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*The labor and health economics of breast cancer**

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Abstract

We estimate the long-run labor market and health effects of breast cancer among Austrian women. Compared to a random sample of same-aged non-affected women, those diagnosed with breast cancer face a 22.8 percent increase in health expenses, 6.2 percent lower employment, and a wage penalty of 15 percent five years after diagnosis. Although affected women sort into higher quality jobs post-diagnosis, this is offset by a reduction in working hours. We argue that the hours reduction is more likely driven by an increase in the time preference rate, meaning that patients increasingly value the present over the future, rather than by an incapacitation effect or employer discrimination.

JEL Classification: I10, J22, I12
Keywords: Breast cancer, labor supply, health shocks, time discounting

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I. Introduction

Breast cancer is the most common cancer in women and the second most common cancer overall (Cancer Today 2024). In the United States, 240,000 new cases of breast cancer were diagnosed in 2020; one in eight women will be diagnosed at some point in her lifetime (CDC 2024). Although medical advances continue to make breast cancer better treatable—the five-year survival rate is over 90 percent—it is expensive to society. The annual medical cost alone are estimated at $29.8 bn (National Cancer Institute 2024*a*), with the full economic cost likely being much larger than that.[1] Even after successful treatment women may suffer from fatigue, sleep problems, mental distress, changes in hearing and vision, and hormonal imbalances (American Cancer Society 2024), so breast cancer can also limit women's ability to participate in the labor market. This has important implications for the governments' budget constraint, because both spending and tax revenues are potentially affected. And since better treatment and increased survival means that more and more women today are living with breast cancer, it is all the more important to understand the long-run effects of the disease.

In this paper we use linked administrative registers to estimate the labor market and health effects of breast cancer among Austrian women. To construct counterfactuals, we randomly match same-age unaffected women from the general population. This comparison group tracks breast cancer patients well in terms of labor market trajectories and healthcare takeup prior to the diagnosis, suggesting that breast cancer is a health shock that is difficult to anticipate. Using a difference-in-differences design, we document dynamic effects on health and labor market outcomes within a ten-year window around the diagnosis.

We find that after five years, breast cancer patients have 22.8 percent higher healthcare expenditures than similar women without a diagnosis. This is largely due to persistent increases in drug and hospital expenditures. We also test for effects on drugs that are likely to have important implications for patient wellbeing: antidepressants, opioids, and benzodiazepines. We find sharp increases in

[1] According to Lidgren et al. (2007), medical cost only make up 30 percent of the overall cost of breast cancer, with lost wages, sick leave, and other factors accounting for the remaining 70 percent.

prescriptions for each of these drugs. Antidepressant takeup goes up by 8.3 percent, opioid takeup by 40.5 percent, and benzodiazepine takeup by 36.6 percent. Breast cancer also affects women's livelihoods. We find that employment permanently decreases by about 6.2 percent, while social safety program takeup increases, especially for sick leave and disability insurance. We show that these effects are likely conservative and biased toward zero due to selection into survival.

We then turn our focus to women who remain in the labor market. We find that breast cancer reduces wages, conditional on working, by 15 percent in the five years after diagnosis. This is not because women move to lower-paying jobs: we find that average firm quality (proxied by Abowd et al. 1999 firm fixed effects) actually *increases* after diagnosis, and even women who do not change jobs experience a statistically similar wage penalty. Also, breast cancer patients in low-quality firms are much more likely to exit the labor market than those in high-quality firms. Instead, we find evidence that breast cancer leads affected women to reduce work hours, as parttime work increases by about 16 percentage points relative to unaffected women.

Finally, we ask *why* breast cancer patients reduce hours and earn lower wages. We have three hypotheses. First, the disease could lead to an incapacitation effect, meaning that physical or mental strain prohibits women from working more. Second, breast cancer might affect the labor-leisure tradeoff by increasing women's time preference rate, in the sense that they attach higher utility to the present than to the future. Third, employers might discriminate against women with breast cancer, for example because they expect future productivity to decline. We find that incapacitation is unlikely to explain the full wage effect. For example, predicted cancer severity does not seem to affect the wage penalty and sick leave takeup quickly returns to pre-diagnosis levels for women still participating in the labor market. Instead, we find some evidence consistent with an increase in the time preference rate: affected women are less likely to make human capital investments, they engage in more risky behavior, and they reduce fertility (all of which are tradeoffs between future benefits and present-day costs). This is in line with evidence in psychology and behavioral economics that stressful events affect people's time discounting (Bogliacino et al. 2021, Cassar et al. 2017, Haushofer & Fehr 2014). Finally, we show that there is little evidence that the hours reduction and the resulting wage penalty are driven by employer discrimination.

Our paper primarily contributes to the literature on the labor market effects of breast cancer. Bradley, Bednarek & Neumark (2002*a*) and Bradley, Bednarek & Neumark (2002*b*) use data from the Health and Retirement Survey and regress labor supply and wages on whether women have a history of breast cancer. They find that breast cancer is negatively associated with employment, but wages appear to be higher in women diagnosed with breast cancer than for other women. In later work, Bradley, Neumark, Bednarek & Schenk (2005) use propensity score matching and find that breast cancer is negatively related to employment in the Current Population Survey too. Our work is most closely related to a series of design-based studies that combine matching and difference-in-differences to establish causal effects of breast cancer on labor market outcomes, including Moran, Short & Hollenbeak (2011), Heinesen & Kolodziejczyk (2013), Jeon (2017), and Vaalavuo (2021). These papers show that breast cancer negatively affects employment and wages in the U.S., Denmark, Canada, and Finland.

We view our contributions relative to this literature as fivefold. First, we look beyond labor market outcomes and estimate effects on a variety of health and healthcare takeup-related outcomes to provide a more comprehensive picture of the societal impact of breast cancer.[2] Second, we are the first to systematically examine mechanisms that drive labor supply responses to breast cancer—in particular, we find that women actually match to better firms but reduce work hours post-diagnosis, and we propose an increase in the time discount rate as a novel factor that is likely to explain why affected women reduce hours. Third, we provide evidence on high-frequency dynamic effects for five years before and after diagnosis across outcomes, which allows us to test for anticipation and selection effects that potentially confound findings in the earlier literature.[3] Fourth, we show that using a control group of same-aged women is sufficient to generate credible causal estimates for

[2]Most of the medical and epidemiological literature on breast cancer focuses either on quantifying mortality after diagnosis, especially with regard to socioeconomic heterogeneity (Lundqvist et al. 2016), or on estimating the effectiveness of different treatment options (Waks & Winer 2019). There is also some evidence on how breast cancer affects healthcare utilization and mental health (see Jansana et al. 2019 and Fortin et al. 2021 for meta studies), but these studies are largely descriptive and often rely on self-reported data. We contribute by studying effects on healthcare takeup using causal inference and population register data.

[3]Assuming that we would see differential selection into breast cancer in pre-treatment outcome trends.

the effects of breast cancer, which requires less strong assumptions than propensity score matching procedures used by the previous literature. Fifth, we systematically compare wage effects across different cancer types for both men and women, which reveals that breast cancer carries a larger wage penalty than cancers with similar survival rates and that wage penalties are quadratically increasing with cancer severity.

We also contribute to recent work on how technological advances in breast cancer treatment affect patient labor market outcomes (Daysal, Evans, Pedersen & Trandafir 2024). Daysal et al. exploit changes in Danish treatment guidelines and find that combined radiotherapy and chemotherapy leads to substantially better labor market outcomes than chemotherapy alone. While we are largely agnostic about the effects of cancer treatment—essentially, we estimate aggregate impacts of diagnosis and therapy—we do find that wage penalties are somewhat smaller for patients treated with radiotherapy. In fact, we see the best outcomes in patients who can still have breast surgery, which typically indicates that the cancer has not yet metastasized.

Our results also speak to the literature on the effectiveness of breast cancer screening (e.g., Bitler & Carpenter 2016, Churchill & Lawler 2023). These papers find that nudges and financial incentives work to motivate women to get mammograms, and that mammograms indeed help to detect early-stage breast cancer. We do not evaluate the effects of mammograms per se, but we do provide evidence that breast cancer patients are not more likely to select into mammography before diagnosis. This has important implications for policy, because the absence of selection effects implies that it may be necessary to better target high-risk women with incentives to undergo mammography.

More broadly, we contribute to a literature analyzing effects of health shocks on labor market outcomes, of workers themselves (e.g., Datta Gupta, Kleinjans & Larsen 2015, García-Gómez, van Kippersluis, O'Donnell & van Doorslaer 2013, Glaser & Pruckner 2023, Lindeboom, Llena-Nozal & van der Klaauw 2016, Lundborg, Nilsson & Vikström 2015, Simonetti, Belloni, Farina & Zantomio 2022), their spouses (e.g., Fadlon & Nielsen 2021), and children (e.g., Frimmel, Halla, Paetzold & Schmieder 2023). These papers usually focus on acute cardiovascular events, like heart attacks and strokes, which disproportionately affect men. We narrow in on a different health shock that almost exclusively befalls women.[4] We also study a much broader set of outcomes, dig deeper into potential mechanisms driving effects of health shocks, and compare our breast cancer estimates with those of other types of cancer.

II. Institutional setting

II.1. The Austrian healthcare system

Austria has a *Bismarckian* universal healthcare system with compulsory insurance. Outpatient healthcare and drugs are financed via social security contributions, inpatient care is financed partly by social security contributions and federal taxes. Since enrollment to the system is automatic, virtually all Austrian residents are covered by health insurance. Importantly, workers do not lose health insurance when they become unemployed or retire, and cost-sharing is limited to minor copayments for drug prescriptions (€ 6 per prescription in 2018) and overnight hospital stays (€ 10.19 per day in 2018).

Outpatient medical services are mainly performed by general practitioners (GPs) in solo practices. They provide acute care, offer well visits, and refer patients to specialists and hospitals when necessary. For female patients, there is a national network of outpatient gynecologists who can be consulted even without prior referral. Their services include annual routine check-ups, including Pap smears and breast exams, as well as medical services related to pregnancy and childbirth.

II.2. Breast cancer diagnosis and treatment

If breast cancer is suspected, for example based on palpation findings, the patient will be invited for breast cancer screening, which includes a mammogram and, in certain cases, an additional ultrasound or magnetic resonance imaging (MRI) scan. If the mammography findings are abnormal, a tissue sample (biopsy) is taken from the patient, and microscopic examination of the sample

[4]Men only account for less than one percent of breast cancer cases (National Cancer Institute 2024*b*).

allows a reliable diagnosis to be made. Mammographies and biopsies are performed by outpatient specialists and in hospitals. If the breast cancer diagnosis is confirmed, the patient is referred to a hospital for oncological treatment.

Treatments differ by cancer stage, but may also vary depending on individual characteristics and preferences. Tumors that are small and have not yet metastasized can usually be treated by surgery, where the goal is to preserve as much of the breast tissue as possible. If the tumor is larger but still has not yet metastasized, a mastectomy—that is, the removal of the breast—is usually necessary. After surgery, patients are often further treated with radiotherapy to lower the chance of cancer recurrence. If the tumor has already metastasized, surgery is often not possible anymore. Patients are then treated with chemotherapy or radiotherapy, both of which usually come with severe side effects.

Breast cancer types that are hormone receptor-positive can also be treated using drugs other than chemotherapy, which usually have milder side effects. These so-called HR+ cancer types are found in roughly 80 percent of cases, and can be treated with hormonal or endocrine therapy hindering the growth of cancer cells. For another subtype—approximately 15 percent of cases—new drugs have been developed in the last few years that directly attack breast cancer cells (American Cancer Society 2019). Most breast cancer cases are treated by a mixture of these treatment options to maximize survival rates. In addition to the cancer treatment itself, patients are often prescribed vitamin supplements and bone strengthening or osteoporosis medication to combat side effects of chemotherapy and radiation. Another group of medications prescribed after a breast cancer diagnosis are psychotropic drugs to treat any mental health issues that may arise.

II.3. The labor market

The Austrian labor market is characterized by strong industrial relations with centrally bargained wages and working conditions (Böheim 2017). The labor market is relatively flexible, with weak job protection and high turnover compared to other OECD countries (OECD 2020).[5] Employment

[5]In 2018, job turnover for female workers was 9.6 percent. In comparison, the European Union average was 8.6 for female workers. The OECD employment protection legislation indicator is 1.7 for Austria, which is the fifth-lowest

contracts can generally be terminated without giving a reason, but unilateral terminations require a notice period be observed. Unemployment rates have historically remained low, ranging, for example, from 4.7 in 1998 to 5.2 in 2018 (OECD 2023). Female labor force participation is particularly low, and almost 50 percent of female workers work part-time.

Besides health insurance, Austria has three other types of social insurance programs that are relevant for our analysis: Sick leave, unemployment insurance (UI), and disability insurance (DI). Sick leave compensates workers' earnings losses due to both occupational and nonoccupational disease. Workers are entitled to full wage compensation for 6 to 12 weeks, depending on job tenure. After this period, workers receive 80 percent of their wage for another 4 weeks, but the wage bill is shared equally between firms and social security. After these 4 weeks, workers are entitled to public sickness benefits that replace 60 percent of the current wage.

The UI program is also compulsory and funded through a 6 percent payroll tax shared equally by workers and firms. The minimum replacement rate is 55 percent of daily net income, which is calculated based on pre-unemployment wages. The potential benefit duration is between 20 and 52 weeks, depending on age and labor market experience (although most workers are eligible for either 30 or 39 weeks of benefits). A prerequisite to receiving UI benefits is that claimants are willing and able to work. This implies that they must prove that they frequently apply for new jobs and undergo retraining, if necessary.

Finally, Austria's DI program is financed by a payroll tax and provides partial earnings replacement to workers below the full retirement age. Disabilities must be attested by a licensed medical professional. A disability is classified as a mental or physical change in the wellness of an individual, sufficiently hindering them from gainful employment. Once benefits are awarded, DI beneficiaries receive monthly payments until their return to work, medical recovery, or death, although nearly all beneficiaries choose to remain out of the labor force. DI has an approximate 70 percent replacement rate, calculated based on indexed capped earnings, age, and work experience.

value among OECD countries. The United States rank last with an indicator of 1.3.

III. Data

Our analysis combines two sources of administrative data. First, we use health data from the *Upper Austrian Health Insurance Fund* (UAHIF). The UAHIF is the main statutory health insurance provider in Upper Austria—one of the nine states of Austria—and covers all private-sector workers and their dependents, which represent around 75 percent of the population.[6] The UAHIF provides us with individual-level healthcare claims, including drug prescriptions, sick leaves, outpatient physician visits, and hospital stays. Diagnoses, which are available for hospital stays, are recorded using ICD-10 codes. Drugs are classified using the ATC system. We do not observe emergency department visits. Full healthcare utilization data are available from 2005 to 2018, although information on outpatient claims and inpatient visits (but not treatments) is available from 1998.

Second, we use employment histories from the *Austrian Social Security Database* (ASSD, Zweimüller et al. 2009). The ASSD is an employer-employee panel that covers the universe of Austrian workers from 1972 to 2022. We have daily information on employment status and data on basic demographics, such as age and gender. A drawback of these data is that they do not contain information on working hours and occupational codes. To construct outcome variables that may potentially be affected by breast cancer, we calculate days in regular employment, days on sick leave, days on unemployment insurance (UI), and days on disability insurance (DI) per quarter. We also observe wages up to a social security cap (€5,670 per month in 2022, which is a little above the 95$^{\text{th}}$ percentile of the wage distribution), but only on a yearly basis.[7]

[6] We cannot observe healthcare records for self-employed persons, farmers, and civil servants.

[7] We verify this using pay slip data from the Austrian Department of Finance, which have uncensored wage information until 2012. The social security cap in 2012 was €4,230 and the 95$^{\text{th}}$ percentile of the wage distribution was €4,025 per month.

IV. Design

IV.1. The treatment group and the control group

The population we study is all Upper Austrian women born between 1930 and 1990 who were continuously insured between 2005 and 2009 at the UAHIF and who were *not* diagnosed with breast cancer between 2005 and 2009. We do this to ensure that we capture only first diagnoses. Although we cannot exclude the possibility that a small share of women may have had breast cancer already before 2005, this is rather unlikely: cancer patients require continuous monitoring, so we would most likely be observing those in remission at some point during these five years.[8]

The treatment group consists of all women diagnosed with ICD-10 code C50 between 2010q1 and 2018q4. To construct the control group, we draw a random sample from all remaining women, which has the same age distribution as the treatment group. We explain this procedure in more detail in Appendix B. In short, we estimate the age-at-diagnosis probability distribution function (pdf) for women in our treatment group, draw random treatment ages from the empirical pdf, and assign those to women not diagnosed with breast cancer. We then keep only control group observations whose placebo treatment is between 2010q1 and 2018q4. Our final sample has 5,180 women in the treatment group and 42,435 women in the control group.

We report summary statistics for the treatment and control group in Table A.1. Breast cancer patients are, on average, 61.6 years old, have 0.8 children, and 15 percent have a college degree. Yearly health expenditures before diagnosis are around €645, almost half of that amount is due to inpatient expenditures. About 18 percent receive antidepressants, 4 percent have an opioid prescription, and 9 percent receive benzodiazepines. Average quarterly employment is 27 days, owing to the fact that part of the sample has already retired and therefore has zero employment days.[9] The average annual wage is around €23,149. These means are similar in the control group.

[8] In any case, observing recurrent diagnoses would likely attenuate our estimated labor market and health effects.

[9] We do not restrict our sample to younger women when analyzing labor market outcomes. This has two reasons: (1) we want to use the same sample for analyzing labor market and health outcomes, and (2) women who have zero employment days throughout the observation period do not contribute to our fixed effects estimates anyways.

IV.2. Estimation

Let $\bar{q}_i$ be the calendar quarter woman i is (placebo) diagnosed with breast cancer, with placebo diagnoses being assigned according to the procedure we discuss in Appendix B. We estimate the following event study model,

$$y_{it} = \sum_{\substack{k=-20 \\ k\neq -1}}^{20} \beta_k \left(\mathbb{I}\{k = t - \bar{q}_i\} \times D_i\right) + \text{rel. time FEs} + \text{age FEs} + \text{year FEs} + \phi_i + \varepsilon_{it}, \tag{1}$$

where y_{it} is the outcome of interest for woman i in quarter t, D_i indicates if i was diagnosed with breast cancer or not, and $\mathbb{I}\{k = t - \bar{q}_i\}$ is an indicator for relative time k.[10] We also control for relative time fixed effects, age fixed effects, and calendar year fixed effects to account for life and business cycle effects in health and labor market outcomes, and an individual fixed effect ϕ_i, which accounts for unobserved heterogeneity across patients.

Because we fully saturate the model in relative time fixed effects and, by construction, we only compare effects relative to a group of never-treated women, forbidden comparisons and negative weighting à la Goodman-Bacon (2021) are not a problem in our setting. We therefore estimate our baseline models using standard twoway fixed effects, but we also provide sensitivity checks using the interaction-weighted estimator by Sun & Abraham (2021), which is robust to potential effect heterogeneity across treatment cohorts.[11]

We use outcome data from 2005q1 until 2018q4, which means that our panel is not balanced. If a woman is diagnosed, for instance, in 2018q3, we only observe her post-diagnosis outcomes for 2018q3 and 2018q4. All women diagnosed between 2010q1 and 2013q4 we observe for 20 quarters (i.e., five years) after diagnosis. Importantly, women who die leave the sample and do not contribute to our estimates anymore, so our dynamic effects can always be interpreted as conditional on survival. We discuss this in more detail in section IV.3.

[10] The reference period in most of our regressions is -1. For healthcare utilization outcomes we use -2 to allow for Ashenfelter-like dips in the last pre-diagnosis quarter. These dips occur mechanically, for example because treatment group women are slightly more likely to see a doctor in $t-1$ (but not in the other 19 quarters before $t-1$).

[11] Another reason why we estimate our baseline models with twoway fixed effects is computational efficiency, and because twoway fixed effects confidence intervals are often more conservative in our setting.

Inference is based on individual-level clustered standard errors. To avoid artificially inflating t-statistics because we have many more control group than treatment group observations, we calculate unit weights that sum to the same integer for the treatment group T and control group C in each birth cohort. In particular, for cohort k and both groups $G = \{T, C\}$, we define

$$w_k^G = \begin{cases} 1 & \text{if} \quad G = T \\ N_k^T / N_k^C & \text{if} \quad G = C, \end{cases} \tag{2}$$

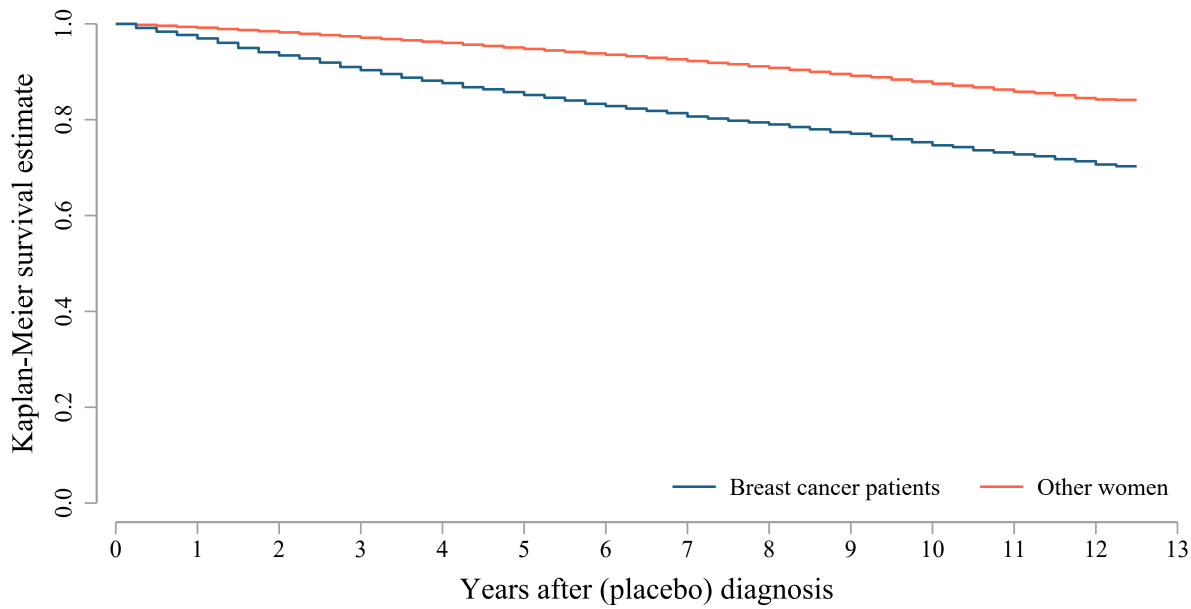
where $\sum_{i \in k,G} w_k^G = N_k^T$ for both $G = T$ and $G = C$. Applying these weights before estimating equation (1) ensures that we essentially compare the same number of observations in the treatment and control group.

The identification assumption underlying our empirical model is that trends in outcomes for women diagnosed with breast cancer and randomly matched non-affected women would have been similar absent the diagnosis. Below, we show various pieces of evidence indicating that breast cancer is a health shock that is difficult to anticipate. In particular, labor market and health outcomes for the treatment and control group are not only similar in trends before diagnosis, but also in levels, indicating that there are little systematic pre-diagnosis differences between women who have breast cancer and those who do not. We also show that our results do not depend on a particular specification of equation (1). Even in the raw data and in event study models without any covariates or fixed effects, we see little pre-diagnosis outcome differences and strong evidence for labor market and health effects after diagnosis.

IV.3. Mortality and how it affects the sample composition over time

Although breast cancer is well treatable, it can still be a fatal disease. Therefore, we begin our analysis by studying survival rates. In Figure 1, we plot Kaplan-Meier survival functions for women with and without breast cancer for up to 13 years after the (placebo) diagnosis. Women with breast cancer die at higher rates than the control group, with a survival rate of 85 percent five years after

Figure 1 — Survival after a breast cancer diagnosis



Notes: This figure plots Kaplan-Meier survival functions for women diagnosed with breast cancer and a random sample of same-aged women without a diagnosis for 13 years after the (placebo) diagnosis. We reweight the sample using the unit weights defined in equation (2).

diagnosis, which is about 10 percentage points lower than for the control group. After 10 years, the difference in survival rates increases slightly to 13 percentage points.

A natural question is to what degree these different survival rates in the treatment and control group affect our findings, given that our estimates are always conditional on surviving until a certain point in time *t* and there may be selection in survival. To gauge the impact of potential compositional changes over time, we predict trends in outcome variables based only on pre-diagnosis data on wages and a set of predetermined variables, including age at diagnosis, education, the number of children, as well as full labor market histories. We then ask how predicted outcomes change in the post-diagnosis period given that some individuals leave the sample in Figure A.1.

If there was no post-diagnosis attrition, trends in predicted outcomes would continue to be flat. But in our data it seems that average predicted healthcare expenditures decrease, while employment days and wages increase more strongly for women in the treatment group. This implies that decedents (or women who leave the sample for other reasons) in the treatment group have, on average, higher predicted healthcare expenditures, lower predicted employment days, and lower predicted wages than decedents in the control group. In other words: due to changes in sample composition alone, we expect the model in equation (1) to estimate small negative effects on health expenditures and small positive effects on employment and wages. Since this is the exact opposite of what we will find below, we conclude that we generally underestimate positive effects on health and negative effects on labor market trajectories by conditioning on survival.

V. The effects of breast cancer on health and labor market attachment

In this section, we estimate effects of breast cancer diagnoses on physical and mental healthcare utilization and on labor market outcomes, including employment and social safety program takeup.

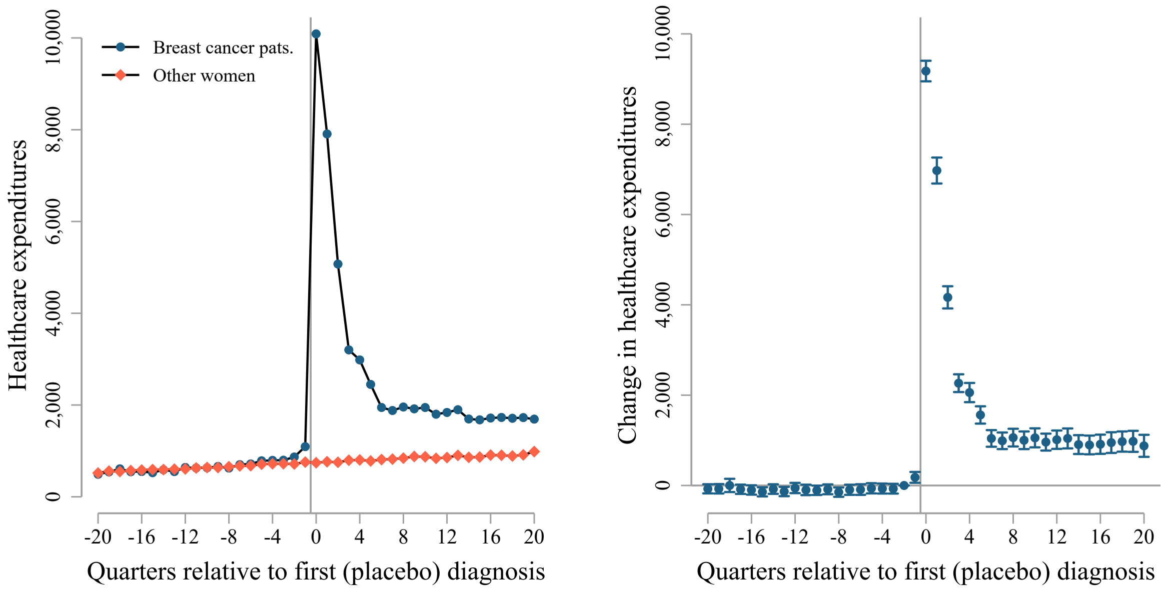
V.1. Physical and mental healthcare utilization

Although survival rates tend to be much higher compared to other cancer types, breast cancer is a serious disease.[12] To better understand its impact on society, it is important to quantify its effects on health and healthcare utilization. In Figure 2, we start by estimating the effect of a breast cancer diagnosis on total healthcare expenditures. In the left graph, we plot raw means for breast cancer patients (the blue dots) and same-aged women without a breast cancer diagnosis (the red diamonds). In the right graph, we display event study estimates from equation (1), which essentially represent the differences between the blue dots and the red diamonds over time, relative to two quarters before the first (placebo) diagnosis.

We note two key findings. First, even before the diagnosis, women with breast cancer and same-aged women in the comparison group behave remarkably similarly. Not only are there virtually no baseline differences in average healthcare utilization before diagnosis, trends are similar as well. This suggests that women in the treatment group do not systematically differ from the control group in terms of healthcare use. Second, a breast cancer diagnosis represents a large shock to

[12] Among the 15 most common types of cancer in women, breast cancer has the second highest five-year survival rate behind thyroid cancer (see section VII).

FIGURE 2 — Effects of breast cancer on total healthcare expenses

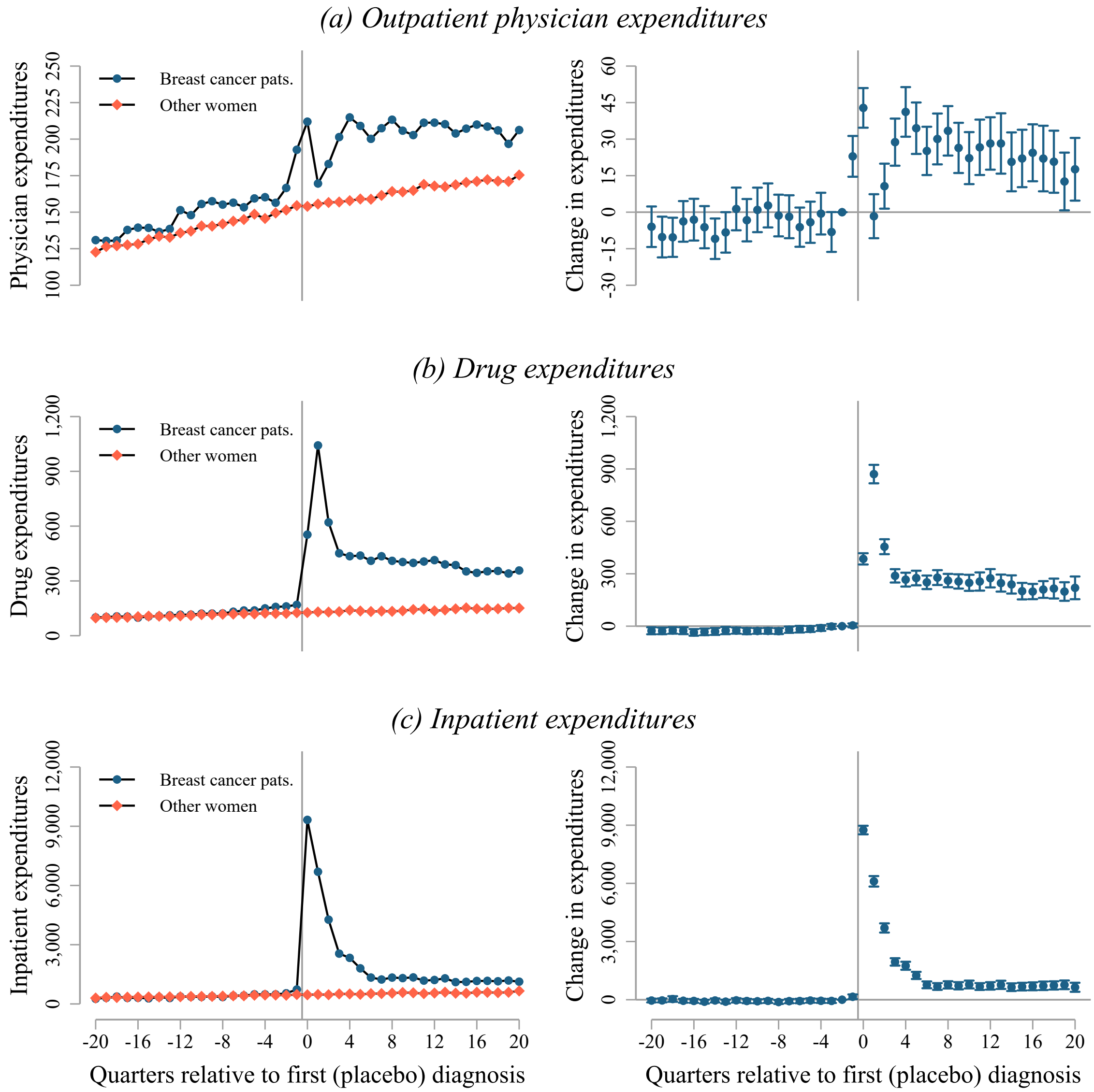


Notes: This figure shows the effect of a breast cancer diagnosis on total healthcare expenditures. In the left panel, we plot raw means over time relative to the first (placebo) breast cancer diagnosis, separately for breast cancer patients and a random sample of same-aged women without a diagnosis. In the right panel, we show event study estimates based on equation (1). We reweight the sample using the unit weights defined in equation (2). Each scatter depicts the estimated difference in total healthcare expenditures between breast cancer patients and the random sample of other women in any given quarter relative to two quarters before the (placebo) diagnosis. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors.

healthcare expenditures that persists over time. In the diagnosis quarter, expenditures are estimated to be €9,177 ($p < 0.001$) higher than before the diagnosis; this is almost 13 times as much as the baseline average of quarterly healthcare expenditures in the control group. Over the next six quarters, expenditures seem to flatten out but remain at an elevated level. After five years, women with breast cancer have about €878 ($p < 0.001$) or 22.8 percent higher expenditures than those without breast cancer.

What drives these increases in healthcare takeup? In Figure 3, we split expenditures into the main components of overall healthcare spending in our data: outpatient physician expenses (panel a), drug expenses (panel b), and inpatient expenses (panel c). We see similar trends for each of these outcomes. Before diagnosis, spending in breast cancer patients is roughly the same as in other women, but we see that outpatient physician expenditures slightly increase in the last quarter before

Figure 3 — Effects of breast cancer on different types of healthcare



Notes: These figures show the effect of a breast cancer diagnosis on outpatient physician expenditures (panel a), drug prescriptions (panel b), and inpatient expenditures (panel c). The graphs on the left display raw means over time relative to the first (placebo) breast cancer diagnosis, separately for breast cancer patients and a random sample of same-aged women without a diagnosis. The graphs on the right show event study estimates based on equation (1). We reweight the sample using the unit weights defined in equation (2). Each scatter depicts the estimated difference in expenditures between breast cancer patients and the random sample of other women in any given quarter relative to two quarters before the (placebo) diagnosis. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors.

diagnosis, relative to the control group. This is likely a mechanical effect, since breast cancer is often first diagnosed in the outpatient sector. The large jump in the diagnosis quarter stems mostly from inpatient spending, which increases almost twenty-fold. Drug expenditures peak in the first quarter after diagnosis, with spending being more than 7 times higher than before diagnosis. While outpatient expenditures more strongly converge to pre-diagnosis levels, inpatient and drug spending remain elevated up to five years after diagnosis.[13] Our estimates suggest a 179 percent ($p < 0.001$) increase in long-term drug expenditures and a 145 percent ($p < 0.001$) increase in inpatient expenditures.

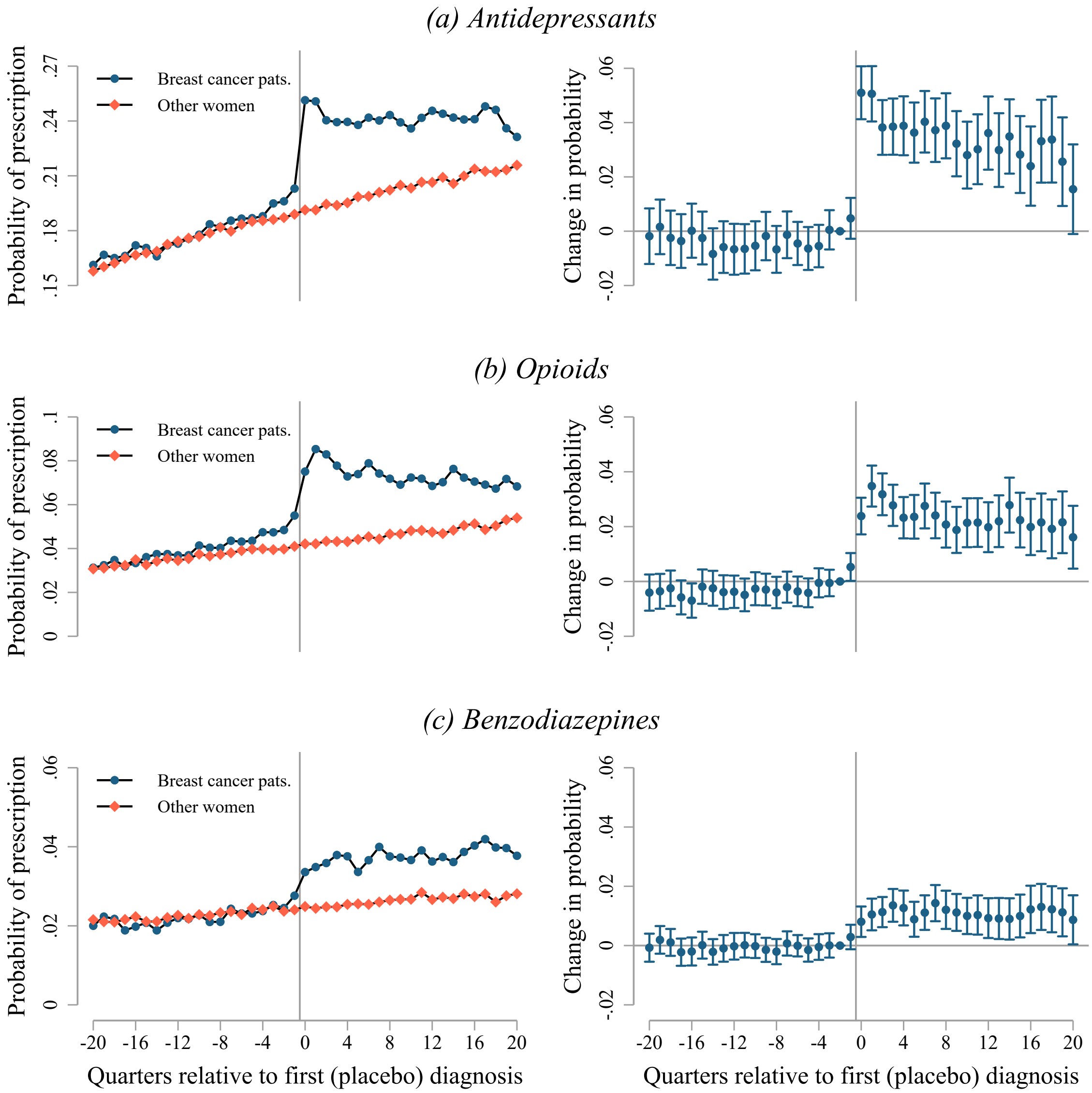
An interesting question is whether women diagnosed with breast cancer were generally more likely to seek medical help even before the diagnosis. The evidence in Figure 3 suggests not, as there are few visible baseline differences in healthcare utilization between breast cancer patients and other women. Additionally, in Figure A.3, we estimate effects on preventative care takeup.[14] This primarily includes mammograms, which are the standard clinical test for breast cancer. It turns out that the treatment group is not more likely to use preventative care before diagnosis, again suggesting that breast cancer constitutes a health shock that is difficult to anticipate. Also, another implication is that there is scope for policy to better target high-risk women, which we discuss below. After diagnosis, breast cancer patients do use significantly more preventative care. After five years, expenses are more than double the pre-diagnosis average. The post-diagnosis effect is probably mechanical, because patients in remission are regularly screened for cancer recurrence.

Lastly, in Figure 4, we test for impacts on certain drug prescriptions that likely have important implications for patient wellbeing: antidepressants (panel a), opioids (panel b), and benzodiazepines (panel c). For all three of these drugs, we see massive increases post-diagnosis. For example, the probability of having an antidepressant prescription increases by 5.1 percentage points ($p < 0.001$) immediately after diagnosis, which is about 27.2 percent of the pre-diagnosis sample mean in the

[13]What drives the increase in drug spending? In Figure A.2, we show what drug prescriptions increase most between quarter $t = 2$ and the pre-diagnosis period (panel a), and what drugs are generally prescribed most often in $t = 2$ (panel b) among breast cancer patients. The largest increases we find for antinauseants (perhaps prescribed to combat side-effects of radiation and chemotherapy), as well as endocrine therapy and immunostimulants (both used as drug-based breast cancer treatment). More than half of all patients receive endocrine therapy post-diagnosis, and around 20 percent receive mineral supplements, ACE inhibitors, antacids, or psychoanaleptics.

[14]To measure preventative care takeup in our data, we identify mammograms, well visits, Pap smears, vitamin prescriptions (ATC code A11), mineral supplements (A12), antihypertensives (C02), statins (C10), antithrombotic drugs (B01), and osteoporosis drugs (M05). We include drugs like statins because their primary purpose is to prevent more serious disease, but our results look similar if we only consider mammograms.

FIGURE 4 — Effects of breast cancer on drug prescriptions



Notes: These figures show the effect of a breast cancer diagnosis on the probability of an antidepressant prescription (panel a), an opioid prescription (panel b), and a benzodiazepine prescription (panel c). The graphs on the left display raw means over time relative to the first (placebo) breast cancer diagnosis, separately for breast cancer patients and a random sample of same-aged women without a diagnosis. The graphs on the right show event study estimates based on equation (1). We reweight the sample using the unit weights defined in equation (2). Each scatter depicts the estimated difference in prescriptions between breast cancer patients and the random sample of other women in any given quarter relative to two quarters before the (placebo) diagnosis. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors.

control group. This is in line with mental health deteriorating after a breast cancer diagnosis. After five years, prescription takeup is still 1.5 percentage points ($p < 0.1$) or 8.3 percent higher than before diagnosis. Opioids, which are powerful and highly addictive pain medications, increase too. After five years, prescriptions are up by about 1.6 percentage points ($p < 0.05$) or 40.5 percent from a low baseline. Benzodiazepines prescriptions—similarly powerful and addictive drugs, often used to treat anxiety or sleeping problems—increase by around 1 percentage point ($p < 0.05$) or 36.6 percent. Taken together, these results imply that breast cancer has important consequences for the healthcare sector and patient wellbeing.

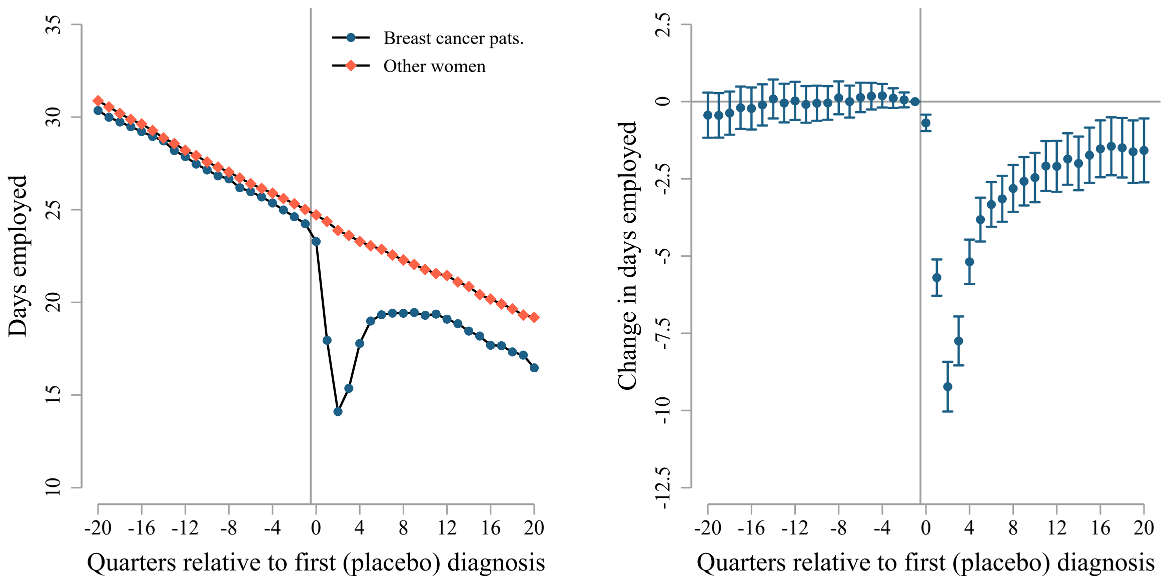
In Table A.2, we probe the robustness of these estimates to different specifications of equation (1). We provide average post-diagnosis effect estimates when estimating the model without any control variables (column 1) and then sequentially add individual-level fixed effects (column 2), age fixed effects (column 3), and calendar year fixed effects (column 4). In column (5), we also show estimates when we do not use unit weights as in equation (2). Across outcomes in panel (a), estimates do not seem to depend on covariate choice or reweighting. In Figures A.4 (total healthcare expenses) and A.5 (types of care and drug prescriptions), we test if our results change if we estimate event studies using the Sun & Abraham (2021) interaction-weighted estimator instead of twoway fixed effects, which accounts for potential effect heterogeneity across treatment cohorts. Results obtained from the Sun & Abraham estimator are virtually the same as our baseline estimates.

V.2. Labor market attachment

We have established that breast cancer affects both physical and mental health, but it may also impact women's livelihoods. In this section, we therefore estimate effects on labor market outcomes and social safety program takeup for women who survive. If breast cancer not only leads to increased healthcare spending but also pushes otherwise healthy women out of the labor market, this has important implications for the government's budget constraint.

In Figure 5, we report effects on days employed. We note that in both the treatment and control group, employment is trending downward over time because most diagnoses occur at a time when women are gradually approaching retirement. We consider average days of employment to capture both the extensive and the intensive margin of labor force participation; women who had already

Figure 5 — Effects of breast cancer on employment



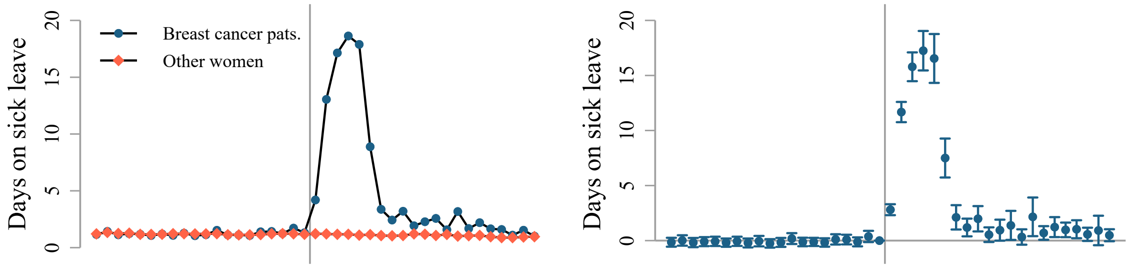
Notes: This figure shows the effect of a breast cancer diagnosis on days of employment. In the left panel we plot raw means over time relative to the first (placebo) breast cancer diagnosis, separately for breast cancer patients and a random sample of same-aged women without a diagnosis. In the right panel we show event study estimates based on equation (1). We reweight the sample using the unit weights defined in equation (2). Each scatter depicts the estimated difference in employment days between breast cancer patients and the random sample of other women in any given quarter relative to the last pre-diagnosis quarter. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors.

been out of the labor force prior to their diagnosis have zero employment days and do not contribute to the fixed effects event study estimates in the right panel. Similar to the previous figures there is virtually no difference in pre-diagnosis employment levels and both the treatment and control group trend along similar paths. After diagnosis, the pattern we observe is almost a mirror image of healthcare spending in Figure 2. There is an immediate drop that is largest in quarter two, where women reduce employment by about 9.2 days ($p < 0.001$) or 36.9 percent. Over time, most affected women appear to return to the labor market, but we still see that average quarterly employment remains 1.6 days ($p < 0.05$) or 6.3 percent lower five years after diagnosis.

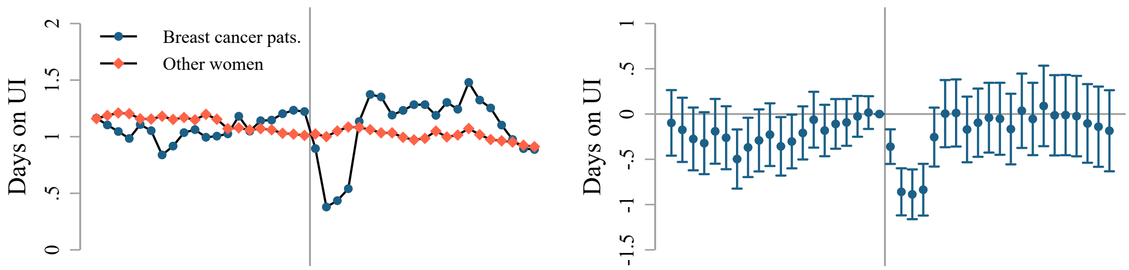
In Figure 6, we examine effects on social safety program takeup. In particular, we consider sick leave (panel a), UI (panel b), and DI (panel c). Sick leave in Austria is designed to keep people in employment despite falling ill. Indeed, we see an immediate uptick in sick leave takeup during

FIGURE 6 — Effects of breast cancer on social safety program takeup

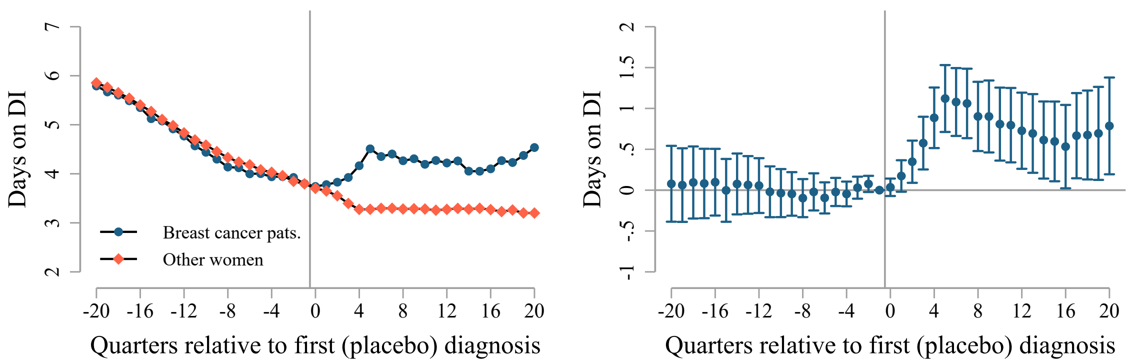
(a) Days on sick leave



(b) Days on UI



(c) Days on DI



Notes: These figures show the effect of a breast cancer diagnosis on days of sick leave (panel a), days on UI (panel b), and days on DI (panel c). The graphs on the left display raw means over time relative to the first (placebo) breast cancer diagnosis, separately for breast cancer patients and a random sample of same-aged women without a diagnosis. The graphs on the right show event study estimates based on equation (1). We reweight the sample using the unit weights defined in equation (2). Each scatter depicts the estimated difference in days on sick leave, UI, or DI between breast cancer patients and the random sample of other women in any given quarter relative to the last pre-diagnosis quarter. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors.

the first year after diagnosis, up to 17 days per quarter ($p < 0.001$) or 15 times as much as the control group baseline, and this effect is larger than the drop in employment shown in Figure 5. Interestingly, over time, sick leave takeup converges back to pre-diagnosis levels.

What happens to women who leave their jobs? We see practically no effects on UI takeup. There is about a one-day drop in the first year after diagnosis, but this is likely a mechanical relationship due to program substitution, because women on sick leave cannot be laid off and claim UI. However, DI takeup increases sharply and persistently. After five years, days on DI are up by around 0.8 days per quarter ($p < 0.05$) or 20.7 percent. This translates to around 72 more DI claims in the treatment group of 5,180 women. To sum up, we find that breast cancer leads to a reduction in labor market attachment accompanied by increases in sick leave and DI. These estimates do not depend on covariate choice or applying unit weights either (Table A.2), and there is virtually no difference between twoway fixed effects and Sun & Abraham (2021) interaction-weighted estimates (see Figure A.6). For some outcomes, the interaction-weighted estimates are much more precise, which is one of the reasons we report the more conservative twoway fixed effects estimates as our baseline.

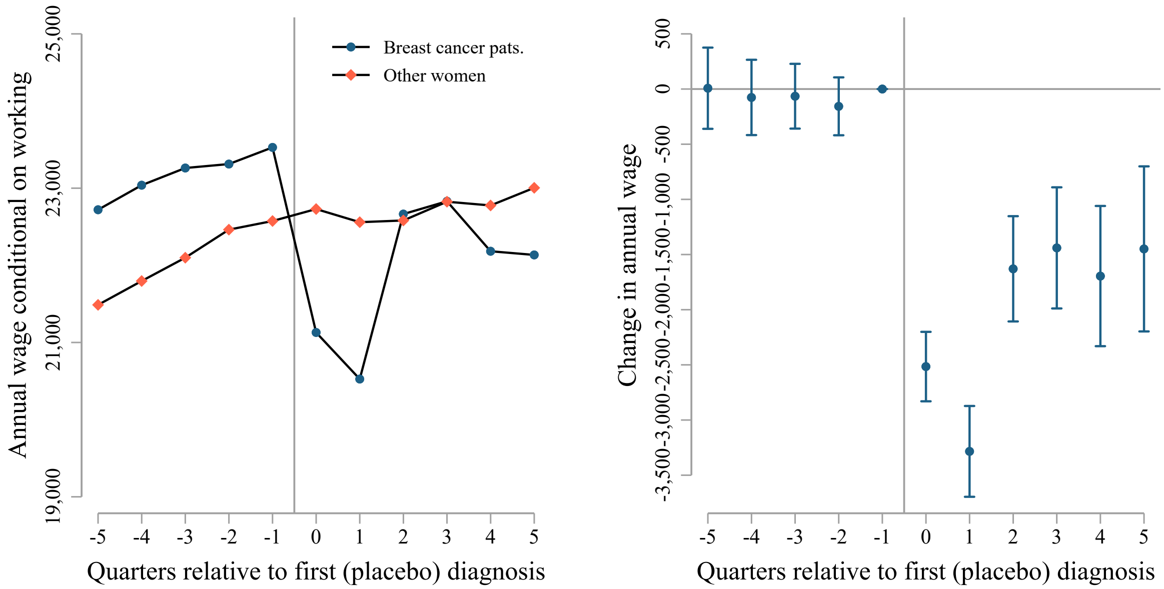
VI. Breast cancer and wages

We now turn our focus to women who, despite having a breast cancer diagnosis, are able to remain in the labor market. To better understand how labor market outcomes are affected by the cancer diagnosis, we consider a catch-all measure that reflects a variety of behavioral choices and is intuitive to economists: wages. In this section, we first establish the baseline effect of breast cancer on wages, estimate wage effects at different parts of the pre-diagnosis wage distribution, and then ask through which mechanisms a breast cancer diagnosis can affect equilibrium wages.

VI.1. The wage penalty of breast cancer

In Figure 7, we show effects on annual wages in € for women who remain in the labor market. We now use annual data with t in equation (1) ranging from -5 to 5, because wages are only available on a yearly basis in our data. We note that, although breast cancer patients earn slightly more than other women, on average, their wage trajectories prior to the (placebo) diagnosis are similar. When comparing covariate-adjusted changes in annual wages over time, we see a large drop in the

FIGURE 7 — Effects of breast cancer on wages

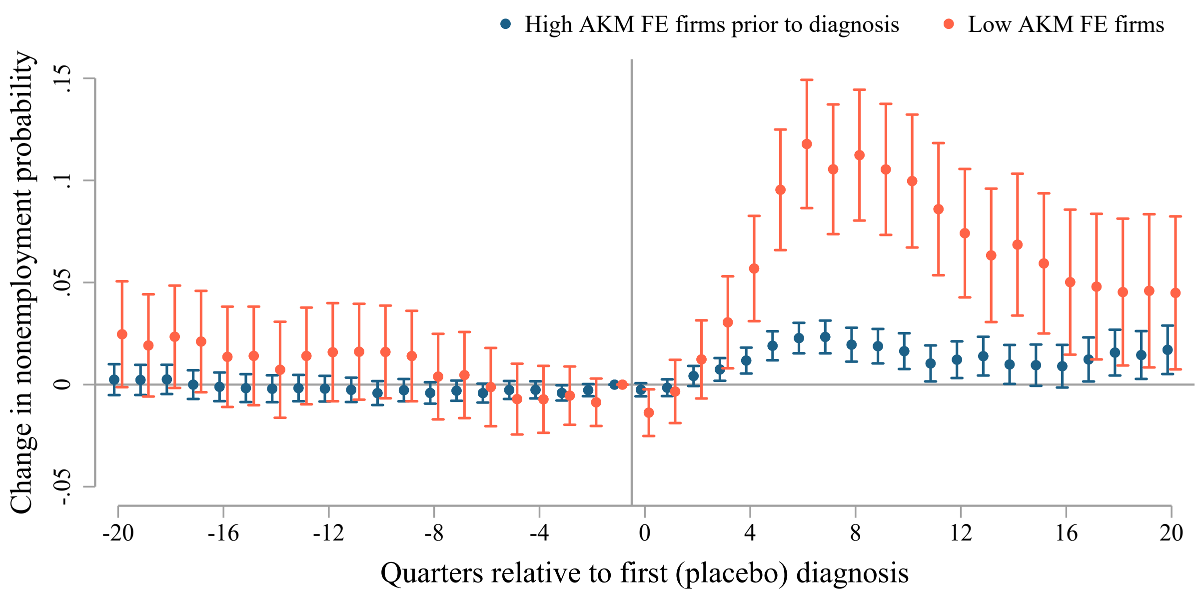


Notes: This figure shows the effect of a breast cancer diagnosis on annual wages conditional on employment. In the left panel, we plot raw means over time relative to the first (placebo) breast cancer diagnosis, separately for breast cancer patients and a random sample of same-aged women without a diagnosis. In the right panel, we show event study estimates based on equation (1). We reweight the sample using the unit weights defined in equation (2). Each scatter depicts the estimated difference in wages between breast cancer patients and the random sample of other women in any given quarter relative to the last pre-diagnosis year. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors.

first year after diagnosis. The coefficient estimate for $t = 1$ is €3,284 ($p < 0.001$), suggesting a wage reduction of nearly 15 percent compared to pre-diagnosis levels. Even after five years, wages remain around €1,450 ($p < 0.001$) or 6.4 percent lower. We interpret this as the long-term wage penalty of breast cancer. Estimates are similar across model specifications (see Table A.2) and applying the Sun & Abraham (2021) interaction-weighted estimator does not change results either (see Figure A.7).

We now ask how breast cancer affects the *distribution* of post-diagnosis wages, and in particular if we see a uniform shift in the distribution or if certain parts of the distribution are more affected than others. In Figure A.8, we estimate separate difference-in-differences models for different intervals of the wage pdf. The orange bars represent the baseline wage distribution. We see that the probability of being in the middle of the wage distribution decreases due to breast cancer, while

FIGURE 8 — Effects of breast cancer on the probability of nonemployment, by average of pre-diagnosis AKM firm fixed effects



Notes: This figure depicts the effect of a breast cancer diagnosis on the probability of exiting the labor market separately by the pre-diagnosis average of AKM fixed effects across firms the patient worked at (see Appendix C), which we split at the sample median. We show event study estimates based on equation (1). We reweight the sample using the unit weights defined in equation (2). Each scatter depicts the estimated difference in nonemployment probability between breast cancer patients and the random sample of other women in any given quarter relative to the last pre-diagnosis quarter. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors.

the probability of being at the bottom of the distribution increases. Women at the very top of the distribution are hardly affected. This implies that breast cancer moves some women from the middle to the left tail of the wage distribution. In the next section, we will explore what can explain the breast cancer wage penalty.

VI.2. What explains the persistent wage penalty?

We propose two hypotheses that may explain the 6.4 percent long-term wage penalty: Either women sort into lower-wage jobs or, conditional on staying in a similar job, they work fewer hours. Breast cancer certainly has the potential to change preferences over jobs or incapacitate women to the extent that they are no longer able to perform in their preferred job. We now discuss these two mechanisms in turn.

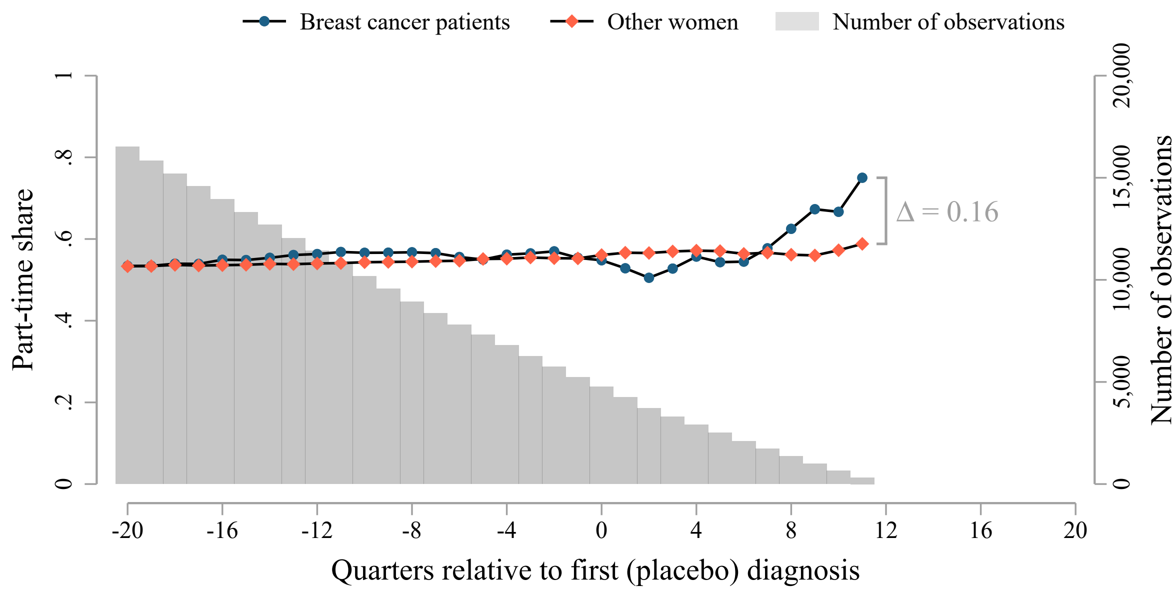
We find little evidence that affected women are moving to lower-quality jobs. Since we have no information on occupations, we approximate job quality using estimated AKM firm fixed effects (Abowd et al. 1999).[15] Consistent with the literature, we classify firms with larger AKM fixed effects as higher-quality firms. In Figure 8, we estimate treatment effects on the probability that women leave the labor market separately by whether they were employed in a firm with high and low AKM fixed effects before diagnosis. We find that women increasingly exit the labor market after a breast cancer diagnosis, but the effect is much stronger for those in low-quality firms. This would imply that average firm quality is actually *increasing* for women in the treatment group. Indeed, if we estimate dynamics in firm quality over time (Figure A.9), we find positive point estimates for women with breast cancer relative to other women. This implies that changes in firm quality alone cannot explain the wage penalty, because women actually seem to sort into better jobs after a breast cancer diagnosis.

Even for women who do not change jobs, we find a significant wage penalty. In Figure A.10, we plot treatment effects on wages from equation (1) separately for workers who stay with their employer and for those who switch firms at some point in the five years after diagnosis. Switching is relatively rare; more than 82 percent of women do not change firms, and this share is similar in the treatment and control group. We find that women who move firms have lower long-term negative wage effects, likely reflecting that they tend to move to better firms, on average. Women who remain with the same employer suffer a long-term wage penalty of about €1,043, which is similar to our baseline estimate. To sum up, women do not seem to sort to worse firms, and even those staying with the same employer face a wage penalty. This is clear evidence that firm quality is unlikely to explain the breast cancer wage penalty.

An alternative explanation for the wage penalty is that women are reducing hours, especially because even those staying with the same firm experience a reduction in wages. This is difficult to test because we do not observe working hours in our data, but we can look at two indicators for

[15] The AKM model assumes that wages can be written as an additive function of worker characteristics and firm fixed effects, where the firm fixed effects can be interpreted as a measure of firm quality. We estimate these fixed effects based on the full Austrian employer-employee panel from 1972 to 2022. See Appendix C for more information.

Figure 9 — Share of parttime workers around a (placebo) breast cancer diagnosis



Notes: This figure plots the share of parttime workers based on payslip data from the Austrian Department of Finance between breast cancer patients and the random sample of other women at any given quarter relative to the quarter before the (placebo) diagnosis. Because we have parttime information only between 2005q1 and 2012q4, we also plot the number of available observations in each quarter. The longest we can observe a woman is 11 quarters post-diagnosis—i.e., all women with a (placebo) diagnosis in 2010q1, where $t = 1$ is in 2010q2, and $t = 11$ is in 2012q4, the last quarter we have in our payslip data. We reweight the sample using the unit weights defined in equation (2).

hours reductions in different registers. First, we match payslip data from the Austrian Department of Finance, which contain information on parttime work.[16] These data end in 2012, so the longest we can observe the parttime share post-diagnosis is 11 quarters, and the farther we look into the future, the fewer observations we have. We plot changes in the share of women who work parttime in Figure 9. Before the diagnosis, around 50 percent of women work parttime, in both the treatment and control group. However, 11 quarters after the diagnosis, women in the treatment group who we still observe are 16 percentage points more likely to work parttime than other women, which is a relatively large increase.

As a second indicator, we can match information on old-age parttime work from the Austrian UI office. This is a policy that allows people to reduce hours by 40 to 60 percent five years before

[16]Parttime work can mean anything below the collectively bargained standard working hours, which are usually either 38.5 or 40 hours per week. In most cases, parttime workers are employed for either 20 or 30 hours per week.

the statutory retirement age (65 for men and 60 for women), with the government covering half of lost wages. Participation in the old-age parttime work scheme is relatively low; in our sample the share of workers participating is always below 2 percent in a given year. When estimating effects on old-age parttime work in Figure A.11, our event study estimates are noisy and not statistically significant. However, considering the raw data trends in the left graph and the point estimates in the right graph, it seems that participation is slightly increasing post-diagnosis. Taken together, these results suggest that a reduction in working hours, and not changes in job quality, are responsible for the decline in wages. In the next section, we ask what motivates women to reduce work hours.

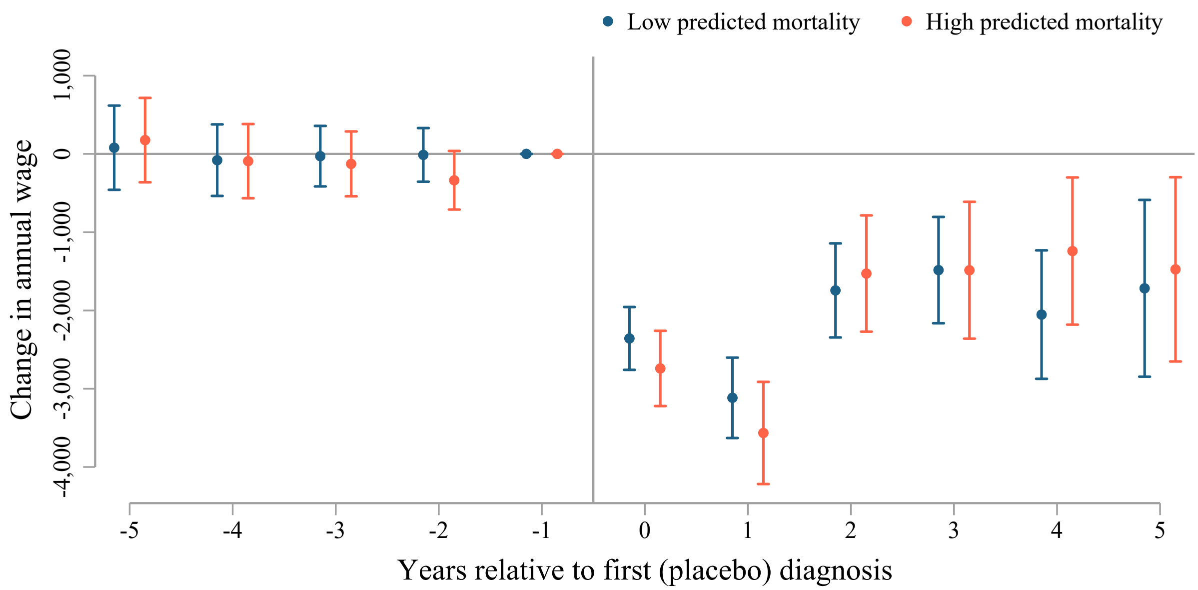
VI.3. What explains the reduction in hours?

We have seen that women diagnosed with breast cancer suffer persistent wage losses because they increase parttime work. There are three plausible explanations for that. First, breast cancer could lead to an incapacitation effect, either because of the mental and physical demands of the disease itself or because of breast cancer treatment, which often involves invasive and time-intensive procedures, such as breast or tissue removal, radiation, and/or chemotherapy. Second, a breast cancer diagnosis might affect the labor-leisure tradeoff by increasing women's time preference rate, in the sense that the present becomes more important than the future and labor supply decreases.[17] There is evidence in psychology and behavioral economics that stressful events change time discounting (e.g., Bogliacino et al. 2021, Cassar et al. 2017, Haushofer & Fehr 2014) and that time preference is correlated with health status (Ciancio et al. 2024, Norrgren 2022). Third, employers might discriminate against women with breast cancer, which could also lower equilibrium wages. We now discuss each of these three potential mechanisms.

A first sign that incapacitation is unlikely to explain the full wage effect is that sick leave takeup converges back to pre-diagnosis levels (see Figure 6, panel a). If breast cancer had long-term health effects serious enough to prevent surviving women from working fulltime, we would likely see that sick leaves remain elevated for the treatment group but not the control group. Also, if

[17] Or, in more formal terms, that the relative weight of current utility over future utility increases. If labor is a disutility, we expect more leisure be consumed if the time preference rate increases.

Figure 10 — Wage effect by cancer severity



Notes: In this figure, we plot the effect of breast cancer on wages from Figure 7 separately for patients with high predicted mortality and low predicted mortality based on the sample median of predicted mortality. In Appendix D, we explain how we calculate predicted mortality. We reweight the sample using the unit weights defined in equation (2). Each scatter depicts the estimated difference in annual wages between breast cancer patients and the random sample of other women in any given year relative to the last pre-diagnosis year. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors.

incapacitation mattered, we would expect women with more severe forms of breast cancer to have stronger negative labor market effects than other affected women. In lieu of information on tumor size and metastases, we (1) compare wage outcomes between women who undergo different types of cancer therapy (drugs, radiation, chemotherapy, surgery) and (2) predict severity based on a number of characteristics that potentially affect severity (including age, baseline health, the number of children, education, and the types of cancer therapy; see Appendix D for more information).[18]

In Figure A.12, we plot average wage effects by treatment type. The categories are not mutually exclusive; drugs, for example, are almost always prescribed, also in combination with chemotherapy or radiation. Surgery is usually a sign that the cancer has not metastasized yet. While effects differ in magnitude, we observe significant and substantial wage penalties for all four types of therapy.

[18] Some types and combinations of therapy are an indicator that the cancer was more severe. For example, breast surgery is usually only performed if the cancer has not metastasized yet. If only drug therapy is prescribed, it is likely that the cancer has already metastasized when the patient presents to the clinic. See section II.2 for more information.

In Figure 10, we use predicted mortality as a proxy for cancer severity. We split the sample into women with low and high predicted mortality, based on observable characteristics. Women with more severe types of cancer have larger initial wage penalties, but the long-term wage effect is not statistically different between women with low and high predicted mortality. We argue that this result is not consistent with incapacitation playing a crucial role in explaining the breast cancer wage penalty.

We find more evidence that a change in the time preference rate seems to explain the hours reduction and the resulting wage penalty. We first note that we have seen in section V.1 that both opioid and benzodiazepine use not only increase sharply after diagnosis but remain elevated even after five years. While short-term use of opioids and benzodiazepines may be medically necessary to combat pain and anxiety, prolonged use over multiple years may be a sign of risky behavior, which is consistent with an increase in the time preference rate. In Figure A.13, we additionally look at other measures of risky behavior, including hospital diagnoses indicating smoking, alcohol misuse, obesity, sexually transmittable disease, or accidents and injuries. Risky behavior is difficult to measure in medical claims data, hence this variable is noisy, but we do find suggestive evidence that it is increasing after a breast cancer diagnosis.

We present two more pieces of evidence that are consistent with an increase in the time preference rate: Women diagnosed with breast cancer reduce human capital investment and fertility, which we view as tradeoffs between future benefits and present-day costs. We plot trends in adult education takeup (which is sometimes government-subsidized in Austria) and fertility around breast cancer diagnoses in Figures A.14 and A.15, respectively. For these outcomes, we have little quarterly outcome variation, so we cannot reasonably estimate event studies. However, we can compare outcome means before and after diagnosis between women with breast cancer and other women (akin to a simple two-by-two difference-in-differences model). We see that both adult education and fertility decrease. Although we note that the fertility effect could well be a biological consequence of breast cancer itself, both patterns are in line with patients valuing current utility more than future utility.

So far, we have insinuated that the hours reduction is a deliberate choice that women make. Another possibility is that women's preferences over labor supply do not actually change, but that employers discriminate against breast cancer patients (e.g., because they expect future productivity to decline). To test for employer discrimination, we compare employment trends between women who had been employed at firms with varying degrees of monopsony power before diagnosis. The idea is that, in competitive labor markets, it is more difficult for employers to discriminate against certain types of workers. If discrimination played a role, we would therefore expect women in firms with less monopsony power to exit the labor market at lower rates than their counterparts in firms with high monopsony power. This is not what we find. In Figure A.16, we plot treatment effects on the probability of nonemployment over time separately by whether women had worked at firms with low or high monopsony power before diagnosis. We approximate monopsony power by sectoral Herfindahl-Hirschman indices (HHI) and split the sample at the median HHI across firms.[19] There is little evidence that labor market exits differ between women employed at firms with low and high monopsony power, which is not consistent with employer discrimination systematically driving the results above. We conclude that a change in the time preference rate is likely more important in explaining the wage penalty than incapacitation or employer discrimination.

VII. Comparison with other cancer types

We have shown that breast cancer diagnoses lead to higher healthcare expenditures, reduced labor market attachment, and lower wages for women who continue to participate in the labor market. But how does breast cancer compare to other forms of cancer? This is a particularly interesting question, because medical advances have made other types of cancer better treatable too. For example, so-called PARP inhibitors have revolutionized treatment of ovarian cancer, significantly improving survival rates for women with certain genetic mutations (Hockings & Miller 2023). The same is true for cancers that primarily befall men. Prostate cancer patients, in particular, have much

[19] We assume that a higher HHI means higher concentration at the labor market and more monopsony power for firms, and vice versa.

higher survival rates than in the past thanks to targeted hormone therapy (Halabi et al. 2024). Since more people today are living with cancer than ever before, it is useful to examine labor market and health outcomes of the disease more broadly.

In this section, we therefore compare wage penalties across different cancer types, for both men and women. We do this by repeating our sampling and estimation procedure outlined in section IV for the 15 most common cancers for each gender; first drawing all patients with a cancer diagnosis, then matching similarly-aged men or women without cancer diagnoses, and finally estimating models similar to the one in equation (1). We then plot average wage effects for each cancer against the five-year survival rate, which we interpret as a proxy for disease severity.

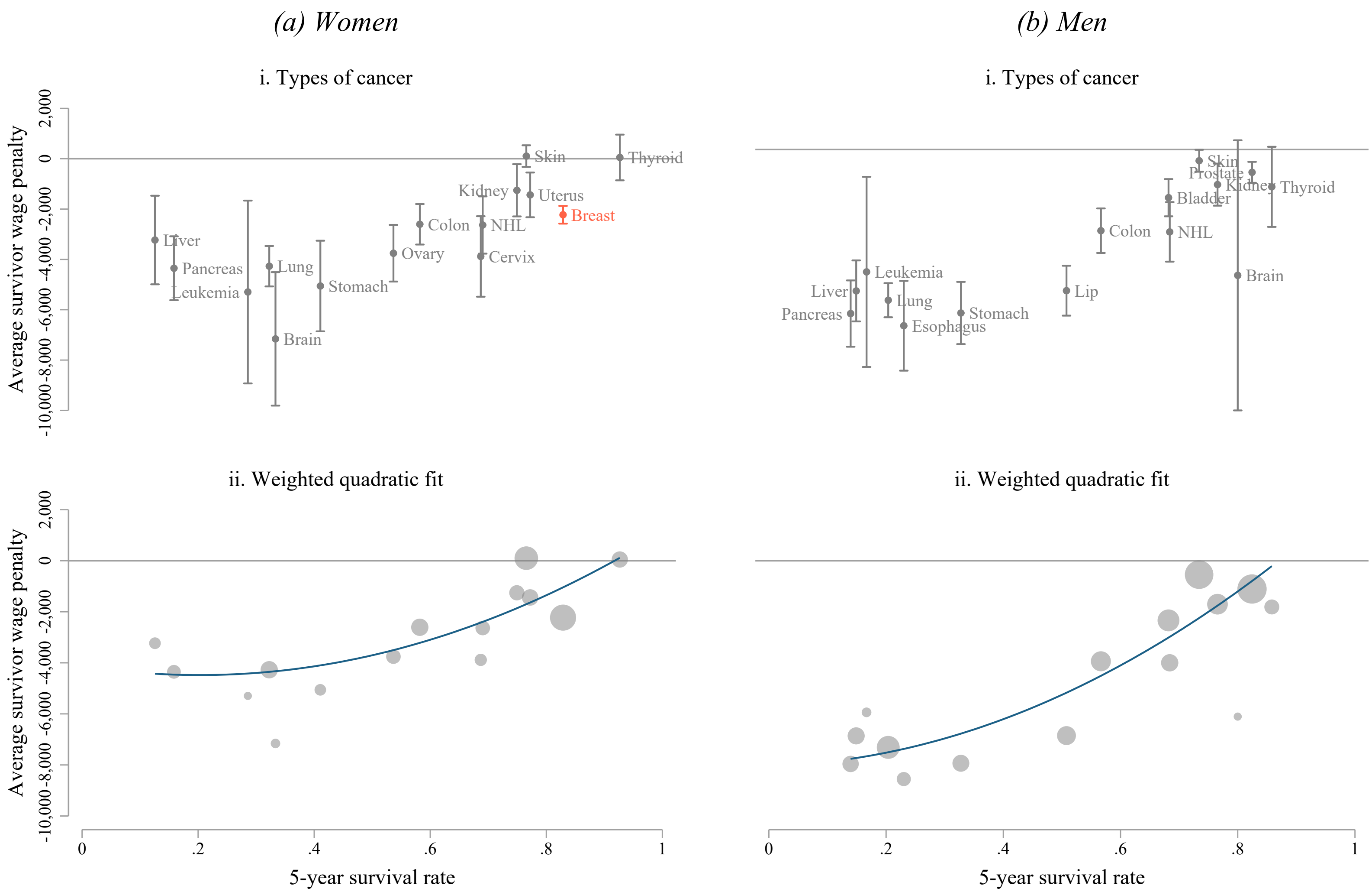
We summarize results in Figure 11. In panels (a) i. and (b) i., we plot average five-year wage penalties with 95 percent confidence intervals across cancer types for women and men, respectively. In panels (a) ii. and (b) ii., we weight the wage penalty estimate by its inverse standard error, which accounts for the fact that some estimates are more precise than others, and provide quadratic fits for the relationship between the wage penalty and the five-year survival rate.

We note three key results. First, breast cancer has the second highest survival rate but carries a larger wage penalty than thyroid, uterus, skin, or kidney cancer in women, which have similarly high survival rates. Second, we find the largest wage penalties for brain cancer, leukemia, and stomach cancer in women, and esophagus, stomach, and pancreas cancer in men. Wage penalties for these more severe forms of cancer are much larger than for breast cancer, ranging from €6,000 to €8,000 per year, on average. Third, wage penalties seem to be quadratically decreasing for less severe types of cancers, which we proxy by the five-year survival rate.

VIII. Conclusion

Medical advances have made breast cancer better treatable in recent years, but this also means that more and more women today are living with the disease. In this paper we study the long-term health and labor effects of a breast cancer diagnosis. Relative to same-aged unaffacted women, those diagnosed with breast cancer have higher healthcare expenses, more antidepressant, opioid,

FIGURE 11 — Wage effects by type of cancer for women and men



Notes: In this figure, we plot average five-year wage penalties for the 15 most common cancer types in Upper Austria for both men and women. To obtain wage penalty estimates, we repeat the same sampling, reweighting, and estimation procedure as outlined in section IV. Panels (a.i) and (b.i) plot estimated wage penalties with 95 percent confidence intervals based on individual-level clustered standard errors against the five-year cancer survival rate, which we estimate from data on all Upper Austrian cancer patients first diagnosed between 2005q1 and 2016q2 (we can calculate five-year survival rates until 2022q2). 'NHL' means Non-Hodgkin lymphoma. In panels (a.ii) and (b.ii), the scatters and the quadratic fit are weighted by the inverse standard errors from panels (a.i) and (b.i).

and benzodiazepine prescriptions, and lower labor market attachment. Those women who do return to the labor market face a long-term wage penalty of around 6.4 percent. Although affected women tend to match to better firms after diagnosis, this is offset by a reduction in work hours. We posit that incapacitation or employer discrimination can likely not explain the full wage penalty, but that a change in time discounting potentially plays an important role.

We derive three main conclusions for policy from our analysis. First, breast cancer affects both sides of the government budget constraint, because not only does healthcare spending increase, tax revenues (in particular labor tax revenues) are likely to decrease too. Second, there is some evidence that breast cancer screening needs to be better targeted at high-risk women. This observation is based on the fact that we see that breast cancer patients are *not* generally more likely to takeup mammograms prior to diagnosis. Third, differences in breast cancer therapy shape patient labor market outcomes less than previously thought, at least for those who find their way back to the labor market. Although we find slightly smaller wage penalties for patients treated with radiation and/or surgery, wage penalties are negative and large across therapy options.

Our analysis can be applied to other types of cancer too. We find that breast cancer carries a larger wage penalty than cancers with similar survival rates and that wage penalties are quadratically increasing with cancer severity, for both men and women. Overall, we believe that our results can help policy makers to better understand the societal cost of cancer and better target policies that aim at preventing cancer and helping patients return to the labor market if they are diagnosed.

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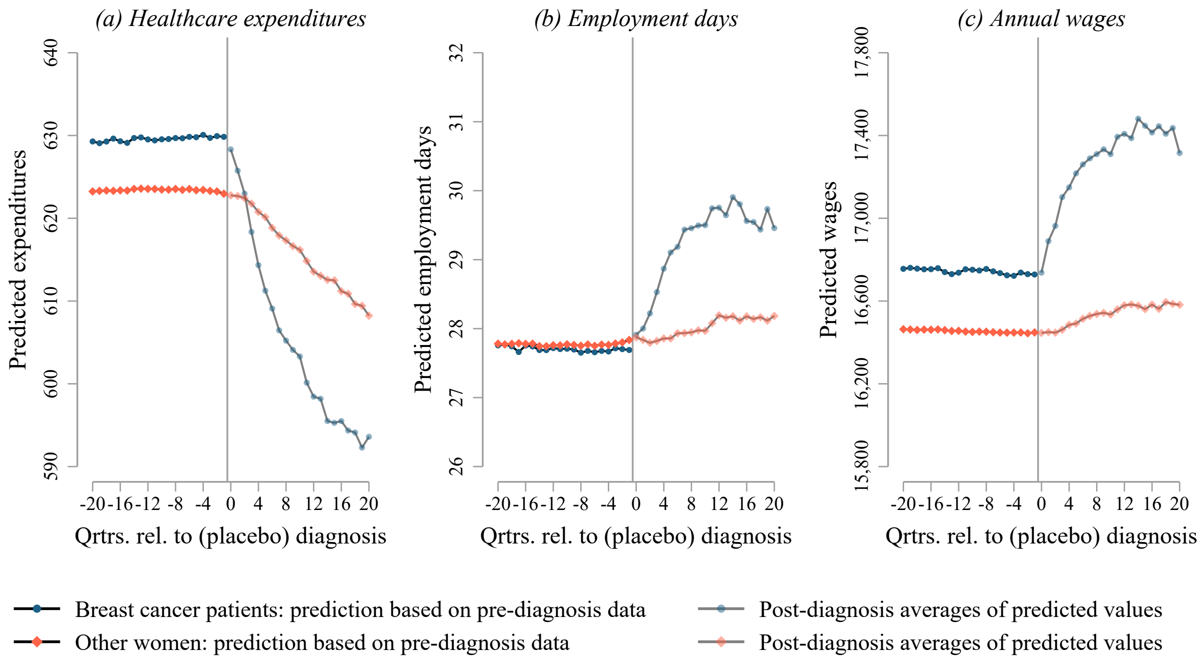
Web appendix

This web appendix contains additional tables and figures for the paper *"The labor and health economics of breast cancer"* by Alexander Ahammer, Gerald J. Pruckner, and Flora Stiftinger.

Contents

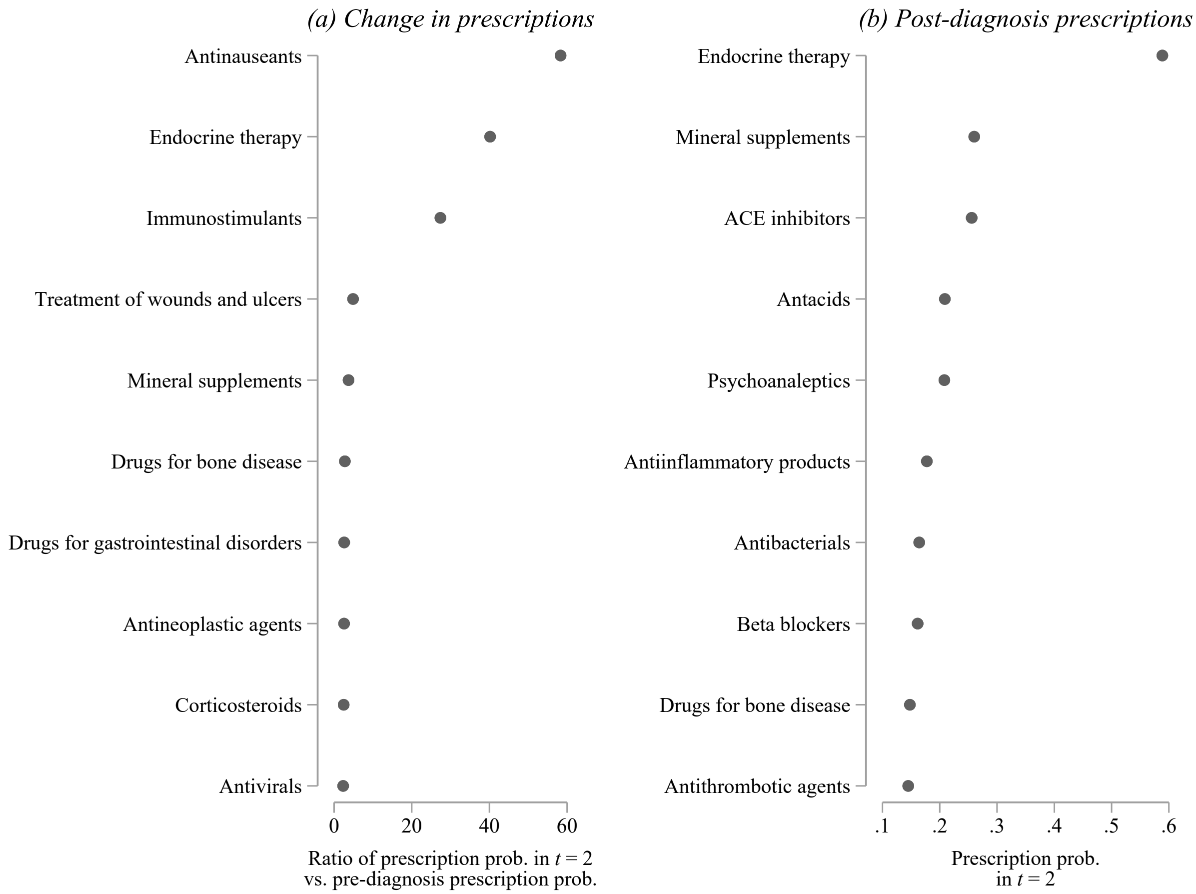
A. Additional tables and figures

Figure A.1 — Change in sample composition: Predicted outcomes based on predetermined characteristics over time



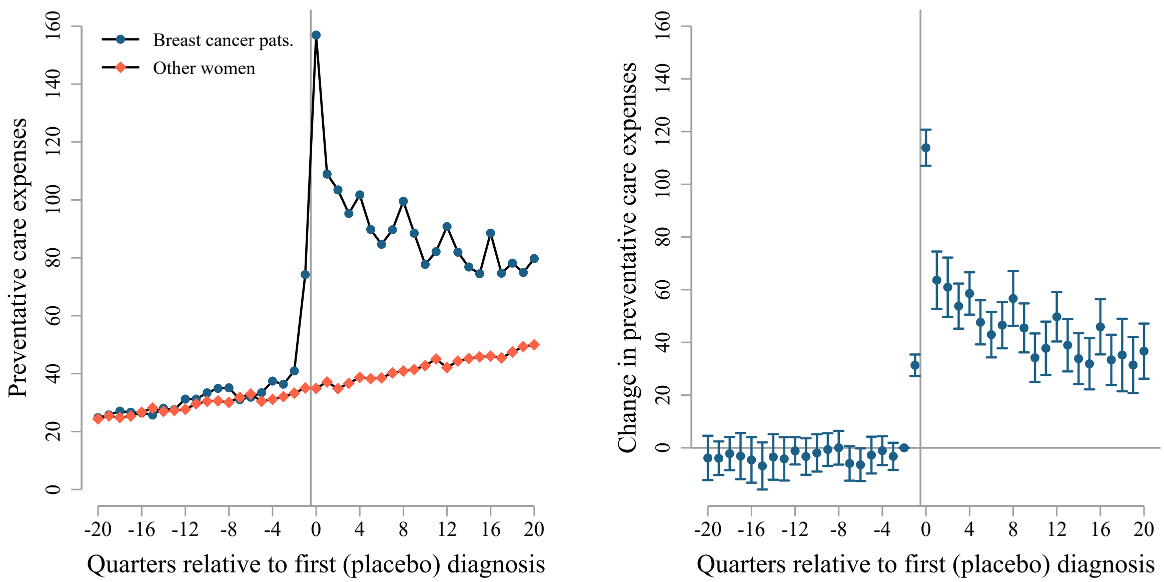
Notes: In this figure, we analyze how post-diagnosis changes in sample composition can affect our event study estimates. The dark blue dots indicate predicted outcomes based only on pre-diagnosis information, including age-at-diagnosis, education, the number of children, wages, and full labor market histories. The light blue dots are, in this case, an out-of-sample prediction: they show how average predicted outcomes evolve given that there is sample attrition after diagnosis. If the light blue dots have a decreasing trend, it means that observations with high predicted outcome values (i.e., characteristics that would predict high values of the outcome) disproportionally leave the sample, and vice versa. We do the same for women in the control group, where predicted values are depicted as orange squares. In column (1) we analyze predicted healthcare expenditures, in column (2) we analyze predicted employment days, and in column (3) we analyze predicted annual wages. We reweight the sample using the unit weights defined in equation (2).

Figure A.2 — Drug prescriptions before and after diagnosis among breast cancer patients



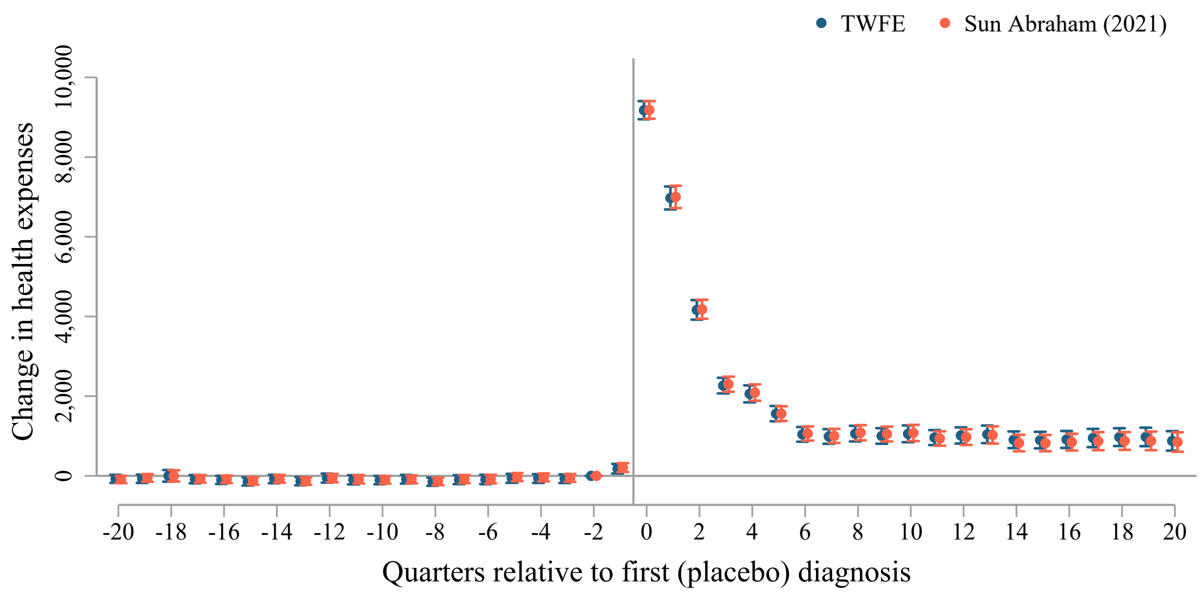
Notes: This figure shows changes in prescriptions and prescription probabilities of certain drugs among women diagnosed with breast cancer. In panel (a), we plot the ratio of the probability of having a certain drug prescription in quarter $t = 2$ relative to the pre-diagnosis prescription probability for women diagnosed with breast cancer, focusing on the ten drugs where the change in the prescription probability is largest. In panel (b), we depict the overall probability that a certain drug is prescribed in quarter $t = 2$ after diagnosis, focusing on the ten drugs that are most likely to be prescribed.

Figure A.3 — Effect of breast cancer on preventative care expenses



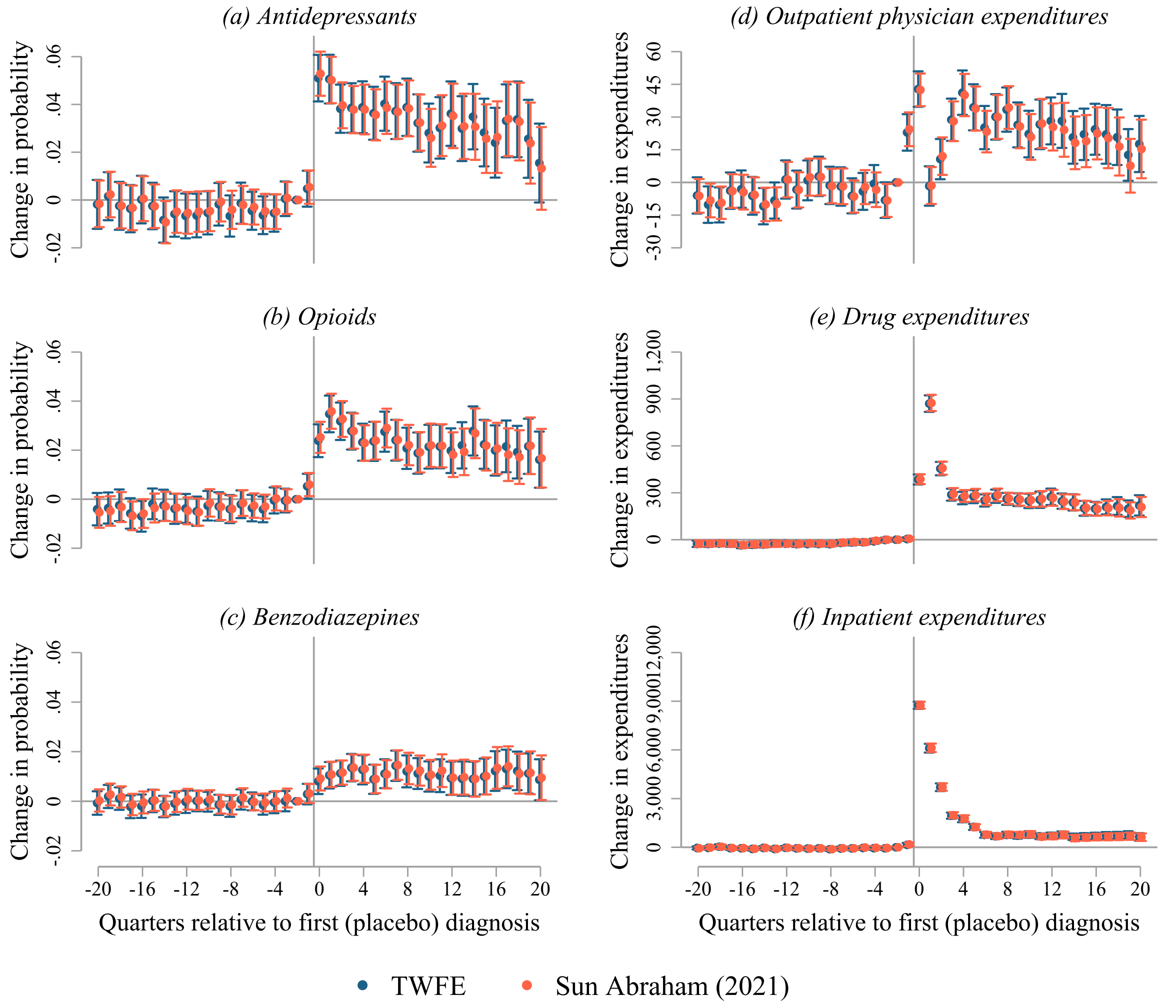
Notes: This figure shows the effect of a breast cancer diagnosis on preventative care expenses, including expenses for mammograms, well visits, Pap smears, vitamin prescriptions (ATC code A11), mineral supplements (A12), antihypertensives (C02), statins (C10), antithrombotic drugs (B01), and osteoporosis drugs (M05). In the left panel we plot raw means over time relative to the first (placebo) breast cancer diagnosis, separately for breast cancer patients and a random sample of same-aged women without a diagnosis. In the right panel we show event study estimates based on equation (1). We reweight the sample using the unit weights defined in equation (2). Each scatter depicts the estimated difference in preventative care expenses between breast cancer patients and the random sample of other women in any given quarter relative to two quarters before the (placebo) diagnosis. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors.

FIGURE A.4 — Effect of breast cancer on total healthcare expenditures, estimated by twoway fixed effects and the Sun & Abraham (2021) interaction-weighted estimator



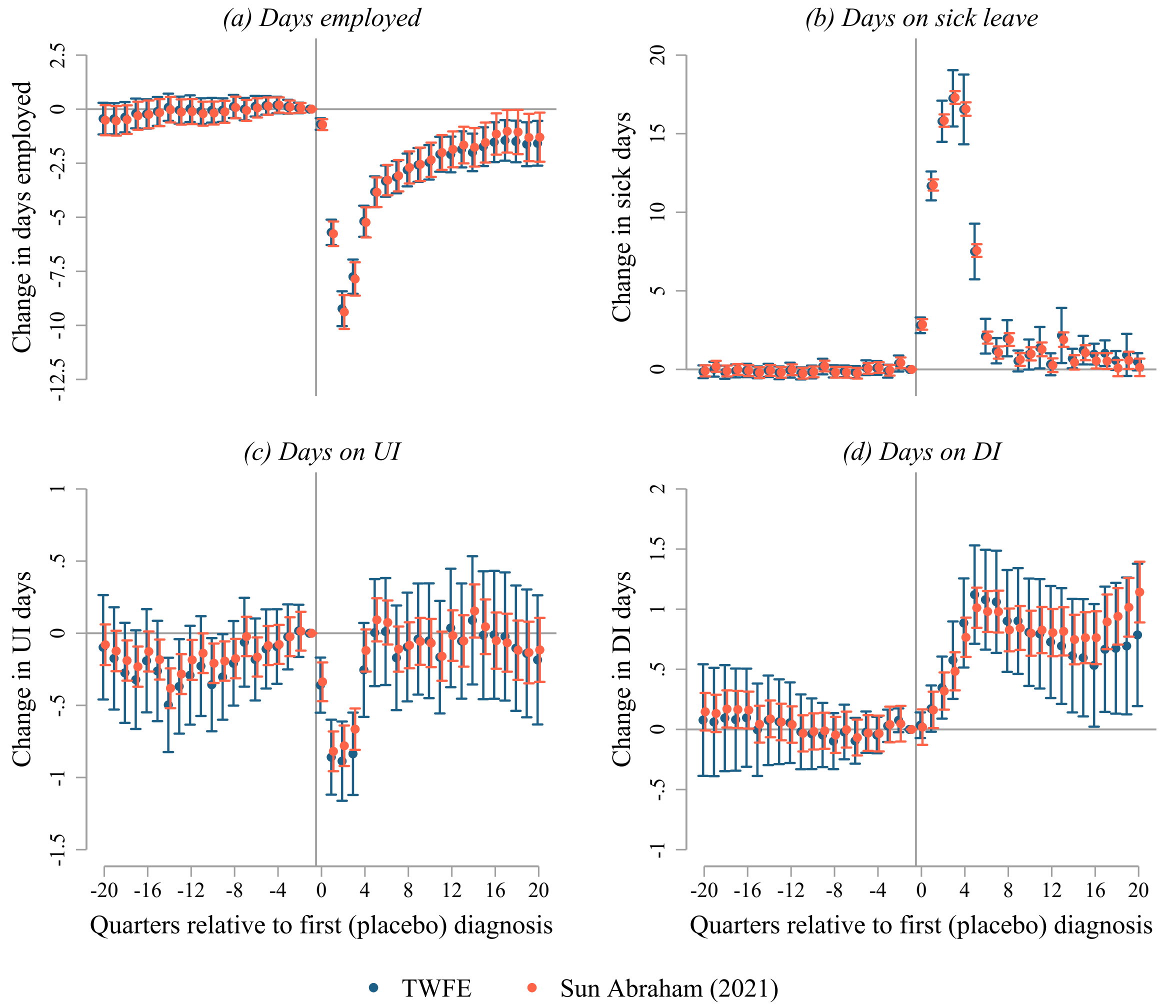
Notes: This graph compares estimates of the effect of a breast cancer diagnosis on total healthcare expenditures using a twoway fixed effects (TWFE) estimator as in equation (1) and the Sun & Abraham (2021) interaction-weighted estimator, which takes into account effect heterogeneity across treatment cohorts. Each scatter depicts the estimated difference in total healthcare expenditures between breast cancer patients and the random sample of other women in any given quarter relative to two quarters before the (placebo) diagnosis, where the blue scatters reflect twoway fixed effects estimates and the orange scatters show interaction-weighted estimates. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors. In each regression, we reweight the sample using the unit weights defined in equation (2).

Figure A.5 — Effect of breast cancer on types of healthcare and drug prescriptions, estimated by twoway fixed effects and the Sun & Abraham (2021) interaction-weighted estimator



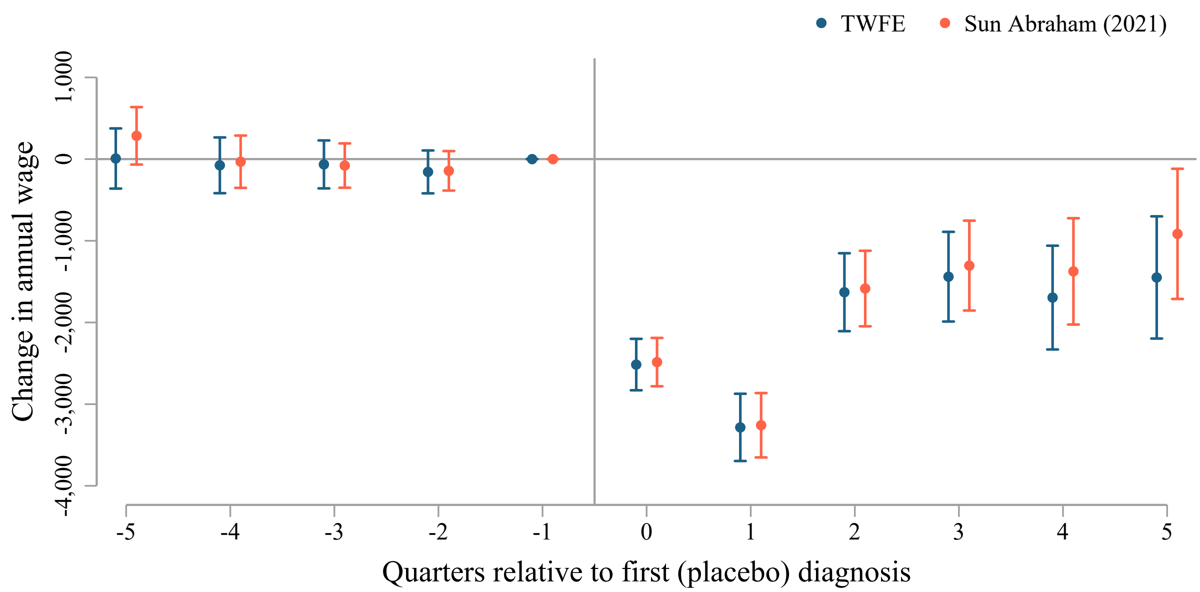
Notes: These graphs compare estimates of the effect of a breast cancer diagnosis on types of healthcare takeup and drug prescriptions using a twoway fixed effects (TWFE) estimator as in equation (1) and the Sun & Abraham (2021) interaction-weighted estimator, which takes into account effect heterogeneity across treatment cohorts. Each scatter depicts the estimated difference in types of healthcare takeup or prescriptions between breast cancer patients and the random sample of other women in any given quarter relative to two quarters before the (placebo) diagnosis, where the blue scatters reflect twoway fixed effects estimates and the orange scatters show interaction-weighted estimates. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors. In each regression, we reweight the sample using the unit weights defined in equation (2).

Figure A.6 — Effect of breast cancer on labor market outcomes, estimated by twoway fixed effects and the Sun & Abraham (2021) interaction-weighted estimator



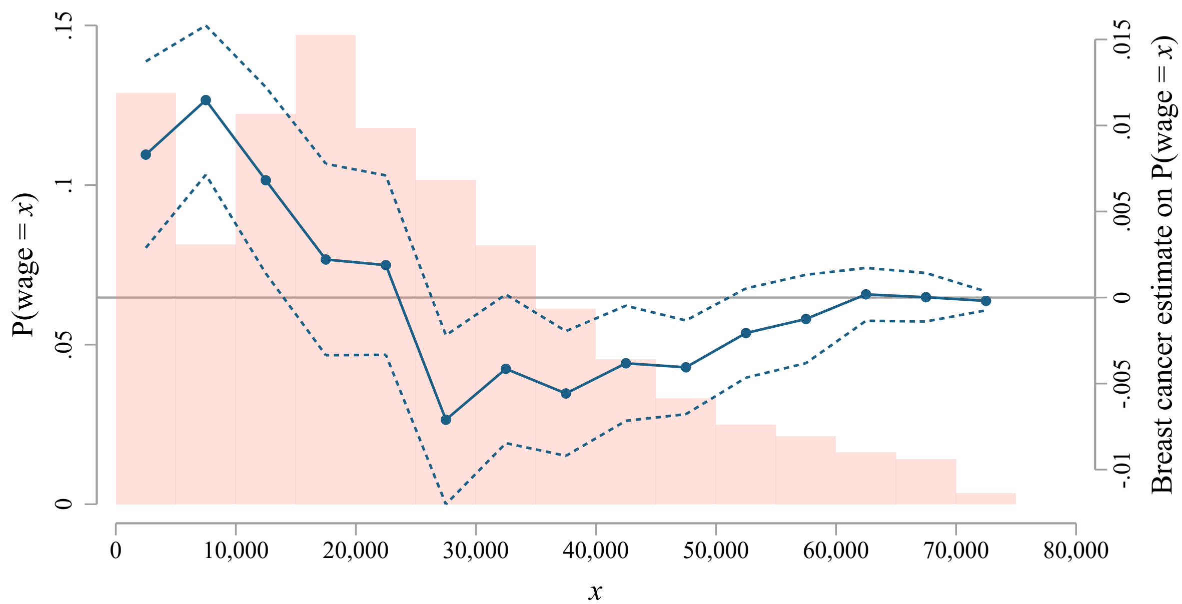
Notes: These graphs compare estimates of the effect of a breast cancer diagnosis on labor market outcomes using a twoway fixed effects (TWFE) estimator as in equation (1) and the Sun & Abraham (2021) interaction-weighted estimator, which takes into account effect heterogeneity across treatment cohorts. Each scatter depicts the estimated difference in labor market outcomes between breast cancer patients and the random sample of other women in any given quarter relative to two quarters before the (placebo) diagnosis, where the blue scatters reflect twoway fixed effects estimates and the orange scatters show interaction-weighted estimates. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors. In each regression, we reweight the sample using the unit weights defined in equation (2).

Figure A.7 — Effect of breast cancer on annual wages, estimated by twoway fixed effects and the Sun & Abraham (2021) interaction-weighted estimator



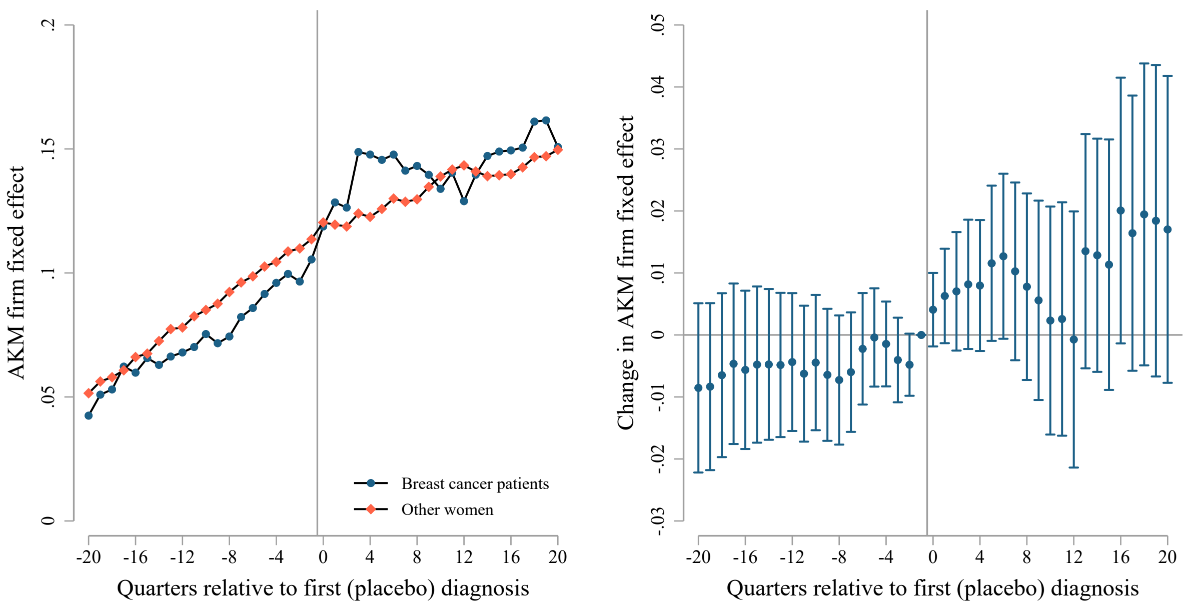
Notes: This figure compares estimates of the effect of a breast cancer diagnosis on annual wages, conditional on being employed, using a twoway fixed effects (TWFE) estimator as in equation (1) and the Sun & Abraham (2021) interaction-weighted estimator, which takes into account effect heterogeneity across treatment cohorts. Each scatter depicts the estimated difference in wages between breast cancer patients and the random sample of other women in any given year relative to the last year before the (placebo) diagnosis, where the blue scatters reflect twoway fixed effects estimates and the orange scatters show interaction-weighted estimates. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors. In each regression, we reweight the sample using the unit weights defined in equation (2).

Figure A.8 — Effects on the wage pdf



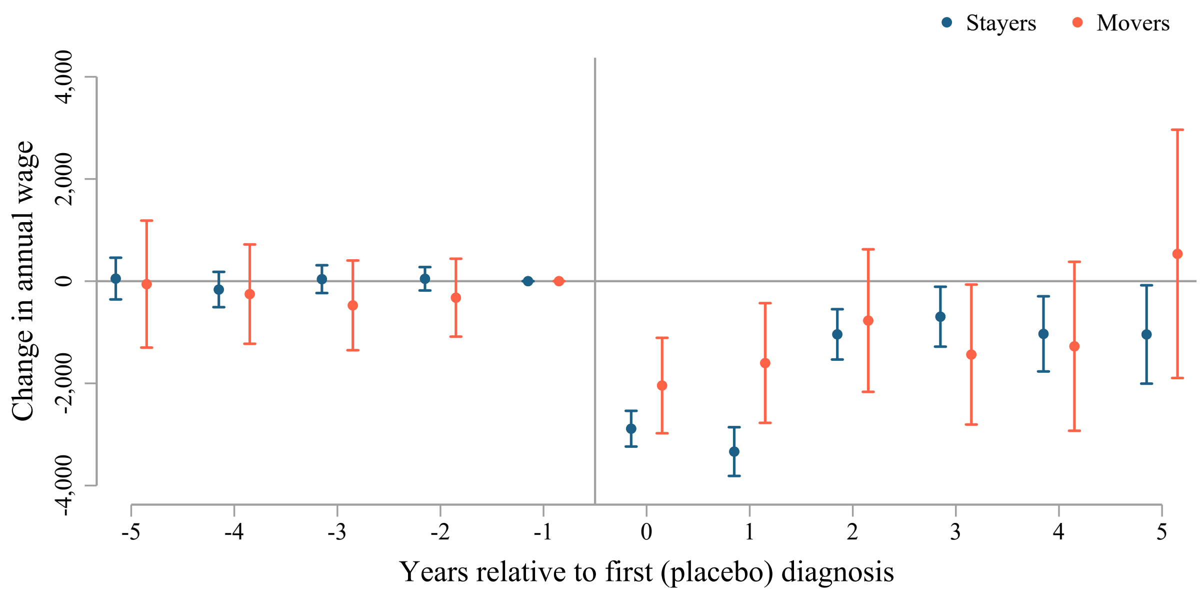
Notes: In this figure, we estimate the effects of a breast cancer diagnoses along different intervals of the annual wage pdf. We run separate difference-in-differences models as in equation (1) for each interval and plot average effect estimates for the probability of earning wages within this interval as blue dots on the right vertical axis. The dashed lines represents a 95 percent confidence band based on individual-level clustered standard errors. The orange bars on the left vertical axis depict the pre-diagnosis wage pdf. In each regression, we reweight the sample using the unit weights defined in equation (2).

FIGURE A.9 — Effects on AKM firm FEs



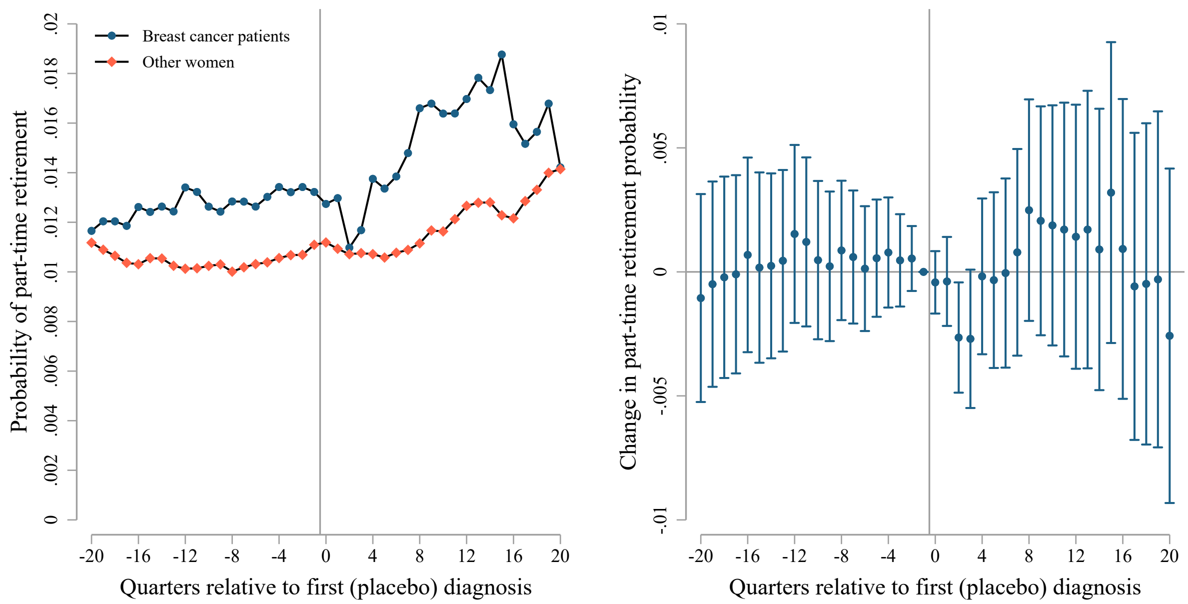
Notes: This figure shows the effect of a breast cancer diagnosis on average estimated AKM firm fixed effects (see Appendix C), where larger values indicate better firm quality. In the left panel we plot raw means over time relative to the first (placebo) breast cancer diagnosis, separately for breast cancer patients and a random sample of same-aged women without a diagnosis. In the right panel we show event study estimates based on equation (1). We reweight the sample using the unit weights defined in equation (2). Each scatter depicts the estimated difference in AKM firm fixed effects between breast cancer patients and the random sample of other women in any given quarter relative to two quarters before the (placebo) diagnosis. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors.

FIGURE A.10 — Wage effects for workers that switch jobs and those that remain with the same employer



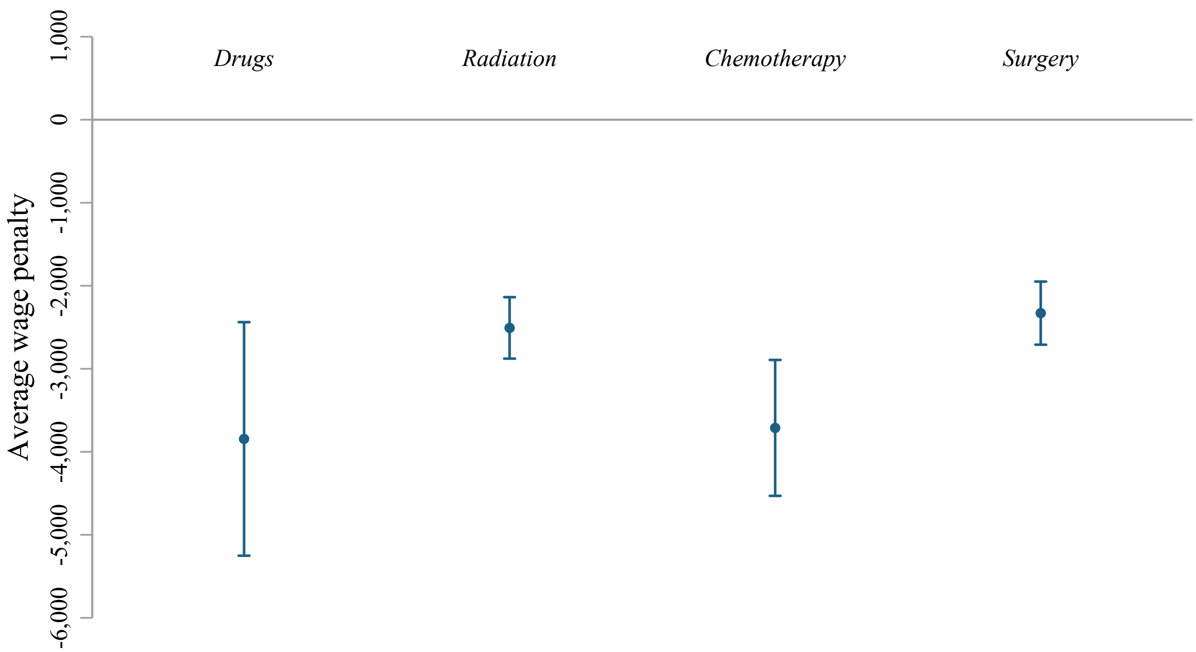
Notes: This figure compares wage penalty estimates from Figure 7 between women who stay in the same firm during the five years after diagnosis and women who switch firms at some point during these five years. We reweight the sample by using the unit weights defined in equation (2). Each scatter depicts the estimated difference in wages between breast cancer patients and the random sample of other women in any given year relative to the last year before the (placebo) diagnosis. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors.

Figure A.11 — Effects on old-age parttime work



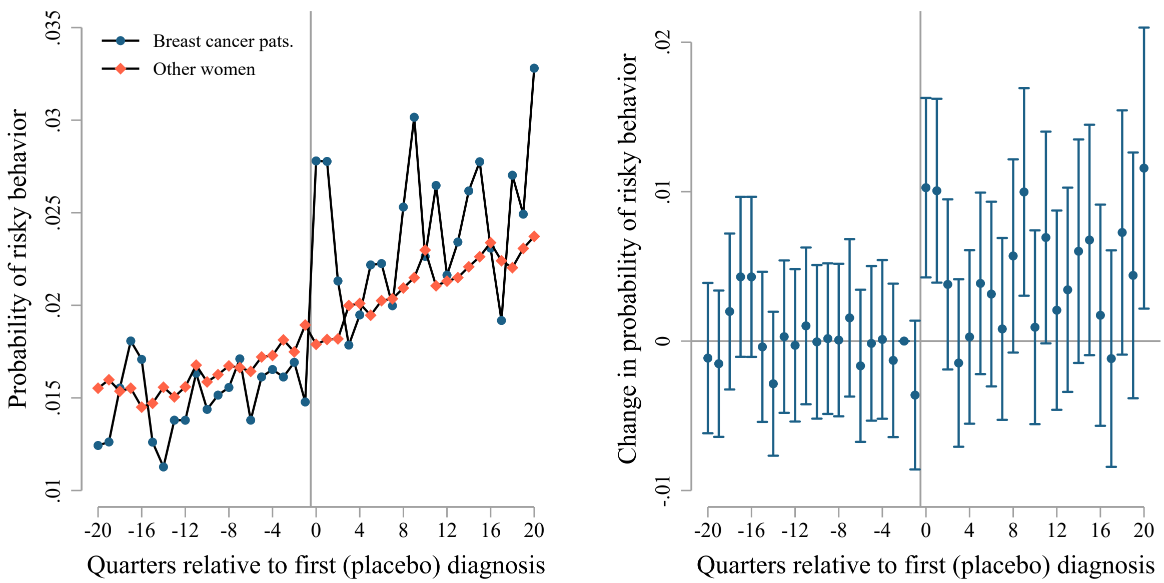
Notes: This figure shows the effect of a breast cancer diagnosis on government-subsidized old-age parttime work. In the left panel we plot raw means over time relative to the first (placebo) breast cancer diagnosis, separately for breast cancer patients and a random sample of same-aged women without a diagnosis. In the right panel we show event study estimates based on equation (1). We reweight the sample using the unit weights defined in equation (2). Each scatter depicts the estimated difference in old-age parttime work between breast cancer patients and the random sample of other women in any given quarter relative to two quarters before the (placebo) diagnosis. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors.

FIGURE A.12 — Wage effect by type of treatment (categories are not mutually exclusive)



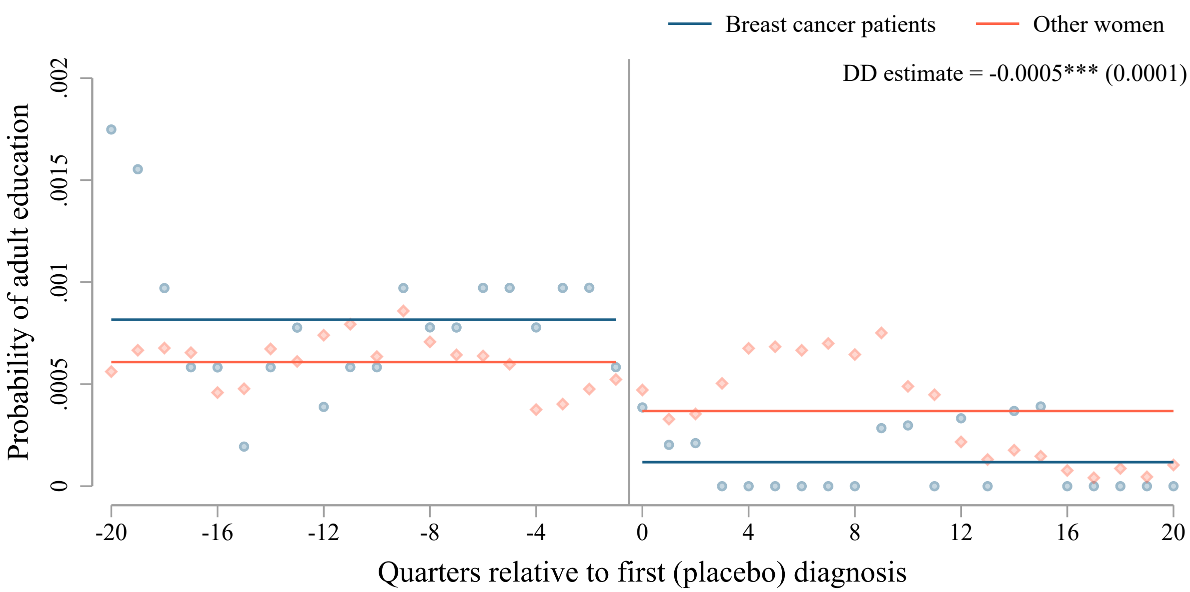
Notes: In this graph, we compare average wage effects across different types of treatment. We obtain the wage effects by estimating equation (1) with a single post-treatment indicator instead of relative time indicators. The categories are not mutually exclusive—for example, a patient can receive both drugs and radiation. Each scatter can be interpreted as the average estimated difference in wages between breast cancer patients between the pre- and post-treatment period, if the woman in the treatment group has received the respective treatment. We use the full control group in each specification (because the control group does not receive any of the treatments), and we reweight the sample using the unit weights defined in equation (2) in each regression. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors.

FIGURE A.13 — Effects on risky behavior



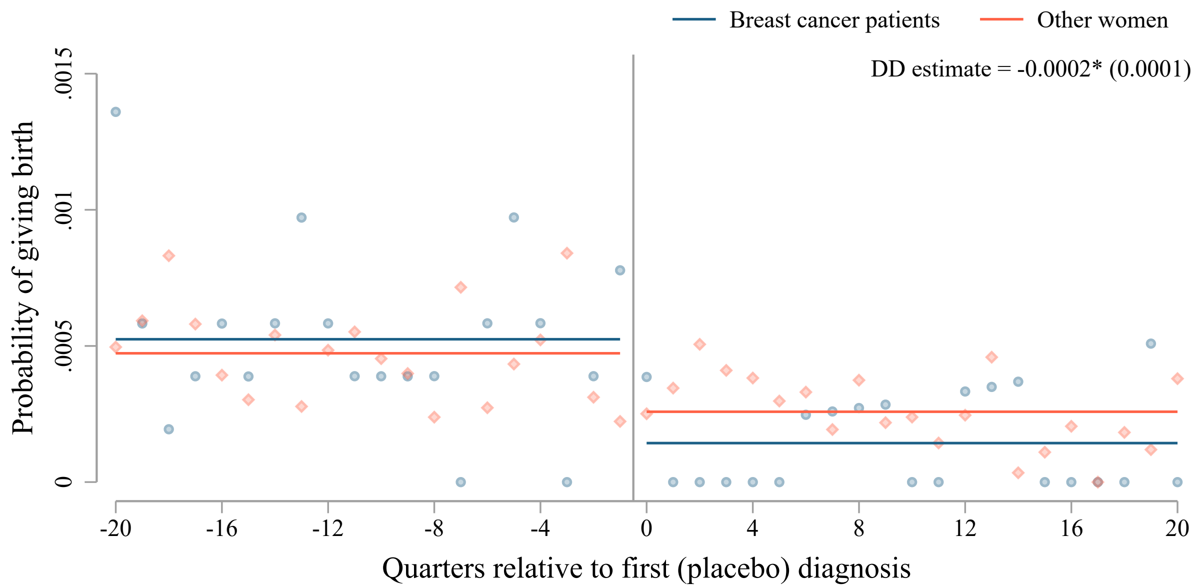
Notes: This figure shows the effect of a breast cancer diagnosis on risky behavior, including binary indicators for diagnoses indicating smoking, alcohol misuse, obesity, sexually transmittable disease, or accidents and injuries. In the left panel we plot raw means over time relative to the first (placebo) breast cancer diagnosis, separately for breast cancer patients and a random sample of same-aged women without a diagnosis. In the right panel we show event study estimates based on equation (1). We reweight the sample using the unit weights defined in equation (2). Each scatter depicts the estimated difference in risky behavior between breast cancer patients and the random sample of other women in any given quarter relative to two quarters before the (placebo) diagnosis. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors.

Figure A.14 — Effects on adult education



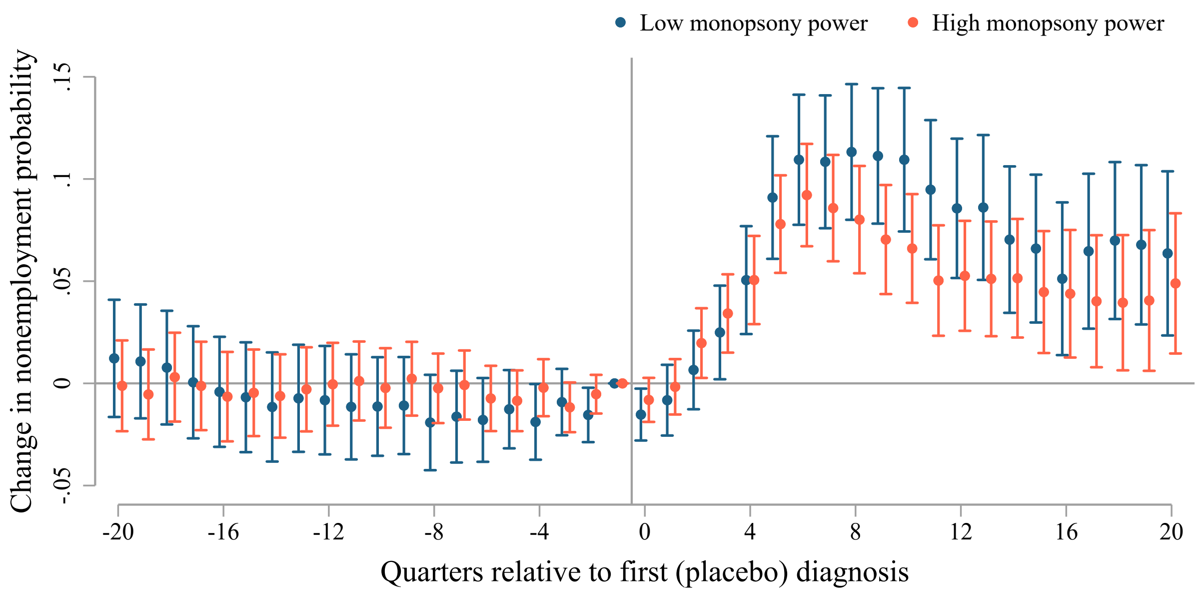
Notes: This figure shows changes in government-subsidized adult education takeup before and after a (placebo) breast cancer diagnosis. The blue lines depict pre- and post-treatment means for women diagnosed with breast cancer, the orange lines show means for other same-aged women. The 'DD' estimate in the top right is a two-by-two difference-in-differences estimate comparing the probability of adult education before and after diagnosis between women in the treatment and in the control group. We reweight the sample using the unit weights defined in equation (2).

Figure A.15 — Effects on fertility



Notes: This figure shows changes in the probability of having a child before and after a (placebo) breast cancer diagnosis. The blue lines depict pre- and post-treatment means for women diagnosed with breast cancer, the orange lines show means for other same-aged women. The 'DD' estimate in the top right is a two-by-two difference-in-differences estimate comparing the probability of having a child before and after diagnosis between women in the treatment and in the control group. We reweight the sample using the unit weights defined in equation (2).

FIGURE A.16 — Effects on the probability of nonemployment by firms' monopsony power



Notes: This figure depicts the effect of a breast cancer diagnosis on the probability of exiting the labor market separately by the pre-diagnosis average of the sectoral Herfindahl-Hirschman index across firms the patient worked at, which we split at the sample median. We show event study estimates based on equation (1). We reweight the sample using the unit weights defined in equation (2). Each scatter depicts the estimated difference in nonemployment probability between breast cancer patients and the random sample of other women in any given quarter relative to the last pre-diagnosis quarter. The vertical bars represent 95 percent confidence intervals based on individual-level clustered standard errors.

Table A.1 — Summary statistics

	Breast cancer patients		Other women	
	Mean	Std. dev.	Mean	Std. dev.
	(1)	(2)	(3)	(4)
(a) Demographic information				
Age (years)	61.64	(12.00)	61.41	(11.86)
Number of children	0.79	(0.99)	0.81	(1.04)
College degree (0/1)	0.15	(0.36)	0.15	(0.36)
(b) Health outcomes				
Total health expenses (€)	645.11	(2,399.55)	628.83	(2,682.63)
i. Expenditures by type of care				
Physician expenses (€)	147.57	(220.74)	137.26	(223.60)
Drug expenses (€)	121.46	(469.65)	111.36	(578.38)
Inpatient expenses (€)	376.08	(2,290.06)	380.22	(2,556.33)
ii. Drug prescriptions				
Antidepressants (0/1)	0.18	(0.38)	0.17	(0.38)
Opioids (0/1)	0.04	(0.19)	0.04	(0.19)
Benzodiazepines (0/1)	0.09	(0.28)	0.09	(0.29)
(c) Labor market attachment				
Days employed	27.39	(41.61)	27.87	(41.83)
Days on UI	1.08	(9.09)	1.13	(9.32)
Days on sick leave	1.24	(10.27)	1.20	(10.08)
Days on DI	4.65	(20.07)	4.74	(20.25)
(d) Wages				
Annual wage (€)	23,149.28	(14,943.45)	22,154.41	(14,179.37)
(e) Labor market histories				
Years in white-collar jobs	11.94	(12.73)	11.01	(12.61)
Years in blue-collar jobs	6.46	(9.57)	7.08	(9.90)
Years in marginal jobs	0.91	(2.68)	1.02	(2.88)
Years on UI	1.14	(1.97)	1.16	(2.04)
Lifetime income (EUR)	327,465.86	(307,093.03)	312,392.52	(293,516.06)

Notes: This table shows summary statistics for women with breast cancer and randomly matched same-aged women. All variables measured during the five years before the (placebo) diagnosis. We reweight the sample using the unit weights defined in equation (2).

Table A.2 — Robustness to different covariate specifications and weighting

	No covariates (1)	+ i FEs (2)	+ Age FEs (3)	+ Year FEs (4)	Unweighted (5)
(a) Health outcomes					
Total health expenses (€/1,000)	2.27***	2.45***	2.44***	2.44***	2.46***
	(0.025)	(0.026)	(0.026)	(0.026)	(0.026)
i. Expenditures by type of care					
Physician expenses (€/1,000)	0.03***	0.03***	0.03***	0.03***	0.03***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Drug expenses (€/1,000)	0.30***	0.32***	0.32***	0.32***	0.32***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Inpatient expenses (€/1,000)	1.94***	2.10***	2.08***	2.08***	2.10***
	(0.024)	(0.025)	(0.025)	(0.025)	(0.025)
ii. Drug prescriptions					
Antidepressants (0/1)	0.04***	0.04***	0.04***	0.04***	0.04***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Opioids (0/1)	0.02***	0.03***	0.03***	0.03***	0.03***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Benzodiazepines (0/1)	0.02***	0.02***	0.02***	0.02***	0.02***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
(b) Labor market outcomes					
Days employed	−3.31***	−3.71***	−3.46***	−3.49***	−3.52***
	(0.424)	(0.372)	(0.310)	(0.311)	(0.320)
Days on sick leave	5.37***	5.72***	5.67***	5.67***	5.76***
	(0.207)	(0.222)	(0.204)	(0.203)	(0.216)
Days on UI	0.08	−0.08	−0.07	−0.06	−0.08
	(0.100)	(0.101)	(0.100)	(0.100)	(0.102)
Days on DI	0.89***	0.85***	0.73***	0.65***	0.59***
	(0.239)	(0.223)	(0.220)	(0.223)	(0.215)
(c) Wages					
Annual wages (€/1,000)	−2.10***	−2.28***	−2.16***	−2.22***	−2.24***
	(0.313)	(0.194)	(0.179)	(0.180)	(0.178)
Individual fixed effects	No	Yes	Yes	Yes	Yes
Age fixed effects	No	No	Yes	Yes	Yes
Year fixed effects	No	No	No	Yes	Yes

Notes: In this table, we estimate equation (1) with a single post-treatment indicator instead of period-specific relative time indicators and report the average treatment effect estimate for each outcome when we sequentially add control variables. In column (1), we only regress the outcome on a full interaction of D_i and the post-treatment dummy. In column (2), we add individual-level fixed effects. In column (3), we add year-of-age fixed effects. In column (4), we add year fixed effects. This is our baseline specification. In column (5), we additionally show estimates when we do not apply individual weights as defined in equation (2). We rescale variables measured in € for presentational purposes. Individual-level clustered standard errors in parentheses, stars indicate statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B. Matching procedure

We draw the control group of same-aged non-affected women as follows:

1. Estimate the age at diagnosis pdf for women in the treatment group (Figure B.1).
2. Draw random ages at diagnosis from the empirical pdf with replacement and assign those to women in the potential control group (i.e., all Upper Austrian women never diagnosed with breast cancer born between 1930 and 1990). In particular, we conduct 2,759 draws from the empirical pdf, each draw of size 200, which gives us a total of 551,800 draws which we assign randomly to the 551,610 women in our data not diagnosed with breast cancer. The resulting age distribution in the potential control group is depicted in Figure B.2.
3. Calculate the placebo treatment quarter as the birth quarter plus the placebo age at diagnosis.
4. Keep if placebo treatment quarter is between 2010q1 and 2018q4 (Figure B.3).

In the last step, we reduce the potential control group to 42,863 women who are placebo treated between 2010q1 and 2018q4. The distribution of birth cohorts in this group is similar to the treatment group (Figure B.4), and treatment quarters are more or less uniformly distributed (Figure B.5). Since there are many more women in the control group than in the treatment group, we apply unit weights as in equation (2).

Figure B.1 — Age at first diagnosis

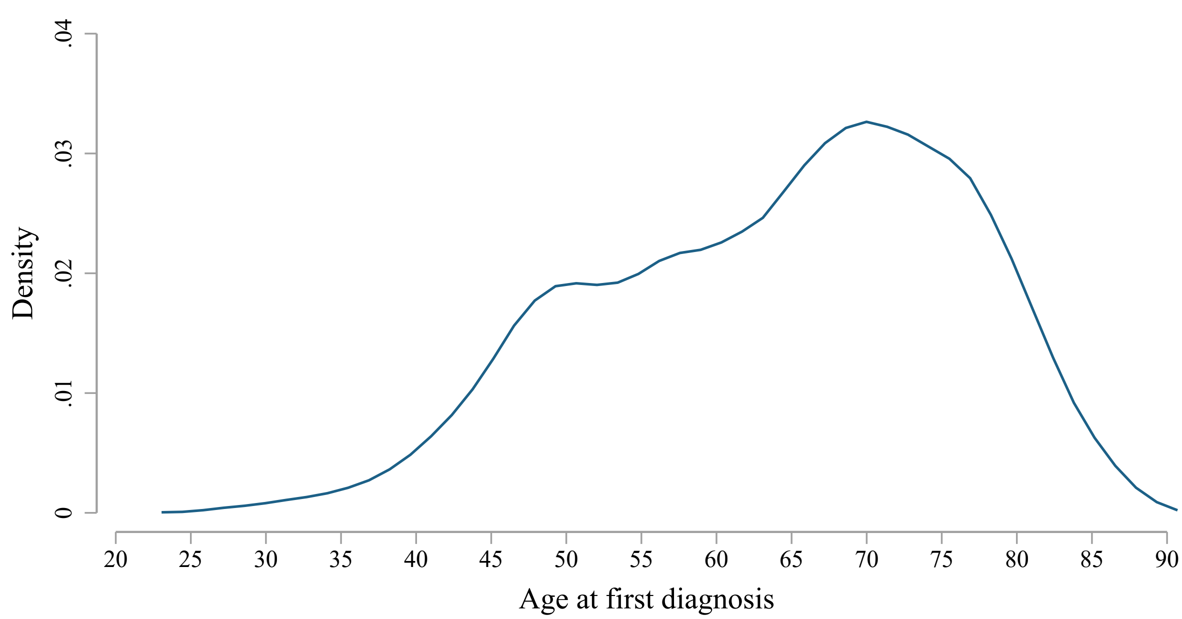


Figure B.2 — Age at first diagnosis for placebo treatments

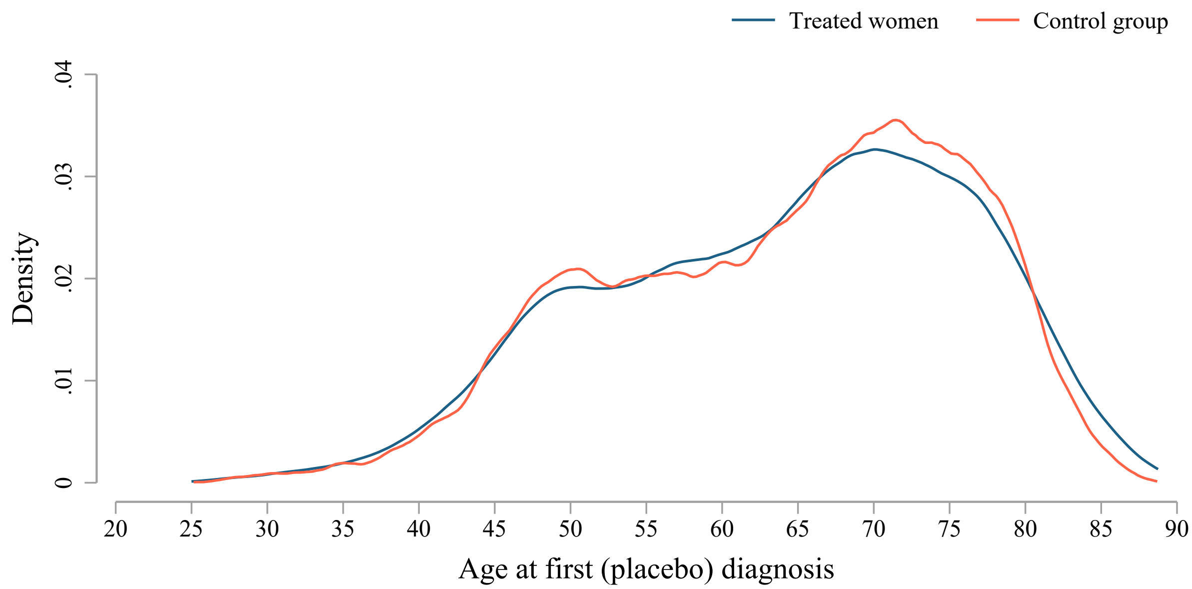


Figure B.3 — Placebo treatment quarters in the full population

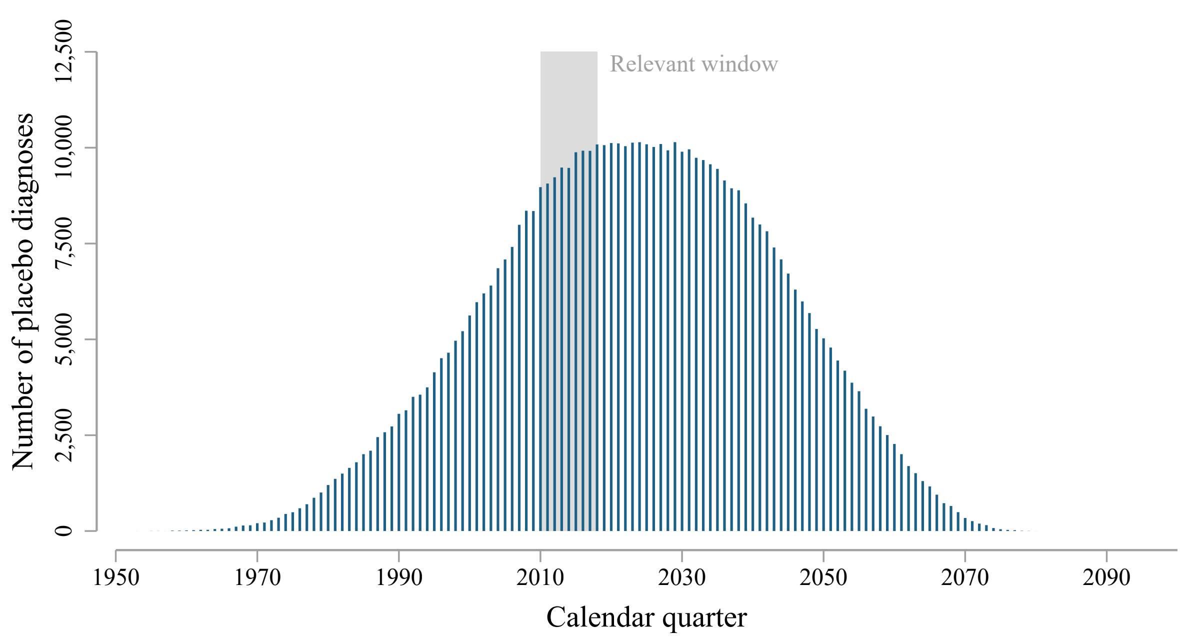


Figure B.4 — (Placebo) diagnoses by cohort

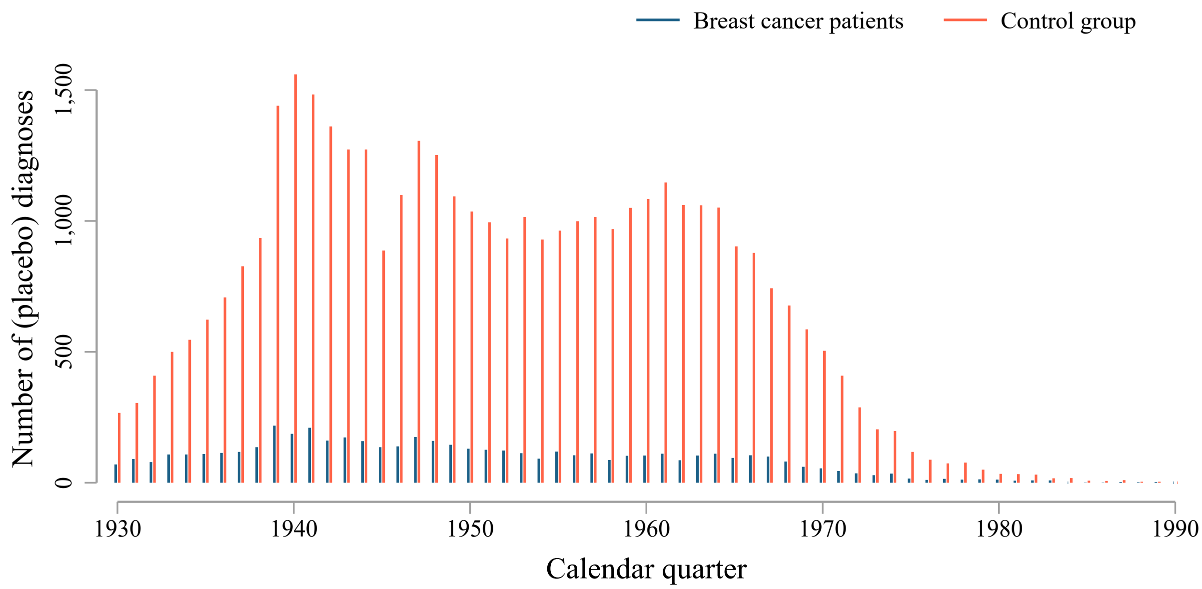
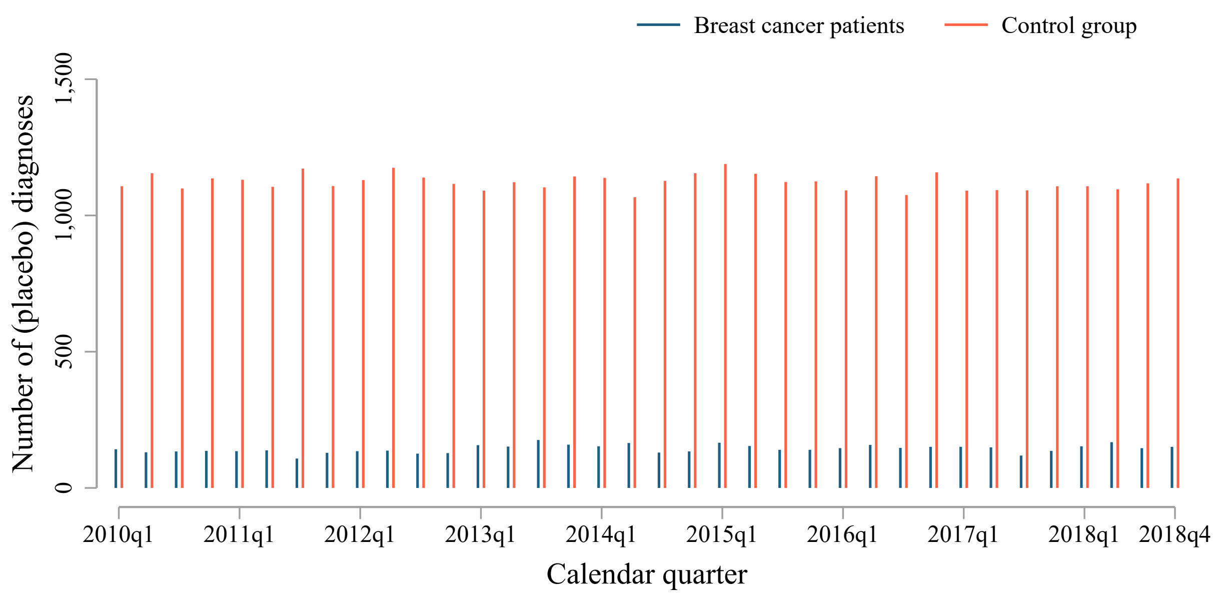


Figure B.5 — (Placebo) diagnoses by calendar quarter



C. Estimating job quality

Because we do not have information on occupations, we approximate job quality by estimating (Abowd et al. 1999, AKM) firm fixed effects. This strategy exploits worker moves between firms to isolate firm components of wages, which can be interpreted as wage premia that firms always pay regardless of worker characteristics. In the literature, estimated AKM fixed effects are often used as a measure of firm quality.

To estimate the AKM model, we build a full yearly panel of Austrian workers observed between 1972 and 2022. We then run the following twoway fixed effects regression:

$$\log w_{it} = \beta X_{it} + \alpha_i + \psi_{J(it)} + \varepsilon_{it} \tag{C.1}$$

where wages w_{it} for worker i in year t are assumed to be an additive function of time-varying workers characteristics X_{it}, a worker fixed effect α_i, a fixed effect for firm J where worker i was employed in t, $\psi_{J(it)}$, and a residual ε_{it}. We estimate equation (C.1) using a conjugate gradient algorithm and retrieve predictions $\hat{\psi}_J$ for all firms J in the data, which we match to our main sample. Larger values for $\hat{\psi}_J$ indicate higher firm quality and vice versa.

D. Predicting cancer severity

In lieu of information on tumor size and metastases, we predict cancer severity based on observables in the data, especially the types of treatment that are prescribed, which strongly correlate with cancer severity. In particular, we use data on all women with a breast cancer diagnosis between 2005 and 2018 and run a probit of 3.5-year death probability $dead_i$ on a set of predictors X_i,[20]

$$P(dead_i = 1) = \Phi(\beta X_i), \tag{D.1}$$

where X_i consists of a cubic in age fully interacted with a squared term in pre-diagnosis aggregate health expenditures, the number of children,[21] education, calendar-quarter dummies, and non-mutually-exclusive dummies indicating different cancer therapies that were administered immediately after diagnosis (drug therapy, chemotherapy, radiation, and/or breast surgery).

We then obtain predicted death probabilities for each woman i given her characteristics X_i, $\hat{P}(dead_i = 1 \mid X_i)$, which we match to our main sample. Higher values of $\hat{P}(dead_i = 1 \mid X_i)$ imply that a woman's characteristics make her more likely to die within 3.5 years after diagnosis, which we interpret as a sign that the cancer was more severe.

[20] We have mortality data until July 2022, which allows us to observe death for 3.5 years following diagnosis.

[21] Women who had children have a lower cumulative incidence of breast cancer, especially if their age at first birth was 35 years or younger (Rosner et al. 1994).