beta}_{2012})\right] \end{aligned} \tag{6}$$

A second limitation of non-linear models is that difference-in-difference models are a type of fixed-effects model that introduces the incidental parameters problem. The maximum likelihood estimation of fixed-effects models "need not be consistent" because the number of parameters increases with the number of groups [41]. Although a small simulation study by Heckman [42] found the bias in a fixed-effects probit model with eight observations per group to be surprisingly small, subsequent research by Greene [43] showed that this result was incorrect. A more recent study by Katz [44] suggested that the unconditional fixed-effects logit estimator can safely be used when the group size exceeds 16 observations. Although the evidence is inconclusive, we argue that the average number of 93.6 women per canton and year (Table 7 in appendix 1) is sufficiently large to yield consistent and efficient parameter estimates.

Sensitivity analysis

Effect heterogeneity across socioeconomic groups

In the first sensitivity analysis, we assess the heterogeneity of policy effects across socioeconomic groups. Researchers have shown that women with higher incomes and better education are more likely to be screened [13, 14, 16, 45, 46]. It is possible that these inequalities reflect differences in constraints rather than differences in preferences. The heterogeneity of the policy effects across socioeconomic groups is thus an important outcome in an evaluation of organized screening programs.

We add interaction terms between the screening variable s_i and indicators of the educational level e_i and the position in the income distribution r_i to the base-case model in Eq. (3). We also include the interaction terms of e_i and r_i with the canton and time fixed effects c_i and t_i to adjust the policy effects for inter-temporal and geographical variation in the association between mammography use and socioeconomic status. To ensure simplicity, we neglect the duration of the organized programs d_i in this sensitivity analysis.

$$\begin{aligned} P[y_i = 1|x_i, c_i, t_i, s_i, d_i] &= \alpha + x_i\beta_x + c_i\beta_c + t_i\beta_t + s_i\beta_s \\ &\quad + e_i\beta_e + r_i\beta_r + e_ic_i\beta_{ec} + e_it_i\beta_{et} + r_ic_i\beta_{rc} + r_it_i\beta_{rt} \\ &\quad + e_is_i\beta_{es} + r_is_i\beta_{rs} + u_i \end{aligned} \tag{7}$$

Placebo intervention tests

In the second sensitivity analysis, we test the common trend assumption using two placebo intervention tests. The first placebo intervention emulates the introduction of the organized programs 5 years before their actual implementation. The leading placebo intervention variable thus takes the value one in the wave before an organized screening

Table 2 Variable description

Variable	Description	Range/unit
evermam	Ever had a mammography	{0,1}
target	Age at the time of the interview = 50–69 years	{0,1}
screening	Organized program established in the canton of residence at the time of the interview	{0,1}
duration	Time since the establishment of the organized program at the time of the interview	[years]
age	Age in the year of the interview	[years]
married	Marital status	{0,1}
urban	Living in an urban area	{0,1}
foreign	Foreign nationality	{0,1}
educ1	Highest education = mandatory schooling	{0,1}
educ2	Highest education = professional education	{0,1}
educ3	Highest education = a-level degree	{0,1}
educ4	Highest education = higher professional education	{0,1}
educ5	Highest education = university degree	{0,1}
working	Working a paid job	{0,1}
income	Monthly net income adjusted for the number of household members	[CHF 1000]
smoker	Regular smoker	{0,1}
alcohol	Weekly alcohol intake	[g]
movdays0-7	Physical activity over 0–7 days	{0,1}
overweight	Body mass index ≥ 25	{0,1}
obese	Body mass index ≥ 30	{0,1}
lifestyle	Health-oriented lifestyle	{0,1}
health1	Self-rated health = 1 (very poor)	{0,1}
health2	Self-rated health = 2	{0,1}
health3	Self-rated health = 3	{0,1}
health4	Self-rated health = 4	{0,1}
health5	Self-rated health = 5 (very good)	{0,1}
badhealth	Self-rated health $\in \{1, 2\}$	{0,1}
hormones	Currently in hormone therapy	{0,1}
cancer	Treated for cancer during the last 12 months	{0,1}
dedhi	Deductible above the minimum at the time of the interview	{0,1}
suppins	Supplementary private insurance plan	{0,1}
modalt	Alternative managed care insurance plan	{0,1}

program has been implemented in a screening canton. This variable captures deviating trends in screening cantons before the introduction of organized programs.

The second placebo intervention test is based on 1000 estimations of the regression model including a random placebo intervention variable in the comparison group. The random placebo intervention variable is constructed at the canton level and takes the value one if an interview was carried out after a randomly determined placebo intervention year in the canton of residence. The random placebo variable is only correlated with the actual screening variable through time and thus captures changes in the difference between non-screening cantons and screening cantons over time that are unrelated to the presence of organized programs in screening cantons.

Difference-in-difference-in-difference estimation

In the third sensitivity analysis, we relax the common trend assumption and estimate a difference-in-difference-in-difference specification in which women between the ages of 40 and 49 years who do not receive invitation letters and who must pay for screening mammograms themselves constitute an additional comparison group. This model describes how the organized programs affect the difference in the utilization rate between the group of women aged 50–69 (i.e., the target group) and those aged 40–49 (i.e., the comparison group).

Let g_i be an indicator of belonging to the target group. The coefficient β_g measures the average difference in mammography use between the target group and the comparison group. Because the initial difference between the target and comparison groups may not be the same in the screening and non-screening cantons, we observe the interaction of g_i with the canton fixed effects c_i. We also allow for differential trends through an interaction between g_i and the year of the survey wave t_i.[1] The coefficient β_s captures the change in the average utilization rate of the comparison group during the introduction of the organized programs. The coefficient β_{sg} measures how the organized programs affect the difference between the target and comparison groups. This model identifies the causal policy effect in the target group under the assumption that the difference between the target and comparison groups would have evolved similarly in screening and non-screening cantons.

$$P[y_i = 1|x_i, c_i, t_i, g_i, s_i] = \alpha + x_i\beta_x + c_i\beta_c + t_i\beta_t + g_i\beta_g + s_i\beta_s + g_ic_i\beta_{gc} + g_it_i\beta_{gt} + s_ig_i\beta_{sg} + u_i \quad (8)$$

Under the relaxed common trend assumption, the coefficient β_s captures the difference in time trends, and the coefficient β_{sg} measures the causal policy effect on the utilization rate in screening cantons adjusted for the differential time trends if the organized programs did not have any spillover effects on younger women. The *ATET* is then defined as the incremental effect of β_{sg}.

Data

This study combines four waves of the Swiss Health Survey (1997, 2002, 2007, 2012) [47]. The first wave (1997) describes the situation in which no screening programs had been installed. The respondents of the Swiss Health Survey were selected randomly from the population aged 15 years and older living in private households. After an initial telephone interview, the respondents were sent a written questionnaire for questions that were difficult to answer over the phone or that required the consultation of documents. The respondents were not interviewed repeatedly, and they were included in only one of the four survey waves.

The sample of this study includes 13,874 women between 40 and 69 years of age. In the base-case analysis, we use only the 8609 women between 50 and 69 years of age and compare the evolution of the utilization rate in the screening and non-screening cantons. In the sensitivity analysis, we include the 5265 women between 40 and 49 years of age as an additional control group. The sample weights provided with the Swiss Health Survey are not used because our sample may not be representative due to missing values. All results apply only to our sample.

The binary dependent variable *evermam* indicates whether a woman has ever undergone mammography (Table 2). This variable is the only measure of screening uptake that is recorded consistently in all four survey waves. The main explanatory variable of interest is the dummy variable *screening*, which takes the value 1 if an organized program has been installed in the canton of residence at the time of the interview and 0 otherwise. Because some organized programs did not reach full coverage in the very first year, this variable is subject to a measurement error. We define the timing of the introduction as the first year in which the program sent out invitation letters. This definition avoids an overestimation of the screening initiation rate before the introduction but can lead to an underestimation of the initial effect of the organized programs. To assess the consequences of this measurement error, we control for the time trend in the policy effects and carry out several sensitivity analyses. The variable *duration* measures the number of years since the introduction of the organized mammography screening program. The age of the women is an important control variable because it measures the duration of the period of opportunity to become a mammography user. The other covariates include demographic and socioeconomic characteristics, information on health-related behavior (smoking, drinking, physical activity, body weight, health-oriented lifestyle), and variables describing the women's medical history (self-rated health, history of cancer, hormone therapy). The *income* variable represents the household income net of social insurance and pension fund contributions divided by the weighted number of household members using the OECD-modified scale [48]. The weighted number of household members assigns a weight of 1 to the household head, 0.5 to each additional adult and 0.3 to each child living in the household.[2] This variable does not express the true financial situation of respondents' households but rather captures income-related gradients. To control for geographic variation in supply-side factors, we use an indicator of residence in an urban area. Physician density is available only at the canton level and would be absorbed by the canton fixed effects. Binary variables describing the insurance plans of the respondents provide information regarding the effects of compensation modalities.

[1] The duration of the organized programs cannot be used as an explanatory variable in this specification because it would be highly collinear with g_it_i.

[2] In the survey waves of the years 1997, 2002 and 2007, children were defined as household members below the age of 15 years. In the 2012 wave, this definition was changed to household members below the age of 14 years.

Results

Descriptive statistics

The proportion of women between 50 and 69 years of age who have ever undergone mammography increases from 58% in 1997 to 80% in 2007 and then remains constant until 2012 (Table 3). Among 40- to 49-year-old women, the average utilization rate decreases from 47% in 1997 to 42% in 2002 and then remains at this level until 2012. Screening cantons exhibit a higher utilization rate than non-screening cantons even before the introduction of the first organized programs (Fig. 3). The screening and non-screening cantons exhibit a similar pattern among women in the target group (50–69 years), but the utilization rates evolve differently in the comparison group (40–49 years).

Table 3 Unweighted sample means by age group and year

	50–69 years old				40–49 years old			
	1997	2002	2007	2012	1997	2002	2007	2012
evermam	0.58	0.73	0.80	0.80	0.47	0.42	0.41	0.42
screening	0.00	0.16	0.32	0.40	0.00	0.18	0.30	0.40
duration	0.00	0.48	1.92	3.25	0.00	0.53	1.75	3.28
age	58.91	59.07	59.30	58.46	44.23	44.13	44.16	44.63
married	0.56	0.58	0.58	0.62	0.63	0.63	0.61	0.68
urban	0.71	0.76	0.71	0.72	0.68	0.71	0.68	0.69
foreign	0.11	0.07	0.05	0.07	0.11	0.08	0.10	0.15
educ1	0.29	0.22	0.13	0.12	0.15	0.11	0.06	0.05
educ2	0.60	0.64	0.64	0.55	0.64	0.67	0.62	0.53
educ3	0.05	0.05	0.03	0.10	0.07	0.06	0.04	0.10
educ4	0.04	0.05	0.08	0.09	0.07	0.08	0.09	0.11
educ5	0.03	0.04	0.12	0.14	0.07	0.08	0.18	0.21
working	0.43	0.48	0.53	0.65	0.74	0.78	0.82	0.86
income	3.75	4.13	4.44	4.59	3.70	4.10	3.82	3.88
smoker	0.20	0.23	0.21	0.24	0.37	0.35	0.30	0.25
alcohol	6.48	7.38	6.36	5.97	6.37	6.40	5.12	5.06
movdays0	0.49	0.43	0.33	0.38	0.41	0.35	0.28	0.30
movdays1	0.17	0.18	0.17	0.17	0.23	0.21	0.21	0.20
movdays2	0.11	0.15	0.18	0.20	0.16	0.19	0.20	0.25
movdays3	0.08	0.10	0.13	0.12	0.07	0.11	0.14	0.13
movdays4	0.04	0.03	0.04	0.03	0.03	0.04	0.05	0.04
movdays5	0.02	0.03	0.03	0.02	0.03	0.03	0.03	0.03
movdays6	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
movdays7	0.09	0.09	0.09	0.06	0.07	0.07	0.08	0.04
overweight	0.29	0.30	0.27	0.26	0.19	0.17	0.16	0.20
obese	0.11	0.10	0.10	0.11	0.05	0.07	0.06	0.08
lifestyle	0.92	0.94	0.93	0.92	0.92	0.92	0.92	0.91
health1	0.04	0.04	0.03	0.04	0.03	0.03	0.02	0.03
health2	0.17	0.14	0.11	0.15	0.11	0.10	0.06	0.09
health3	0.57	0.61	0.67	0.46	0.58	0.63	0.71	0.43
health4	0.22	0.21	0.19	0.34	0.27	0.25	0.21	0.45
badhealth	0.21	0.18	0.14	0.20	0.15	0.12	0.08	0.11
hormones	0.20	0.33	0.19	0.14	0.06	0.08	0.04	0.05
cancer	0.04	0.04	0.03	0.04	0.03	0.02	0.02	0.02
dedhi	0.46	0.48	0.47	0.48	0.48	0.52	0.57	0.60
suppins	0.53	0.45	0.45	0.30	0.51	0.42	0.33	0.22
modalt	0.04	0.05	0.15	0.43	0.04	0.07	0.18	0.47
N	1335	2604	2132	2538	775	1393	1331	1766

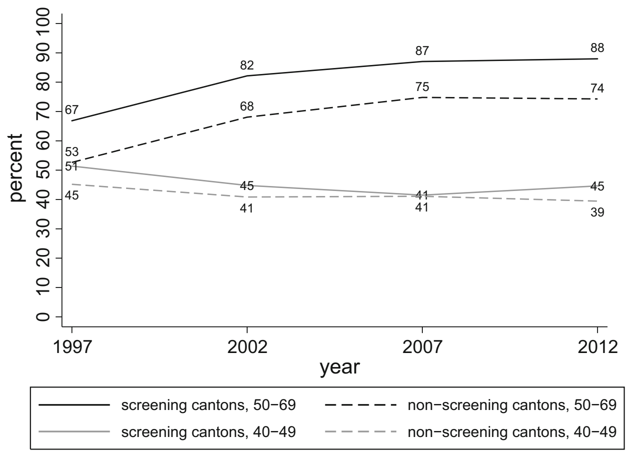

Fig. 3 Proportion of mammography users [%] by age group, screening versus non-screening cantons

The deviating trends in the comparison group demonstrate the need to relax the common trend assumption in the sensitivity analysis. However, the observed time trends do not provide evidence of a violation of the common trend assumption because organized programs have been implemented gradually and because the composition of the target population could have changed differently in screening and non-screening cantons.

Because of the gradual implementation of the organized screening programs, the proportion of women living in a canton with an established screening program increases from 0% in 1997 to 40% in 2012. The women's educational level, labor market participation, and income increase markedly over the four survey waves (Table 3). This trend is no surprise, as the 1997 cohort consists of women who were born between 1928 and 1947, whereas the 2012 cohort includes primarily women who were born after the Second World War.

Women in the target group clearly differ from women in the comparison group. 50- to 69-year-old women are more likely to live in an urban area, are less well educated, are less likely to work, and smoke less but drink more than 40- to 49-year-old women. Although older women more often report a health-oriented lifestyle, they are more likely to be physically inactive, overweight, obese, or in bad health than younger women. In addition, a history of cancer or hormone therapy is more prevalent among older women. In general, the differences between younger and older women do not change significantly over time. Two striking exceptions are the proportion of women of foreign nationality, which increases since 2002 in younger but not in older women and the proportion of smokers, which decreases continuously among younger women while it remained constant among older women.

Table 4 Average marginal effects of independent variables on the probability of having ever used mammography, base-case estimates

	Coef.	*p* value
screening	0.046**	0.022
duration	0.001	0.863
age	0.003***	0.006
married	0.042***	0.000
urban	0.037***	0.010
foreign	0.006	0.748
educ2	−0.007	0.531
educ3	0.009	0.712
educ4	−0.004	0.842
educ5	−0.035	0.101
working	0.011	0.420
income	0.006**	0.023
smoker	−0.006	0.573
alcohol	0.001*	0.097
movdays1	0.030***	0.008
movdays2	0.031**	0.044
movdays3	0.031**	0.024
movdays4	0.023	0.202
movdays5	−0.010	0.721
movdays6	0.021	0.588
movdays7	0.021	0.184
overweight	0.016	0.160
obese	−0.012	0.358
lifestyle	0.039*	0.080
health2	−0.036*	0.083
health3	−0.051***	0.002
health4	−0.076***	0.000
hormones	0.141***	0.000
cancer	0.112***	0.000
dedhi	−0.020**	0.030
suppins	0.057***	0.000
modalt	−0.016	0.305
canton	FE	
year	FE	
N	8609	

Standard errors are computed using the delta method

FE fixed effects

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Regression results

The fixed-effects logit model estimates a significant and positive average marginal effect (4.6% points) for the organized programs on screening initiation (Table 4). The coefficient of the program duration is close to zero and is not significant. These estimates indicate that the organized programs have only an immediate effect and do not increase the screening initiation rate after the year of their introduction. In 2012, 3.5% points of the proportion of women with at least one mammography in the screening cantons can be attributed to the organized screening programs (Table 6).

As anticipated, the probability of mammography uptake increases with age (+0.3% points per year). Married women are more likely (+4.2% points) to have ever used mammography. Education does not exhibit a significant effect on the screening rate, and the effect of monthly income is relatively small, with only +0.6% points per CHF 1000. However, the magnitude of the effect of income is difficult to interpret as an increase in the equivalized household income corresponds to an even larger increase in the absolute household income. It is further not clear whether this coefficient measures the relative position in the income distribution or the absolute endowment of households. Women who are physically active 1–3 days a week are 3.0–3.1% points more likely to have used mammography than are completely inactive women. Similarly, a health-oriented lifestyle is associated with a higher probability of mammography uptake (+3.9% points). Healthier women exhibit a lower participation rate, and women with a history of cancer or hormone therapy are more likely to have used mammography. Although mammography is performed on an outpatient basis and is covered by basic health insurance, supplementary hospital insurance plans increase the probability of mammography use (+5.7% points), whereas high deductibles decrease it (−2.0% points). Women who are more sensitive to costs and have higher deductibles may tend to refuse mammography screening because of its questionable cost-effectiveness ratio. Supplementary hospital insurance plans may be positively associated with mammography use because they measure the willingness to pay for insurance against future risk, which is the purpose of mammography screening. However, the characteristics of insurance plans are likely to be endogenous, as they depend on a woman's health history and her previous service use.

Sensitivity analysis

Effect heterogeneity across socioeconomic groups

In the first sensitivity analysis, we assess the heterogeneity of the policy effects across socioeconomic groups. The model with interactions between the *screening* variable and indicators of education and income shows substantial heterogeneity in the policy effects across socioeconomic groups (Fig. 4). The average marginal effect of the organized screening programs on screening initiation is strongest among women with a professional education (apprenticeship) and weakest among women with a higher-level professional education. Women with a higher-level professional education could either be less susceptible to information campaigns or have high screening rates even in the absence of an organized screening program. Women with a university degree also respond positively to the organized screening programs, but the effect is rather uncertain because of the small group size.

The policy effects vary substantially over the income distribution but do not follow a clear pattern. The level of responsiveness is particularly high among the poorest 30% of women. The large effects for underprivileged women could be a consequence of the cost-free access guaranteed within organized screening programs. Women in the upper middle class who earn more than the poorest 50% also respond well to the policy, whereas the richest women and median-income earners are not more likely to initiate screening after the introduction of the organized programs.

Placebo intervention tests

The first placebo intervention emulates the introduction of organized programs 5 years before their actual implementation and is used to test for deviating trends in screening programs prior to the introduction of the programs. The coefficient of the leading placebo intervention is non-significant and close to zero and the average marginal effect of the organized programs is not affected by the addition of the leading placebo variable (Table 5). In the model with leading placebos, the predicted probabilities and the *ATET* are identical to the base-case estimates (Table 6).

The second placebo intervention test is based on 1000 estimations of the base-case model, including an indicator of randomly assigned placebo screening programs in non-screening cantons. The mean of all 1000 average marginal effects of the organized programs equals the base-case estimate, and none of the 1000 marginal effects is less than zero. The mean of the average marginal effects of the random placebo programs is zero, the 95% percentiles include zero, and the *p* values are distributed evenly between 0 and 1 (Fig. 5). The predicted probabilities and the *ATET* only differ marginally from the base-case estimates (Table 6). In summary, the placebo intervention tests do not provide evidence that our regression results are biased by deviating time trends in screening and non-screening cantons.

Difference-in-difference-in-difference estimation

In the third sensitivity analysis, we estimate a difference-in-difference-in-difference model in which women between 40 and 49 years of age serve as an additional comparison group. The positive marginal effect of the variable *target* shows that women in the target group are

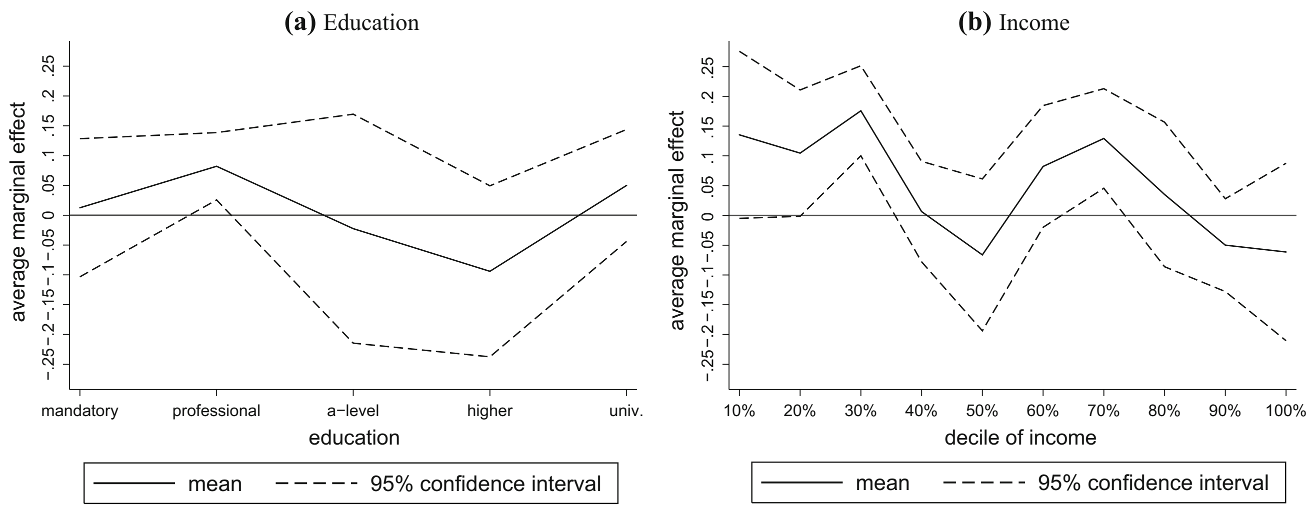

Fig. 4 Average marginal effects by education and income

Table 5 Average marginal effects of screening variables on the probability of having ever used mammography, sensitivity analyses

	Coef.	*p* value	95% ll	95% ul
Base-case				
Screening	0.046	0.022	0.007	0.085
Duration	0.001	0.863	−0.009	0.011
Leading placebo intervention in screening cantons				
Screening	0.046	0.009	0.012	0.080
Duration	0.001	0.856	−0.009	0.010
Leading placebo	0.001	0.977	−0.036	0.037
Random placebo intervention in non-screening cantons				
Screening	0.046	0.000	0.031	0.060
Duration	0.001	0.157	−0.001	0.003
Random placebo	0.000	0.482	−0.043	0.041
Difference-in-difference-in-difference				
Screening	−0.029	0.198	−0.072	0.015
Target	0.083	0.000	0.047	0.119
Target × screening	0.089	0.024	0.012	0.166

In the random placebo test, *p* values indicate the proportion of all coefficients <0, and 95% credible intervals indicate 2.5 and 97.5 percentiles over all iterations

more likely to have used mammography (Table 5). The average marginal effects of *screening* and *target* × *screening* suggest that the utilization rate in the comparison group decreases by 2.9% points after the introduction of an organized program, whereas the difference between the target and comparison groups increases by 8.9% points (Table 6).

If we believe that the comparison group is suitable for estimating general trends in the screening and non-screening cantons and that the organized programs did not have any spillover effects on women in the comparison group, then the negative coefficient of *screening* indicates that the time trend was more downward sloping in the screening cantons. Under the assumption of deviating trends, the coefficient of the interaction term *target* × *screening* can be interpreted as the corrected policy effect in the target group, which is approximately twice as large (0.089 vs. 0.046) as the base-case estimate. The *ATET* under the relaxed common trend assumption is estimated at 5.4% points.

Discussion

This study estimates the causal effect of organized mammography screening programs on the probability that 50- to 69-year-old women have ever used mammography. This probability is interpreted as an indicator of the decision to initiate screening and thus of the demand for mammography. The base-case analysis shows that the organized screening programs account for 3.5% points of the

Table 6 Predicted probabilities of women living in screening cantons in 2012 having ever used mammography, sensitivity analyses

	Base-case	Leading placebo		Random placebo		DDD
	Screening	Screening	Placebo	Screening	Placebo	Screening
Treated	0.879	0.879	0.862	0.879	0.744	0.878
Counterfactual	0.844	0.844	0.862	0.843	0.743	0.824
ATET	0.035	0.035	0.000	0.036	0.001	0.054

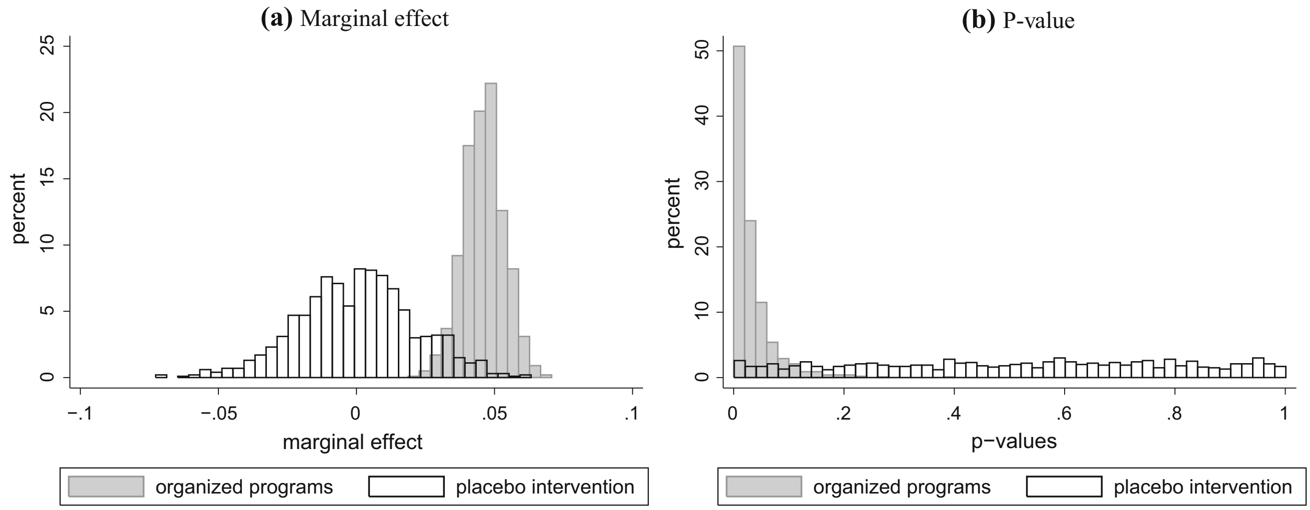

Fig. 5 Frequency distribution of the results from the random placebo test

proportion of women with at least one mammography in the screening cantons in 2012. We further estimate that the organized programs increase the screening initiation rate only during the first year after their implementation. The placebo intervention tests suggest that the base-case results are not affected by deviating time trends in screening and non-screening cantons. However, the difference-in-difference-in-difference estimation shows that the base-case results might be biased downward by 1.9% points because the general time trend might be more negative in the screening cantons.

The results of this study indicate that organized screening programs can motivate women who have never undergone mammography to initiate screening but that the effect is relatively small. Although an effect size of 3.5–5.4% points seems considerable compared to the proportion of women who have never done mammography, it is relatively small compared to the total target population. An absolute increase of 3.5–5.4% points suggests that an organized program must invite 18.5–28.6 women to motivate one woman to begin using mammography. This means that the vast majority of women in the target population would either have started screening regardless of the organized programs or were not motivated to get their first mammography.

Because some organized programs did not reach full coverage in their first year, the timing of their introduction is subject to a measurement error, and the base-case analysis might underestimate the initial policy effects. The absence of a significant effect of the *duration* variable cannot be interpreted as evidence against a time lag in the policy effects because the *duration* variable is only measured at the cantonal level, in large time intervals and with varying time lags since the delivery of the first invitation letters.

Because our study examines the effects of organized screening programs on screening initiation, it does not give any information about the effects of these programs on mammography use at a given point in time or on regular screening. Although cantonal organized programs did not motivate many women to begin using mammography, they still might be effective in increasing the screening rate among those who already have used mammography. Similarly, our finding that organized programs only increased screening initiation in the first year of the programs but not later on does not mean that repeated invitation letters could not motivate women to undergo mammography more regularly. In their analysis of the Survey of Health, Ageing and Retirement in Europe (SHARE), Carrieri and Wübker [19] used mammography uptake in the last 2 years before the survey as the dependent variable and showed that organized programs did increase the utilization in the target population and thus can motivate women to undergo mammography more regularly. The comparison of our own results and the findings by Carrieri and Wübker [19] suggest that the effects of organized screening programs on mammography use rather stem from an increase of the number of mammograms per women than from a decrease of the proportion of unscreened women.

The definition of the dependent variable might also explain why our estimates of the effects of covariates differ from the results of other studies. While previous studies documented an education-related gradient in screening uptake [13, 14] and more pronounced effects of organized screening among women with lower education [17], we do not find clear income- or education-related gradients in screening initiation. Although we find a significant effect of income on screening initiation [13, 16] and stronger effects of the organized programs among women with lower incomes [20], these gradients are rather moderate. If residency in an urban area is interpreted as an indicator of access to care, the significant effect of this variable is in line with the finding of a positive effect of access to care on screening uptake reported by Schueler et al. [13].

The policy implications of our results depend on the net medical benefit of screening mammography. If screening mammography creates more benefit than harm but incurs additional costs [5], the estimated effects of organized programs on screening initiation should be used to compare organized programs with opportunistic screening and to consider the number needed to invite to prevent a breast cancer-related death in the calculation. A potential cost-effectiveness analysis should then include the costs of both the organized programs and the executed mammograms. If screening mammography even does more harm than good because of false-positive diagnoses and overdiagnoses [4], the organized programs should be suspended regardless of the results of this study.

The econometric model used in this study allows for identifying the causal effect of organized screening programs on screening initiation. The specification is more robust to exogenous shocks in the screening cantons than a standard pre-post treatment-control design because the organized programs were introduced at different points in time. We also choose the empirical model based on residual-based specification tests and relax the vital common trend assumption using younger women as a comparison group in a difference-in-difference-in-difference specification. Note that this analysis has some limitations. First, our analysis relies on a small number of cantons and time periods, which may lead to a downward bias in the standard errors as a result of serial correlation [49]. Although this bias means that the effect of organized mammography screening programs may not be significant, this limitation does not change the conclusion that the organized programs had little effect on the demand for mammography. Second, the timing of the introduction of the organized programs is subject to a measurement error because the programs did not reach full coverage in the first year or were preceded by a pilot project. Third, we only observe a binary indicator of having ever used mammography consistently in all four survey waves. Therefore, our study informs policy makers about the effects of organized programs on women's propensity to begin screening but does not reveal the frequency of mammography use or the number of regularly screened women. Fourth, our study cannot explain how the programs affected screening initiation. A possible hypothesis is that organized programs also increased the demand for mammography because of quality improvements or changed recommendations by referring general practitioners. Fifth, we cannot discriminate between screening and diagnostic mammography. It is conceivable that invitation letters also motivated women with signs or a history of cancer to undergo diagnostic mammography. Sixth, households could move between cantons within large time intervals which can not be controlled for using repeated cross sections.

Future research could focus on those waves of the Swiss Health Survey containing information regarding the purpose and frequency of mammography use. Such a study could adopt the identification strategy used in the sensitivity analysis of this study and compare the difference between the target and comparison groups across cantons. The observation that the first organized programs were established in cantons with a high density of radiologists and the difference between our own results and those reported by Carrieri and Wübker [19] raise the question whether supply-side factors can partly explain increased screening initiation rates and more regular screening within organized programs. A supply-side effect of organized programs on women's decision to undergo their first mammography could occur when general practitioners or gynecologists in screening cantons are more likely to refer women to a radiologist. Cost-free access to care might also decrease physicians' cost of demand inducement. The variation of the density of radiologists across cantons could be investigated further to shed some light on the physicians' role in the decisions about mammography use in Switzerland. Socioeconomic inequality in mammography uptake and the contribution of organized screening programs to this inequality are further topics of research that deserve more attention in the future.

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Appendix I: Number of observations per canton and year

Table 7 Number of observations per canton and year

Canton	50–69 years old					40–49 years old				
	1997	2002	2007	2012	Total	1997	2002	2007	2012	Total
AG	94	147	165	143	549	64	59	85	118	326
AR-AI	5	37	65	58	165	5	25	39	39	108
BE	180	231	215	210	836	101	104	140	140	485
BL	40	148	62	98	348	18	62	21	63	164
BS	22	152	38	130	342	11	58	25	68	162
FR	29	112	102	124	367	15	76	77	96	264
GE	125	148	110	122	505	72	85	61	86	304
GR	45	39	25	84	193	28	22	15	47	112
JU	16	81	45	58	200	6	46	30	48	130
LU	83	129	122	136	470	50	73	91	106	320
NE	38	111	80	73	302	14	44	40	45	143
OW-NW	7	45	15	9	76	12	23	8	11	54
SG	55	125	82	64	326	24	63	55	46	188
SH	13	37	26	10	86	9	16	9	8	42
SO	22	134	44	33	233	21	80	30	34	165
SZ	16	22	44	60	142	16	16	56	47	135
TG	20	39	40	136	235	19	31	27	95	172
TI	111	195	159	187	652	69	110	100	140	419
UR-GL	17	55	52	66	190	8	36	40	52	136
VD	114	154	179	205	652	49	91	93	153	386
VS	101	116	163	140	520	59	71	96	83	309
ZG	18	114	19	130	281	10	78	11	67	166
ZH	164	233	280	262	939	95	124	182	174	575
Total	1335	2604	2132	2538	8609	775	1393	1331	1766	5265

Appendix II: Specification tests

The link test checks the linearity of the response on the scale of estimation by regressing the raw scale variable y on the predicted value of $x\beta$ and $(x\beta)^2$ [50]. An insignificant and small coefficient of $(x\beta)^2$ indicates the linearity of responses. The RESET test also includes the cubic and quartic terms of $x\beta$, which should be individually and jointly insignificant [51]. The modified Hosmer–Lemeshow test checks the linearity of the residuals over the predicted probability [52]. We also use the modified Hosmer–Lemeshow test to assess the linearity of the residuals over the continuous explanatory variables *duration*, *age*, *income,* and *weekly alcohol consumption* and to test the benefits of higher-order terms of these variables.

The linear probability model failed all three residual-based tests. Both the link test and the RESET test rejected the linearity of y_i over the linear predictor $x_i\hat{\beta}$ (Table 8). The coefficients of $x_i\hat{\beta}$ were different from one, and the higher-order terms were (jointly) significant. The modified Hosmer–Lemeshow test showed an inverse U-shaped pattern for the residuals over the predicted probability (Fig. 6).

Table 8 Model comparison using the link test and the RESET test

	LPM		FE logit	
	Coef.	p value	Coef.	p value
Link test				
$x\hat{\beta}$	2.103***	0.000	0.947***	0.000
$x\hat{\beta}^2$	−0.755***	0.000	0.027	0.281
RESET test				
$x\hat{\beta}$	−1.816	0.389	0.965***	0.000
$x\hat{\beta}^2$	6.226	0.172	0.056	0.378
$x\hat{\beta}^3$	−4.988	0.233	−0.029	0.517
$x\hat{\beta}^4$	1.172	0.397	0.006	0.524
$x\hat{\beta}^2, x\hat{\beta}^3, x\hat{\beta}^4$		0.000		0.592

LPM linear probability model, *FE logit* fixed-effects logit

The fixed-effects logit model passed all three residual-based tests. The coefficients of $x_i\hat{\beta}$ were close to one, and the effects of higher-order terms of $x_i\hat{\beta}$ were small and non-significant. The modified Hosmer–Lemeshow test showed

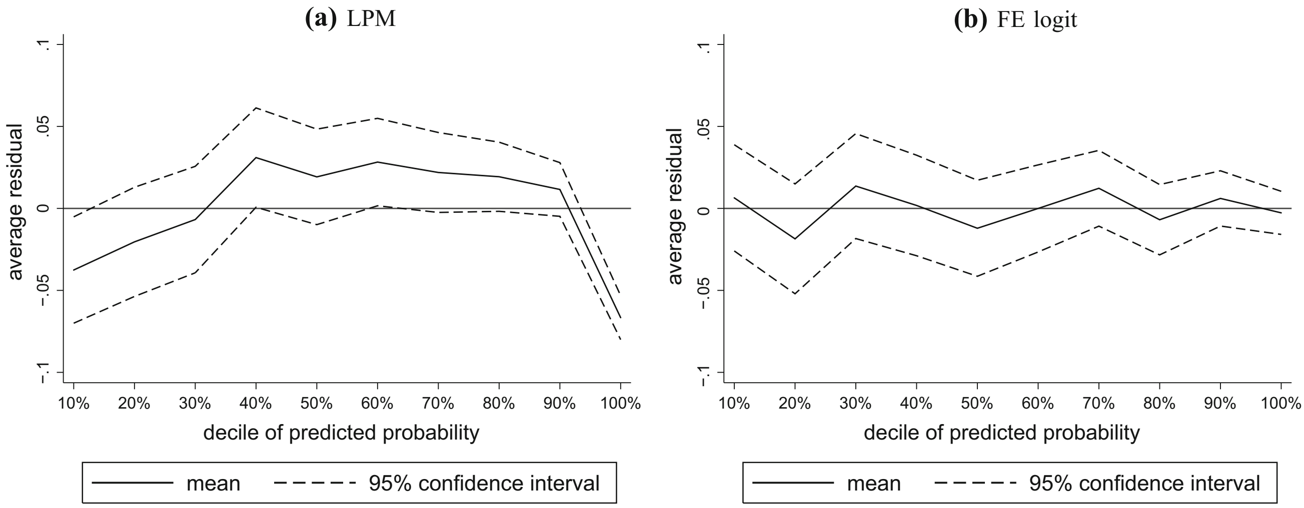

Fig. 6 Modified Hosmer–Lemeshow test: Mean residuals (95% confidence intervals) over deciles of the predicted probability

Table 9 Information criteria

	LPM	FE logit
log-likelihood	−4371	−4188
AIC	8786	8419
BIC	8941	8575

a linear pattern of residuals with no significant deviation from zero. The logit model yielded a lower value of the log-likelihood than the linear probability model (Table 9), which confirmed the results of the residual-based specification tests.

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