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ABSTRACT

We investigate the effects of radiation therapy on the mortality and economic outcomes of breast cancer patients. We implement a 2SLS strategy within a difference-in-difference framework exploiting variation in treatment stemming from a medical guideline change in Denmark. Using administrative data, we reproduce results from an RCT showing the lifesaving benefits of radiotherapy. We then show therapy also has economic returns: ten years after diagnosis, treatment increases employment by 37% and earnings by 45%. Mortality and economic results are driven by results for more educated women, indicating that equalizing access to treatment may not be sufficient to reduce health inequalities.

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1 Introduction

Following the seminal work of Grossman (1972) on the theory of health capital, an extensive body of research in economics suggests that healthier individuals have better socio-economic outcomes (Stephens Jr. and Toohey, 2022; Bleakley, 2007; Fogel, 2004; Currie and Madrian, 1999). Existing evidence also suggests that health affects economic outcomes at the national level (Acemoglu and Johnson, 2007; Weil, 2007). A natural question then is whether — and by how much — medical interventions that affect health also affect economic outcomes.

Understanding the effects of medical treatments on economic outcomes may have fundamental implications for health policy. However, rigorous evidence addressing this question is scarce, for at least two reasons. The first is the endogenous assignment of medical treatments. Patients in worse health tend to receive more intensive medical treatments. At the same time, most determinants of health likely affect economic outcomes, making empirical identification challenging. Second, addressing this question requires detailed linked data on individual health, medical treatments, and economic outcomes. The ability to observe these outcomes for an extended period of time is essential to capture any long-run adjustments.

In this paper, we overcome these challenges by investigating the effects of radiation therapy on the mortality and labor market outcomes of breast cancer patients in Denmark. Breast cancer has several features that make it well suited to study the effects of medical treatments. It is the most commonly diagnosed cancer among women, with more than 2.2 million new cases in 2020. Accounting for more than 12% of all newly diagnosed cancer cases annually, it is also the most common form of cancer worldwide (OECD, 2023). In addition, roughly a third of breast cancer patients are diagnosed between the ages of 25–54, thus still during their working years.[1] Finally, survival rates are high with more than 90% of patients in high-income countries remaining alive 5 years after diagnosis (Arnold et al., 2022).

Denmark constitutes an ideal setting to study the economic effects of radiation therapy for several reasons. To begin with, it has rich clinical and administrative data

[1] Authors' own calculation using data from the Global Cancer Observatory of the World Health Organization, available at https://gco.iarc.fr, last accessed on 7 February 2024.

that allow us to observe the health and labor market outcomes of the near-universe of breast cancer patients for up to ten years after diagnosis. Second, a change in medical guideline expanded the eligibility for radiotherapy in January 1995 without affecting the allocation to any other types of breast cancer treatments. This guideline change provides us with plausibly exogenous variation in assignment to treatment, allowing us to address identification challenges.

Using data on women diagnosed with breast cancer between 1990–1998, we show that the guideline change increased the probability of radiation therapy among targeted women by 75.3 percentage points relative to unaffected patients with similar disease characteristics. We then estimate the effects of radiotherapy on patient outcomes through an instrumental variables strategy. The instrument is defined as the interaction between an indicator for belonging to the group of patients with characteristics targeted by the guideline change and a dummy variable for being diagnosed after January 1995. In our setup, almost all patients receive chemotherapy. Thus, our results can be interpreted as the effect of combined radiation and chemotherapy as compared to receiving only chemotherapy.

Given that numerous randomized controlled trials consistently show that breast cancer treatments are effective in reducing mortality (e.g., Early Breast Cancer Trialists' Collaborative Group, 2011, 2005; Overgaard et al., 1997; Ragaz et al., 1997), we first document the effects of radiotherapy on survival. Consistent with prior medical studies, we find that radiation therapy leads to substantial mortality reductions: women who receive combined radiotherapy and chemotherapy are about 10 percentage points less likely to die 5–10 years after diagnosis relative to women who are treated with chemotherapy alone. The mortality gains we estimate using 2SLS are identical to those found in a randomized controlled trial that examined the impact of adding radiotherapy to chemotherapy among women diagnosed with breast cancer ten years earlier (Overgaard et al., 1997). This suggests that the returns to radiotherapy did not diminish during our study period.

We next turn to the effects on our labor market outcomes: employment, income, and welfare use. We address a potential bias from selective survival by coding non-survivors as out of the labor force with no income and no welfare use. We find that radiation therapy has major economic benefits. Our results suggest that women who receive radiotherapy in addition to chemotherapy are 15.5 percentage points (37%) more likely to be employed ten years after diagnosis. The employment gains are mainly due to a reduction in the likelihood of exiting the labor force. We also find that treatment improves labor earnings by 13–45% and total income by 7–27% in the ten years following cancer diagnosis. The different effects on earnings and total income are due to changes in welfare use. Specifically, we find that radiotherapy mitigates the cumulative risk of being on welfare by 33–41%.

What mechanisms drive these effects? While data limitations prevent us from pinpointing the precise mechanisms behind these effects, we try to shed light on potential pathways by examining treatment heterogeneity along two dimensions: disease severity and patient socio-economic status (proxied by education). Using the predicted 10-year breast cancer mortality as a measure of disease severity (Abadie et al., 2018), we show that the returns to radiation therapy are larger for patients with a higher disease burden, both for long-term mortality and for labor supply outcomes.

We next document substantial treatment heterogeneity based on patient's education. In particular, we find that radiotherapy has similar mortality effects among highly and low-educated patients, but that the long-term mortality gains are observed only among women with postsecondary education. The differences in mortality gains are entirely due to differences in the risk of breast cancer recurrence in the long-run. We show that these results are not driven by differences in provider quality, in access to screening programs, or disease severity at the time of diagnosis. If anything, our results suggest that low-educated patients have higher predicted mortality risk and, as such, would be expected to have larger gains from treatment. Based on prior medical studies highlighting the importance of lifestyle factors in recurrence (Cannioto et al., 2023) and associations between education and such lifestyle factors (Puka et al., 2022), we speculate that differences in diet, exercise, smoking and alcohol use may play a role.

Our results also suggest significant heterogeneity in economic gains by patient education: ten years after diagnosis highly educated patients treated with radiotherapy are 19.3 percentage points more likely to be employed, while the effects on low-educated women are much smaller and not statistically significant. The positive effects on employment are larger than the reduction in mortality, suggesting that economic returns are not solely due to highly educated individuals surviving after breast cancer. We contend that one explanation for the divergent labor market effects could be that low-educated patients have physically more demanding occupations and the well-established side effects of radiation treatment (e.g., swelling, fatigue, lymphedema, pain or weakness in the arm and shoulder) makes it difficult for them to remain in the labor force. We provide suggestive evidence on this by documenting that a higher share of low educated women are employed in occupations characterized as physically moderately demanding and, among patients employed in physically light jobs, low-educated women have tasks that are physically more demanding.

Our paper makes two contributions. First, we add to previous studies documenting a pronounced decline in labor supply among survivors of breast cancer compared to those without cancer (e.g., Heinesen and Kolodziejczyk, 2013; Bradley et al., 2005, 2002a,b). Our work compliments this as it focuses on whether specific treatment patterns can lessen the impact of the disease on economics outcomes. Second, we contribute to a growing body of work in economics that considers the impact on labor supply of medical treatments.[2] This work has considered antiretroviral therapy for HIV/AIDS patients (Papageorge, 2016; Baranov et al., 2015; Thirumurthy and Graff Zivin, 2012; Habyarimana et al., 2010; Thirumurthy et al., 2008), Cox-2 inhibitors (Butikofer and Skira, 2018; Garthwaite, 2012), mental health treatments (Biasi et al., 2023; Cronin et al., 2020; Timbie et al., 2006), and prescription opioids (Beheshti, 2023; Harris et al., 2020). The impact of breast cancer treatments on labor market outcomes is largely unexplored. One exception is the study by Jeon and Pohl (2019) that uses data from Canada to examine the impact of medical innovation on the labor market outcomes of prostrate and breast cancer patients. The paper documents that medical innovation – measured by the number of approved drugs and a patent index – reduced the negative effects of cancer on employment and that the economic gains were experienced only by cancer patients with postsecondary education. However, the paper is unable to disentangle the effects of medical innovation from the improvements in diagnostics as they lack clinical information on disease charac-

[2] A strand of medical literature examines how cancer treatment patterns, especially for breast cancer, can alter the return to work (e.g., Carlsen et al., 2014; Lindbohm et al., 2014; Damkjæ et al., 2011; Johnsson et al., 2009; Balak et al., 2008; Drolet et al., 2005). These studies rely on multivariate regression models that do not account for selection into treatment.

teristics. It also estimates only intention-to-treat effects as the authors lack data on the treatments received by patients. In our paper, we estimate the causal effect of a specific and common cancer treatment against a clearly-defined counterfactual. Our ability to examine long-run effects also distinguish our paper from previous studies.

Our results speak to a growing emphasis in oncology care to include quality of life measures as secondary outcomes in cancer treatment clinical trials (Wilson et al., 2015). Despite this, only 45% of National Cancer Institute-sponsored cancer treatment trials with an initial publication about health outcomes subsequently report quality of life outcomes (St Germain et al., 2020). Therefore, obtaining a better understanding the impact of therapies on non-medical outcomes is critically important. Our findings are also pertinent to the ongoing discussions on the role of medical treatments in the increase in overall health spending. Costs of cancer treatment are rising worldwide. For example, the United States spent an estimated USD 161.2 billion in 2017 on cancer related healthcare expenditures. In the European Union, healthcare spending for cancer care was EUR 57.3 billion (Jemal et al., 2019). With roughly USD 30 billion in medical costs in 2020, breast cancer has the highest treatment cost among all cancer types (Mariotto et al., 2020). These medical expenditures are expected to increase dramatically in the coming years due to population aging. Our results suggest that breast cancer treatments not only impact survival but that they have long-term economic benefits, even in a country like Denmark, with its universal health care access, and strong social safety net. As such, they underline the need to consider the potential economic benefits when making decisions on the reimbursement of new cancer treatments. Furthermore, the heterogeneity in the returns to treatment by patient education underscores that equalizing access to treatment may not be sufficient to reduce inequalities in health. A deeper understanding of how these treatments interact with lifestyle factors and occupational characteristics is necessary to address emerging inequalities.

2 Institutional Background

This section describes the diagnosis and treatment of breast cancer in Denmark. As we detail below, Denmark has a universal health insurance system that covers almost all health care costs. In addition, there are well-established guidelines on breast cancer care. Therefore, out of pocket expenditures on medical care or uncertainty on the appropriate procedures are unlikely to impact access to treatment. Given our focus on labor market outcomes, we also discuss how the Danish Social Security system insures individuals against income losses from severe health shocks.

2.1 Diagnosis and Treatment of Breast Cancer

The majority of Danish health care services, including all stages in the diagnosis and treatment of breast cancer, are free of charge and all residents have equal access. The patient's general practitioner acts as a gatekeeper for specialist treatment. The general practitioner reviews the patient's medical history and conducts a clinical breast exam. If this raises concerns about a potential breast cancer, the patient is referred to a specialist, where she receives a mammography often supplemented with ultrasonography and needle biopsy.[3]

Patients who are diagnosed with breast cancer receive medical treatments according to the guidelines set by the Danish Breast Cancer Cooperative Group (DBCG).[4] According to these guidelines, all women diagnosed with early-stage breast cancer (95% of all breast cancer patients; see Møller et al., 2008) are offered primary surgery within two weeks after diagnosis, which consists of either removal of the breast (mastectomy), or breast-conserving surgery where only the tumor is removed (lumpectomy). In both cases, any positive sentinel lymph nodes into which the tumor drains are also removed. After primary surgery, some patients are further offered adjuvant treatment consisting of systemic therapy and/or radiation therapy, depending on their demographic and disease characteristics. Systemic therapies are drugs that spread throughout the body to treat cancer cells. They include chemotherapy, hormonal therapy (endocrine), and immunotherapy (anti-HER2). Radiation therapy is de-

[3]In Denmark, the national breast cancer screening program was rolled out between 2007 and 2010. There were only a few regional screening programs before the introduction of the national plan: in the municipality of Copenhagen (starting from April 1991), in the county of Funen (starting from November 1993), and in the municipality of Frederiksberg (starting from June 1994). All programs offered bi-annual screening to women aged 50 to 69. For more details, see Lynge et al. (2017). In addition, opportunistic screening is rare (Jensen et al., 2005).

[4]DBCG is a multidisciplinary organization founded in 1976 by the Danish Surgical Society in order to standardize breast cancer care across all Danish hospitals (Blichert-Toft et al., 2008).

signed to provide highly-targeted treatment to kill any cancer cells that may remain in the breast after surgery. As with other treatments, radiotherapy has some adverse effects. Significant short-term side-effects include pain (Andersen and Kehlet, 2011), fatigue (Minton and Stone, 2008), loss of cognitive function (Debess et al., 2010) and pulmonary and upper limb morbidity (Gomide et al., 2007). Long-term late effects of radiotherapy include an increased risk of ischemic heart disease if the radiation is applied on the left side of the chest (Darby et al., 2013).

In Denmark, there are ongoing national clinical trials on breast cancer treatments at all times. All eligible patients are offered to participate in the trial running at the time of diagnosis.[5] While patients can refuse to participate in trials, in practice this is very rare. Ineligible patients and those who decline to participate receive the standard course of treatment available at the time of diagnosis. Participants in the trial receive treatment according to the guidelines set in the specific trial. The treatment guidelines for systemic therapies and for radiation therapy are determined independently.

Our paper focuses on the period January 1990–December 1998 when the DBCG89 national clinical trial was in place. The trial compared the impact of different chemotherapy treatments for pre-menopausal women and of different hormone therapy treatments for post-menopausal women.[6] DBCG changed the guidelines for use of radiation therapy in the middle of this trial when the results of an earlier clinical trial indicated long-term mortality gains from radiation therapy (Møller et al., 2008; Overgaard et al., 1997). Treatment guidelines for systemic therapies were not affected.

Eligibility for radiation therapy during this period is detailed in the decision tree represented in Appendix Figure A1. In the decision tree, diagnoses or demographic characteristics are listed in regular font and the text in italics represents the medical decision concerning radiation therapy. As the Figure shows, patients who had lumpectomy as primary surgery were eligible to receive radiotherapy regardless of any other demographic or disease characteristics. Among women receiving mastec-

[5]Patients with distant metastases, bilateral carcinomas, those with previous malignancies, and those whose cancer is inoperable are always excluded from clinical trials. Each trial can add additional criteria for exclusion (e.g., age limits).

[6]In addition to the usual exclusion criteria, the clinical trial excluded all patients aged 75 and above. Around 2.4% of all patients diagnosed with breast cancer between 1990-1998 and who were eligible for the DBCG89 trial refused to participate.

tomies, post-menopausal women are never offered radiation. The guidelines for pre-menopausal patients receiving a mastectomy changed in January 1995. Before January 1995, only pre-menopausal women 45 years of age and younger with at least 4 positive lymph nodes were eligible to receive radiotherapy. After January 1995, eligibility was expanded to all high-risk pre-menopausal women with at least 1 detected positive lymph node or with a tumor of at least 50mm. Our empirical strategy exploits this guideline change as described in Section 4 below.

2.2 Income Insurance Against Health Shocks

Working age Danish residents who experience severe health shocks are insured against earnings losses mainly through sickness benefits and disability pension. Sickness benefits compensate for the earnings losses of persons in the labor force. During our study period individuals could receive compensation for up to a year within 36 calendar months. Benefit levels corresponded to 90% of the earnings before the onset of the health shock up to a maximum benefit level per month. During 1984-2000, benefits represented on average 65% of lost earnings (Pedersen and Larsen, 2008).

Disability pension provides financial support to those whose ability to work is permanently and substantially reduced. Eligibility is decided by municipal caseworkers taking into account both medical needs and social considerations (Bingley et al., 2012). The disability pension is granted permanently and recipients transition into the old-age pension program when they reach the retirement age. During the period of our analysis there were three different benefit levels depending on the severity of disability. Benefit levels also differed among married and single individuals.[7]

Individuals who are still unable to work after the expiration of sickness benefits but do not qualify for disability pension may receive financial support through un-

[7] The base level was paid out to individuals whose work capacity was reduced by more than 50% and amounted in 1995 to 6,280 DKK (1,373 USD in 2015 prices) per month for married/cohabiting individuals and 6,531 DKK (1,428 USD) for single individuals. The intermediate group included individuals younger than 60 whose work capacity was reduced to a third as well as individuals aged 60 to 66 years who had no capacity for work. In 1995, married/cohabiting individuals in this group received a monthly pension of 7,143 DKK (1,562 USD) while single individuals received 7,394 DKK (1,616 USD). Finally, individuals younger than 60 with no work capacity were classified as the high level and received 9,634 DKK (2,106 USD) monthly if they were married/cohabiting or 9,885 DKK (2,161 USD) if they were single.

employment insurance benefits, social assistance benefits or early retirement pension. Appendix A1 describes these additional sources of income insurance.

3 Data Sources and Analysis Sample

We use several population-level administrative data sets from Denmark. These data include individual-level records with unique personal identifiers, allowing us to follow the entire population over time. We use information for the period 1990 to 2008.

Treatment Variable. Our primary data source is the clinical *Breast Cancer Database* collected by the DBCG.These data provide detailed information on patients with invasive breast cancer, including histopathological information (e.g., tumor size, malignancy grade, number of nodes examined, number of tumor positive nodes, estrogen and/or progesterone status), menopausal status, the medical treatments administered (e.g., type of primary surgery, receipt of radiation therapy and of systemic therapy), as well as the date of diagnosis and of major medical interventions (Møller et al., 2008). Using these data, we define an indicator for receipt of radiotherapy.

Outcome Variables. Our main health outcome is mortality, obtained from the *Register of Causes of Death*. The register includes death records for all residents who die in Denmark, with information on the exact date and cause of death using the World Health Organization's International Classification of Disease. We measure mortality with indicators for all-cause and breast cancer mortality. We examine effects for each year from the date of diagnosis, up to 10 years after diagnosis.

Our primary labor market outcomes are measures of labor force participation and income. Information on labor force participation is derived from the *Register-Based Labour Force Statistics*, a dataset based on tax records with records on the labor market status of the entire Danish population as of November. We construct indicators for being employed, unemployed, and out of the labor force. We use the *Income Statistics Register* to construct two measures of income: annual labor earnings (equal to zero for people who are not employed), and gross personal income, which includes government transfers.[8] We study these labor market outcomes for each calendar year from the year of diagnosis up to 10 years later. Finally, we examine effects on government transfers. The data come from *DREAM*, a weekly register of all persons who receive government transfers. We consider four types of payments: sickness leave benefits, welfare benefits paid to unemployed individuals without unemployment insurance, welfare benefits paid to individuals who work reduced hours due to health limitations, and disability benefits. An individual is included in *DREAM* if they receive a benefit for at least one day during the week but the amount of the transfer is not recorded. We define indicators for receipt of any benefits as well as separately for sickness benefits and disability benefits. We also calculate the number of weeks an individual receives these benefits. We construct these variables as cumulative measures for the periods 1–5 and 6–10 years after diagnosis. In order to take into account a potential bias from selective survival, we assign the value one to the out of labor force indicator and zero to all other labor market outcomes of non-survivors.

Control Variables. We observe a rich set of patient characteristics in the clinical *Breast Cancer Database*. Using these data we construct the following indicators and all possible interactions among them: having mastectomy, being younger than 45 years of age at diagnosis, the number of positive nodes (0, 1–3, 4+), having a tumor larger than 50mm, and having the tumor removed micro-radically. This allows us to flexibly control for the determinants of radiation therapy eligibility. We also construct indicators for the type of chemotherapy received (DBCG89 clinical trial arm).

Some of our specification checks use additional nationwide registers to construct demographic characteristics of patients at the time of diagnosis. We construct indicators for marital/cohabitation status, immigration status, and level of urbanization of the municipality of residence from the *Population Register*, which provides a snapshot of all residents as of January 1st of each year. In addition, we calculate the number of years of schooling from the *Education Register*, a database with information on the highest level of completed schooling from administrative school records.

[8] All monetary variables are expressed in 2015 Danish Kroner. 100 Kroner in 2015 are roughly equivalent to 15 USD in 2015.

Analysis Sample. Our analysis sample includes a subset of female breast cancer patients diagnosed between 1990 and 1998. Appendix Table A1 details the construction of the analysis sample. Our starting sample includes 26,900 patients. We impose four main restrictions to construct the analysis sample. First, we drop observations on women who were not enrolled in the DBCG89 clinical trial. The primary reasons for exclusion from the trial are contraindications due to old age (61%), previous malignancies (7%), distant metastases (6%), and bilateral carcinomas (4.4%). Second, we restrict our attention to only high-risk pre-menopausal women in order to ensure that our sample is homogeneous in terms of risk classification and menopausal status. We also exclude a small subset of cancer patients who were eligible for radiation therapy regardless of when they were diagnosed, but for whom the intensity of radiotherapy increased if they were diagnosed after 1995. Third, we exclude patients for whom we have incomplete clinical information on receipt of radiation therapy, tumor size, and on whether the tumor was removed microradically because otherwise we cannot characterize their radiation therapy eligibility status. Finally, we exclude women 55 and older at the time of diagnosis because we need individuals to be below the retirement age 10 years after diagnosis in order to be able to investigate long-term effects on labor market outcomes. The final sample consists of 2,823 observations.

The women in the analysis sample can be divided into three groups. The first group, which we call $T95$, includes women with characteristics that make them eligible for radiotherapy only if they are diagnosed after 1995 ($N = 1{,}290$). These are high-risk pre-menopausal women whose risk classification was not due to only staging, who had a mastectomy, and who were either (i) older than 45 at the time of diagnosis or (ii) younger than 45 with fewer than 4 positive lymph nodes. The second group (*always eligible*, $N = 874$) includes patients who are eligible for radiation therapy regardless of when they are diagnosed. This includes pre-menopausal high-risk patients who had a lumpectomy, as well as pre-menopausal mastectomy patients younger than 45 years of age with at least 4 positive lymph nodes. The last group (*never eligible*, $N = 659$) includes pre-menopausal women who are classified as high-risk only because of a stage II or III ductal carcinoma (i.e., they have tumors smaller than 50mm and no positive lymph nodes). These patients are never eligible to receive radiation therapy during the period under study.

4 Empirical Strategy

We are interested in estimating the impact of radiation therapy on health and labor market outcomes of breast cancer patients. The baseline model takes the form:

$$Y_{it}^{a} = \alpha_1 + RT_{it}\beta_1 + \mathbf{X}_{it}\gamma_1 + u_{1t} + \epsilon_{1it}, \tag{1}$$

where Y_{it}^{a} is an outcome observed a years after the diagnosis of patient i who was diagnosed with breast cancer in year t. Our main independent variable, RT_{it}, is a variable indicating receipt of radiation therapy. $\mathbf{X}_{it}$ is a vector of demographic and clinical patient characteristics measured at the time of diagnosis. Finally, u_{1t} are fixed effects for the type of chemotherapy received (DBCG89 clinical trial arm) and for year of diagnosis. We cluster the standard errors at the hospital level.

The key coefficient of interest in Equation (1), β_1, measures the average difference in the outcomes of breast cancer patients who receive radiation therapy in addition to chemotherapy as compared to those who only receive chemotherapy, after controlling for observed characteristics of the patient. Empirical identification of β_1 is complicated since medical treatments are unlikely to be randomly assigned: patients in worse health tend to receive more intensive medical treatments.

In order to address this endogeneity problem, we employ a two-stage least-squares (2SLS) approach that exploits the plausibly exogenous variation in radiation therapy stemming from the 1995 change in guidelines. In particular, we define our instrument as the interaction between an indicator for belonging to the group of breast cancer patients to whom eligibility was expanded in January 1995 ($T95_i$) and a dummy variable for being diagnosed after January 1995 ($Post95_t$). This motivates the following first-stage equation capturing the impact of the proposed instrument on receipt of radiation therapy:

$$RT_{it} = \alpha_2 + T95_i Post95_t \beta_2 + \mathbf{X}_{it}\gamma_2 + u_{2t} + \epsilon_{2it}, \tag{2}$$

and the following reduced-form equation relating the instrument to outcome variables:

$$Y_{it}^{a} = \alpha_3 + T95_i Post95_t \beta_3 + \mathbf{X}_{it}\gamma_3 + u_{3t} + \epsilon_{3it}, \tag{3}$$

where the vector of patient characteristics $\mathbf{X}_{it}$ flexibly controls for the determinants of radiotherapy eligibility. Note that our first-stage and reduced-form equations are equivalent to a difference-in-differences model with $T95$ as the treatment group.

In order for 2SLS to yield consistent estimates of the parameter of interest, three conditions must be satisfied. First, the instrument should be a sufficiently-strong determinant of radiation therapy treatment so as to reduce finite-sample bias inherent in 2SLS (the relevance condition). In our context this means that the adoption of the 1995 guidelines should lead to a sufficiently large increase in receipt of radiotherapy among women impacted by the eligibility expansion. The relevance condition is easily tested using the results of the first-stage equation. Recent research indicates that finite-sample bias is of little concern if the first-stage F-statistic testing the significance of the instrument is greater than 104.7 (Lee et al., 2022).

Second, the instrument needs to be as good as randomly assigned (the exogeneity condition), conditional on observed characteristics. In our difference-in-differences setting, this assumption requires that, given the set of patient characteristics that determine radiotherapy eligibility, the comparison group provides a valid counterfactual for the outcomes that would occur in the treatment group in the absence of the guideline change. While this assumption is not directly testable, we assess its plausibility in several ways. We initially show that the characteristics of women that are not tied to radiotherapy eligibility are balanced between the treatment and comparison groups. Similarly, we document that the outcomes of women diagnosed before the guideline change are similar between the treatment and comparison groups. We also confirm that our results are robust to the comparison group used.

Third, the instrument should affect the outcome of interest only through its effect on the treatment variable (the exclusion restriction). This assumption rules out other guideline changes or public policies that coincide with the 1995 radiation therapy guideline change and target the $T95$ group of patients. This assumption is assured by institutional design: there were no other guideline changes implemented by the DBCG during this period that targeted the patients in the $T95$ group. In addition, the fact that the eligibility for radiotherapy is determined by a set of both clinical and demographic characteristics makes it very unlikely that any other public policy would only affect the women in the treatment group.

If the instrument also satisfies the condition of monotonicity, our instrumental variable strategy will provide the Local Average Treatment Effect (LATE) of radiation therapy for patients who receive radiotherapy due to the expanded eligibility conditions, but would not have received it otherwise (Angrist et al., 1996). The monotonicity condition requires that being diagnosed after 1995 only increases the chance that a patient in the $T95$ group receives radiation therapy. The LATE cannot be estimated if eligibility of radiotherapy reduces a patient's likelihood of undergoing radiation therapy, for example, due to congestion effects. Monotonicity cannot be tested formally but we provide evidence of its plausibility in Section 5 by presenting estimates from the first-stage equation in different subsamples. The comparability of the LATE to the average treatment effect in the population depends on the size of the "complier" population. As we will document in Section 5, compliers comprise around 75% of our analysis sample, suggesting that our results are broadly relevant.

5 Results

5.1 Descriptive Statistics

Table 1 provides descriptive statistics for the overall analysis sample (column 1), for women who receive combined radiation therapy with chemotherapy (column 2) and for those who receive only chemotherapy (column 3). Variable names ending in a question mark are indicators with one being yes and zero being no. The final column reports the p-value for the test of equality of means between patients receiving and not receiving radiotherapy. About 52% of the patients receive radiation therapy.

Panel A summarizes the demographic characteristics of patients at the time of diagnosis. The average cancer patient in our sample is 43.5 years old with 13 years of schooling. About 70% are married and 84% work in the 2–4 years prior to diagnosis. Patients who receive both chemotherapy and radiotherapy are slightly younger, slightly more educated and substantially less likely to be married at the time of diagnosis relative to patients who receive only chemotherapy. While there is no difference in pre-cancer employment rates between the two groups, the pre-diagnosis income and labor earnings of women receiving radiotherapy are 4–5% higher.

Panel B focuses on disease pathology. The statistics suggest that patients who undergo radiotherapy tend to have substantially worse clinical characteristics. Their average tumor size is 14% larger than the average tumor size of those who only receive chemotherapy. This is primarily due to the higher share of tumors larger than 50mm among patients treated with radiation therapy. Radiotherapy patients also have a higher average number of lymph nodes that contain cancer. This is not surprising given that during the initial part of our analysis period, only patients with at least 4 positive nodes were eligible to receive radiotherapy. Similarly, the near-universe of patients who do not receive radiation therapy have mastectomy as the primary surgery because lumpectomy patients are always eligible to receive radiotherapy.

Panels C presents the post-diagnosis health outcomes for the different subsamples. Given the negative selection of patients into different treatment regimens, it is not surprising that mortality is significantly higher among patients who receive radiotherapy combined with chemotherapy. Mortality differences appear as early as one year after diagnosis and grow over time. 10 years after diagnosis, the mortality rate of radiotherapy patients is 5 percentage points higher than the mortality rate of women who do not receive radiation therapy. These mortality differences are almost entirely driven by mortality from breast cancer.

Panel D describes the labor market outcomes. The summary statistics suggest that radiotherapy patients tend to have worse labor market performance. They are less likely to be employed and more likely to be out of the labor force. While their labor earnings and total income remain higher, the difference relative to the group of patients who only receive chemotherapy declines over time. The differences in these outcomes are small in magnitude and generally not statistically significant. In contrast, there are economically large differences in welfare use between the two groups, with radiotherapy patients receiving government transfers at much higher rates.

The raw correlations described in Table 1 show that radiotherapy patients are the highest risk patients and it is therefore no surprise that, in raw averages, they have the highest cancer mortality. Identifying the complete set of characteristics that determine mortality and are correlated with radiotherapy is unlikely to eliminate concerns about omitted variables bias. To form a baseline case, Appendix Tables A2 and A3 present the OLS estimates of the relationship between radiation therapy and the outcomes of cancer patients. Each cell presents estimates from a different regression with the outcome variable indicated in the row and the period after diagnosis indicated in the column. All regressions flexibly control for the clinical characteristics determining radiation therapy eligibility, as well as fixed effects for the type of chemotherapy treatment and for year of diagnosis (see Section 4). Standard errors are clustered at the hospital level. The results indicate no correlation with mortality in the short-run but statistically significant mortality declines starting from five years after diagnosis. For example, we find that radiation therapy is associated with a 5.6 percentage point decline in all-cause mortality five years after diagnosis. This association grows to 6.8 percentage points after ten years. The mortality gains are due to a reduction in breast cancer mortality. Even though controlling for observable characteristics reverses the sign of the association between radiation therapy and patient outcomes, the results raise the concern that the same could hold for other, unobserved characteristics, and that the estimated associations are biased because of these omitted variables.

For completeness, the remainder of the results in Appendix Table A2 document the relationship between radiation treatment and labor market outcomes. Recall that we assign the value one to the out of labor force indicator and zero to all other labor market outcomes of non-survivors to address a potential bias from selective survival. The OLS associations suggest a weak relationship between radiation therapy and the likelihood of dropping out of the labor force, in both statistical significance and magnitude, starting from three years after cancer diagnosis. While radiotherapy is also consistently positively associated with the likelihood of being employed and both of our measures of income, these associations are generally not statistically significant at conventional levels. Similarly the OLS results in Appendix Table A3 indicate a consistently negative but generally statistically insignificant relationship between radiation therapy and the likelihood of receiving government transfers.

In the next section, we turn to our quasi-experimental approach that leverages the variation in radiation therapy stemming from the 1995 change in guidelines.

5.2 Effects of Radiation Therapy on Mortality and Labor Market Outcomes

We first provide visual evidence on the first-stage relationship between the 1995 radiotherapy guideline change and the likelihood of receiving radiotherapy. Given that the women impacted by the eligibility expansion and those in the comparison group (i.e., remaining high-risk pre-menopausal women) differ along clinical and demographic characteristics by design, we present in Figure 1 the regression-adjusted probability of receiving radiation therapy by year of diagnosis. Specifically, we regress the indicator for receipt of radiation therapy on the characteristics that determine radiotherapy eligibility, separately for $T95$ and the comparison group, and then plot the average of the residuals from these regressions for women diagnosed in the year indicated on the horizontal axis. The solid line represents the women in $T95$ while the dashed line represents the group of women who are never or always eligible for treatment.

Figure 1 shows that take-up of radiotherapy is constant among the comparison group throughout the entire period. In contrast, take-up in the $T95$ group is fairly stable before and after the 1995 guideline change with a sharp level shift in 1995.[9] Consistent with the visual evidence, the regression estimate for the first-stage relationship between the instrument and treatment take-up, based on Equation (2), is economically large and highly statistically significant. In particular, we find that the 1995 guideline change led to an increase of 75.3 percentage points (s.e. 1.8) in the probability of radiotherapy among women in the $T95$ group relative to other high-risk pre-menopausal patients. The associated F-statistic is 1,842.3, well above the recent rule-of-thumb value of roughly 100 (Lee et al., 2022). First-stage estimates for the full sample and subsequent sub-samples are given in Appendix Table A9.

We next turn to effects on mortality. Figure 2 plots the 2SLS coefficients on the indicator for radiotherapy and corresponding 95% confidence intervals from separate models with the mortality indicators as outcomes, measured at the time indicated on the horizontal axis. Circles represent effects on all-cause mortality and diamonds

[9]Figure 1 shows that take-up of radiotherapy among $T95$ women increases already in 1994. Appendix Figure A2 shows that this is due to an increase in the last two quarters of 1994 when some hospitals adopted the new guidelines before the official enactment date. Our results are robust to excluding 1994.

represent effects on breast cancer mortality. Regression coefficients corresponding to Figure 2 are provided in the first two rows of Appendix Table A4, while the first two rows in Appendix Table A5 present regression coefficients on the instrument from the reduced-form Equation (3).

The results suggest that radiation therapy leads to substantial mortality reductions. The benefits appear as early as three years post diagnosis and the coefficients are statistically significant at the 5% level starting from five years after cancer diagnosis. Women who receive combined radiotherapy and chemotherapy are about 10 percentage points less likely to die 5–10 years after diagnosis relative to women who are treated with chemotherapy alone, representing 35–60% reductions relative to the mean mortality among the untreated patients. The reduction in all-cause mortality is entirely driven by the reduction in breast cancer mortality.

Having established the mortality gains from radiotherapy, we next plot in Figure 3 the 2SLS coefficients on the indicator for radiotherapy and corresponding 95% confidence intervals from separate models with labor market outcomes, measured at the time indicated on the horizontal axis. Corresponding regression coefficients are provided in rows 3–7 of Appendix Table A4 and the reduced-form results are presented in rows 3–7 of Appendix Table A5. In Figure 3a, circles, diamonds and squares represent effects on the likelihood of being employed, unemployed, and out of the labor force, respectively. We find that radiation therapy leads to statistically significant increases in the probability of employment. The magnitudes are sizeable ranging from 8.6 percentage points (11% at the mean) in the first year after diagnosis to 15.5 percentage points (37%) ten years after. The rise in employment is entirely due to a reduction in the likelihood of exiting the labor force.

Figure 3b focuses on our measures of income, with circles representing effects on annual labor earnings and diamonds representing effects on gross personal income (including government transfers). Consistent with the results on employment, we find that receipt of radiotherapy leads to an increase in annual labor earnings of about DKK 25,864–65,107 (USD 3,845–9,679) and in annual gross personal income of about DKK 18,745–62,744 (USD 2,787–9,327) during the ten years following cancer diagnosis. These are economically large gains representing 13–45% of average annual labor earnings and 7–27% of average gross personal income.

The fact that the gains in labor earnings are higher than those in total income is consistent with the compensating role of income insurance in Denmark that partially covers for the lost earnings of individuals who experience severe health shocks. For this reason, we provide in Table 2 evidence on the effects of radiation therapy on government transfers. The 2SLS results indicate that women who receive radiation therapy are about 10 percentage points less likely to receive government transfers during the first 10 years after they are diagnosed with cancer. This is a large effect considering that around 33–41% of untreated women receive government transfers. The reduction in the likelihood of receiving government transfers is mainly driven by a decline in the receipt of sickness benefits (i.e., a fall in the likelihood of being on sick leave). The estimated effects on the likelihood of being on disability insurance are large but not statistically significant. Similarly, the effect sizes at the intensive margin are economically large, with the average number of weeks on government transfers falling by 3.5–9 weeks (relative to means of 28–31 weeks), but only the effect on the number of weeks on sickness benefits is marginally significant.

5.3 Comparing the Estimated Effects to the Existing Literature

How do our estimated mortality effects compare to those documented in the DBCG82 randomized clinical trial that led to the 1995 guideline change? The DBCG82 clinical trial examined the impact of adding radiotherapy to chemotherapy among high-risk pre-menopausal women diagnosed with breast cancer between 1982–1989. Overgaard et al. (1997) report that the 10-year mortality rate among women randomized to receive radiation therapy in addition to chemotherapy was 9 percentage points lower than among women who received only chemotherapy. The fact that the mortality gains we estimate using 2SLS are almost identical to those found in an earlier randomized control trial raises confidence in the validity of the key identification assumptions in our observational study. In addition, the mortality gains are identical to the gains observed in women treated 10 years earlier, which suggests that the returns to radiotherapy did not diminish during this period.

Our results suggest that radiation therapy has major economic benefits: it increases the probability of employment by 11–38%, it improves labor earnings by 13–

45%, and it mitigates the cumulative risk of being on welfare by 33–41%. These effect sizes are generally comparable to those found in other studies evaluating the economic effects of medical treatments. For example, Biasi et al. (2023) focused on the pharmaceutical treatment of bipolar disorder and find that access to lithium by age 20 increases labor market participation by 30% and earnings by 26%. Garthwaite (2012) found that Cox-2 inhibitors, medications used in the treatment of chronic pain and inflammation, increase the likelihood of working by 22 percentage points relative to a mean of almost 40%. Butikofer and Skira (2018) documented that the market entry of Vioxx, a popular Cox-2 inhibitor, reduced the number of sickness leave days among individuals with joint pain by 7–12% while its removal from the market increased sickness absence days by 12–16%.

It may also be helpful to benchmark our estimates against the effects of breast cancer on women's labor market outcomes. Among all Danish women aged 21–54 during 1990–1998, the difference between the employment rate of women with and without breast cancer ranges from 5–22 percentage points one to ten years after diagnosis. These employment gaps are larger than those found in the United States (Bradley et al., 2002a,b) but comparable to those documented in Denmark in prior studies (Heinesen and Kolodziejczyk, 2013). Overall, our results imply that radiation therapy can reduce the long-run employment gap by around 70%.

5.4 Instrument Validity and Robustness Checks

The 2SLS method yields consistent estimates if the instrument satisfies the relevance assumption, the exogeneity assumption, and the exclusion restriction. The change in guidelines has an economically large and statistically significant effect on radiotherapy take-up, so we can safely conclude that the relevance assumption is satisfied.

The exogeneity assumption requires that the comparison group provide a valid counterfactual for the time path of the outcomes of women in the $T95$ group in the absence of the guideline change. We bring suggestive evidence on the plausibility of this assumption in several ways. First, we present in Appendix Table A7 descriptive statistics separately for women in the $T95$ and in the comparison group who are diagnosed before the guideline change. Since the 1995 guideline change targeted patients based on clinical characteristics and age at diagnosis, it is not surprising that we find differences between women in the $T95$ and in the comparison group along these dimensions. However, when we compare the characteristics that are not tied to radiotherapy eligibility, we find relatively small and generally statistically insignificant differences. In the cases when the differences are statistically significant (employment status, years of education), they are economically small.

We next estimate an event-study type of model in which the time to event is given by the difference between the year of diagnosis and 1995:

$$Y_{it}^{a} = \alpha_4 + \sum_{\substack{j=-5 \\ j \neq -1}}^{3} RT_{it} \mathbb{1}(t - 1995 = j)\beta_{4j} + \mathbf{X}_{it}\gamma_4 + u_{4t} + \epsilon_{4it}. \tag{4}$$

A test of parallel pre-intervention trends is given by a test of joint significance of β_{4j} for $j < 0$.[10] The distribution of the p-values corresponding to these tests is provided in Appendix Figure A3. Most of the p-values are above 0.1, indicating statistically insignificant differences between the outcomes of women in $T95$ and in the comparison group in the period before the guideline change. In fact, the null hypothesis of parallel trends is rejected in 13% of the cases, which is about what we would expect when conducting multiple hypothesis tests at 10% significance.

As a final check of the exogeneity assumption, we estimate our reduced-form model using demographic characteristics that are not tied to radiation therapy eligibility as well as average outcomes 2–4 years before diagnosis as the dependent variable.[11] The results presented in Appendix Table A8 indicate, with the exception of unemployment, no statistically significant changes in these predetermined characteristics of women in the $T95$ group relative to the comparison group.

We next turn to the exclusion restriction. In our setup, this assumption implies that the radiotherapy guideline change is the only factor that can affect the outcomes of women in the $T95$ group after 1995. We explore the plausibility of this assumption through a placebo regression. We restrict our sample to only the comparison group

[10] We also implement some of the more recent event study methods (e.g., Sun and Abraham, 2021). The results, available upon request, are quantitatively and qualitatively similar.

[11] We are unable to examine effects on pre-diagnosis welfare receipt as the data on government transfers begins in 1991.

(i.e., women who are either always or never eligible for radiotherapy) and we assign the 1995 radiation therapy guideline change to always-eligible women. If our baseline estimates pick up an improvement in the outcomes of women diagnosed after 1995 unrelated to the effectiveness of radiotherapy, then we would likely see an association between the guideline change and the outcomes of always-eligible women as well. However, the reduced-form results plotted in Appendix Figure A4 show that the guideline change did not lead to any statistically significant differences in the outcomes of always-eligible women relative to never-eligible women.

Finally, we discuss the validity of the monotonicity assumption, which allows us to interpret our results as LATE of radiation therapy. Monotonicity requires that the 1995 guideline change only increases the likelihood of a patient receiving treatment. This assumption would be violated if the expansion of eligibility for radiotherapy reduced the likelihood of undergoing radiation therapy for some, for example, due to congestion effects. Intuitively, we do not expect such a violation to be present in our sample because radiotherapy is provided in a handful of locations with large treatment capacities. For example, in 2007, six radiotherapy centers provided a total of 220,000 treatments (Olsen et al., 2007). The radiotherapy guideline expanded the number of cancer patients eligible for treatment by 500. Each of these patients were eligible to receive 20–25 treatments, corresponding to a 6% increase in treatment demand based on the treatment capacity in 2007.

Formally, a violation of the monotonicity assumption implies that the first-stage coefficient on the guideline change indicator is negative for certain patients. In the spirit of Mueller-Smith (2015), we estimate the first stage across subgroups defined by education, pre-diagnosis income, marital status, and predicted mortality risk. The results presented in Appendix Table A9 show that the estimated first-stage coefficient is remarkably stable in magnitude across all these subgroups.

In the remainder of the section, we examine the robustness of the 2SLS estimates to alternative modeling choices and to alternative ways of constructing the analysis sample. We start by examining the sensitivity of the results to the inclusion of additional controls. If the exogeneity assumption holds (i.e., there are no systematic differences between the $T95$ and the control group beyond the characteristics determining radiotherapy eligibility), adding more covariates should not change our baseline estimates. Appendix Figure A5 plots the 2SLS coefficients on the indicator for radiotherapy and corresponding 95% confidence intervals from separate models with outcomes measured at the time indicated on the horizontal axis. Our estimates are very similar when we include additional demographic characteristics (years of schooling, marital status, immigration status, level of urbanization of residence), the average of outcomes two to four years before diagnosis, or hospital fixed effects.[12]

We next check if the way we selected the analysis sample has any influence on our estimates. Appendix Figure A6, constructed in the same way as Appendix Figure A5, shows that the estimates are robust when we exclude (i) women diagnosed in 1994, the year when some hospitals already adopted the revised guidelines, (ii) women residing in areas where breast cancer screening programs were piloted, and (iii) women who received lumpectomy as primary surgery.[13]

It is important to emphasize that the key identifying assumptions of 2SLS are ultimately untestable and we can never rule out all scenarios that can lead to their violation. On their own, none of the checks described above is sufficient to claim the validity of the 2SLS assumptions. However, taken together they provide consistent evidence that these assumptions are likely to hold in our context, and suggest that our model is likely to yield causal estimates of radiation therapy.

5.5 Treatment Effect Heterogeneity and Potential Pathways

In this section, we explore treatment effect heterogeneity and discuss how radiotherapy may impact patient health and labor supply. We present the heterogeneity in our estimates along two dimensions: disease severity and socio-economic status. We summarize disease severity using the predicted 10-year breast cancer mortality from a specification including all disease characteristics using out-of-sample untreated units (Abadie et al., 2018). In particular, we estimate a probit specification where the outcome is an indicator for dying from breast cancer during the ten years after diagnosis and the control variables are the disease characteristics described in Section 3. We

[12] Our inference is also robust to alternative levels of clustering. In particular, we confirm that our results (available upon request) are not sensitive to clustering at the level of age at diagnosis or the health care region in which the patient resides.

[13] Our results are also similar when excluding women diagnosed with breast cancer in the last two quarters of 1994 or who were treated in hospitals that implemented the new guidelines early.

estimate this model using all the breast cancer patients diagnosed during our sample period who are not included in our analysis sample and who are not treated with radiation therapy. We next use these estimates to predict the probability of death from breast cancer within 10 years for all the women in our analysis sample. Finally, in each year we take the median predicted mortality rate in our sample and classify women diagnosed in that year as above or below the median.

Appendix Figure A7 plots the 2SLS coefficients on the indicator for radiotherapy and corresponding 95% confidence intervals for the full analysis sample and for the sub-samples with high (above median) and low (below median) breast-cancer mortality risk. Similar to the previous figures, each coefficient is obtained from a separate model with outcome measured at the time indicated on the horizontal axis. The mortality gains are similar in the two subsamples soon after diagnosis but they start to diverge in year six. Ten years after diagnosis, higher-risk women treated with radiation therapy are 18.0 percentage points less likely to die (mean mortality among the untreated patients: 34.2%), while the mortality rate of lower-risk women is reduced by a statistically insignificant 3.6 percentage points (mean mortality among the untreated patients: 25.1%). In addition, the labor market benefits documented in the full sample are mainly driven by women with high mortality risk and that these differences emerge as early as two years after diagnosis. Among high-risk women, radiotherapy increases the probability of employment by 21.3 percentage points and annual labor earnings by more than DKK 63,000 (USD 9,400) ten years after cancer diagnosis. The corresponding estimates for women with below-the-median mortality risk are much smaller, both in absolute and in relative terms, and not statistically significant.

Given the previous literature that finds a socioeconomic gradient in breast cancer mortality (e.g., Palme and Simeonova, 2015), in Appendix Figure A8 we show the heterogeneity in the estimates by patient socioeconomic status as proxied by education. We classify women as highly educated if they have at least some postsecondary education (i.e., more than 12 years of schooling). Appendix Figures A8a and A8b show that highly-educated and low-educated women have similar short-run mortality gains from radiation therapy but treatment reduces long-run mortality only among highly-educated patients. Our results suggest that highly-educated women who receive combined chemotherapy and radiation therapy are 13.6 percentage points less likely to die during the ten years after diagnosis, a 50% decline relative to the 28% mean mortality risk among the untreated. The results for low-educated individuals, on the other hand, indicate a statistically insignificant 1 percentage point reduction in mortality risk (relative to a 31.7% baseline) ten years after diagnosis. The remaining figures present heterogeneity results for labor market outcomes. We find that the economic gains related to radiotherapy are observed exclusively among individuals with postsecondary education and appear soon after cancer diagnosis. Ten years after diagnosis, highly-educated women treated with radiation therapy are 19.3 percentage points (40%) more likely to be employed and have DKK 87,061 (USD 12,942 or 51%) higher labor earnings. The point estimates among the low-educated women are much smaller and not statistically significant. The fact that the employment benefits are concentrated among highly-educated women is consistent with prior research that finds that the economic benefits from medical innovation (measured by the number of approved drugs and a patent index) in Canada are experienced only by cancer patients with postsecondary education (Jeon and Pohl, 2019).

What explains the documented treatment heterogeneity by education? In Appendix Table A10, we examine the effects of radiation therapy on long-run breast cancer recurrence. The first panel presents 2SLS estimates for highly-educated women while the second panel focuses on low-educated patients. In column (1), we estimate our baseline specification using an indicator for breast cancer recurrence within ten years of diagnosis as the outcome. We find that radiotherapy leads to significant declines in the likelihood of recurrence among highly-educated women but not among low-educated women. The magnitudes suggest that the effects on the long-run mortality of highly-educated women are entirely due to a reduction in recurrence. In column (2), we examine whether the observed differences may be due to the fact that highly-educated patients have access to better quality providers. Including hospital fixed effects has no impact on the estimated gaps by education. In column (3), we check whether the differences may be driven by differential adoption of cancer screenings by highly-educated women residing in areas where pilot screening programs were implemented. The estimated heterogeneity by education remains when we exclude patients residing in the areas where universal breast cancer screening programs were introduced during our study period. Appendix Figure A9 confirms that the heterogeneity in our main outcomes are also robust to these checks.[14]

Appendix Table A11 provides further evidence on the comparability of disease severity by patient education. Column (1) reproduces descriptive statistics on disease characteristics in the analysis sample, while columns (2)–(3) describe disease characteristics of low- and highly-educated patients, respectively. The results suggest that aside from a minor difference in the type of primary surgery, patients with high and low education have very similar disease traits. The last row summarizes these differences by presenting the average predicted 10-year breast cancer mortality in the two groups. We find that low-educated women have a slightly higher predicted mortality but we cannot reject the equality of the predicted mortality risks in the two groups.[15]

The fact that the education gradient in the effects on mortality is driven by differences in recurrence and not by differences in provider quality or disease severity suggests that lifestyle factors, such as diet, exercise, smoking, and alcohol use, may be an important mechanism. Our data are not suitable to examine this hypothesis but such an explanation is consistent with prior medical studies that find a strong correlation between lifestyle differences and risk of breast cancer recurrence (Cannioto et al., 2023) and between education and lifestyle factors (Puka et al., 2022).

Differences in health behaviors and the resulting differences in mortality, however, cannot explain fully the estimated differences in the economic gains by education.[16] One possible pathway behind the heterogeneity in the effects of treatment on labor market outcomes could be that low-educated patients have more physically de-

[14]Another potential explanation could be differences by education in adherence to cancer treatment. Such differences can occur even in Denmark where health care is universal, for example, because low-educated women are more likely to live in rural areas farther away from treatment centers (27.6% as compared to 21.7% for highly-educated women). Register data on the number of treatments exist only starting from 1999, so we can only provide suggestive evidence on this mechanism. In the sample of women diagnosed with breast cancer between 1999–2007, who had similar disease characteristics as the women in our analysis sample, and for whom we were able to find information on treatments, the average number of radiotherapy treatments is 23.4 for highly-educated women and 23.1 for low-educated women. The corresponding numbers for chemotherapy treatments are 5.3 and 4.9. This suggests that the education gradient in the effect of radiation therapy on mortality and recurrence is unlikely to be due to differences in treatment adherence.

[15]Recall that we find larger returns to radiotherapy treatment among patients with higher predicted mortality risk. As such, disease severity differences cannot explain the lower returns to treatment among low-educated patients.

[16]We find that radiation therapy reduces highly-educated women's long-run mortality risk by 13.8 percentage points and increases their probability of being employed by 19.3 percentage points.

manding occupations that make it difficult to work while undergoing more intensive treatments. To shed light on this, we characterize the physical intensity of a patient's pre-diagnosis occupation using the Metabolic Equivalent of Task (MET) values provided by Deyaert et al. (2017).[17] Occupations are considered physically light if the MET value is less than 3, physically moderate if the MET value is 3–6, and vigorous if the MET value is above 6. We have information on 562 women's occupation two years before diagnosis (255 in $T95$ and 307 in the comparison group).[18] Similar to the full analysis sample, roughly 63% of these women are highly educated. The overwhelming majority of women are employed in occupations that are physically light, regardless of educational attainment (97.2% of highly-educated and 92.3% of low-educated) and no women in our sample have physically vigorous occupations. That said, a higher proportion of low-educated women (7.7%) work in physically moderate occupations relative to highly-educated women (2.8%). Moreover, when we focus on the distribution of MTE values among women with physically light jobs, we see that the distribution is shifted to the left for highly-educated women (see Appendix Figure A10), suggesting that low-educated women employed in physically light occupations have physically more demanding tasks. Overall, we cautiously interpret this evidence to suggest a role for the physical demands of occupations on the employment effects of radiotherapy treatment.

6 Conclusions

This paper uses rich clinical and administrative data from Denmark to study the effects of radiation therapy on the mortality and labor supply of breast cancer patients.

[17] MET value was originally developed by (Ainsworth et al., 1993). The authors complied an extensive list of daily tasks and calculated the associated physical activity energy expenditure. MET value of a task represents the intensity of the activity as the ratio of work metabolic rate to resting metabolic rate. One MET is equal to 1 kcal/kg/h and roughly captures the energy cost of sitting quietly. Deyaert et al. (2017) used detailed task definitions under each 2008 International Standard Classification of Occupations (ISCO-08) to calculate mean MET values.

[18] The occupation codes (available from 1991) are extracted by Statistics Denmark from the occupations reported in payrolls by employers. If there is no such information, then the occupation is imputed from the type of the highest degree obtained, the unemployment fund to which the person contributes, or the industry of employment. Even with the imputation it is still impossible to determine the occupation of a relatively large number of persons during our analysis period.

In order to identify the causal effects, we exploit variation in radiotherapy eligibility stemming from a medical guideline change in 1995. We find that patients who receive combined chemotherapy and radiotherapy are significantly less likely to die relative to patients who are only treated with chemotherapy. Our results suggest that radiation therapy reduces the likelihood of death by roughly 35% within the ten years after diagnosis. We next examine the effects of treatment on labor market outcomes and find that radiation therapy has major economic benefits. Our findings indicate that, ten years after diagnosis, treated women are 37% more likely to be employed and earn 45% more than untreated patients. We also find some evidence that treated patients are less likely to rely on welfare, with treatment reducing the cumulative risk of receiving government transfers ten years after diagnosis by 10 percentage points.

In the last part of the paper, we show that the estimated mortality and economic gains are driven by patients with post-secondary education. Data limitations do not allow us to investigate the precise mechanisms behind these heterogeneous effects but we are able to rule out that they are due to differences in provider quality, in access to screening programs, and in disease severity by education. We also document that the long-run mortality reductions are entirely due to a decline in breast cancer recurrence. Given prior studies documenting lifestyle differences by education and those linking lifestyle factors to risk of recurrence, we argue that health behaviors could be an important pathway. Finally, we explore the role of differences in the physical demands of jobs as a mediator of the heterogeneous effects on labor market outcomes. We show that low educated women are more likely then high educated women to work in jobs with moderate physical demands and that they have more physically-demanding tasks in occupations that are generally not physically demanding.

Given that an increasing share of breast cancer patients are diagnosed during their working years, understanding the effects of cancer treatments on socio-economic outcomes becomes even more important. Taken together, our results suggest that cancer treatments not only impact survival but also lead to large economic gains which should be considered when assessing the cost-effectiveness of new cancer treatments. Our finding that mortality and economic gains are driven by the highly educated in a country with universal health insurance further suggests that equalizing access to treatment may not be sufficient to reduce inequalities in health.

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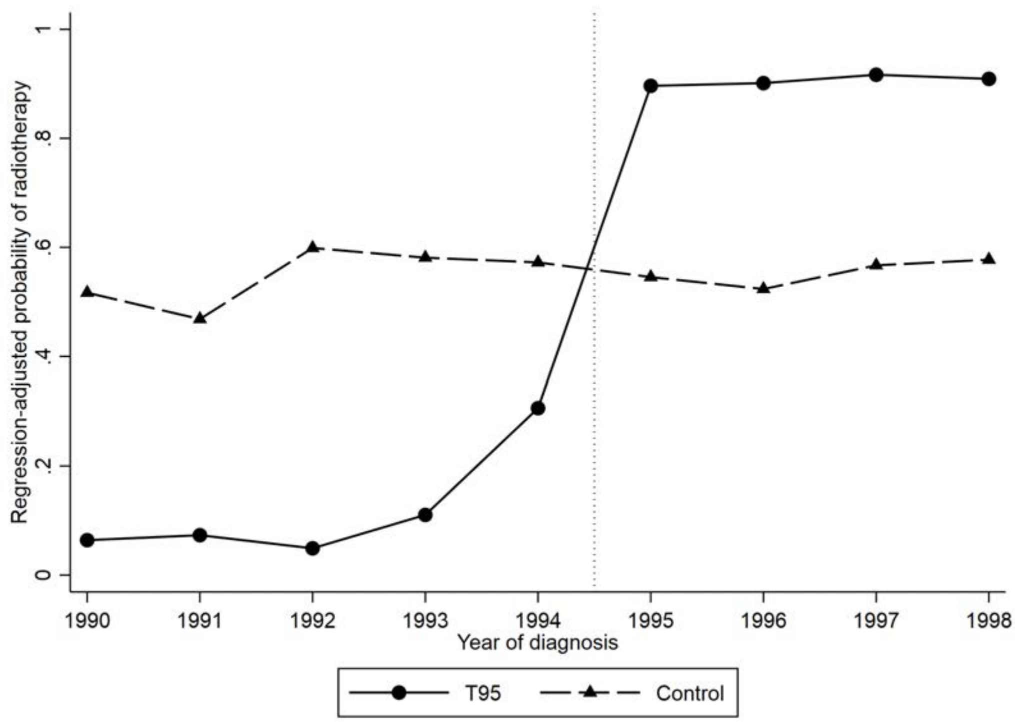
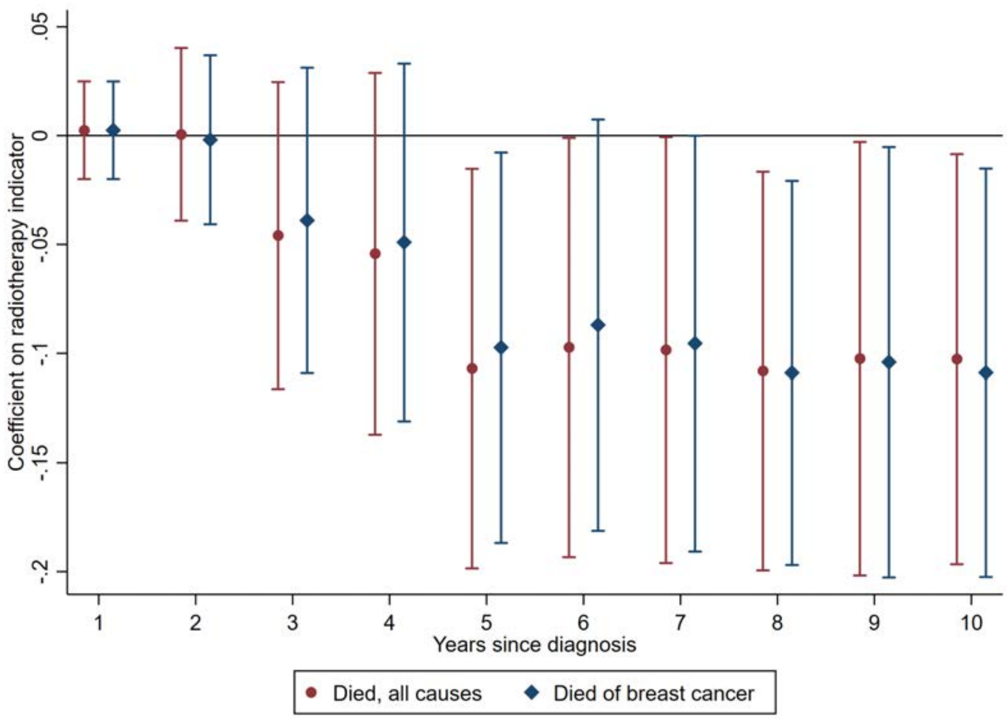


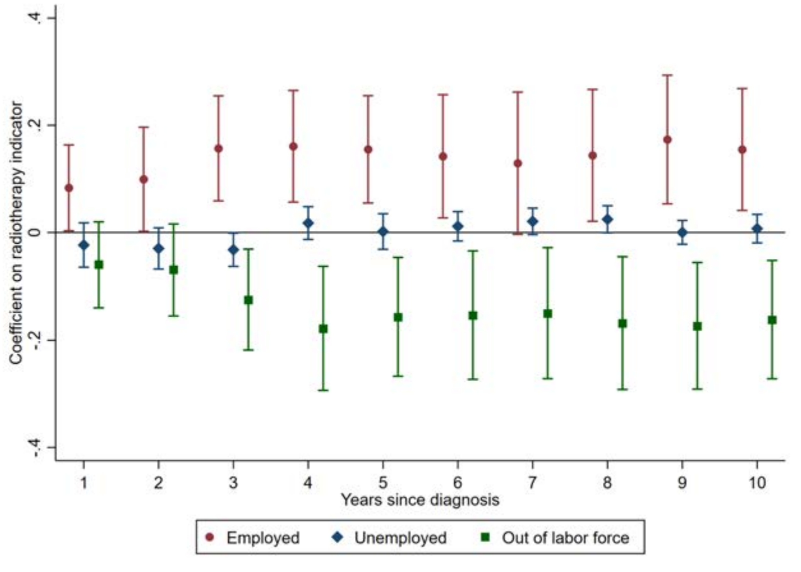
Notes: This figure presents the regression-adjusted probability of receiving radiation therapy by year of diagnosis. We regress the indicator for receipt of radiation therapy on the characteristics that determine radiotherapy eligibility, separately for $T95$ and the comparison group. Each dot plots the average of the residuals from these regressions for women diagnosed in the year indicated on the horizontal axis. The solid line represents the women in $T95$ while the dashed line represents the women in the comparison group.

Figure 1: The Effect of the 1995 Guideline Change on Radiation Therapy Take-Up

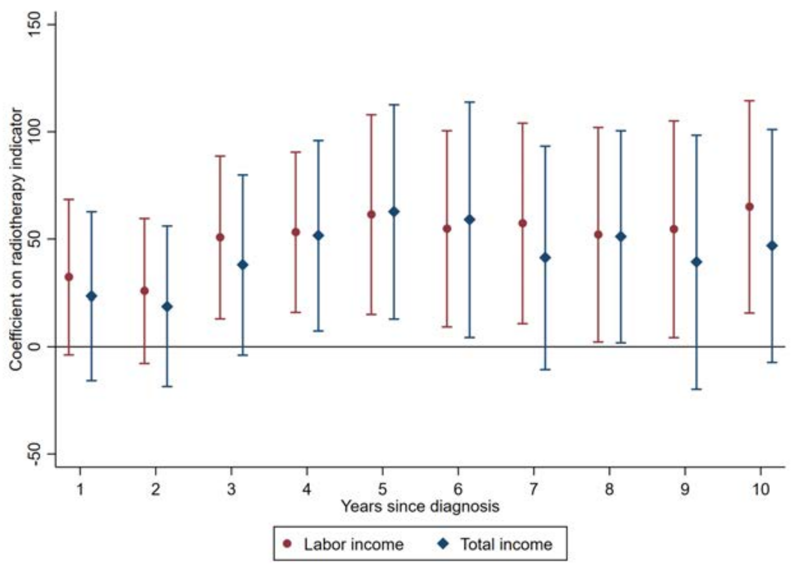


Notes: Each point and vertical segment represent the 2SLS estimate and its corresponding 95% confidence interval for the coefficient of the radiotherapy indicator from a different regression based on Equation (1) for the outcome indicated, measured at the time shown on the horizontal axis.

Figure 2: Effects of Radiation Therapy on Mortality, 2SLS Estimates



(a) Employment Status



(b) Income

Notes: Each point and vertical segment represent the 2SLS estimate and its corresponding 95% confidence interval for the coefficient of the radiotherapy indicator from a different regression based on Equation (1) for the outcome indicated, measured at the time shown on the horizontal axis.

Figure 3: Effects of Radiation Therapy on Labor Market Outcomes, 2SLS Estimates

Table 1: Descriptive Statistics

Variable name	All (1)	RT (2)	No RT (3)	p-value (4)
Panel A: Demographic Characteristics				
Age at diagnosis	43.52	42.90	44.18	0.000
	(5.64)	(5.91)	(5.25)	
Years of education	12.87	13.04	12.70	0.003
	(2.99)	(2.88)	(3.09)	
Married?	0.70	0.66	0.73	0.000
Immigrant?	0.04	0.04	0.04	0.971
Characteristics 2-4 years pre-diagnosis				
Employed?	0.84	0.84	0.84	0.862
Unemployed?	0.06	0.06	0.06	0.862
Out of the labor force?	0.10	0.10	0.10	0.909
Labor earnings (thousands)	220.39	224.90	215.58	0.080
	(141.51)	(145.99)	(136.45)	
Gross personal income (thousands)	278.43	285.63	270.74	0.001
	(122.64)	(125.86)	(118.67)	
Panel B: Disease Pathology				
Tumor size in mm	26.04	27.63	24.34	0.000
≤ 20mm?	0.49	0.46	0.52	0.000
21–50mm?	0.44	0.45	0.43	0.249
≥ 51mm?	0.07	0.10	0.05	0.000
Number of positive nodes	2.61	3.64	1.51	0.000
Zero?	0.39	0.30	0.50	0.000
1–3?	0.34	0.31	0.38	0.000
4+?	0.26	0.39	0.12	0.000
Carcinoma not removed micro-radically?	0.04	0.06	0.01	0.000
Had mastectomy?	0.82	0.69	0.96	0.000
Had lumpectomy?	0.15	0.29	0.01	0.000
Had lumpectomy followed by mastectomy?	0.03	0.03	0.03	0.859
Panel C: Health Outcomes				
Died:				
1 year after diagnosis?	0.01	0.02	0.01	0.002
5 years after diagnosis?	0.20	0.22	0.18	0.014
10 years after diagnosis?	0.32	0.35	0.30	0.006
Died of breast cancer:				
5 years after diagnosis?	0.19	0.21	0.17	0.005
10 years after diagnosis?	0.30	0.33	0.28	0.002

Table 1 (cont.): Descriptive Statistics

Variable name	All (1)	RT (2)	No RT (3)	*p*-value (4)
Panel D: Labor Market Outcomes				
Employed:				
5 years after diagnosis?	0.56	0.55	0.57	0.340
10 years after diagnosis?	0.43	0.43	0.42	0.778
Unemployed:				
5 years after diagnosis?	0.02	0.02	0.02	0.872
10 years after diagnosis?	0.02	0.01	0.02	0.071
Out of the labor force:				
5 years after diagnosis?	0.42	0.42	0.41	0.311
10 years after diagnosis?	0.56	0.56	0.55	0.843
Labor earnings (thousands):				
5 years after diagnosis?	174.01	178.65	169.04	0.143
	(174.39)	(178.07)	(170.28)	
10 years after diagnosis?	147.85	151.73	143.70	0.243
	(183.12)	(188.50)	(177.16)	
Gross personal income (thousands):				
5 years after diagnosis?	233.37	235.10	231.52	0.578
	(171.22)	(178.49)	(163.14)	
10 years after diagnosis?	202.35	200.39	204.45	0.559
	(185.06)	(189.80)	(179.89)	
Any government transfer:				
1–5 years after diagnosis?	0.48	0.55	0.41	0.000
6–10 years after diagnosis?	0.35	0.38	0.33	0.006
Sickness benefits:				
1–5 years after diagnosis?	0.35	0.41	0.29	0.000
6–10 years after diagnosis?	0.24	0.25	0.23	0.323
Disability benefits:				
1–5 years after diagnosis?	0.16	0.19	0.13	0.000
6–10 years after diagnosis?	0.15	0.17	0.13	0.006
Number of Observations	2,823	1,459	1,364	

Notes: Columns 1–3 present the mean (standard deviation in parentheses) of the characteristic indicated in the row in the sample indicated in the column. Variable names ending in a question mark are dummy variables with one being yes and zero being no, while variables with monetary values are expressed in thousands of 2015 DKK. Demographic characteristics are measured in the year of diagnosis or averaged over the period 2–4 years before diagnosis, as indicated. Column 4 presents *p*-values for the test of equality of the means between patients receiving and not receiving radiotherapy.

Table 2: Effects of Adjuvant Radiation Therapy on Government Transfers, 2SLS Estimates

	Years since diagnosis	
	1–5 (1)	6–10 (2)
Any government transfer?	−0.091**	−0.097*
	(0.037)	(0.049)
Mean outcome	0.411	0.328
Sickness benefits?	−0.078**	−0.085*
	(0.039)	(0.044)
Mean outcome	0.285	0.230
Disability pension?	−0.030	−0.049
	(0.033)	(0.035)
Mean outcome	0.125	0.128
Number of weeks with any government transfer	−4.837	−9.225
	(6.424)	(7.950)
Mean outcome	28.181	31.187
Number of weeks on sickness benefits	−3.463	−3.449*
	(2.520)	(2.041)
Mean outcome	8.443	9.558
Number of weeks on disability pension	−2.831	−5.554
	(6.195)	(8.041)
Mean outcome	21.961	31.596

Notes: 2SLS estimates of Equation (1), estimated in the full analysis sample (N = 2,823). Each cell presents the estimate of the coefficient on the indicator for radiotherapy from a separate regression for the outcome indicated in the row aggregated over the period indicated in the column. All specifications include all possible interactions of the characteristics that determine eligibility for radiation treatment, as well as indicators for year of diagnosis and for the type of chemotherapy treatment (trial arm). The instrument is the interaction between an indicator for the woman belonging to the $T95$ group and an indicator for being diagnosed after 1995. Variable names ending in a question mark are dummy variables with one being yes and zero being no. The reported mean of the outcome is calculated among women who do not receive radiotherapy. Standard errors are clustered at the hospital level. Significance levels: * p<0.1 ** p<0.05 *** p<0.01.

Do Medical Treatments Work for Work? Evidence from Breast Cancer Patients

Online Appendix

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A1 Additional Sources of Income Insurance Against Health Shocks

Unemployment Insurance. In Denmark, employers have the right to terminate the employment of workers who have been on sick leave for extended periods of time, typically a total of 120 days during a period of 12 months. Unlike other forms of social security, unemployment insurance is not automatic. Instead, individuals must apply to become members of an unemployment fund (A-kasse). During our study period around 78% of individuals were members of an A-kasse.

Members of an A-kasse are entitled to unemployment benefits if they are employed for at least 52 weeks during the previous 3 years. During our study period, members could receive benefits for a maximum period of 7 years (until 1993) or 5 years (after 1994). Benefit amounts were calculated as 90% of the earnings in the year before the job loss with a maximum weekly amount of DKK 2,615 (559 USD) in 1996.

Social Assistance. Benefits provided by the social assistance program are means-tested and also depend on age and marital status. During our study period, social assistance benefits typically amounted to 60 to 80% of the maximum level of unemployment benefits (Pedersen and Larsen, 2008). In contrast to unemployment benefits, individuals could receive social assistance benefits for an unlimited period of time.

Early Retirement. Individuals who are members of an unemployment insurance fund and have been so for a sufficiently long period of time are eligible for early retirement before the full retirement age. The full retirement age for individuals born before July 1, 1939 is 67. Individuals born after that date are eligible for retirement at age 65. Both groups are eligible for early retirement at age 60. Individuals transition into the old-age pension program at the full-retirement age. The early retirement pension was reformed several times during the course of our study. The benefit levels typically equaled 80-100% of the maximum unemployment insurance benefit level, depending on the age of entry into early retirement (Bingley et al., 2012).

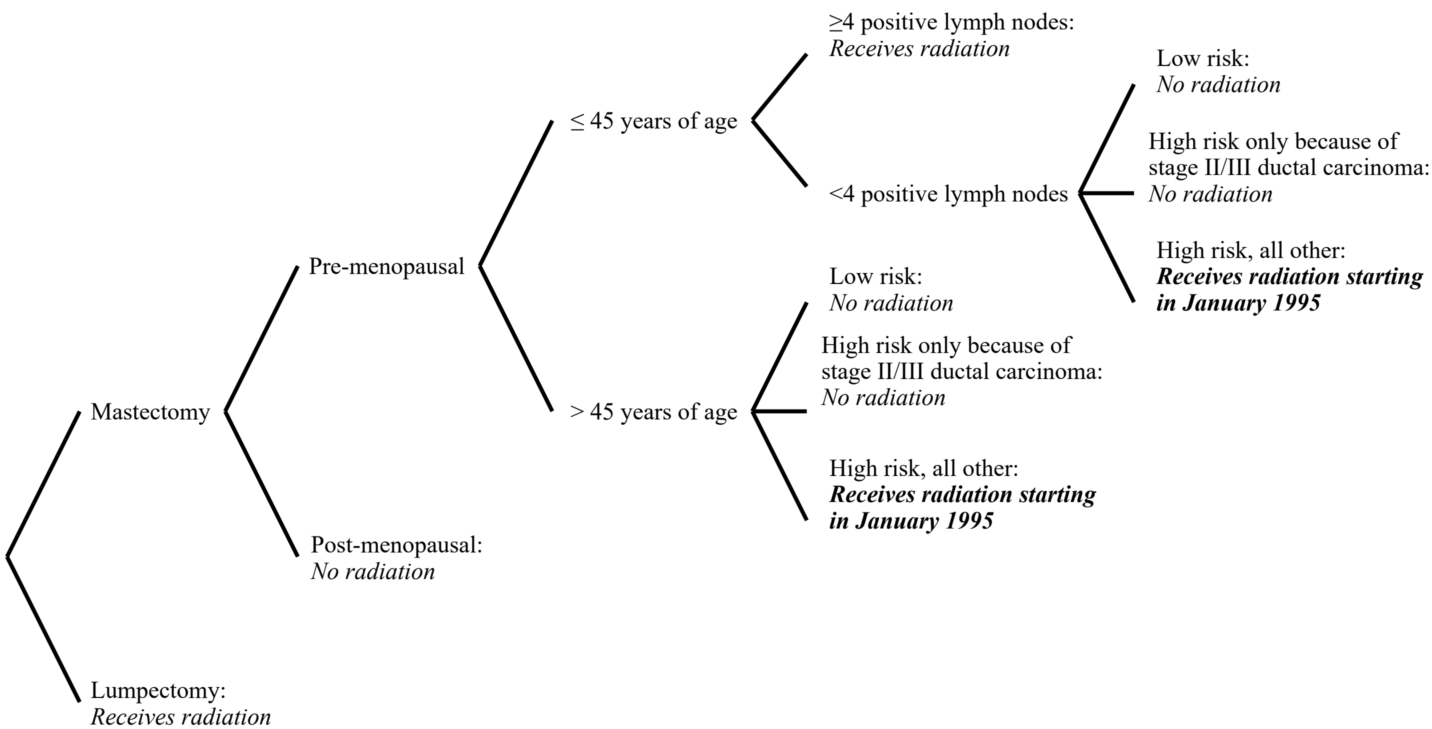
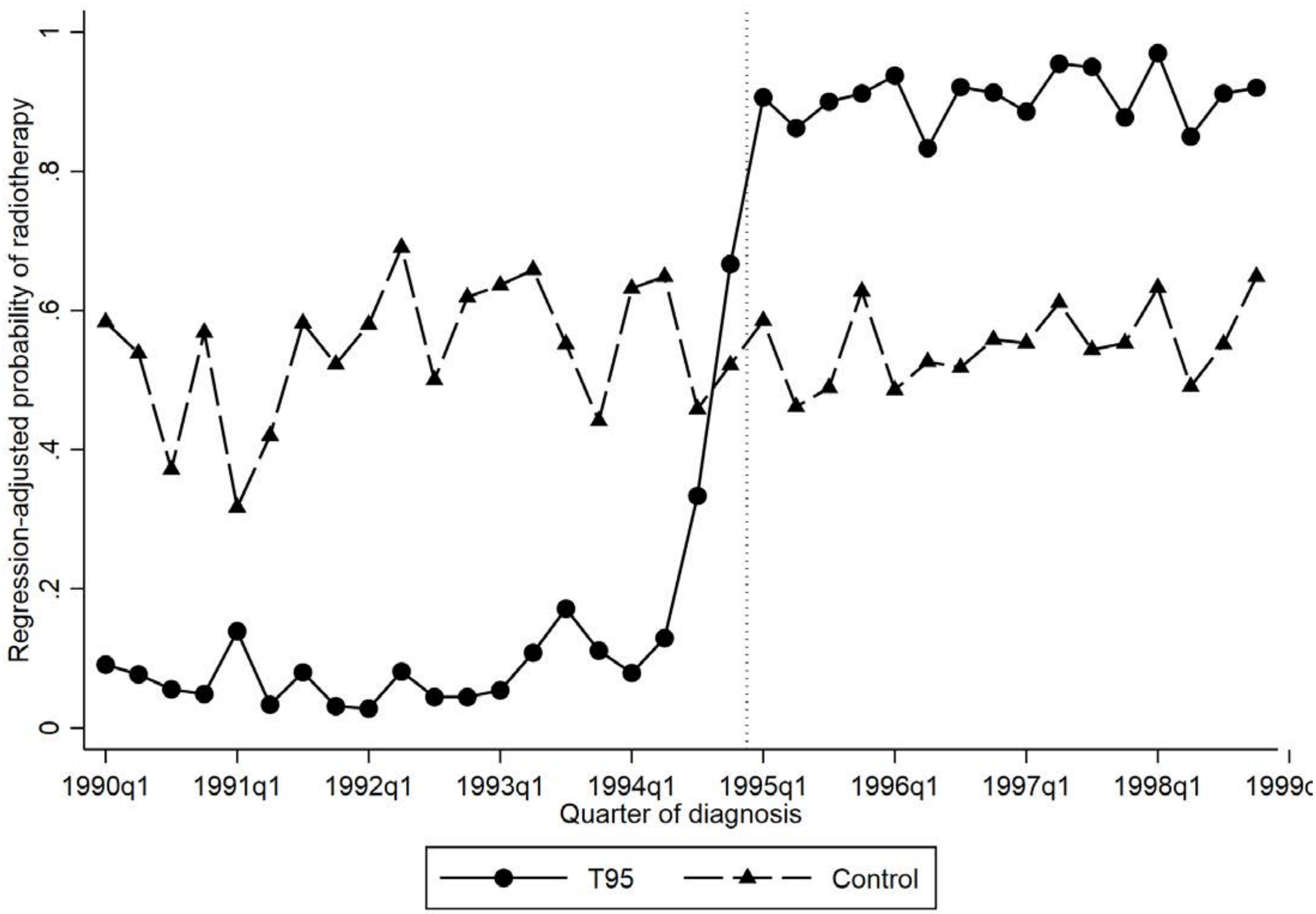
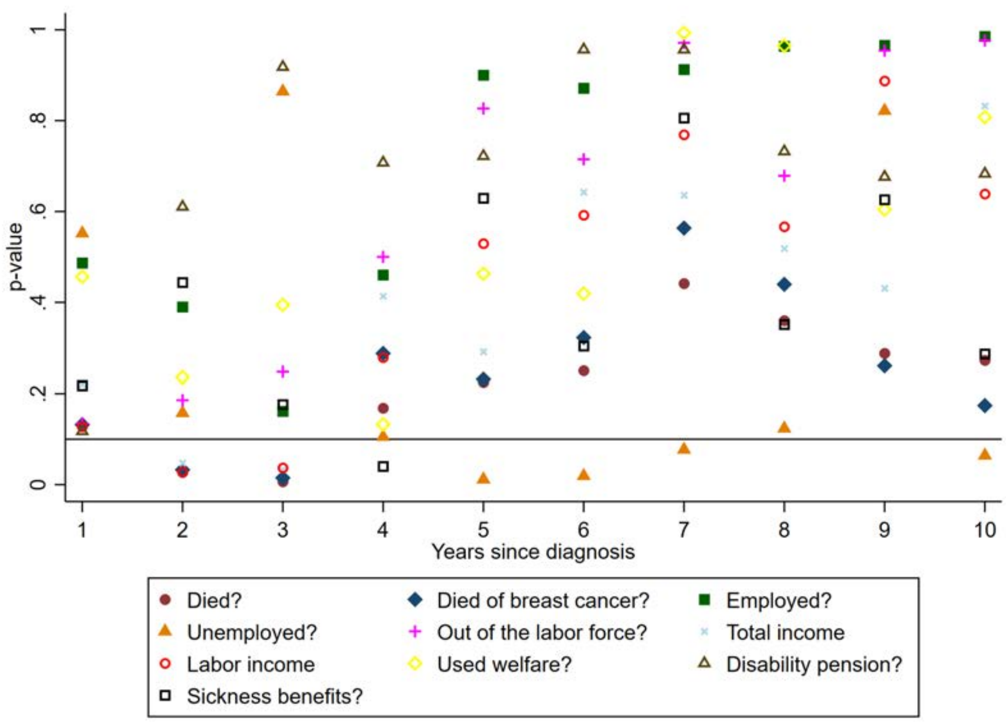


Figure A1: Eligibility for Radiation Therapy, 1990-1998



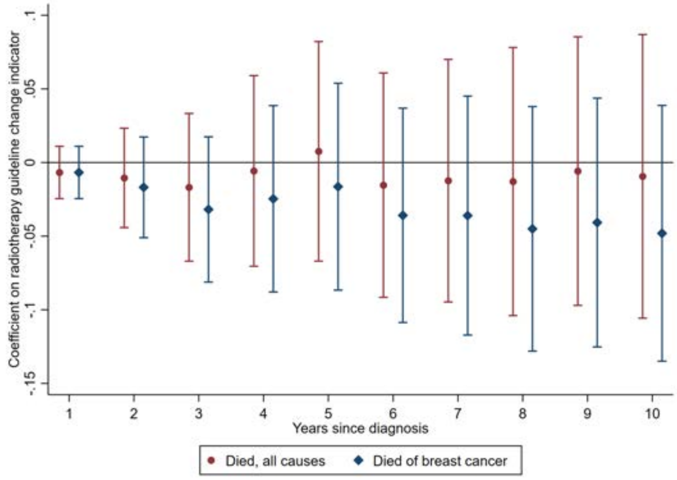
Notes: This figure presents the regression-adjusted probability of receiving radiation therapy by year and quarter of diagnosis. We regress the indicator for receipt of radiation therapy on the characteristics that determine radiotherapy eligibility, separately for $T95$ and the comparison group. Each dot plots the average of the residuals from these regressions for women diagnosed in the year and quarter indicated on the horizontal axis. The solid line represents the women in $T95$ while the dashed line represents the women in the comparison group.

Figure A2: The Effect of the 1995 Guideline Change on Radiation Therapy Take-Up, Quarterly Data

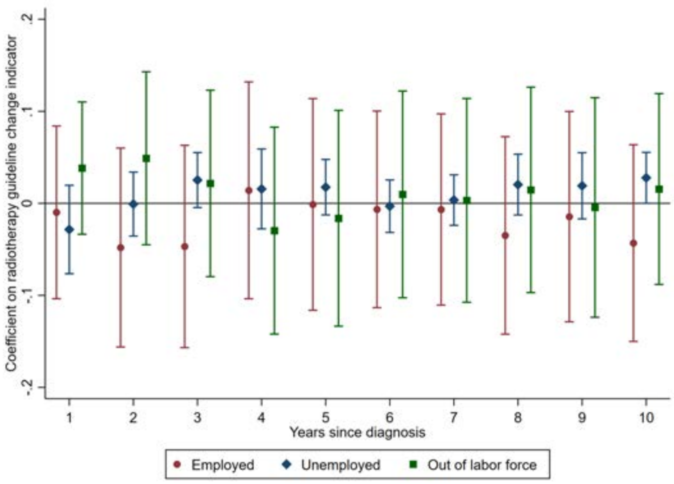


Notes: Each marker represents the *p*-value for a test of joint significance of the interaction terms between the *T*95 indicator and the year of diagnosis indicators for the period 1990–1993 (reference year = 1994) from a specification based on Equation (4), with the outcome indicated measured at the time shown on the horizontal axis.

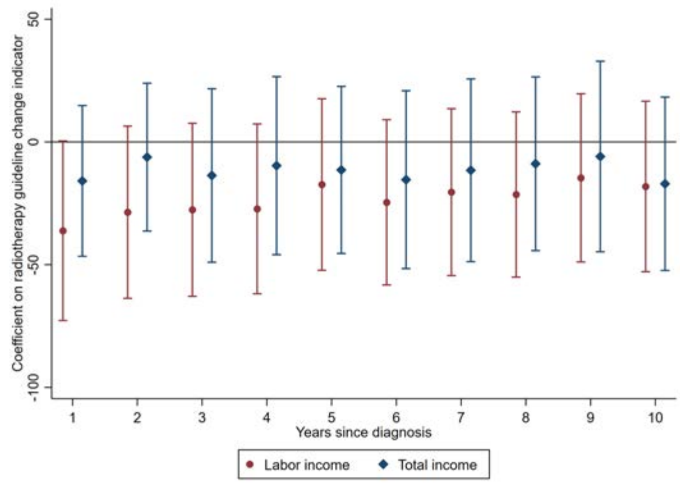
Figure A3: Distribution of *p*-values Corresponding to Tests of Parallel Trends, All Outcomes



(a) Mortality



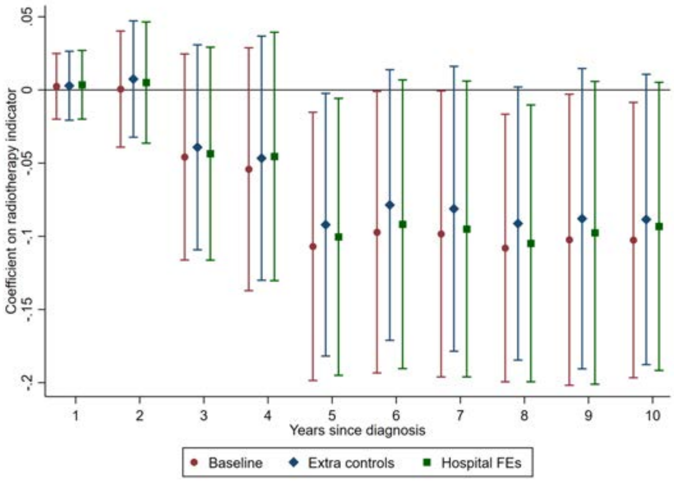
(b) Employment Status



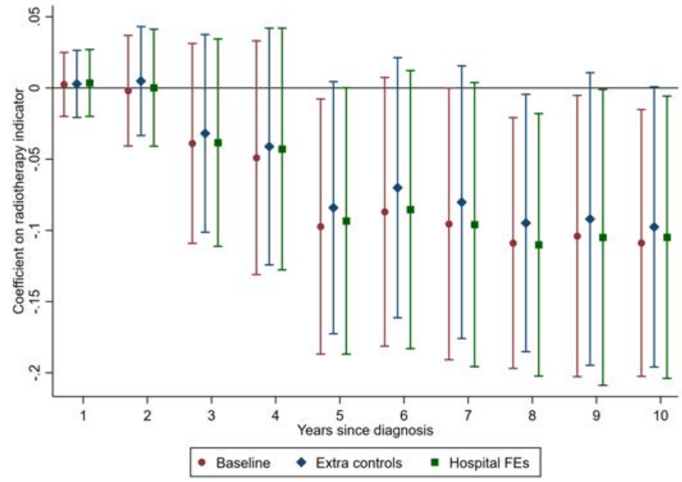
(c) Income

Notes: Each point and vertical segment represent the 2SLS estimate of Equation (1) and its corresponding 95% confidence interval for the coefficient of the radiotherapy indicator. We estimate separate 2SLS models for the outcome indicated, measured at the time shown on the horizontal axis.

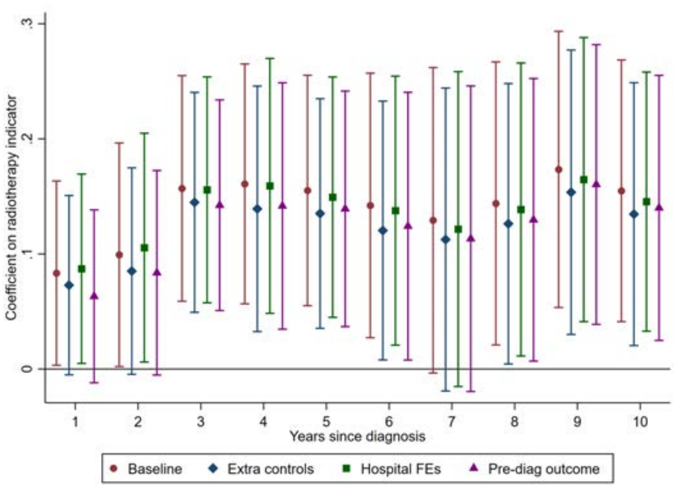
Figure A4: Placebo Effects of Radiation Therapy Guideline Change Among the Control Group



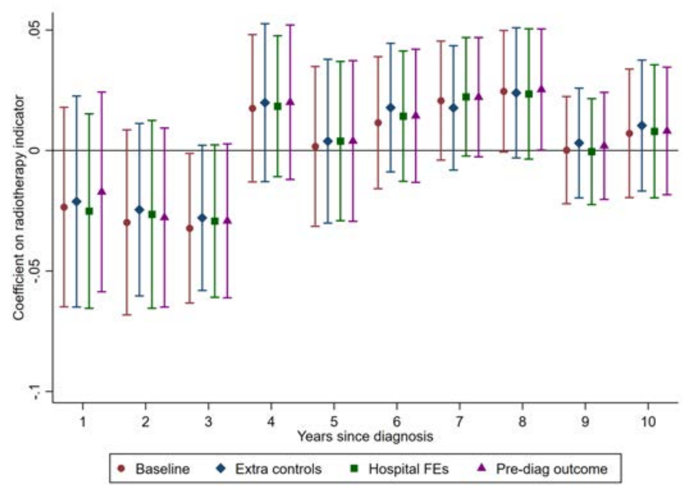
(a) Died of any cause



(b) Died of breast cancer



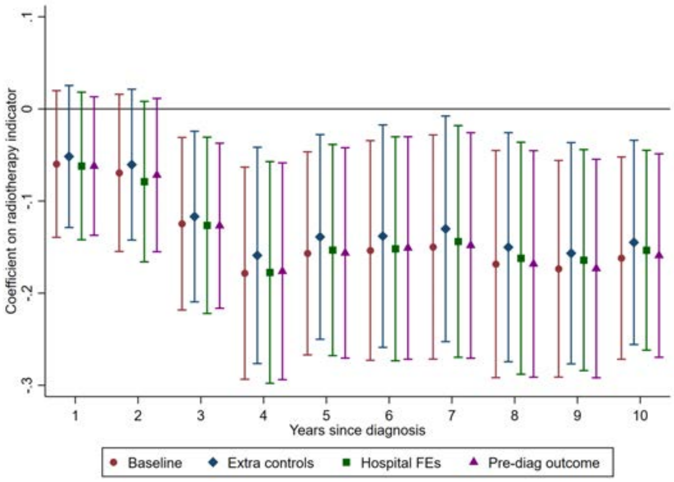
(c) Employed



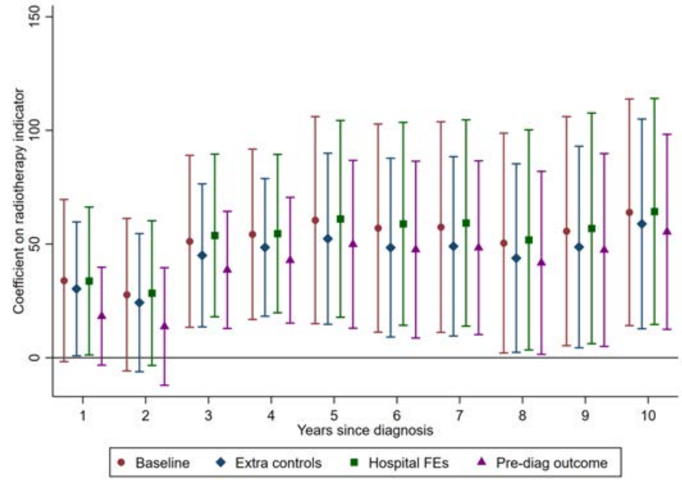
(d) Unemployed

Notes: Each point and vertical segment represent the 2SLS estimate of Equation (1) and its corresponding 95% confidence interval for the coefficient of the radiotherapy indicator. We estimate separate 2SLS models for the outcome indicated, measured at the time shown on the horizontal axis. "Baseline" represents our baseline specification, Equation (1). "Extra controls," "Hospital FEs," and "Pre-diag outcomes" indicate specifications that add to Equation (1) the demographic characteristics listed in Table 1, fixed effects for the treatment hospital, or the average of the corresponding outcome over the period 2–4 years before diagnosis, respectively.

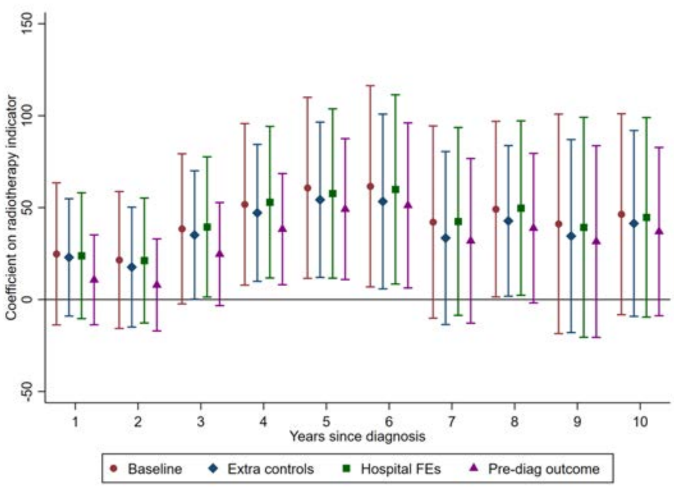
Figure A5: Robustness of Results to Model Specification



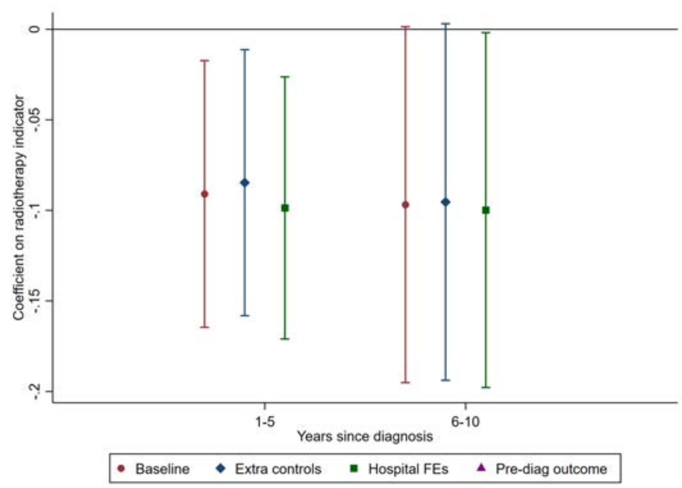
(e) Out of the labor force



(f) Labor income



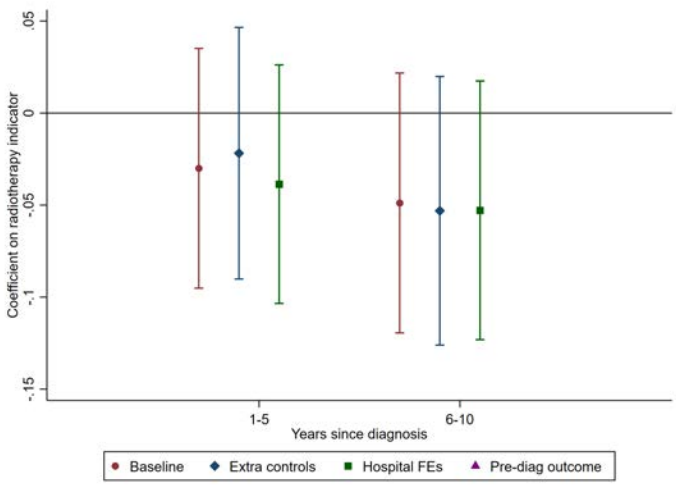
(g) Total income



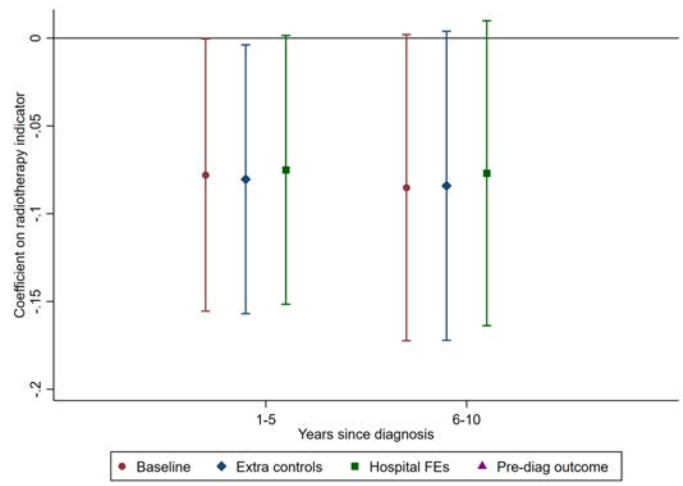
(h) Any welfare

Notes: Each point and vertical segment represent the 2SLS estimate of Equation (1) and its corresponding 95% confidence interval for the coefficient of the radiotherapy indicator. We estimate separate 2SLS models for the outcome indicated, measured at the time shown on the horizontal axis. "Baseline" represents our baseline specification, Equation (1). "Extra controls," "Hospital FEs," and "Pre-diag outcomes" indicate specifications that add to Equation (1) the demographic characteristics listed in Table 1, fixed effects for the treatment hospital, or the average of the corresponding outcome over the period 2–4 years before diagnosis, respectively.

Figure A5 (cont.): Robustness of Results to Model Specification



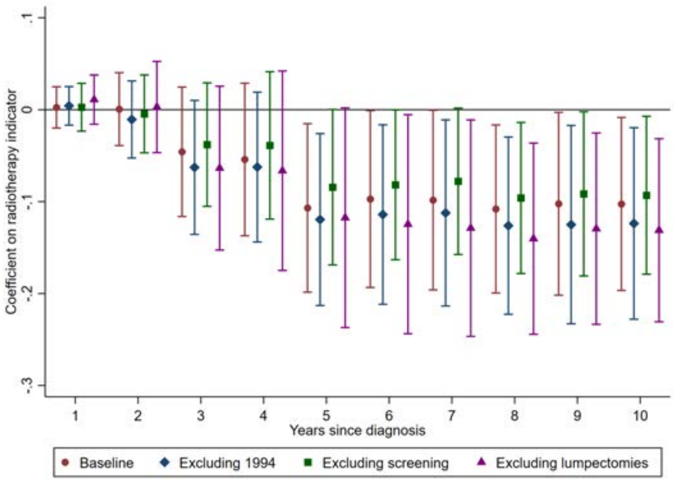
(i) Disability benefits



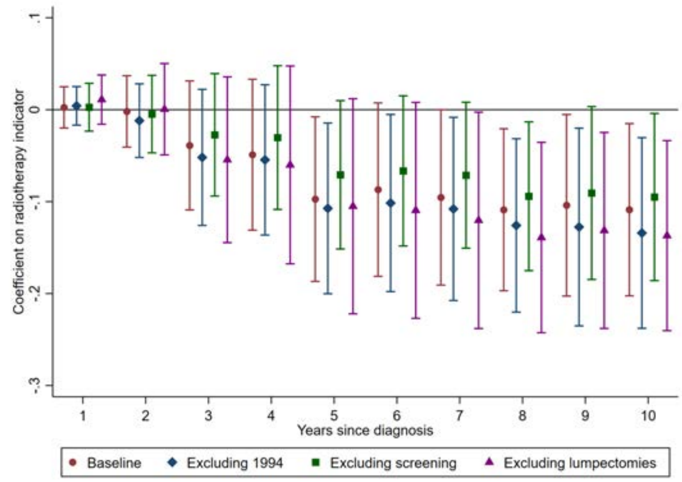
(j) Sickness benefits

Notes: Each point and vertical segment represent the 2SLS estimate of Equation (1) and its corresponding 95% confidence interval for the coefficient of the radiotherapy indicator. We estimate separate 2SLS models for the outcome indicated, measured at the time shown on the horizontal axis. "Baseline" represents our baseline specification, Equation (1). "Extra controls," "Hospital FEs," and "Pre-diag outcomes" indicate specifications that add to Equation (1) the demographic characteristics listed in Table 1, fixed effects for the treatment hospital, or the average of the corresponding outcome over the period 2–4 years before diagnosis, respectively.

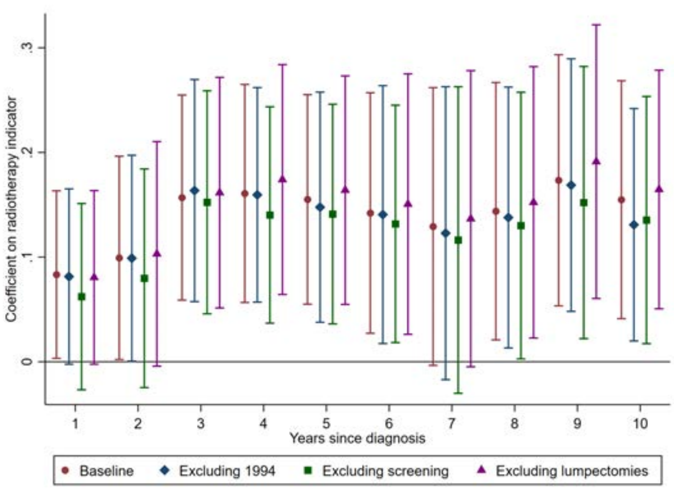
Figure A5 (cont.): Robustness of Results to Model Specification



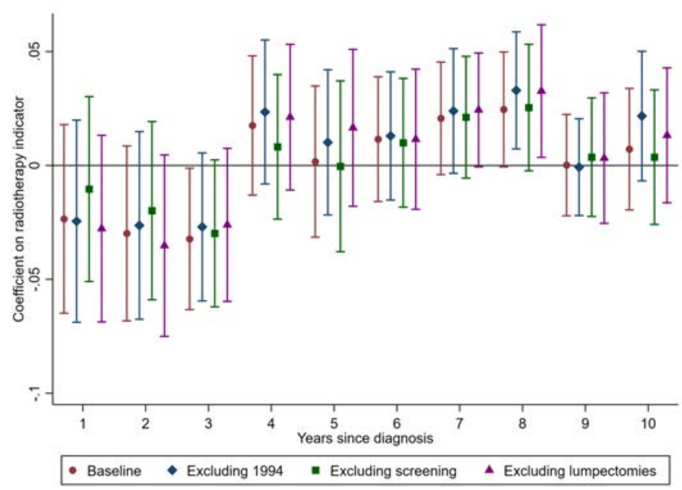
(a) Died of any cause



(b) Died of breast cancer



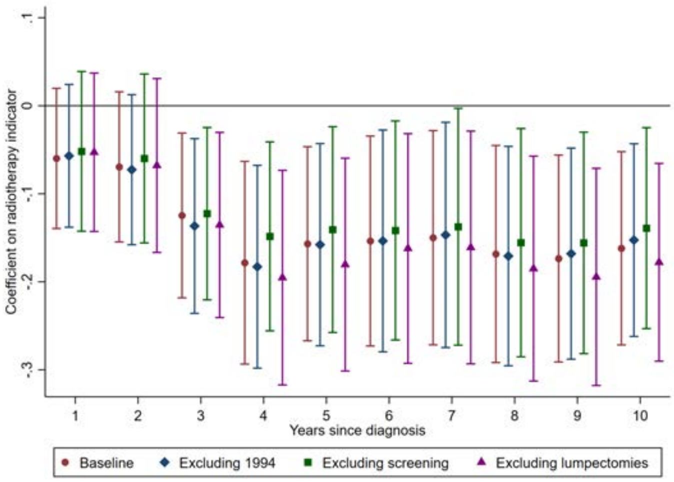
(c) Employed



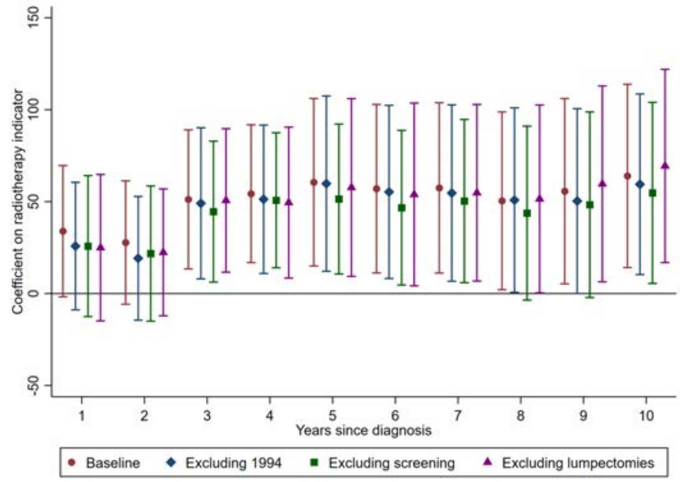
(d) Unemployed

Notes: Each point and vertical segment represent the 2SLS estimate of Equation (1) and its corresponding 95% confidence interval for the coefficient of the radiotherapy indicator. We estimate separate 2SLS models for the outcome indicated, measured at the time shown on the horizontal axis. "Baseline" represents our baseline specification, Equation (1), estimated in the full analysis sample. "Excluding 1994," "Excluding screening," and "Excluding lumpectomies" indicate that the estimation is conducted after excluding from the analysis sample women diagnosed in 1994, women residing in areas that introduced universal breast cancer screening programs during our sample period, or women whose tumors were removed through lumpectomy, respectively.

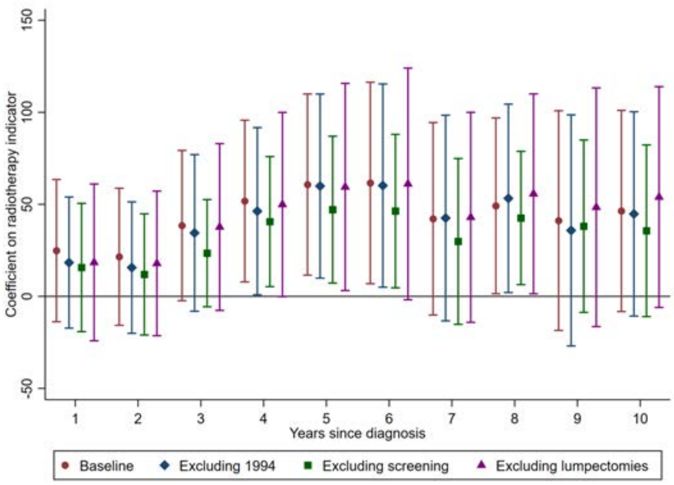
Figure A6: Robustness of Results to Sample Selection



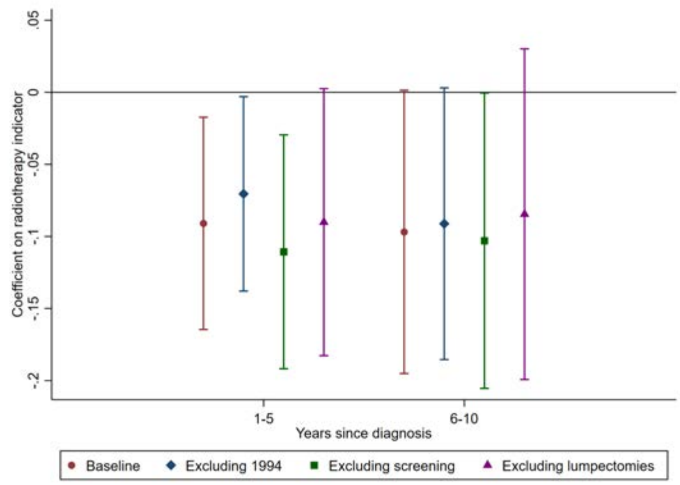
(e) Out of the labor force



(f) Labor income



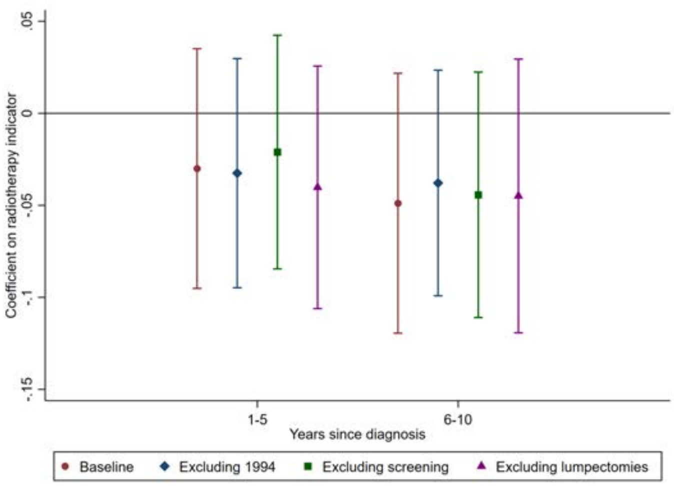
(g) Total income



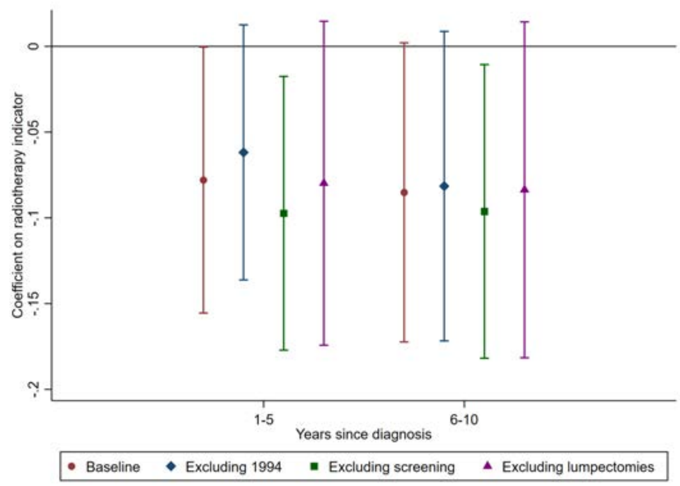
(h) Any welfare

Notes: Each point and vertical segment represent the 2SLS estimate of Equation (1) and its corresponding 95% confidence interval for the coefficient of the radiotherapy indicator. We estimate separate 2SLS models for the outcome indicated, measured at the time shown on the horizontal axis. "Baseline" represents our baseline specification, Equation (1), estimated in the full analysis sample. "Excluding 1994," "Excluding screening," and "Excluding lumpectomies" indicate that the estimation is conducted after excluding from the analysis sample women diagnosed in 1994, women residing in areas that introduced universal breast cancer screening programs during our sample period, or women whose tumors were removed through lumpectomy, respectively.

Figure A6 (cont.): Robustness of Results to Sample Selection



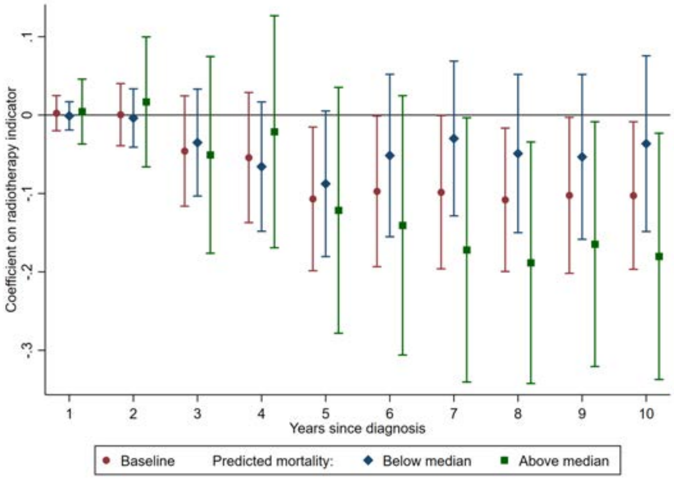
(i) Disability benefits



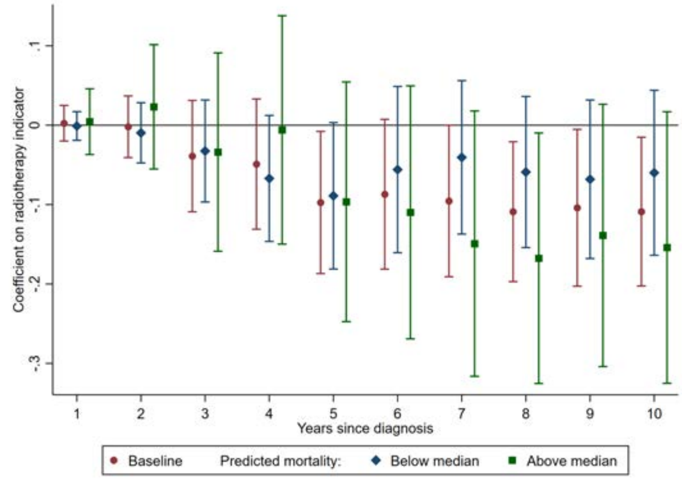
(j) Sickness benefits

Notes: Each point and vertical segment represent the 2SLS estimate of Equation (1) and its corresponding 95% confidence interval for the coefficient of the radiotherapy indicator. We estimate separate 2SLS models for the outcome indicated, measured at the time shown on the horizontal axis. "Baseline" represents our baseline specification, Equation (1), estimated in the full analysis sample. "Excluding 1994," "Excluding screening," and "Excluding lumpectomies" indicate that the estimation is conducted after excluding from the analysis sample women diagnosed in 1994, women residing in areas that introduced universal breast cancer screening programs during our sample period, or women whose tumors were removed through lumpectomy, respectively.

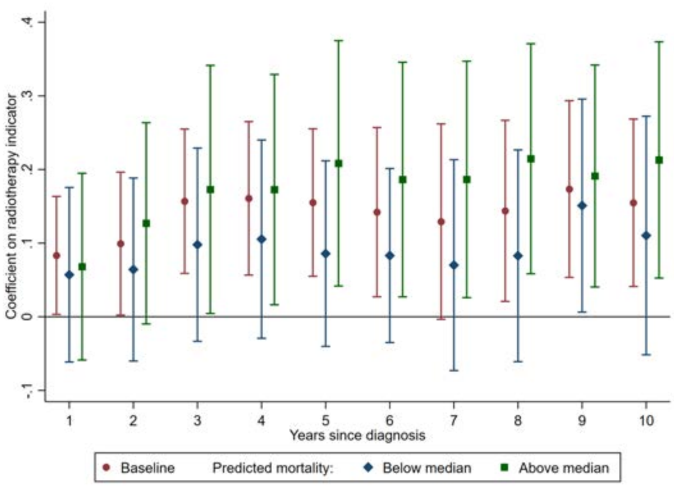
Figure A6 (cont.): Robustness of Results to Sample Selection



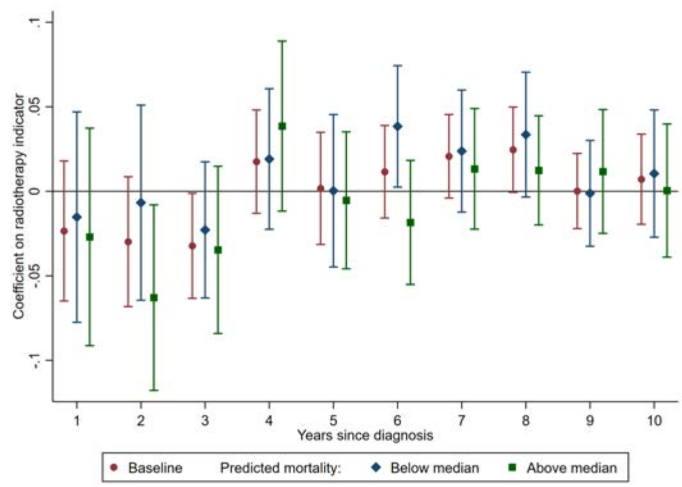
(a) Died of any cause



(b) Died of breast cancer



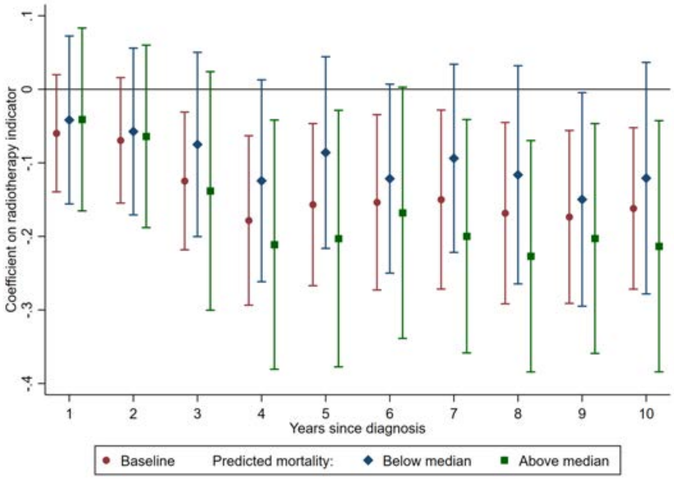
(c) Employed



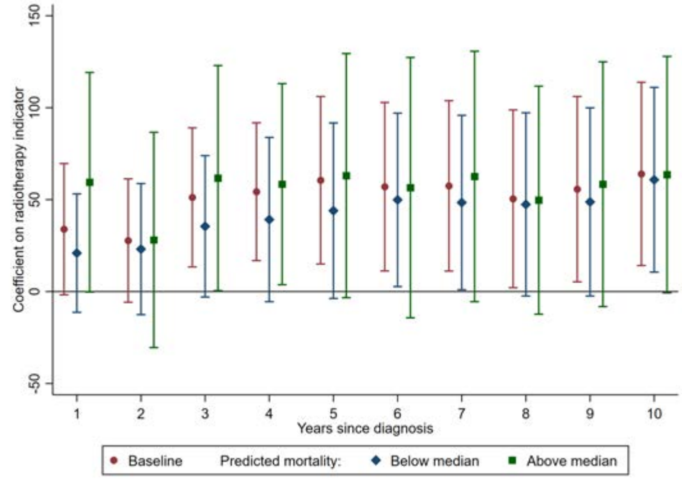
(d) Unemployed

Notes: Each point and vertical segment represent the 2SLS estimate of Equation (1) and its corresponding 95% confidence interval for the coefficient of the radiotherapy indicator. We estimate separate 2SLS models for the outcome indicated, measured at the time shown on the horizontal axis. "Baseline" represents our baseline specification, Equation (1), estimated in the full analysis sample. "Below median" and "Above median" indicate that the estimation is conducted in the subsample of women with predicted 10-year breast cancer mortality above or below the median in their year of diagnosis, respectively. Predicted 10-year breast cancer mortality is obtained by applying to our analysis sample the prediction from a probit regression of an indicator for dying from breast cancer during the ten years after diagnosis on the disease characteristics described in Section 3, estimated in the sample of all the breast cancer patients diagnosed during our sample period who are not included in our analysis sample and who are not treated with radiation therapy.

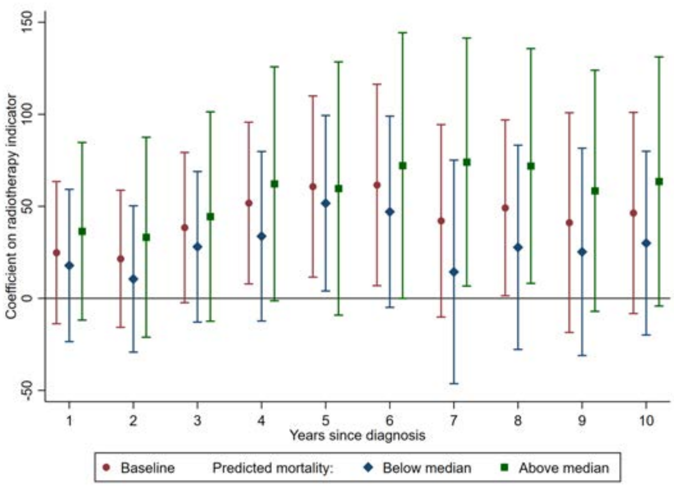
Figure A7: Heterogeneous Effects of Radiation Therapy by Predicted 10-year Mortality



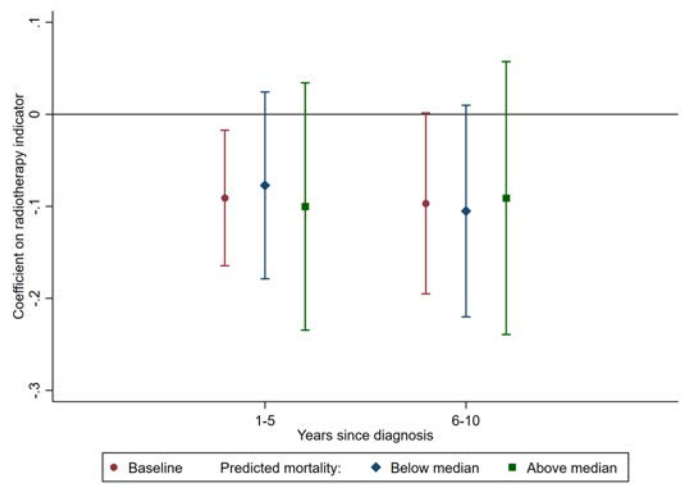
(e) Out of the labor force



(f) Labor income



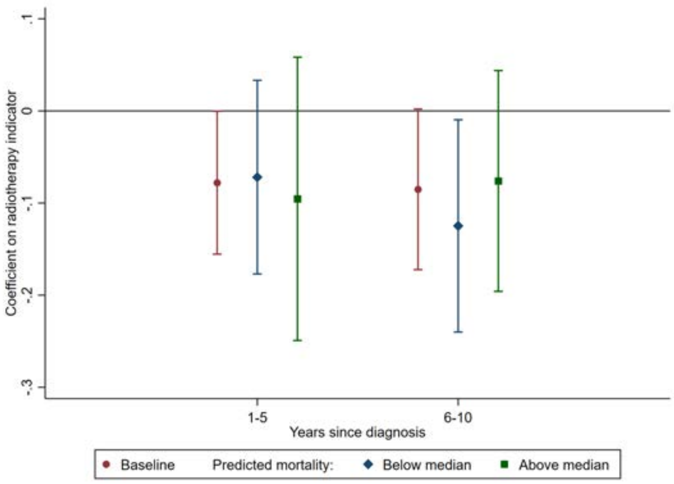
(g) Total income



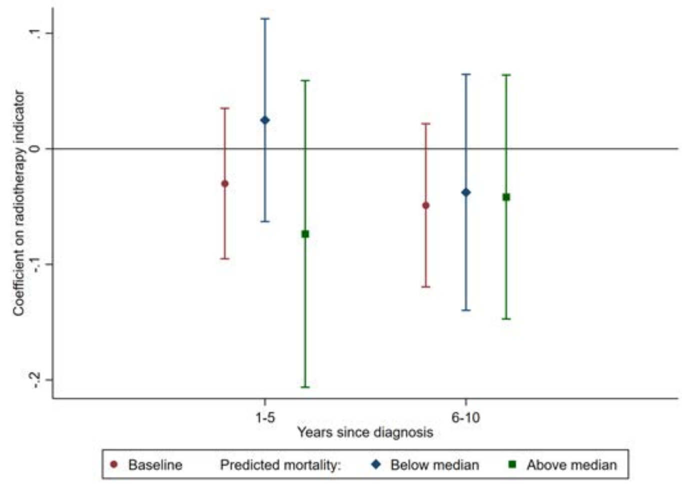
(h) Any welfare

Notes: Each point and vertical segment represent the 2SLS estimate of Equation (1) and its corresponding 95% confidence interval for the coefficient of the radiotherapy indicator. We estimate separate 2SLS models for the outcome indicated, measured at the time shown on the horizontal axis. "Baseline" represents our baseline specification, Equation (1), estimated in the full analysis sample. "Below median" and "Above median" indicate that the estimation is conducted in the subsample of women with predicted 10-year breast cancer mortality above or below the median in their year of diagnosis, respectively. Predicted 10-year breast cancer mortality is obtained by applying to our analysis sample the prediction from a probit regression of an indicator for dying from breast cancer during the ten years after diagnosis on the disease characteristics described in Section 3, estimated in the sample of all the breast cancer patients diagnosed during our sample period who are not included in our analysis sample and who are not treated with radiation therapy.

Figure A7 (cont.): Heterogeneous Effects of Radiation Therapy by Predicted 10-year Mortality



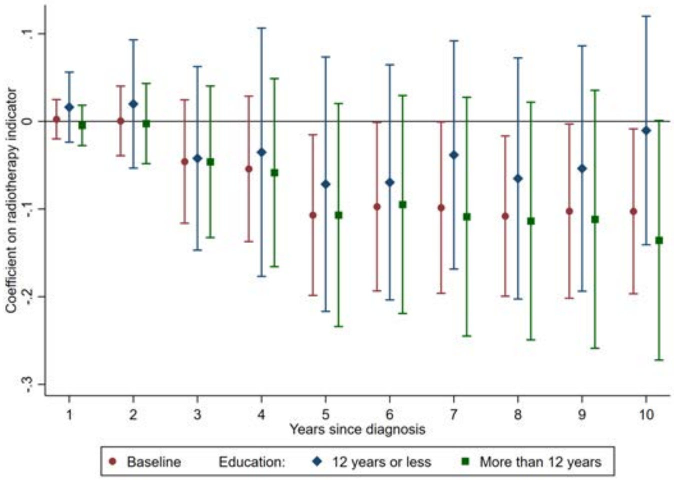
(i) Sickness benefits



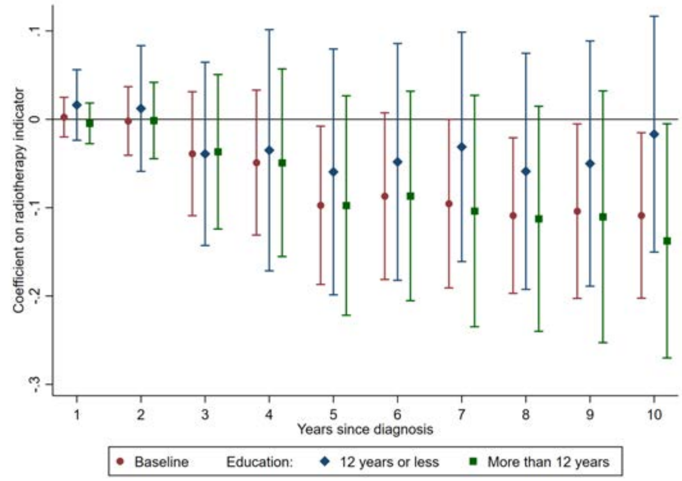
(j) Disability benefits

Notes: Each point and vertical segment represent the 2SLS estimate of Equation (1) and its corresponding 95% confidence interval for the coefficient of the radiotherapy indicator. We estimate separate 2SLS models for the outcome indicated, measured at the time shown on the horizontal axis. "Baseline" represents our baseline specification, Equation (1), estimated in the full analysis sample. "Below median" and "Above median" indicate that the estimation is conducted in the subsample of women with predicted 10-year breast cancer mortality above or below the median in their year of diagnosis, respectively. Predicted 10-year breast cancer mortality is obtained by applying to our analysis sample the prediction from a probit regression of an indicator for dying from breast cancer during the ten years after diagnosis on the disease characteristics described in Section 3, estimated in the sample of all the breast cancer patients diagnosed during our sample period who are not included in our analysis sample and who are not treated with radiation therapy.

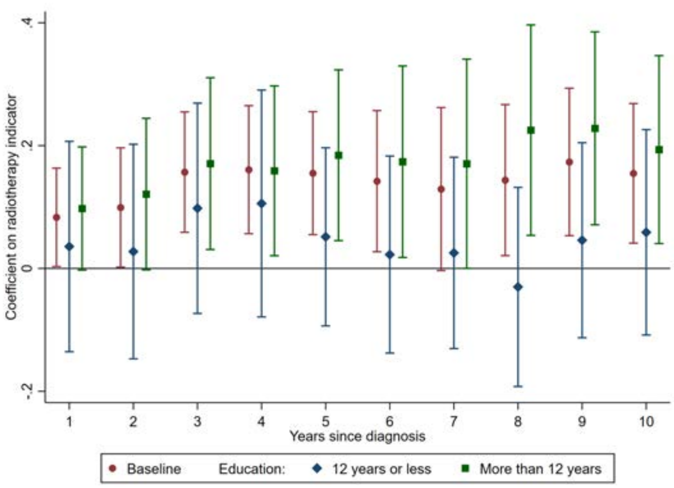
Figure A7 (cont.): Heterogeneous Effects of Radiation Therapy by Predicted 10-year Mortality



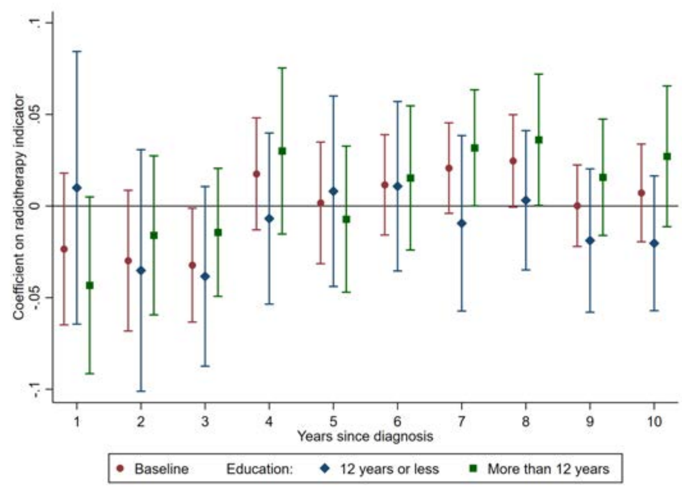
(a) Died of any cause



(b) Died of breast cancer



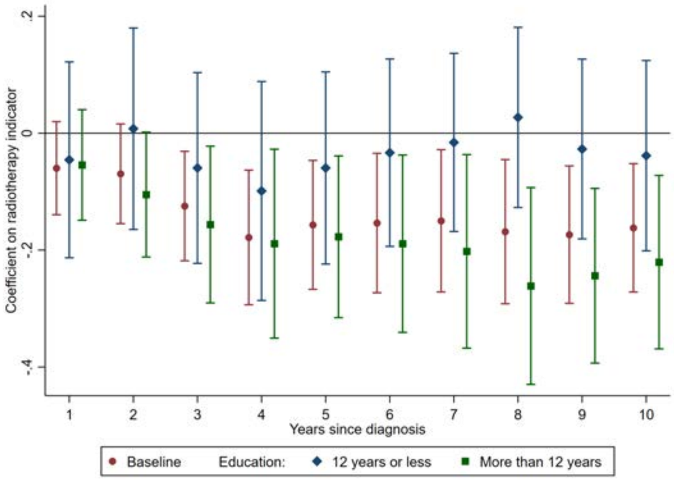
(c) Employed



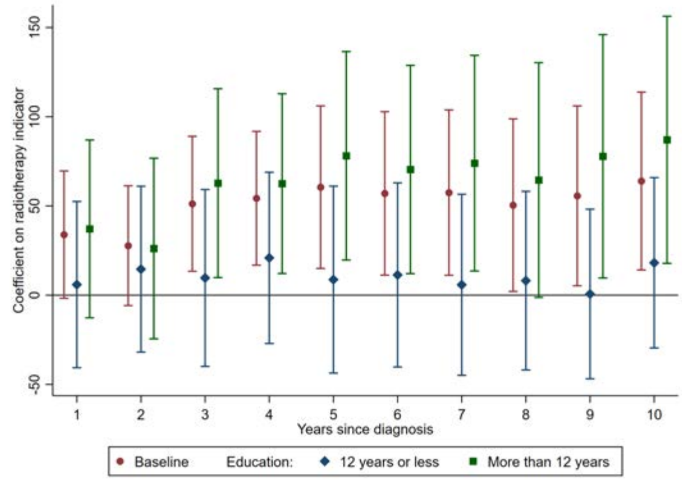
(d) Unemployed

Notes: Each point and vertical segment represent the 2SLS estimate of Equation (1) and its corresponding 95% confidence interval for the coefficient of the radiotherapy indicator. We estimate separate 2SLS models for the outcome indicated, measured at the time shown on the horizontal axis. "Baseline" represents our baseline specification, Equation (1), estimated in the full analysis sample. "12 years or less" and "More than 12 years" indicate that the estimation is conducted in the subsample of women with at most 12 years of education or with more than 12 years of education, respectively.

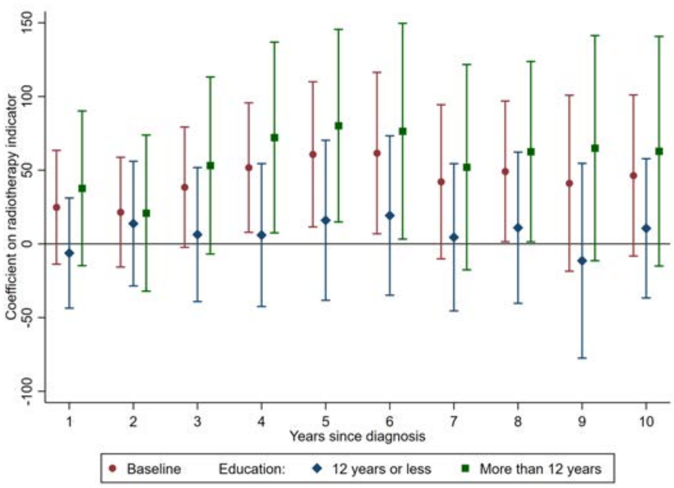
Figure A8: Heterogeneous Effects of Radiation Therapy by Patient Education



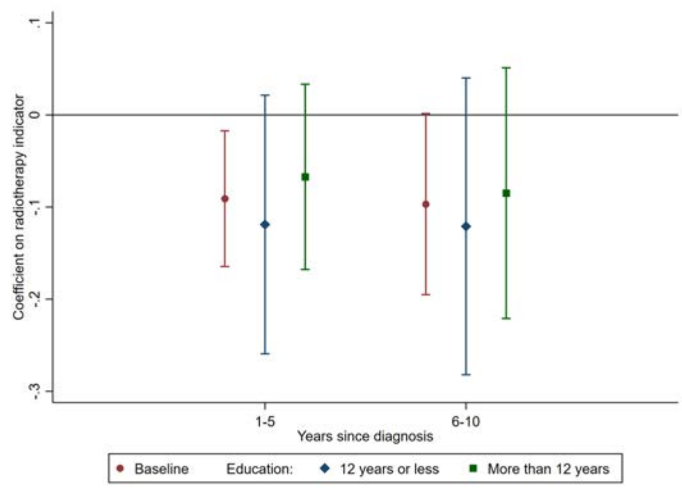
(e) Out of the labor force



(f) Labor income



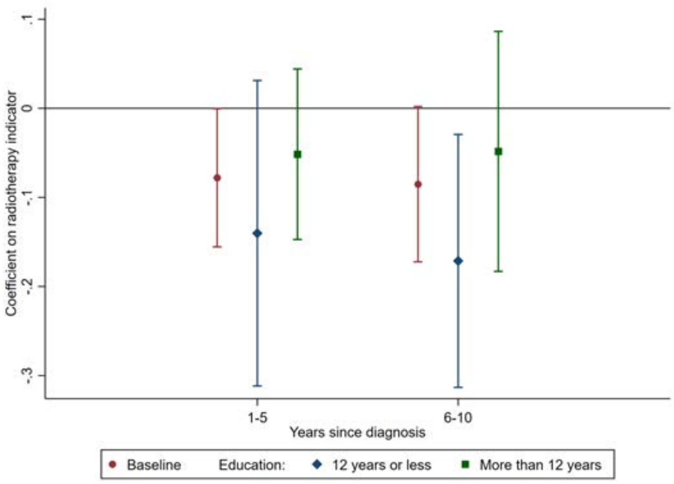
(g) Total income



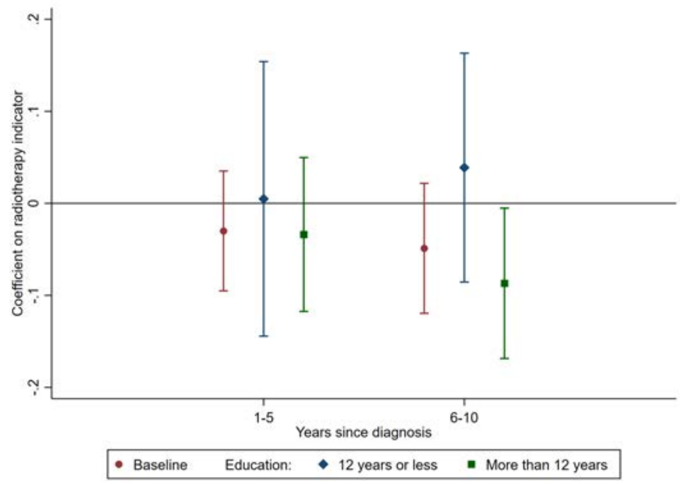
(h) Any welfare

Notes: Each point and vertical segment represent the 2SLS estimate of Equation (1) and its corresponding 95% confidence interval for the coefficient of the radiotherapy indicator. We estimate separate 2SLS models for the outcome indicated, measured at the time shown on the horizontal axis. "Baseline" represents our baseline specification, Equation (1), estimated in the full analysis sample. "12 years or less" and "More than 12 years" indicate that the estimation is conducted in the subsample of women with at most 12 years of education or with more than 12 years of education, respectively.

Figure A8 (cont.): Heterogeneous Effects of Radiation Therapy by Patient Education



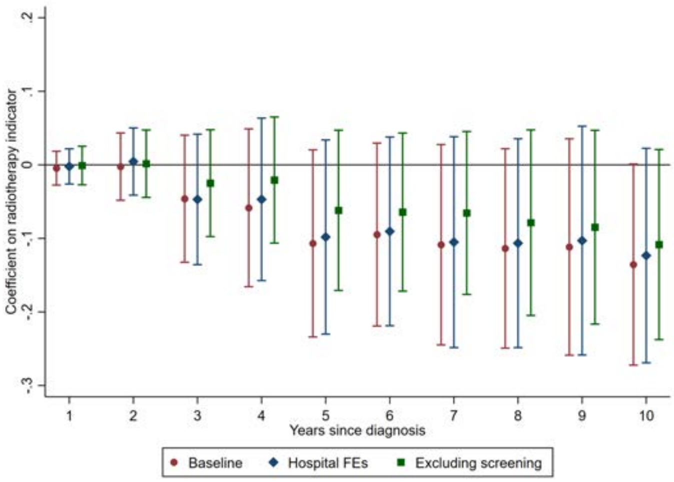
(i) Sickness benefits



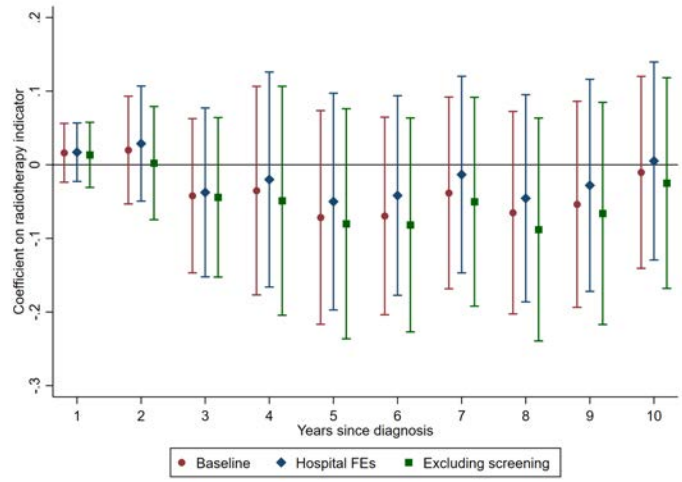
(j) Disability benefits

Notes: Each point and vertical segment represent the 2SLS estimate of Equation (1) and its corresponding 95% confidence interval for the coefficient of the radiotherapy indicator. We estimate separate 2SLS models for the outcome indicated, measured at the time shown on the horizontal axis. "Baseline" represents our baseline specification, Equation (1), estimated in the full analysis sample. "12 years or less" and "More than 12 years" indicate that the estimation is conducted in the subsample of women with at most 12 years of education or with more than 12 years of education, respectively.

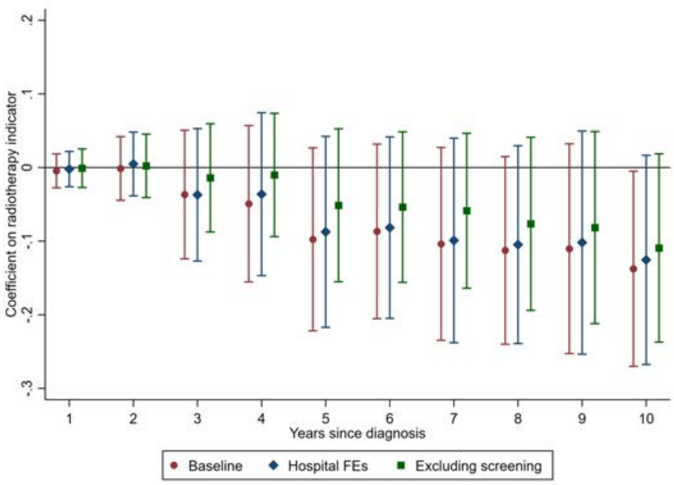
Figure A8 (cont.): Heterogeneous Effects of Radiation Therapy by Patient Education



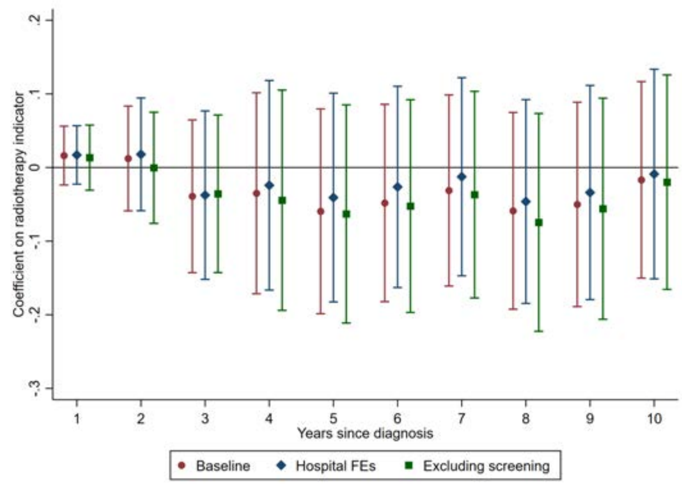
(a) Died (any cause), Highly-educated women



(b) Died (any cause), Low-educated women



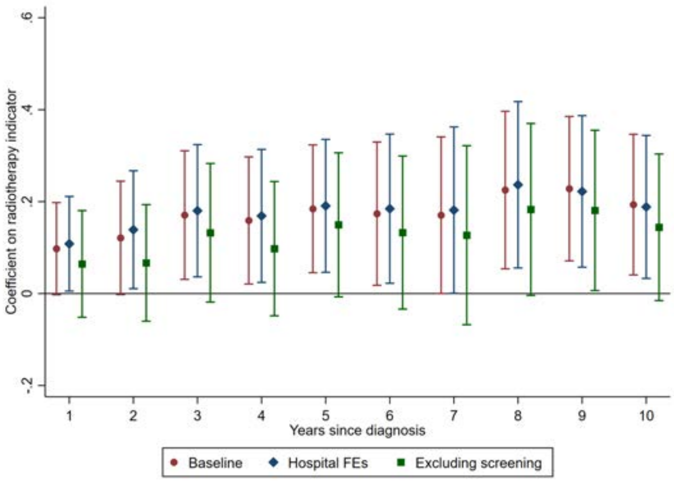
(c) Died from breast cancer, Highly-educated women



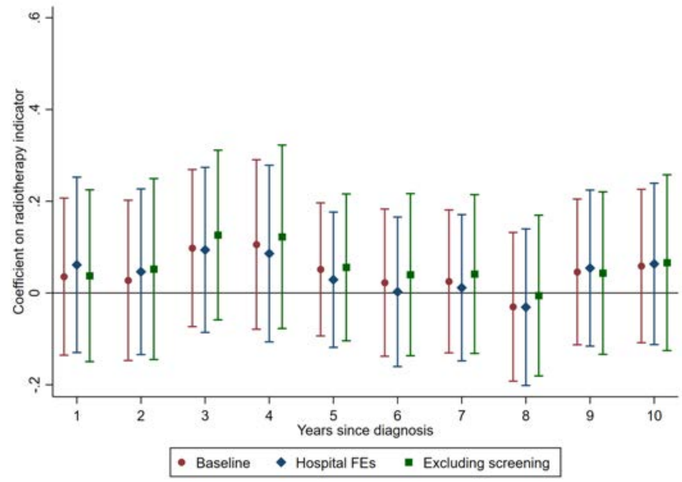
(d) Died from breast cancer, Low-educated women

Notes: Each point and vertical segment represent the 2SLS estimate of Equation (1) and its corresponding 95% confidence interval for the coefficient of the radiotherapy indicator. We estimate separate 2SLS models for the outcome indicated, measured at the time shown on the horizontal axis. "Baseline" represents our baseline specification, Equation (1), estimated in the subsample indicated in the figure caption. "Hospital FEs" indicate a specification that adds to Equation (1) fixed effects for the treatment hospital, while "Excluding screening" indicates that the estimation of Equation (1) is conducted after excluding from the subsample women residing in areas that introduced universal breast cancer screening programs during our sample period.

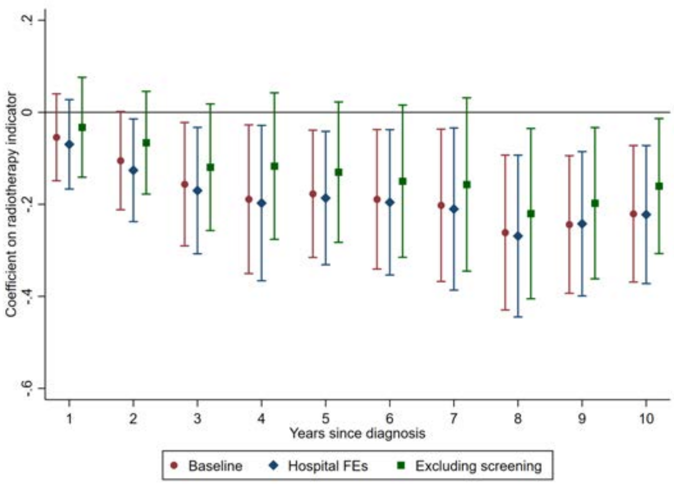
Figure A9: Robustness of Heterogeneous Effects by Education to Model Specification



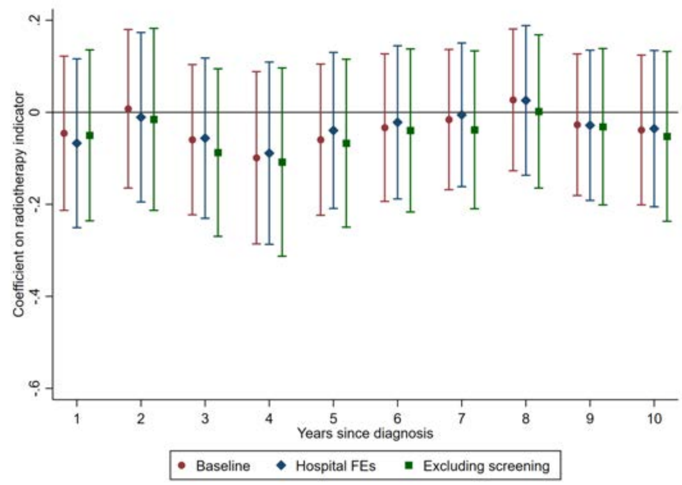
(e) Employed, Highly-educated women



(f) Employed, Low-educated women



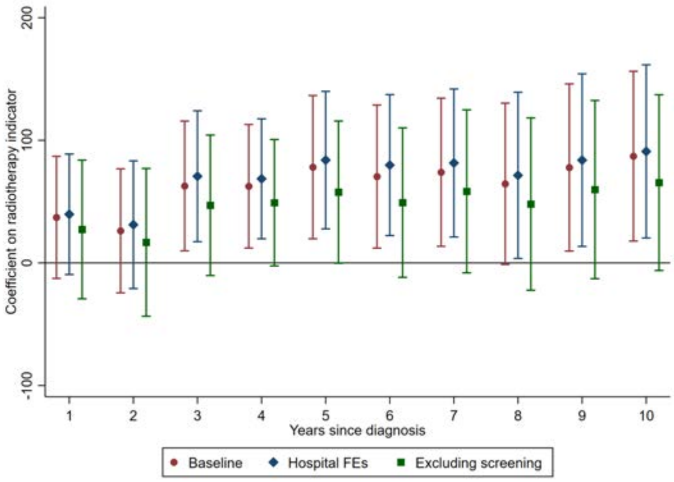
(g) Out of the labor force, Highly-educated women



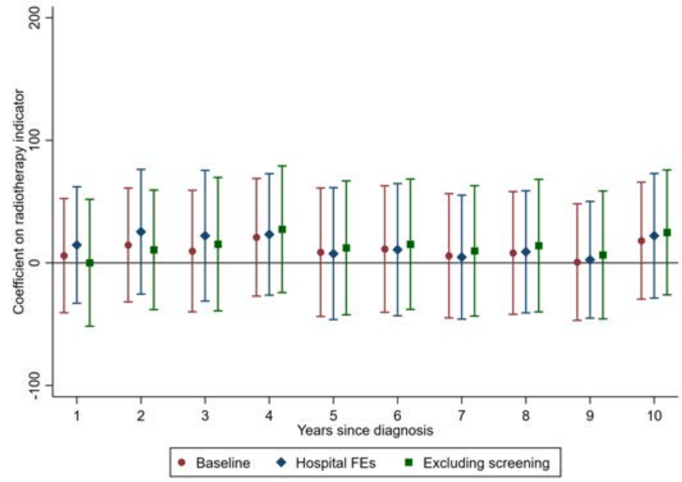
(h) Out of the labor force, Low-educated women

Notes: Each point and vertical segment represent the 2SLS estimate of Equation (1) and its corresponding 95% confidence interval for the coefficient of the radiotherapy indicator. We estimate separate 2SLS models for the outcome indicated, measured at the time shown on the horizontal axis. "Baseline" represents our baseline specification, Equation (1), estimated in the subsample indicated in the figure caption. "Hospital FEs" indicate a specification that adds to Equation (1) fixed effects for the treatment hospital, while "Excluding screening" indicates that the estimation of Equation (1) is conducted after excluding from the subsample women residing in areas that introduced universal breast cancer screening programs during our sample period.

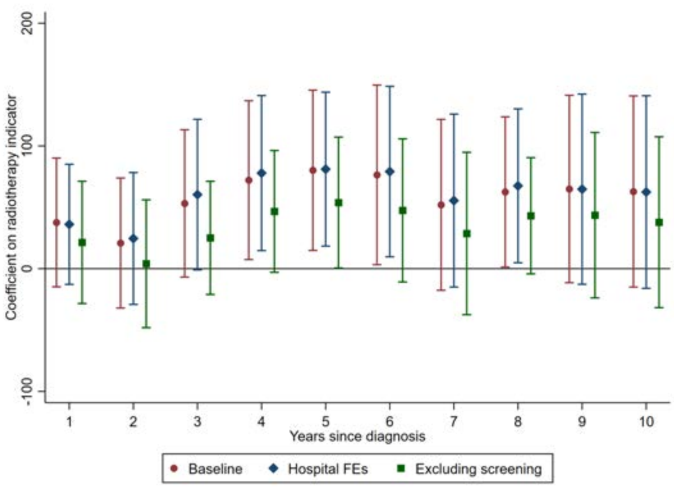
Figure A9 (cont.): Robustness of Heterogeneous Effects by Education to Model Specification



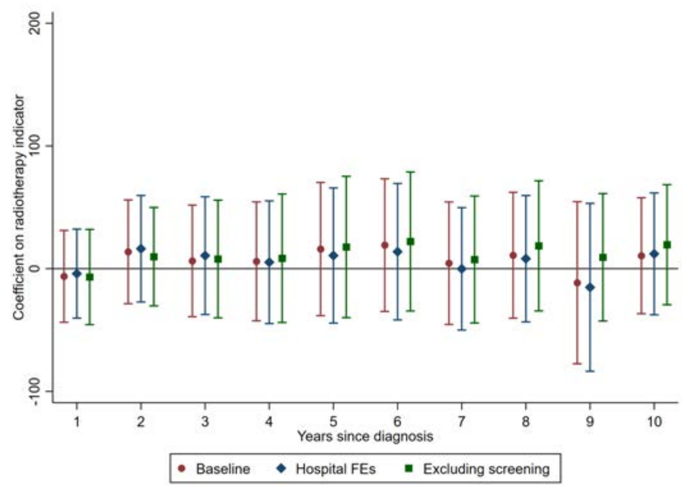
(i) Labor income, Highly-educated women



(j) Labor income, Low-educated women



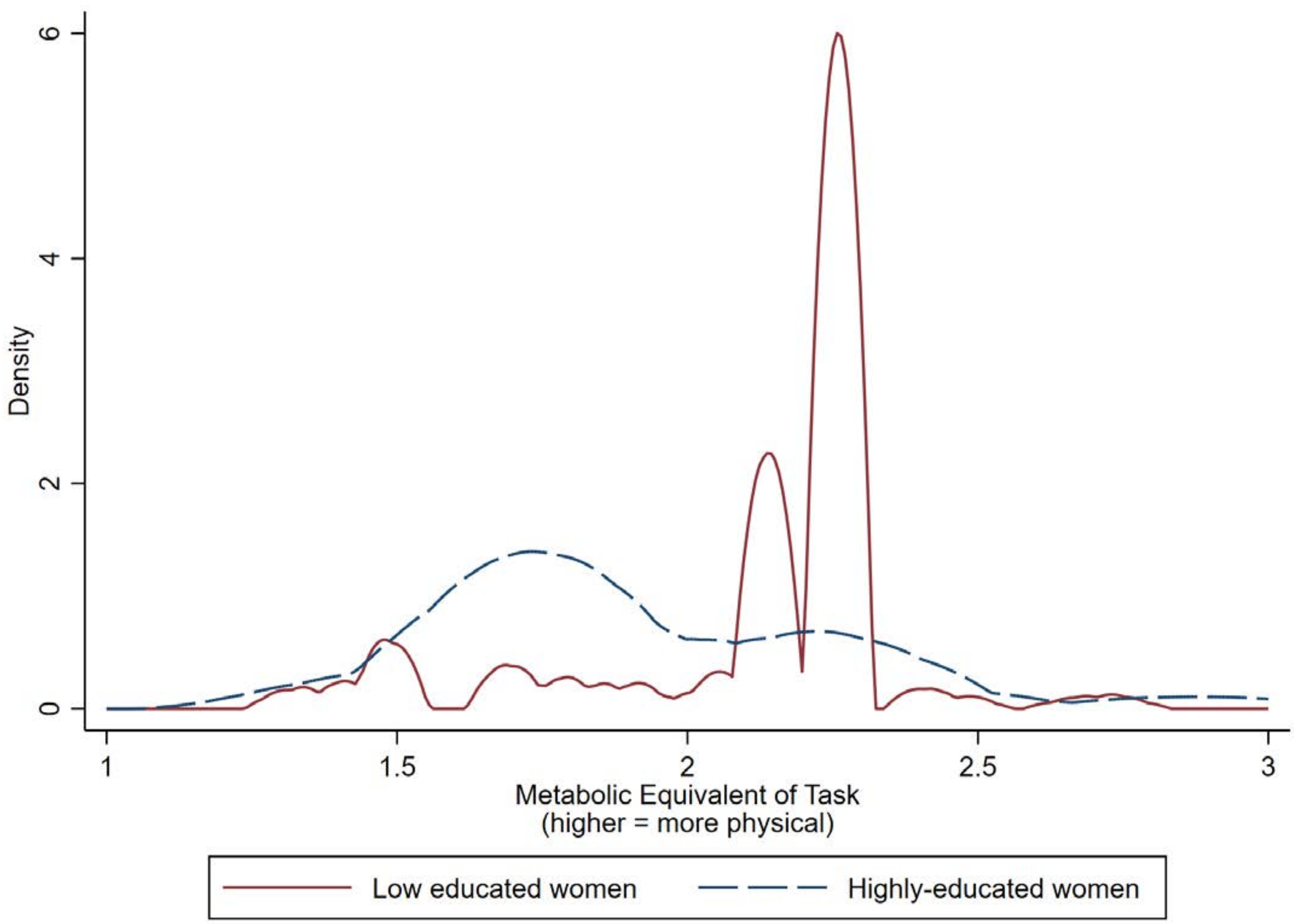
(k) Total income, Highly-educated women



(l) Total income, Low-educated women

Notes: Each point and vertical segment represent the 2SLS estimate of Equation (1) and its corresponding 95% confidence interval for the coefficient of the radiotherapy indicator. We estimate separate 2SLS models for the outcome indicated, measured at the time shown on the horizontal axis. "Baseline" represents our baseline specification, Equation (1), estimated in the subsample indicated in the figure caption. "Hospital FEs" indicate a specification that adds to Equation (1) fixed effects for the treatment hospital, while "Excluding screening" indicates that the estimation of Equation (1) is conducted after excluding from the subsample women residing in areas that introduced universal breast cancer screening programs during our sample period.

Figure A9 (cont.): Robustness of Heterogeneous Effects by Education to Model Specification



Notes: Kernel density estimates of the distribution of Metabolic Equivalent of Task values in our analysis sample. Low educated women (solid line) are women with at most 12 years of education, while highly-educated women (dotted line) have more than 12 years of education. The Metabolic Value of Task is assigned to each woman based on their occupation two years prior to diagnosis (see Deyaert et al., 2017, for details on the definition and calculation of the Metabolic Equivalent of Task values). The distribution is truncated at 3 (less than 5% of women are excluded).

Figure A10: Distribution of Metabolic Task Equivalent by Patient Education

Table A1: Sample Construction

	Number of observations
Diagnosed between 1990-1998:	26,900
— not in DBCG89	9,644
— post-menopausal	11,929
— low or unknown risk	1,646
— always eligible for whom intensity changed	564
— missing values for key variables:	277
— age 55+ at the time of diagnosis:	17
Analysis sample	2,823

Table A2: Effects of Adjuvant Radiation Therapy on Mortality and Labor Market Outcomes, OLS Estimates

	Years since diagnosis									
	1	2	3	4	5	6	7	8	9	10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Died?	0.009	0.007	−0.018	−0.028	−0.056**	−0.057**	−0.061**	−0.066***	−0.071***	−0.068***
	(0.008)	(0.014)	(0.017)	(0.022)	(0.024)	(0.024)	(0.024)	(0.022)	(0.023)	(0.022)
Mean outcome	0.005	0.037	0.091	0.142	0.182	0.210	0.235	0.262	0.278	0.298
Died of breast cancer?	0.009	0.009	−0.011	−0.022	−0.049**	−0.049*	−0.055**	−0.062***	−0.068***	−0.069***
	(0.008)	(0.014)	(0.017)	(0.022)	(0.024)	(0.025)	(0.025)	(0.023)	(0.023)	(0.023)
Mean outcome	0.005	0.033	0.084	0.132	0.172	0.196	0.220	0.243	0.258	0.276
Employed?	0.004	0.015	0.051*	0.050	0.033	0.029	0.025	0.047	0.052*	0.046
	(0.027)	(0.029)	(0.028)	(0.031)	(0.026)	(0.031)	(0.032)	(0.028)	(0.028)	(0.029)
Mean outcome	0.729	0.683	0.625	0.590	0.569	0.540	0.521	0.496	0.456	0.424
Unemployed?	0.008	−0.010	−0.002	0.015	0.009	0.020**	0.025**	0.014	−0.001	0.010
	(0.012)	(0.012)	(0.012)	(0.010)	(0.008)	(0.009)	(0.012)	(0.009)	(0.006)	(0.007)
Mean outcome	0.054	0.047	0.040	0.032	0.025	0.028	0.017	0.022	0.025	0.022
Out of labor force?	−0.031	−0.005	−0.049*	−0.065**	−0.042*	−0.049*	−0.051*	−0.060**	−0.051*	−0.056**
	(0.020)	(0.026)	(0.027)	(0.031)	(0.025)	(0.029)	(0.029)	(0.027)	(0.027)	(0.027)
Mean outcome	0.152	0.271	0.336	0.378	0.406	0.433	0.463	0.482	0.519	0.554
Annual labor income ('000 DKK)	9.961	3.717	11.060	16.142	15.272	13.485	13.581	11.997	16.878	19.892*
	(9.617)	(9.983)	(11.045)	(9.914)	(10.659)	(11.034)	(11.935)	(11.961)	(10.981)	(10.973)
Mean outcome	207.445	199.525	188.281	176.824	168.533	162.338	158.934	154.349	150.507	143.179
Annual gross income ('000 DKK)	6.488	6.502	8.990	19.274*	18.227	19.770	17.100	16.422	15.396	17.900
	(8.632)	(9.306)	(10.029)	(10.951)	(11.069)	(11.956)	(12.226)	(11.314)	(12.685)	(11.806)
Mean outcome	275.107	263.301	252.104	237.931	230.073	220.608	217.998	212.011	211.982	203.078

Notes: OLS estimates based on Equation (1), estimated in the full analysis sample ($N = 2{,}823$). Each cell presents the estimate of the coefficient on the indicator for radiotherapy from a separate regression for the outcome indicated in the row measured at the time indicated in the column. All specifications include all possible interactions of the characteristics that determine eligibility for radiation treatment, as well as indicators for year of diagnosis and for the type of chemotherapy treatment (trial arm). Variable names ending in a question mark are dummy variables with one being yes and zero being no, while variables with monetary values are expressed in thousands of 2015 DKK. The reported mean of the outcome is calculated among the control group (always and never eligible women) diagnosed in the period 1990–1995, before the change in guidelines. Standard errors are clustered at the hospital level. Significance levels: * p<0.1 ** p<0.05 *** p<0.01.

Table A3: Effects of Adjuvant Radiation Therapy on Government Transfers, OLS Estimates

	Years since diagnosis	
	1–5 (1)	6–10 (2)
Any government transfer?	−0.032	−0.031
	(0.023)	(0.026)
Mean outcome	0.411	0.328
Sickness benefits?	−0.031	−0.046*
	(0.026)	(0.025)
Mean outcome	0.285	0.230
Disability pension?	−0.007	−0.001
	(0.016)	(0.020)
Mean outcome	0.125	0.128
Number of weeks with any government transfer	0.191	1.135
	(3.495)	(4.383)
Mean outcome	28.181	31.187
Number of weeks on sickness benefits	2.553	4.558
	(3.666)	(5.244)
Mean outcome	21.961	31.596
Number of weeks on disability pension	−0.031	−0.046*
	(0.026)	(0.025)
Mean outcome	0.285	0.230

Notes: OLS estimates based on Equation (1), estimated in the full analysis sample ($N = 2{,}823$). Each cell presents the estimate of the coefficient on the indicator for radiotherapy from a separate regression for the outcome indicated in the row aggregated over the period indicated in the column. All specifications include all possible interactions of the characteristics that determine eligibility for radiation treatment, as well as indicators for year of diagnosis and for the type of chemotherapy treatment (trial arm). Variable names ending in a question mark are dummy variables with one being yes and zero being no. The reported mean of the outcome is calculated among the control group (always and never eligible women) diagnosed in the period 1990–1995, before the change in guidelines. Standard errors are clustered at the hospital level. Significance levels: * p<0.1 ** p<0.05 *** p<0.01.

Table A4: Effects of Adjuvant Radiation Therapy on Mortality and Labor Market Outcomes, 2SLS Estimates

	Years since diagnosis									
	(1) 1	(2) 2	(3) 3	(4) 4	(5) 5	(6) 6	(7) 7	(8) 8	(9) 9	(10) 10
Died?	0.002	0.001	−0.046	−0.054	−0.107**	−0.097**	−0.098**	−0.108**	−0.102**	−0.103**
	(0.011)	(0.020)	(0.035)	(0.042)	(0.046)	(0.048)	(0.049)	(0.046)	(0.050)	(0.047)
Mean outcome	0.005	0.037	0.091	0.142	0.182	0.210	0.235	0.262	0.278	0.298
Died of breast cancer?	0.002	−0.002	−0.039	−0.049	−0.097**	−0.087*	−0.095**	−0.109**	−0.104**	−0.109**
	(0.011)	(0.019)	(0.035)	(0.041)	(0.045)	(0.047)	(0.048)	(0.044)	(0.049)	(0.047)
Mean outcome	0.005	0.033	0.084	0.132	0.172	0.196	0.220	0.243	0.258	0.276
Employed?	0.086**	0.099**	0.157***	0.161***	0.155***	0.142**	0.129*	0.144**	0.173***	0.155***
	(0.039)	(0.049)	(0.049)	(0.052)	(0.050)	(0.058)	(0.067)	(0.062)	(0.060)	(0.057)
Mean outcome	0.796	0.683	0.625	0.590	0.569	0.540	0.521	0.496	0.456	0.424
Unemployed?	−0.018	−0.030	−0.032**	0.018	0.002	0.012	0.021*	0.025*	0.000	0.007
	(0.022)	(0.019)	(0.016)	(0.015)	(0.017)	(0.014)	(0.012)	(0.013)	(0.011)	(0.013)
Mean outcome	0.052	0.047	0.040	0.032	0.025	0.028	0.017	0.022	0.025	0.022
Out of labor force?	−0.068*	−0.069	−0.125***	−0.178***	−0.157***	−0.154**	−0.150**	−0.168***	−0.174***	−0.162***
	(0.037)	(0.043)	(0.047)	(0.058)	(0.055)	(0.060)	(0.061)	(0.062)	(0.059)	(0.055)
Mean outcome	0.152	0.271	0.336	0.378	0.406	0.433	0.463	0.482	0.519	0.554
Annual labor income ('000 DKK)	32.328*	25.864	50.832***	53.255***	61.506**	54.875**	57.395**	52.117**	54.663**	65.107**
	(18.103)	(16.875)	(18.949)	(18.662)	(23.274)	(22.859)	(23.368)	(25.008)	(25.263)	(24.749)
Mean outcome	207.445	199.525	188.281	176.824	168.533	162.338	158.934	154.349	150.507	143.179
Annual gross income ('000 DKK)	23.458	18.745	37.963*	51.629**	62.744**	59.074**	41.302	51.139**	39.289	46.874*
	(19.667)	(18.696)	(20.996)	(22.192)	(24.981)	(27.445)	(26.050)	(24.718)	(29.628)	(27.154)
Mean outcome	275.107	263.301	252.104	237.931	230.073	220.608	217.998	212.011	211.982	203.078

Notes: 2SLS estimates based on Equation (1), estimated in the full analysis sample ($N = 2{,}823$). Each cell presents the estimate of the coefficient on the indicator for radiotherapy from a separate regression for the outcome indicated in the row measured at the time indicated in the column. All specifications include all possible interactions of the characteristics that determine eligibility for radiation treatment, as well as indicators for year of diagnosis and for the type of chemotherapy treatment (trial arm). The instrument is the interaction between an indicator for the woman belonging to the $T95$ group and an indicator for being diagnosed after 1995. Variable names ending in a question mark are dummy variables with one being yes and zero being no, while variables with monetary values are expressed in thousands of 2015 DKK. The reported mean of the outcome is calculated among women who do not receive radiotherapy. Standard errors are clustered at the hospital level. Significance levels: * p<0.1 ** p<0.05 *** p<0.01.

Table A5: Effects of the 1995 Guideline Change on Mortality and Labor Market Outcomes (Reduced-Form Estimates)

	Years since diagnosis									
	(1) 1	(2) 2	(3) 3	(4) 4	(5) 5	(6) 6	(7) 7	(8) 8	(9) 9	(10) 10
Died?	0.002	0.000	−0.034	−0.041	−0.080**	−0.073**	−0.074**	−0.081**	−0.077**	−0.077**
	(0.008)	(0.015)	(0.027)	(0.031)	(0.035)	(0.037)	(0.037)	(0.035)	(0.038)	(0.036)
Mean outcome	0.005	0.037	0.091	0.142	0.182	0.210	0.235	0.262	0.278	0.298
Died of breast cancer?	0.002	−0.001	−0.029	−0.037	−0.073**	−0.065*	−0.072**	−0.082**	−0.078**	−0.082**
	(0.008)	(0.015)	(0.027)	(0.031)	(0.034)	(0.036)	(0.036)	(0.034)	(0.038)	(0.036)
Mean outcome	0.005	0.033	0.084	0.132	0.172	0.196	0.220	0.243	0.258	0.276
Employed?	0.063**	0.075**	0.118***	0.121***	0.117***	0.107**	0.097*	0.108**	0.131***	0.117***
	(0.030)	(0.037)	(0.037)	(0.039)	(0.038)	(0.043)	(0.050)	(0.046)	(0.046)	(0.043)
Mean outcome	0.729	0.683	0.625	0.590	0.569	0.540	0.521	0.496	0.456	0.424
Unemployed?	−0.018	−0.022	−0.024**	0.013	0.001	0.009	0.016*	0.019**	0.000	0.005
	(0.016)	(0.015)	(0.012)	(0.012)	(0.013)	(0.010)	(0.009)	(0.009)	(0.008)	(0.010)
Mean outcome	0.054	0.047	0.040	0.032	0.025	0.028	0.017	0.022	0.025	0.022
Out of labor force?	−0.045	−0.052	−0.094***	−0.134***	−0.118***	−0.116**	−0.113**	−0.127***	−0.131***	−0.122***
	(0.030)	(0.032)	(0.035)	(0.043)	(0.042)	(0.045)	(0.046)	(0.046)	(0.044)	(0.041)
Mean outcome	0.152	0.271	0.336	0.378	0.406	0.433	0.463	0.482	0.519	0.554
Annual labor income ('000 DKK)	24.334*	19.469	38.263***	40.086***	46.298***	41.306**	43.203**	39.230**	41.146**	49.008***
	(13.603)	(12.681)	(14.260)	(13.986)	(17.515)	(17.262)	(17.646)	(18.861)	(19.037)	(18.729)
Mean outcome	207.445	199.525	188.281	176.824	168.533	162.338	158.934	154.349	150.507	143.179
Annual gross income ('000 DKK)	17.657	14.110	28.576*	38.863**	47.230**	44.467**	31.090	38.494**	29.574	35.284*
	(14.768)	(14.074)	(15.820)	(16.763)	(18.889)	(20.782)	(19.730)	(18.718)	(22.419)	(20.536)
Mean outcome	275.107	263.301	252.104	237.931	230.073	220.608	217.998	212.011	211.982	203.078

Notes: OLS estimates of the reduced-form in Equation (3), estimated in the full analysis sample ($N = 2{,}823$). Each cell presents the estimate of the coefficient on the instrument ($T95_i Post95_t$) from a separate regression for the outcome indicated in the row measured at the time indicated in the column. All specifications include all possible interactions of the characteristics that determine eligibility for radiation treatment, as well as indicators for year of diagnosis and for the type of chemotherapy treatment (trial arm). Variable names ending in a question mark are dummy variables with one being yes and zero being no, while variables with monetary values are expressed in thousands of 2015 DKK. The reported mean of the outcome is calculated among the control group (always and never eligible women) diagnosed in the period 1990–1995, before the change in guidelines. Standard errors are clustered at the hospital level. Significance levels: * p<0.1 ** p<0.05 *** p<0.01.

Table A6: Effects of the 1995 Guideline Change on Government Transfers (Reduced-Form Estimates)

	Years since diagnosis	
	1–5 (1)	6–10 (2)
Any government transfer?	−0.068**	−0.073**
	(0.028)	(0.037)
Mean outcome	0.411	0.328
Sickness benefits?	−0.059**	−0.064*
	(0.029)	(0.033)
Mean outcome	0.285	0.230
Disability pension?	−0.023	−0.037
	(0.025)	(0.026)
Mean outcome	0.125	0.128
Number of weeks with any government transfer	−3.641	−6.944
	(4.810)	(5.938)
Mean outcome	28.181	31.187
Number of weeks on sickness benefits	−2.607	−2.596*
	(1.901)	(1.525)
Mean outcome	8.443	9.558
Number of weeks on disability pension	−2.131	−4.181
	(4.653)	(6.044)
Mean outcome	21.961	31.596

Notes: OLS estimates of the reduced-form in Equation (3), estimated in the full analysis sample ($N = 2{,}823$). Each cell presents the estimate of the coefficient on the instrument ($T95_i Post95_t$) from a separate regression for the outcome indicated in the row aggregated over the period indicated in the column. All specifications include all possible interactions of the characteristics that determine eligibility for radiation treatment, as well as indicators for year of diagnosis and for the type of chemotherapy treatment (trial arm). Variable names ending in a question mark are dummy variables with one being yes and zero being no. The reported mean of the outcome is calculated among the control group (always and never eligible women) diagnosed in the period 1990–1995, before the change in guidelines. Standard errors are clustered at the hospital level. Significance levels: * p<0.1 ** p<0.05 *** p<0.01.

Table A7: Descriptive Statistics, $T95$ versus Comparison Patients Diagnosed During 1990–1994

Variable name	T95 (1)	Control group (2)	p-value (3)
Panel A: Disease Pathology			
Tumor size in mm	28.86	25.30	0.000
	(19.35)	(16.62)	
≤ 20mm?	0.45	0.50	0.034
21–50mm?	0.45	0.44	0.810
≥ 51mm?	0.11	0.06	0.001
Number of positive nodes	2.94	2.46	0.010
	(3.22)	(4.12)	
Zero?	0.02	0.67	0.000
1–3?	0.76	0.00	0.000
4+?	0.22	0.33	0.000
Carcinoma not removed micro-radically?	0.00	0.09	0.000
Had mastectomy?	0.98	0.72	0.000
Had lumpectomy?	0.00	0.26	0.000
Had lumpectomy followed by mastectomy?	0.02	0.02	0.587
Panel B: Demographic Characteristics			
Age at diagnosis	45.04	41.99	0.000
	(5.24)	(5.44)	
Years of education	12.49	12.79	0.062
	(3.15)	(3.04)	
Married?	0.71	0.71	0.910
Immigrant?	0.03	0.04	0.422
Characteristics 2-4 years pre-diagnosis			
Employed?	0.83	0.86	0.063
Unemployed?	0.07	0.05	0.046
Out of the labor force?	0.10	0.08	0.375
Labor earnings (thousands)	208.92	217.47	0.216
	(133.18)	(134.24)	
Gross personal income (thousands)	262.15	268.20	0.311
	(115.98)	(114.72)	
Number of Observations	703	804	

Notes: Columns 1 and 2 present the mean (standard deviation in parentheses) of the characteristic indicated in the row in the sample indicated in the column among women diagnosed between 1990–1994. Variable names ending in a question mark are dummy variables with one being yes and zero being no, while variables with monetary values are expressed in thousands of 2015 DKK. Demographic characteristics are measured in the year of diagnosis or averaged over the period 2–4 years before diagnosis, as indicated. Column 3 presents p-values for the test of equality of the means between patients receiving and not receiving radiotherapy.

Table A8: Effects of the 1995 Guideline Change on Predetermined Patient Characteristics and Pre-diagnosis Outcomes

A. Pre-diagnosis outcomes		**B. Predetermined characteristics**	
	(1)		(2)
Employed?	0.021	Years of education	0.142
	(0.026)		(0.210)
Observations	2,812	Observations	2,751
Mean outcome	0.829	Mean outcome	12.485
Unemployed?	−0.034**	Married?	−0.031
	(0.015)		(0.032)
Observations	2,812	Observations	2,823
Mean outcome	0.075	Mean outcome	0.714
Out of labor force?	0.012	Immigrant?	−0.004
	(0.023)		(0.018)
Observations	2,812	Observations	2,823
Mean outcome	0.095	Mean outcome	0.030
Labor earnings	10.319		
	(12.178)		
Observations	2,814		
Mean outcome	208.924		
Total income	10.631		
	(11.542)		
Observations	2,815		
Mean outcome	262.151		

Notes: OLS estimates of the reduced-form in Equation (3), estimated in the full analysis sample. Each cell presents the estimate of the coefficient on the instrument ($T95_i Post95_t$) from a separate regression for the outcome indicated in the row averaged over the period 2–4 years before diagnosis (Panel A) or measured in the year of diagnosis (Panel B). All specifications include all possible interactions of the characteristics that determine eligibility for radiation treatment, as well as indicators for year of diagnosis and for the type of chemotherapy treatment (trial arm). Variable names ending in a question mark are dummy variables with one being yes and zero being no, while variables with monetary values are expressed in thousands of 2015 DKK. The reported mean of the outcome is calculated among the control group (always and never eligible women) diagnosed in the period 1990–1995, before the change in guidelines. Standard errors are clustered at the hospital level. Significance levels: * p<0.1 ** p<0.05 *** p<0.01.

Table A9: Effects of the 1995 Guideline Change on Adjuvant Radiation Therapy Take-Up in Different Subsamples

	Baseline	Years of education		Pre-diagnosis labor income	
		≤ 12	> 12	$\leq$ median	$>$ median
	(1)	(2)	(3)	(4)	(5)
$T95 \times Post95$	0.753***	0.751***	0.750***	0.731***	0.787***
	(0.018)	(0.035)	(0.021)	(0.031)	(0.022)
Observations	2,823	1,022	1,729	1,407	1,407

	Baseline	Marital status		Predicted 10-year mortality	
		Single	Married	$\leq$ median	$>$ median
	(1)	(6)	(7)	(8)	(9)
$T95 \times Post95$	0.753***	0.766***	0.758***	0.825***	0.680***
	(0.018)	(0.034)	(0.021)	(0.025)	(0.031)
Observations	2,823	860	1,963	1,400	1,398

Notes: OLS estimates based on the first-stage Equation (2). Each cell presents the estimate of the coefficient on the instrument ($T95_i Post95_t$) from a separate regression estimated in the sample indicated in the column heading. All specifications include all possible interactions of the characteristics that determine eligibility for radiation treatment, as well as indicators for year of diagnosis and for the type of chemotherapy treatment (trial arm). YEars of education and marital status are measured in the year of diagnosis. Pre-diagnosis labor income is the average of the yearly labor income earned over the period 2–4 years before diagnosis. Predicted 10-year breast cancer mortality is obtained by applying to our analysis sample the prediction from a probit regression of an indicator for dying from breast cancer during the ten years after diagnosis on the disease characteristics described in Section 3, estimated in the sample of all the breast cancer patients diagnosed during our sample period who are not included in our analysis sample and who are not treated with radiation therapy. Standard errors are clustered at the hospital level. Significance levels: * p<0.1 ** p<0.05 *** p<0.01.

Table A10: Heterogeneous Effects of Adjuvant Radiation Therapy on Recurrence, 2SLS Estimates

	Recurrence 10 years after diagnosis		
	Baseline (1)	Hospital fixed effects (2)	Exclude screening areas (3)
High-educated women	-0.179^{***}	-0.174^{***}	-0.193^{***}
	(0.048)	(0.047)	(0.056)
Observations	1,729	1,729	1,484
Mean outcome	0.486	0.489	0.484
Low-educated women	−0.036	−0.039	−0.071
	(0.076)	(0.076)	(0.084)
Observations	1,022	1,022	910
Mean outcome	0.487	0.486	0.485

Notes: 2SLS estimates based on Equation (1), estimated in the subsample indicated in the row. Each cell presents the estimate of the coefficient on the indicator for radiotherapy from a separate regression using an indicator for recurrence at any point during the 10 years after diagnosis. All specifications include all possible interactions of the characteristics that determine eligibility for radiation treatment, as well as indicators for year of diagnosis and for the type of chemotherapy treatment (trial arm). The specification in Column 2 adds fixed effects for the treatment hospital. The sample in Column 3 excludes women residing in areas that introduced universal breast cancer screening programs during the sample period. The instrument is the interaction between an indicator for the woman belonging to the $T95$ group and an indicator for being diagnosed after 1995. Low-educated women are women with at most 12 years of schooling, while highly-educated women have more than 12 years of schooling. The reported mean of the outcome is calculated among women who do not receive radiotherapy. Standard errors are clustered at the hospital level. Significance levels: * p<0.1 ** p<0.05 *** p<0.01.

Table A11: Disease Characteristics by Patient Education

	All women (1)	Low educated women (2)	Highly-educated women (3)	*p*-value (4)
Age at diagnosis	43.53	43.85	43.35	0.022
	(5.64)	(5.42)	(5.76)	
Tumor size in mm	26.06	26.12	26.02	0.881
	(17.17)	(17.06)	(17.24)	
$\leq$ 20mm?	0.49	0.49	0.49	0.988
21–50mm?	0.44	0.43	0.44	0.749
$\geq$ 51mm?	0.07	0.08	0.07	0.530
Number of positive nodes	2.58	2.59	2.58	0.901
	(3.80)	(3.74)	(3.83)	
Zero?	0.40	0.39	0.40	0.555
1–3?	0.34	0.35	0.34	0.552
4+?	0.26	0.26	0.26	0.991
Carcinoma not removed micro-radically?	0.04	0.04	0.03	0.337
Had mastectomy?	0.82	0.84	0.80	0.014
Had lumpectomy?	0.16	0.14	0.17	0.026
Had lumpectomy followed by mastectomy?	0.03	0.02	0.03	0.382
Predicted 10-year breast cancer mortality	0.50	0.53	0.48	0.010
Observations	2,751	1,022	1,729	

Notes: Columns 1–3 present the mean (standard deviation in parentheses) of the characteristic indicated in the row in the sample indicated in the column. Variable names ending in a question mark are dummy variables with one being yes and zero being no. Predicted 10-year breast cancer mortality is obtained by applying to our analysis sample the prediction from a probit regression of an indicator for dying from breast cancer during the ten years after diagnosis on the disease characteristics described in Section 3, estimated in the sample of all the breast cancer patients diagnosed during our sample period who are not included in our analysis sample and who are not treated with radiation therapy. Column 4 presents *p*-values for the test of equality of the means between low- and highly-educated women. Low-educated women are women with at most 12 years of schooling, while highly-educated women have more than 12 years of schooling.