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Long-Term Employment Effects of Surviving Cancer[1]

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Abstract

We compare employment and usual hours of work for prime-age cancer survivors from the Penn State Cancer Survivor Survey to a comparison group drawn from the Panel Study of Income Dynamics using cross-sectional and difference-in-differences regression and matching estimators. Because earlier research has emphasized workers diagnosed at older ages, we focus on employment effects for younger workers. We find that as long as two to six years after diagnosis, cancer survivors have lower employment rates and work fewer hours than other similarly-aged adults.

Keywords

cancer; employment; matching

1. Introduction

As improvements in cancer screening and treatment intersect with increasing cancer incidence attributable to population aging, cancer survivorship is emerging as a new chronic condition in the United States and other developed countries (Rowland, 2004). In the U.S., the average 5-year survival rate has reached 64% (Jemal et al., 2004). As the number of cancer survivors approaches 12 million (Office of Cancer Survivorship, 2009), understanding and minimizing the long-term effects of cancer and its treatment on the health and well-being of survivors has become a priority (The President's Cancer Commission,

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2004; Hewitt, Greenfield and Stovall, 2005; Centers for Disease Control and Prevention and the Lance Armstrong Foundation, 2004).

Among the long-term outcomes under investigation, employment stands out as especially important for survivors and for society as a whole (Steiner et al., 2004; American Cancer Society, 2009). For society, as improved survival reduces the economic burden associated with cancer mortality, productivity losses due to disability and reduced employment are likely to increase. At an individual level, any disruption of employment resulting from cancer and its treatment threatens the incomes and economic well-being of survivors and their families. Beyond these economic considerations, work figures importantly in the psychological and social well-being of survivors because of its links to personal identity, self-esteem, life purpose, and social relationships (Peteet, 2002; Holahan, Holahan, and Wonacott, 1999).

In the short term, during the initial treatment of newly diagnosed patients, cancer clearly takes a major toll on employment (Short, Vasey, and Tunceli, 2005; Bradley et al., 2005a; Bradley et al., 2005b; Kessler, 2001). There is also a good deal of evidence that long-term survivors report more health-related limitations in ability to work compared to other adults (Bradley and Bednarek, 2002; Chirikos, Russell-Jacobs, and Cantor, 2002; Bradley et al., 2005 a,b; Short, Vasey and BeLue, 2008; Yabroff et al., 2004; Hewitt, Rowland, and Yancik, 2003). However, the extent of cancer's long-term effects on employment itself is less clear, an apparent contradiction that could be explained by survivors who continue to work in spite of impairments and disabilities. For example, a 2002 study of cancer survivors with leg amputations (Hoffman, Saltzman, and Buckwalter, 2002) found that they were employed at the same rate as age- and gender-matched controls, despite demonstrably lower physical health and functioning scores.

A recent meta-analysis (de Boer et al., 2009) identified 15 U.S. studies published between 1966 and June 2008 that compared employment rates (or, in some instances, disability rates) for cancer survivors and healthy controls. Five U.S. studies with particularly strong research designs (Steiner, 2004) have compared cancer survivors who were working at diagnosis to other adults with a similar employment history. Two of these studies, by Bradley and colleagues, of female breast cancer (Bradley et al., 2005a) and prostate cancer (Bradley et al., 2005b) survivors in Detroit, found that significant reductions in employment rates for cancer survivors 6 months after diagnosis did not persist after 12 or 18 months (Bradley et al., 2007). A third study of older working breast cancer survivors in the Health and Retirement Study (HRS) found that the employment rate of survivors was reduced by 7 percentage points, but that hours, wages, and earnings of survivors who continued to work were significantly higher compared to working women in the control group (Bradley, Bednarek, and Neumark, 2002). A fourth study by Chirikos, Russell-Jacobs, and Jacobsen (2002) found that the employment rate of female breast cancer survivors was reduced by about 6 percentage points after five years. The fifth study compared older survivors (those aged 55–65) in the Penn State Cancer Survivor Survey, the data source utilized in this article, to older adults in the HRS with no history of cancer (Short, Vasey and Moran, 2008). In that study, the employment rate of older female survivors 2–6 years after diagnosis was reduced by 4 to 8 percentage points, while the employment rate for older male survivors was not significantly lower than the employment rate for similarly-aged men who were not cancer survivors.

Although eligibility for Bradley's study of prostate cancer survivors was not limited to older workers, the mean age of the survivors in that study was 56. Consequently, none of the better designed studies of U.S. cancer survivors provides much information about the employment of younger male survivors. Furthermore, the two studies that included younger women were limited to breast cancer survivors.

This study was designed to extend the literature on employment and long-term[2] cancer survivorship in the U.S. by focusing on younger, prime-age workers of both genders. It also includes survivors of virtually all types of cancers. We focus on younger survivors, those between the ages of 28 and 54, because in the absence of substantial financial assets, cancer may take a larger toll on one’s economic well being, and because retirement from the labor force is probably a less attractive option, overall, for workers in this age group. The research design matches survivors from the Penn State Cancer Survivor Survey with controls identified from the Panel Survey of Income Dynamics (PSID) on initial employment (i.e., at diagnosis in the survivor sample) and provides separate estimates for cancer-free survivors and those with second cancers or recurrences.[3]

The paper proceeds as follows. In Section 2, we describe our data and empirical approach. Section 3 presents estimates of the effects of cancer survivorship on employment, fulltime employment, and usual weekly hours for prime-age workers. Section 4 concludes with a discussion of how our findings contribute to the small, but growing, literature on the labor market consequences of cancer. Several limitations of the analysis are also discussed.

2. Data and Methods

2.1 Data

Cancer Sample—The Penn State Cancer Survivor Survey (PSCSS) was a longitudinal study of nearly 1800 cancer survivors who were identified from the tumor registries at three large medical centers in central and northeastern Pennsylvania and at Johns Hopkins Hospital in Baltimore, Maryland. The research protocol was approved by Institutional Review Boards at Penn State’s University Park campus and each hospital. The subjects were first diagnosed with cancer during the 3-year period from 1997 through 1999. They were surveyed in the first of four annual interviews from October 2000 to December 2001. For the analyses in this article, we compared employment data from the second PSCCS interview, conducted in 2002, with PSID data for 2002.

Patients with all types of cancer (except superficial skin cancers) were eligible for the cancer survey. However, most cases diagnosed at Stage 4 were excluded, except for patients with Stage 4 leukemia, lymphoma, and plasma cell cancers who had a good chance of surviving to the end of the study. Because male urological cancers were not entered in the main cancer registry at Johns Hopkins, they were also excluded from the survey. Eligibility was further restricted to adults of working age (25 to 62 years old at diagnosis) and to subjects who could be interviewed in English.

About 5000 cases, 2000 from Johns Hopkins and 3000 from the Pennsylvania registries, met the eligibility criteria and were contacted by hospital employees who solicited participation in the study and obtained informed consent. Of the 43% of eligible surviving cases who gave consent (2076 subjects), 88% (1763 subjects) were successfully interviewed (Short, Vasey, and Tunceli 2005). Eighty-nine percent of surviving subjects who completed the first interview also completed the second (1511 cases).

[2]We use the phrase “long-term” mainly to distinguish between survivors who are in treatment and those who have survived beyond that stage. In our sample, time since diagnosis ranged from 26 to 71 months. The vast majority of survivors will have completed treatment for their initial cancer by this point. A subgroup analysis was performed for survivors who may have been in active treatment due to recurrences or second cancers.

[3]A previous study, based on the same sample of cancer survivors, examined differences in employment and work disability related to cancer site, stage at diagnosis, and treatment status (Short, Vasey, and Tunceli, 2005).

Using de-identified data for non-participants (Short and Mallonee, 2006), we investigated concerns raised by the low participation rate by identifying correlates of non-participation and testing multivariate models for non-participation biases. Although differences in participation by gender, race, cancer site, and facility were statistically significant in univariate analyses, propensity scores for survey participation were not significant in multivariate models similar to those estimated here. Further examination of attrition after the first interview found significant differences related to age, nonwhite race, any college education, poverty, and treatment at the facility with the lowest initial participation rate.

Our analyses rely on the date of diagnosis, along with cancer site and stage, recorded in the cancer registries. The survey determined the person's cancer at each interview by asking two questions: “Since you were first diagnosed in [month, year of diagnosis], has your cancer spread to other parts of your body?” and “Since you were first diagnosed, have you had any new cancers?” We used the responses to distinguish between survivors “with new cancers” (encompassing metastases, recurrences, and second primaries) and cancer-free survivors. Retrospective questions in the first interview asked about employment and job characteristics, health insurance, and marital status at diagnosis. Current employment status and usual hours per week were determined at each of the four interviews.

After the 2002 cancer sample was restricted to individuals aged 28–54 who were working at diagnosis, there were 673 cancer cases available for comparison to the PSID: 562 cases who were cancer-free, 110 cases with new cancers, and one with missing data on new cancers. At the second interview, 68% of the cancer sample was 3 to 5 years post-diagnosis. Time from diagnosis ranged from 25 to 71 months (mean = 46, standard deviation = 11).

Comparison Group—The Panel Study of Income Dynamics is an ongoing longitudinal study conducted by the Institute for Social Research at the University of Michigan. It has collected data from the same families and individuals since 1968. The PSID is designed to maintain a nationally representative sample of individuals and families in the U.S. population over time by following the children of initially-sampled families as they form new households, thereby mimicking the process of family formation in the population. A sample of immigrant families was added in 1997 to maintain the representativeness of the sample in the face of increased immigration into the U.S. Since 1997, interviews have been conducted every two years (in odd years) to collect information about the previous (even) year.

We used the 2003 PSID family and individual files to obtain information on employment and other data elements for 2002.[4] Because eligibility for the cancer survey was limited to individuals born from 1935 to 1974, inclusive, we also restricted the PSID comparison group to people born in those years (who were 28 to 67 years old in 2002). Although the PSID questionnaire obtains extensive information for the householder and (if applicable) the householder's spouse, there is so little information about other adult members of the family that we were forced to exclude them from our analyses. We also dropped PSID subjects with any history of cancer (ignoring superficial skin cancers) from our analyses. Finally, because we required information for the PSID sample over the same time period as the cancer sample, we restricted our analytic PSID sample to primary adults with data in the 1997, 1999, 2001, and 2003 family files (corresponding to reference years 1996, 1998, 2000, and 2002 respectively).

[4]Some of the data used in this analysis are derived from Sensitive Data Files of the Panel Study of Income Dynamics, obtained under special contractual arrangements design to protect the anonymity of respondents. These data are not available from the authors. Persons interested in obtaining PSID Sensitive Data Files should contact the PSID at psidhelp@isr.umich.edu.

To characterize employment status in the PSID sample at a "baseline" comparable to the calendar month of diagnosis in the cancer sample, we assigned baseline calendar months to the PSID sample by randomly drawing from the distribution of diagnosis months observed for the cancer sample (January 1997 through December 1999). This procedure has been used in other studies comparing employment trajectories over time for cancer survivors and non-cancer controls (Bradley, Bednarek, and Neumark, 2002; Short, Vasey and Moran, 2008). We determined employment and job characteristics in the baseline month from monthly questions in the PSID about individual employment in the reference year, along with job beginning and ending dates. We also made use of monthly earnings for 1997 and 1999 reported in supplemental "t-2 individual income files" constructed by the Institute for Social Research from the 1999 and 2001 interviews to fill between reference years. After the PSID sample was further restricted to people aged 28–54 who were working at baseline, there were 4141 non-cancer cases for analysis.

2.2 Employment Outcomes

To estimate the effect of cancer on employment, we compared prime-age cancer survivors, those ages 28–54 in 2002, to otherwise similar adults based on three employment outcomes observed in 2002: the probability of working; the probability of working full time (35+ hours in a usual week); and usual hours per week. In the PSCSS, working was defined by respondents selecting the first option when asked, "Now I am going to ask you some questions about your current employment situation. Are you working for pay, temporarily laid off or on leave, unemployed and looking for work, retired, disabled and unable to work, a homemaker, a student or something else?" In the PSID, working was defined based on answers to the question, "In which months during [year] were you working for [each employer identified during the year]?" All models were estimated separately by gender because of the different underlying employment tendencies of men and women.

2.3 Methods

We seek to estimate the average effect of treatment on the treated (ATT), in this case, the average effect on employment and hours of having survived cancer, among those who have survived cancer. This parameter measures the consequences, in terms of reduced employment and hours, of having survived cancer for current cancer survivors, which could differ from what the consequences would be if the incidence of cancer were to shift. However, the ATT is a useful parameter for comparing the costs and benefits of policies and clinical interventions designed to promote greater labor market participation on the part of today's cancer survivors, or on the part of future survivors who are similar to today's survivors.

Because cancer histories are not subject to random assignment, and it is difficult to conceive of instrumental variables in this context, our analysis rests primarily on a "selection-on-observables" assumption (Heckman and Robb, 1985). In our case, this corresponds to assuming that after conditioning on a set of observable covariates, potential employment outcomes would be the same for those who did, and did not, develop cancer. The extent to which cancer survivors differ from others in their unobservable propensity to work is hard to determine *a priori*. For the subset of cancers that have been linked to lifestyle choices, one could argue that forward-looking individuals are likely to invest more heavily in both their health and human capital, perhaps in ways not captured by our control variables. If that conjecture is correct, cross-sectional estimates will overstate the negative effect of cancer on employment.5

The selection-on-observables assumption can be relaxed if longitudinal data are available that include observations from both the pre- and post-treatment periods. In such a setting, standard difference-in-differences (DD) estimators, as well as the semi-parametric difference-indifferences matching estimators developed by Heckman, Ichimura, Smith and Todd (1998), can be used to eliminate any time-invariant differences between the treatment and comparison groups, thereby allowing for selection on both observed characteristics and unobserved characteristics that are constant over time. Baseline (at time of diagnosis) employment and hours were measured through retrospective interviewing of cancer survivors in the PSCSS, and we use that information to construct DD estimates of the effect of surviving cancer on the employment outcomes that vary at both baseline and follow-up: full-time employment and usual weekly hours.[6] We also present cross-sectional estimates based on employment outcomes measured at follow-up.

As a first cut at adjusting for observable differences between cancer survivors and others we regressed each of our three outcome variables on an indicator for cancer survivorship and the same set of covariates included in the matching models. For discrete outcomes, such as the probability of working or the probability of working full-time, our cross-sectional regression-based estimates were derived from Probit models, while estimates for usual weekly hours came from a Tobit model.[7] In the case of our DD estimates, we estimated the regression models using OLS and rely on the estimates obtained from our semiparametric DD matching estimator, as well as those derived in the absence of covariates, as a check on the linearity assumption.

Kernel matching has been shown to perform well in settings, such as ours, where the ratio of comparison to treated observations is 4:1 or better (Frolich, 2004).[8] The matching estimates presented below are based on the Epanechnikov kernel and a bandwidth of 0.06, which are the defaults for kernel matching using PSMATCH2.[9] To ensure that only comparable individuals were compared, we imposed the common support condition using a method known as “trimming” that removes *q* percent of the treated observations whose matched control observations have an estimated density below an endogenously-determined cut-off (Heckman, Ichimura and Todd, 1997). We used a trimming level of 2 percent.

Robust standard errors were estimated for all regression models. For the matching estimators, we display two sets of standard errors: the analytic standard errors from the PSMATCH2 program and bootstrapped standard errors that incorporate the estimation error in the propensity scores. Each of the 500 bootstrap replications was based on the combined estimation of the propensity scores and the matching estimator in question.[10]

[5]We also examined the sensitivity of our estimates to dropping survivors with lung cancer and head/neck cancer, two malignancies that have a known behavioral component through their association with smoking and heavy drinking. Because of its relatively poor prognosis, we had very few lung cancer cases in our sample of long-term survivors. Even after combining those with lung and head/neck cancer, we only had 19 and 18 cases for females and males, respectively. Omitting these individuals from our analysis had virtually no effect on the resulting estimates.

[6]It is conceivable that at the time of diagnosis baseline hours of work might already reflect the effect of having cancer, due either to time spent seeking a diagnosis or from early symptoms. We believe this is unlikely because the hours measure we use is based on the respondent’s “usual weekly hours,” not their actual hours worked. Thus, someone taking time off for diagnostic tests would have had to view the time off as a reduction in their *usual* hours for it to show up in our baseline hours variable. Moreover, for cancers like breast, colorectal, and prostate, which account for most of our sample, a diagnosis is typically made through screening, prior to the onset of work-limiting symptoms. This is especially true for a sample of long-term survivors, who are typically diagnosed at earlier stages. The effect of capturing initial hours reductions associated with undiagnosed symptoms or diagnostic procedures, if present, would result in our underestimating the negative effect of surviving cancer on usual hours of work.

[7]We also estimated each of these models by ordinary least squares, finding little difference between the marginal effects from the Probit/Tobit models and the corresponding OLS regression coefficients.

[8]We obtained similar estimates using nearest neighbor matching based on the ten nearest neighbors.

[9]Heckman, Ichimura and Todd (1997) also use a bandwidth of 0.06. As a robustness check, we experimented with a Gaussian (Normal) kernel and obtained similar results.

[10]Bootstrapping may not produce valid standard errors for “nonsmooth” matching estimators, such as the nearest neighbor estimator (Abadie and Imbens, 2008), but should lead to valid inferences for “smooth” estimators, such as those based on kernel matching.

2.4 Estimation

Propensity Scores—Because the PSCSS was drawn from a limited geographic area, while the PSID is national in scope, there are several (observable) differences between our cancer and comparison group samples that must be accounted for. The most important of these relate to urbanicity and measures of socioeconomic status, such as race and education (Table 1). The covariates included in the propensity score models, estimated as Probits, include socio-demographic characteristics such as age, race, marital status, the presence of children under 18 in the home, and educational attainment. We controlled for age with indicators for each year and measured schooling using indicators for less than high school (the omitted category), high school completion, some college, college completion, and any post-college education. The models also include indicators for five common chronic conditions (diabetes, chronic lung disease, heart disease, stroke, and arthritis) at follow-up. 11 To account for any mechanical relationship between the probability of an employment transition and the length of the follow-up period, we controlled for the number of months from diagnosis/baseline to the 2002 interview. To control for the degree of initial labor market attachment, we restrict attention to individuals who were working at baseline, and condition on baseline hours of work (entered as a cubic function), self-employment status, job tenure, and an indicator for managerial/professional/technical occupations, all measured at baseline.

Differences in local labor market conditions have been shown to be an important source of bias when measuring employment effects, most notably in evaluations of job training programs (Heckman, Ichimura, and Todd, 1997). Given the previously noted differences in the geographic distribution and urbanicity of the PSCSS and PSID samples, we obtained county geocodes for the PSID sample to adjust for differences in local job availability with three county-level variables. The first variable was a set of three rural-urban indicators based on groups of Beale codes: (a) counties in metropolitan areas of 1 million population or more (code 1); (b) counties in smaller metropolitan areas (codes 2 and 3); and (c) all nonmetropolitan counties (codes 4 – 9). Another proxy for the size and diversity of the local labor market is population density, which we measured as the population per square mile in the respondent's county of residence. Finally, we included the unemployment rate in the respondent's county of residence. Each of the foregoing variables was measured at follow-up, in calendar year 2002, to coincide with the measurement of employment outcomes.

Our matching estimates were based on propensity scores estimated separately for each gender and for each of the survivor group sub-samples (all survivors, survivors with new cancers, and survivors without new cancers). The dependent variable in each propensity score model was equal to "one" if the respondent was in the treatment group (cancer survivor, survivor with a new cancer, or survivor with no new cancers), and "zero" otherwise. In the models involving the two cancer subgroups (with and without new cancers), the other cancer subgroup was excluded.

3. Results

The estimated propensity score models, which we do not present in the interest of brevity, revealed patterns similar to the univariate cross-sample comparisons depicted in Table 1. Regardless of gender, relative to respondents from the PSID, respondents from the PSCSS were more likely to be white, have a college or post college education, and live in counties with lower unemployment rates. PSCSS respondents were less likely to live in large

[11]In some cases, it seemed more appropriate to measure a variable at follow-up, coincident with when our employment outcomes were measured, rather than at baseline. To check for possible endogeneity, we re-estimated each of our matching models including only variables measured at baseline. Results were similar in all cases.

metropolitan areas and were more likely to have other chronic medical conditions, such as heart disease. Standard *t*-tests for equality of means in the treatment and comparison groups, after matching on the scores, were performed for every covariate in every specification, using the PSTEST procedure in PSMATCH2.[12] In no case was the null hypothesis of equality of the covariates across samples rejected.

Univariate comparisons of employment outcomes for cancer survivors and PSID respondents without a history of cancer are presented in Table 2. Focusing first on cancer survivors as a whole ("all cancer survivors"), survivors of both genders were less likely to work, less likely to work full-time, and worked fewer hours per week than similarly-aged adults with no history of cancer. Not surprisingly, these differences were most pronounced among survivors whose cancers had recurred or who had developed malignancies at other sites ("cancer survivors with new cancers"), and were less pronounced among survivors who remained cancer free ("cancer survivors with no new cancers"). For ease of comparison with our multivariate regression and matching estimates, these mean differences, along with their associated standard errors, are presented in the first column of Tables 3 and 4.

3.1 All Cancer Survivors

We turn first to the estimates for all cancer survivors, displayed in the top panel of Tables 3 and 4. For this group, all differences between cancer survivors and those in the comparison group were significant at the 0.05 level or better. Adjusting for observable differences between the PSCSS and PSID samples, females who survived cancer had employment rates that were 7 to 8 percentage points lower than similarly-aged females from the PSID comparison group, fulltime employment rates that were 6 to 10 percentage points lower, and usual weekly hours that were lower by 3 to 4 hours per week, which represents a 10% reduction in weekly hours relative to the PSID average. For males, the corresponding differences for cancer survivors were −7 to −8 percentage points for employment, −8 to −10 percentage points for full-time employment, and −5 to −6 hours per week, which represents a 12% reduction in usual hours relative to the PSID baseline.

For both genders, the cross-sectional regression and matching estimates were broadly similar to the unadjusted mean differences, suggesting modest selection on the observable characteristics we are able to account for. When the DD variants of our mean difference, regression, and matching estimators were used to estimate survivorship effects on full-time employment and hours,[13] the resulting DD estimates were quite similar for usual hours of work, but were a bit smaller for full-time employment, especially for females. For both males and females, the DD estimates for hours generally differed by less than 1 hour per week relative to their cross-sectional counterparts, while the DD estimates for full-time employment differed by about 1 percentage point for males, but from 2 to 4 percentage points for females. Thus, overall, our estimates appear to be robust to the presence of time-invariant unobserved differences between our sample of cancer survivors and the PSID comparison group.

3.2 Comparison of Survivors With and Without New Cancers

Turning next to the survivor group subsamples (displayed in the middle and lower panels of Tables 3 and 4), we found much larger employment reductions for those with new cancers and smaller, but nontrivial, reductions among those who remained cancer free. For females

[12]In the interest of space, we do not present the balancing tests, which are available upon request from the authors. For information on the PSMATCH2 program, see Leuven and Sianesi (2003).

[13]Because we focus on survivors who were working at the time of diagnosis, DD estimates for working at all would utilize the same variation as our cross-sectional estimates based on employment rates in the follow-up period.

with new cancers, employment rates were 19 to 21 percentage points lower than among similarly-aged females in the PSID; full-time employment rates ranged from 16 to 23 percentage points lower, depending on the estimation method; and usual weekly hours were lower by 8 to 10 hours, which corresponds to a 26% reduction in weekly hours relative to females in the PSID. Effects were similarly large for male survivors with new cancers, with reductions in employment rates of 28 to 30 percentage points, reductions in full-time employment rates of 25 to 28 percentage points, and reductions in usual weekly hours of 16 to 17 hours per week, which corresponds to a 38% reduction in usual hours relative to the PSID average for males.

Employment outcomes for cancer-free survivors were far more favorable, although there were still marked differences relative to those with no history of cancer. For females, employment rates were about 5 percentage points lower; full-time employment rates ranged from 4 to 7 percentage points lower, with the smaller estimates statistically indistinguishable from zero; and weekly hours were lower by a statistically significant 2 to 3 hours per week. For males, the corresponding estimates were −3 to −4 percentage points for employment, −5 to −7 percentage points for full-time employment, and −3 to −4 hours per week, with most estimates statistically significant at the 10% level or better.

As was true for cancer survivors as a whole, the DD estimates for the survivor group subsamples appeared to differ meaningfully from the cross-sectional estimates only for full-time employment among females, suggesting little selection bias attributable to fixed differences between the cancer survivors in the PSCSS and those in the PSID comparison group.

3.3 Sensitivity Analysis

Ichino, Mealli, and Nannicini (2008) propose a simulation procedure for assessing the sensitivity of matching estimates to the selection-on-observables assumption. Their procedure, which is described in more detail in their paper, allows one to simulate the effect on the ATT parameter of an omitted binary variable based on assumptions about the distribution of the omitted variable conditional on treatment assignment and outcome.[14] They propose two types of sensitivity analysis: one that simulates the effect of an omitted variable whose statistical properties mimic those of a given included covariate; and another that determines the magnitude of the selection and outcome effects of the omitted variable, expressed as odds ratios, that would be required to yield an ATT estimate of zero. We adopt the former approach because we believe it provides a more intuitive way of thinking about the influence of potential confounders.

Appendix Tables A1 and A2 display the results of this sensitivity analysis for our cross-sectional and difference-in-differences matching estimates for all cancer survivors, using the SENSATT command in Stata (Nannicini, 2007). We conducted separate sensitivity analyses for each binary covariate with significantly different mean values for the treatment and comparison groups. The estimated ATT parameter for each gender, employment outcome, and simulated omitted confounder is based on 1000 replications of the simulation procedure.[15] Overall, there was little difference between the baseline ATT estimates and the ATT estimates obtained from simulating omitted variables similar to the covariates listed in Tables A1 and A2. The largest difference occurred in the case of female full-time

[14]The procedure can be extended to accommodate continuous outcomes, such as usual weekly hours. For details, see Ichino, Mealli, and Nannicini (2008) or Nannicini (2007).

[15]The small differences between our ATT estimates, calculated using the PSMATCH2 program, and the baseline SENSATT ATT estimates, displayed in the first two rows of Tables A1 and A2, are entirely attributable to differences in the default kernels used by each program and the manner in which the support condition was imposed.

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employment and hours, for an omitted variable that resembles the "nonwhite" indicator. This discrepancy was greatly attenuated when DD matching was used, which is consistent with the DD estimator allowing for selection on time-invariant characteristics such as race.

4. Conclusion

By examining the effects of cancer on the employment of both male and female workers who were diagnosed with cancer at younger ages (28 to 54 years), this study fills an important gap in a growing body of research on the long-term consequences of cancer for the 12 million cancer survivors in the United States. Our estimates of the long-term effects of cancer on the employment of prime-age workers are robust across estimation methods and are similar for male and female cancer survivors. The effect of cancer on the employment rate, averaging across survivors who remain cancer-free and those with new cancers, is 7–8 percentage points. The reduction in full-time employment is similar in magnitude to the reduction in any employment among females, but a bit larger for males. The average reduction in usual hours per week is about 3.5 hours for female survivors and about 5.5 hours for male survivors, including those who stopped working. These new estimates for younger workers are generally not very different from estimates for older workers found previously. For example, using the HRS to estimate cancer effects for older workers (55 to 65 at follow-up), the average reduction in usual weekly hours for survivors was 3–4 hours among older workers (Short, Vasey, and Moran, 2008).

The long-term effects of cancer were markedly larger for survivors with new cancers than for cancer-free survivors. The reappearance of cancer adds considerably to the long-term effects of the disease on the employment of survivors, and it appears that younger male survivors are particularly hard hit by recurrences and second cancers. The cancer effects are not estimated precisely enough for the gender differences to be statistically significant, but they are noticeably larger for male survivors with new cancers compared to female survivors, especially for working at all (−28 to −30 percentage points for males versus −19 to −21 percentage points for females) and usual weekly hours (−16 to −17 hours for males versus −8 to −10 hours for females). These differences may reflect differences in the types of cancers that were initially diagnosed in prime-age males compared to prime-age females (see Table 1). Admittedly, the strikingly larger effects on male survivors could also be a statistical artifact arising from the small numbers of survivors with new cancers in our sample.

For survivors who remain cancer-free, the new estimates provided in this article are perhaps a bit less reassuring than estimates previously published from the Penn State Cancer Survivor Survey for older workers. The average reduction in usual hours for younger, cancer-free survivors was similar in magnitude but statistically significant here. Including those who stopped working entirely, the long-term reduction in the employment of younger, cancer-free survivors averaged 3.5 hours per week. This may not seem like a major disruption in the lives of cancer-free survivors, but it amounts to an 8 to 10 percent reduction in hours worked, and is in keeping with the elevated rate of work disability reported even by survivors who remain cancer free.

Although not nationally representative in terms of geographic location, race/ethnicity, and socioeconomic status, the Penn State Cancer Survivor Survey offers many advantages in studying long-term effects of cancer on employment. The PSCSS captured employment data for a sample of nearly 2000 incident cancer cases prior to diagnosis and extending over a period of several years. The sample was diagnosed in the late 1990's, during an era characterized by major improvements in long-term survival for many types of cancer. Recurrences and second cancers, which have a major impact on employment as we show here, are identified. Furthermore, there are no issues concerning the accuracy of either the diagnosis or the date of diagnosis taken from cancer registries, as would be the case with cancer diagnoses in self-reported surveys. There is currently no national survey that tracks the employment of as large a sample of workers from ages 28 to 54 who were recently diagnosed with cancer.

Some of the limitations of our study (the mixing of retrospective and prospective data collection for survivors, and the reliance on comparison groups drawn from another, national survey), reflect deliberate strategies to reduce the cost of collecting detailed employment data for a large sample of cancer survivors over a long time horizon. Furthermore, while there are great advantages to using cancer registries to identify subjects for survivor studies, sampling from cancer registries also carries special obligations to safeguard the privacy of patients and their right to decline participation. Those obligations take a heavy toll on consent rates and raise concerns about non-participation bias.

Given questions about the comparability of the cancer and non-cancer samples (and data items) taken from different surveys, and the representativeness of the survivors who participated in the cancer survey, we took special pains to consider a wide variety of estimation techniques that account for both observable and unobservable sources of bias, and conducted sensitivity analyses that simulated the influence of omitted variables with statistical properties analogous to the covariates included in our regression and matching models. Also, to account for the geographic differences between the cancer and non-cancer samples, we specifically controlled for local labor market conditions. Nevertheless, while there is a great deal of consistency to the estimates across methods, the possibility of bias remains. Further, because our ATT estimates are specific to the types of cancer survivors represented in our cancer sample, our estimates may understate the effects of cancer on the national population of cancer survivors (which is more urban, racially and ethnically diverse, and educationally and economically disadvantaged) than the cancer survivors in our study.

Finally, two additional points are worth noting. First, because we focus on longer term employment outcomes for survivors, our estimates capture only one component of the overall burden of cancer; we do not account for the costs of treatment, mortality, or employment disruptions associated with initial treatment. Second, our data do not allow us to ascertain whether the observed reductions in employment and hours of cancer survivors are voluntary, reflecting changing labor-leisure preferences, or the result of constraints imposed by others, such as employment discrimination or inadequate workplace accommodations. Determining the relative magnitude of these factors will be important for formulating policy responses to the reduced labor market activity documented here and in other studies.

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Table 1

Characteristics of subjects ages 28–54 working at baseline/diagnosis, by gender and survey

Characteristic	Females		Males	
	Cancer	PSID	Cancer	PSID
Sample size	**463**	**2020**	**213**	**2133**
Cancer Type				
Blood	3%	--	10%	--
Breast	50%	--	0%	--
Central nervous system	3%	--	6%	--
Colorectal	2%	--	11%	--
Head/neck	2%	--	7%	--
Lymphoma	5%	--	12%	--
Other	10%	--	23%	--
Prostate	0%	--	8%	--
Respiratory	2%	--	1%	--
Skin	4%	--	8%	--
Thyroid	10%	--	9%	--
Urinary	0.4%	--	6%	--
Uterus	8%	--	0%	--
Months since diagnosis/baseline (mean)	46*	45	47*	45
Any new cancers	17%	--	15%	--
Chronic conditions				
Diabetes	4%	5%	4%	5%
Chronic lung disease	3%	3%	4%	2%
Heart disease	10%*	3%	10%*	4%
Stroke	1%	1%	1%	1%
Arthritis	24%*	13%	15%*	8%
Full-time work (baseline)	76%	81%	95%	96%
Self-employed (baseline)	10%	8%	11%	12%
Manager, professional, technical (baseline)	52%*	39%	44%	38%
Job tenure in months (baseline)	110*	81	124*	100
Age in years (mean)	46*	42	45*	42
Nonwhite	8%*	40%	5%*	31%
Married/partner (baseline)	74%*	65%	79%	79%
Children < 18	41%*	59%	43%*	57%
Education				
Less than high school	2%*	7%	4%*	9%
High school	28%*	40%	27%*	37%
Some college	26%	28%	16%*	25%
College	23%*	16%	32%*	19%
Post college	20%*	9%	21%*	10%

Characteristic	**Females**		**Males**	
	Cancer	**PSID**	**Cancer**	**PSID**
Sample size	**463**	**2020**	**213**	**2133**
Urbanicity				
Urban	32%*	49%	30%*	48%
Suburban	48%*	34%	46%*	33%
Not urban	20%	17%	24%	19%
County population per square mile (mean)	873*	1726	811*	1534
County unemployment rate (mean)	5.1%*	5.8%	5.2%*	5.8%

PSID : Panel St udy of Income Dynamics. Variables are measured at follow-up, unless otherwise indicated. An asterisk denotes a significant difference between the PSCSS and PSID samples at the 0.05 level.

Table 2

Employment, full-time employment, and usual hours per week in 2002 for subjects ages 28–54 working at baseline/diagnosis, by survey and cancer status

	Females	Males
Panel Study of Income Dynamics (PSID)		
Sample size	2020	2133
Working (percent)	89.4%	95.8%
Working full-time (percent)	72.6%	91.5%
Usual hours per week (mean)	34.7	44.0
All cancer survivors		
Sample size	463	213
Working (percent)	83.4%	88.3%
Working full-time (percent)	61.3%	82.6%
Usual hours per week (mean)	30.8	39.0
Cancer survivors with no new cancers		
Sample size	385	180
Working (percent)	85.7%	92.2%
Working full-time (percent)	64.1%	85.6%
Usual hours per week (mean)	31.8	40.8
Cancer survivors with new cancers		
Sample size	77	33
Working (percent)	71.4%	66.7%
Working full-time (percent)	48.1%	66.7%
Usual hours per week (mean)	25.7	29.2

Table 3

Effect of Cancer Survivorship on Long-Term Employment Outcomes, 2–6 Years Post-Diagnosis: Females (Ages 28–54)

	Cross-Sectional Estimates			Difference-in-Differences Estimates		
	Unadjusted Mean Difference	**Regression Adjusted Difference**	**Kernel Matching 2% Trim**	**Unadjusted DD**	**Regression Adjusted DD**	**DD Kernel Matching 2% Trim**
	I. All Cancer Survivors					
Working	−0.060**	−0.081**	−0.072**			
(% points)	(0.019)	(0.022)	(0.020)	--	--	--
(N = 2483)						
Working Full Time	−0.113**	−0.100**	−0.077**	−0.072**	−0.058**	−0.058**
(% points)	(0.025)	(0.029)	(0.025)	(0.026)	(0.028)	(0.028)
(N = 2468)						
Usual Weekly	− 3.954**	−4.306**	−3.536**	−3.524**	−3.367**	−3.398**
Hours	(0.893)	(0.933)	(0.963)	(0.913)	(0.986)	(0.984)
(N = 2468)						
	II. Cancer Survivors With New Cancers					
Working	−0.180**	−0.209**	−0.191**			
(% points)	(0.052)	(0.058)	(0.057)	--	--	--
(N = 2097)						
Working Full Time	−0.246**	−0.228**	−0.207**	−0.167**	−0.155**	−0.161**
(% points)	(0.058)	(0.067)	(0.062)	(0.063)	(0.063)	(0.070)
(N = 2085)						
Usual Weekly	−9.059**	−10.017**	−8.628**	−8.126**	−7.908**	−8.239**
Hours	(2.258)	(1.943)	(2.463)	(2.343)	(2.341)	(2.581)
(N = 2085)						
	III. Cancer Survivors Without New Cancers					
Working	−0.037*	−0.054**	−0.045**			
(% points)	(0.019)	(0.022)	(0.022)	--	--	--
(N = 2405)						

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	Cross-Sectional Estimates			Difference-in-Differences Estimates		
	Unadjusted Mean Difference	**Regression Adjusted Difference**	**Kernel Matching 2% Trim**	**Unadjusted DD**	**Regression Adjusted DD**	**DD Kernel Matching 2% Trim**
Working Full Time	−0.085**	−0.068**	−0.057**	−0.051*	−0.035	−0.042
(% points)	(0.027)	(0.030)	(0.029)	(0.028)	(0.029)	(0.031)
(N = 2390)						
Usual Weekly	−2.934**	−3.050**	−2.681**	−2.588**	−2.378**	−2.604**
Hours	(0.941)	(0.980)	(1.004)	(0.963)	(1.029)	(1.062)
(N = 2390)						

Notes: Analyses included the following covariates: age, race, education, marital status, presence of children under 18 in the household, presence of specific chronic conditions, usual hours per week at baseline/diagnosis (cubic), self-employment, managerial/technical/professional occupation, job tenure, urbanicity, county population density, the county unemployment rate, and time since baseline/diagnosis. For regression-based estimates, robust standard errors are shown in parentheses. For matching estimates, analytic standard errors are in parentheses; bootstrapped standard errors are in brackets. Bootstrapped standard errors are based on 500 replications of the combined propensity score - matching estimation routine.

* Estimates are statistically significant at $p < 0.10$

** $p < 0.05$.

Statistical significance of the matching estimates is based on the bootstrapped standard errors.

Table 4

Effect of Cancer Survivorship on Long-Term Employment Outcomes, 2–6 Years Post-Diagnosis: Males (Ages 28–54)

	Cross-Sectional Estimates			Difference-in-Differences Estimates		
	Unadjusted Mean Difference	**Regression Adjusted Difference**	**Kernel Matching 2% Trim**	**Unadjusted DD**	**Regression Adjusted DD**	**DD Kernel Matching 2% Trim**
I. All Cancer Survivors						
Working	−0.075**	−0.074**	−0.077**			
(% points)	(0.023)	(0.023)	(0.023)	--	--	--
(N = 2292)						
Working Full Time	−0.088**	−0.099**	−0.087**	−0.078**	−0.087**	−0.089**
(% points)	(0.027)	(0.029)	(0.029)	(0.030)	(0.031)	(0.033)
(N = 2346)						
Usual Weekly	−5.026**	−6.252**	−5.657**	−5.308**	−5.732**	−5.707**
Hours	(1.196)	(1.015)	(1.277)	(1.337)	(1.370)	(1.407)
(N = 2346)						
II. Cancer Survivors With New Cancers						
Working	−0.290**	−0.279**	−0.304**			
(% points)	(0.083)	(0.088)	(0.090)	--	--	--
(N = 2114)						
Working Full Time	−0.248**	−0.276**	−0.273**	−0.255**	−0.263**	−0.268**
(% points)	(0.084)	(0.093)	(0.090)	(0.092)	(0.091)	(0.107)
(N = 2166)						
Usual Weekly	−14.756**	−17.004**	−15.926**	−16.167**	−16.077**	−16.368**
Hours	(3.743)	(2.382)	(4.297)	(4.284)	(4.243)	(4.911)
(N = 2166)						
III. Cancer Survivors Without New Cancers						
Working	−0.035*	−0.033**	−0.036*			
(% points)	(0.021)	(0.019)	(0.022)	--	--	--
(N = 2259)						
Working Full Time	−0.059**	−0.066**	−0.059**	−0.046	−0.053*	−0.059*

	Cross-Sectional Estimates			Difference-in-Differences Estimates		
	Unadjusted Mean Difference	**Regression Adjusted Difference**	**Kernel Matching 2% Trim**	**Unadjusted DD**	**Regression Adjusted DD**	**DD Kernel Matching 2% Trim**
(% points)	(0.027)	(0.028)	(0.029)	(0.031)	(0.032)	(0.033)
(N = 2313)						
Usual Weekly	−3.243**	−4.153**	−3.881**	−3.317**	−3.702**	−3.890**
Hours	(1.183)	(1.066)	(1.295)	(1.315)	(1.333)	(1.484)
(N = 2313)						

Notes: Same as for Table 3.

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Table A1

Sensitivity Analysis for Cross-Sectional Matching Estimates

	Females			Males		
	Working	**Full Time**	**Hours**	**Working**	**Full Time**	**Hours**
ATT: PSMATCH2	−0.072	−0.077	−3.536	−0.077	−0.087	−5.657
ATT: SENSATT	−0.074	−0.083	−3.687	−0.074	−0.084	−5.588
ATT: omitted binary variable similar to:						
Heart Disease	−0.069	−0.075	−3.456	−0.072	−0.083	−5.561
Arthritis	−0.072	−0.079	−3.575	−0.071	−0.080	−5.577
Occupation	−0.078	−0.086	−3.757	−0.075	−0.083	−5.653
Nonwhite	−0.078	−0.053	−2.886	−0.082	−0.091	−6.150
Children < 18	−0.079	−0.093	−3.937	−0.070	−0.077	−5.489
Married/partner	−0.074	−0.077	−3.524	−0.074	−0.084	−5.577
High school	−0.073	−0.080	−3.590	−0.076	−0.083	−5.685
Some college	−0.074	−0.083	−3.688	−0.073	−0.085	−5.562
College	−0.074	−0.081	−3.633	−0.077	−0.088	−5.757
Post college	−0.076	−0.079	−3.539	−0.073	−0.080	−5.646
Urban	−0.071	−0.078	−3.557	−0.075	−0.088	−5.693
Suburban	−0.073	−0.084	−3.705	−0.074	−0.087	−5.663

Notes: This table displays results of the sensitivity analysis developed by Ichino, Mealli, and Nannicini (2008) using the SENSATT command in Stata. We simulate the effect on the estimated ATT parameters of an omitted binary confounder with the same statistical properties as each covariate listed in the first column of the table. We focus on binary covariates whose mean values differed significantly across the treatment and comparison groups. The estimated ATT parameter for each gender, employment outcome, and simulated omitted confounder is based on 1000 replications of the simulation procedure. Results are for all cancer survivors.

Table A2

Sensitivity Analysis for Difference-in-Differences Matching Estimates

	Females			Males		
	Working	Full Time	Hours	Working	Full Time	Hours
ATT: PSMATCH2	--	−0.058	−3.398	--	−0.089	−5.707
ATT: SENSATT	--	−0.069	−3.737	--	−0.084	−5.595
ATT: omitted binary variable similar to:						
Heart Disease	--	−0.063	−3.618	--	−0.085	−5.568
Arthritis	--	−0.065	−3.641	--	−0.082	−5.533
Occupation	--	−0.072	−3.724	--	−0.084	−5.566
Nonwhite	--	−0.066	−3.491	--	−0.093	−5.514
Children < 18	--	−0.074	−3.790	--	−0.080	−5.535
Married/partner	--	−0.068	−3.725	--	−0.084	−5.593
High school	--	−0.068	−3.634	--	−0.085	−5.556
Some college	--	−0.069	−3.738	--	−0.085	−5.607
College	--	−0.068	−3.694	--	−0.090	−5.643
Post college	--	−0.072	−3.753	--	−0.085	−5.549
Urban	--	−0.063	−3.633	--	−0.088	−5.614
Suburban	--	−0.068	−3.692	--	−0.088	−5.586

Notes: Same as for Table A1.