Long-Term Effects of Cancer Survivorship on the Employment of Older Workers

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Objective. To estimate the long-term effects of cancer survivorship on the employment of older workers.

Data Sources. Primary data for 504 subjects who were 55–65 in 2002 and were working when diagnosed with cancer in 1997–1999, and secondary data for a comparison group of 3,903 similarly aged workers in the Health and Retirement Study (HRS) in 2002.

Study Design. Three employment outcomes (working, working full time, usual hours per week) were compared between the two groups. Both Probit/Tobit regressions and propensity score matching were used to adjust for potentially confounding differences between groups. Sociodemographic characteristics, baseline employment characteristics, and the presence of other health conditions were included as covariates.

Data Collection Methods. Four telephone interviews were conducted annually with cancer survivors identified from tumor registries at four large hospitals in Pennsylvania and Maryland. Many of the questions were taken from the HRS to facilitate comparisons.

Principal Findings. Cancer survivors of both genders worked an average of 3–5 hours less per week than HRS controls. For females, we found significant effects of survivorship on the probability of working, the probability of working full-time, and hours. For males, survivorship affected the probability of full-time employment and hours without significantly reducing the probability of working. For both genders, these effects were primarily attributable to new cancers. There were no significant effects on the employment of cancer-free survivors.

Conclusions. Survivors with recurrences or second primary tumors may particularly benefit from employment support services and workplace accommodation. Reassuringly, any long-term effects on the employment of cancer-free survivors are fairly small.

Key Words. Cancer survivor, employment, chronic illness

There is little doubt that cancer treatment takes a toll on the employment and productivity of patients in the short term. About 40 percent stop working during treatment (Short, Vasey, and Tunceli 2005). In a recent prospective

study by Bradley et al. (2005a, b), employment rates 6 months after diagnosis were 10 and 17 percentage points lower for patients with prostate and breast cancer, respectively, compared with control groups without cancer. A study of absenteeism found that patients being treated for cancer missed more work than patients with a number of other chronic conditions (Kessler et al. 2001).

Most of the published evidence suggests that cancer also has long-term effects on employment, that is, effects extending beyond the initial period of treatment and recovery. Not only are survivors at risk for recurrence of the original cancer, but treatment puts them at increased risk for second primary cancers (National Cancer Institute 2006) and a range of health problems including functional and cosmetic changes after surgery, organ damage, hormonal changes, cognitive deficits, and psychological distress (Hewitt, Greenfield, and Stovall 2005). Several surveys have found evidence of impairments that could affect employment over the long run (Bradley and Bednarek 2002; Chirikos, Russell-Jacobs, and Cantor 2002; Short, Vasey, and BeLue in press; Short, Vasey, and Tunceli 2005; Bradley et al. 2005a, b). Studies of breast cancer survivors at 3 years (Maunsell et al. 2004), 5 years (Chirikos, Russell-Jacobs, and Jacobsen 2002), and an average of 7 years after diagnosis (Bradley, Bednarek, and Neumark 2002) found lower employment rates for the survivors than for similar women with no cancer history. A study of the predominantly long-term survivors in the National Health Interview Survey found that significantly fewer survivors were employed compared with matched controls (Yabroff et al. 2004). However, findings from the Bradley study have recently raised doubts about the significance of long-term employment effects, because the significant differences between controls without cancer and prostate or breast cancer survivors 6 months after diagnosis did not persist at 12 or 18 months (Bradley et al. 2007).

Whether and how much the employment of survivors is affected over the long run is a question that has taken on greater importance as the average 5-year survival rate has reached 64 percent (Jemal et al. 2004), and the number of cancer survivors in the United States has increased to more than 10 million (Hewitt, Greenfield, and Stovall 2005). Experts now emphasize the importance of viewing cancer as a chronic condition that requires ongoing,

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systematic, and coordinated services (Centers for Disease Control and Prevention, Lance Armstrong Foundation 2004; President's Cancer Panel 2004; Hewitt, Greenfield, and Stovall 2005).

We undertook this study to learn more about long-term employment outcomes associated with cancer survivorship. As in most well-designed studies of this topic in the literature, we compared changes in employment from diagnosis to follow-up for a sample of survivors with a comparison group without cancer (Steiner et al. 2004). Economists commonly refer to this quasi-experimental design as a "difference-in-differences" analysis. The comparison group is critical in distinguishing the effects of cancer from general influences on the employment of survivors over time, such as changes in macroeconomic conditions or the normal propensity of older workers to retire. Comparing changes in individual behavior over time nets out unobservable, individual-specific differences in propensity to work. We compared primary data from the Penn State Cancer Survivor Study (PSCSS) with secondary data from the Health and Retirement Study (HRS), an ongoing national cohort study of Americans born from 1931 to 1947.

Most earlier studies of long-term survivors have focused on female breast cancer, but our study included both males and females and virtually all cancer types. It distinguished between survivors with and without new cancers that are likely to reduce employment later in survivorship. In making comparisons with the HRS, we specifically focused on older survivors (55–65 years of age in 2002) as they approached retirement. Cancer is likely to have different effects on workers at different stages of their lives and careers, and older workers are far more likely to be affected by cancer than younger workers. Three-quarters of adult cancer survivors in the age group from 30 to 64 are 50 or older (Hewitt, Greenfield, and Stovall 2005), and nearly half of new cancers diagnosed in the age group from 20 to 64 are diagnosed after age 54 (Ries et al. 2006).

METHODS

Data

Cancer Sample. In the PSCSS, four computer-assisted telephone interviews were conducted with the survivors of incident cases diagnosed in 1997, 1998, and 1999. During the first interview, conducted from October 2000 to December 2001, subjects were asked retrospectively about their employment at diagnosis, "in [month and year of diagnosis], just before you learned you had cancer." In the first and subsequent interviews, they were also asked

about their current employment. For the analyses in this article, we compared data from the second PSCSS interview, conducted in 2002, with HRS data for 2002. At the second PSCSS interview, 71 percent of the cancer sample was 3–5 years postdiagnosis. Time from diagnosis ranged from 26 to 70 months (mean = 45.9, SD = 10.3).

The cancer cases were identified from tumor registries at three large hospitals in central and northeastern Pennsylvania and at Johns Hopkins Hospital in Baltimore, Maryland. With the exception of some Stage 4 leukemias, lymphomas, and plasma cell cancers with relatively favorable prognoses, Stage 4 cases at diagnosis were ineligible because of the small probability of surviving to the end of a multiyear study. Otherwise, the survivors of all types of cancer (other than superficial skin cancers) were eligible. There was an additional exception at Johns Hopkins, where the registry excluded male urological cancers. Eligibility was restricted to adults of working age at diagnosis (25–62 years old), because of the study's emphasis on employment. Subjects who could not be interviewed in English were also excluded. Approximately 5,000 cases met the eligibility criteria, about 2,000 from Johns Hopkins and 3,000 from the Pennsylvania registries.

The research protocol was approved by Institutional Review Boards at Penn State's University Park campus and each hospital. Hospital employees solicited participation in the study and obtained informed consent. Of the 43 percent of eligible cases (2,076 subjects) who initially gave consent and survived, 88 percent (1,763 subjects) were successfully interviewed (Short, Vasey, and Tunceli 2005). Eighty-nine percent of survivors who completed the first interview also completed the second (1,511 cases).

Considering the low rate of recruitment into the study, we conducted extensive analyses using deidentified data for nonparticipants from the registries to identify correlates of nonparticipation and to test for nonparticipation biases (Short and Mallonee 2006). Although there were significant differences in participation by gender, race, cancer site, and facility among the roughly 5,000 eligible individuals, further tests did not find evidence of residual biases in multivariate models similar to those estimated here. We also assessed attrition after the first interview and found a few significant differences related to age, nonwhite race, any college education, poverty, and treatment at the facility with the lowest initial participation rate.

Many of the questions in the cancer survey were taken from the HRS, to facilitate comparisons between the surveys. The first interview asked retrospectively about marital status and health insurance, as well as employment, at diagnosis. Dates of diagnosis and cancer site were

identified from the cancer registries. Survivors were asked, “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 responses to these questions to distinguish between survivors “with new cancers” (encompassing metastases, recurrences, and second primaries) and cancer-free survivors.

After the 2002 cancer sample was restricted to the age range common to both surveys (55–65 years old in 2002), there were 660 cancer cases available for comparison to the HRS. We further excluded 13 cases still in initial treatment for active cancer in 2002 who did not report any new cancers. Then we restricted the sample to 504 individuals who were working at diagnosis. The resulting cancer sample comprised 405 cases who were cancer-free, 94 cases with new cancers, and five with missing data on new cancers.

Comparison Group. The HRS is an ongoing panel study conducted by the University of Michigan with funding primarily from the National Institute on Aging (ISR 2006). The HRS began in 1992 with a nationally representative cohort of 51- to 61-year-olds, born from 1931 to 1941. A second cohort, born from 1942 to 1947, was added in 1998. HRS interviews are conducted biannually in even years. HRS subjects with any history of cancer, other than superficial skin cancers, were dropped from our analyses. HRS data were drawn from RAND’s longitudinal compilation wherever possible (RAND 2004), and otherwise from wave-specific public use files. After the HRS sample was restricted to the age range common to both surveys, there were 5,988 cases for comparison with the cancer survey.

We implemented a procedure similar to one used by Bradley, Bednarek, and Neumark (2002) to characterize employment status in the HRS sample at a starting point or “baseline” comparable with the date of diagnosis in the cancer sample. We assigned baseline dates to the HRS sample by randomly assigning dates of diagnosis from the cancer sample to HRS subjects of like gender, with replacement. Then we determined employment at the baseline date from a large number of questions asked in the HRS. After the HRS sample was restricted to people working at baseline, there were 3,903 HRS cases for analysis.

Statistical Methods

Employment Outcomes. We compared the samples in terms of three employment outcomes: working, working full time (35+ hours in a usual

week), and usual hours per week. 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?"

Comparing Outcomes between Samples. If the cancer and HRS samples were identical on all characteristics likely to influence employment over the study period, except for cancer, then a simple comparison of means would yield a valid estimate of cancer's effect on employment. However, there were a number of significant differences between the samples. Additionally, because the cancer sample was drawn from a limited geographic area, while the HRS is national in scope, there was a possibility that geographic differences would confound the comparisons.

We adjusted for the differences between samples in two ways. First, after combining data from the two surveys, we regressed each outcome variable on an indicator for cancer survivorship and a large set of covariates likely to influence employment outcomes. In the case of discrete outcomes, such as the probability of working or the probability of working full-time, our regression-based estimates were derived from Probit models, while estimates for usual weekly hours came from a Tobit model.

As an alternative way of accounting for observable differences between the samples, we employed two widely used propensity score matching methods to obtain estimates of the average effect of "treatment" on the treated (ATT)—in this case, the average effect of cancer on each of the three employment outcomes among cancer survivors (Leuven and Sianesi 2003). The two matching methods, kernel matching and k -nearest neighbor matching, differ in the estimation of counterfactual outcomes for each treated individual. Kernel matching estimates the counterfactual using a distance-weighted mean of the comparison group observations, with weights inversely related to the distance between the propensity score of each observation in the comparison group and the propensity score of the treated observation.¹ K -nearest neighbor-matching constructs counterfactual outcomes (means) by using the k comparison group observations with propensity scores most similar to each treated observation, with equal weighting of the observations. The estimates presented below are based on drawing the 10 nearest neighbors from the comparison group for each treated observation, with replacement. Similar estimates were obtained for $k = 5$ or 1.²

To ensure that only comparable individuals were compared, we imposed the common support condition before matching and present estimates based on two alternative methods for defining the region of common support.³ The first, known as the “max–min” criteria, discards all treatment group observations outside the range of propensity scores for the control observations. The second, known as “trimming,” discards an additional q percent of treated observations whose corresponding control observations have an estimated density below an endogenously determined cut-off (Smith and Todd 2005). We use a trimming level of 2 percent.

Specification. We obtain estimates of the effect of cancer, relative to HRS subjects with no history of cancer for three groups of cancer survivors: all cancer survivors; survivors with new cancers, and survivors without new cancers. All models were estimated separately by gender. The covariates included sociodemographic characteristics that are known predictors of employment status (age, race, marital status, the presence of children under 18 in the household, educational attainment). We controlled for age with dummy variables for each year. The models also included indicators for five common chronic conditions (diabetes, chronic lung disease, heart disease, stroke, arthritis) at follow-up. Expecting that employment transitions would be more likely over longer time intervals, we controlled for the number of months from diagnosis/baseline to the 2002 interview.

To account for geographic differences between the samples, we utilized three indicators of the urban–rural continuum based on groups of Beale codes that were publicly available from the HRS: (a) large metropolitan areas with more than 1 million population (code 0); (b) smaller metropolitan areas and adjacent areas with their own cities (codes 1 and 2); and (c) all other nonurban areas. As robustness checks, we reestimated the regression models with indicators for Census region and division (Mid-Atlantic, which includes Pennsylvania; South Atlantic, which includes Maryland; New England; rest of South; Midwest; West) and also obtained separate matching estimates after restricting the HRS sample to the two Census divisions where the cancer sample was located.

By making use of the extensive data on employment and job characteristics available from both surveys, we were able to control for many aspects of employment at diagnosis/baseline. In addition to restricting the analysis to cancer and comparison groups that were initially working at diagnosis, we conditioned on a cubic function of baseline hours-of-work

to capture, in a flexible way, initial labor market attachment. We also distinguished the self-employed from wage earners and workers in managerial, professional, and technical occupations from all others.

In addition, we controlled for health insurance and pensions—two types of fringe benefits that enter importantly into decisions about work. Because workers might be especially reluctant to leave jobs with health insurance, we included an indicator for health insurance from the subject's own employer at baseline/diagnosis. Workers with all other types of coverage, including the uninsured, were combined into a single reference group where losing job-related health insurance would not be a concern. We also distinguished between married workers with spouses who did or did not have job-related insurance, because the spouse's plan might free a worker to leave his/her own job-related insurance.⁴ Because pensions with defined benefits are less neutral with respect to age of retirement than pensions with defined contributions (Gustman and Steinmeier 1986; Gustman, Mitchell, and Steinmeier 1994), we characterized pensions as none, defined benefit, defined contribution, and both.

Our matching estimates were based on propensity scores estimated separately for each gender and for each of the survivor group subsamples (all survivors, survivors with new cancers, and survivors without new cancers). The dependent variable in each propensity score model equaled "one" for respondents in the relevant treatment group (cancer survivor, survivor with a new cancer, or survivor with no new cancers) and "zero" for respondents in the comparison group. Tests for covariate balance, conditional on the estimated scores, were performed for every covariate in every specification and in no case was the null hypothesis of equality of the covariates across samples rejected.⁵

Robust standard errors were estimated for all regression models. For the matching estimators, we display two sets of *t*-statistics: one based on the analytic standard errors from the PSMATCH2 program and another that uses bootstrapped standard errors to account for the estimation error in the propensity scores.

RESULTS

Even when restricted to people who were initially working, the cancer and HRS samples differed along dimensions likely to contribute to employment differences between the two groups (Table 1). The cancer sample was somewhat younger, with 57 percent of females and 43 percent of males under age

Table 1: Characteristics of Subjects Working at Baseline/Diagnosis, by Gender and Survey

<i>Characteristic</i>	<i>Male</i>		<i>Female</i>	
	<i>Cancer</i>	<i>HRS</i>	<i>Cancer</i>	<i>HRS</i>
Sample size	215	1,933	289	1,970
		Column percent		
Any new cancers	19	—	19	—
Chronic conditions				
Diabetes	17	16	13	11
Chronic lung disease ^b	8	6	12	7
Heart disease ^b	21	16	19	11
Stroke	3	3	2	3
Arthritis	36	42	50	55
Full-time work (baseline)	86	88	73	73
Self-employed (baseline)	19	21	15	14
Manager, professional, technical (baseline) ^b	41	35	40	31
Own employer insurance (baseline) ^{a,b}	52	60	46	55
Pension (baseline) ^{a,b}				
None	29	36	36	38
Defined benefit	37	22	29	23
Defined contribution	27	25	28	23
Both	5	13	2	11
Unknown	4	4	5	5
Age ^{a,b}				
55–59	43	35	57	44
60–61	17	24	18	21
62–64	33	33	18	27
65	7	9	7	8
Nonwhite ^{a,b}	5	15	7	21
Married/partner (baseline)	91	87	74	69
Spouse has employer insurance (baseline) ^a	25	35	41	39
Children < 18 ^{a,b}	12	8	10	2
Education ^{a,b}				
Less than high school	6	21	5	19
High school	34	45	36	55
Some college	19	5	24	5
College	17	16	13	12
Post-college	25	13	23	10
Urbanicity ^{a,b}				
Urban	19	40	28	43
Suburban	53	28	40	27
Not urban	28	32	31	30

^aMale cancer and HRS samples are significantly different ($p < .05$).

^bFemale cancer and HRS samples are significantly different ($p < .05$).

HRS, Health and Retirement Study.

Variables are measured at follow-up, unless otherwise indicated.

60 in 2002 compared with 44 percent of females and 35 percent of males in the HRS. For males and females, there were also significant differences between surveys in own health insurance, pensions, and education. The proportion of high school dropouts was more than three times greater in the HRS compared with the cancer sample, while the proportion with more than a college education was about half as large in the HRS. The proportion of nonwhites in the cancer sample was one-third the proportion in the HRS, a reflection of the populations served by the Pennsylvania hospitals. As noted previous, the cancer sample was concentrated in one area of the country. The percentages located outside of urban areas were similar, but relatively more of the HRS sample was in large metropolitan areas, while relatively more of the PSCSS sample was in small cities and metropolitan areas.

In univariate comparisons (Table 2) there were significant differences between the samples in the rate of full-time employment (12 percentage points lower in the cancer sample) and average hours per week (4.9 hours less in the cancer sample) for males. The male employment rate did not differ significantly between samples and, overall, there were no significant differences in female employment between samples. Cancer survivors of both genders who experienced new cancers worked significantly less than subjects in the HRS on all three employment measures. Except for the rate of full-time employment for males, mean rates of employment and hours for cancer survivors who remained cancer-free were not significantly different from the means in the HRS.

In multivariate analyses that adjusted for the differences in covariates, the overall effects of cancer survivorship on female employment were somewhat larger than the unadjusted differences between means and were generally significant at a level of 0.10 or less (Table 3). Without conditioning on Census division, estimated effects on the female employment rate ranged from -4.0 to -6.8 percentage points, with the matching estimators yielding slightly larger values than the Probit regression. Estimated effects on the rate of full-time employment ranged from -6.3 to -7.7 , with closer agreement between the Probit and nearest neighbor matching than with kernel matching. The estimated reduction in hours of work was consistently between 3 and 4 hours/week across all estimation methods.

There was little evidence from any of the multivariate estimates that cancer survivorship significantly affected the male employment rate (Table 4). However, in keeping with the differences in unadjusted means, multivariate estimates of the effect of cancer on the rate of full-time employment for males were much larger (ranging from -7.8 to -12.1 percentage points without

Table 2: Employment, Full-Time Employment, and Usual Hours per Week in 2002 for Subjects Working at Baseline/Diagnosis by Survey and Cancer Status

	<i>Male</i>	<i>Female</i>
Health and Retirement Study (HRS)		
Sample size	1,933	1,969
Working (%)	75.9	73.9
Working full-time (%)	63.4	51.0
Usual hours/week (mean)	33.0	26.8
All cancer survivors		
Sample size	215	289
Working (%)	71.2	71.3
Working full-time (%)	51.4*	48.3
Usual hours/week (mean)	28.1*	25.3
Cancer survivors with no new cancers		
Sample size	173	232
Working (%)	74.0	74.1
Working full-time (%)	55.8*	50.9
Usual hours/week (mean)	30.1	26.4
Cancer survivors with new cancers		
Sample size	41	53
Working (%)	61.0*	58.5*
Working full-time (%)	34.2*	36.5*
Usual hours per week (mean)	20.4*	20.5*

*Significantly different from HRS ($p < .05$).

conditioning on Census division) and were statistically significant. Estimated effects of cancer survivorship on hours of work for males were also consistently significant and ranged from -3.5 to -5.0 hours/week.

Distinguishing between survivors who did and did not remain cancer-free showed that new cancers accounted for most of the long-term effect of survivorship on employment. For females with new cancers, multivariate estimates of the cancer effects were -14.5 to -17.3 percentage points for the employment rate, -14.2 to -18.4 percentage points for the full-time employment rate, and -6.97 to -8.48 for hours per week. For males with new cancers, the estimated effects were -11.5 to -13.0 percentage points for the employment rate, -17.4 to -30.7 percentage points for the full-time employment rate, and -8.8 to -13.6 hours/week. The estimates for cancer-free survivors were a fraction of the estimates for survivors with new cancers. Furthermore, none of the estimates for cancer-free survivors were statistically significant at the 0.05 level.

Table 3: Effect of Cancer Survivorship on Long-Term Employment Outcomes, 2–6 Years Postdiagnosis: Females

	<i>Unadjusted Mean Difference</i>	<i>Regression Adjusted Difference</i>	<i>Kernel Matching Max-Min</i>	<i>Kernel Matching 2% Trim</i>	<i>NN-10 Matching Max-Min</i>	<i>NN-10 Matching 2% Trim</i>	<i>Regression Adjusted (Census Division)</i>
I. All cancer survivors							
Working (% points) (<i>N</i> = 2,142)	-2.8 (-0.99)	-4.0 (-1.26)	-6.2 (-1.82)* [-1.88]	-5.9 (-1.74)* [-1.78]	-6.8 (-1.96)* [-1.86]	-6.6 (-1.92)* [-1.81]	-4.4 (-1.25)
Working full-time (% points) (<i>N</i> = 2,115)	-2.2 (-0.70)	-6.3 (-1.65)*	-7.7 (-2.02)** [-2.38]	-7.5 (-1.96)** [-2.30]	-6.4 (-1.63)* [-1.69]	-6.5 (-1.65)* [-1.69]	-10.0 (-2.35)**
Usual weekly hours (<i>N</i> = 2,115)	-1.44 (-1.09)	-3.33 (-1.95)*	-3.64 (-2.35)** [-2.70]	-3.59 (-2.32)** [-2.62]	-3.16 (-1.98)** [-2.03]	-3.15 (-1.98)** [-2.00]	-3.89 (2.06)**
II. Cancer survivors with new cancers							
Working (% points) (<i>N</i> = 1,912)	-15.5 (-2.49)	-14.5 (-2.07)**	-15.9 (-2.13)** [-2.21]	-17.3 (-2.30)** [-2.17]	-14.7 (-1.95)* [-1.83]	-14.7 (-1.89) [-1.63]	-15.4 (-2.13)**
Working full-time (% points) (<i>N</i> = 1,887)	-13.2 (-1.84)	-16.0 (-1.94)*	-16.2 (-2.02)** [-2.16]	-18.4 (-2.50)** [-2.29]	-14.2 (-1.83)* [-1.77]	-16.7 (-2.12)** [-1.97]	-19.6 (2.34)**
Usual weekly hours (<i>N</i> = 1,887)	-6.21 (-1.84)	-8.48 (-2.31)**	-7.26 (-1.96)** [-2.11]	-7.88 (-2.17)** [-2.29]	-6.97 (-1.93)* [-1.92]	-7.69 (-2.03)** [-2.01]	-9.28 (-2.48)**
III. Cancer survivors without new cancers							
Working (% points) (<i>N</i> = 2,087)	0.0002 (0.01)	-1.3 (-0.40)	-3.4 (-0.94) [-0.92]	-3.0 (-0.83) [-0.81]	-3.0 (-0.81) [-0.76]	-2.7 (-0.72) [-0.67]	-1.3 (-0.35)
Working full-time (% points) (<i>N</i> = 2,061)	0.002 (0.05)	-3.8 (-0.92)	-4.4 (-1.07) [-1.10]	-4.0 (-0.97) [-1.00]	-6.3 (-1.48) [-1.42]	-6.1 (-1.45) [-1.39]	-7.2 (-1.58)
Usual weekly hours (<i>N</i> = 2,061)	-0.39 (-0.28)	-2.04 (-1.13)	-2.56 (-1.58) [-1.57]	-2.41 (-1.50) [-1.51]	-3.04 (-1.82) [-1.63]	-2.94 (-1.77) [-1.59]	-2.42 (-1.21)

Notes: Analyses included the following covariates: age, race, education, marital status, any children under 18 in the household, urbanicity, presence of specific chronic conditions, usual hours per week at baseline/diagnosis (cubic), self-employment, managerial/technical/professional occupation, any pension and type, own employer health insurance at diagnosis/baseline, spouse with employer health insurance at baseline/diagnosis, time since baseline/diagnosis. Regression-adjusted differences are marginal effects from Probit or Tobit models. For regression-based estimates, *t*-statistics are based on robust standard errors and 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 200 replications of the combined propensity score -matching estimation routine. Differences between unadjusted means are for observations with nonmissing values of all covariates. Statistically significant with bootstrapped standard errors at **p* < .10, ***p* < .05.

Table 4: Effect of Cancer Survivorship on Long-Term Employment Outcomes, 2–6 Years Postdiagnosis: Males

	Unadjusted Mean Difference	Regression Adjusted Difference	Kernel Matching Max-Min	Kernel Matching 2% Trim	NN-10 Matching Max-Min	NN-10 Matching 2% Trim	Regression Adjusted (Census Division)
I. All cancer survivors							
Working (% points) (<i>N</i> = 2,036)	-4.6 (-1.47)	-4.2 (-1.24)	-3.4 (-0.90)	-3.9 (-1.05)	-3.7 (-0.95)	-3.5 (-0.92)	-6.2 (-1.56)
Working full-time (% points)	-12.1 (-3.44)**	-12.1 (-2.93)**	-7.8 (-1.86)*	-8.6 (-2.10)**	-9.5 (-2.20)**	-9.9 (-2.30)**	-15.4 (-3.15)**
(<i>N</i> = 1,993)							
Usual weekly hours	-4.86 (-2.93)**	-5.05 (-2.41)**	-3.50 (-1.82)**	-3.77 (-1.98)**	-4.05 (-2.04)**	-4.12 (-2.08)**	-6.70 (-2.77)**
(<i>N</i> = 1,993)							
II. Cancer survivors with new cancers							
Working (% points) (<i>N</i> = 1,862)	-13.6 (-1.98)**	-13.0 (-1.78)*	-11.5 (-1.40)	-12.4 (-1.49)	-11.5 (-1.34)	-12.6 (-1.44)	-16.9 (-2.15)**
Working full-time (% points)	-28.7 (-3.72)**	-30.7 (-3.57)**	-20.1 (-2.45)**	-21.4 (-2.60)**	-17.4 (-2.01)*	-19.2 (-2.20)*	-35.2 (-3.98)**
(<i>N</i> = 1,820)							
Usual weekly hours	-12.25 (-3.64)**	-13.6 (-3.03)**	-9.60 (-2.67)**	-10.5 (-2.99)**	-8.80 (-2.29)**	-9.94 (-2.61)**	-15.69 (-3.36)**
(<i>N</i> = 1,820)							
III. Cancer survivors without new cancers							
Working (% points) (<i>N</i> = 1,995)	-2.1 (-0.61)	-1.7 (-0.48)	-0.8 (-0.20)	-1.5 (-0.38)	-2.2 (-0.54)	-2.1 (-0.53)	-3.2 (-0.77)
Working full-time (% points)	-7.9 (-2.05)**	-7.7 (-1.73)*	-5.3 (-1.18)	-5.0 (-1.12)	-6.7 (-1.45)	-6.9 (-1.50)	-10.3 (-1.99)**
(<i>N</i> = 1,952)							
Usual weekly hours	-2.98 (-1.63)	-2.94 (-1.30)	-2.11 (-1.02)	-2.01 (-0.97)	-3.01 (-1.40)	-3.00 (-1.40)	-4.38 (-1.70)*
(<i>N</i> = 1,952)							

Notes: Analyses included the following covariates: age, race, education, marital status, any children under 18 in the household, urbanicity, presence of specific chronic conditions, usual hours per week at baseline/diagnosis (cubic), self-employment, managerial/technical/professional occupation, any pension and type, own employer health insurance at diagnosis/baseline, spouse with employer health insurance at baseline/diagnosis, time since baseline/diagnosis. Regression-adjusted differences are marginal effects from Probit or Tobit models. For regression-based estimates, *t*-statistics are based on robust standard errors and 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 200 replications of the combined propensity score-matching estimation routine. Differences between unadjusted means are for observation with nonmissing values of all covariates. Statistically significant with bootstrapped standard errors at **p* < .10, ***p* < .05.

Reassuringly, considering the geographic differences between samples, adding controls for census division/region did not have a large influence on the estimated effect of cancer survivorship on employment. In the regression models combining all cancer survivors, tests of the joint significance of dummy variables for division/region were significant for male full-time employment ($p = .03$), marginally significant for female full-time employment ($p = .06$) and male hours ($p = .07$), and not significant for female or male employment ($p = .51, .17$) or female hours ($p = .41$). When the estimates differed, regression estimates from models with these geographic variables (last column of Tables 3 and 4) were somewhat larger than estimates from models without them. Matching estimates were also somewhat larger with the HRS restricted to the same Census divisions as the cancer survey. For females, the restricted matching estimates ranged from -7.3 to -7.9 percent for any employment, -10.5 to -12.2 percent for full-time employment, and -4.6 to -5.1 for hours (not shown in table). For males, the restricted matching estimates ranged from -8.7 to -10.4 percent for employment, -9.3 to -12.3 percent for full-time employment, and -4.4 to -5.8 for hours.

DISCUSSION

In absolute value our estimates of the marginal effect of cancer survivorship on female employment rates (ranging from -4 to -7 percentage points) overlap the lower end of the range from -6 to -10 percentage points reported in three previous studies of long-term breast cancer survivors (Bradley, Bednarek, and Neumark 2002; Chirikos, Russell-Jacobs, and Jacobsen 2002; Maunsell et al. 2004). The study at the low end of the range (Chirikos, Russell-Jacobs, and Jacobsen 2002) was limited to cancer-free survivors. We are not aware of any comparable estimates of cancer survivorship's long-term effects on the male employment rate. Our estimates suggest that cancer survivorship has a greater effect on the number of hours that males work, rather than whether they work at all. When survivors who stopped working altogether are included, the average reduction in hours was similar for males and females working at diagnosis (on the order of 3–5 hours/week).

We estimate that survivors with new cancers reduced their employment by an average of 7–14 hours/week (including those who stopped working) compared with an average of 2–3 hours/week for cancer-free survivors. Maunsell et al. (2004) also found much larger employment effects for survivors with new cancers compared with cancer-free survivors. The prevalence of new

cancers was nearly 20 percent by 2002 in our sample, which was diagnosed in the late 1990s and excluded anyone with a solid tumor diagnosed at Stage 4. Thus, unfavorable clinical outcomes that are not evident in the short term contributed significantly to the adverse effects of cancer on employment over the long term, an average of about 4 years postdiagnosis in the data we analyzed. Survivors who experience such relapses may particularly benefit from supportive services to help them continue working. For survivors who remained cancer-free, we did not find significant effects on employment.

Our HRS comparison group did not allow us to obtain estimates for workers under age 55, and we cannot say for sure if the effects of cancer on the employment of younger survivors would be larger or smaller. Older survivors were more likely to quit working after diagnosis and treatment than the younger survivors whom we interviewed in our larger study, which included survivors diagnosed from ages 25 to 62. However, the older survivors would have also been more likely to cut back on work in the absence of a cancer diagnosis, as evidenced by the 24 percentage-point drop in the employment rate for our HRS comparison group. In a previously published study of employment within our entire sample of younger and older survivors, the self-reported likelihood of quitting work for cancer-related reasons did not vary significantly by age (Short, Vasey, and Tunceli 2005). Studies designed to estimate and compare the effects of cancer survivorship on workers at different stages of their lives and careers would be useful.

Studying long-term employment outcomes is challenging and potentially quite costly. To reduce costs, we mixed retrospective and prospective data collection approaches and constructed a comparison group from secondary data. By making use of these strategies, we were able to collect detailed longitudinal employment data for a large sample of cancer survivors over an extended time. Nevertheless, extended recall of the survivors' circumstances at diagnosis is a potential weakness that could undermine the validity of our findings. Furthermore, although our estimates were relatively robust across a variety of estimation methods, it is still possible that we have not adequately controlled for differences between the cancer and noncancer samples. In addition, employment patterns in a relatively well educated, mostly white sample of survivors who were treated at large medical centers may understate the adverse effects of cancer on the national population of survivors (which is somewhat less advantaged). The low recruitment rate in the cancer study is another potential limitation.

Because we did not collect primary data for a comparison sample in the same geographic locations as the cancer sample, but fell back on comparisons to

secondary data from a national survey, we adjusted for geographic differences in urbanization in all analyses. We also experimented with adjusting for Census division/region, which was statistically significant in only one of six regression models. Conditioning on Census division/region yielded estimates that were slightly larger than but qualitatively similar to estimates without division/region. We are inclined to put more weight on estimates that ignored division/region and made fuller use of the HRS sample in accounting for other important variables, as locations in the same Census division may still be quite dissimilar in geographical terms (e.g., New York City and Hershey, Pennsylvania).

While this study adds to the evidence that cancer survivorship does indeed have long-term effects on employment after the initial period of treatment and recovery, the magnitude and distribution of these effects should be fairly reassuring to workers who are diagnosed with cancer. Two to 6 years postdiagnosis, employment outcomes did not differ between survivors who remained cancer free and otherwise similar workers who were not diagnosed with cancer. Employers, who are generally required to accommodate the absences and health problems of employees who undergo cancer treatment, should be reassured that these employees are not more likely to quit working afterwards. Cancer professionals should be reassured that gains in long-term survival are not coming at a major cost to the economic well-being of most survivors and their families.

ACKNOWLEDGMENTS

This research was supported by the National Cancer Institute, U.S. Department of Health and Human Services under grant R01 CA 82619. The authors are indebted to the study's Patient Advisory Committee; Witold Rybka and Margaret Davitt (Milton S. Hershey Medical Center), James Evans and Sharon Reeder (Geisinger Medical Center), Gregory Harper and Andrea Geshan (Lehigh Valley Hospital and Health Network), and James Zabora and Barbara Abdullah (The Johns Hopkins Hospital) for advice and patient recruiting; the Survey and Epidemiology Services division of Social and Scientific Systems Inc. of Silver Spring, MD for conducting the interviews; and Michael Markowski and Rajeshwari Punekar for research assistance with this article.

NOTES

1. Frolich (2004) presents Monte Carlo evidence that kernel matching is often the best estimator in settings, such as ours, where the ratio of comparison with treated observations is 4:1 or better.

2. Numbers of neighbors in the range from 1 to 10 are common in the evaluation literature (Heckman, Ichimura, and Todd 1997; Smith and Todd 2005).
3. In comparing matching estimators to experimental estimates based on randomization, Heckman, Ichimura, and Todd (1997) find that violating the support condition accounts for a substantial portion of the bias in nonexperimental estimators.
4. We experimented with including the interaction of own job-related insurance and having a spouse with job-related insurance, but the interaction term never approached statistical significance.
5. Probit models were used to estimate the propensity scores. The full regression models, propensity score models, and balancing tests are available upon request from the authors.

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