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

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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. We reproduce the results from an RCT showing the lifesaving benefits of radiotherapy. We show radiation therapy also has economic returns: *Ten years after diagnosis*, *treatment increases employment by 37 percent and earnings by 45 percent. Previous work has documented a substantial employment drop after a breast cancer diagnosis. Our results imply that radiation therapy can reduce this effect by 70 percent.* (*JEL* H51, I12, I18, J16, J22, J31)

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 socioeconomic outcomes (Currie and Madrian 1999; Fogel 2004; Bleakley 2007; Stephens Jr. and Toohey 2022). Existing evidence also suggests that health affects economic outcomes at the national level (Weil 2007; Acemoglu and Johnson 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

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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 about 2.2 million new cases worldwide in 2020, accounting for more than 12 percent of all newly diagnosed cancer cases annually. It is also the most common form of cancer worldwide (OECD 2023). In addition, roughly one-third of breast cancer patients are diagnosed between the ages of 25–54, which are peak working years for women.[1] Finally, survival rates are high with more than 90 percent 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 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 guidelines 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 (RCTs) consistently show that breast cancer treatments are effective in reducing mortality (e.g., Overgaard et al. 1997; Ragaz et al. 1997; Early Breast Cancer Trialists' Collaborative Group 2005, 2011), 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 two-stage least squares (2SLS) are identical to those found in an RCT that examined the impact of adding radiotherapy to chemotherapy among women diagnosed with breast cancer

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

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 labor market outcomes: employment, income, and welfare use. We address a potential bias from selective survival by coding nonsurvivors 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 percent) more likely to be employed 10 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 percent and total income by 7–27 percent in the 10 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 percent.

What mechanisms drive these labor market improvements? Given the documented mortality gains, one possibility is that the estimated economic benefits are due to selective survival. We provide four pieces of evidence to suggest that this is not the case. First, we highlight the different dynamics of the estimated effects on mortality versus labor market outcomes: The labor market effects occur before any mortality gains are realized. Second, we consider what the labor market outcomes of nonsurvivors would need to have been to eliminate our baseline effects. Using a simulation exercise, we find that the outcomes of nonsurvivors would need to have been in the extreme right tail of the distributions of labor market outcomes among survivors in the treatment group diagnosed in the post-1995 period in order to wipe out the baseline effects. Third, we show that the baseline effects are robust to using only the sample of survivors. Finally, we estimate the effects of radiotherapy on labor market outcomes taking into account selective survival through a Heckman selection model with endogenous covariates (Schwiebert 2015). The estimates from this model are very similar to the baseline estimates, suggesting that the nonsurvivors come from the very left part of the distribution of the outcomes.

Reductions in recurrence could be another explanation for the labor market gains. Even if recurrence is not fatal, it could reduce labor supply by inducing incapacitation due to the need for medical treatments. Consistent with prior medical studies documenting reduced cancer recurrence after radiation therapy (Overgaard et al. 1997), we find that radiotherapy reduces the likelihood of recurrence five to ten years after diagnosis by 14–21 percentage points. However, the patterns of estimated effects on recurrence versus labor market outcomes do not point to a strong link between the two: The reductions in recurrence are experienced during the first few years after diagnosis, but the economic gains are experienced throughout the entire ten years after diagnosis.

We instead argue that the improved labor market outcomes may reflect a "horizon" effect: With increased life years, individuals must finance additional lifetime consumption, which increases worklife (Kalemli-Ozcan and Weil 2010). Similarly, exogenous improvements in health driven by medical innovations may free up time previously spent on health-related investments and illness and allow individuals to devote more time to the labor market to increase consumption (Jeon and Pohl 2019). Our administrative data are not suitable to explicitly test the predictions of these

theories, but we can confirm that radiotherapy improves general health: It reduces by 20 percent the number of (inpatient and outpatient) hospital contacts during the 10 years after diagnosis.

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., Bradley, Bednarek, and Neumark 2002a, b; Bradley et al. 2005; Heinesen and Kolodziejczyk 2013). Our work complements this as it focuses on whether specific treatment patterns can lessen the impact of the disease on economic outcomes. Second, we contribute to a growing body of work in economics that considers the impact of medical treatments on labor supply.[2] This work has considered antiretroviral therapy for HIV/AIDS patients (Thirumurthy, Zivin, and Goldstein 2008; Habyarimana, Mbakile, and Pop-Eleches 2010; Thirumurthy and Graff Zivin 2012; Baranov, Bennett, and Kohler 2015; Papageorge 2016), Cox-2 inhibitors (Garthwaite 2012; Butikofer and Skira 2018), mental health treatments (Timbie et al. 2006; Cronin, Forsstrom, and Papageorge 2025; Biasi, Dahl, and Moser 2023), and prescription opioids (Harris et al. 2020; Beheshti 2023). 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 prostate 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. However, the paper is unable to disentangle the effects of medical innovation from the improvements in diagnostics as they lack clinical information on disease characteristics. 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 distinguishes our paper from previous studies.

Insurance providers of all types are increasingly looking at formal cost-effectiveness analysis to determine what treatments to cover, benefits design, and price negotiations. In most cases, carriers are making narrow decisions based primarily on medical costs and benefits. The "Second Panel on Cost-Effectiveness in Health and Medicine" (Sanders et al. 2016) recognized this fact and, in their 2016 report, recommended a greater emphasis on the nonmedical cost and benefits of treatment. Their goal was to "ensure that all consequences, including those outside the formal health care sector, are considered regularly and comprehensively, which has generally not been the case to date," (Sanders et al. 2016, 1096). The emphasis on nonmedical outcomes is especially important in cancer care as treatment like chemotherapy may greatly impact the quality of life. As a result, there is a growing emphasis in oncology care to include quality of life measures as secondary outcomes in cancer treatment clinical trials (Wilson, Karakasis, and Oza 2015). Despite

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

this, only 45 percent 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 of the impact of therapies on nonmedical outcomes is critically important. This paper helps illustrate how the use of administrative data can be incorporated into medical intervention in order to broaden the set of outcomes.

Finally, our findings are also pertinent to the ongoing discussions on the role of medical treatments in the increase in overall health spending. The costs of cancer treatment are rising worldwide. For example, the United States spent an estimated $161.2 billion in 2017 on cancer related healthcare expenditures. In the European Union, healthcare spending for cancer care was €57.3 billion (Jemal et al. 2019). With roughly $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 cost-effectiveness of new cancer treatments.

I. 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.

A. *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. A 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 mammogram often supplemented with ultrasonography and needle biopsy.[3]

[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 biannual 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).

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 percent 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 designed 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, Matheus, and Candido dos Reis 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 consisted of four different arms comparing the impact of different chemotherapy treatments for high-risk premenopausal and postmenopausal women and of different hormone therapy treatments for high-risk postmenopausal women (see Supplemental Appendix Figure A1).[6] The allocation to radiation therapy was deterministically tied to clinical and demographic characteristics. 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 its use (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 Figure 1. In the decision tree, diagnoses or demographic characteristics are listed in regular font and the text in italics represents the medical decision

[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, Christiansen, and Mouridsen 2008).

[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 percent of all patients diagnosed with breast cancer between 1990–1998 and who were eligible for the DBCG89 trial refused to participate.

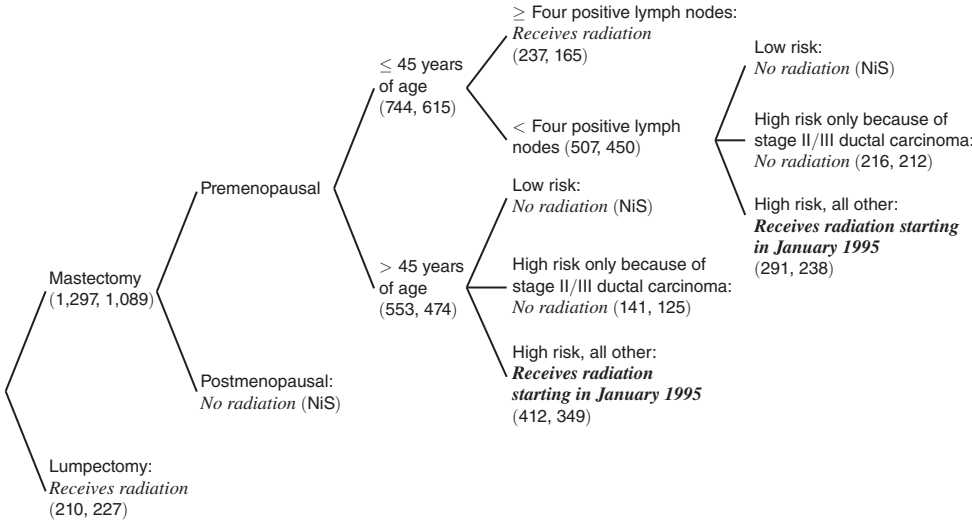

FIGURE 1. ELIGIBILITY FOR RADIATION THERAPY, 1990–1998

Notes: Summary of the rules for allocation to radiation therapy during the DBCG89 RCT, based on the official protocol (https://dbcg.dk/PDF/DBCG_2014_protokoller_oversigt_01.08.2014.pdf) provided by the DBCG, last accessed on December 20, 2024, and the description in Møller et al. (2008). The numbers in brackets correspond to the number of women falling in that category who were diagnosed between 1990–1994 and between 1995–1998, respectively. "NiS" indicates that none of the women in that category are included in our sample. The categories highlighted in bold and italic comprise the *T*95 group.

concerning radiation therapy. The text in bold face represents the new groups that were eligible for radiotherapy after a change in guidelines in 1995. The two numbers in brackets are the number of patients in our sample at each decision node before and after the reforms in 1995.

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

B. *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 percent 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 percent 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, Gupta, and Pedersen 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 unemployment insurance benefits, social assistance benefits, or early retirement pension. Supplemental Appendix Section A1 describes these additional sources of income insurance.

II. Data Sources and Analysis Sample

We use several population-level administrative datasets 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

[7]The base level was paid out to individuals whose work capacity was reduced by more than 50 percent and amounted in 1995 to 6,280 kr ($1,373 in 2015 prices) per month for married/cohabiting individuals and 6,531 kr ($1,428) 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 kr ($1,562), while single individuals received 7,394 kr ($1,616). Finally, individuals younger than 60 with no work capacity were classified as the high level and received 9,634 kr ($2,106) monthly if they were married/cohabiting or 9,885 kr ($2,161) if they were single.

with indicators for all-cause and breast cancer mortality. We examine effects for each year from the date of diagnosis, up to ten years after diagnosis.

Our primary labor market outcomes are measures of labor force participation and income. Measures for the former are derived from the *Register-Based Labor Force Statistics*, a dataset based on tax records with records on the labor market status of the entire Danish population as of November. From this data, 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 ten years later. A final set of outcomes examines 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 one–five and six–ten 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 nonsurvivors.

Control Variables.—We observe a rich set of patient characteristics in the clinical *Breast Cancer Database*. Using these data, we construct separate indicators for age at diagnosis in full years, tumor size in centimeters, the number of positive nodes (zero, one–three, four+), having the tumor removed microradically, the type of surgery (lumpectomy, mastectomy, or lumpectomy followed by mastectomy), and the type of chemotherapy received (DBCG89 clinical trial arm). From this set, we generate all possible interactions among the following indicators: having mastectomy, being younger than 45 years of age at diagnosis, the number of positive nodes (zero, one–three, four+), having a tumor larger than 50 mm, and having the tumor removed microradically. This allows us to flexibly control for the determinants of radiation therapy eligibility.

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 1 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 kr in 2015 are roughly equivalent to $15 in 2015.

Table 1—Sample Construction

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

Analysis Sample.—Our analysis sample includes a subset of female breast cancer patients diagnosed between 1990 and 1998. Table 1 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 percent), previous malignancies (7 percent), distant metastases (6 percent), and bilateral carcinomas (4.4 percent). Second, we restrict our attention to only high-risk premenopausal 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 ten 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. In Figure 1, the two numbers in parentheses below each node represent the number of patients in the sample pre- and post-1995 period.

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 (observations $= 1,290$). These are high-risk premenopausal 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 four positive lymph nodes. The second group (always eligible, observations $= 874$) includes patients who are eligible for radiation therapy regardless of when they are diagnosed. This includes premenopausal high-risk patients who had a lumpectomy, as well as premenopausal mastectomy patients younger than 45 years of age with at least four positive lymph nodes. The last group (never eligible, observations $= 659$) includes premenopausal women who are classified as high-risk only because of a stage II or III ductal carcinoma (i.e., they have tumors smaller than 50 mm and no positive lymph nodes). These patients are never eligible to receive radiation therapy during the period under study.

III. 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

$$Y_{it}^{a} = \alpha_1 + RT_{it}\beta_1 + \mathbf{X}_{it}\boldsymbol{\gamma}_1 + u_{1t} + \epsilon_{1it}, \tag{1}$$

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.[9] 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 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:

$$RT_{it} = \alpha_2 + T95_i Post95_t \beta_2 + \mathbf{X}_{it}\boldsymbol{\gamma}_2 + u_{2t} + \epsilon_{2it}, \tag{2}$$

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

$$Y_{it}^{a} = \alpha_3 + T95_i Post95_t \beta_3 + \mathbf{X}_{it}\boldsymbol{\gamma}_3 + u_{3t} + \epsilon_{3it}, \tag{3}$$

where the vector of patient characteristics $\mathbf{X}_{it}$ flexibly controls for the determinants of radiotherapy eligibility.[10] Note that our first-stage and reduced-form equations are equivalent to a difference-in-difference model with $T95$ as the treatment group.

[9] Our sample includes only premenopausal women (because the radiotherapy guidelines changed only for premenopausal women), which were included only in arms B and D of the trial. Therefore, we include fixed effects for these two arms.

[10] The patient characteristics that are flexibly included in $\mathbf{X}_{it}$ subsume the way in which the treatment group is defined.

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). 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-difference 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 not tied to radiotherapy eligibility are balanced between the treatment and comparison groups. Second, we plot the dynamics of employment and labor earnings among four groups of women (those in the $T95$ group diagnosed before and after 1995, and those in the control group diagnosed before and after 1995) and show that all four groups have very similar same prediagnosis patterns and levels for both outcomes. Third, we use prediagnosis outcomes as the dependent variable in our baseline model and show that there are no differences between the treatment and comparison groups. Finally, we 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, Imbens, and Rubin 1996). The monotonicity condition requires that being diagnosed after 1995 only increases the chance that a patient in the $T95$ group receives radiation therapy. We cannot interpret the 2SLS as a LATE 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 IV 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 IV, compliers comprise around 75 percent of our analysis sample, suggesting that our results are broadly relevant.

TABLE 2—DESCRIPTIVE STATISTICS

	All	Treated with radiotherapy		*p*-value
		Yes	No	
	(1)	(2)	(3)	(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 two–four years prediagnosis				
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	220.39	224.90	215.58	0.080
	(141.51)	(145.99)	(136.45)	
Gross personal income	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
$\leq$ 20 mm?	0.49	0.46	0.52	0.000
21–50 mm?	0.44	0.45	0.43	0.249
$\geq$ 51 mm?	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
One–three?	0.34	0.31	0.38	0.000
Four+?	0.26	0.39	0.12	0.000
Carcinoma not removed microradically?	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:				
Five years after diagnosis?	0.20	0.22	0.18	0.014
Ten years after diagnosis?	0.32	0.35	0.30	0.006
Died of breast cancer:				
Five years after diagnosis?	0.19	0.21	0.17	0.005
Ten years after diagnosis?	0.30	0.33	0.28	0.002

(*continued*)

IV. Results

A. *Descriptive Statistics*

Table 2 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).[11] Variable names ending in a question mark are indicators with one being yes and zero being no. The

[11] Supplemental Appendix Table A1 provides these descriptives separately for women diagnosed with breast cancer before and after the radiotherapy guideline change.

Table 2—Descriptive Statistics (*continued*)

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

Notes: Columns 1–3 list means and standard deviation (in parentheses) of the characteristics 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 Danish kroner. Demographic characteristics are measured in the year of diagnosis or averaged over the period two–four years before diagnosis, as indicated. Column 4 presents *p*-values for the test of equality of the means between patients treated and not treated with radiotherapy.

final column reports the *p*-value for the test of equality of means between patients receiving and not receiving radiotherapy. About 52 percent 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 percent are married and 84 percent 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 precancer employment rates between the two groups, the prediagnosis income and labor earnings of women receiving radiotherapy are 4–5 percent 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 percent larger than the average tumor size of those who only receive chemotherapy. This is primarily due to the higher share of tumors larger than 50 mm 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 four 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 postdiagnosis 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. Ten 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 2 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, we estimate the relationship between radiation therapy and the mortality of cancer patients using OLS (see Supplemental Appendix Table A2). 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, we also estimate OLS models for 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 nonsurvivors to address a potential bias from selective survival. The OLS associations suggest a weak

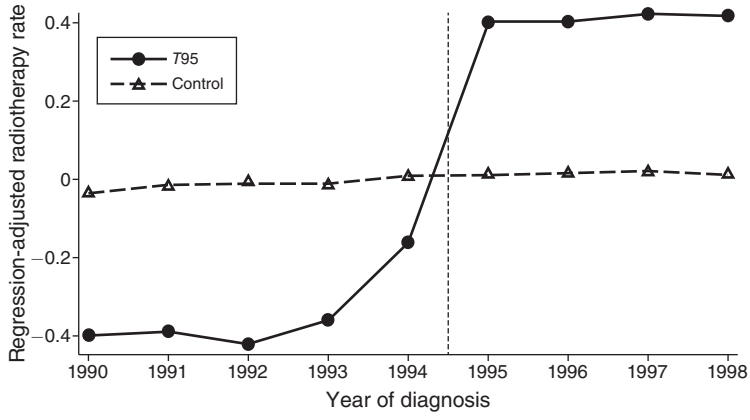

Figure 2. The Effect of the 1995 Guideline Change on Radiation Therapy Take-Up

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 indicators for age at diagnosis in full years, tumor size in centimeters, the number of positive nodes (zero, one–three, four+), having the tumor removed microradically, the type of surgery (lumpectomy, mastectomy, or lumpectomy followed by mastectomy), and the type of chemotherapy received (DBCG89 clinical trial arm). The regressions are estimated separately for *T*95 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 *T*95 while the dashed line represents the women in the comparison group.

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 indicate a consistently negative but generally statistically insignificant relationship between radiation therapy and the likelihood of receiving government transfers (see Supplemental Appendix Table A3).

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.

B. *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 premenopausal women) differ along clinical and demographic characteristics by design, we present in Figure 2 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 *T*95 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 *T*95, while the dashed line represents the group of women who are never or always eligible for treatment.

TABLE 3—EFFECTS OF THE 1995 GUIDELINE CHANGE ON ADJUVANT RADIATION THERAPY TAKE-UP IN DIFFERENT SUBSAMPLES

		Years of education		Prediagnosis labor income	
	Baseline	≤ 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
		Marital status		Predicted ten-year mortality	
	Baseline	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 among the following indicators: having mastectomy, being younger than 45 years of age at diagnosis, the number of positive nodes (zero, one–three, four+), having a tumor larger than 50mm, and having the tumor removed microradically; separate indicators for age at diagnosis in full years, tumor size in centimeters, the number of positive nodes (zero, one–three, four+), having the tumor removed microradically, the type of surgery (lumpectomy, mastectomy, or lumpectomy followed by mastectomy), and the type of chemotherapy received (DBCG89 clinical trial arm); and year-of-diagnosis fixed effects. Years of education and marital status are measured in the year of diagnosis. Prediagnosis labor income is the average of the yearly labor income earned over the period two–four years before diagnosis. Predicted ten-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 indicators for age at diagnosis in full years, tumor size in centimeters, the number of positive nodes (zero, one–three, four+), having the tumor removed microradically, the type of surgery (lumpectomy, mastectomy, or lumpectomy followed by mastectomy), and the type of chemotherapy received (DBCG89 clinical trial arm); and year-of-diagnosis fixed effects. This regression is 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.

Figure 2 shows that take-up of radiotherapy is constant among the comparison group throughout the entire period. In contrast, take-up in the $T95$ group remains steady before 1995, experiences a sharp increase following the 1995 guideline change, and then stabilizes at a higher level.[12] 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 (SE $=$ 1.8) in the probability of radiotherapy among women in the $T95$ group relative to other high-risk premenopausal patients (see column 1 of Table 3). The associated F-statistic is 1,842.3, well above the recent rule-of-thumb value of roughly 100 to minimize finite sample bias (Lee et al. 2022).

[12] Supplemental Appendix Figure A2.a provides the corresponding figure based on raw data. Figure 2 shows that take-up of radiotherapy among $T95$ women increases already in 1994. Supplemental Appendix Figure A2.b 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.

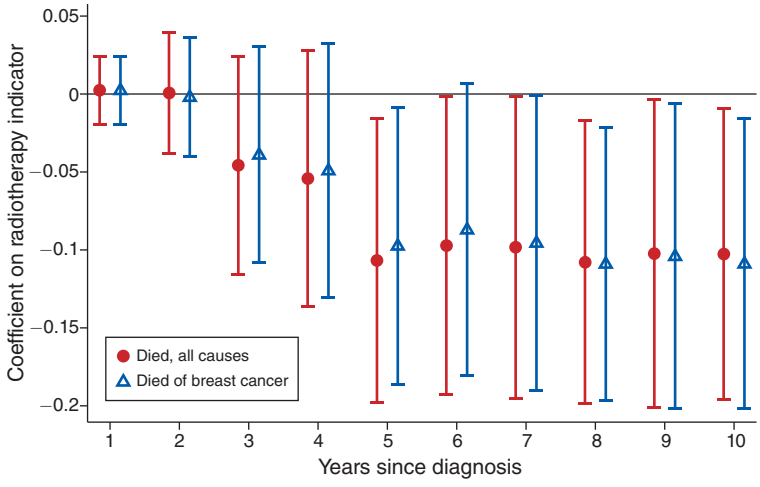

FIGURE 3. EFFECTS OF RADIATION THERAPY ON MORTALITY, 2SLS ESTIMATES

Notes: Each point and vertical segment represent the 2SLS estimate and its corresponding 95 percent confidence interval for the coefficient of the radiotherapy indicator from a different regression of the outcome indicated, measured at the time shown on the horizontal axis, on all possible interactions among the following indicators: having mastectomy, being younger than 45 years of age at diagnosis, the number of positive nodes (zero, one–three, four+), having a tumor larger than 50 mm, and having the tumor removed microradically; separate indicators for age at diagnosis in full years, tumor size in centimeters, the number of positive nodes (zero, one–three, four+), having the tumor removed microradically, the type of surgery (lumpectomy, mastectomy, or lumpectomy followed by mastectomy), and the type of chemotherapy received (DBCG89 clinical trial arm); and year-of-diagnosis fixed effects. The instrument is the interaction between an indicator for the woman belonging to the *T*95 group and an indicator for being diagnosed in or after 1995. Standard errors are clustered at the hospital level.

We next turn to effects on mortality. Figure 3 plots the 2SLS coefficients on the indicator for radiotherapy and corresponding 95 percent 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 triangles represent effects on breast cancer mortality. Regression coefficients corresponding to Figure 3 are provided in the first two rows of Supplemental Appendix Table A5, while the first two rows in Supplemental 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 percent level starting from 5 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 percent 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 4 the 2SLS coefficients on the indicator for radiotherapy and corresponding 95 percent confidence intervals from separate models with labor market outcomes, measured at the time indicated on the horizontal axis. Corresponding regression coefficients

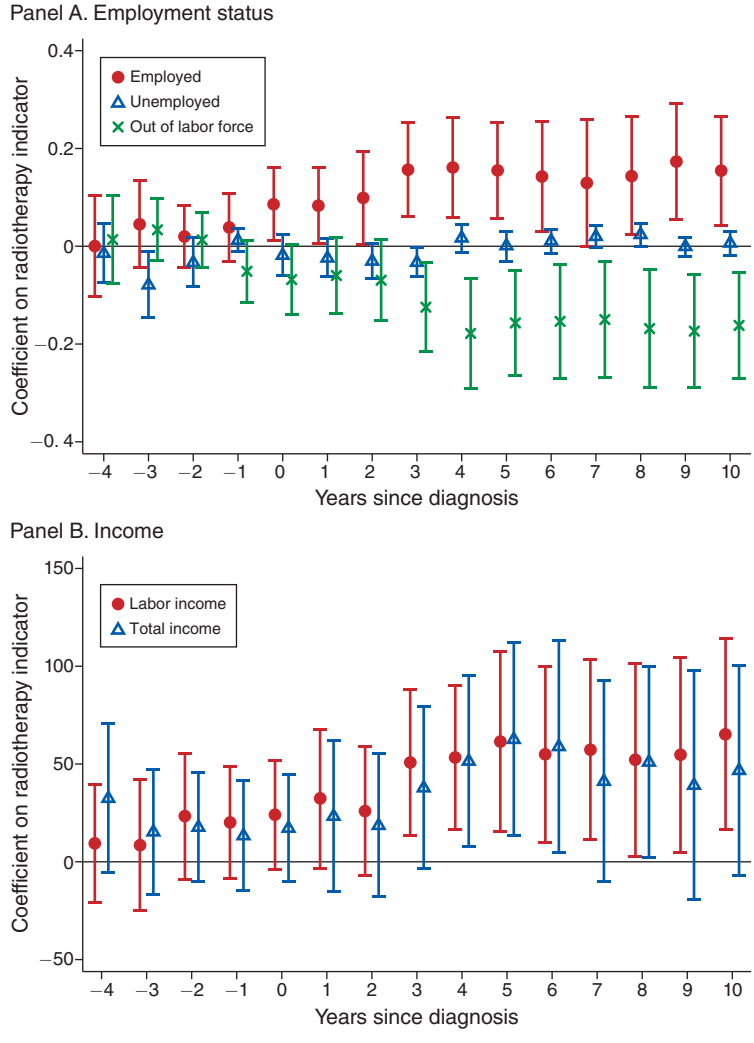

FIGURE 4. EFFECTS OF RADIATION THERAPY ON LABOR MARKET OUTCOMES, 2SLS ESTIMATES

Note: See the notes to Figure 3.

are provided in rows 3–7 of Supplemental Appendix Table A4 and the reduced-form results are presented in rows 3–7 of Supplemental Appendix Table A5. In Figure 4 panel A, circles, triangles, and crosses represent effects on the likelihood of being employed, unemployed, and out of the labor force, respectively. We provide estimates from four years before through ten years after diagnosis. If the assumptions of our difference-in-difference model are correct, we should see little effect in the four prediagnosis years. We find that radiation therapy leads to statistically significant increases in the probability of employment. The magnitudes are sizable, ranging from 8.6 percentage points (11 percent at the mean) in the first year after diagnosis to 15.5 percentage points (37 percent) 10 years after. The rise in employment is entirely due to a reduction in the likelihood of exiting the labor force.

Figure 4 panel B focuses on our measures of income, with circles representing effects on annual labor earnings and triangles 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 25,864 kr–65,107 kr ($3,845–$9,679) and in annual gross personal income of about 18,745 kr–62,744 kr ($2,787–$9,327) during the ten years following cancer diagnosis. These are economically large gains representing 13–45 percent of average annual labor earnings and 7–27 percent of average gross personal income.

Focusing on the coefficients in the prediagnosis periods in Figure 4, there is no persistent pattern in the results, and only one of the 20 coefficients is statistically significant—a result expected due to chance with a p-value of 0.05.

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 4, evidence on the effects of radiation therapy on government transfers.[13] 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 percent 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 statistically significant.

C. *Comparing the Estimated Effects to the Existing Literature*

The DBCG82 clinical trial examined the impact of adding radiotherapy to chemotherapy among high-risk premenopausal women diagnosed with breast cancer between 1982–1989. How do our estimated mortality effects compare to those documented in the DBCG82 randomized clinical trial that led to the 1995 guideline change? Overgaard et al. (1997) report that the ten-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 RCT 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 ten years earlier, which suggests that the returns to radiotherapy did not diminish during this period.

[13] Supplemental Appendix Table A6 provides the corresponding reduced-form estimates.

Table 4—Effects of Adjuvant Radiation Therapy on Government Transfers, 2SLS Estimates

	Years since diagnosis	
	One–five (1)	Six–ten (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 based on equation (1), estimated in the full analysis sample (observations = 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 among the following indicators: having mastectomy, being younger than 45 years of age at diagnosis, the number of positive nodes (zero, one–three, four+), having a tumor larger than 50mm, and having the tumor removed microradically; separate indicators for age at diagnosis in full years, tumor size in centimeters, the number of positive nodes zero, one–three, four+), having the tumor removed microradically, the type of surgery (lumpectomy, mastectomy, or lumpectomy followed by mastectomy), and the type of chemotherapy received (DBCG89 clinical trial arm); and year-of-diagnosis fixed effects. The instrument is the interaction between an indicator for the woman belonging to the *T*95 group and an indicator for being diagnosed in or 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.

Our results suggest that radiation therapy has major economic benefits: It increases the probability of employment by 11–38 percent, it improves labor earnings by 13–45 percent, and it mitigates the cumulative risk of being on welfare by 33–41 percent. These effect sizes are generally comparable to those found in other studies evaluating the economic effects of medical treatments. For example, Biasi, Dahl, and Moser (2023) focused on the pharmaceutical treatment of bipolar disorder and find that access to lithium by age 20 increases labor market participation by 30 percent and earnings by 26 percent. 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 percent. 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 percent, while its removal from the market increased sickness absence days by 12–16 percent.

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 1 to 10 years after diagnosis. These employment gaps are larger than those found in the United States (Bradley, Bednarek, and Neumark 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 percent.

D. *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 examine the observable characteristics of women in the $T95$ and in the comparison group who are diagnosed before the guideline change (see Supplemental Appendix Table A7). Since the 1995 guideline change targeted patients based on clinical characteristics and age at diagnosis, we not surprisingly 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.

Second, we plot the raw data on the evolution of labor market outcomes.[14] We observe each patient's outcomes for a total of 15 years: four years prior to diagnosis, the diagnosis year, and ten years after. We combine patients into four different groups: women in the $T95$ group diagnosed before and after 1995, and women in the control group diagnosed over the same periods. We then generate means by year in relation to diagnosis. Figure 5 presents the results. We find that all four groups have very similar prediagnosis patterns and levels for all outcomes. All groups experience a decline in employment and earnings after diagnosis. The pre-1995 $T95$ group has a more severe decline in employment and earnings after diagnosis, so the treatment effects we are estimating are generated by these negative events not occurring. The large decline in economic outcomes for the $T95$ group diagnosed in the pre-1995 period is not surprising—as discussed before, these women have worse clinical characteristics and are older on average.

[14] We are unable to produce these graphs for welfare use as the data on government transfers begin in 1991.

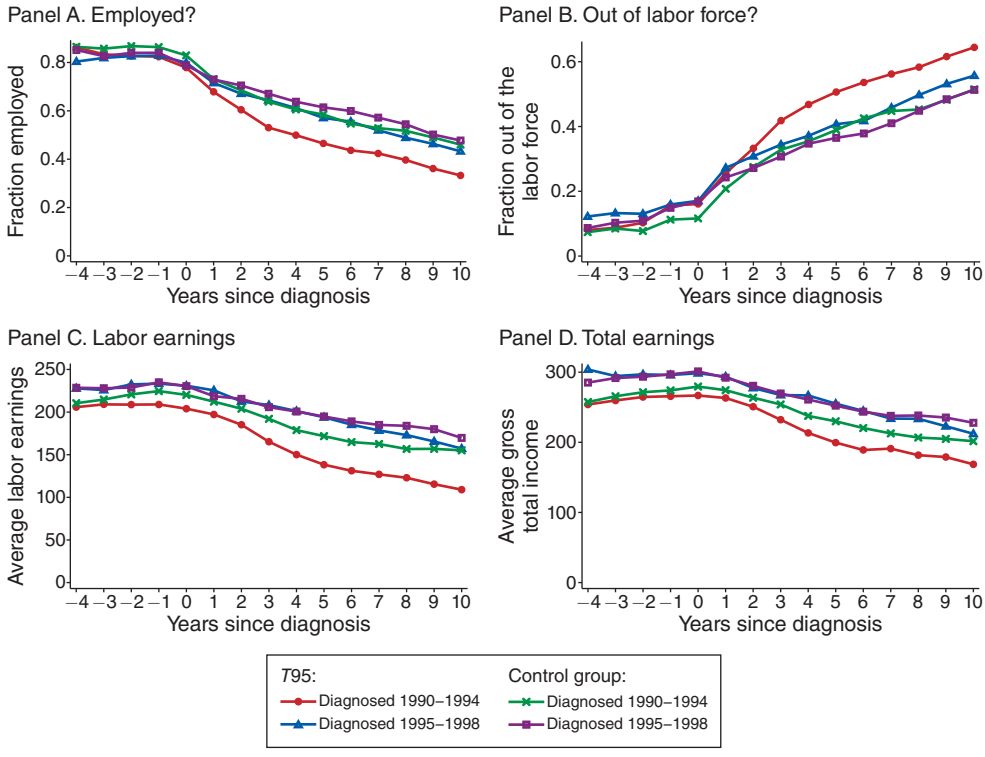

FIGURE 5. THE EVOLUTION OF OUTCOMES IN THE *T*95 AND CONTROL GROUP BY YEAR SINCE DIAGNOSIS

Notes: Each point represents the average of the outcome indicated in the panel, calculated among the group of women indicated in the legend and measured at the time indicated on the horizontal axis.

Third, we estimate our baseline model using prediagnosis outcomes as the dependent variable.[15] We report these estimates in Figure 4 panel A for employment, unemployment, and out of the labor force, and in Figure 4 panel B for labor and total income. There are 20 prediagnosis parameters and only one is statistically different from zero (three years before diagnosis in unemployment), which is the expected type II error rate for a *p*-value of 0.05. In general, there are little persistent pretreatment trends in the outcomes that appear to be contaminating the postdiagnosis results.[16]

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 *T*95 group after 1995. As noted, this assumption is assured due to the institutional setup. Women in our sample are part of an ongoing RCT and we have information on the systemic therapies they received. In order to provide suggestive evidence on the plausibility of this assumption, we further estimate a placebo regression: We restrict the sample to women in the always- and never-treated groups, we

[15] Since everyone lives until the year of diagnosis, we cannot estimate these models for mortality. We are also unable to examine effects on prediagnosis welfare use as the data on government transfers begin in 1991.

[16] We also estimated our baseline model using demographic characteristics that are not tied to radiation therapy eligibility as the dependent variable. The results presented in Supplemental Appendix Table A8 indicate no statistically significant effects on these predetermined characteristics.

assume that the always-treated group experiences the medical guideline change, and we reestimate our reduced-form regressions. If our baseline estimates pick up an improvement in the outcomes of women diagnosed after 1995 unrelated to the effectiveness of radiotherapy (but possibly correlated with the receipt of radiotherapy), then we would likely see an association between the guideline change and the outcomes of always-eligible women as well. However, we find no evidence that the guideline change led to statistically significant differences in the outcomes of always-eligible women relative to never-eligible women (see Supplemental Appendix Figure A3). In addition, the estimated effects have the wrong sign and are substantially smaller in absolute value than our baseline reduced-form estimates: by a factor of 2–3 in the case of income and 11–16 in the case of employment and labor force participation.

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 percent 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, prediagnosis income, marital status, and predicted mortality risk. The results presented in Table 3 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. The results are presented in Supplemental Appendix Figures A4 and A5.[17] 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. 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.[18] We next check if the way we selected the analysis sample has any influence on our estimates and show that the estimates are robust when

[17] To enhance readability, we present estimates grouped one–five years after diagnosis and six–ten years after diagnosis, rather than providing yearly estimates since diagnosis.

[18] 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.

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.[19] Finally, we explore the sensitivity of the results to the choice of the control group and confirm that the results are robust to using either always-eligible or never-eligible women as a control group alone.

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 are 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.

E. *Potential Mechanisms behind the Labor Market Improvements*

Given the large gains documented for both mortality and labor market outcomes, a natural question is whether the improvements in economic outcomes may be driven by selective survival. We provide four pieces of evidence to suggest that this is not the case. First, we note that the labor supply effects appear from the first years after diagnosis when the large mortality benefits have not yet been realized.

Second, we ask what the labor market outcomes of nonsurvivors would need to have been to eliminate our baseline effects. In particular, we eliminate the mortality gains from radiotherapy by randomly selecting a number of nonsurvivors in the $T95$ group in the preintervention period (before 1995).[20] The number of selected nonsurvivors corresponds to the estimated mortality decline for that period (e.g., five or ten years after diagnosis). We then assign to these recoded nonsurvivors a labor market outcome based on a specific value in the distribution among survivors in the $T95$ group in the postperiod (after 1995) and reestimate our baseline specification. We repeat these steps 100 times and save the estimated effects. We repeat the procedure for multiple points along the distribution among survivors in $T95$ in the postperiod: We use employment rates between 0–100 percent and zero–ninetieth percentiles of labor earnings in steps of 10 percentiles. Finally, we plot the average of the estimated effects and the corresponding 95 percent confidence intervals against the values chosen for the outcomes of nonsurvivors. We find that when the outcomes of nonsurvivors are the same as the average employment rate or median labor income of survivors in $T95$ in the post-1995 period, the estimated effect sizes are reduced to around 40 percent of the baseline estimates but are still statistically and economically significant. In all cases, the effects of radiotherapy are eliminated only if nonsurvivors are drawn from the extreme right tail of the distributions (see Supplemental Appendix Figure A6).

[19] 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.

[20] This procedure is similar to one used by Bharadwaj, Løken, and Neilson (2013), who examine whether the test scores gains from medical treatments received by very low birth weight infants are due to selective mortality. For brevity, we report the results for two of our main outcomes: employment and labor earnings, five and ten years after diagnosis.

Third, we show that the baseline effects on labor market outcomes are not due to the fact that nonsurvivors are recorded to have no employment and no income. To that end, we estimate our baseline model using only survivors and show that these estimates are similar to our main results (see Supplemental Appendix Table A9).

Finally, we explicitly model the role of mortality in determining labor market outcomes by estimating a Heckman selection model via the control function approach, using a procedure suggested by Schwiebert (2015). This requires that we have variation that determines mortality but not labor supply. To this end, we add to the mortality equation (the selection equation) the triple interactions between the $T95$ indicator, the $Post95$ indicator, and (i) an indicator for having a tumor larger than 5 centimeters (cm) or (ii) indicators for the type of surgery (i.e., breast conserving or mastectomy). These variables strongly predict mortality but should not necessarily be predictive of labor supply.[21] The effects of radiation therapy on labor market outcomes estimated through this method are slightly smaller than our baseline estimates five years after diagnosis but of virtually the same magnitude ten years after diagnosis (see Supplemental Appendix Table A9). This suggests that the nonsurvivors in the $T95$ group diagnosed before 1995 come from the left tail of the distributions of labor market outcomes.

We next consider whether some of the labor supply effects could be driven by recurrence. If treatment reduces recurrence, even if the reemergence of cancer is not fatal, it could reduce labor supply. Overgaard et al. (1997) report that radiation therapy reduces the cumulative risk of recurrence ten years after diagnosis by 21 percentage points. We find that radiotherapy reduces the likelihood of recurrence five–ten years after diagnosis by 14–21 percentage points in our sample.[22] In order to check whether recurrence may drive the labor market exits, we use recurrence in a given year as the outcome in our baseline model and compare the effect of radiotherapy on recurrence to its effects on employment and labor force participation. We find that the estimated effects have very different patterns. For example, most of the reduction in recurrence occurs in the first two years after diagnosis, while the employment and income benefits tend to be experienced throughout the entire period (see Supplemental Appendix Figure A7). This suggests that recurrence may have a limited role in explaining the labor market benefits of radiation therapy.

What then might explain the effects on labor market outcomes? We argue that there is a simple economic answer as to why labor market outcomes improve.[23]

[21] For computational reasons, we also need to replace the full set of interactions between the indicators for clinical characteristics determining radiotherapy eligibility with the $T95$ indicator and the set of year-of-diagnosis fixed effects with the $Post95$ indicator. This change leaves our baseline results virtually unchanged (see Supplemental Appendix Table A9). The full details of the Heckman model are given in Supplemental Appendix Section A2.

[22] The clinical data from DBCG do not include full information on recurrence as follow-up is incomplete. In order to create a consistent measure of recurrence, we supplement the DBCG data when there is no follow-up with hospital discharge data and classify a patient as experiencing recurrence in a given year if they are flagged in the clinical data or if they have an inpatient admission for breast cancer in that year.

[23] We also considered whether additional treatments may offer mental health benefits. Unfortunately, we are not able to test this with the administrative data as information on mental health care utilization starts only in 1995. The existing literature on this topic, however, suggests that it may not play a large role: Previous RCTs show no differences in key nonclinical outcomes such as quality of life, pain, body image, anxiety, or depression for those treated with or without radiotherapy (Whelan et al. 2000; Pignol et al. 2016; Rayan et al. 2003; Velikova et al. 2018).

Dynamic models of retirement show that a reduction in mortality will, holding all else constant, typically lead to a later retirement age (Kalemli-Ozcan and Weil 2010; Bloom, Canning, and Moore 2014; Chen and Lau 2016). Kalemli-Ozcan and Weil (2010) label this the "horizon" effect, which is simply that with increased life years, individuals must finance additional lifetime consumption, which increases worklife.

Models specific to cancer treatment suggest this same pattern of results between increased life expectancy and time at work. Jeon and Pohl (2019) attempt to explain the negative effects of a breast cancer diagnosis on labor market outcomes by building a Grossman-style model (Grossman 1972) where time can be spent in work, home production, investing in health, or unproductive sick time. Exogenous improvements in health via technology free up time investing in health and time spent sick. As this increases full income, and assuming consumption is a normal good, this would encourage individuals to allocate more time to the labor market so as to increase consumption.

While we do not have suitable outcomes from administrative data to explicitly test the predictions of Jeon and Pohl (2019), we shed light on the possibility that radiotherapy improves general health by examining its effects on (inpatient and outpatient) hospital contacts. In particular, we calculate the cumulative number of inpatient and outpatient contacts since diagnosis, using the exact dates of diagnosis and hospital visits, and use this as the outcome in the baseline equation. As before, we assign zero visits to nonsurvivors after death. Consistent with our conjecture, we find sizable reductions in visits: Radiotherapy leads to 2.6 (20 percent) fewer hospital contacts during the 10 years after diagnosis (see Supplemental Appendix Figure A8).

V. 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. 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 percent 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 percent more likely to be employed and earn 45 percent 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. Finally, we provide suggestive evidence that the labor market benefits of radiotherapy are not due to selective survival or the incapacitation effects of recurrence, but rather to the fact that women are in better health, which encourages them to spend more time in the labor market so as to increase consumption.

Given that an increasing share of breast cancer patients are diagnosed during their working years, understanding the effects of cancer treatments on socioeconomic

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.

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