Essays on Health and Social Insurance

by

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Abstract

This thesis consists of three chapters on the economics of health and social insurance.

In the first chapter, I examine the distribution of income risk that adults face from severe illness and the social insurance provided by taxes and transfers using an event study research design with linked Canadian hospital and tax records. I find that adults with lower incomes face larger pre-tax earnings risk from hospitalization events, primarily due to extensive margin exits from employment. Canada's tax and transfer system insures 44% of post-hospitalization income losses in the bottom income quintile and 12% of losses in the top income quintile. But less than two thirds of this insurance comes from replacing lost earnings with increased transfers. In the bottom income quintile, 30% of insurance is due to a stable stream of transfers; in the top income quintile, 30% of insurance is due to progressive taxation. Using a calibrated model, I find that the marginal value of additional insurance against hospitalization risk is approximately flat across the income distribution.

In the second chapter, I show that employer-provided short-term disability insurance (STDI) increases long-term disability insurance (LTDI) take-up and imposes a negative fiscal externality on the government budget. Using variation in private STDI coverage caused by Canadian firms ending their plans, I find that private STDI raises two-year flows onto LTDI by 0.07 percentage points (33%). Extrapolating to Canada's entire population, private STDI generated 18,300 LTDI recipients and CA$230 million dollars (5%) of public LTDI spending in 2015.

In the third chapter, Raj Chetty, Sarah Abraham, Shelby Lin, Benjamin Scuderi, Nicholas Turner, Augustin Begeron, David Cutler and I examine the relationship between income and life expectancy in the United States from 2001 to 2014. Using 1.4 billion linked earnings and mortality records, we document the levels of life expectancy and changes in life expectancy over time by income group, at a national level and within local areas. We also examine the factors correlated with differences in life expectancy across local areas.

JEL Classification: I38, H53, I14

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Chapter 1

Insuring the Labor Market Risks of Illness

1.1 Introduction

Developing an illness is one of the largest risks humans face. Half of Americans develop a new chronic health condition in their 50s, during their prime working years, independent of whether they had prior chronic health conditions (Smith 2005). Health insurance is therefore important, but it only insures one component of health risks: medical bills. Recent studies have shown that post-hospitalization earnings losses can rival medical costs (García-Gómez et al. 2013; Fadlon and Nielsen 2017; Dobkin et al. 2018). And these earnings losses may disproportionately affect low-income households, who experience higher rates of illness and disability (Marmot et al. 1991; Canadian Institute for Health Information 2016; Baker, Currie, and Schwandt 2017). Yet despite extensive academic and policy interest in the

I am grateful to Amy Finkelstein, Heidi Williams, David Autor and Raj Chetty for their advice and support throughout the preparation of this paper. George Akerlof, Sydnee Caldwell, David Cutler, Joe Doyle, Itzik Fadlon, Colin Gray, Jon Gruber, Ray Kluender, Christina Patterson, Jim Poterba, Otis Reid and Daniel Waldinger provided valuable comments and feedback. Sung-Hee Jeon, René Morisette, Michelle Rotermann, Claudia Sanmartin and Grant Schellenberg provided data resources and guidance. Financial support was provided by the Social Sciences and Humanities Research Council of Canada, the U.S. Social Security Administration through grant #RRC08098400-09 to the National Bureau of Economic Research as part of the SSA Retirement Research Consortium, and the Alfred P. Sloan Foundation Pre-Doctoral Fellowship on the Economics of an Aging Workforce awarded through the NBER. The views expressed in this paper do not necessarily reflect the views of Statistics Canada or any agency of the Government of Canada or the Government of the United States.

universality and adequacy of health insurance, we know little about how the labor market risk of illness is distributed or how well it is insured.

This paper uses a panel of more than 730,000 inpatient hospital stays linked to population income tax records in Canada to examine the distribution of income risk that adults bear from severe illness. These newly linked administrative hospital and tax records allow me to study two aspects of risk and insurance of poor health that have been difficult to address in prior work using survey data.[1] First, I leverage the size of the population-wide data to estimate how the consequences of hospitalizations vary across the income distribution and within income groups. Second, I use the detailed observations of transfer benefits received and taxes owed to decompose the sources of social insurance and how they vary across the income distribution. The resulting estimates are relevant for positive economics—to understand who bears risk and what types of risk they bear—as well as for normative economics—to characterize the welfare implications of the design of the tax and transfer system and the value of marginal increases in insurance.

I use an event study research design to identify the causal effects of adverse health events associated with an inpatient hospital stay. I compare the longitudinal outcomes (employment, earnings, transfer income and taxes) of individuals hospitalized at ages 40 to 54 in the years 2003 to 2010 to a matched sample of similar adults who were not hospitalized in the same year. To isolate the timing of new adverse health events, both the hospitalized group and the matched control group are restricted to individuals who had no hospitalizations or disability claims in the previous three years, and I exclude pregnancy-related hospitalizations. For each outcome measured, I show that the hospitalized individuals and matched controls were on parallel trends from the beginning of the sample until one year prior to hospitalization, then diverge sharply in the year of the hospitalization event.

I find that adverse health events associated with a hospitalization cause large and persistent declines in earnings, primarily due to extensive margin declines in employment. During the five years following a hospitalization, annual employment declines by 5 percentage points (6%) and annual earnings decline by $4,100 (8%) on average. Those declines are immediate in the first year post-hospitalization, and there is no recovery in earnings or employment during the subsequent five years. The post-hospitalization decline in employment rates is

[1] In the United States, for example, the Health and Retirement Study is the most comprehensive survey linking health and income information and there are presently no avenues for linking administrative health and tax records (Dobkin et al. 2018).

the driving factor behind the decline in earnings. Five years after the hospitalization event, hospitalized individuals are 5.5 percentage points more likely to have no earnings or to earn less than 10% of their mean pre-hospitalization earnings; they are only 1.0 percentage points more likely to be earning 10 to 80% of their mean pre-hospitalization earnings.

The earnings consequences of hospitalization events differ across the income distribution: people with lower incomes have larger subsequent declines in employment and lose a larger share of their earnings. For people in the bottom income quintile three years pre-hospitalization, hospitalization events cause an 8 percentage point decline in annual employment and a 17% decline in earnings over six years. For those in the top income quintile, annual employment declines only 3 percentage points and earnings decline by only 5%. These larger losses also happen more frequently to those in the bottom income quintile, who are 30% more likely to experience a hospitalization event each year than those in the top quintile (a 2.9% annual hazard vs. a 2.2% annual hazard).

Spousal labor supply could be a source of insurance against income losses, but I find that spousal earnings decline by 2% following a hospitalization. Spouses are known to provide insurance against income losses following job displacements (Cullen and Gruber 2000; Stephens 2002) or disability insurance claim rejections (Autor et al. 2017). But hospitalization events should have an offsetting effect of raising spouses' demand for leisure, due to a need for caregiving or a desire to jointly time retirement. The decline in spousal earnings that I observe suggests that spouses' desire to reduce their labor supply due to complementarities in spousal leisure dominates spouses' desire to increase their labor supply to replace uninsured earnings losses. This result is consistent with prior work showing that spousal earnings decline modestly in the Netherlands and Denmark (García-Gómez et al. 2013; Fadlon and Nielsen 2017) and do not change in the United States (Dobkin et al. 2018) following a hospitalization event. Spousal earnings declines imply that formal insurance of post-hospitalization losses is high enough that spouses aren't pressed into work, but cannot reveal whether the level of insurance is optimal without strong assumptions about the value of spousal leisure complementarities.

Canadian social insurance reduces post-hospitalization income losses by 23%: average household income falls by 4.4% before taxes and transfers and by 3.4% after taxes and transfers. Two thirds of this social insurance is provided by increases in transfers, which replace 16% of household income losses. The income replacement rate in Canada is higher than the 10% replacement rate of similar hospitalization events in the United States (Dobkin et al. 2018), but far lower than the 50% replacement rate observed following heart attacks and strokes in Denmark (Fadlon and Nielsen 2017). But the replacement rate describes only part of the social insurance system. I show that social insurance can be decomposed into three components, and the remaining one third of insurance against hospitalization risk comes from the combination of stable transfer income, which provides an income stream that is not sensitive to labor market risk, and progressive taxation, which provides tax rates that fall as earnings fall. These two sources of insurance capture the theoretical insight developed in Varian (1980) that redistributive taxation is a source of social insurance. Accounting for all three components of social insurance likely magnifies the differences in insurance across countries, since Canada's tax and transfer system is smaller and less progressive than Denmark's and larger and more progressive than America's.

I find that Canadian social insurance is highly progressive, which mitigates but does not eliminate the inequalities in average income losses after a hospitalization event. Tax and transfer programs reduce the share of income lost post-hospitalization by 44% for the bottom income quintile and by 12% for the top income quintile. The progressivity of social insurance is due to both higher income replacement rates for lower income households and the fact that transfers are a larger share of household income for lower income households regardless of whether they experience a hospitalization event. Cash welfare is a particularly important source of insurance for low-income households, consistent with the finding of Low and Pistaferri (2015) that food stamps are an important source of insurance against disability risk in the United States. But despite the progressivity of Canadian social insurance, low-income households continue to bear larger average losses in income after taxes and transfers. Household income falls post-hospitalization by an average of 6.1% in the bottom income quintile and only 2.5% in the top income quintile.

In the last section of the paper, I estimate the marginal value of an actuarially fair increase in transfers to individuals who experience a hospitalization event. I show that in the presence of heterogeneous losses, representative agent models of social insurance like the widely-used Baily-Chetty formula understate the value of insurance benefits (Baily 1978; Chetty 2006b). The Baily-Chetty formula estimates the value of insurance based on the mean percent loss in consumption, but heterogeneous losses correspond to a mean-preserving spread, which raises the marginal value of insurance. However the information requirements of a model with heterogeneous agents are higher, requiring an estimate of the entire distribution of consumption losses. I estimate this distribution by leveraging the administrative tax data and event study research design to measure the effect of hospitalization events on average consumption-equivalent (or "equivalized") income during the five years following the hospitalization event.[2]

Contrasting the results of the heterogeneous agent model with the representative agent model, I show that accounting for heterogeneous losses doubles the marginal value of insurance against hospitalization events at moderate levels of risk aversion. Moreover, the marginal value of insurance is approximately flat throughout the income distribution, despite the fact that mean post-hospitalization losses are larger for lower income households. If each income quintile could be accurately described by a representative agent, then the marginal value of insurance would be more than twice as large for the bottom income quintile than the top income quintile at any level of risk aversion. But progressive social insurance reduces both the mean *and* the variance of outcomes more strongly for individuals near the bottom of the income distribution. After taxes and transfers, mean post-hospitalization income declines reflect a higher probability of a smaller loss at the bottom of the income distribution and a lower probability of a larger loss at the top of the income distribution. Analyzing social insurance through the lens of the mean loss in post-event income therefore understates both the insurance value and the progressivity of the social insurance system.

1.1.1 Related Literature

This paper builds on a long literature studying the economic effects of adverse health events. Many studies have used survey data from the Panel Study of Income Dynamics (PSID) or the Health and Retirement Study (HRS) to estimate the effects of changes in health conditions or disability on average earnings, consumption and wealth (e.g. Cochrane 1991; Smith 2005; Chung 2013; Poterba, Venti, and Wise 2017; Meyer and Mok 2018; Dobkin et al. 2018). More recently, economists have used linked administrative data to study the effects of hospitalization events in Sweden and the Netherlands (Lundborg, Nilsson, and Vikström 2011; García-Gómez et al. 2013), automobile accidents in Denmark and Austria (Dano

[2] I calculate equivalized income by dividing household income by the square root of the number of members of the household—the most commonly used equivalence scale. Equivalence scales are designed to account for the fact that resources are divided among members of the household but there are economies of scale in consumption. A two person family can spend less than twice as much (on housing, food, etc.) to achieve comparable consumption utility.

2005; Halla and Zweimüller 2013), and heart attacks and strokes in Denmark (Fadlon and Nielsen 2017). This paper contributes new detail on the economic effects of health risk by estimating the distributions of post-hospitalization outcomes both across and within income groups.

This paper also connects to the literature on redistributive income taxation as a source of social insurance against labor market risk. This role of taxation was explored by the influential work of Varian (1980). More recent papers have characterized the implications of uninsurable labor market risk on the theoretical design of optimal income taxes (Huggett and Parra 2010; Farhi and Werning 2013; Golosov, Troshkin, and Tsyvinski 2016; Heathcote, Storesletten, and Violante 2017). Yet there has been correspondingly little empirical work measuring the implications of the current tax and transfer system on the level or distribution of insurance against major sources of income risk, such as illness, disability or unemployment. My results show that redistributive taxation is a quantitatively important source of insurance following hospitalization events.

Finally, the results in this paper are relevant to a large literature on optimal social insurance that estimates mean consumption losses and applies the Baily-Chetty formula to study unemployment insurance (Gruber 1997; Chetty and Szeidl 2007; Kroft and Notowidigdo 2016; Hendren 2017), worker's compensation (Bronchetti 2012), and disability insurance (Meyer and Mok 2018). I show that the value of social insurance estimated using Baily-Chetty will be biased downward in the presence of heterogeneous losses. And in practice this downward bias varies systematically across income groups, understating the demand for social insurance against hospitalization risk among high-income individuals relative to low-income individuals. Of course, heterogeneity in income losses may be especially severe when insuring all inpatient hospitalization events, as with the hypothetical insurance program I consider. But Browning and Crossley (2001), Bronchetti (2012), Hendren (2017) and Ganong and Noel (2019) document substantial heterogeneity in consumption declines among individuals receiving unemployment insurance and worker's compensation.

The remainder of the paper is organized as follows. Section 1.2 describes the data sources and sample construction. Section 1.3 explains the matching procedure and estimating equations used to identify the event study research design. Section 1.4 presents the results on earnings and employment effects, spousal insurance and social insurance of hospitalization events. Section 1.5 analyzes the marginal value of additional insurance. Section 1.6 concludes.

1.2 Data

I measure the income risks and social insurance of severe illnesses by linking administrative hospital and tax records from Canada. The tax records reveal annual employment, earnings, taxes and benefits from individual transfer programs. The linkage to hospital records allows me to study how outcomes evolve each year pre- and post-hospitalization, and among people who are not hospitalized. The analysis focuses on annual samples of 40- to 54-year-old adults who had no hospitalizations or disability claims in the prior three years.

1.2.1 Data Sources

I construct an annual panel of inpatient hospitalizations and economic outcomes by linking administrative hospital records with tax records. The hospital records are drawn from the Discharge Abstract Database, which includes all inpatient admissions to Canadian acute care hospitals outside Quebec and Manitoba from 2000 to 2014.[3] Outpatient visits to the hospital and emergency room visits that did not result in an admission are not included in the database. The tax records are drawn from the T1 Family File, which includes 100% of Canadian tax filers from 1998 to 2015. Individuals are linked to their spouses and cohabiting children using information from their tax returns and child benefit claims. Wages earned by non-filers are observed using T4 "Statement of Remuneration Paid" slips filed by employers, which are equivalent to W-2 slips in the United States. All together, 96% of Canadians are observed in the T1 Family File (Statistics Canada 2016b). I topcode earnings and income at the 99.95th percentile in each year to mitigate the influence of outliers or erroneous data, but find that the results are not sensitive to topcoding.

Individuals' hospitalization records and tax records are linked using an exact deterministic match on date of birth, sex and postal code.[4] 88.4% of hospital records were successfully linked to a person in the tax records, and the linkage rate was consistently high across years, provinces, diagnoses and sex. 7.6% of hospital records did not match to anyone in the tax records and, to the extent that these belong to tax filers, these unlinked hospitalizations

[3]Quebec does not contribute its hopsital data to the Discharge Abstract Database, and Manitoba did not begin contributing data until April 2004.

[4]The average Canadian postal code contains fewer than 40 individuals, so it is very rare for two different people to share the same date of birth, sex and postal code.

will attenuate my results. 4% of hospital records were matched to more than one person in the tax records, and the associated hospital records and tax records were excluded from the sample. Further details on the linkage procedure and quality assessments are described by Sanmartin et al. (2017).

Statistics Canada protects individuals' privacy during the linkage process and subsequent use of linked files. The data linkage was approved by Statistics Canada's Executive Management Board, and its use is governed by Statistics Canada's Directive on Record Linkage (2017). Only employees directly involved in the linkage process had access to the unique identifying information, and those employees did not have access to health-related or tax-related information. After the data linkage was completed, an analytical file was created with the identifying information removed (Statistics Canada 2016a). The de-identified files were used for this analysis, and all data processing was performed on a secure server onsite at Statistics Canada in Ottawa, Ontario.

1.2.2 Sample Construction

This section describes the analysis sample I construct to study the impacts of hospitalizations in an event study framework. In each index year $k \in \{2003, \ldots, 2010\}$, I divide the population into a treatment group that was hospitalized in year k and a control group that was not hospitalized in year k. In order to isolate hospitalization *shocks*, I restrict the sample to people who had no disability claims or hospitalizations for at least three years before the index year. I therefore exclude anyone who received long-term disability insurance benefits through the Canada Pension Plan or who claimed the disability tax credit during the three years preceding the index year. I also exclude anyone who was hospitalized in an acute care hospital during the three years prior to their index year.

I focus the analysis on individuals who were 40 to 54 years old on December 31 of the index year.[5] I exclude residents of Quebec and Manitoba in the index year, since hospital admissions from those two provinces are not available throughout the entire sample period. I also exclude residents of the northern territories: the Yukon, the Northwest Territories and Nunavut. Health care in the territories is limited and many residents are flown south to hospitals in the provinces, including Quebec and Manitoba, for treatment. I require each

[5] I select individuals who are no older than 54 in the index year so that the entire sample can be followed for 5 years without becoming eligible for public pension benefits. Canadians can claim early retirement benefits through the Canada Pension Plan starting at age 60.

person to have worked at least once during the 5 years prior to (but not including) the index year. The sample population therefore covers 40 to 54 year olds with prior labor force attachment living in eight of the ten Canadian provinces during the index year.

Pooling all index years, the analysis sample contains 6.5 million Canadians aged 40 to 54 between 2003 and 2010. Each person is assigned to the treatment group, the control group, or excluded from analysis in *each* index year. Pooling the eight index years, the sample contains 738,287 hospitalization events matched to 30 million control events. Table 1.1 presents summary statistics for this analysis sample.

1.3 Empirical Methods

The key to measuring the income risks of hospitalization is identifying the causal effects of hospitalization events on employment, earnings, taxes and transfers. To identify these causal effects, I use an event study framework to compare how outcomes diverge between people who were hospitalized in a given year and similar people who were not hospitalized in the same year. Here, I describe the matching procedure I use to obtain a control group with similar characteristics to the to the hospitalized group. I then present the event study regression equations that estimate the evolution of the outcomes among the treatment and control group. The resulting estimates reveal whether the hospitalized group and the control group followed the same trend pre-hospitalization and identify how much the groups diverge post-hospitalization.

1.3.1 Matching Procedure

The econometric challenge in this paper is to estimate the counterfactual outcomes of people who were hospitalized, which would have occurred absent the hospitalization event.[6] I estimate these counterfactuals using the outcomes of people with similar lagged characteristics who were not hospitalized in the same year. Specifically, I reweight the individuals in the control group in index year k to exactly match the hospitalized group in index year k on saturated interactions of age, sex, province of residence in year $k-1$, marital status in $k-1$

[6]The "hospitalization event" considered in this research design encompasses the health decline that led to the hospital admission. The counterfactual therefore represents what would have happened absent the health decline that led to a hospitalization, not what would have happened to someone with the same health decline who did go to the hospital.

and own earnings decile in $k-3$.[7] This coarsened exact matching procedure ensures that the event study regressions are identified by comparing individuals with similar observables prior to the hospitalization event.[8] Note that I do not match on trends in the pre-period, so the pre-trends remain flexible and can be used to evaluate potential bias due to differences in unobservables.

More than 99% of hospitalized individuals in the sample are successfully matched to controls (Table 1.1). Unmatched individuals are excluded from the sample. The resulting sample contains 736,329 hospitalization events matched to 27 million control events, with the lagged observables well-balanced by construction. Following the index year, some sample attrition is caused by mortality in the hospitalized group and the control group (shown in Appendix Figure A.1). To maintain the comparability of the two groups, I reweight the survivors in the control group in each year $r \geq 0$ to match the survivors in the hospitalized group. Additionally, when I study outcomes that are only observable for taxfilers, the panel becomes unbalanced due to incomplete taxfiling rates.

For hospitalized individuals and matched controls in each index year and each relative year, I reweight the taxfiling sample to match the full sample using the same cells of fully interacted matching variables that were initially used to match hospitalized individuals to controls. Individuals in cells with low tax filing rates receive greater weight, which preserves the composition of the sample with respect to the characteristics used for the initial match. I show below that the event studies for earnings and employment (which are observable for non-filers) are nearly identical in the full sample and the matched reweighted sample of taxfilers (Appendix Figure A.2).

1.3.2 Event Study Regressions

I identify the causal effects of hospitalization events by comparing the evolution of outcome y (such as earnings) among individuals hospitalized in index year k with the evolution of earnings among the matched control group. For each index year $k \in \{2003, \ldots, 2010\}$, I estimate a separate non-parametric event study regression using weighted OLS:

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[7]The earnings decile variable has 11 bins: deciles of positive earnings and an additional bin for individuals with zero earnings.

[8]Coarsened exact matching is widely used to construct appropriate control groups in event study designs: recent examples include Jaëger (2016) and Sarsons (2017). Iacus et al. (2012) describes and contrasts the statistical properties of coarsened exact matching with other matching procedures such as propensity score matching.

$$y_{ir}^{k} = \alpha_i^k + \sum_{r \neq -3} \beta_r^k + \sum_{r \neq -3} \delta_r^k \cdot \text{Hospitalized}_i^k + \varepsilon_{ir}^k \tag{1.1}$$

Subscript i denotes individuals and subscript r denotes the number of years elapsed relative to the index year k when the hospitalization event occurs. α_i^k denotes individual fixed effects, which allow each individual to have an arbitrarily different level of y. β_r^k estimates the evolution of the mean of y over time among the matched control group. δ_r^k are the coefficients of interest: they estimate the difference in the evolution of mean y over time between the hospitalized group and the matched control group.

The estimates δ_r identify the average effect of treatment on the treated if, within each matched cell, hospitalized and control individuals would have evolved in parallel absent the hospitalization event. This identifying assumption can be evaluated by looking for parallel trends in the pre-period ($\delta_r = 0$ for $r \leq -1$). Even if the pre-trends are parallel, the identifying assumption could be violated if economic shocks are correlated at high frequencies with hospitalization shocks. For instance, if job separations cause a spike in hospitalizations and a long term decline in economic outcomes y_{it}, the event study regression would falsely attribute the direct effect of job separations on y_{it} to the associated hospitalizations.

1.3.3 Pooled Event Study

In practice only one index year is required to study the impacts of hospitalization shocks: a single event study provides a panel of hospitalized and control individuals to compare over time. But the analyses below will pool the samples from eight index years spanning 2003 to 2010. I calculate the pooled event study estimator using an unweighted average over the estimates from each index year: $\bar{\delta}_r \equiv \mathbb{E}_t\, \delta_r^t$.[9] This pooled estimator identifies an average effect of treatment-on-treated over the hospitalization cohorts (Borusyak and Jaravel 2016; Abraham and Sun 2018). Pooling the index years also increases the sample size, which helps to identify the effect of hospitalization events in fine-grained subsamples by income.

Hospitalized individuals will cycle back into later cohorts if they experience no subsequent hospitalizations for three consecutive years (similar to the research design of Autor, Donohue, and Schwab 2006). This potential source of bias could be reduced by increasing the washout period prior to the index year during which individuals in the sample are required to have

[9] The choice not to weight the index years is inconsequential in practice because the sample sizes and estimated effects are stable across index years.

no hospitalizations, at the cost of dropping early index years. I find that my results are robust to varying the washout period from 3 to 10 years.

1.4 Results

1.4.1 Effects of Hospitalization Events on Earnings and Employment

This section describes the effects of adverse health events on the earnings of hospitalized individuals. First, I show that average earnings fall immediately after a hospitalization and do not recover for at least five years. Next, I show that these earnings declines are generated primarily by extensive margin exits from employment. Finally, I document that hospitalized individuals with lower incomes pre-hospitalization are more likely to exit employment and lose a larger share of their earnings post-hospitalization.

Average Earnings and Employment

Figure 1.1A plots the difference in annual earnings (δ_r^k) between the hospitalized group and the matched control group for each index year $k \in \{2003, \ldots, 2010\}$, from 1997 (the first year observed) until five years after hospitalization. Earnings refer to wage earnings, which are observed for the full population regardless of whether they filed their taxes.

Average earnings decline immediately in the year of hospitalization by more than \$4,000 and remain roughly \$4,000 lower throughout the subsequent five years (Figure 1.1A). The hospitalized individuals and matched control individuals followed parallel earnings trajectories starting up to 13 years prior and lasting until two years prior to hospitalization. Only outcomes in the third year prior to hospitalization were used for matching, so the parallel evolution during the rest of the pre-period was not predetermined mechanically. These parallel pre-trends support the identifying assumption that the hospitalized individuals would have continued along the same trajectory absent the hospitalization event.

The small dip in the average earnings among hospitalized individuals one year prior to hospitalization (Figure 1.1A) implies that the event study does not isolate the causal effect of the events occurring during the hospital stay itself. Rather, the inpatient hospitalization event is an observable proxy for an underlying deterioration in health that begins prior to the hospitalization and starts to affect earnings during the preceding year. The event study estimates should therefore be interpreted as the causal effect of the illness associated with the hospitalization event.

Figure 1.1B pools the results obtained from each index year and plots the evolution of mean earnings from five years before to five years after the hospitalization event. I plot the observed mean earnings of hospitalized individuals, weighting each index year equally. I plot the counterfactual earnings of the matched control group by pooling the cohort-specific event study estimates of the difference between the two groups: $\bar{\delta}_r \equiv \mathbb{E}_k \, \delta_r^k$. Figure 1.1C follows the same procedure to plot the evolution of annual employment rates among the hospitalized and control groups. I define an individual as employed during a given year if their earnings exceeded Canada's substantial gainful activity (SGA) threshold for that year. The SGA threshold was CA$5,300 in 2015, and was indexed to inflation in increments of $100 throughout the sample period.[10]

Annual earnings fall by an average of $4,121 (8%) during the year of hospitalization and the five subsequent years (Figure 1.1B, numbers reported in Table 1.2). Employment rates follow the same pattern as earnings around a hospitalization event (Figure 1.1C). There is an immediate 5 percentage point (6%) decline in employment during the year after a hospitalization, with no recovery five years later. The fact that employment rates decline by 6% and earnings decline by 8% suggests that the earnings decline is generated in large part by hospitalized individuals exiting the labor force. The next section explores the relationship between the earnings decline and employment decline in greater detail by studying the effect of hospitalization events on the distribution of earnings.

Distribution of Earnings

Figure 1.2A plots a histogram of the difference in the earnings distribution of hospitalized individuals and matched controls five years after the hospitalization event. To construct this figure, I first bin the earnings distribution into $5 000 bins ranging from $1 to $150 000, with separate bins for those with no earnings and those with earnings above $150 000. Appendix Figure A.3 directly plots the earnings distributions of hospitalized individuals and matched controls in years $r \in \{-5, \ldots, 5\}$. Figure 1.2A plots a histogram of the difference between those earnings distributions in year $r = 5$, while Appendix Figure A.4 plots the analogous

[10] Canada's substantial gainful activity threshold is defined by the *Disability Basic Exemption* of the Canada Pension Plan Disability (CPP-D) program. It is the maximum amount that a disabled CPP-D beneficiary can earn while retaining their CPP-D benefit eligibility. It is also the minimum amount a person must earn during the year to have that year count as a year of covered employed for the purposes of establishing a sufficient work history for CPP-D coverage.

histograms for each year $r \in \{-5, \ldots, 5\}$.

Five years after the hospitalization event, nearly all of the excess mass in the earnings distribution of hospitalized individuals is below \$5 000, which is below the substantial gainful activity threshold (Figure 1.2A). This pattern is potentially consistent with roughly 6 percent of hospitalized individuals experiencing large earnings declines because they cease working. However the same pattern could be caused by a larger number of hospitalized individuals experiencing small earnings declines, with the lower tail bunching near zero. This ambiguity is common to all estimates that measure the effect of a treatment on the distribution of outcomes (e.g. quantile regressions).

In order to distinguish between extensive and intensive margin earnings changes, I analyze how individuals' earnings evolve compared to their earnings before the hospitalization event. I define an individual's relative earnings gains or losses in year r by dividing their earnings in that year by their average earnings in the five years prior to the index year:

$$g_{ir} = \frac{y_{ir}}{(y_{i,-5} + y_{i,-4} + y_{i,-3} + y_{i,-2} + y_{i,-1})/5} - 1$$

I then bin this variable into 22 bins ranging from a "100% loss in earnings", "99 to 90% loss", ..., "90 to 99% gain", "100% or greater gain" (where this last bin includes everyone whose earnings more than doubled). Appendix Figure A.5 plots the distributions of relative earnings gains and losses among the hospitalized group and the matched control group during years $r \in \{0, \ldots, 5\}$. Figure 1.2B plots a histogram of the *difference* in the distributions of relative earnings gains and losses five years after the hospitalization event, and Appendix Figure A.6 plots the analogous histograms for each year from zero to five years after the hospitalization event.

Five years after a hospitalization event, hospitalized individuals are 5.5 percentage points more likely to have zero earnings or to be earning less than 10% of their average pre-hospitalization earnings (Figure 1.2B). They are only 1.0 percentage points more likely to be earning 10 to 80% of their pre-hospitalization average earnings. In other words, hospitalization events substantially increase the likelihood of a person losing nearly all of their earnings, without much change in the likelihood of a person experiencing more moderate earnings losses.

Heterogeneity in Earnings and Employment Effects by Income

Hospitalization events reduce average annual earnings by 8% and average annual employment by 6%, but these risks are not felt equally throughout the income distribution. To measure inequality in the labor market risks of hospitalization, I first construct percentile ranks of household income separately by age, sex and year. I then group people into equal-sized groups based on their household income percentile three years prior to the index year. By construction, each group has the same age and sex composition. As I document below, people with lower incomes experience hospitalization events more often, have larger subsequent declines in employment and lose a larger share of their earnings.

In my sample of 40- to 54-year-old adults with no disability claims and no hospitalizations in the previous three years, the annual hazard of hospitalization events is 41% higher in the bottom income decile than the top income decile (Appendix Figure A.7). Each year, 3.0% of adults in the bottom decile of household incomes experience hospitalization events, compared to 2.1% in the top decile of household incomes. Because adverse health events simultaneously cause income declines and increase the likelihood of future hospital admissions, the income gradient in hospitalization rates is even larger in the full population than among people with no recent disability claims or hospitalizations. Restricting the sample to people with no recent hospital admissions or disability claims mitigates this reverse causality and helps to identify the start of discrete health events.

The magnitude of the post-hospitalization earnings decline also varies across the income distribution. I measure inequalities in the earnings effects of a hospitalization by first splitting the sample by quintile or decile of household income three years prior to the index year. I then re-estimate the event study regression defined in equation (1.1) on each subsample.

Both the bottom and top income quintiles experience persistent declines in employment and earnings following a hospitalization, but the declines are substantially larger in the bottom quintile (Figure 1.3). Employment rates decline by 8 percentage points (13%) in the bottom income quintile and by 3 percentage points (3%) in the top income quintile during the year of the hospitalization and subsequent five years. For people in the bottom income quintile, hospitalization events cause a 17% decline in average annual earnings, which fall from $20,400 to $17,000. Average annual earnings decline by only 5% in the top income quintile, falling from $88,300 to $83,700. Although high-income individuals lose more dollars after a hospitalization than low-income individuals, I emphasize the percentage decline because it is a better reflection of the changes in labor supply and the labor market risk to be insured.

The labor market risks of hospitalization decline with income throughout the entire income distribution, as shown in Figure 1.4. This figure plots the average 6-year effect of hospitalization events on annual employment and annual earnings for each decile of household income prior to hospitalization. The percent decline is measured as the average loss during the year of hospitalization and five subsequent years, divided by the average value in those six years among the matched control group—the same percentage that was reported on the panels of Figures 1.1 and 1.3. Appendix Figure A.8 shows the declines in annual employment and annual earnings for each income decile in levels instead of percentages. Appendix Figures A.9 and A.10 show the underlying event study plots for each income decile.

The annual employment rate declines by 16% for hospitalized individuals in the bottom income decile but only declines by 2% in the top income decile (Figure 1.4; Table 1.2). This pattern reflects two underlying facts: people with lower incomes experience larger absolute declines in employment and those larger declines come off of a smaller base of people working (Appendix Figures A.8B and A.10). There is even greater inequality in earnings losses after a hospitalization. Earnings decline by 21% post-hospitalization among individuals in the bottom income decile and by 4% among individuals in the top income decile. At all income levels the percent decline in employment is more than half as large as the percent decline in earnings: extensive margin exits from the workforce generate the majority of post-hospitalization earnings declines throughout the income distribution.

1.4.2 Spousal Insurance Post-Hospitalization

Spousal labor supply can provide insurance against income risks within a household: spouses may choose to increase labor supply or enter the work force after a negative shock to household income (Ashenfelter 1980; Heckman and MaCurdy 1980; Lundberg 1985). Empirical studies have shown that spouses increase their earnings after their partner is laid off (Cullen and Gruber 2000; Stephens 2002) or after their partner's long-term disability insurance claim is rejected (Autor et al. 2017). Yet recent studies have found that spousal earnings decrease modestly or are unchanged after their partner is hospitalized (García-Gómez et al. 2013;

Fadlon and Nielsen 2017; Dobkin et al. 2018).

This section examines how spousal earnings respond to hospitalization events in the Canadian setting. Consistent with prior work, I show that spousal earnings decrease modestly post-hospitalization. I then study heterogeneity by pre-hospitalization income and show that the modest decline in spousal earnings occurs in households of all income levels. Even so, spousal earnings provide a passive buffer against income losses: the percent decline in household earnings is smaller than the percent decline in individual earnings post-hospitalization due to the presence of a second income. I conclude by discussing how the spousal earnings responses to hospitalization events are consistent with the existence of complementarities in spousal leisure post-hospitalization that dominate the income effects of uninsured income losses.

Average Spousal Earnings Response

To study spousal earnings responses to hospitalization events I must restrict the sample to taxfiling households, for whom marital status and spousal earnings are observable. 95% of individuals in my sample are in taxfiling households in each year (which is equal to the overall taxfiling rate among 40 to 54-year old Canadians). The effect of hospitalization events on earnings and employment in the taxfiling sample is virtually identical to the effect in the full sample (Appendix Figure A.2). I observe all labor income for the tax filing sample, including including self-employment earnings and earnings not reported on employer-issued tax slips. Appendix Figure A.11 shows that the effects of hospitalization events on labor income are very similar to the effects on employment earnings.

I estimate how spousal labor income responds to a hospitalization event among the subsample of individuals who were married three years prior to the index year using the event study regression defined in equation (1.1). I follow the evolution of spousal labor income from five years before to five years after the hospitalization event. Spousal labor income is equal to zero in years where the hospitalized or control individual is not married.

Figure 1.5A plots the event study of spousal labor income pooling all index years. Spousal labor income is similar among hospitalized individuals and matched controls prior to the hospitalization event, but 2% ($742) lower on average during the year of hospitalization and five subsequent years.

Heterogeneity in Spousal Earnings Responses by Income

The effects of hospitalization events on household labor income can be decomposed into the effect on the labor income of the hospitalized individual, the share of household income earned by the hospitalized individual, and the relative size of the spousal response:

$$\frac{\Delta L_{HH}}{\widetilde{L}_{HH}} = \frac{\Delta L_i + \Delta L_s}{\widetilde{L}_i + \widetilde{L}_s} = \underbrace{\frac{\Delta L_i}{\widetilde{L}_i}}_{\text{own response}} \cdot \underbrace{\frac{\widetilde{L}_i}{\widetilde{L}_i + \widetilde{L}_s}}_{\text{own earnings share}} \cdot \underbrace{(1 + \frac{\Delta L_s}{\Delta L_i})}_{\text{spousal response}} \tag{1.2}$$

where Δx denotes the average change due to the hospitalization event and $\widetilde{x}$ denotes the counterfactual value among the matched controls. These values are measured as the 6-year average during the year of the hospitalization event and the five subsequent years.

I examine heterogeneity in the role households perform in stabilizing post-hospitalization income losses by performing this decomposition separately by decile of household income three years prior to hospitalization. For this analysis, I include both single and married households. The presence of a second earner is a stabilizing force in some households but not others, and the results reflect this variation. Figure 1.5B plots, separately for each pre-hospitalization income decile, the average post-hospitalization decline in own labor income ($\Delta L_i/\widetilde{L}_i$), household labor income ($\Delta L_{HH}/\widetilde{L}_{HH}$) and household labor income purged of spousal labor supply responses ($\Delta L_i/\widetilde{L}_i \cdot \frac{\widetilde{L}_i}{\widetilde{L}_i+\widetilde{L}_s}$).

Hospitalized individuals lose a smaller share of their household earnings than their own earnings following a hospitalization event (Figure 1.5B). This occurs despite the fact that average spousal labor income declines modestly post-hospitalization for households in all income deciles: 6% in the bottom income decile, 2-3% in all other income deciles, as shown in Appendix Figure A.12.

The fact that the labor market risks of hospitalization are larger for low-income than high-income households remains true for household labor income as it was for the hospitalized individual's labor income. Among hospitalized individuals in the bottom income decile, own labor income falls by 20% while household labor income falls by 15%. In the top income decile, own labor income falls by 4% and household labor income falls by 3% (Figure 1.5B).

Discussion of Spousal Earnings Declines

The post-hospitalization decline in spousal earnings can be reconciled with increases in spousal labor supply following job displacements and disability insurance claim rejections by considering the respective effects of these events on spousal demand for leisure and income. Hospitalization events should have offsetting effects on a spouse's demand for leisure and demand for income. On the one hand, spousal labor supply may decrease due to complementarities in spousal leisure. Many spouses provide caregiving after an adverse health event, and spouses often choose to time their retirement together to jointly consume leisure activities. On the other hand, spousal labor supply may increase due to the income effect of their sick partner's earnings decline. The magnitude of this income effect will depend on the extent to which losses in post-hospitalization earnings are replaced by formal insurance and the extent to which the hospitalization event increases or decreases the marginal utility of consumption (i.e. state dependence in consumption preferences). By contrast, earnings losses following job displacements are imperfectly insured but should have a smaller effect on spousal leisure complementarity than adverse health events, due to the absence of caregiving demands and since unemployment spells following job displacements are usually temporary. Disability insurance claim rejections generate pure income effects which should unambiguously raise spousal earnings, since the effects of adverse health on spousal leisure complementarity and state dependence will be realized around the onset of the disability and not the time of disability benefit determination.

The fact that spousal labor supply declines modestly after a hospitalization event suggests that spouses' desire to reduce their labor supply due to complementarities in spousal leisure dominates spouses' desire to increase their labor supply to replace uninsured earnings losses. Insurance through spousal labor supply is a substitute for formal insurance (Fadlon and Nielsen 2018). If hospitalized individuals have full insurance against post-hospitalization income losses, which equalizes their marginal utilities of consumption pre- and post-hospitalization, then the income effect on spousal labor supply is zero and the negative effect of spousal leisure complementarities dominates. If hospitalized individuals are incompletely insured then the negative effect of leisure complementarities and the positive income effect operate in offsetting directions on spousal labor supply.[11]

[11] With extreme levels of state dependence, it is possible that the marginal utility of consumption within the household *falls* post-hospitalization despite the hospitalized individual's earnings decline—because con-

Since prior research has established that spousal labor supply is elastic to income shocks, we can conclude that on average there is sufficient formal insurance following hospitalization events that spouses aren't pressed into work to replace lost earnings. Differences in insurance generosity may help explain why Fadlon and Nielsen (2017) observe a decline in spousal earnings in Denmark while Dobkin et al. (2018) observe no change in the United States, since Danish social insurance provides greater replacement of lost earnings than American social insurance post-hospitalization. But it is impossible to say whether the level of insurance is optimal without making strong assumptions about state dependence in spousal leisure and household consumption preferences: spouses may have preferred to work even less if they had access to the optimal amount of insurance. In the next section I describe the level and distribution of social insurance offered by Canadian taxes and transfers after a hospitalization event, before proceeding in Section 1.5 to analyze how much hospitalized individuals would value additional insurance beyond what is provided by existing tax and transfer programs.

1.4.3 Social Insurance Post-Hospitalization

Income does not fall one-for-one with lost earnings: earnings losses after a hospitalization will be cushioned by the social insurance provided by government tax and transfer programs. Some of these tax-and-transfer programs are designed as formal insurance programs, like long-term and short-term disability insurance. But even the general design of the redistributive tax schedule and safety net transfer programs is a source of social insurance against labor market risks (Mirrlees 1974; Varian 1980).

This section examines the generosity, progressivity and sources of Canadian social insurance against hospitalization shocks. I begin with a framework that decomposes the social insurance provided by taxes and transfers into three components: the replacement rate of lost earned income by increased transfers, the stablilizing stream of transfer income independent of hospitalization, and the progressivity of the tax schedule. I then show that the Canadian tax and transfer system reduces post-hospitalization income losses by 23%. Two thirds of that social insurance comes from replacement of lost earned income, while one third of social insurance comes from the size of stable transfers and the decline in progressive tax

sumption is so much less valuable in the sick state. This is equivalent to the case of overinsurance from formal insurance sources, and would cause the income effect to reduce spousal labor supply.

rates as income declines. Canadian social insurance against the income risks of hospitalization is highly progressive, reducing income losses by 44% for the bottom income quintile and only 12% for the top income quintile. Nevertheless, low-income Canadians still bear larger average losses in after-tax income during the years after a hospitalization event.

Decomposing Social Insurance from Transfers and Taxes

I measure social insurance as the difference between the percent decline in income *before* taxes and transfers versus the percent decline in income *after* taxes and transfers. The effect of taxes and transfers on the percent decline in income is useful as a summary metric because welfare losses for a risk averse individual are well-approximated by the percent decline in consumption multiplied by their coefficient of relative risk aversion (Baily 1978; Chetty 2006b).

The social insurance provided by transfers and taxes can be decomposed into three components. The first component is the insurance that comes from replacing lost income with increases in transfers. These increases in transfers may include targeted insurance programs (like long-term disability insurance) as well as means-tested transfer programs (like cash welfare) for which individuals become newly eligible after the adverse event. The second component is the quantity of transfer income received independent of the adverse event, which provides a stream of income that is not affected by labor market risks. If individuals receive substantial transfer income then risks to earned income affect a smaller share of total income. The third component captures the insurance value of tax progressivity, as represented by the gap between average tax rates and marginal tax rates. When an individual's marginal tax rate is higher than their average tax rate, then as their earnings decline their average tax rate falls as well. By lowering an individual's tax burden when their income falls after the adverse event, progressive taxes provide social insurance.

These three components of social insurance can be derived algebraically as follows. Let Y denote income after taxes and transfers, let E denote income before taxes and transfers, let $\theta > 0$ denote transfers and let $\tau < 0$ denote taxes. So income after taxes and transfers is $Y = E + \theta + \tau$. For each variable, I will use Δx to indicate the average annual change due to the hospitalization event and $\widetilde{x}$ to indicate the counterfactual value absent hospitalization. Then the percent change in income after taxes and transfers is:

$$
\begin{aligned}
\frac{\Delta Y}{\widetilde{Y}} &= \frac{\Delta E + \Delta\theta + \Delta\tau}{\widetilde{E} + \widetilde{\theta} + \widetilde{\tau}} \\
&= \frac{\Delta E}{\widetilde{E}} \underbrace{\left(1 + \frac{\Delta\theta}{\Delta E}\right)}_{\text{(i)}} \underbrace{\frac{\widetilde{E}}{\widetilde{E} + \widetilde{\theta}}}_{\text{(ii)}} \underbrace{\left(\frac{\Delta E + \Delta\theta + \Delta\tau}{\Delta E + \Delta\theta}\right) \frac{\widetilde{E} + \widetilde{\theta}}{\widetilde{E} + \widetilde{\theta} + \widetilde{\tau}}}_{\text{(iii)}}
\end{aligned} \tag{1.3}
$$

The tax and transfer system scales losses in earned income ($\Delta E/\widetilde{E}$) by the four factors described above. Factor (i) is less than 1 and shrinks the post-tax-and-transfer income losses as transfers replace lost income ($\Delta\theta/\Delta E < 0$). Factor (ii) is less than 1 and declines as counterfactual transfers ($\widetilde{\theta}$) become larger relative to counterfactual earned income ($\widetilde{E}$). Factor (iii) contains two multiplicative terms: the first measures the marginal tax rate faced as earned income plus transfer income changes, and the second measures the average counterfactual tax rate.[12] If tax rates are progressive then the product contained in (iii) is less than 1, shrinking the losses in income after taxes and transfers.[13] In a linear tax schedule with a flat tax rate of τ, marginal tax rates are always equal to average tax rates and the tax schedule provides no social insurance:[14]

$$
\frac{\Delta E + \Delta\theta + \Delta\tau}{\Delta E + \Delta\theta} \cdot \frac{\widetilde{E} + \widetilde{\theta}}{\widetilde{E} + \widetilde{\theta} + \widetilde{\tau}} = \frac{(1-\tau)(\Delta E + \Delta\theta)}{\Delta E + \Delta\theta} \cdot \frac{\widetilde{E} + \widetilde{\theta}}{(1-\tau)(\widetilde{E} + \widetilde{\theta})} = 1
$$

In the next section, I apply this decomposition framework to study the average social insurance provided by Canadian taxes and transfers against the income risks of hospitalization. Then in the following section, I examine whether the social insurance provided varies by pre-hospitalization income level.

[12]If taxable income declines by a marginal amount ε, factor (iii) is equal to 1 minus the marginal tax rate. For non-marginal income declines, it is equal to the 1 minus the integral of the marginal tax rate measured over the segment of the income distribution covered by the decline in pre-tax income.

[13]State-dependent tax credits, such as tax deductions for medical expenses, are an additional source of social insurance provided through the tax schedule. These credits reduce the average tax rate paid on reduced earnings in the bad state, and therefore have the effect of reducing factor (iii).

[14]Varian (1980) describes the social insurance provided by a linear tax schedule with a demogrant (i.e. a basic income) funded by the taxes collected. In terms of the decomposition described by equation (1.3), a linear tax schedule with a demogrant provides social insurance solely because the demogrant increases the denominator in factor (ii). Only a nonlinear or piecewise linear progressive tax schedule with increasing marginal tax rates provides social insurance through the shape of the tax schedule itself.

Average Social Insurance

Figure 1.6A plots an event study of household income before taxes and transfers, estimated using regression equation (1.1) and pooling all index years. Household income before taxes and transfers is the sum of household labor income (studied in Section 1.4.2) and household nonlabor income; Appendix Figure A.13 shows that hospitalization events have little effect on nonlabor income. Figure 1.6B follows the same procedure as Figure 1.6A to plot an event study of household income *after* taxes and transfers, along with the changes in taxes and changes in transfers. The underlying event studies showing the changes in taxes and transfers are shown in Appendix Figure A.14. The individual components of labor income, nonlabor income, transfers and taxes are described in Appendix Table A.1.

During the six years following a hospitalization event, average household income falls by 4.4% ($5039) before taxes and transfers and by 3.4% ($3281) after taxes and transfers (Figure 1.6; Table 1.3). Transfer benefits increase post-hospitalization by an average of $803 annually, replacing 16% of lost household earnings (Appendix Figure A.14; Table 1.3). These increased transfers are derived primarily from public short-term disability benefits ($101 annual average, concentrated in the year of hospitalization), public long-term disability insurance benefits ($342 annual average, increasing over time) and cash welfare benefits ($153 annual average, flat over time). Other transfer benefits increase by a cumulative $208 annual average (Appendix Figure A.15).

The mean increase in transfers measured among all hospitalized individuals obscures the fact that these transfer programs are providing large transfer benefits to the subsample of the hospitalized group with the largest earnings losses. Recall that five years after a hospitalization event, there is a 5.6 percentage point decline in employment (defined as earnings above the substantial gainful activity threshold, shown in Figure 1.2A). The increase in recipients of long-term disability benefits and cash welfare benefits is almost the same size. Five years post-hospitalization, the number of individuals receiving public long-term disability benefits increases by 4.0 percentage points, and those recipients receive an average annual benefit of $12 045 (Appendix Figure A.16B). The number of individuals receiving cash welfare benefits increases by 1.4 percentage points five years post-hospitalization, and those cash welfare recipients receiving an average annual benefit of $9 978 (Appendix Figure A.16C).[15]

[15]Cash welfare benefits and long-term disability insurance benefits are not mutually exclusive. Individuals whose houshold income plus long-term disability benefits total less than their cash welfare benefit entitlement

These programs are targeted to individuals with little or no labor force attachment, and participation increases post-hospitalization by almost as much as employment rates decline.

Table 1.3 reports the decomposition of social insurance into the three components defined in equation (1.3). Applying logs to equation (1.3), I also calculate the share of social insurance provided by (a) transfers replacing lost earnings, (b) the size of transfers in counterfactual earned income plus transfers, and (c) tax progressivity:

$$\log(\frac{\Delta Y}{\widetilde{Y}}) - \log(\frac{\Delta E}{\widetilde{E}}) = \underbrace{\log\left(1 + \frac{\Delta\theta}{\Delta E}\right)}_{\text{(a)}} + \underbrace{\log\left(\frac{\widetilde{E}}{\widetilde{E}+\widetilde{\theta}}\right)}_{\text{(b)}} + \underbrace{\log\left(\frac{\Delta E + \Delta\theta + \Delta\tau}{\Delta E + \Delta\theta}\frac{\widetilde{E}+\widetilde{\theta}}{\widetilde{E}+\widetilde{\theta}+\widetilde{\tau}}\right)}_{\text{(c)}} \tag{1.4}$$

Canadian social insurance shrinks the relative income losses post-hospitalization by 23%, from a 4.4% average loss in earned household income to a 3.4% loss after taxes and transfers (Table 1.3). 65% of that social insurance comes from the increases in transfers post-hospitalization described above. Labor market risks only affect 96% of the average hospitalized individual's income stream since, if they are not hospitalized, 4% of their pre-tax income comes from transfers. The difference between 96% and 100% of income being at stake from labor market risks accounts for 16% of social insurance post-hospitalization. The remaining 19% of social insurance comes from the progressivity of Canada's tax system: their taxes fall at a marginal rate of 23% as their income declines post-hospitalization, which is greater than their average counterfactual tax rate of 19%. Appendix Figure A.14B plots the event study of the change in taxes owed post-hospitalization.

Many empirical papers focus on the replacement rate from transfer income as the sole source of social insurance, overlooking the two sources of social insurance highlighted by Varian (1980): the quantity of transfer income and the progressivity of the tax schedule. For example, Fadlon and Nielsen (2017) and Dobkin et al. (2018) focus exclusively on changes in pre-tax income after a hospitalization event. Dobkin et al. note that in the United States, "only about 10 percent of the [post-hospitalization] earnings decline is insured through social insurance. In Denmark, by contrast, [. . .] almost 50 percent of the earnings decline is insured through various insurance programs." But 10 percent and 50 percent are replacement rates of lost earnings in the United States and Denmark respectively. In

receive cash welfare benefits as a top-up to bring their income up to the entitlement level.

Canada the replacement rate is 16 percent, but I find that one third of social insurance against post-hospitalization income losses comes from the amount of counterfactual transfer income and the progressivity of the Canadian tax schedule. Comparing the replacement rates in Denmark and the United States almost certainly understates the discrepancy in social insurance between the two countries, since Denmark's tax-and-transfer system is larger and more progressive than in the United States.

Heterogeneity of Social Insurance by Pre-Hospitalization Income

Although people with lower incomes lose a larger share of their household earnings following a hospitalization event, the tax and transfer system can mitigate the inequality in risk by providing progressive social insurance. This section examines how the quantity and sources of social insurance vary for hospitalized individuals with different levels of pre-hospitalization income. I begin by plotting the fraction of lost earned income that is replaced by increased transfers during the six years including the year of the hospitalization and the five subsequent years. Figure 1.7A depicts these replacement rates separately by decile of household income three years before the hospitalization event and by transfer program: short-term disability, long-term disability, cash welfare or other transfers. The underlying event studies are plotted in Appendix Figures A.17 to A.20.

The replacement rates from transfer programs post-hospitalization are highly progressive, with greater replacement of lost household income for people who had lower incomes pre-hospitalization (Figure 1.7A; Table 1.3). Increased transfers replace 27% of losses in the bottom income quintile, 17% in the middle income quintile and 7% in the top income quintile. Low-income and high-income Canadians also rely on different transfer programs to insure their post-hospitalization income losses. In particular, cash welfare benefits replace almost 20% of income losses in the bottom income decile and 10% of income losses in the second lowest decile. These results highlight the value of safety net programs as a source of social insurance, and echo the findings of Low and Pistaferri (2015) who show that food stamps in the United States provide substantial insurance against disability risk.

Figure 1.7B shows how the cumulative social insurance from the Canadian tax and transfer system varies by pre-hospitalization income decile. For each income decile, I plot the percent decline in income *before* taxes and transfers and the percent decile *after* taxes and transfers during the six years post-hospitalization (as was shown for the full sample in Figure 1.6). A dashed line depicts the social insurance provided accounting only for the replacement rate provided by increased transfers (factor i in equation 1.3). Appendix Figures A.21 and A.22 show the underlying event studies for income before and after taxes and transfers.

The cumulative social insurance from Canadian taxes and transfers is highly progressive, shrinking relative income losses by 44% for hospitalized individuals from the bottom income quintile but only 12% for hospitalized individuals from the top income quintile (Figure 1.7B; Table 1.3). Uninsured income losses still loom larger for low-income Canadians. Average income after taxes and transfers falls by 6.1% in the bottom income quintile and 2.5% in the top income quintile. But the inequalities in income risk are much smaller because of progressive social insurance. Average income before taxes and transfers fell by 10.8% in the bottom income quintile and by 2.9% in the top income quintile following a hospitalization event.

Only 54-61% of social insurance for each income quintile is accounted for by increases in transfers post-hospitalization (Table 1.3; calculated using equation 1.4). Redistributive taxes and transfers increase the progressivity of social insurance. Individuals in the bottom income quintile bear smaller risk from labor market losses because 16% of their counterfactual income comes from transfers—for individuals in the top income quintile, only 1% of counterfactual income comes from transfers. Progressive tax rates provide social insurance across the income distribution, but account for a relatively larger share of social insurance in the top income quintile (28%) than in the bottom income quintile (14%). These results underscore the importance of measuring all of the social insurance effects of taxes and transfers in order to quantify the level and distribution of social insurance against the income risks of hospitalization events.

1.4.4 Summary of Results

The results in this section describe the distribution of labor market risks generated by hospitalization events and the distribution social insurance provided by Canadian taxes and transfers. I showed that adverse heath events associated with an inpatient hospital admission generate persistent declines in earnings, largely because of hospitalized individuals who cease working. Low-income individuals are more likely to stop working post-hospitalization and lose a larger share of their earnings. Spousal labor income declines modestly post-hospitalization across the income distribution, implying that spouses are not induced to increase their labor supply to offset uninsured income losses. And although the Canadian tax and transfer system provides more social insurance to low-income Canadians, they continue to have larger average losses in income after taxes and transfers.

1.5 Value of Additional Insurance

My empirical results show that progressive social insurance in Canada reduces the magnitude and inequality in the income risks of hospitalization, but does not eliminate those risks. Income after taxes and transfers falls post-hospitalization by an average of 6.1% in the bottom income decile and 2.5% in the top income decile. In this section, I develop a framework to analyze the welfare costs of that uninsured risk and the potential welfare gains from providing additional insurance. Guided by my empirical results, the framework I develop accounts for heterogeneity in income risks across the income distribution and for a heterogeneous distribution of losses within each income group.

1.5.1 Valuing Insurance with a Representative Agent

The optimal level of social insurance for the income risks of adverse health events, as with any risk, depends on three factors: the size of the losses, the shape of consumption preferences (risk aversion and state-dependence), and the fiscal externality of behavioral distortions generated by insurance (Chetty 2006b; Chetty and Finkelstein 2012; Hendren 2016). The first-best optimum would involve full insurance, equalizing the marginal utility of consumption across states so that the hospitalization event has no effect on marginal utility. But nearly all insurance programs distort incentives and behavior, generating costs that are not internalized by the insured individuals. For example, disability insurance generates moral hazard because of the wedge between the private returns to work and the social returns to work, so full insurance against disability risks would result in many individuals choosing not to work (Gruber 2000; Maestas, Mullen, and Strand 2013). Therefore the constrained optimal level of social insurance involves trading off the marginal benefits of increased insurance against the social marginal costs of the behavioral distortions (Pauly 1974; Baily 1978).

The most common approach to measuring the marginal benefits of social insurance is based on the formula derived by Baily (1978) and Chetty (2006b) using a representative agent model with a quadratic or cubic Taylor approximation around the agent's utility function. Under these assumptions, the gap in marginal utilities between the good state and the bad state can be approximated using the mean drop in consumption and parameters describing risk aversion and state depenedence (with state dependence usually assumed to be zero). The value of moving a dollar from the good state to the bad state of the world depends on the gap between marginal utilities in each state. Let $v(c_h)$ denote utility over consumption in the good state, $u(c_l)$ denote utility over consumption in the bad state, and $\gamma_v = -\frac{v''(c_h)c_h}{v'(c_h)}$ denote the coefficient of relative risk aversion in the good state. Let $\eta = \frac{u'(c_l)-v'(c_l)}{v'(c_h)}$ describe state-dependence in the utility function: the gap between the marginal utility of consumption in the good state and the bad state starting from equal levels of consumption. Then the marginal benefit of raising insurance, calculated using a quadratic approximation of the utility functions, is:

$$\frac{u'(c_l) - v'(c_h)}{v'(c_h)} = \gamma_v \frac{c_h - c_l}{c_h} + \eta \tag{1.5}$$

This formula—or the analogous cubic approximation which adds a term for the coefficient of relative prudence—has been applied widely in the empirical literature on optimal unemployment insurance (e.g. Gruber 1997; Chetty and Szeidl 2007; Kroft and Notowidigdo 2016; Hendren 2017) and more recently to optimal workers' compensation (Bronchetti 2012). Chetty (2006b) shows that this formula applies under a general class of dynamic models and that the approximation error from a cubic Taylor approximation to a CRRA utility function is small. But the approximation error generated by assuming a representative agent has received much less scrutiny.

The representative agent assumption is key to measuring the size of the risk using the mean drop in consumption. If the risk being insured is borne by heterogeneous agents who have heterogeneous losses, then the formula above using the mean change in consumption will understate the gap in the marginal utilities and the benefits of insurance (by Jensen's inequality).[16] Given the results shown above in Section 1.4, heterogeneous risks are certainly

[16] Andrews and Miller (2013) address a different type of heterogeneity than the one I consider here. They introduce heterogeneous risk aversion and demonstrate that the covariance between risk aversion and consumption losses is an important parameter in optimal insurance, because the value of insurance is higher if larger losses are borne by more risk averse agents (and smaller if the opposite is true). My analysis addresses an issue that occurs even under homogenous risk aversion: with heterogeneous losses, the Taylor approximation using the mean consumption loss is downward biased, and this bias increases with the level of heterogeneity.

relevant to the insurance of adverse health events, but not exclusively so. Browning and Crossley (2001) find very heterogeneous effects of unemployment insurance benefits on consumption and note that "this finding of considerable heterogeneity suggests that conclusions drawn from studies that use a representative agent framework (such as Baily (1978)) may be misleading."

In the next section, I derive a more general formula for the benefits of insurance that allows for heterogeneity in risks.

1.5.2 Valuing Insurance with Heterogeneous Agents

This section develops a static model that highlights the key economic parameters in the optimal insurance problem with heterogeneous agents. The model builds on the work of Bound et al. (2004) and Hendren (2016) and delivers an optimal insurance formula that generalizes the familiar Baily-Chetty formula in two ways. First, the marginal benefit of insurance will depend on the entire distribution of consumption losses, not just the mean loss. Second, the cost of insurance will depend on the fiscal externality generated by all behavioral distortions, which may include distortions other than the elasticity of extensive margin labor supply to insurance levels. As with Chetty (2006b), the model generalizes to a dynamic setting or to decision-making over a wider set of choice parameters (e.g. household decision-making) so long as agents optimize. Agent optimization is a crucial assumption because optimizing agents equalize their marginal utility over all choice parameters, which allows me to apply the envelope theorem and value transfers using individuals' marginal utilities of consumption.

Model Setup. Each individual i arrives with exogenous assets A_i. At the beginning of the period, they experience an adverse health event with exogenous probability p_i. I denote the sick state with a superscript S, and the healthy state with a superscript H. The realization of the health state can alter individuals' preferences by changing the marginal utility of consumption and the disutility of labor. It can also alter their budget constraint by changing their labor productivity and the tax-and-transfer schedule they face. After they learn their health state, individuals choose consumption (c_i) and labor supply (l_i) to maximize their state-specific utility function:

$$
\begin{aligned}
U_i^S(c,l) &= (1+\theta)\frac{c^{1-\gamma}}{1-\gamma} - f_i^S(l) \\
U_i^H(c,l) &= \frac{c^{1-\gamma}}{1-\gamma} - f_i^H(l)
\end{aligned}
$$

This utility function reflects the same two simplifying assumptions used in Bound et al. (2004). First, I assume that individuals have homogeneous preferences over consumption that exhibit constant relative risk aversion with CRRA coefficient γ. Second, I assume that utility is separable in consumption and labor supply. $f_i^S(\cdot)$ and $f_i^H(\cdot)$ are convex functions representing the individual's disutility of labor supply, which may differ between the sick state and the healthy state. The state dependence parameter θ describes the change in the marginal utility of consumption when sick. For example, $\theta = 0.1$ would indicate that consumption is 10% more valuable when sick than when healthy.

Individuals maximize their state-specific utility functions subject to a state-specific budget constraint:

$$
\begin{aligned}
c_i^S &= A_i + y_i^S(l_i^S) - \tau^S\left(y_i^S(l_i^S)\right) \\
c_i^H &= A_i + y_i^H(l_i^H) - \tau^H\left(y_i^H(l_i^H)\right)
\end{aligned}
$$

The budget constraint reflects the fact that individuals may be less productive when sick: $y_i^S(l) \leq y_i^H(l)$. They may also face a different tax-and-transfer schedule when sick, $\tau^S(y)$, than they face when healthy, $\tau^H(y)$, due to taxes and transfers that are conditioned on health or disability.

Value of Insurance. Let λ_i^S and λ_i^H denote the Lagrange multipliers which represent the value of a relaxation of individual i's budget constraint in the sick state and the healthy state respectively. Assuming individuals optimize over their choice parameters, the value of an additional dollar can be measured through its effect on utility when "spent" on consumption or leisure (by an application of the envelope theorem). Using the marginal utility of consumption to value an additional dollar yields:

$$
\begin{aligned}
\lambda_i^S &= \frac{\partial U_i^S}{\partial c_i^S} = (1+\theta)(c_i^S)^{-\gamma} \\
\lambda_i^H &= \frac{\partial U_i^H}{\partial c_i^H} = (c_i^H)^{-\gamma}
\end{aligned}
$$

The markup that individual i is willing to pay over actuarially fair insurance to transfer money from the sick state to the healthy state is therefore:

$$\text{WTP}_i = \frac{\lambda_i^S}{\lambda_i^H} = (1+\theta)\left(\frac{c_i^S}{c_i^H}\right)^{-\gamma} \tag{1.6}$$

Planner's Problem. The government can provide social insurance and transfer resources from the healthy state to the sick state using the state-specific tax-and-transfer schedules. Individuals pay $\tau^H(y_i^H)$ on their income in the healthy state and $\tau^S(y_i^S)$ in the sick state. To provide an optimal level of social insurance, the government adjusts taxes and transfers to maximize individuals' expected utility subject to a balanced budget constraint, taking into account the fact that individuals' optimal choices of $c_i^S, c_i^H, l_i^S, l_i^H$ are endogenous to the chosen tax-and-transfer schedules $\tau^S(y)$, $\tau^H(y)$:

$$\begin{aligned} &\max_{\tau^S(\cdot),\tau^H(\cdot)} \mathop{\mathbb{E}}_i p_i \cdot U_i^S(c_i^S, l_i^S) + (1-p_i)\cdot U_i^H(c_i^H, l_i^H) \\ &\text{s.t. } 0 = G = \mathop{\mathbb{E}}_i \; p_i \cdot \tau^S\left(y_i^S(l_i^S)\right) + (1-p_i)\cdot \tau^H\left(y_i^H(l_i^H)\right) \end{aligned}$$

Consider a marginal policy change $d\tau^S$, $d\tau^H$ that raises individual i's transfers by $-d\tau_i^S$ in the sick state and raises taxes by $d\tau_i^H$ in the healthy state, conditional on their current choices of y_i^S, y_i^H. The fiscal cost of this policy change can be decomposed:

$$\mathop{\mathbb{E}}_i\left[\frac{p_i}{(1-p_i)}\frac{-d\tau_i^S}{d\tau_i^H}\right] = 1 + \underbrace{\mathop{\mathbb{E}}_i\left[\frac{p_i}{1-p_i}\frac{dy_i^S}{d\tau_i^H}\tau^{S\prime}(y_i^S) + \frac{dy_i^H}{d\tau_i^H}\tau^{H\prime}(y_i^H)\right]}_{\equiv \alpha(d\tau^S, d\tau^H)} \tag{1.7}$$

Equation (1.7) shows that the cost of a marginal change in social insurance can be decomposed into the mechanical cost and the fiscal externality of individuals' behavioral responses on the government budget constraint (as in Hendren 2016). I denote the fiscal externality $\alpha(d\tau^S, d\tau^H)$.

Optimal Insurance. Finding a general solution to the optimal choice of τ^S, τ^H is equivalent to the optimal non-linear taxation problem with a tag for the realization of health shocks, and selecting a globally optimal tax schedule is beyond the scope of this paper. To make the social insurance problem more tractable, I will restrict my attention to the welfare effects of a simple adjustment to the existing tax and transfer system: a lump sum transfer to all individuals in the sick state. This structure is similar to the constant benefit studied in the Baily-Chetty model.

In a model with heterogeneous agents, the welfare implications of increasing social insurance will depend on the distribution of taxes used to finance the benefit. In general, raising taxes to finance an insurance transfer will affect welfare through both an *insurance effect* and a *redistribution effect* whenever the policy change generates transfers in expectation across individuals (Andrews and Miller 2013). Bound et al. (2004) demonstrate that the distribution of financing can be highly relevant in practice. They find a negative welfare effect of expanding U.S. Social Security Disability Insurance (SSDI) and show that it is driven by regressive transfers from the working poor to the disabled. Since my goal is to apply this model to estimate the value of a marginal increase in insurance, I will isolate the insurance effect of any policy change. A social planner can choose the level of redistribution separately from the level of insurance, and I do not want my welfare estimates to stem from assuming that the existing tax and transfer system provides too much or too little redistribution.

To isolate the insurance value of raising social insurance, I make the (unrealistic) assumption that the lump-sum transfer is financed by an individual-specific tax that depends on the individual's exogenous risk p_i. Specifically, I assume that each individual finances a marginal increase in benefits in the sick state by paying a tax in the healthy state equal to their actuarially fair price multiplied by a fixed markup that covers the fiscal cost of behavioral distortions. Then the tax in the healthy state is $d\tau_i^H = \frac{p_i}{1-p_i}\frac{1}{1+\alpha}$, where α is the fiscal externality defined in equation (1.7). A negative fiscal externality ($\alpha < 0$) requires a positive markup ($\frac{1}{1+\alpha} > 1$) for the policy to be budget neutral.

Proposition 1. Consider a marginal lump sum increase in transfers in the bad state, which occurs for individual i with probability p_i. Suppose increased transfers are financed by a tax equal to the actuarially fair price for each individual multiplied by a constant markup such that the policy change is budget neutral. Then this policy will raise social welfare if:

$$(1+\theta)\frac{\mathbb{E}_i\ p_i(c_i^S)^{-\gamma}}{\mathbb{E}_i\ p_i(c_i^H)^{-\gamma}} > \frac{1}{1+\alpha} \tag{1.8}$$

Discussion. Like the Baily-Chetty formula, equation (1.8) describes how the constrained optimal level of social insurance trades off the marginal benefit of transferring resources to the bad state (on the left-hand side) against the marginal cost of the behavioral distortions generated (on the right-hand side).

The left-hand side of equation (1.8) measures the gap in marginal utilities between the sick state and the healthy state, but requires more information than the Baily-Chetty version in equation (1.5). Without a representative agent and a Taylor approximation to the utility function, the gap in marginal utilities depends on the entire distributions of consumption in the sick state and the healthy state—not just the difference in average consumption in each state. Yet as I show in the next two sections, this formula has a natural implementation using the research design developed in Section 1.3 and delivers substantively different results.

The right-hand side of equation (1.8) measures the marginal cost of increasing transfers to the sick state induced by behavioral distortions that affect the government budget constraint. Baily (1978) and Chetty (2006b) assume the sole behavioral distortion is a change in effort to avoid the bad state, and the fiscal externality can therefore be represented as a behavioral elasticity. I have assumed above that the probability of the bad state is exogenous but that changes in taxes and transfers distort labor supply. Hendren (2016) emphasizes that the key is to measure all behavioral changes that affect the government budget—which naturally may differ for different types of social insurance policies. For the analysis I perform below the question of which behavioral distortions to measure for fiscal externalities is moot, since my setting lacks variation in social insurance policies that could be used to estimate behavioral responses. I therefore focus on estimating the value of additional insurance against the risks of adverse health events (the left-hand side). Estimating the costs of additional insurance remains an important topic for future research.

1.5.3 Connecting the Model to the Data

This section describes how I leverage my event study research design and the adminstrative tax data to estimate the value of a marginal increase in insurance against post-hospitalization income losses.

Consider the marginal value of a hypothetical lump sum transfer to all individuals who experience a hospitalization event financed by an actuarially fair tax. The left-hand side of equation (1.8) estimates this marginal value and has a natural implementation using my matched event study research design. The numerator $\mathbb{E}_i \; p_i(c_i^S)^{-\gamma}$ is the average value of $(c_i)^{-\gamma}$ within the hospitalized group. The denominator $\mathbb{E}_i \; p_i(c_i^H)^{-\gamma}$ is the average counterfactual value of $(c_i)^{-\gamma}$ absent the hospitalization event. The coarsened exact matching procedure described in Section 1.3.1 reweights individuals who were not hospitalized by approximating p_i using the matching variables. The average value of $(c_i)^{-\gamma}$ in the matched control group is equal to the desired counterfactual, assuming c_i was equal in the hospitalized and control groups pre-event and would have continued along the same trajectories absent the hospitalization event—as was shown for all the results in Section 1.4. The challenge, then, is estimating consumption (c_i) and the preferences parameters governing risk aversion (γ) and state dependence (θ).

I approximate consumption using a measure of consumption-equivalent (or "equivalized") disposable income, by dividing each taxfiling household's income after taxes and transfers by the square root of the household size.[17] Equivalized income exceeds the per capita household income because of economies of scale in consumption: a two person family can spend less than twice as much (on housing, food, etc.) to achieve comparable consumption utility. I bottom-code equivalized incomes at \$8000, which is approximately equal to annual cash welfare benefits for a single person during the study period. I measure each individual's equivalized income as their average during the five years following the hospitalization event: 85% of individuals in my sample filed taxes in all five years, for other households I average over their filing years. The effect of hospitalization events on the five year average of equivalized income approximates the effect on permanent income, not transitory income, and implicitly assumes that individuals are able to smooth their consumption across year-to-year income fluctuations.

The main advantage of estimating consumption using equivalized income is the level of distributional detail afforded by the administrative tax data. The literature on optimal unemployment insurance typically estimates the effect of unemployment on consumption using a first difference in food consumption from a few thousand respondents to longitudinal consumption surveys (e.g. Gruber 1997; Browning and Crossley 2001; Kroft and Notowidigdo 2016). Because longitudinal tax data are available for the full population, I am able to estimate the effect of hospitalization events on the entire distribution of equivalized income and estimate heterogeneity in those effects by pre-hospitalization income. Equivalized income also approximates a broader basket of consumption than food expenditures, allowing us to reason about welfare using individuals' risk preferences over disposable income rather than food expenditures. The fact that part of an individual's equivalized income may be spent on saving rather than consumption is not a source of bias under the assumption that individuals optimize, because optimizing agents equalize the marginal utility of their spending

[17] Numerous equivalence scales have been proposed to map household income to equivalized income as a function of the number of adults and children within the household. The square root scale used here is commonly used in recent OECD publications, but the results are comparable using alternative scales.

on consumption and savings.

The main weakness of the equivalized income measure is that it does not account for self-insurance through dissaving or informal insurance through transfers from friends or family outside the household. Additionally, some benefits from private disability insurance plans are not taxable, and are therefore not observable in the administrative tax data.[18] Equivalized income measures therefore understate the amount of insurance that individuals have under the status quo, and overstate the welfare benefits of a marginal increase in insurance.

There is no agreement in the literature on the appropriate values of risk aversion γ or state dependence θ, and I therefore follow standard practice and calibrate my estimates using a range of values.[19] Consumption may be more valuable when sick ($\theta > 0$) because of increased demand for health-related goods that were not valuable prior to their illness (like prescription drugs or mobility aids), as found by Lillard and Weiss (1997). On the other hand, consumption may be less valuable when sick ($\theta < 0$) because many types of goods are complements to good health (like travel or sports equipment), as found by Viscusi and Evans (1990) and Finkelstein, Luttmer, and Notowidigdo (2013). Because the literature provides scant and conflicting evidence on the value of θ, I calibrate the model using both positive and negative values for θ.

1.5.4 Estimates of the Marginal Value of Increased Insurance

Table 1.4 reports my estimates of the marginal value of an actuarially fair increase in transfers to individuals who experience a hospitalization event—the left hand side of equation (1.8), which I repeat here for convenience:

$$(1+\theta)\frac{\mathbb{E}_i\ p_i(c_i^S)^{-\gamma}}{\mathbb{E}_i\ p_i(c_i^H)^{-\gamma}} > \frac{1}{1+\alpha}$$

The right hand side is equal to the marginal cost of an insurance expansion, since the government cannot finance an actuarially fair insurance program if the insurance distorts

[18]Canadian private disability insurance benefits are considered nontaxable income and are not observable in the tax data if the insurance premiums were paid by entirely by employees out of their after-tax income. If any portion of the premiums were paid by employers using pre-tax income, the disability insurance benefit is taxable and is observable as part of the recipient's taxable income. To my knowledge, there are no national statistics on the share of private disability insurance benefits paid on a taxable/nontaxable basis.

[19]Chetty (2006a) and Chetty and Szeidl (2007) argue based on the elasticity of labor supply to wages that the coefficient of relative risk aversion over long-run income changes is approximately $\gamma = 1$. But Cohen and Einav (2007), Barseghyan, Prince, and Teitelbaum (2011) and Einav et al. (2012) find substantial heterogeneity in risk preferences across individuals and within individuals across decision-making contexts.

behavior and imposes a negative fiscal externality ($\alpha < 0$). Since I do not observe variation in the social insurance of hospitalization events that could be used to estimate fiscal externalities, I focus on estimating the marginal value of insurance using the procedure described in the previous section. An expansion of insurance would be welfare improving if the marginal value exceeds the marginal cost.

Panel A of Table 1.4 contrasts the marginal value of insurance estimated using equation (1.8) from the heterogeneous agent model to the marginal value of insurance estimated using equation (1.5) from the representative agent Baily-Chetty model. For now I assume that there is no state depenence ($\theta = 0$) and calibrate the model using coefficients of relative risk aversion spanning $\gamma = 1$ to $\gamma = 4$. I find that the representative agent model substantially understates the value of insurance given the heterogeneous losses individuals experience following a hospitalization event. At moderate risk aversion ($\gamma = 2$), individuals would be willing to pay a 12% markup for insurance that pays out if they experience a hospitalization event—twice as much as the 6% markup that a representative agent who experiences the mean post-hospitalization loss would be willing to pay.

The value of insurance against any event that generates heterogeneous losses will be understated by the Baily-Chetty formula. Heterogeneous losses correspond to a mean-preserving spread in the risk and unambiguously raise the marginal value of insurance. Of course, inpatient hospitalizations are an especially heterogeneous set of events to insure. Some conditions for which individuals are hospitalized cause no lasting health consequences while others cause permanent impairments, and section 1.4.1 showed that most of the decline in earnings is due to individuals who cease working entirely post-hospitalization. But even in the context of unemployment insurance, Hendren (2017) estimates that the maximum causal effect of unemployment shocks on consumption is twice as large as the mean effect.

Panel B of Table 1.4 calibrates the marginal value of insurance using varying levels of state dependence in the marginal utility of consumption, ranging from a 10% decrease in marginal utility post-hospitalization ($\theta = -0.1$) to a 10% increase in marginal utility ($\theta = 0.1$). At moderate risk aversion ($\gamma = 2$), if consumption is at least 10% less valuable following a hospitalization then the current level of social insurance is excessive. But if consumption were 10% more valuable following a hospitalization, individuals would be willing to pay a 23% markup to obtain additional insurance. These results underscore a fact noted throughout the optimal insurance literature: the value of insurance is very sensitive to the level of risk aversion and state dependence but there is tremendous uncertainty regarding the appropriate value of these parameters (Chetty and Finkelstein 2012; Finkelstein, Luttmer, and Notowidigdo 2013). Moreover, both risk aversion and state dependence are likely to be heterogeneous and correlated with the size of the losses post-event, which further complicates the welfare analysis relative to the homogenous preferences assumed here and throughout the optimal insurance literature (Andrews and Miller 2013). Estimating the distribution of these preference parameters and the correlation of preferences with the risk of losses remains a major stumbling block for normative analyses of optimal insurance, and an important challenge for research.

Panel C of Table 1.4 reports heterogeneity in the marginal value of insurance by quintile of household income three years prior to the hospitalization event. The mean percent loss in equivalized income declines with pre-hospitalization income, and is more than twice as large in the bottom income quintile (5.5% loss) than the top income quintile (2.5% loss). If each income quintile could be accurately described by a representative agent, then the marginal value of insurance would be more than twice as large for the bottom income quintile than the top income quintile at any level of risk aversion. Yet at moderate risk aversion ($\gamma = 2$) with no state dependence ($\theta = 0$), I estimate that the value of insurance is roughly flat across the income distribution. Individuals in the bottom quintile would be willing to pay an 11% markup, while individuals in the top quintile would be willing to pay a 10% markup. At higher risk aversion ($\gamma = 3$), the top income quintile values additional insurance more than the bottom income quintile.

The surprisingly high value of insurance to high income household is a result of how the social insurance system alters the distribution of risk faced by low-income and high-income Canadians in a way that is not captured by the mean loss in income. Recall that individuals with low incomes prior to the hospitalization event are more likely to cease working post-hospitalization, and that these extensive margin exits are the primary source of earnings losses across the income distribution (Figure 1.4). But transfer programs replace a larger share of lost earnings for low-income individuals (Figure 1.7A), and earnings are a smaller share of income after transfers for low-income individuals because of redistributive taxation (Table 1.3). As a result, low-income and high-income Canadians face different distributions of losses following a hospitalization event. In the bottom income quintile, the 5.5% mean decline in equivalized income reflects a relatively high probability of a relatively small loss after taxes and transfers. In the top income quintile, the 2.5% mean decline in equivalized income reflects a relatively low probability of a relatively large loss after taxes and transfers. As the coefficient of relative risk aversion rises, the utility loss from a gamble with a small probability of a large loss rises. And at $\gamma \approx 2$, the distribution of risks from hospitalization events after taxes and transfers is generates roughly equal valuations of additional insurance throughout the income distribution.

Analyzing social insurance through the lens of the mean loss in post-event income therefore understates both the insurance value and the progressivity of the social insurance system. The cumulative effect of progressive replacement rates, redistributive transfers and progressive tax rates shrinks the share of income lost post-hospitalization, as shown in Table 1.3. But risk averse agents care about the entire distribution of losses imposed by an event, and are especially averse to large losses. And the safety net catches low-income individuals after a shorter fall, limiting the maximum downside of labor market risks for low income individuals.

1.6 Conclusion

Illness is a major source of income risk for workers, even those with health insurance. This paper identifies the distribution of income risk from hospitalization events across the income distribution, before and after taxes and transfers. I find that hospitalization events cause long-term declines in average earnings, primarily due to extensive margin exits from the labor force. These declines in employment and earnings are especially large for individuals with low-incomes prior to hospitalization, and remain so even after the progressive social insurance provided by Canada's tax and transfer system.

In addition to measuring the quantity of social insurance provided, this paper also characterizes the nature and sources of social insurance against hospitalization risk. I show that redistributive taxes and transfers are a major source of social insurance against the labor market risks of hospitalization. Increases in means-tested transfers (such as cash welfare) provide substantial insurance for low-income individuals. And throughout the income distribution, more than one third of social insurance against hospitalization risk is provided by a combination of stable transfer income and progressive income tax rates. These findings complement the proposition from the optimal income tax literature that insuring labor market risk is a key function of redistributive taxes and transfers. I find that social insurance from redistributive taxation is quantitatively important for health risk, even in the presence of formal insurance programs like disability insurance. And more broadly, these results imply that it would be worthwhile to quantify the effects of potential tax reforms on the social insurance of labor market risk in addition to standard analyses of redistribution.

Finally, this paper shows how heterogeneity in the distribution of income losses, both across and within income groups, affects the marginal value of social insurance. Accounting for the heterogeneous losses in post-hospitalization income doubles the marginal value of insurance at moderate levels of risk aversion, and flattens the marginal value of insurance across the income distribution. These results demonstrate that in cases where the representative agent assumption breaks down, mean outcomes cease to function as a sufficient statistic for the value of insurance. The distributional effects quantified by this paper therefore constitute a useful input for a normative analysis of social insurance. And looking beyond hospitalization risk, estimating the distributional effects of other major labor market risks (such as layoffs) and evaluating the optimal level of social insurance for those risks given observed heterogeneity in outcomes constitutes a promising avenue for future research.

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Tables and Figures

Table 1.1: Summary Statistics Before and After Matching

	Before Matching		After Matching	
	Hospitalized in Index Year	Not Hospitalized in Index Year	Hospitalized in Index Year	Not Hospitalized in Index Year
Female	56%	50%	56%	56%
Age in Index Year	47.3	46.7	47.3	47.3
40-44	30%	35%	30%	30%
45-49	35%	35%	35%	35%
50-54	35%	30%	35%	35%
Province in Index Year				
BC, AB, SK	39%	36%	39%	39%
ON	49%	53%	49%	49%
NS, NB, PEI, NL	12%	10%	12%	12%
Married, Year r=–3	75%	78%	75%	75%
Own Earnings, Year r=–3	48,566	53,357	48,577	48,616
Household After-Tax Income, Year r=–3	85,183	89,114	85,231	85,373
Individuals, Pooled	738,287	29,634,402	736,329	27,416,731
Index Year 2003	92,907	3,482,454	92,633	3,220,275
Index Year 2004	98,626	3,651,566	98,367	3,392,106
Index Year 2005	96,400	3,669,779	96,136	3,409,538
Index Year 2006	93,701	3,715,958	93,440	3,445,536
Index Year 2007	91,818	3,749,024	91,571	3,464,987
Index Year 2008	88,989	3,786,882	88,777	3,494,782
Index Year 2009	88,989	3,786,882	88,777	3,494,782
Index Year 2010	86,857	3,791,857	86,628	3,494,725
Unique Individuals	6,503,370		6,369,043	

Notes: This table presents frequencies, means and counts from the analysis sample, before and after the matching procedure. The sample before matching consists of Canadians ages 40 to 54 living in eight provinces in the index year, with no inpatient hospitalizations or disability claims in the three prior years, who have been employed at least once during the five prior years (as described in Section 1.2.2). The matching procedure selects hospitalized and unhospitalized individuals in the same index year with the same age, sex, province of residence one year prior, marital status one year prior and earnings decile three years prior (as described in Section 1.3.1). Unmatched individuals are excluded from the sample after matching.

Table 1.2: Effects of Hospitalizations on Individual Earnings and Employment, Household Labor and Nonlabor Income

	Full Sample	Household Income Quintile 3 Years Pre-Hospitalization				
		Bottom	2nd	3rd	4th	Top
Employment Earnings						
6-Year Average Post-Hospitalization	47,309	16,985	31,783	41,367	50,858	83,717
Change in Dollars	-4,121	-3,429	-3,960	-4,132	-4,226	-4,576
Percent Change	-8.0%	-16.8%	-11.1%	-9.1%	-7.7%	-5.2%
Employment Rate (Earnings > Substantial Gainful Activity)						
6-Year Average Post-Hospitalization	77.4	55.4	75.1	80.3	84.0	85.8
Change in Dollars	-5.0	-8.1	-6.3	-5.0	-3.9	-2.6
Percent Change	-6.0%	-12.7%	-7.7%	-5.8%	-4.5%	-2.9%
Household Labor Income						
6-Year Average Post-Hospitalization	99,245	32,476	57,222	81,684	110,282	186,721
Change in Dollars	-4,945	-4,142	-4,313	-4,831	-5,175	-5,666
Percent Change	-4.7%	-11.3%	-7.0%	-5.6%	-4.5%	-2.9%
Household Nonlabor Income						
6-Year Average Post-Hospitalization	9,536	2,481	3,954	5,916	8,405	23,427
Change in Dollars	-95	-92	-33	24	-25	-525
Percent Change	-1.0%	-3.6%	-0.8%	0.4%	-0.3%	-2.2%

Notes: This table presents the average effects of hospitalization estimated using the event study regressions in equation (1.1), pooling all index years. The effects by household income quintile 3 years pre-hospitalization are produced by re-estimating equation (1.1) on the five subsamples. The 6-year average effects include the year of hospitalization and the five subsequent years. For each outcome, this table reports the average among individuals who are hospitalized, the level difference between the hospitalized group and the control group, and the percent difference (which is the level difference divided by the average outcome for those who were not hospitalized). The estimates for employment earnings and employment rate are calculated using the full sample. The estimates for household labor income and household nonlabor income are calculated using the weighted sample of taxfilers (described in further detail in Section 1.3.1).

Table 1.3: Decomposition of Social Insurance from Transfers and Taxes

	Full Sample	Household Income Quintile 3 Years Pre-Hospitalization				
		Bottom	2nd	3rd	4th	Top
Panel A: Effects of Hospitalization on Average Income						
Household Income *Before* Tax and Transfers						
6-Year Average Post-Hospitalization	108,781	34,957	61,177	87,600	118,687	210,147
Change in Dollars	-5039	-4234	-4347	-4807	-5200	-6191
Percent Change	-4.4%	-10.8%	-6.6%	-5.2%	-4.2%	-2.9%
Household Income *After* Tax and Transfers						
6-Year Average Post-Hospitalization	93,522	39,222	59,595	79,552	102,623	164,019
Change in Dollars	-3281	-2542	-2691	-3111	-3419	-4248
Percent Change	-3.4%	-6.1%	-4.3%	-3.8%	-3.2%	-2.5%
Panel B: Decomposition of Social Insurance						
Reduction in % Income Decline Due to Taxes and Transfers	**23%**	**44%**	**35%**	**28%**	**23%**	**12%**
Share of Social Insurance from Increased Transfers	65%	54%	61%	59%	58%	61%
Share of Social Insurance from Relative Size of Transfer Income	16%	31%	21%	17%	13%	11%
Share of Social Insurance from Progressive Taxation	19%	14%	18%	24%	29%	28%

Notes: Panel A presents the average effects of hospitalization, estimated using the event study regressions in equation (1.1), pooling all index years, among the weighted sample of taxfilers. Panel A is constructed analogously to Table 1.2. Panel B presents the reduction in the percentage decline in income due to taxes and transfers, calculated as 1 – (% Change After Tax and Transfers / % Change Before Tax and Transfers). Panel B also presents the results of the algebraic decomposition of the sources of social insurance calculated using equation (1.4).

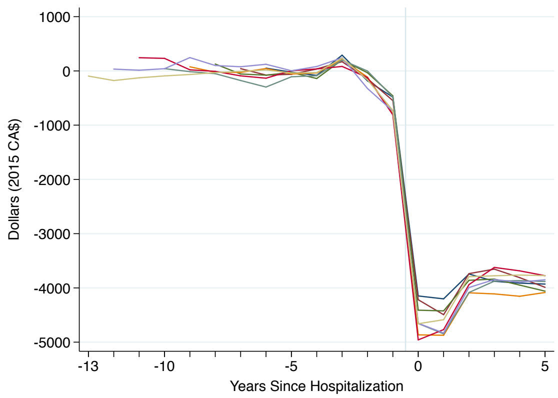
Table 1.4: Marginal Value of Increasing Transfers Post-Hospitalization

	Mean Change in Equivalized Income Post-Hospitalization	Marginal Value of Actuarially Fair Transfer to Hospitalized Individuals			
		Coefficient of Relative Risk Aversion			
		γ=1	γ=2	γ=3	γ=4
Panel A: Full Population, No State Dependence					
Heterogeneous Individuals	-3.0%	1.05	1.12	1.16	1.17
Representative Agent Approximation	-3.0%	1.03	1.06	1.09	1.13
Panel B: Full Population, Varying State Dependence					
Relative Value of Consumption Post-Hospitalization					
θ=−0.10	-3.0%	0.95	1.00	1.04	1.05
θ=−0.05	-3.0%	1.00	1.06	1.10	1.11
θ=0	-3.0%	1.05	1.12	1.16	1.17
θ=0.05	-3.0%	1.10	1.17	1.22	1.22
θ=0.10	-3.0%	1.16	1.23	1.27	1.28
Panel C: By Pre-Hospitalization Income, No State Dependence					
Household Income 3 Years Pre-Hospitalization					
Bottom Quintile	-5.5%	1.06	1.11	1.13	1.12
2nd Quintile	-3.6%	1.06	1.13	1.21	1.25
3rd Quintile	-2.9%	1.05	1.12	1.22	1.29
4th Quintile	-2.4%	1.04	1.10	1.17	1.20
Top Quintile	-2.5%	1.04	1.10	1.19	1.22

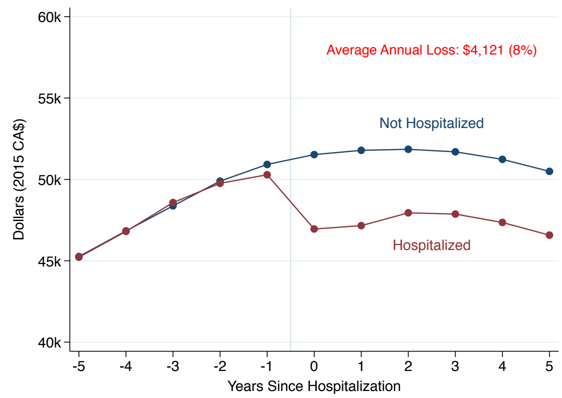
Notes: Column 1 reports the mean change in equivalized income during the five years post-hospitalized, calculated as household income divided by the square root of household size using the weighted taxfiler sample. Columns 2 to 4 report the marginal value of an actuarially fair increase in lump sum insurance to all individuals who experience a hospitalization event, calculated using the left hand side of equation (1.8) in all rows except for the "representative agent approximation", which uses the Baily-Chetty formula described in equation (1.5). Section 1.5.3 describes the formulas and estimation in more detail.

Figure 1.1: Hospitalization Events Reduce Average Earnings and Employment

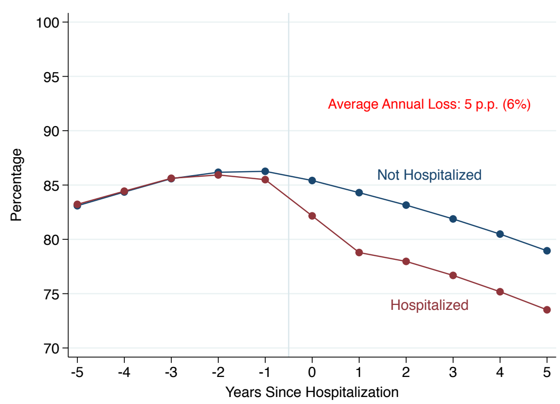
(A) Difference in Annual Earnings,
Plotting Each Cohort



(B) Annual Earnings,
Pooling Cohorts



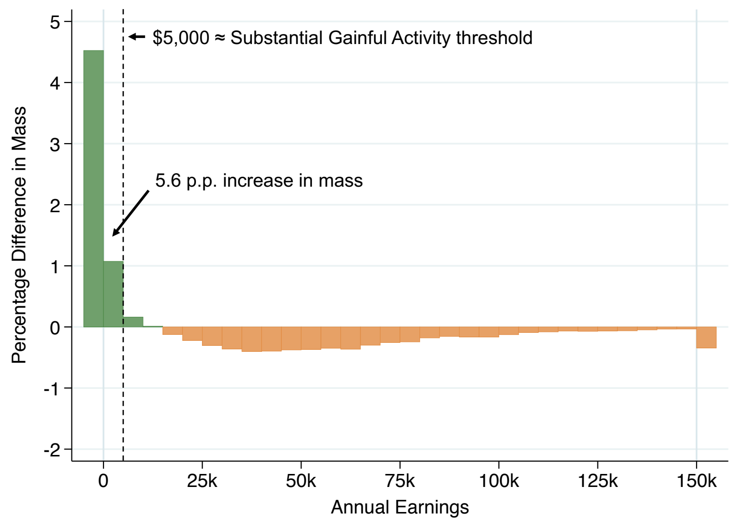
(C) Annual Employment Rate,
Pooling Cohorts



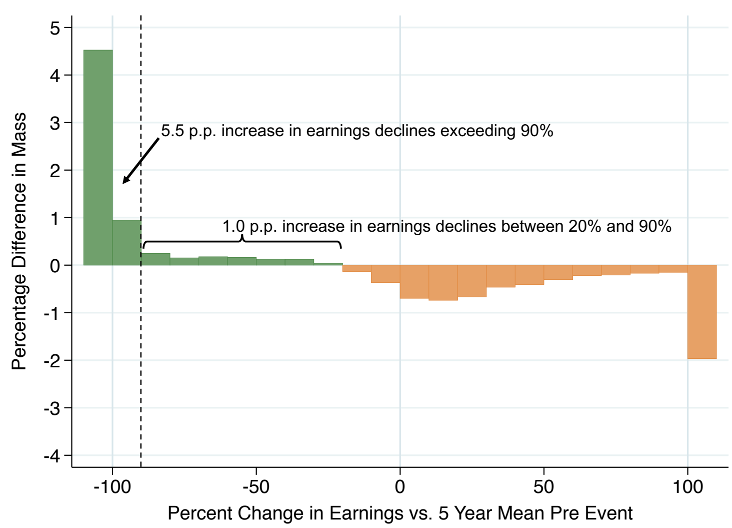
Notes: Panel A plots the difference in annual earnings between the hospitalized and matched control group estimated using regression equation (1.1), with a separate series for each of the eight index years from 2003 to 2010. Each series spans from 1997 (the first year of observed data) until five years after the index year. In Panels B and C, the Hospitalized series plots the mean observed outcomes of people who were hospitalized. The Not Hospitalized series plots the mean counterfactual outcome, estimated by pooling the results of regression equation (1.1) in each index years. In all panels, the mean difference during relative years -5 to -3 is normalized to zero. Average annual losses are measured as a six-year average over relative years 0 to 5. Annual earnings are measured using T4 tax slips issued by employers. The annual employment rate is the fraction of people with earnings greater than the substantial gainful activity threshold for that calendar year ($5,300 in 2015). The sample includes both taxfilers and nonfilers.

Figure 1.2: Post-Hospitalization Earnings Losses Are Primarily Extensive Margin

(A) Difference in Distribution of Earnings,
Relative Year 5



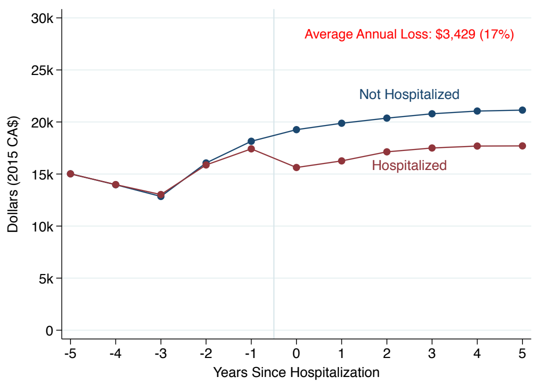
(B) Difference in Distribution of Earnings Losses,
Relative Year 5 vs. Relative Years -5 to -1



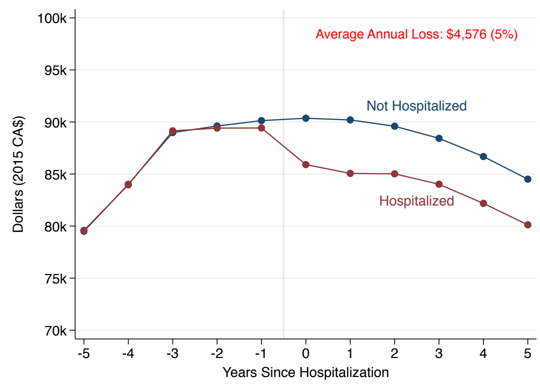
Notes: Panel A plots a histogram of the difference in the distribution of earnings between hospitalized individuals and the matched control group, five years after the hospitalization event. The distribution is discretized in \$5 000 bins ranging from \$1 to \$150 000, with separate bins for those with no earnings and earnings above \$150 000. Panel B plots a histogram of the percent change in individuals' earnings in the 5th year following the hospitalization event compared to a 5-year average pre-hospitalization. The distribution is discretized into 22 bins ranging from "100% loss in earnings", "99 to 90% loss", ..., "90 to 99% gain", "100% or greater gain". Appendix Figures A.3 to A.6 plot the underlying distributions and histograms of the differences for each year $r \in \{-5, \ldots, 5\}$.

Figure 1.3: Heterogeneity in Earnings and Employment Effects by Pre-Hospitalization Household Income Quintile

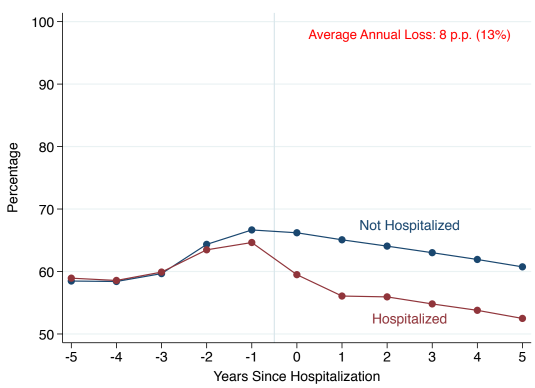
(A) Annual Earnings, Bottom Quintile



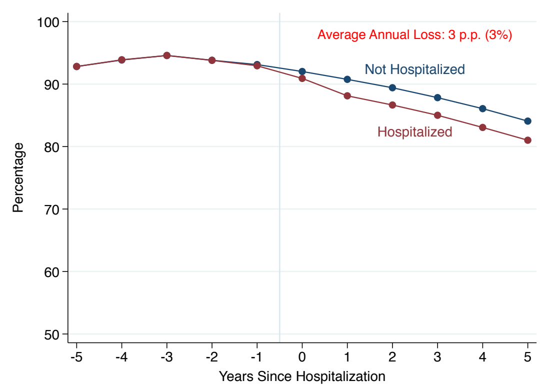
(B) Annual Earnings, Top Quintile



(C) Annual Employment, Bottom Quintile

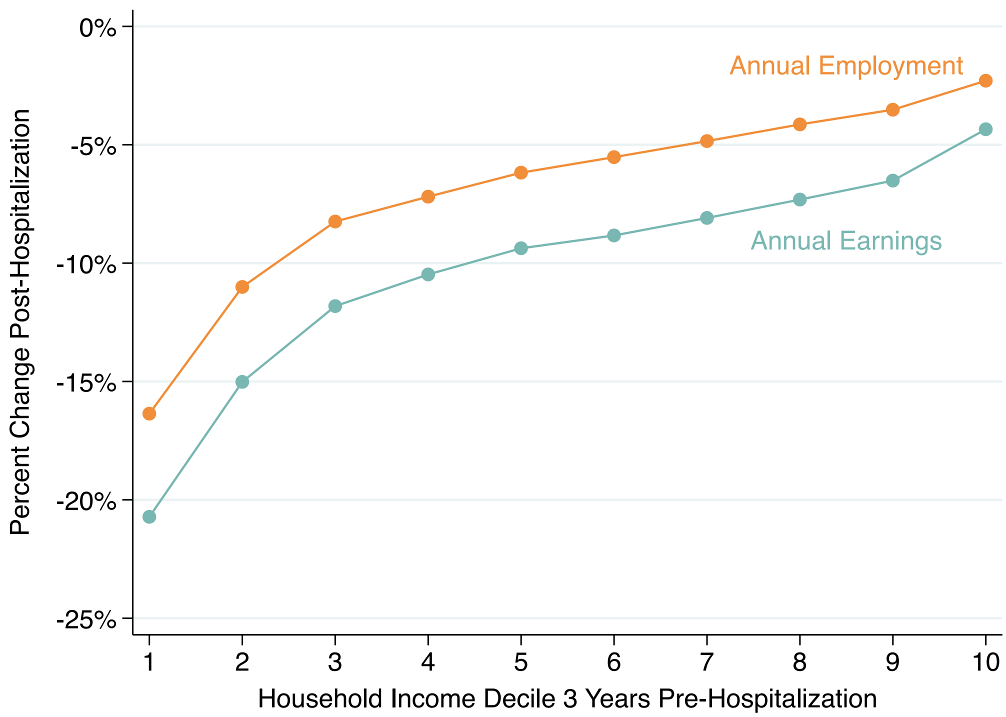


(D) Annual Employment, Top Quintile



Notes: Panels A and B replicate Figure 1.1B using the subsamples of individuals who were in the bottom or top quintile of the household income distribution three years prior to the hospitalization event. Panels C and D replicate Figure 1.1C using the same two subsamples. Household incomes were assigned to quintiles separately by age, sex and year. Then regression equation (1.1) was estimated on the subsample of individuals in each quintile. See the notes to Figure 1.1 for details on how the plots are constructed.

Figure 1.4: Low-Income Hospitalized Individuals Have Larger Relative Losses

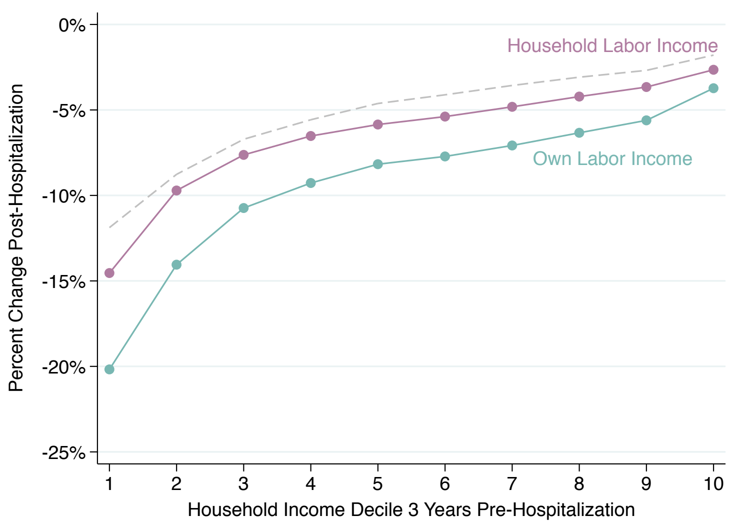


Notes: The percent change in annual employment and annual earnings is defined as the average loss during relative years 0 to 5 compared to the average outcome during those six years of people who were not hospitalized—as reported in red text on Figures 1.1 and 1.3. These percent changes are estimated and displayed separately by decile of household income three years pre-hospitalization (computed separately for each age, sex and year). Appendix Figure A.8 is analogous to this figure, but shows the declines in levels instead of percentages. The losses post-hospitalization are estimated using regression equation (1.1), and the corresponding event study plots are shown in Appendix Figures A.9 and A.10.

Figure 1.5: Spousal and Household Labor Income Decline Post-Hospitalization

(A) Spousal Labor Income

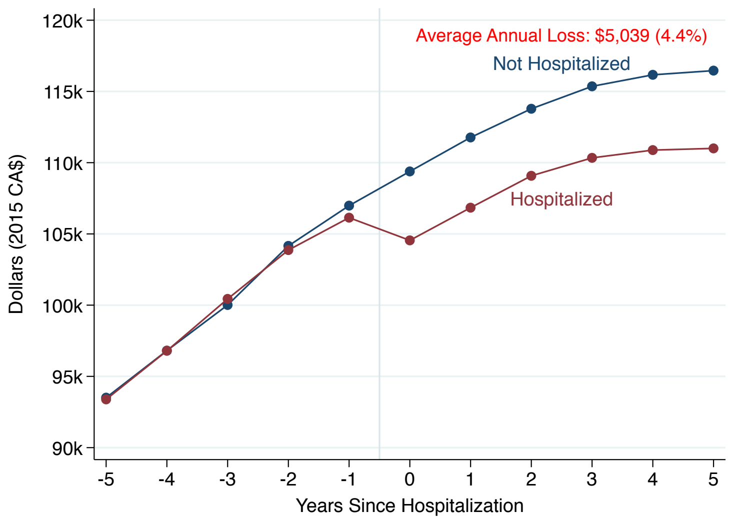
(B) Post-Hospitalization Losses in Household Labor Income



Notes: Panel A plots an event study of labor income earned by the spouses of individuals hospitalized and the matched controls, estimated using the weighted sample of tax filers who were married in relative year -3. See the notes to Figure 1.1 for details on how the plot is constructed. The components of labor income are defined in Appendix Table A.1. Panel B plots the six-year effect of hospitalization on the percent change in individual labor income and household labor income, averaged over the year of hospitalization and five subsequent years. These effects are estimated using the weighted sample of tax filers, separately by decile of household income three years pre-hospitalization. The dashed line plots the percent change in household income purged of spousal labor supply responses, as described in equation (1.2).

Figure 1.6: Changes in Taxes and Transfers Insure Post-Hospitalization Income Losses

(A) Household Income Before Taxes and Transfers

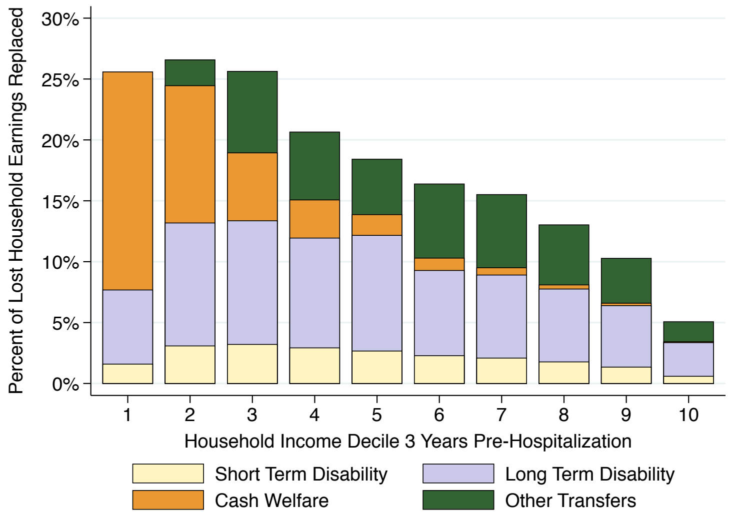


(B) Household Income After Taxes and Transfers

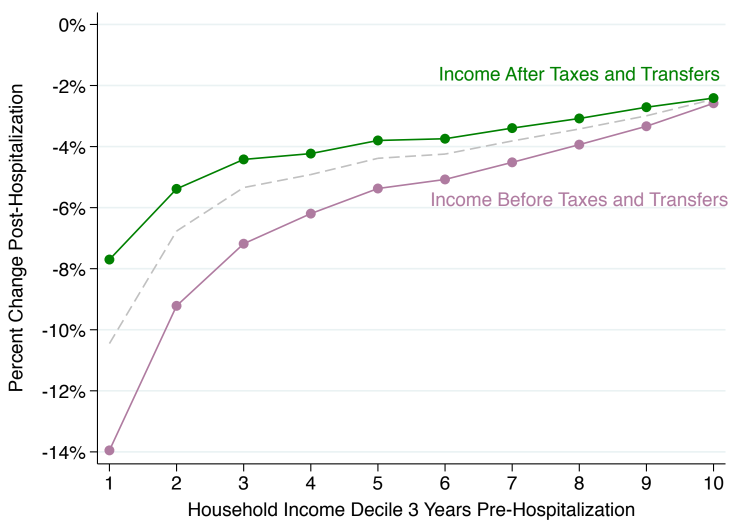
Notes: Panel A plots an event study of household income before taxes and transfers, estimated using the weighted sample of tax filers. Panel B plots the analogous event study for household income after taxes and transfers. In Panel B, the average increases in transfers and decreases in taxes for hospitalized individuals are computed using the same regression equation (1.1) and plotted as shaded areas: the underlying event studies for transfers and taxes are shown in Appendix Figure A.14. Panel B also plots the counterfactual income of hospitalized individuals holding transfers and taxes fixed as a dashed line beneath the shaded areas. See the notes to Figure 1.1 for details on how each plot is constructed. The components of income, taxes and transfers are defined in Appendix Table A.1.

Figure 1.7: Social Insurance Against Income Risks is Progressively Distributed

(A) Replacement Rate from Increased Transfers



(B) Distribution of Income Losses
and Social Insurance



Notes: Panel A plots the percentage of earnings losses insured by increased transfers, averaged over the year of hospitalization and the five subsequent years. Panel B plots the percentage of post-hospitalization losses in household income before and after taxes and transfers during the same six years. These effects are estimated using the weighted sample of tax filers, separately by decile of household income three years pre-hospitalization (computed separately by age, sex and year). The dashed line in Panel B shows the percent change in household income accounting only for the replacement rate provided by increased transfers plotted in panel A, without accounting for the effects of stable transfers and progressive taxation. The dashed line is calculated by incorporating factor (i) while omitting factors (ii) and (iii) from equation (1.3). The underlying event study plots are shown in Appendix Figures A.17 to A.22.

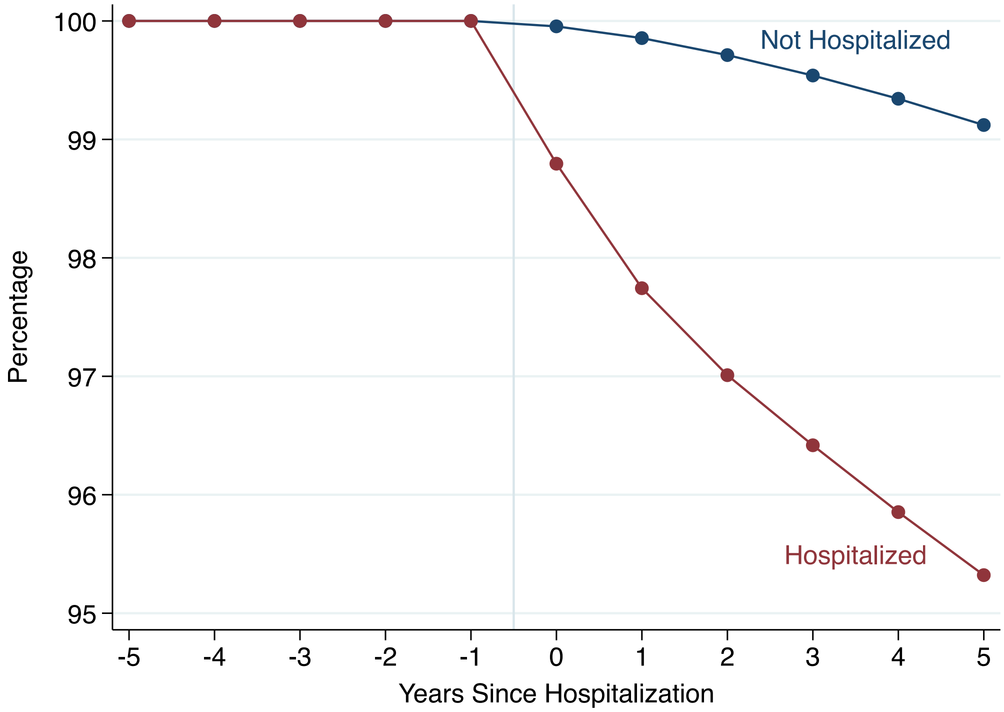
Appendix Tables and Figures

Appendix Table A.1: Definitions of Income Components

Income Components	Statistics Canada Variable Name	Definition
Labor Income		
Employment earnings	T4E	Total earnings from T4 'Statement of Remuneration Paid' slips
Self-employment income	SEI	Sum of net self-employment income from: business, professional, commission, farming, fishing, Indian exempt self-employment
Other employment income	OEI	Other employment income (ex: tips, director's fees. varies over time.)
Exempted income for Status Indians	EXIND	Employment income for a Status Indian exempted from income tax
Nonlabor Income		
Limited partnership income	LTPI	Net partnership income for limited or non-active partners of a partnership that did not include a rental or farming operation
Dividends	XDIV	Amount of dividends received by the taxfiler
Interest and other investment income	INVI	Income earned from interest such as government bonds, corporate bonds, trusts, bank deposits, mortgages, notes, foreign dividend income, etc.
Rental income, net	RNET	Net income from rental income after costs and expenses are deducted
Alimony or separation allownaces	ALMI / TALIR	Taxable income received from a former spouse for spousal support (alimony) and/or for child support (maintenance)
Other income	OI	Taxable income not listed elsewhere on the tax return (ex: artist's project grants, research grants, retiring allowances, etc.)
Pension and superannuation income	SOP4A	Pension income from private sources, including foreign pensions
RRSP income of individuals aged 65 or over	RRSPO	Income from Registered Retirement Savings Plans (RRSPs) at ages 65+
Transfers and Credits		
Old Age Security pension	OASP	The pension is part of the Old Age Security program, a federal porgram that provides financial security to Canadian Seniors. Excludes Guaranteed Income Suppment (GIS) and Spousal Allowance (SPA): see NFSL.
Canada/Quebec pension plan	CQPP	Income received from Canada or Quebec Pension Plan (CPP/QPP), such as retirement, disability, survivor's, children's and death benefits.
Net federal supplements	NFSL	Combination of Guaranteed Income Supplement (GIS), Allowance for the Survivor, and Spouse's Allowance which are parts of the Old Age Security (OAS) program for seniors with low or no income
Employment insurance	EINS	Employment insurance benefits including layoff, sickness, injury, pregnancy, birth or adoption of a child
Goods and services tax credit	GHSTC	Sales tax credit intended to offset the cost of the General Sales Tax (GST) for lower income individuals and families
Provincial refundable tax credits	PTXC	Provincial refundable tax credits (ex: Child's Fitness, healthy homes renovation tax credit, etc.)
Refundable medical expenses	MDREF	Refundable tax credit that can be claimed for medical expenses incurred by low income residents of Canada
Social assistance	SASPY	Social assistance provided to meet the cost of basic requirements for a single person or a family
Workers' compensation	WKCPY	Payments received for worker's compensation for eligible injuries
Child tax benefits	CTBI	Income supplement for individuals with at least 1 qualified dependent child
Family benefits	FABEN	Benefits received from Family Allowance and family benefits from both federal and provincial programs
Universal child care benefit	UCCB	An amount of $100 paid for each dependent child under age 6
Registered disability savings plan	RDSP	Income from a registered disability savings plan for persons with long-term disability who hold a valid disability certificate (Government provides deposits and matching into RDSP)
Working income tax benefit	WITB	Federal refundable tax credit with a basic amount and a disability supplement for low-income individuals who are in the workfroce
Taxes		
Federal taxes	NFTXC	Income tax required to be paid to the federal Government of Canada
Provincial taxes	NPTXC	Income tax required to be paid to a provincial government
Quebec abatement	ABQUE	Reduction of the federal income tax paid by Quebec residents

Notes: These income components are reported on Canadian tax returns or tax slips. The Statistics Canada variable names are used by the Income Statistics Division in their databases such as the T1 Family File (T1FF) and the Longitudinal Administrative Database (LAD). Further details about these variables can be found in the data dictionary for the LAD (Statistics Canada 2016).

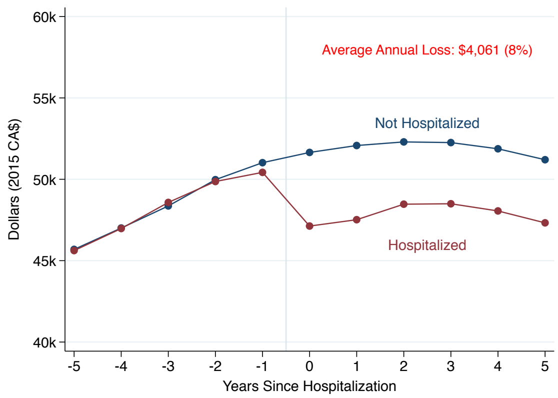
Appendix Figure A.1: Survival Rates Post-Hospitalization



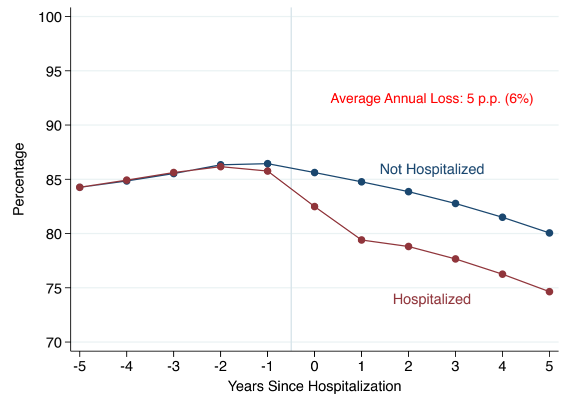
Notes: This figure plots an event study of survival rates for individuals who were hospitalized in the index year and the matched control group, estimated using regression equation (1.1) and pooling all index years. Individuals are excluded from the analysis sample during the years in which they are deceased, so this figure illustrates the attrition over time due to mortality. Section 1.3.1 describes how survivors in the matched control group during each year $r \geq 0$ are reweighted to match survivors in the hospitalized group.

Appendix Figure A.2: Effect of Hospitalization Events on Earnings and Employment Among Taxfilers

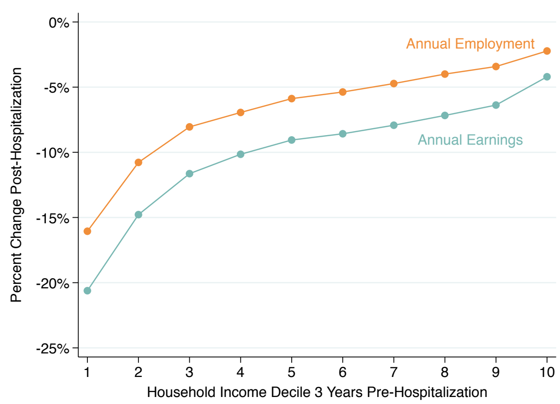
(A) Annual Earnings



(B) Annual Employment

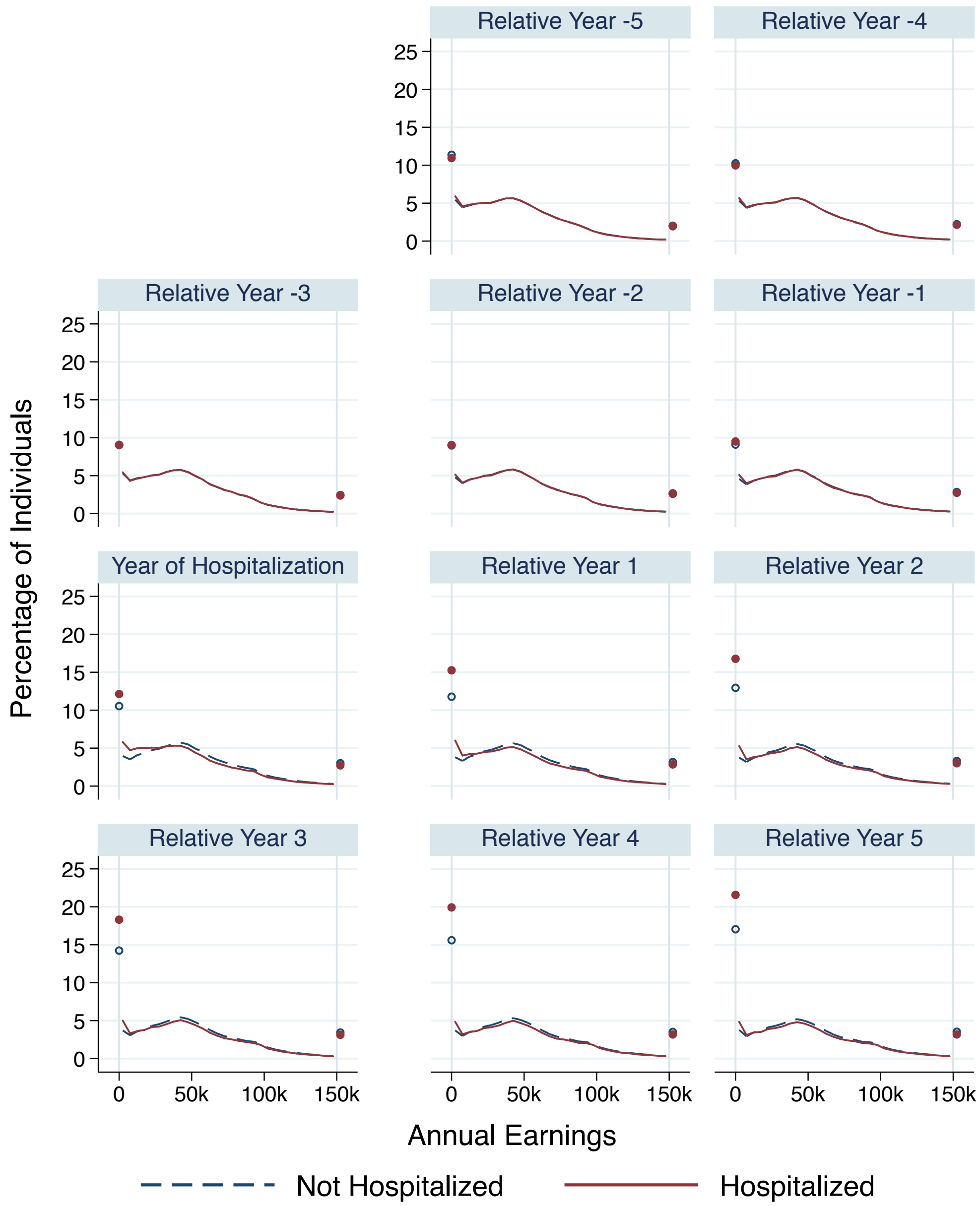


(C) Inequality in Earnings and Employment Losses by Pre-Hospitalization Income



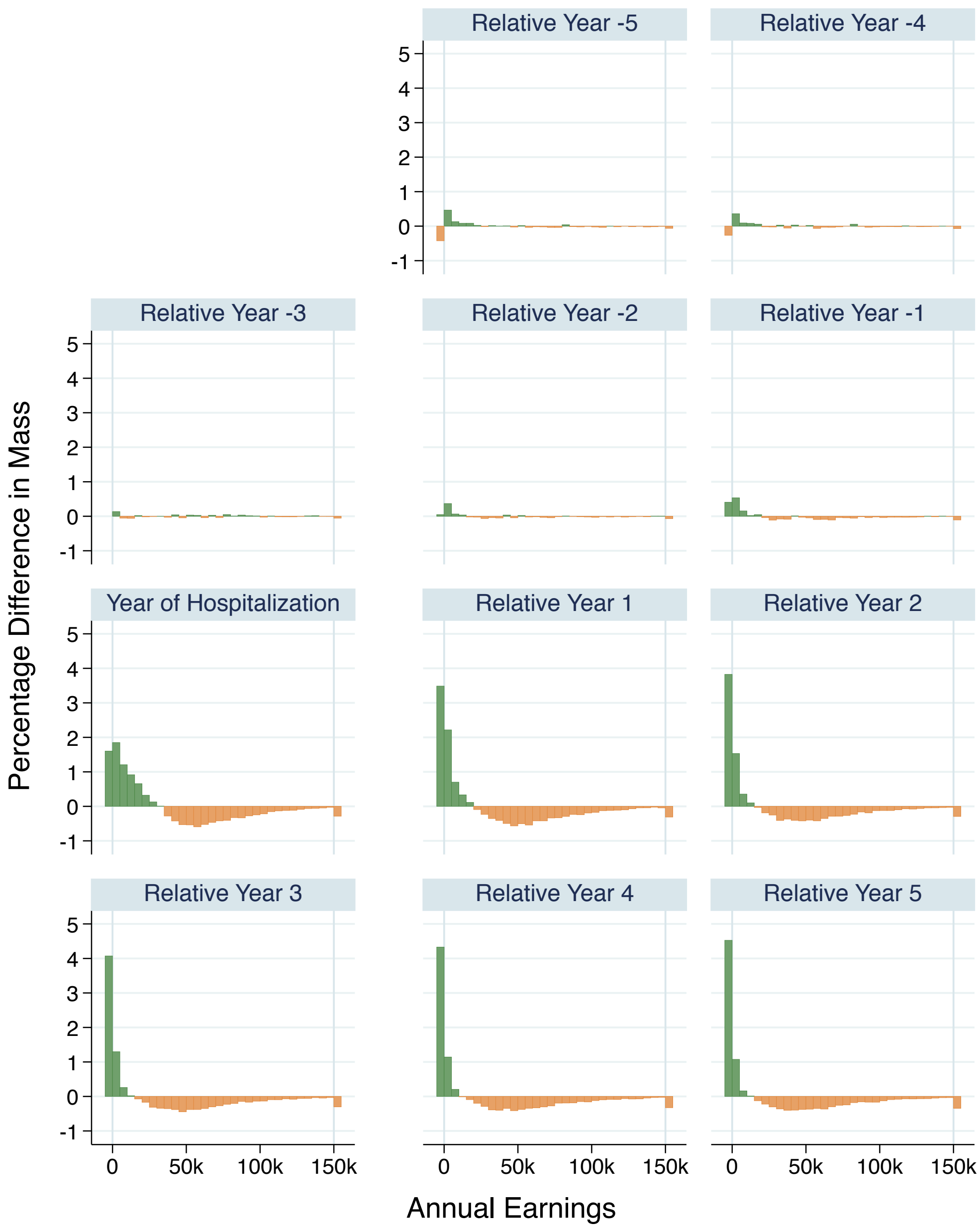
Notes: This figure replicates the results reported in Section 1.4.1 using the subsample of taxfiling households instead of all surviving individuals. For hospitalized and control individuals in each index year cohort and relative year, the taxfiling sample is reweighted to match the full sample using the same cells of fully interacted matching variables that were initially used to match hospitalized individuals to controls, as described in Section 1.3.1. Panel A replicates Figure 1.1B. Panel B replicates Figure 1.1C. Panel C replicates Figure 1.4.

Appendix Figure A.3: Distributions of Earnings Each Year Pre- and Post-Hospitalization



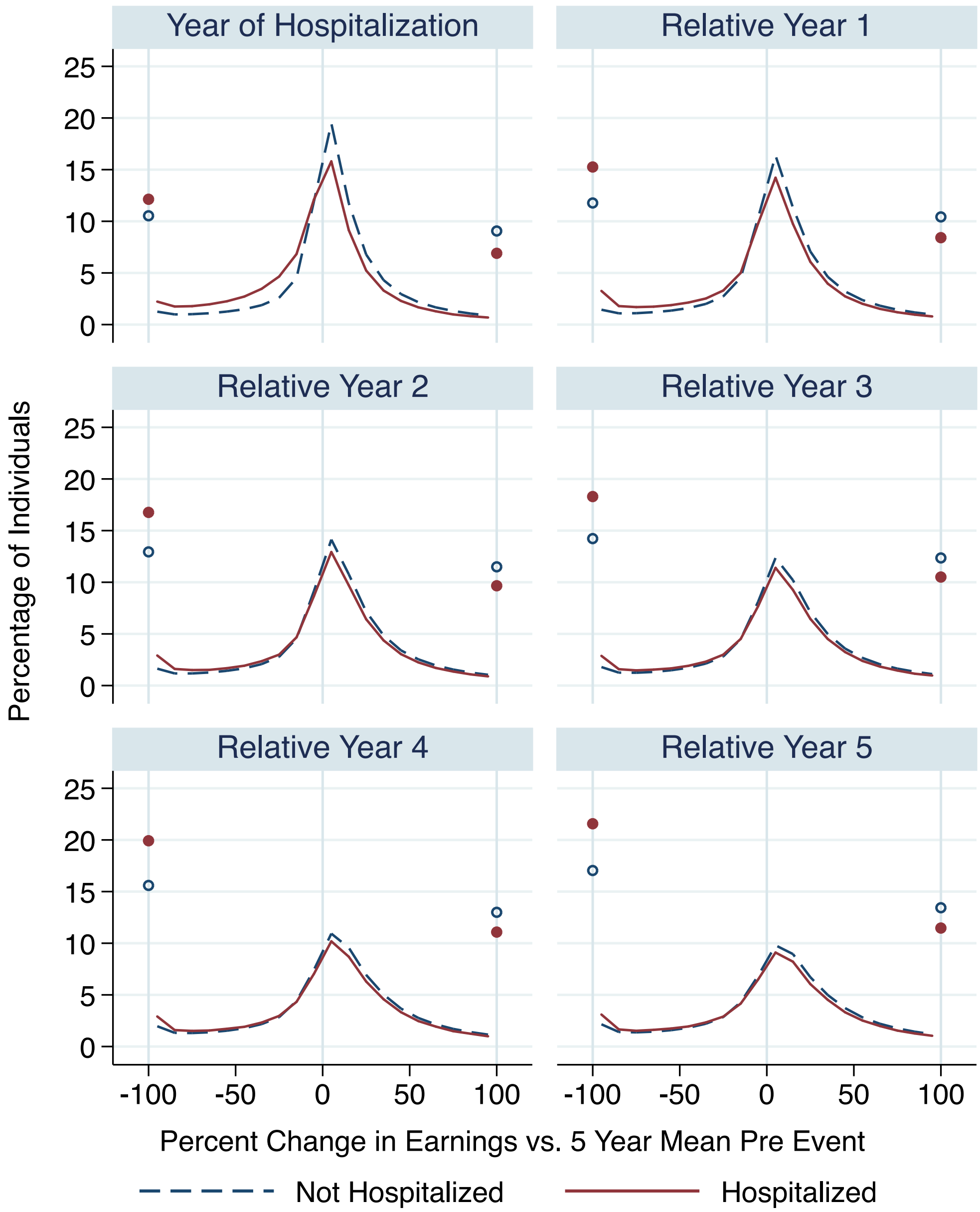
Notes: This figure plots the distribution of earnings of hospitalized individuals and the matched controls during each relative year $r \in \{-5, \ldots, 5\}$. The distribution is discretized in \$5 000 bins ranging from \$1 to \$150 000 (shown as lines), with separate bins for those with no earnings and earnings above \$150 000 (shown as dots).

Appendix Figure A.4: Differences in Distributions of Earnings Each Year Pre- and Post-Hospitalization



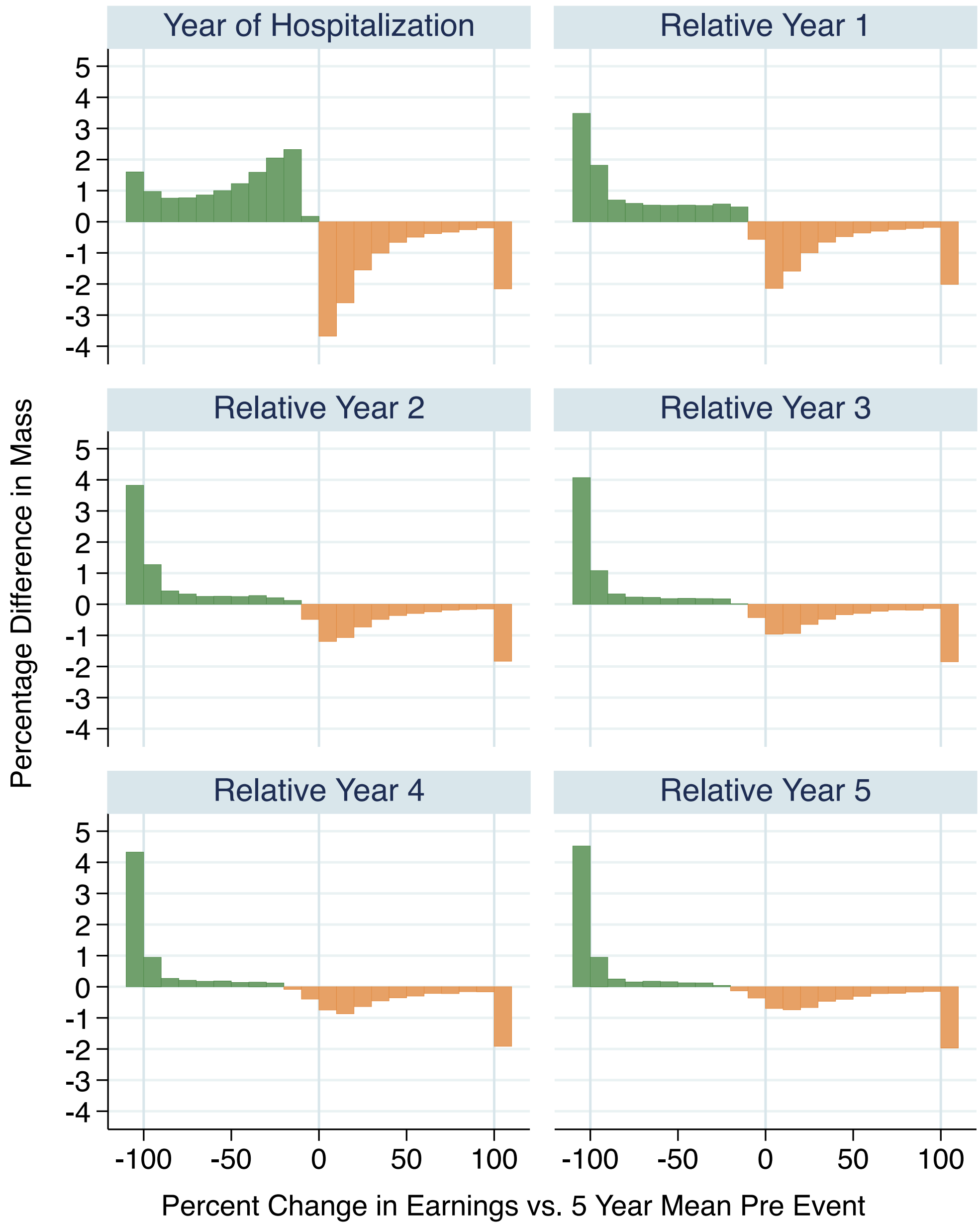
Notes: This figure plots histograms of the differences in the distribution of earnings between hospitalized individuals and the matched controls during each relative year $r \in \{-5, \ldots, 5\}$. The underlying distributions are plotted in Appendix Figure A.3. The histogram shown here for relative year 5 is identical to Figure 1.2A.

Appendix Figure A.5: Distributions of Earnings Changes Post-Hospitalization Relative to Pre-Hospitalization Mean



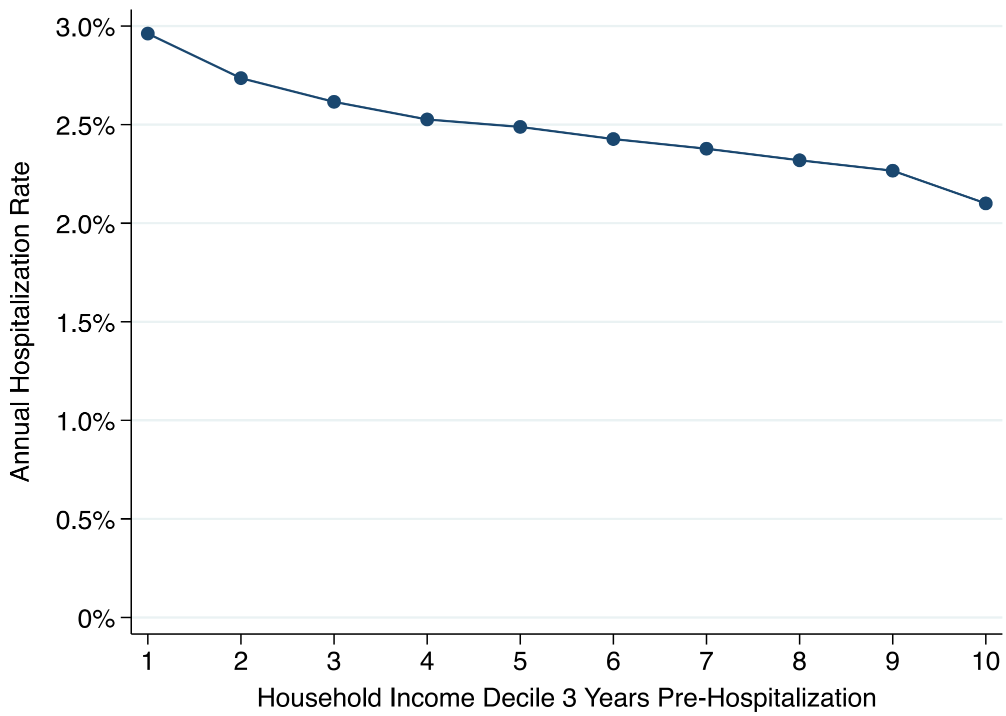
Notes: This figure plots the distribution of earnings changes during each relative year $r \in \{0, \ldots, 5\}$ compared to the 5-year average prior to the index year. The distribution is discretized into 22 bins ranging from "100% loss in earnings", "99 to 90% loss", ..., "90 to 99% gain", "100% or greater gain". The first and last bins are plotted as dots and all middle bins are plotted using lines.

Appendix Figure A.6: Differences in Distributions of Earnings Changes Post-Hospitalization Relative to Pre-Hospitalization Mean



Notes: This figure plots histograms of the differences between hospitalized individuals and the matched controls in the distribution of earnings changes during each relative year $r \in \{0, \ldots, 5\}$ compared to the 5-year average prior to the index year. The underlying distributions are plotted in Appendix Figure A.5. The histogram shown here for relative year 5 is identical to Figure 1.2B.

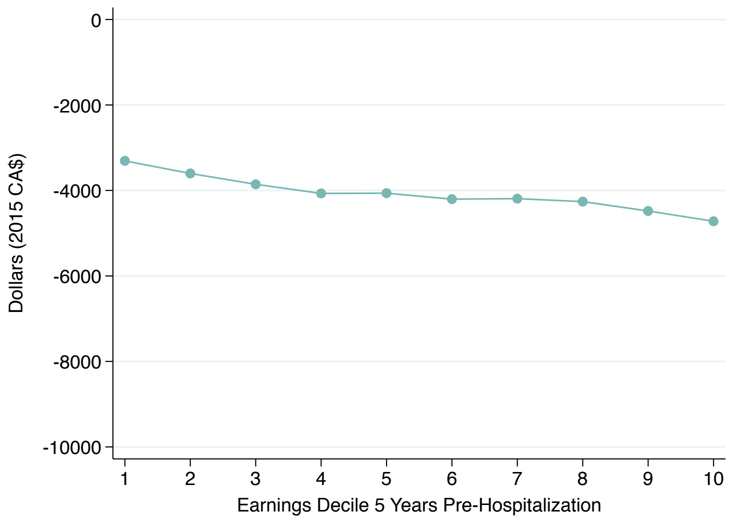
Appendix Figure A.7: Annual Hospitalization Rates in Analysis Sample by Pre-Hospitalization Household Income



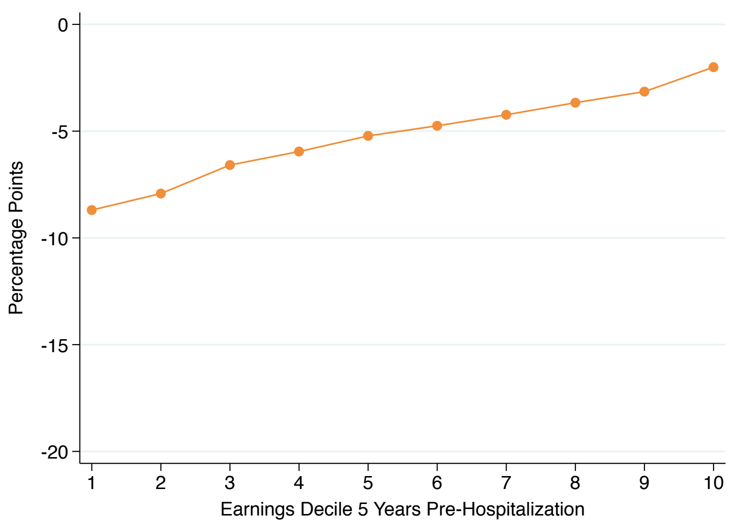
Notes: This figure plots the annual rate of inpatient hospitalization events among 40 to 54 year old Canadians in the analysis sample prior to the matching procedure (i.e. the sample described by Columns 1 and 2 of Table 1.1). These hospitalization rates are estimated separately in each of the eight index years (2003 to 2010) by decile of household income three years pre-hospitalization (computed separately for each age, sex and year). The hospitalization rates displayed for each decile are the unweighted average over the rates estimated in each index year.

Appendix Figure A.8: Heterogeneity by Pre-Hospitalization Household Income Decile in Absolute Losses in Earnings and Employment

(A) Annual Earnings

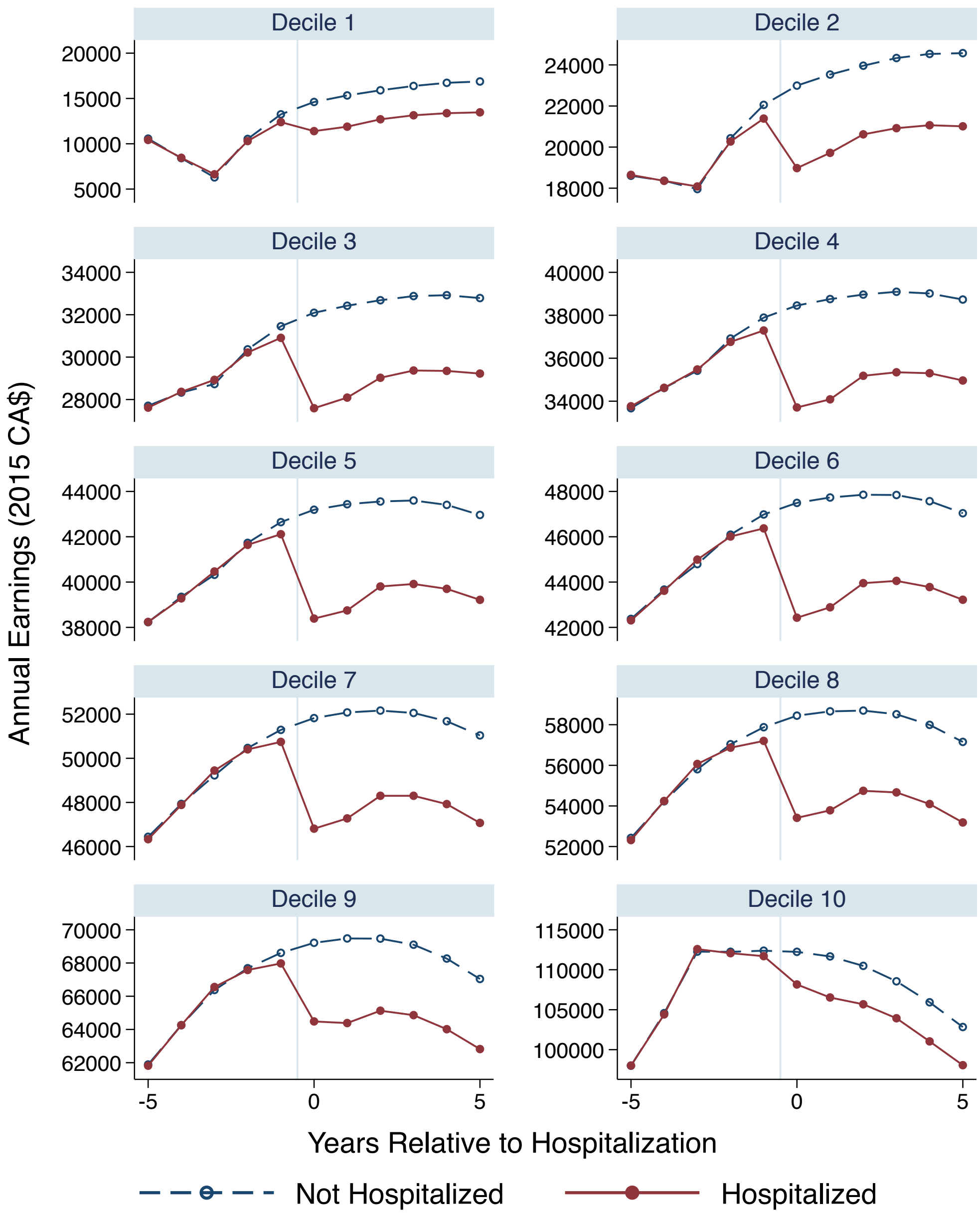


(B) Annual Employment Rate



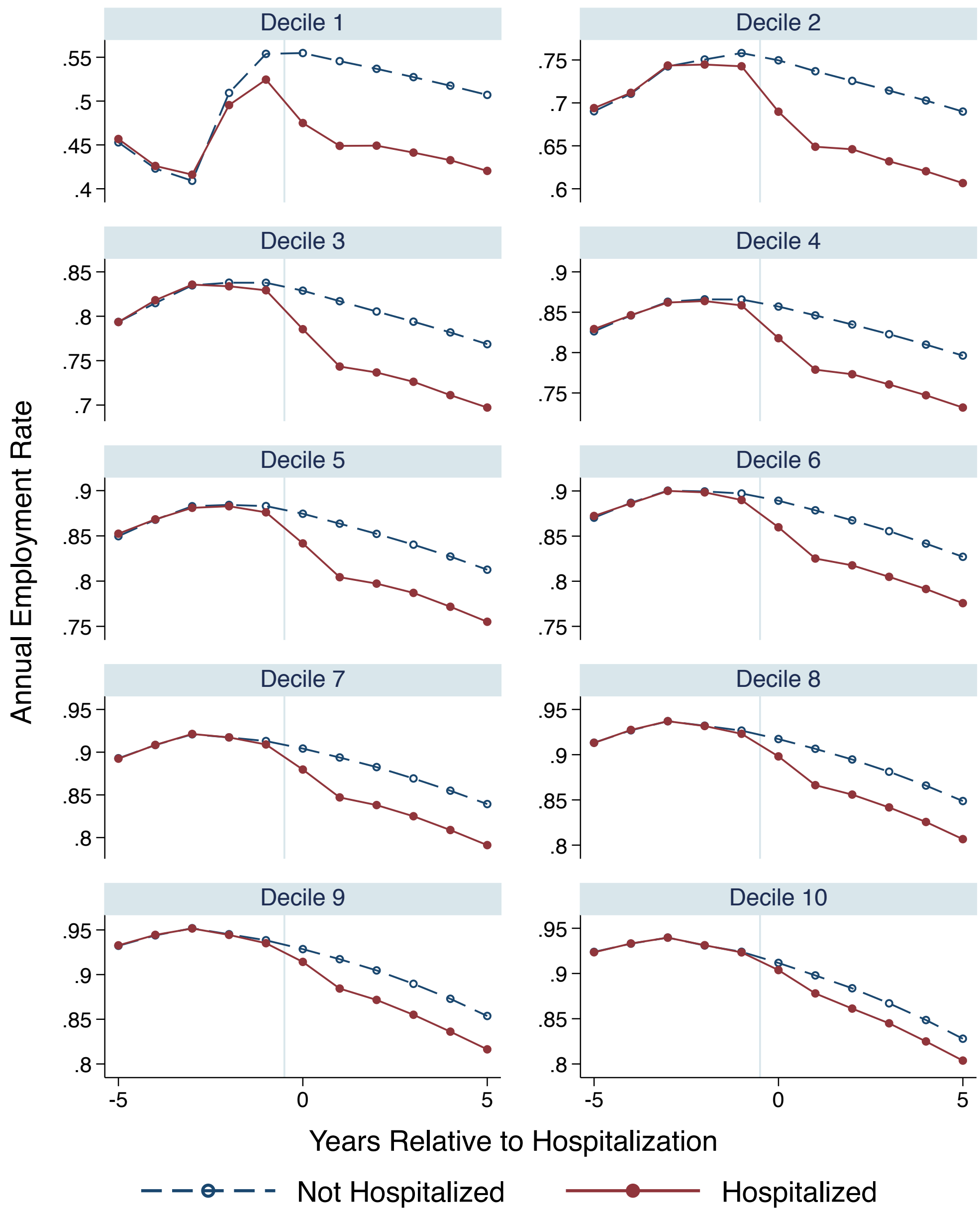
Notes: This figure plots the average change in annual earnings and annual employment rates during relative years 0 to 5 compared to the average outcome during those six years of people who were not hospitalized. These level changes are estimated using regression equation (1.1) and displayed separately by decile of household income three years pre-hospitalization (computed separately for each age, sex and year). Figure 1.4 is analogous to this figure, but shows the declines in percentages instead of levels. The corresponding event study plots are shown in Appendix Figures A.9 and A.10.

Appendix Figure A.9: Event Studies of Annual Earnings by Decile of Pre-Hospitalization Household Income



Notes: The panels of this figure replicate Figure 1.1B using the subsamples of individuals who were in each decile of the household income distribution three years prior to the hospitalization event. Household incomes were assigned to deciles separately by age, sex and year. Then regression equation (1.1) was estimated on the subsample of individuals in each decile. See the notes to Figure 1.1 for details on how the plots are constructed.

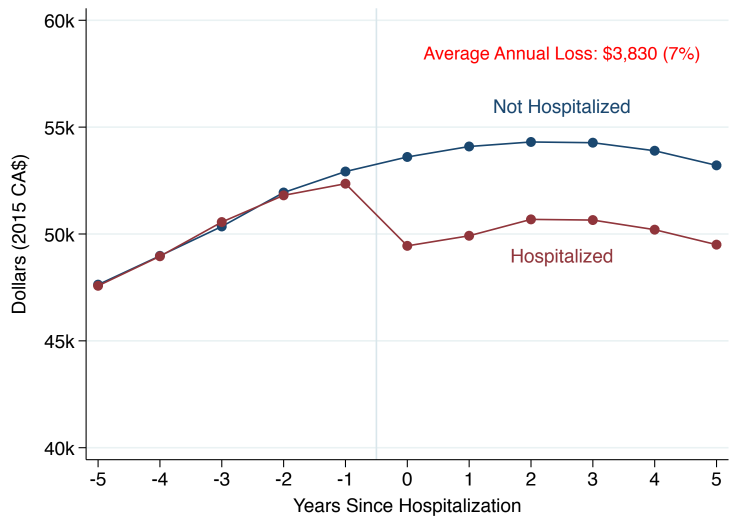
Appendix Figure A.10: Event Studies of Annual Employment by Decile of Pre-Hospitalization Household Income



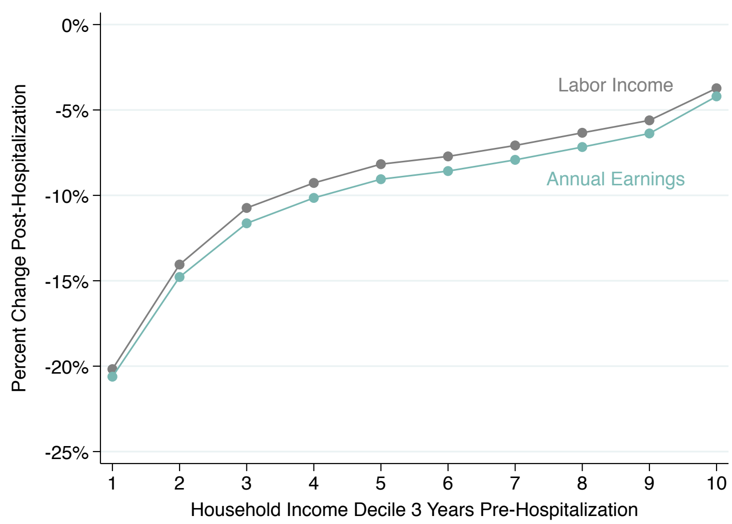
Notes: The panels of this figure replicate Figure 1.1C using the subsamples of individuals who were in each decile of the household income distribution three years prior to the hospitalization event. Household incomes were assigned to deciles separately by age, sex and year. Then regression equation (1.1) was estimated on the subsample of individuals in each decile. See the notes to Figure 1.1 for details on how the plots are constructed.

Appendix Figure A.11: Earnings and Labor Income Respond Similarly to Hospitalization Events

(A) Labor Income



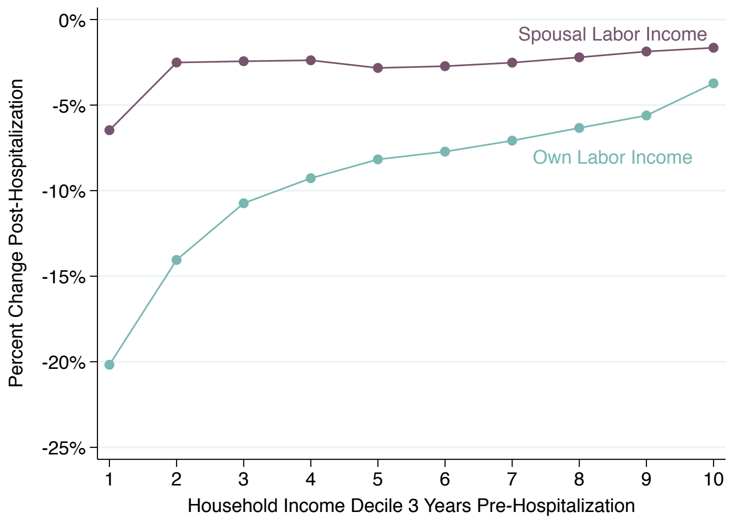
(B) Inequality in Earnings and Labor Income Losses by Pre-Hospitalization Income



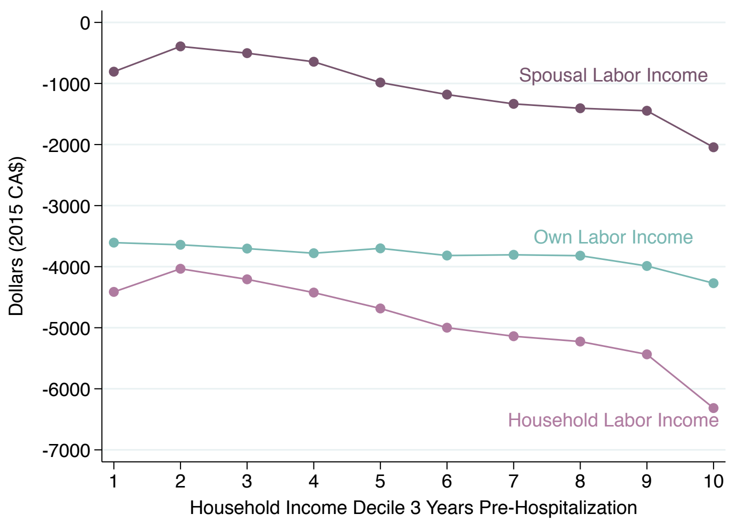
Notes: Panel A replicates Appendix Figure A.2A using labor income as an outcome instead of wage earnings. Labor income includes wage earnings, self-employment income and earnings not reported on employer-issued tax slips: see Appendix Table A.1 for details. Panel B replicates the annual earnings series from Appendix Figure A.2C alongside an equivalent series for the effects of hospitalization events on labor income.

Appendix Figure A.12: Heterogeneous Effects of Hospitalization Events on Own, Spousal and Household Labor Income by Household Income Decile

(A) Percent Effect



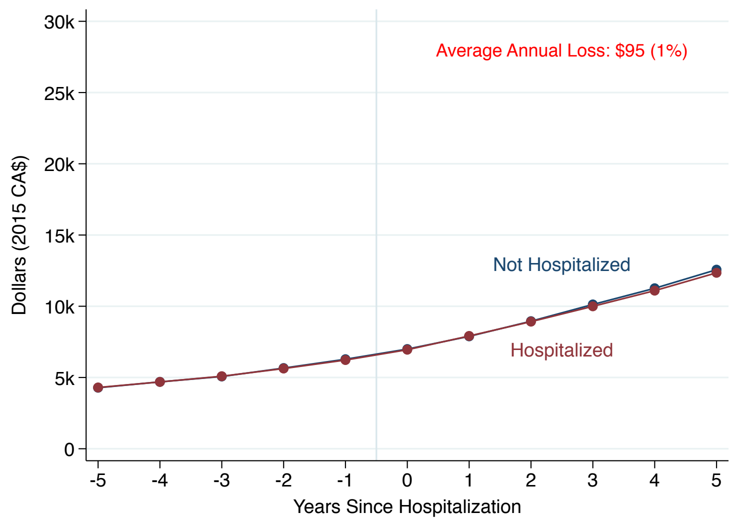
(B) Level Effect



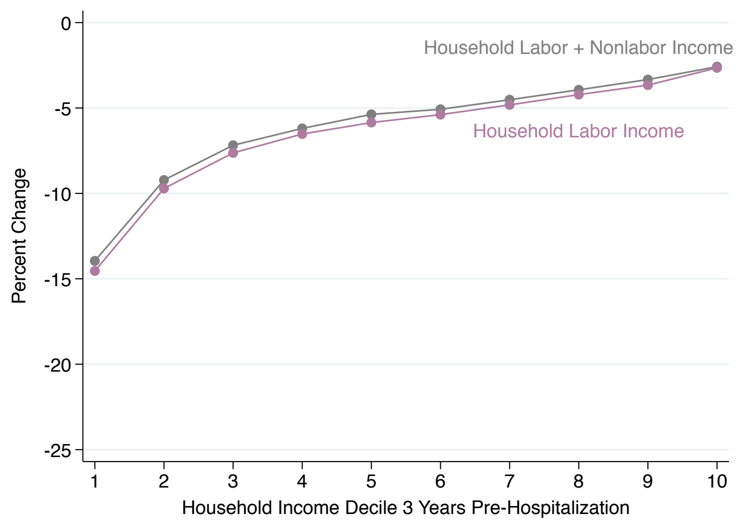
Notes: Panel A reproduces the series shown in Figure 1.5B for the six-year effect of hospitalization on the percent change in own labor income, alongside a new series plotting the percent change in spousal labor income. Panel B plots the six-year effect of hospitalization on own, spousal and household labor income in annual levels, instead of the percentage effects shown in Panel A and Figure 1.5B. These effects are estimated using the weighted sample of tax filers (including those without spouses), separately by decile of household income three years pre-hospitalization.

Appendix Figure A.13: Hospitalization Events Have Little Effect on Nonlabor Income

(A) Household Nonlabor Income



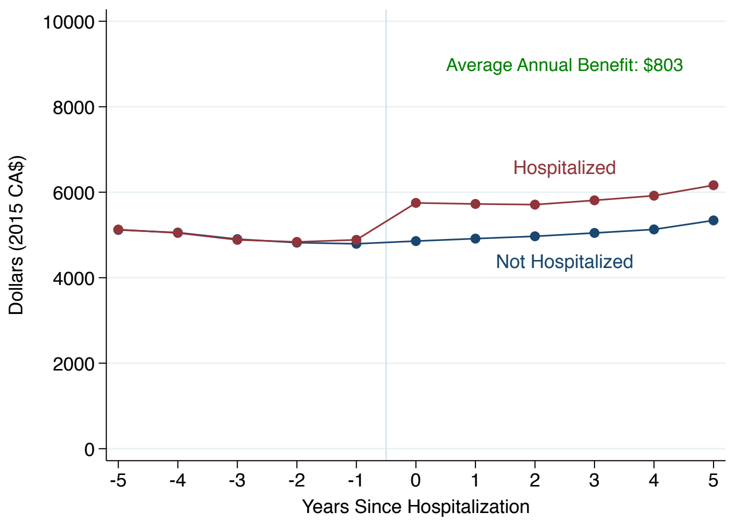
(B) Inequality in Labor + Nonlabor Income Losses
by Pre-Hospitalization Income



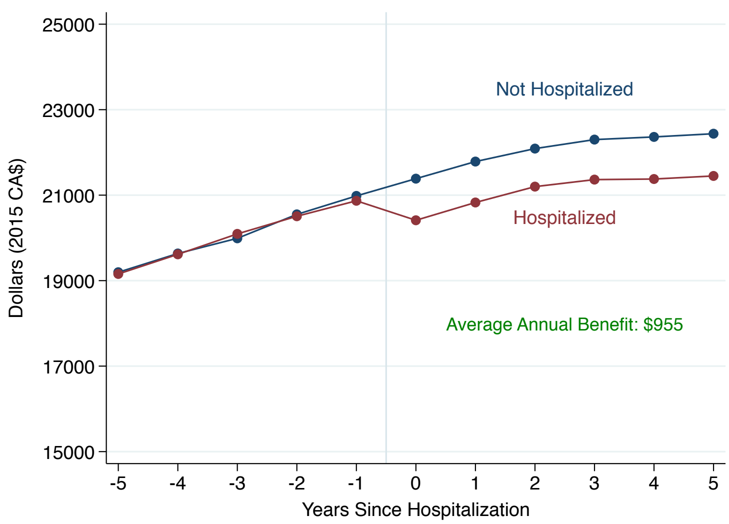
Notes: Panel A plots an event study of household nonlabor income, estimated using the weighted sample of tax filers. Nonlabor income includes dividends, interest, alimony, and other income components: see Appendix Table A.1 for details. See the notes to Figure 1.1 for details on how the plot is constructed. Panel B reproduces the series shown in Figure 1.5B for the six-year effect of hospitalization on the percent change in household labor income, alongside a new series plotting the percent change in houshold labor and nonlabor income.

Appendix Figure A.14: Transfers Increase and Taxes Decrease Post-Hospitalization

(A) Household Transfers



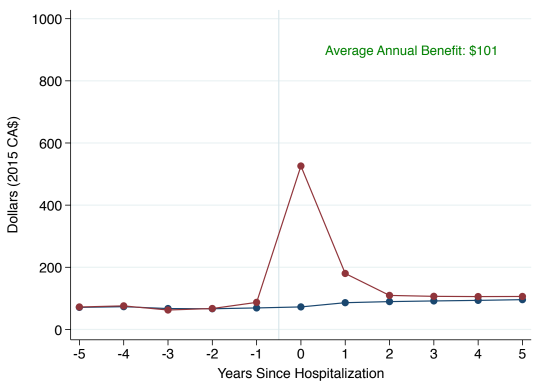
(B) Household Taxes



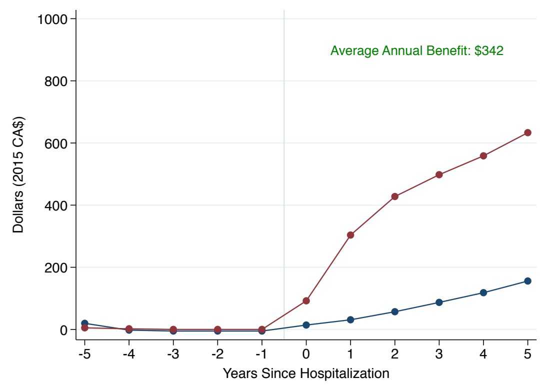
Notes: Panel A plots an event study of government transfers, estimated using the weighted sample of tax filers. Panel B plots the analogous event study for taxes owed. See the notes to Figure 1.1 for details on how each plot is constructed. The components of transfers and taxes are defined in Appendix Table A.1.

Appendix Figure A.15: Effect of Hospitalization Events on Transfer Program Benefits

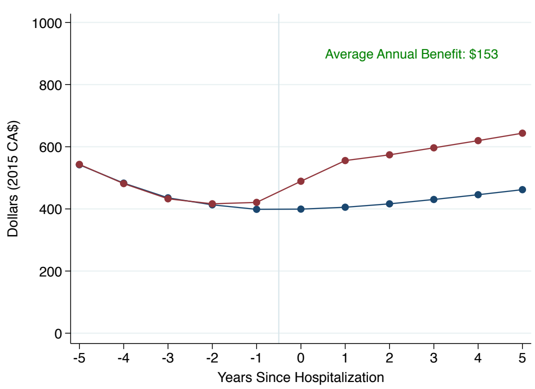
(A) Short-Term Disability Benefits



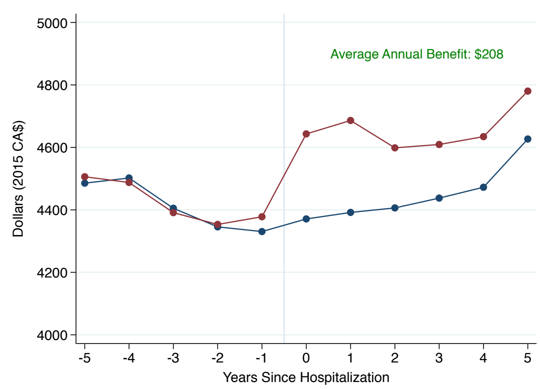
(B) Long-Term Disability Benefits



(C) Cash Welfare Benefits



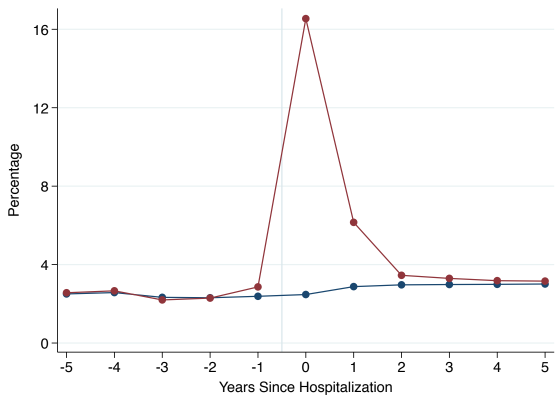
(D) Other Transfer Benefits



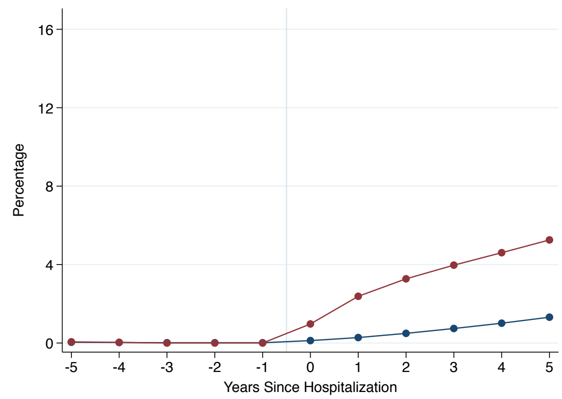
Notes: Each panel plots an event study of government transfers from a specific program, estimated using the weighted sample of tax filers. Other transfer benefits includes all transfers described in Appendix Table A.1 apart from those plotted in panels A to C: short-term disability (EI Sickness), long-term disability (CPP Disability) and cash welfare (social assistance) benefits. The sum of the benefits reported across the four panels is equal to total household transfer benefits, as plotted in Appendix Figure A.14A. See the notes to Figure 1.1 for details on how each plot is constructed.

Appendix Figure A.16: Effect of Hospitalization Events on Transfer Program Receipt

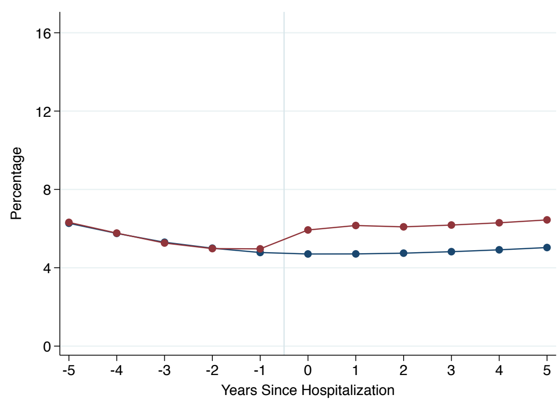
(A) Short-Term Disability Recipients



(B) Long-Term Disability Recipients

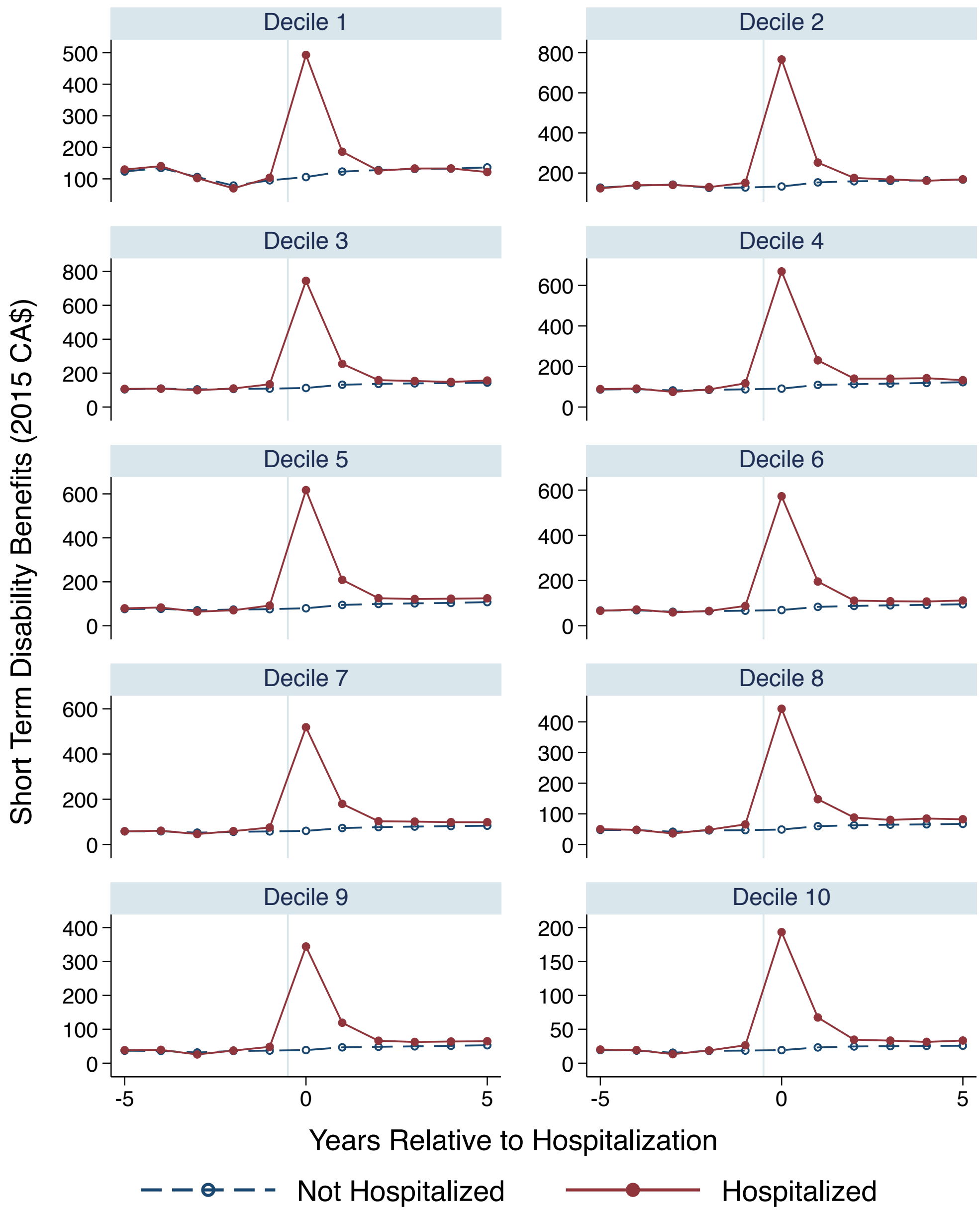


(C) Cash Welfare Recipients



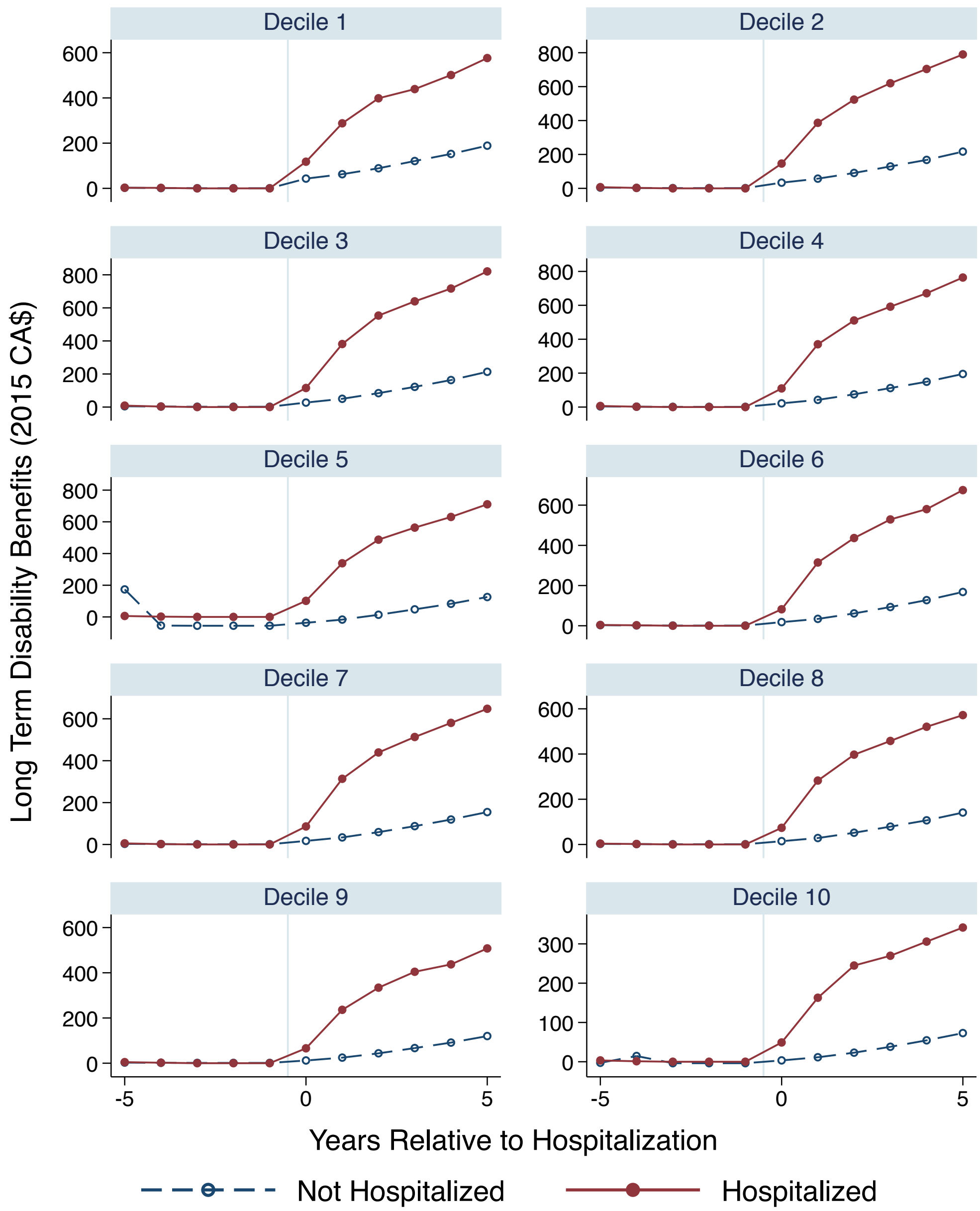
Notes: Each panel replicates the corresponding event study shown in Appendix Figure A.15ABC, but plots the percentage of recipients for each transfer program instead of the number of dollars of transfers received. The event studies are estimated using the weighted sample of tax filers. See the notes to Figure 1.1 for details on how each plot is constructed.

Appendix Figure A.17: Event Studies of Short Term Disability Benefits by Decile of Pre-Hospitalization Household Income



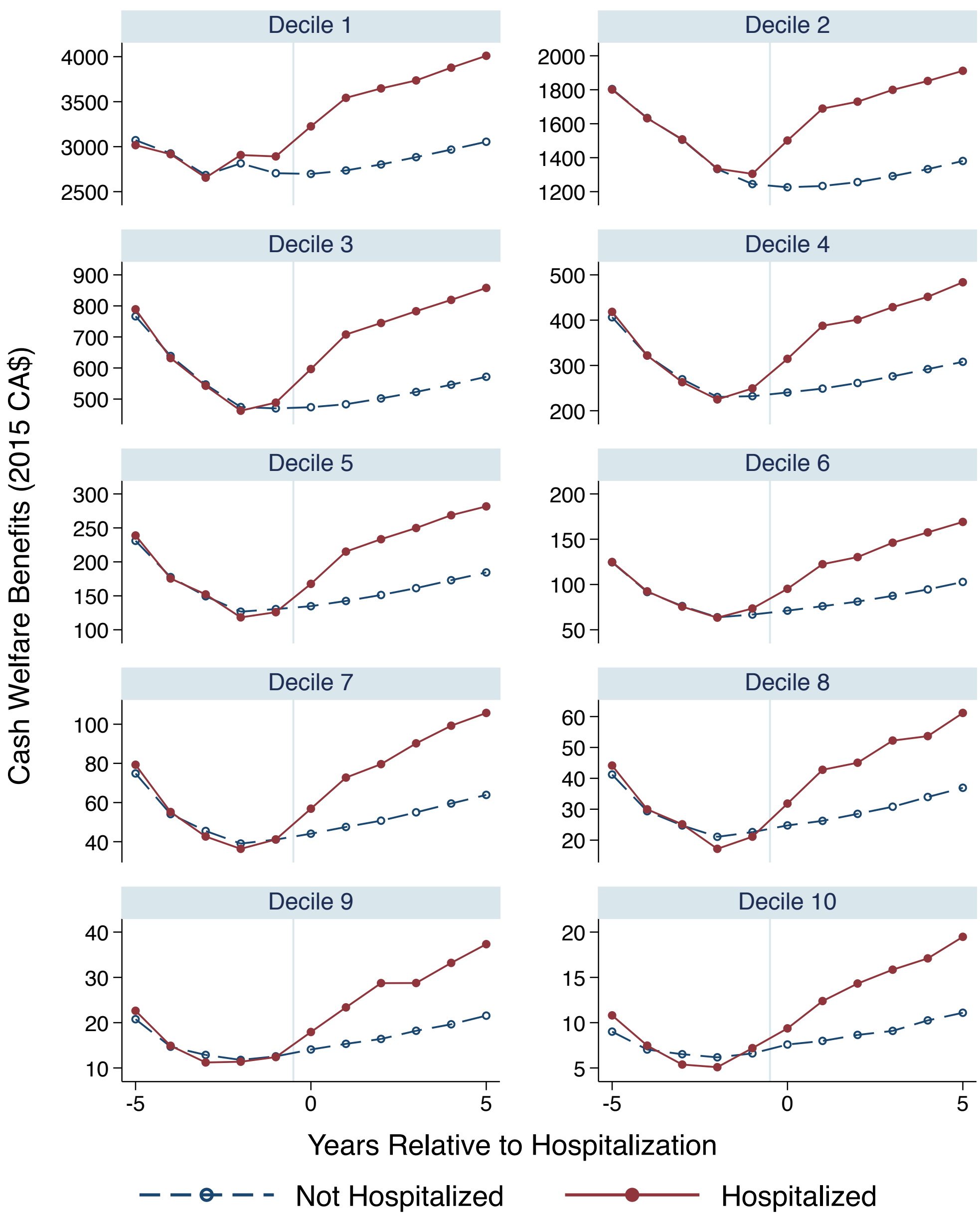
Notes: The panels of this figure replicate Appendix Figure A.15A using the subsamples of individuals who were in each decile of the household income distribution three years prior to the hospitalization event. Household incomes were assigned to deciles separately by age, sex and year. Then regression equation (1.1) was estimated on the subsample of individuals in each decile. See the notes to Figure 1.1 for details on how the plots are constructed.

Appendix Figure A.18: Event Studies of Long Term Disability Benefits by Decile of Pre-Hospitalization Household Income



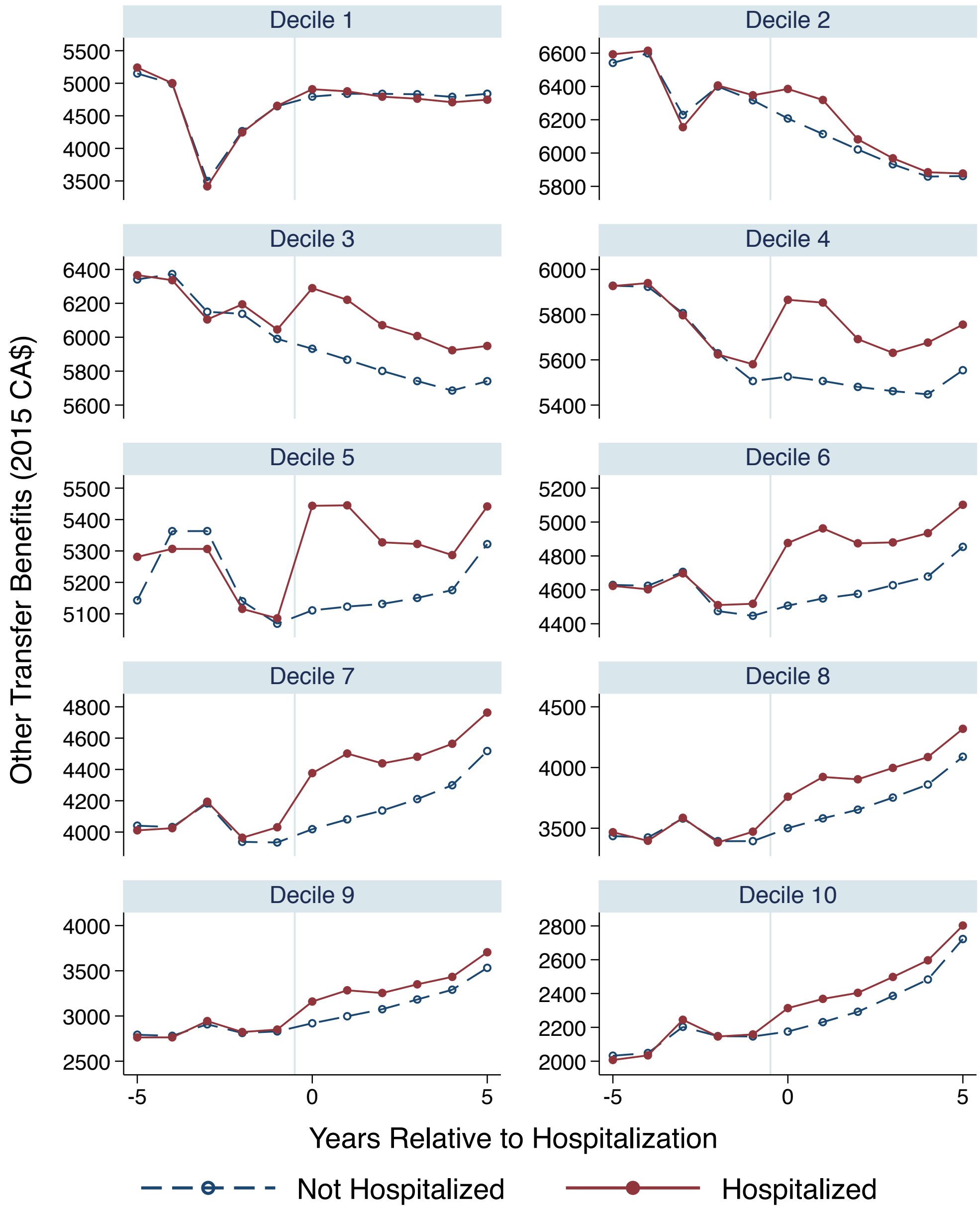
Notes: The panels of this figure replicate Appendix Figure A.15B using the subsamples of individuals who were in each decile of the household income distribution three years prior to the hospitalization event. Household incomes were assigned to deciles separately by age, sex and year. Then regression equation (1.1) was estimated on the subsample of individuals in each decile. See the notes to Figure 1.1 for details on how the plots are constructed.

Appendix Figure A.19: Event Studies of Cash Welfare Benefits by Decile of Pre-Hospitalization Household Income



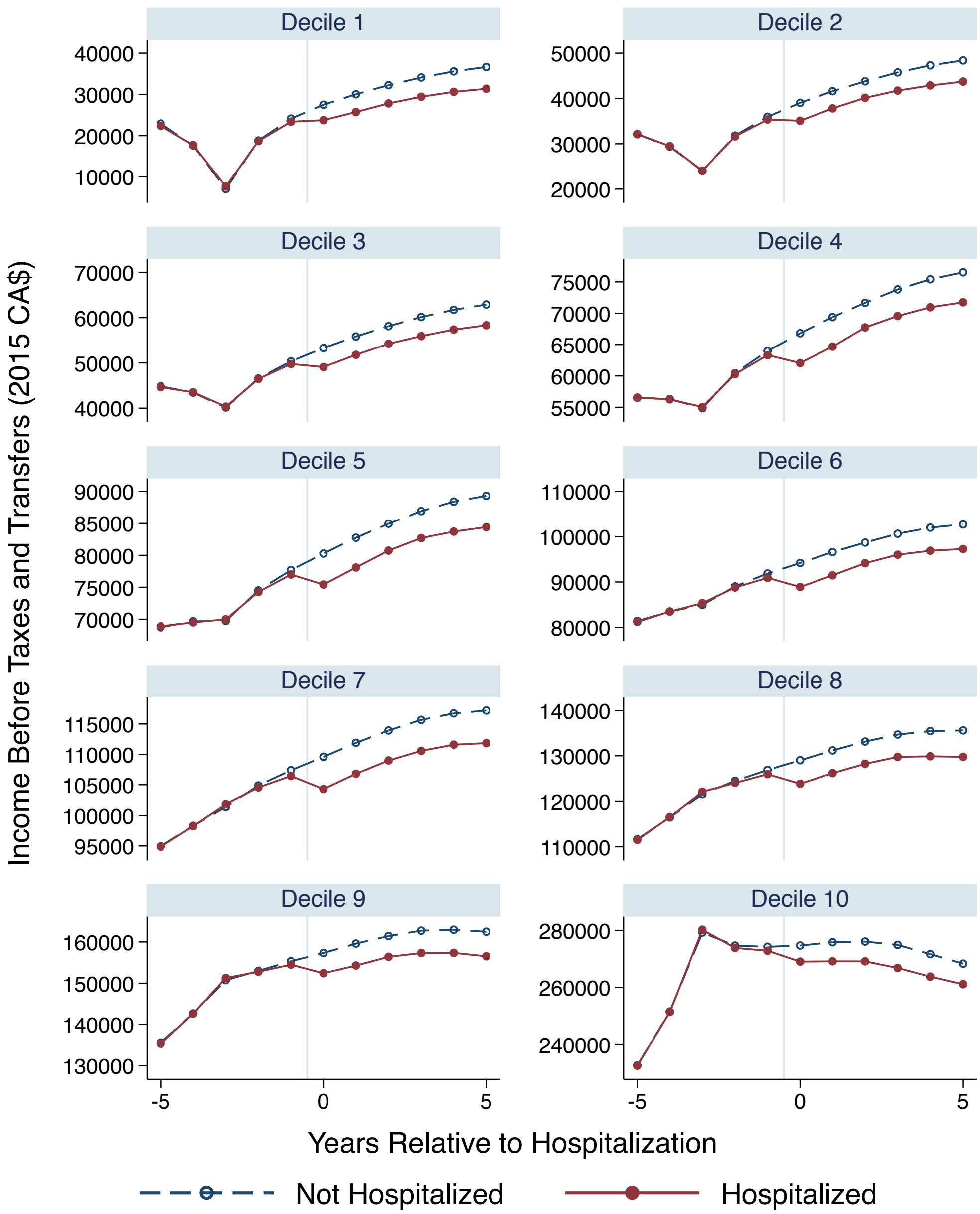
Notes: The panels of this figure replicate Appendix Figure A.15C using the subsamples of individuals who were in each decile of the household income distribution three years prior to the hospitalization event. Household incomes were assigned to deciles separately by age, sex and year. Then regression equation (1.1) was estimated on the subsample of individuals in each decile. See the notes to Figure 1.1 for details on how the plots are constructed.

Appendix Figure A.20: Event Studies of Other Transfer Benefits by Decile of Pre-Hospitalization Household Income



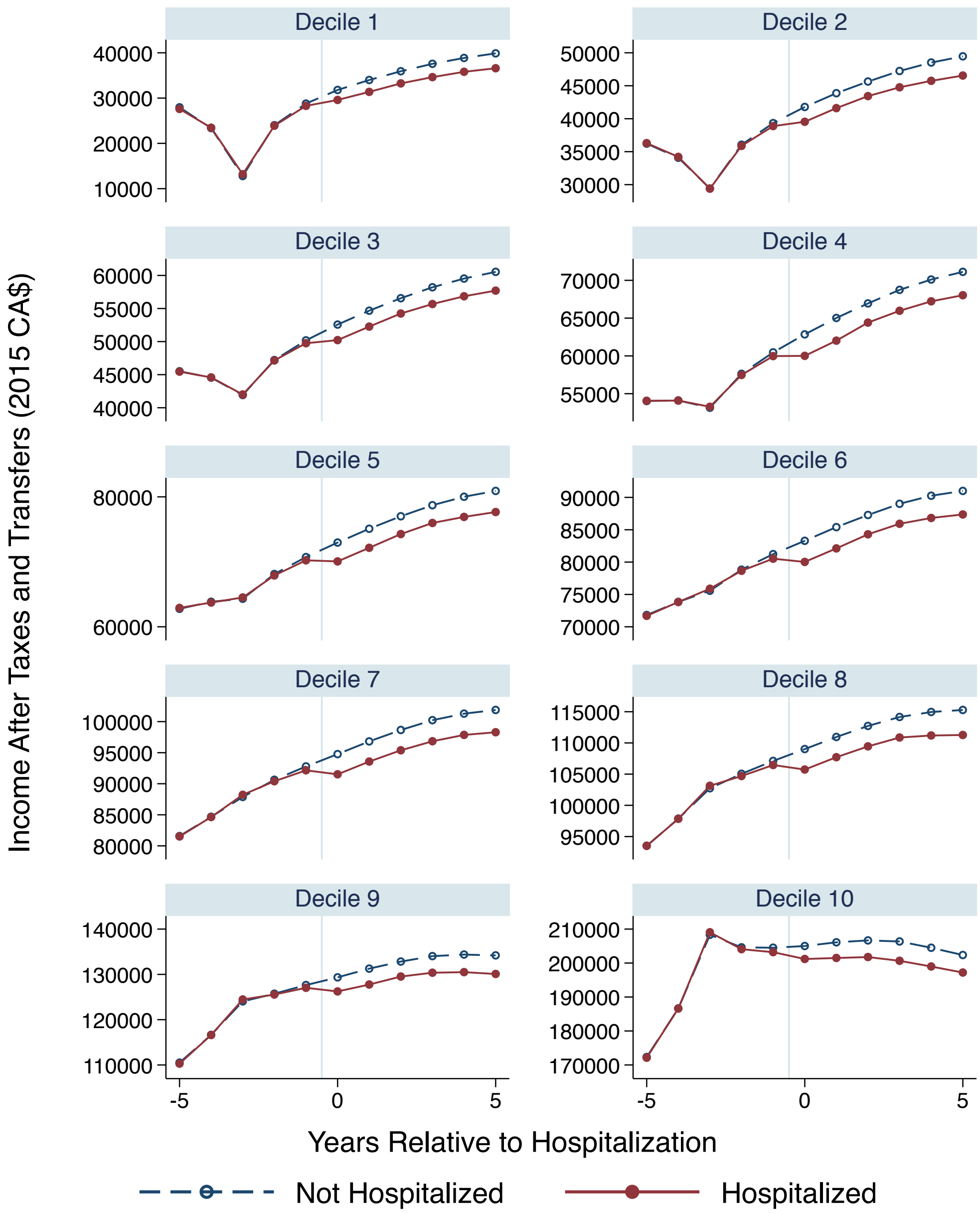
Notes: The panels of this figure replicate Appendix Figure A.15D using the subsamples of individuals who were in each decile of the household income distribution three years prior to the hospitalization event. Household incomes were assigned to deciles separately by age, sex and year. Then regression equation (1.1) was estimated on the subsample of individuals in each decile. See the notes to Figure 1.1 for details on how the plots are constructed.

Appendix Figure A.21: Event Studies of Income Before Taxes and Transfers by Decile of Pre-Hospitalization Household Income



Notes: The panels of this figure replicate Figure 1.6A using the subsamples of individuals who were in each decile of the household income distribution three years prior to the hospitalization event. Household incomes were assigned to deciles separately by age, sex and year. Then regression equation (1.1) was estimated on the subsample of individuals in each decile. See the notes to Figure 1.1 for details on how the plots are constructed.

Appendix Figure A.22: Event Studies of Income After Taxes and Transfers by Decile of Pre-Hospitalization Household Income



Notes: The panels of this figure replicate Figure 1.6B (without the shaded areas or dashed line) using the subsamples of individuals who were in each decile of the household income distribution three years prior to the hospitalization event. Household incomes were assigned to deciles separately by age, sex and year. Then regression equation (1.1) was estimated on the subsample of individuals in each decile. See the notes to Figure 1.1 for details on how the plots are constructed.

Chapter 2

The Long-Term Externalities of Short-Term Disability Insurance

2.1 Introduction

Multiple insurance programs insuring the same risk impose externalities on each other when they generate moral hazard (Pauly 1974). In general, private insurance plans that supplement public insurance generate negative fiscal externalities because supplemental benefits increase moral hazard and therefore increase public insurance costs (Cabral and Mahoney 2018). The externality imposed by employer-provided short-term disability insurance (STDI) on government-provided long-term disability insurance (LTDI) could be especially large, since more than a third of American and Canadian workers have private STDI coverage and public LTDI is one of the largest government transfer programs, with 2016 expenditures exceeding $145 billion in the U.S. and CA$4 billion in Canada.[1] But economic theory is ambiguous as to whether private short-term disability insurance generates a positive or a

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[1] Private STDI coverage and public LTDI expenditures for the U.S. and Canada are reported by Monaco (2015), ESDC Evaluation Directorate (2016), Social Security Administration (2018), and ESDC (2015).

negative fiscal externality, and there is little empirical evidence on the magnitude or even the direction of this externality.

On the one hand, long-term disability insurance uses a waiting period as a form of cost-sharing and short-term disability insurance pays benefits during this waiting period, which should increase moral hazard among disabled workers and increase flows into long-term disability. On the other hand, employers paying for private short-term disability benefits internalize some of the cost of their employees' disabilities and have an added financial incentive to offer workplace accommodations, which should decrease flows into long-term disability. Both academics and insurers have speculated that the effect of increased accommodation will dominate increased moral hazard and reduce long-term disability rates (Autor and Duggan 2010; Great-West Life 2018).[2] But the net effect remains theoretically ambiguous, and must be measured empirically.

This paper provides quasi-experimental evidence quantifying the externality from employer-provided STDI onto public LTDI. Measuring this externality requires two ingredients: data linking private STDI coverage with public LTDI take-up, and a research design generating variation in STDI coverage that is exogenous from LTDI risk. Prior work using US survey data has been limited along both dimensions and yielded inconclusive results, leading its authors to conclude that new data is required (Autor et al. 2013). I construct a new dataset of linked Canadian administrative tax and benefits records to study quasi-experimental variation in STDI coverage. These administrative records allow me to observe STDI coverage linked to LTDI take-up for the full population because Canada offers a payroll tax rebate for employer-provided STDI that is unique within the OECD (HRSDC 2009). I identify the causal effect of private STDI coverage by comparing public LTDI receipt among employees whose firms have just ended their STDI plans to employees of firms that are about to end their STDI plans. I show that these two groups of employees are nearly identical on observables and that the timing of firms ending their STDI plans is uncorrelated with the observable health and socioeconomic characteristics of their employees.

[2]Autor and Duggan (2010) propose that a system of universal private STDI for the United States "should ultimately reduce total employee and employer disability insurance costs by assisting some workers with work-limiting disabilities to remain in the labor force rather than becoming long-term beneficiaries of the [LTDI] system". One of the three largest private insurance companies in Canada claims that "products like [. . .] Short Term Disability allow us to intervene early, help support shorter claim durations and prevent long-term disability claims" (Great-West Life 2018).

I find that employer-provided STDI raises two-year flows onto LTDI by 0.07 percentage points, an increase of 33%. This result implies that the moral hazard response to increased benefits during the waiting period dominates the effect of any increased workplace accommodations associated with STDI. Employer-provision of STDI therefore imposes a negative fiscal externality on the government budget for LTDI. Extrapolating from my quasi-experimental sample to the full Canadian population, I estimate that if Canadian employers had not provided STDI during the 15 years between 2000 and 2014, there would be 18,300 fewer LTDI recipients in 2015 and government expenditures on LTDI would be CA$230 million dollars (5%) lower.

I estimate that the efficient Pigouvian tax on private STDI, which would make private insurers internalize the externality they impose on the public LTDI budget, is approximately $35 per insured worker per year. The Canadian government already operates a Pigouvian subsidy for private STDI as part of a system of universal public STDI with optional private provision. Canadian employers who provide private STDI become the first payer of STDI benefits and can receive a payroll tax rebate equal to the expected savings they generate for the public STDI budget. These Pigouvian subsidies average $150 per insured worker, and would be reduced by 23% if they incorporated the negative fiscal externality on the public LTDI budget. The implied tax rate on private STDI premiums would be less than 23% because private STDI plans are more generous than public STDI, so the cost of private STDI exceeds the reduction it generates in public STDI spending.

The evidence in this paper can inform active policy discussions. In response to rapid growth in public LTDI expenditures in the United States, Autor and Duggan (2010) proposed that universal private STDI would result in more workers with work limitations receiving assistance and returning to work and thereby reduce long-term disability rates and expenditures. This paper shows that the opposite is true, and that expanding private STDI would increase public LTDI spending. This negative fiscal externality is not only relevant to private provision of STDI. Only five states in the U.S. currently provide universal STDI and only 25% of workers outside those states have STDI coverage.[3] Additional state governments mandating universal STDI would impose a negative fiscal externality on the Social Security Administration trust fund and the federal budget. Within Canada, the federal government

[3]The five U.S. states with universal STDI are California, Hawaii, New Jersey, New York and Rhode Island. Puerto Rico also provides universal STDI.

spends roughly CA$2.4 billion per year on universal public STDI and payroll tax rebates to employers offering private STDI plans (CEIC 2018). Policy changes to the benefit levels or coverage of public or private STDI in Canada would not only impact their own program cost, but affect the much larger CA$4 billion spent per year on public LTDI (ESDC 2015).

This paper contributes to a small but growing literature studying how disability insurance design affects disability rates. The most directly related research is a 2013 working paper by Autor et al. measuring the effect of employer-provided STDI coverage on LTDI take-up using state-by-sector variation induced by five U.S. states with universal STDI. Their initial findings show that private STDI lowers LTDI receipt, opposite to the findings of this paper, but Autor et al. show that their identification assumptions do not hold and "caution against viewing the current results as reliable". More broadly, my results are consistent with the finding that longer waiting periods for LTDI reduce the number of LTDI claims (Autor, Duggan, and Gruber 2014), as private STDI generates variation in benefit levels during the waiting period rather than the length of the wait. The STDI policies studied here create financial disincentives to work during the waiting period for LTDI and raise LTDI receipt, which is consistent with research showing that reducing the financial disincentive to work while receiving LTDI induces recipients to return to work (Kostol and Mogstad 2014). There is also evidence that when employers face experience-rated LTDI premiums, LTDI take-up falls among their employees (de Groot and Koning 2016). My results imply that the positive incentive effects for employers of experience-rated private STDI are dominated by the negative incentive effects for employees of a more generous STDI benefit.

This paper also adds to a broader literature on externalities in overlapping insurance markets. Public insurance programs crowd out private coverage in many insurance markets, such as health insurance (Cutler and Gruber 1996) and long-term care insurance (Brown and Finkelstein 2008). When insurance programs insure a related risk but are mutually exclusive (such as unemployment insurance, disability insurance, workers compensation and cash welfare), there is extensive evidence that changes in the generosity of one program affect take-up and expenditures in the others (e.g. Campolieti and Krashinsky 2003; Koning and Vuuren 2010; Staubli 2011; Borghans, Gielen, and Luttmer 2014). The spillovers between private STDI and public LTDI studied here are a case of insurance programs that overlap but are supplementary rather than exclusive. Theoretical work by Pauly (1974), Golosov and Tsyvinski (2007) and Chetty and Saez (2010) explores the "multiple dealing" externality in this setting, when multiple insurers simultaneously insure the same risk but do not internalize the cost of the moral hazard that their insurance imposes on other insurers. Cabral and Mahoney (2018) quantify this externality in the market for elderly health insurance in the US, showing that private Medigap plans increase public Medicare expenditures 22% by insuring the out-of-pocket costs generated by Medicare's cost sharing. Analogous to Medigap, private STDI insures the cost-sharing provisions of public LTDI. But private STDI is unique because it interacts with a two-sided labor market, reducing the financial incentive for disabled employees to work while increasing the financial incentive for employers to accommodate disabled employees. This paper shows that the net externality of private STDI is negative, increasing the number of LTDI recipients and LTDI costs.

The paper proceeds as follows. Section 2.2 describes the institutions of Canadian STDI and LTDI and the data that I use to study them. Section 2.3 explains how I implement the quasi-experimental research design and the assumptions required for it to identify a causal effect. Section 2.4 presents the results on how private STDI affects take up of LTDI, then uses the results to calculate the efficient Pigouvian tax on private STDI. Section 2.5 concludes.

2.2 Institutions and Data

2.2.1 Short-Term Disability Insurance in Canada

This paper measures the effects of employer-provided private STDI relative to the less generous universal public STDI coverage provided by Canada's EI Sickness program. All Canadian workers have short-term disability insurance, with a mix of public and private provision.[4] Employers select which groups of employees to enroll in private STDI and participation in a private STDI plan is mandatory when offered: individual workers may not opt out. Workers with employer-provided STDI remain eligible for the public EI Sickness program but their private insurance is the first payer of benefits: any private benefits re-

[4]The public EI Sickness program provides STDI benefits to Canadians who have worked at least 600 hours in the previous 52 weeks and are "unable to work because of sicknss, injury or quarantine". After obtaining a medical certificate signed by a doctor and waiting two weeks (one week as of 2017), they are entitled to up to 15 weeks of benefits, with a 55% replacement rate up to a monthly maximum benefit ($2,360 per month in 2017). EI Sickness benefits are financed by a payroll tax up to the maximum insurable earnings ($51,500 in 2017). The EI payroll tax rate is actuarially adjusted each year to break even: the rate is set so that projected revenues cover projected expenditures and eliminate the surplus or deficit accumulated due to past deviations from the projections (Office of the Chief Actuary 2014). All employers and employees pay the same rate, with no experience rating or risk adjustment.

ceived are deducted one-for-one from EI sickness payments. Private STDI therefore provides benefits at least as generous as EI Sickness, but typical private insurance benefits are much more generous. According to a 2007 survey by the Canadian federal government, the average employer-provided plan had a replacement rate of 70% and the average duration of a private STDI spell is 20 weeks, compared to a replacement rate of 55% and an average duration of 9.5 weeks on EI Sickness (HRSDC 2009). Private STDI plans also offer additional ancillary services to assist employees in returning to work, unlike the public EI Sickness program which only provides monetary benefits (Meredith and Chia 2015).

I measure private STDI coverage using administrative tax data by observing participation in Canada's Premium Reduction Program (PRP), which offers payroll tax rebates to employers providing private STDI benefits that meet government-set adequacy criteria. 36% of Canadian workers receive private STDI coverage with a PRP rebate (Appendix Table B.1). The PRP is a Pigouvian subsidy, allowing employers to internalize the positive fiscal externality of their private STDI plans on the government's public STDI budget. Private STDI plans reduce the costs of public EI Sickness benefits because EI Sickness is the second payer with respect to private benefits. The payroll tax rebate for the PRP is calculated each year as the actuarially fair rebate given the anticipated cost reductions for the government in four different categories of private STDI generosity, with no experience rating of firms (Office of the Chief Actuary 2014). In 2014 the Canadian government paid $854 million in PRP payroll tax rebates for 5.7 million workers with private STDI, an average of $150 per worker (Appendix Table B.3 and CEIC 2018).

The Canadian institutional setting will generate a more positive fiscal externality from private STDI to public LTDI than a comparable setting without universal public STDI. Canadian workers with private STDI receive some additional insurance, because private STDI plans are more generous than the public STDI program. The negative component of the fiscal externality reflects the moral hazard generated by the difference between public and private insurance. But private employers and private insurers collectively bear the full cost of the private STDI benefits they provide and the full rewards of reducing their STDI expenditures by assisting employees in returning to work. Therefore the positive component of the fiscal externality reflects the behavioral response of firms to the full private STDI insurance benefit.

2.2.2 Long-Term Disability Insurance in Canada

The effect of private STDI on receipt of public LTDI benefits will include Canadians with private LTDI coverage since Canada's public LTDI program is the first payer of benefits: receipt of private LTDI benefits does not affect eligibility or benefit amounts for public LTDI. All Canadian workers with a sufficient work history are eligible for Canada (or Quebec) Pension Plan Disability (CPP-D) benefits if they develop a "a severe and prolonged disability that renders [them] incapable of regularly pursuing any substantially gainful occupation" (HRSDC 2011). 56% of Canadian workers have additional private LTDI coverage, typically provided through their employer (Canadian Life and Health Insurance Association 2017). All private LTDI plans require their beneficiaries to apply for CPP-D if they are eligible, then deduct CPP-D benefit payments from private LTDI benefits for successful applicants. Private insurers are able to share information with the government: if a private LTDI beneficiary is eligible for CPP-D and refuses to apply their insurer will deduct the amount of CPP-D they would receive if successful from their private benefits.

Private STDI benefits generally transition seamlessly to LTDI benefits, while workers without private STDI coverage often face a gap in benefits between the expiration of STDI benefits and the beginning of LTDI benefits. Nearly all Canadian workers with private STDI also have private LTDI coverage, and the private plans are typically aligned so that there is no gap in benefits when a worker is eligible for LTDI (HRSDC 2009). By contrast, EI Sickness benefits expire 17 weeks after the onset of the disability and CPP-D benefits start no earlier than 4 months after the onset of the disability, but often much later due to processing times and appeals (Meredith and Chia 2015). Even if they have private LTDI coverage, workers on EI Sickness may face a 9 week gap since private LTDI plans typically start paying benefits either 17 or 26 weeks after the onset of a disability. Eliminating the "medium-term" gap in disability benefits could be an important mechanism for why private STDI raises LTDI take-up, although I am unable to isolate the effect of this specific mechanism.

2.2.3 Data Sources

I measure private STDI coverage and public LTDI receipt for all Canadians from 2000 to 2015 using linked administrative tax and benefits records. I link employees to their employers and measure employment rates and employment earnings using T4 tax slips, which are similar to

W-2 tax slips in the U.S. and observed for both tax filers and non-filers. I define employers using their 9-digit Business Number, which is assigned by the Canada Revenue Agency and used in all tax filings.

I define a worker as having private STDI coverage in a given year if they received a T4 tax slip with a PRP payroll tax rebate for private STDI from any employer. In practice, some workers have private STDI coverage from employers that have not registered for the PRP tax rebate. This measurement error is minimal in my quasi-experimental sample, since I study employees of firms that were aware of and participating in the PRP tax rebate then chose to end their participation. To the extent that some workers continue to receive unobserved private STDI from other employers, this would attenuate my results. However this measurement error severely attenuates the results from conducting the reverse quasi-experiment, because I cannot reliably observe the timing of firms *starting* their private STDI plans as I do for firms *ending* their private STDI plans. Many firms likely start offering private STDI plans in the years before they take up the PRP tax rebate. I discuss this issue in detail in Appendix 2.A.

I observe public LTDI receipt using T4A(P) tax slips issued by the government to all recipients of CPP-D and QPP-D public LTDI benefits. I observe age and sex using T1 tax returns for workers who filed their taxes in any year, which includes more than 99.9% of my sample (Appendix Table B.1). I observe industry of employment as a 2-digit NAICS code for each employer and define it for each worker as the industry from which they derived the most earnings in a given year.

In some robustness checks I use inpatient hospitalization data, which I observe for all acute care hospitals outside Quebec and Manitoba. These hospital records are drawn from the Discharge Abstract Database and have been reliably linked to the tax records, as described in Sanmartin et al. (2018). Outpatient visits to the hospital and emergency room visits that did not result in an admission are not included in the database. I exclude admissions for childbirth and exclude all residents of Quebec, Manitoba and the northern territories (who sometimes travel to Quebec and Manitoba for care) when studying hospitalization.

Statistics Canada protects individuals' privacy during the linkage process and subsequent use of linked files. The data linkage was approved by Statistics Canada's Executive Management Board, and its use is governed by Statistics Canada's Directive on Record Linkage (2017). The files used in this analysis had no personal identifiers, and all data processing was performed on a secure server onsite at Statistics Canada in Ottawa, Ontario.

2.3 Empirical Methods

When firms choose to start or stop offering STDI plans they cause a sudden change in private STDI coverage for their incumbent workers. This section describes a quasi-experimental research design that uses the timing of firms ending their STDI plans in order to estimate the causal effect of employer-provided STDI coverage on LTDI take-up rates. Firms' decisions to offer STDI may be endogenous to LTDI rates if firms are responding to anticipated changes in their employees' health or changes in labor market conditions, and I address these concerns below.

2.3.1 Ideal Experiment

This paper seeks to identify the causal effect of employer-provided STDI coverage on subsequent LTDI take-up:

$$\mathbf{1}(\text{LTDI Receipt})_{i,t+1} = \beta \cdot \mathbf{1}(\text{Employer STDI Coverage})_{it} + \theta \mathbf{X}_i + \varepsilon_i \tag{2.1}$$

where $\mathbf{X}_i$ is a vector of individual controls. The ideal experiment to identify the causal effect β would involve randomly assigning which employers are allowed (or prohibited) to provide their employees with private STDI. Employers randomly allowed to provide private STDI would then endogenously select the set of their employees to receive private STDI coverage, as all Canadian employers do presently. The causal effect of private STDI coverage would be identified by observing subsequent LTDI take-up while using the random assignment as an instrument for private STDI coverage.[5]

Estimating equation (2.1) using cross-sectional variation in STDI coverage while controlling for observable differences in worker and firm characteristics is unlikely to identify a causal effect. Workers with private STDI coverage from their employers are very different from those without private STDI: they are older, have higher incomes, and are concentrated in large firms and specific industries (Table 2.1). Workers with private STDI coverage likely

[5]Randomly selecting which employers are allowed to provide private STDI will identify the local average treatment effect of private STDI among employees who are endogenously chosen by their employers to receive private STDI. This is equal to the average treatment effect of private STDI under current Canadian policy. If private STDI has heterogeneous treatment effects, this may not be equal to the average treatment effect of private STDI in a counterfactual setting with employers mandated to provide private STDI to all employees.

differ in unobservable ways as well. For example, employers may be more likely to offer STDI when their employees are healthier than average and when tight labor markets prompt investments in attracting and retaining employees. Both good health and tight labor markets are associated with lower LTDI take-up (Black, Daniel, and Sanders 2002). In practice, the estimated effect β in a linear probability regression of equation (2.1) is highly sensitive to the set of controls included (Appendix Table B.2).

2.3.2 Quasi-Experimental Design

I approximate the ideal randomized experiment using a quasi-experimental research design that isolates variation in STDI coverage generated by the timing of employers ending their STDI plans. The causal effect of STDI coverage on LTDI take-up rates is identified by comparing the difference in LTDI receipt between workers whose employers just ended their STDI plans and workers whose employers still provide STDI plans but are about to end them. The intuition for this quasi-experiment is that these two groups of workers are similar to each other except for the precise timing of their firm choosing to end its STDI plan, and that the timing of firms ending their STDI plans is as good as random within a narrow time interval. The remainder of this section explains how this quasi-experiment is implemented and the identification assumptions required to estimate a causal effect.

I define a firm as ending its STDI plan if the firm provided STDI coverage to some or all of its employees during three consecutive years, then continued to operate but covered none of its employees with STDI in the subsequent three years. By this definition, 5801 Canadian firms ended their STDI plans between 2003 and 2014, ranging from 300 to 670 in a single year.

I consider an employee to be *treated* if they were employed by a firm during the last year it offered STDI coverage and the first year it offered no STDI coverage, which are defined as relative years 0 and 1. An employee is considered treated regardless of whether they were among the employees who actually received STDI coverage from their employer in relative year 0, since the subset of employees assigned to STDI coverage by the firm is potentially endogenous.[6] I construct a group of *control* employees who were employed by a different

[6]It would be troubling if the estimated treatment effect of private STDI were driven by the employees who did not have private STDI in relative year 0. I therefore replicate the baseline estimates (shown in Table 2.2) for the subsample of employees who endogenously had private STDI in relative year 0 (shown in Appendix Table B.4), finding nearly identical results.

firm during relative years 0 and 1, and whose firm ends its STDI coverage in relative year 3, two years after the treated employees' STDI coverage was ended. Workers in the control group are included in the treatment group two cohorts later if they remain employed at the same firm. I restrict the sample to workers ages 25 to 59 who have sufficient earnings in the previous six years to qualify for public LTDI (as shown in Appendix Table B.1).

Because of the precise year in which they ended their STDI plans, the employers of the treatment group did not have private STDI plans during relative years 1 and 2 while the employers of the control group continued to have private STDI plans until relative year 3. Yet the treated and control employees are very similar in other ways. By design, both the treated employees and the control employees were employed during two consecutive years by a firm that chose to provide STDI to some of its employes for at least three years, then chose to stop providing STDI. Table 2.1 shows that this design generates a treatment group and a control group that are nearly identical in observable characteristics, despite the fact that the characteristics of the full set of employees with and without private STDI differ substantially in the cross-section.

This quasi-experiment identifies the causal effect of STDI coverage on LTDI take-up if the timing of firms ending their private STDI is uncorrelated with their employees' risk of long-term disability. This identification assumption is required to ensure that the control group forms a valid counterfactual for the treatment group: i.e. if the firms in the treatment group had not ended their private STDI, then the treated employees would have had the same LTDI rates as the control employees. The assumption that the timing of an event is as good as random within a short time horizon is used in many empirical research designs (e.g. Fadlon and Nielsen 2017; Deshpande and Li 2017). But this assumption would certainly be violated if firms end their STDI coverage because their employees' health is deteriorating, thereby raising the costs of providing insurance. It might also be violated if firms end their STDI coverage during poor economic conditions, as a cost-cutting measure or because they don't need to offer benefits to attract and retain employees. Since poor health and poor economic conditions are both associated with increased LTDI take-up, both of these channels would bias the results toward finding that LTDI rates are higher when firms end their STDI plans (Charles, Li, and Stephens 2017). But in the results below I find the opposite: LTDI rates decrease when firms end their STDI plans. I also show in Section 2.4.2 that hospitalization rates and employment rates are similar in the treatment and control groups throughout the sample period and cannot explain the divergence in LTDI rates.

The key limitation of this research design is that, because the control group becomes treated two years later, I can only identify the effect of employer-provided STDI on LTDI receipt during the subsequent two years. This limitation reflects a trade-off between the similarity of the treatment and control groups and the length over which outcomes can be measured, and it is a limitation common to all research designs that use future treatment groups to form a counterfactual (Fadlon and Nielsen 2017). In order to observe the effect of employer-provided STDI coverage on LTDI receipt over T years, I would need to construct a control group of employees during relative years 0 and 1 whose firm ends its STDI plan in relative year T.[7] But firms grow and change their activities over time, and many employees leave over time. As the choice of time horizon T increases, the employees in the treatment and control groups become less comparable, the first stage effect becomes weaker, and the sample size shrinks. I therefore focus on estimating the causal effect of employer-provided STDI on LTDI receipt in the subsequent year using the shortest time horizon ($T = 2$) with the most credible control group. In Section 2.4.3 I extrapolate from my estimates of the impact of STDI on the short-run flow onto LTDI to its impact on the long-run stock of LTDI recipients, in order to discuss the implications of STDI for the government budget.

2.3.3 Instrumental Variables Implementation

An IV regression using the quasi-experimental sample recovers the effect of private STDI coverage on public LTDI take-up. I instrument the endogenous STDI coverage regressor from equation (2.1) using an indicator for treatment by an employer ending its STDI plan. The first stage regression estimates the effect of the employer ending its STDI plan on its employees' STDI coverage rate in relative year 1:

$$\mathbf{1}(\text{Employer STDI Coverage})_{i,t=1} = \pi_1 \cdot \mathbf{1}(\text{Treated})_i + \theta_1 \mathbf{X}_i + \varepsilon_i \tag{2.2}$$

The reduced form regression estimates the effect of the employer ending its STDI plan on its employees' LTDI receipt in relative year 2:

[7]Since the treatment group is employed in relative years 0 and 1, I cannot construct a control group conditioning on employment after relative year 1. Employment is an outcome which is negatively correlated with LTDI receipt, so conditioning on its value in the post-period would bias the results.

$$\mathbf{1}(\text{LTDI Receipt})_{i,t=2} = \pi_2 \cdot \mathbf{1}(\text{Treated})_i + \theta_2 \mathbf{X}_i + \varepsilon_i \tag{2.3}$$

The IV estimate of the effect of employer-provided STDI coverage on LTDI receipt is $\beta = \frac{\pi_2}{\pi_1}$.

To assess statistical significance I construct bootstrapped standard errors by resampling firms, since the quasi-experimental variation occurs at the firm-level. For each bootstrap draw I construct a new sample of 5801 firms by drawing with replacement from the 5801 firms that ended their STDI plans between 2003 and 2014, then construct the treatment and control groups of employees as before. I perform 100 bootstrap replications for all standard error estimates.

2.4 Results

2.4.1 Impact of Employer-Provided STDI on LTDI Take-Up

When a firm stops providing STDI to its employees, STDI coverage rates fall immediately by 61 percentage points among incumbent employees (Figure 2.1A). The effect is less than 100 percent for two reasons. First, only two thirds of employees had STDI coverage prior to their firm ending its STDI plan. Many firms only provide STDI to a subset of their employees—most often managers and salaried employees rather than laborers and hourly employees. Second, some employees whose STDI coverage is ceased will continue to receive STDI from another employer, either because they switch employers or have multiple employers. As expected, STDI coverage rates in the treated and control groups converge when the control employees' firms end their STDI plans in relative year 3.

Incumbent employees whose firms stopped providing STDI are 0.040 percentage points (13%) less likely to be receiving LTDI benefits two years later (Figure 2.1B). This effect indicates that employer-provided STDI coverage increases take-up of LTDI benefits. The ratio of the event study estimates shown in Figure 2.1 is equal to the instrumental variables estimate of the effect of employer-provided STDI on LTDI take-up: $\beta = \frac{\pi_2}{\pi_1} = \frac{-0.040}{-0.61} = 0.066$ percentage points. Table 2.2 reports the results of this IV regression in Column 1, with fixed effects for treatment cohort, 5-year age bins and sex, earnings deciles and industry added sequentially in Columns 2-5.

The estimated effect of employer-provided STDI on LTDI take-up is extremely stable as controls are added, ranging from 0.06 to 0.07 percentage points, with bootstrapped standard errors indicating significance at the 95% level. The fact that the IV estimate is invariant to controlling for observables, unlike the cross-sectional OLS estimates reported in Appendix Table B.2, reflects the fact the quasi-experiment successfully isolates two groups who are observably similar except for their STDI coverage.

Employer-provided STDI coverage raises the probability of receiving LTDI in relative year 2 by 33%, which represents a large and economically meaningful increase (Table 2.2, Column 5). This percent effect is identified for compliers with the quasi-experiment: workers who cease having private STDI because their employer stops offering it. The IV estimate β captures the local average treatment effect of STDI for this complier group. The percent effect of STDI on LTDI take-up is $\frac{\beta}{\bar{Y}-\beta\bar{D}}$, where $\bar{Y}$ and $\bar{D}$ are the mean LTDI take-up and STDI coverage among compliers. I estimate $\bar{Y}$ and $\bar{D}$ using the kappa-weighting procedure described by Abadie (2003), although the kappa-weighted means barely differ from the unweighted treatment group means in my setting.

Workers whose decision to take up LTDI is sensitive to private STDI coverage—the marginal LTDI recipients—are broadly similar to those who take up LTDI without private STDI (Appendix Table B.5). I measure the characteristics of marginal LTDI recipients by comparing the average characteristics of workers who (endogenously) receive LTDI in relative year 2 in the treatment group and control group. This method follows the approach used by Gruber, Levine, and Staiger (1999), Finkelstein and Notowidigdo (2018), and many others. Appendix Table B.5 shows that the differences in LTDI recipients from the two groups are both economically and statistically insignificant for age, sex, tax-deferred retirement savings rates and hospitalization rates during the pre-period (proxies for liquidity and health), and LTDI benefit amounts in relative years 2 and 3. The only significant difference is that marginal LTDI recipients have higher earnings in the pre-period, as reported in Column 1 and illustrated in Appendix Figure B.1.

2.4.2 Robustness

The quasi-experimental estimates reported in the previous section would be biased if the timing of firms ending their private STDI is correlated with their employees' risk of long-term disability. There are two major threats to identification. Firms may choose to end their private STDI coverage if they observe or anticipate their employees becoming sicker and more expensive to insure. Firms might also choose to end these employee benefits in advance of layoffs or during poor economic conditions, which are associated with increases in LTDI take up (Charles, Li, and Stephens 2017). Both of these channels would violate the exclusion restriction for my instrument and bias my estimates toward finding that private STDI reduces LTDI take up. Since I find that private STDI increases LTDI take up, my estimates would be overly conservative.

In practice, I find that the timing of firms ending their private STDI is uncorrelated with their incumbent employees' hospitalization rates or employment rates (Figure 2.2). I observe inpatient hospital admissions in each year (and exclude childbirths) for the subsample that does not live in Quebec, Manitoba or the northern territories between relative year -3 and relative year 2. Hospitalization rates are similar in the treatment and control groups and stable throughout the sample period.[8] Employment rates are 100% by construction in relative years 0 and 1, but flexible in other years. The employment rates in the treatment and control groups are nearly identical, and follow the same pattern over time.

2.4.3 Fiscal Externality of Employer-Provided STDI

This section uses the quasi-experimental estimate of the impact of employer-provided STDI on the *flow* of LTDI recipients to approximate its impact on the *stock* of LTDI recipients. Using data on LTDI flows from 2001 to 2015, I estimate the counterfactual number of LTDI recipients and dollars of LTDI expenditures in 2015 if employers had not provided STDI during the previous 15 years.

To estimate the effect of employer-provided STDI on the stock of LTDI recipients, I make five assumptions. First, I assume that employer-provided STDI increases annual LTDI flows by 0.036 percentage points, half as much as the two-year effect on LTDI stocks identified in the quasi-experiment (Table 2.2, Column 5).[9] Second, I assume that this treatment effect

[8]The slight increase in hospitalization rates from the pre-period to the post-period for both the treatment and control group reflects the fact that the sample is constructed while conditioning on not receiving LTDI benefits during the pre-period, thereby excluding some people who had disability-inducing hospitalizations in the pre-period.

[9]The effect of private STDI coverage on the stock of LTDI recipients in relative year 2 measures the effect on some individuals with a disability onset in relative year 0, nearly all individuals with a disability onset in relative year 1, and some individuals with a disability onset in the first eight months of relative year 2. On the one hand, the stock in relative year 2 excludes some individuals with a disability onset in relative years 2 (and relative year 1 to a much lesser extent) because there is a minimum four month waiting period for public LTDI after the onset of a disability, and some applicants wait much longer than four months for their

is constant for all workers with employer-provided STDI in the population. Third, I assume that marginal LTDI recipients have the same LTDI exit rates and mean benefit amounts as the average LTDI recipients who had employer-provided STDI in the year prior to LTDI take up (which is consistent with Appendix Table B.5). Fourth, I calculate the effect of employer-provided STDI over the 15 years of LTDI flows observed in my sample period, not on the stock of LTDI recipients in a steady state. Using a 15 year period underestimates the effect in the steady state, but does not require a parametric model of state transitions. Finally, I ignore the second-order effects of marginal individuals who do not join LTDI remaining in the workforce, and therefore remaining at risk of taking up LTDI in subsequent years. If their hazard rate for taking up LTDI in subsequent years is equal to the mean hazard among people with employer-provided STDI, this second order effect is negligible.

Under these five assumptions, if employers did not provide STDI during the 2000 to 2014 period the change in LTDI recipients and LTDI expenditures in 2015 would be:

$$\Delta \mathrm{N}_{2015}^{\mathrm{LTDI}} = \sum_{t=2000}^{2014} n_t \cdot \frac{\beta}{2} \cdot p_{t+1,2015} \tag{2.4}$$

$$\Delta \mathrm{b}_{2015}^{\mathrm{LTDI}} = \sum_{t=2000}^{2014} n_t \cdot \frac{\beta}{2} \cdot p_{t+1,2015} \cdot \bar{b}_{t+1,2015} \tag{2.5}$$

where the parameters are defined as follows. n_t is the number of workers with employer-provided STDI coverage in year t. $\frac{\beta}{2} = 0.036$ percentage points is the effect of employer-provided STDI coverage on LTDI take-up in year $t+1$. $p_{t+1,2015}$ is the probability that someone who had private STDI coverage in year t and took up LTDI in year $t+1$ is still receiving LTDI in 2015. And $\bar{b}_{t+1,2015}$ is the mean LTDI benefit in 2015 among these remaining recipients. Appendix Table B.3 reports the data underlying each of these parameters: the number of people with employer-provided STDI coverage in each year from 2000 to 2014, the number who take up LTDI in the subsequent year, the number still on LTDI in 2015, and the average benefit amount in 2015 among the remaining recipients.

I estimate that there would be 18,300 fewer LTDI recipients in 2015 and government

applications to be processed. On the other hand, some employees in the treated group may have lost their STDI coverage and become treated midway through relative year 0, since I observe whether a firm provides any STDI during the year but I cannot reliably observe the number of months of STDI they provided during relative year 0. I therefore approximate this cumulative effect on the stock of LTDI recipients in relative year 2 as the effect on two years of flows.

expenditures on LTDI benefits would be \$230 million dollars (5%) lower if there had been no employer-provided STDI in the 15-year period since 2000. The 95% confidence interval for these estimates spans 2,900 to 33,700 fewer LTDI recipients in 2015 and a \$40 million to \$410 million reduction in LTDI benefits. These estimates of the effect on LTDI stocks should be considered an illustrative approximation, not a precise value. In addition to the wide confidence intervals, the extrapolation from LTDI flows in the quasi-experimental sample to LTDI stocks in the full population relies on untestable approximations. The main takeaway is that the negative fiscal externality of employer-provided STDI is economically meaningful, and merits consideration when evaluating the consequences of STDI policy reforms.

2.4.4 Efficient Pigouvian Taxation of Employer-Provided STDI

The externality imposed by employer-provided STDI on the public LTDI budget generates economic inefficiencies because private employers and insurers do not internalize these costs when they set the price and choose the quantity of private STDI provided (Pauly 1974). The standard economic policy solution to a negative externality is a Pigouvian tax, which would make the providers of private STDI internalize the externality they impose on the government budget. In this section, I estimate the efficient Pigouvian tax on employer-provided STDI.

There are two key parameters for calculating the Pigouvian tax: the effect of private STDI on take up of LTDI and the net present value of benefits for the marginal LTDI recipients. The effect of private STDI on annual flows onto LTDI was estimated in the quasi-experiment above as $\frac{\beta}{2} = 0.036$ percentage points. To estimate the net present value of LTDI benefits among those induced to take up, I will assume that the marginal LTDI recipients have the same expected benefits as average LTDI recipients who had employer-provided STDI in the year prior to taking up LTDI. This assumption is not rejected by the data, as explained in Section 2.4.2 and reported in Appendix Table B.5.

I estimate that the net present value of expected LTDI benefits for an average LTDI recipient who has STDI coverage is \$97,000 in the year prior to taking up (Figure 2.3). To obtain this estimate, I begin by plotting mean LTDI benefits received in each year after take up by people who took up LTDI between 2001 and 2015 and had employer-provided STDI coverage in the prior year. There is little deviation across these 15 cohorts in the evolution of benefits over time (Figure 2.3A). I therefore calculate mean benefits in each year after take up by combining all observed cohorts. Mean benefits decline over time as LTDI recipients exit the program (typically by transitioning to retirement benefits at age 65 or dying). Figure 2.3B shows that a linear trend describes the evolution of mean benefits well after the first year. Average benefits are elevated in the first year because many recipients receive up to 12 months of retroactive payments to cover benefits during the period in which the LTDI claim was being processed. In the 15th year following take up—the last year I can observe in the data—mean benefits are $3200, and I use a linear extrapolation to approximate mean benefits in subsequent years, which hit zero approximately 19 years after first receiving LTDI. I calculate the net present value of these LTDI benefits in the year prior to LTDI take up (when recipients had private STDI coverage) using a 3% discount rate. Because the later years are heavily discounted, the inclusion of the extrapolated data makes little difference: the net present value is $94,000 using the 15 years of observed data and $97,000 including the additional 4 years of extrapolated data.

The efficient Pigouvian tax on employer-provided STDI is approximately $35 per insured employee per year, which is equal to the 0.036 percentage point increase in LTDI take up multiplied by the $97,000 net present value of benefits for those who take up. This estimate reflects the partial equilibrium effect of employer-provided STDI on the government LTDI budget (excluding administration fees), not the general equilibrium effect on the full government budget. If marginal LTDI recipients would counterfactually be working and paying taxes, the full fiscal externality would be larger than my estimate. If marginal LTDI recipients would instead be dependent on other government benefit programs, as observed in other settings by Staubli (2011) and Borghans, Gielen, and Luttmer (2014), then the fiscal externality on the government LTDI budget overstates the externality on the full government budget. Unfortunately, these spillovers to other tax and transfer programs cannot be precisely estimated due to the limited size of the quasi-experimental sample in this paper.

A Pigouvian tax on employer-provided STDI could be implemented in Canada by adjusting the existing Pigouvian subsidy offered through payroll tax rebates in Premium Reduction Program (PRP). The PRP was introduced at the same time EI Sickness benefits were introduced in 1971, with the goal of ensuring that the new public STDI benefits did not crowd out existing private STDI plans (HRSDC 2009). Private STDI plans are the first payer of benefits, so the PRP provides a payroll tax rebate to employers who provide private STDI coverage that is set actuarially to be equal to the expected savings they generate for the public EI Sickness program. This Pigouvian subsidy reflects the positive fiscal externality from private STDI onto the public STDI budget, but does not reflect the negative fiscal externality onto the public budget LTDI. In 2014, the PRP paid $848 million in payroll tax rebates to 5.7 million workers with private STDI, an average of $150 per insured worker. Incorporating the Pigouvian tax of $35 per insured worker would reduce the PRP payroll tax rebate by 23%, saving roughly $200 million per year. The effective tax rate on private STDI premiums would be less than 23% because private STDI benefits are more generous than public STDI benefits, so the cost of private STDI likely exceeds the PRP payroll tax rebate.

2.5 Conclusion

This paper shows that employer-provided short-term disability insurance increases long-term disability insurance take-up and imposes a negative fiscal externality on the government budget. This represents a specific case of the multiple-dealing externality described by Pauly (1974), where insurers do not internalize the costs of the moral hazard they generate for other parties insuring the same risk. In the case of private STDI, however, economists and insurers had speculated that the incentive for employers to provide increased workplace assistance and accommodation for disabled employees receiving private STDI would outweigh the moral hazard effects of increased benefits during the waiting period for LTDI. The results in this paper demonstrate that the moral hazard effect dominates and increases LTDI take up.

There remain many unresolved questions about the effects of short-term disability insurance and the optimal design of disability insurance policy. One direction for future research would be to identify the effects of specific STDI policy parameters such as the replacement rate, the gap between the end of STDI benefits and the beginning of LTDI benefits, and the nature of the assistance programs offered to workers while receiving STDI benefits. These estimates would be useful to both public and private insurers designing benefit packages that maximize insurance while minimizing behavioral distortions.

More broadly, there is little evidence on the tradeoffs between public and private provision of STDI. Public STDI programs can use experience rating to create financial incentives for workplace accommodations. Employers providing private STDI can match the level of insurance provided to the preferences of their workers better than a one-size-fits-all universal program. A governor proposing universal STDI for one of the 45 U.S. states that does not yet have such a program would have to choose between an employer mandate and public provision, then decide how the risks of short-term disability costs should be distributed between employers and the government. Empirical estimates of the effects of experience rating in short-term disability insurance and of heterogeneity in workers' demand for STDI within firms and across firms would provide evidence to guide those policy decisions.

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Tables and Figures

Table 2.1: Balance on Observable Characteristics

	Workers in 2010		Workers at Firms Ending Private STDI	
	Private STDI	No Private STDI	Treated	Control
Earnings Quintile in 3 Prior Years				
1 — Poorest	7%	32%	5%	4%
2	14%	25%	23%	21%
3	22%	18%	29%	29%
4	27%	13%	24%	25%
5 — Richest	30%	11%	19%	20%
Age Group				
25-29	11%	16%	14%	14%
30-39	27%	27%	28%	27%
40-49	32%	30%	33%	33%
50-59	30%	27%	25%	26%
Female	50%	46%	41%	39%
Industry				
Natural Resources and Mining	3%	3%	2%	3%
Construction	2%	11%	6%	6%
Manufacturing	13%	11%	24%	24%
Trade, Transportation, and Utilities	17%	23%	30%	28%
Information Services	4%	1%	1%	3%
Financial Services	9%	4%	3%	3%
Professional and Business Services	7%	14%	11%	9%
Education and Health Services	26%	13%	10%	10%
Leisure and Hospitality	2%	9%	4%	4%
Public Administration	17%	3%	5%	4%
Other Services	1%	6%	5%	4%
Unknown	0%	2%	0%	0%
N	5,266,000	5,752,864	176,653	187,227

Notes: The sample of workers in 2010 includes Canadians ages 25 to 59 who had positive employment earnings in 2010. Workers in 2010 are considered to have private STDI if they had an STDI payroll tax rebate with any employer at any point during the year. Workers at firms ending private STDI are ages 25 to 59 and employed by a firm that ended its private STDI plan between 2003 and 2014. Control workers are employed by a firm that ends its private STDI two years after the treated workers, as described in more detail in Section 2.3.2. Earnings quintiles are calculated based on the sum of real earnings in 2007-2009 for the 2010 workers and in relative years -3, -2, -1 for the quasi-experimental sample, separately in each age, sex and year. Industries are categorized using 2-digit NAICS codes with groupings from the Bureau of Labor Statistics (2018).

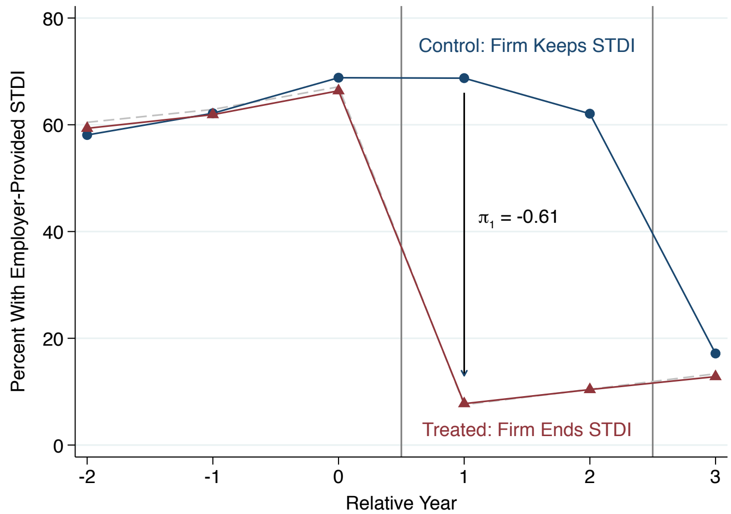
Table 2.2: Quasi-Experimental Effect of Employer-Provided STDI Coverage on LTDI Take-Up

	% Receiving Long-Term DI Benefits in Year 2				
	(1)	(2)	(3)	(4)	(5)
Employer STDI Coverage in Year 1	0.066	0.071	0.061	0.072	0.071
	(0.032)	(0.035)	(0.027)	(0.030)	(0.031)
Fixed Effects:					
Employer Switch Cohort		X	X	X	X
Interacted 5-Year Age Group and Sex			X	X	X
Decile of Earnings in Prev. 3 Years				X	X
Industry of Employment in Year 1					X
First Stage Coefficient	-0.61	-0.61	-0.61	-0.61	-0.61
First Stage F Statistic	232648	233729	233998	239817	244701
Complier Mean Percentage:					
Receiving LTDI Benefits in Year 2	0.252	0.259	0.260	0.256	0.254
With Employer STDI Coverage in Year 1	48.5	48.9	48.8	48.6	48.8
% Effect of Employer STDI Coverage on LTDI Receipt in Year 2	30%	31%	27%	33%	33%
Sample Size	363,880	363,880	363,880	363,880	363,880

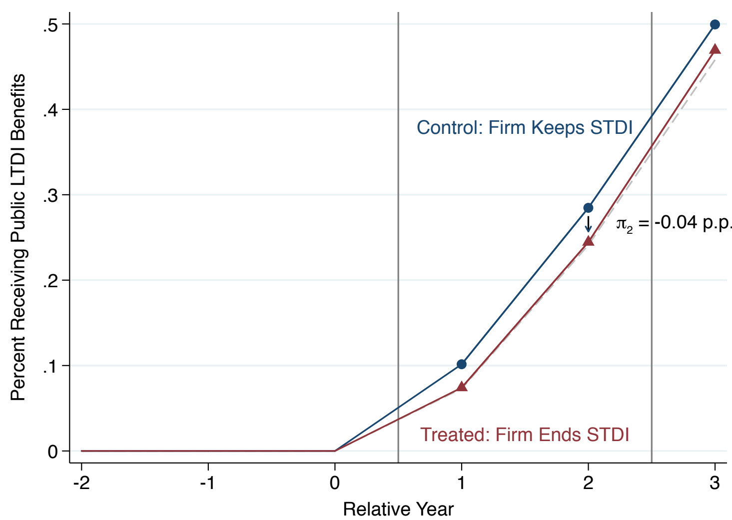
Notes: Coefficients and standard errors are reported in percentage point units. Standard errors are bootstrapped by resampling the 5,801 firms that end their private STDI coverage in the sample period with 1000 replications. Complier means are calculated using Abadie (2003) kappa-weighting.

Figure 2.1: Firms Ending Employer-Provided STDI Lower LTDI Take-Up

(A) First Stage: Change in STDI Coverage



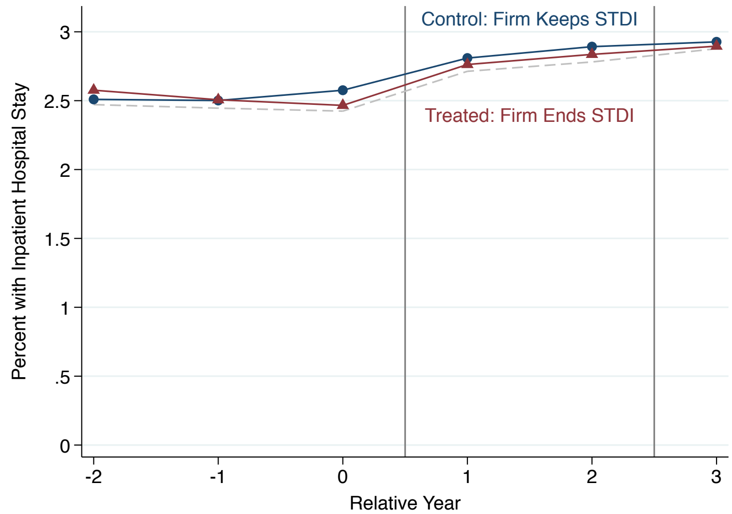
(B) Reduced Form: Change in LTDI Receipt



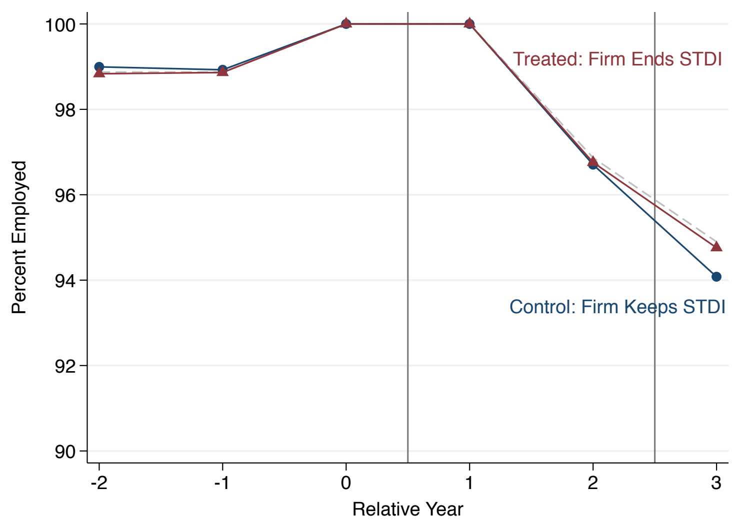
Notes: The treatment and control groups consist of workers who were employed in relative years 0 and 1 by a firm that offered private STDI to some of its employees for three consecutive years then ended its private STDI coverage in relative year 1 (treated) or relative year 3 (control), as described in Section 2.3.2. The red and blue series plot the mean rate of private STDI coverage (Panel A) and public LTDI receipt (Panel B) observed among treated workers (N=176,653) and control workers (N=187,227). The grey dashed line (mostly covered by the red series) plots the mean value for treated workers after controlling for fixed effects in treatment cohort, interacted 5-year age bins and sex, decile of average earnings in the relative years {-2,-1,0}, and industry of employment in relative year 1.

Figure 2.2: Validation Checks on Timing of Firms Ending Employer-Provided STDI

(A) Hospitalization



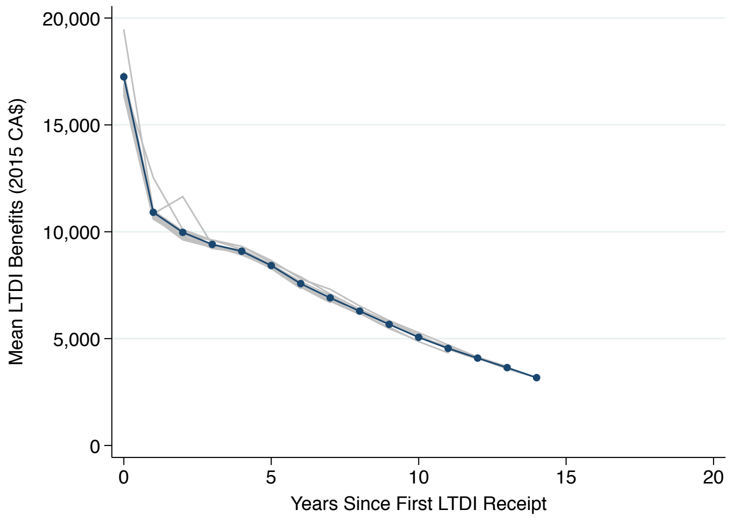
(B) Employment



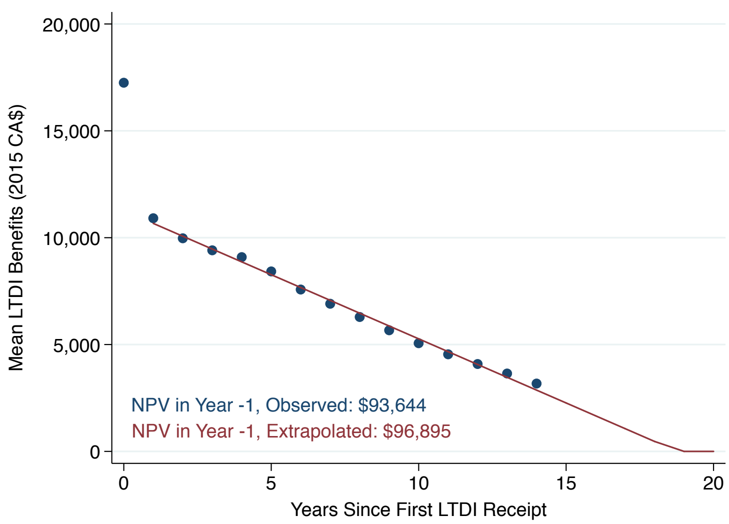
Notes: The treatment and control groups are constructed as described in Section 2.3.2. Panel A plots the annual inpatient hospitalization rate excluding births among treated workers (N=100,738) and control workers (N=108,711) who did not reside in Manitoba, Quebec or the northern territories during relative years {-2,-1,0,1,2,3}, since I do not observe complete hospitalization data for those areas. Panel B plots the annual employment rate for among all treated workers (N=176,653) and control workers (N=187,227), defined as having positive employment earnings during the year, and which is 100% in relative years 0 and 1 by construction. The grey dashed line plots the mean value for treated workers after controlling for fixed effects in treatment cohort, interacted 5-year age bins and sex, decile of average earnings in the relative years {-2,-1,0}, and industry of employment in relative year 1.

Figure 2.3: Net Present Value of LTDI Benefits for Workers with Private STDI

(A) Mean LTDI Benefits Over Time, by Cohort



(B) Mean LTDI Benefits Over Time, Linear Extrapolation



Notes: Each panel shows the mean public LTDI benefits received by workers who had employer-provided STDI coverage in the year prior to taking up LTDI. Workers who subsequently stop receiving LTDI benefits remain in the sample as zeros. In Panel A, the grey lines show mean benefits for each cohort of beneficiaries who took up between 2001 (observed 15 years) and 2015 (observed 1 year). The blue series shows the weighted average across all observed cohorts. In Panel B, the blue series is identical to Panel A and the red line is a linear extrapolation of observed benefits past the 15 years for which they are observed. The extrapolation excludes the first year of benefits (year 0), which is substantially higher than subsequent years because it includes up to 12 months of retroactive payments for the period in which the LTDI claim was being processed. The net present value of benefits in the year prior to LTDI receipt is calculated using a 3% discount rate.

Appendix

2.A Reverse Quasi-Experiment: Firms Starting STDI

The research design used in this paper measures the effect of private STDI coverage using quasi-experimental variation generated by firms *ending* their STDI plans. It would be equally natural to analyze the reverse quasi-experiment and measure the variation in STDI coverage generated by firms *starting* their STDI plans for the first time. This section explains why this reverse quasi-experiment cannot be reliably studied using the Canadian administrative data, presents the results of the reverse quasi-experiment, and argues that the estimates are severely attenuated but nonetheless consistent with the baseline results presented in the paper.

2.A.1 Data Limitations

As described in Section 2.2.3, I observe whether a worker has private STDI coverage indirectly when their employer claims a payroll tax rebate for providing STDI. This payroll tax rebate, called the Premium Reduction Program (PRP), was worth an average of $150 per employee per year in 2014. However, employers must make an active decision to participate and there is substantial evidence that many employers offer private STDI but fail to claim the tax rebate.

According to a 2007 survey of 607 employers conducted by the Canadian federal government, 72 percent of employers *offering private STDI plans* reported being unaware of the PRP tax rebate program (HRSDC 2009). Large firms (with at least 500 employees) are most likely to be aware of the tax rebate, most likely to offer private STDI, and employ most Canadian workers. Therefore the measurement error in private STDI coverage at the worker-level is much smaller than at the firm-level.

The adminstrative tax records can reliably identify the timing of firms ending their private STDI plans. I study the employees of firms that were claiming the PRP tax rebate for three consecutive years, then continued operating for another three years but chose to stop claiming the rebate. All of these firms were aware of the rebate, and would likely only give it up when they stop offering an eligible STDI plan.

By contrast, many firms may offer private STDI plans and only start participating in the PRP tax rebate years later when they learn that it is available. For such firms, the year in which they start claiming the PRP tax rebate does not coincide with a real change in STDI coverage, and would have no effect on incentives to take up LTDI. The inclusion of firms with no change in STDI coverage introduces measurement error that would attenuate the estimated effects of private STDI (proxied by PRP participation).

2.A.2 Results of Reverse Quasi-Experiment

In this section, I estimate the effects of private STDI using the timing of firms starting to claim the PRP tax rebate, which does not necessarily coincide with the timing of them first offering private STDI.

I construct the "reverse quasi-experiment" analogously to the main quasi-experiment in the paper. I define a firm as starting an STDI plan if the firm operated but did not receive a PRP tax rebate for three consecutive years, then received a PRP tax rebate for some or all of its employees during the subsequent three years. By this definition, 6764 Canadian firms started an STDI plan between 2003 and 2014.

I consider an employee treated if they were employed by a firm during the last year it provided no STDI coverage and the first year it provided STDI coverage, defined as relative years 0 and 1. As before, I construct a group of control employees who were employed by a different firm in relative years 0 and 1, and whose firm started its STDI plan in relative year 3. Appendix Table B.6 shows that the employees in the treated and control groups are nearly identical on observables, as was reported in Table 2.1 for the main quasi-experiment.

The reverse quasi-experiment also shows that offering private STDI increases take up of public LTDI, but the estimated effect is roughly four times smaller and is statistically insigificant. Appendix Figure B.2 replicates the event study figures from Figure 2.1 and shows that the first stage is similar, but the reduced form is much smaller. Appendix Table B.7 replicates the IV regressions from Table 2.2. When firms start offering private STDI, as proxied by participating in the PRP tax rebate, private STDI coverage increases take-up of public LTDI by 0.01 to 0.02 percentage points (as opposed to 0.06 to 0.07 percentage points estimated using firms ending their private STDI). This corresponds to a 6 to 11% increase in LTDI take up (as opposed to 27% to 33%).

2.A.3 Analysis of Attenuation Bias

The weak results of the reverse quasi-experiment could be interpreted as evidence that private STDI coverage does not have a significant effect on public LTDI take up, or they could reflect severe attenuation bias. I estimate the effect of private STDI coverage on public *STDI* take up, and use these results to gauge the magnitude of attenuation bias.

When an employer begins providing private STDI coverage, they mechanically shift employees' usage of short-term disability benefits from Canada's universal public STDI plan to the private STDI plan. By law, private STDI plans are the first payer of STDI benefits in Canada. As discussed in Section 2.2.1, workers with private STDI remain eligible for the universal public STDI benefit, but any private benefits received are deducted one-for-one from their public benefit payments. For most workers covered by PRP tax rebates, their private STDI benefits will completely replace public STDI benefits.

As expected, when firms stop participating in the PRP tax rebate and therefore end their private STDI, their employees become more likely to receive public STDI (Appendix Figure B.3A). This effect is mechanical: employees who would have received their short-term disability benefits entirely from a private plan are instead receiving public benefits. However, when firms start participating in the PRP tax rebate, there is almost no change in their employees' likelihood to receive public STDI (Appendix Figure B.3B). Because the effect of private STDI coverage on public STDI receipt is mechanical, the null result implies that few employees began receiving private STDI when their firm began receiving the PRP tax rebate.

Appendix Table B.8 reports the IV estimates for the effect of private STDI coverage on public STDI receipt. Firms starting their STDI plans and firms ending their STDI plans ought have symmetrical effects on their employees' receipt of public STDI. Yet the variation in STDI coverage identified using firms ending their PRP tax rebate produces a drop in public STDI receipt of 3 percentage points, which is four times larger than the 0.8 percentage point effect identified using firms starting their PRP tax rebate.

The magnitude of attenuation bias implied by Appendix Table B.8 can completely explain the differences in the estimated effect of public STDI coverage on private LTDI take up identified by the main quasi-experiment and the reverse quasi-experiment. The estimated effects of private STDI coverage on public *STDI* take up should be considered more credible than the estimated effect on public *LTDI* take up, since the effect on public STDI receipt is mechanical and much larger in magnitude. I therefore use the ratio of the estimated effects on private STDI take up from Column 5 of Appendix Table B.8 to estimate the scale of the attenuation bias. Rescaling the effect of private STDI on public LTDI take up estimated using the reverse quasi-experiment (Column 5 of Appendix Table B.7) by this ratio yields an estimated effect of 0.070 percentage points. This is almost identical to the 0.071 percentage point effect estimated using the main quasi-experiment (Column 5 of Table 2.2).

In sum, the results of the reverse quasi-experiment are attenuated by errors in measuring the timing of firms starting their private STDI plans. But after adjusting for attenuation bias, the results are entirely consistent with the baseline results presented in the paper, which are based on a reliable measure of the timing of firms ending their private STDI plans. The results of the reverse quasi-experiment therefore provide additional evidence that private STDI coverage increases public LTDI take up.

Appendix Tables and Figures

Appendix Table B.1: Sample Selection

	Workers in 2010		Quasi-Experiment, Firms Ending STDI		Reverse Quasi-Experiment, Firms Starting STDI	
	Private STDI	No Private STDI	Treated	Control	Treated	Control
Full Sample	6,456,801	11,310,906	269,210	276,575	400,614	334,758
Unique Individual in Cohort	n/a	n/a	268,207	275,786	399,003	333,182
Not Missing STDI Coverage Indicator	n/a	n/a	268,079	275,634	398,765	333,023
Not Missing Age and Sex	6,453,982	11,252,825	267,871	275,457	398,384	332,707
Ages 25-59	5,624,619	7,447,754	196,920	206,775	314,389	260,954
Sufficient Earnings History for LTDI	5,274,051	5,761,354	176,807	187,376	278,018	230,664
No LTDI in Prior 3 Years	**5,266,000**	**5,752,864**	**176,653**	**187,227**	**277,803**	**230,471**

Notes: This table describes the number of individuals remaining in the three analysis samples after each of the sample selection criteria is applied successively. The full sample for Workers in 2010 consists of all individuals with positive T4 earnings in 2010. The full sample for the quasi-experiment with firms ending STDI consists of workers who were employed in relative years 0 and 1 by a firm that provided private STDI to some of its employees for three consecutive years then ended its private STDI coverage in relative year 1 (treated) or relative year 3 (control). The full sample for the reverse quasi-experiment with firms starting STDI is constructed analogously for employees of firms that did not offer private STDI to any of its employees for three consecutive years, then provided private STDI coverage to some of its employees for at least three years starting in relative year 1 (treated) or relative year 3 (control).

Appendix Table B.2: Cross-Sectional Relationship Between Employer-Provided STDI Coverage and LTDI Take-Up

	% Receiving Long-Term DI Benefits in 2011			
	(1)	(2)	(3)	(4)
Employer STDI Coverage in 2010	0.011	-0.004	0.145	0.129
	(0.003)	(0.003)	(0.003)	(0.004)
Fixed Effects:				
Interacted Age and Sex		X	X	X
Decile of Earnings in 2007-2009			X	X
Industry of Employment in 2010				X
Mean Percentage:				
Receiving LTDI Benefits in 2011	0.268	0.268	0.268	0.268
With Employer STDI Coverage in 2010	47.8	47.8	47.8	47.8
% Effect of Employer STDI Coverage on LTDI Receipt in 2011	4%	-1%	73%	63%
Sample Size	11,018,864	11,018,864	11,018,864	11,018,864

Notes: Coefficients and standard errors are reported in percentage point units. Standard errors are calculated using the regular OIM forumla. Earnings deciles are calculated based on real earnings in 2007-2009 separately for each age and sex. Industry fixed effects use 20 categories of 2-digit NAICS codes, with a separate category for unknown industry.

Appendix Table B.3: LTDI Take-Up, Persistence and Benefits Among People with Employer STDI

Year	Number of Workers with Employer-Provided STDI	...Who Take Up LTDI In The Following Year	...And Still Receive LTDI in 2015	Mean 2015 LTDI Benefit Given Receipt
2000	5,352,030	9,250	2,529	$11,605
2001	5,413,879	10,619	3,392	$11,564
2002	5,420,660	10,681	3,748	$11,496
2003	5,427,436	10,271	3,906	$11,399
2004	5,428,214	10,838	4,676	$11,337
2005	5,563,131	11,694	5,787	$11,443
2006	5,621,993	10,593	5,717	$11,374
2007	5,714,908	11,169	6,540	$11,416
2008	5,700,237	11,082	7,162	$11,365
2009	5,593,129	10,661	7,825	$11,307
2010	5,589,851	10,469	7,980	$11,975
2011	5,675,616	10,913	8,739	$11,973
2012	5,702,720	10,021	8,304	$11,846
2013	5,516,764	9,668	8,797	$12,083
2014	5,693,876	11,134	11,134	$18,865

Notes: For each calendar year, column 2 reports the number of Canadian workers aged 25 to 59 who received private STDI coverage from any employer at any point during the year and received no public LTDI benefits. Column 3 reports the number of the workers among the set in Column 2 who received public LTDI benefits during the subsequent calendar year. Column 4 reports the number of workers among the set in Column 3 who continued to receive public LTDI benefits in 2015. Column 5 reports the mean amount of public LTDI benefits received in 2015 among the set of workers in Column 4.

Appendix Table B.4: IV Regression Analysis using Employees of Firms Ending Private STDI Coverage with STDI in Year 0

	% Receiving Long-Term DI Benefits in Year 2				
	(1)	(2)	(3)	(4)	(5)
Employer STDI Coverage in Year 1	0.060	0.064	0.056	0.066	0.064
	(0.027)	(0.031)	(0.026)	(0.024)	(0.026)
Fixed Effects:					
Employer Switch Cohort		X	X	X	X
Interacted 5-Year Age Group and Sex			X	X	X
Decile of Earnings in Prev. 3 Years				X	X
Industry of Employment in Year 1					X
First Stage Coefficient	-0.86	-0.86	-0.86	-0.86	-0.86
First Stage F Statistic	734528	732140	732587	732695	733131
Complier Mean Percentage:					
Receiving LTDI Benefits in Year 2	0.263	0.267	0.266	0.264	0.264
With Employer STDI Coverage in Year 1	47.6	47.6	47.6	47.6	47.8
% Effect of Employer STDI Coverage on LTDI Receipt in Year 2	26%	27%	23%	29%	27%
Sample Size	245,960	245,960	245,960	245,960	245,960

Notes: Coefficients and standard errors are reported in percentage point units. Standard errors are bootstrapped by resampling the 5,801 firms that end their private STDI coverage in the sample period with 1000 replications. Complier means are calculated using Abadie (2003) kappa-weighting.

Appendix Table B.5: Characteristics of Marginal LTDI Recipients

	Earnings in Prior 3 Years	Retirement Saver in Prior 3 Years	Hospitalized in Prior 3 Years	Female	Age in Year 1	LTDI Benefits in Year 2	LTDI Benefits in Year 3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: LTDI Recipients in Year 2							
IV Estimate of Difference Between People	11176	0.018	-0.020	-0.019	0.21	506	9
With–Without Private STDI Coverage	(3093)	(0.056)	(0.059)	(0.058)	(0.85)	(750)	(471)
Fixed Effects:							
Employer Switch Cohort	X	X	X	X	X	X	X
Mean in Control Group	43251	0.495	0.311	0.409	50.72	11764	9380
Mean in Treated Group	36229	0.484	0.319	0.419	50.55	11428	9356
N	965	965	629	965	965	965	965
Panel B: Full Quasi-Experiment Sample							
IV Estimate of Difference Between People	2556	0.011	0.002	-0.025	0.48		
With–Without Private STDI Coverage	(1486)	(0.010)	(0.002)	(0.014)	(0.22)		
Fixed Effects:							
Employer Switch Cohort	X	X	X	X	X		
Mean in Control Group	53466	0.579	0.069	0.392	41.94		
Mean in Treated Group	51848	0.574	0.068	0.406	41.61		
N	363,880	363,880	202,006	363,880	363,880		

Notes: Each column reports the results of an IV regression analogous to Column 2 of Table 2.2, but with a different outcome variable. In Panel A, the sample is restricted to individuals in the quasi-experimental sample who (endogenously) received LTDI benefits in relative year 2. In Panel B, the sample includes all individuals in the baseline results reported in Table 2.2.

Appendix Table B.6: Balance on Observable Characteristics in Reverse Quasi-Experiment

	Workers at Firms Starting Private STDI	
	Treated	Control
Earnings Quintile in 3 Prior Years		
1 — Poorest	4%	4%
2	17%	18%
3	25%	25%
4	26%	26%
5 — Richest	28%	27%
Age Group		
25-29	16%	16%
30-39	31%	31%
40-49	31%	31%
50-59	21%	22%
Female	41%	42%
Industry		
Natural Resources and Mining	4%	4%
Construction	7%	7%
Manufacturing	18%	18%
Trade, Transportation, and Utilities	22%	24%
Information Services	3%	3%
Financial Services	7%	5%
Professional and Business Services	16%	16%
Education and Health Services	9%	10%
Leisure and Hospitality	6%	4%
Public Administration	5%	5%
Other Services	3%	3%
Unknown	0%	0%
N	277,803	230,471

Notes: This table replicates Table 2.1 for the sample of workers aged 25 to 59 employed by a firm that started receiving a payroll tax rebate for providing a private STDI plan between 2003 and 2014.

Appendix Table B.7: Reverse Quasi-Experiment, Effect of Employer-Provided STDI Coverage on LTDI Take-Up

	% Receiving Long-Term DI Benefits in Year 2				
	(1)	(2)	(3)	(4)	(5)
Employer STDI Coverage in Year 1	0.013	0.011	0.016	0.020	0.018
	(0.024)	(0.022)	(0.024)	(0.021)	(0.023)
Fixed Effects:					
Employer Switch Cohort		X	X	X	X
Interacted 5-Year Age Group and Sex			X	X	X
Decile of Earnings in Prev. 3 Years				X	X
Industry of Employment in Year 1					X
First Stage Coefficient	0.64	0.64	0.64	0.64	0.64
First Stage F Statistic	348527	351151	351849	366002	382969
Complier Mean Percentage:					
Receiving LTDI Benefits in Year 2	0.198	0.197	0.197	0.196	0.197
With Employer STDI Coverage in Year 1	54.7	54.7	54.7	54.8	54.7
% Effect of Employer STDI Coverage on LTDI Receipt in Year 2	7%	6%	8%	11%	10%
Sample Size	508,274	508,274	508,274	508,274	508,274

Notes: Coefficients and standard errors are reported in percentage point units. Standard errors are bootstrapped by resampling the 6,764 firms that start their private STDI coverage in the sample period with 1000 replications. Complier means are calculated using Abadie (2003) kappa-weighting.

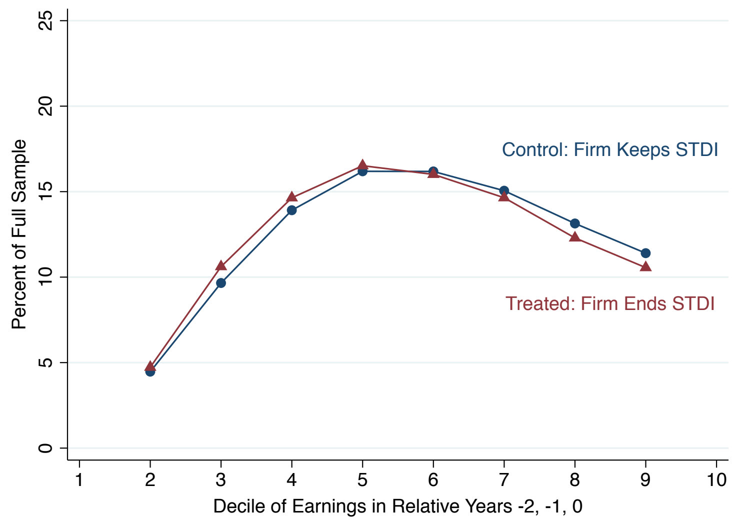
Appendix Table B.8: Quasi-Experimental Effect of Employer-Provided STDI Coverage on Public STDI Take-Up

	% Receiving Public Short-Term DI Benefits in Year 1				
	(1)	(2)	(3)	(4)	(5)
Panel A: Firms Ending Private STDI					
Employer STDI Coverage in Year 1	-3.224	-3.265	-3.259	-3.119	-3.087
	(0.290)	(0.228)	(0.218)	(0.212)	(0.179)
Fixed Effects:					
Employer Switch Cohort		X	X	X	X
Interacted 5-Year Age Group and Sex			X	X	X
Decile of Earnings in Prev. 3 Years				X	X
Industry of Employment in Year 1					X
Sample Size	363,880	363,880	363,880	363,880	363,880
Panel B: Firms Starting Private STDI					
Employer STDI Coverage in Year 1	-0.854	-0.879	-0.850	-0.807	-0.791
	(0.176)	(0.166)	(0.156)	(0.139)	(0.130)
Fixed Effects:					
Employer Switch Cohort		X	X	X	X
Interacted 5-Year Age Group and Sex			X	X	X
Decile of Earnings in Prev. 3 Years				X	X
Industry of Employment in Year 1					X
Sample Size	508,274	508,274	508,274	508,274	508,274

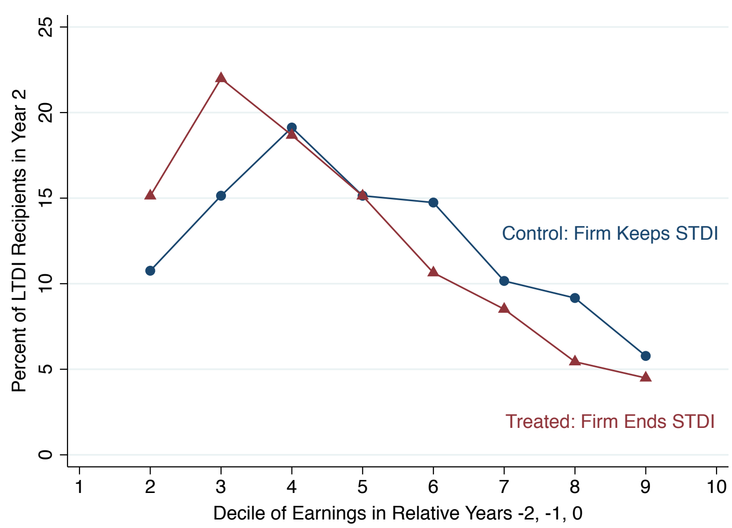
Notes: Coefficients and standard errors are reported in percentage point units. Standard errors are bootstrapped by resampling the 5,801 (6,764) firms that end (start) their private STDI coverage in the sample period with 1000 replications.

Appendix Figure B.1: Marginal LTDI Recipients Induced by Employer-Provided STDI Have Higher Earnings

(A) Distribution of Prior Earnings, Quasi-Experiment Sample



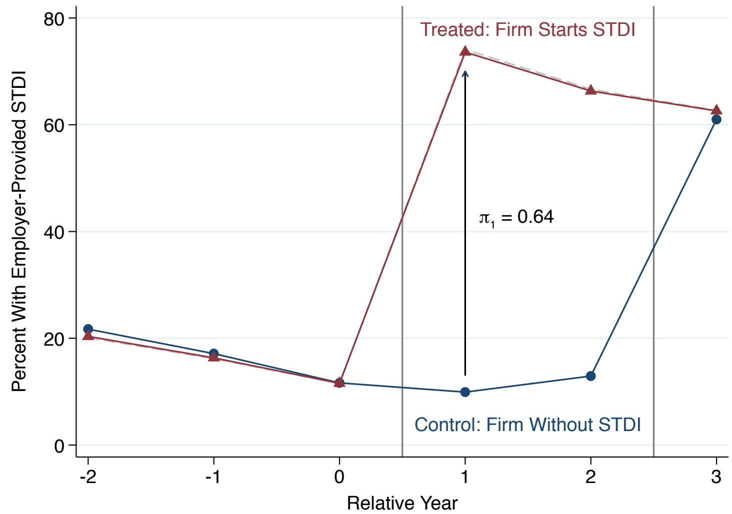
(B) Distribution of Prior Earnings, Subsample Who Take-Up LTDI in Year 2



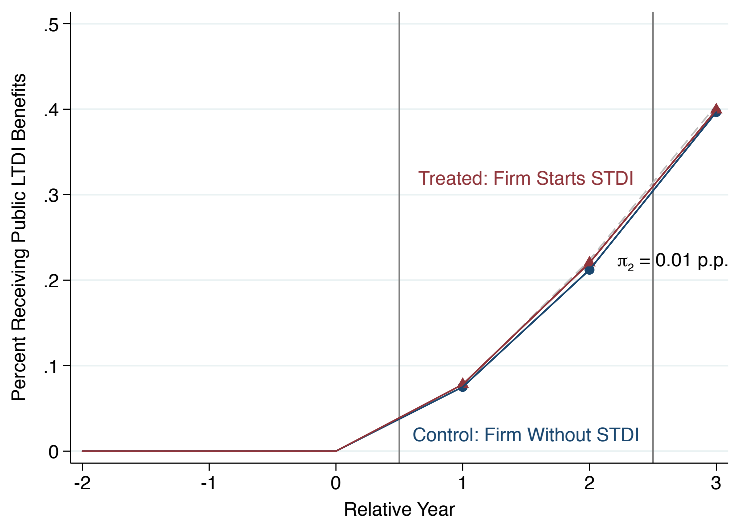
Notes: This figure plots the distribution of earnings deciles during the pre-period (the 3-year sum of earnings during relative years -2, -1 and 0) for workers whose firms ended their private STDI plan in relative year 1 (treated) or relative year 3 (control). Panel A shows the distribution for all treated and control workers in the main quasi-experimental sample. Panel B plots the distribution for the subsample of treated and control workers who endogenously take up LTDI in relative year 2. Earnings deciles are calculated relative to the full population of Canadian workers, separately for each age, sex and 3-calendar-year period. The bottom and top deciles are suppressed due to small sample sizes.

Appendix Figure B.2: Effect of Firms Starting Employer-Provided STDI on Receipt of LTDI

(A) First Stage: Change in STDI Coverage



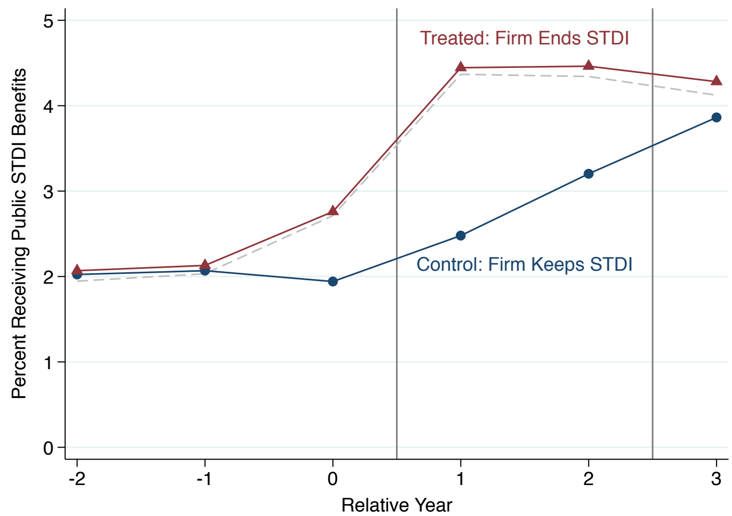
(B) Reduced Form: Change in LTDI Receipt



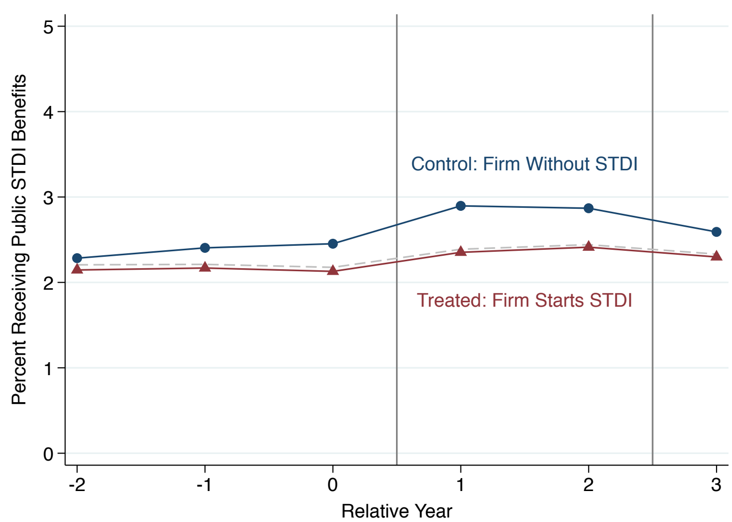
Notes: This figure replicates the analysis shown in Figure 2.1 for the reverse quasi-experiment of firms *starting* their STDI plans, instead of firms *ending* their STDI plans as shown in Figure 2.1. Appendix Section 2.A describes how the treatment and control groups are constructed, and explains how the results shown in Panel B are attenuated by unreliable measurements of the timing of firms starting their STDI plans. The red and blue series plot the mean rate of private STDI coverage proxied by tax rebates (Panel A) and public LTDI receipt (Panel B) observed among treated workers (N=277,803) and control workers (N=230,471). The grey dashed line (mostly covered by the red series) plots the mean value for treated workers after controlling for fixed effects in treatment cohort, interacted 5-year age bins and sex, decile of average earnings in the relative years {-2,-1,0}, and industry of employment in relative year 1.

Appendix Figure B.3: Effect of Employer-Provided STDI on Receipt of Public STDI

(A) Quasi-Experiment: Firms Ending Private STDI



(B) Reverse Quasi-Experiment: Firms Starting Private STDI



Notes: This figure plots the mean annual rate of public STDI benefit receipt, among workers at firms observed *ending* their STDI plans (Panel A, main quasi-experiment) and among workers at firms observed *starting* their STDI plans (Panel B, reverse quasi-experiment). Appendix Section 2.A explains how the results shown in Panel B are mechanically attenuated by unreliable measurements of the timing of firms starting their STDI plans. For details on the treatment and control groups in each quasi-experimental sample, and a description of how each plot was constructed, see the notes to Figure 2.1 for Panel A and the notes to Appendix Figure B.2 for Panel B.

Chapter 3

The Association Between Income and Life Expectancy in the United States, 2001-2014

Joint with Raj Chetty, Sarah Abraham, Shelby Lin, Benjamin Scuderi, Nicholas Turner, Augustin Begeron, and David Cutler.

3.1 Introduction

Higher incomes are associated with longer life expectancy,[1–9] but several aspects of the relationship between income and longevity remain unclear. First, little is known about the exact shape of the income-longevity gradient. Is there a threshold above which additional income is no longer associated with increased life expectancy or a safety net below which

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further reductions in income do not harm health?

Second, there is debate about how socioeconomic gaps in longevity are changing over time. Prior work has shown that longevity gaps increased in recent decades. Some studies suggest a reduction in life expectancy for women of low socioeconomic status in recent years, but the robustness of this conclusion has been questioned.[6,10–14]

Third, most studies have examined the relationship between income and longevity at a national level. To what extent do gaps in longevity vary at the local area level?

Fourth, the sources of the longevity gap remain unclear. The socioeconomic gradient in longevity has been variously attributed to factors such as inequality, economic and social stress, and differences in access to medical care.[15] These theories remain debated.

This study addressed these 4 issues by analyzing newly available data on income and mortality for the US population from 1999 through 2014. The following sets of analyses were conducted: (1) characterizing the association between life expectancy at 40 years of age and income in the United States as a whole; (2) estimating the change in life expectancy by income group from 2001 through 2014; (3) mapping geographic variation in life expectancy by income group during this period; and (4) evaluating factors associated with differences in longevity using the variation across areas.

3.2 Methods

This study was approved by the Office of Tax Analysis of the US Treasury under Internal Revenue Code §6103(h)(1). Institutional review board approval was obtained through the Harvard University Committee on the Use of Human Subjects; participant consent was waived because the analysis used preexisting data. The analysis used a de-identified database of federal income tax and Social Security records that includes all individuals with a valid Social Security Number between 1999 and 2014.

Income data were obtained from tax records for every individual for every year from 1999 through 2014. The primary measure of income was pretax household earnings. For those who filed tax returns, household earnings were defined as adjusted gross income plus tax-exempt interest income minus taxable Social Security and disability benefits. For those who did not file a tax return, household earnings were defined as the sum of all wage earnings (reported on form W-2) and unemployment benefits (reported on form 1099-G).

When individuals had no tax return and no information returns, household earnings were \$0. For nonfilers, earnings did not include the spouse's income. However, the vast majority of nonfilers who are not receiving Social Security benefits are single.[16] Income was adjusted to 2012 dollars using the consumer price index.

Mortality was measured using Social Security Administration (SSA) death records. Total deaths in the SSA data closely match data from the National Center for Health Statistics (NCHS), with correlations exceeding 0.96 across ages and years (part I of the eAppendix, eFigure 1, and eTable 1 in the Supplement). Observations with income of \$0 were excluded because the SSA does not fully track deaths of nonresidents and thus mortality rates for individuals with income of \$0 are mismeasured. After excluding observations with income of \$0, individuals were assigned percentile ranks from 1 to 100 based on their household earnings relative to all other individuals of the same sex and age in the United States during each year.

3.2.1 National Levels of Life Expectancy by Income

The study estimated period life expectancy, which was defined as the expected length of life for a hypothetical individual who experiences mortality rates at each subsequent age that match those in the cross-section during a given year. Period life expectancy conditional on income percentile at 40 years of age (or equivalently, expected age at death, calculated as life expectancy + 40) was constructed by (1) estimating mortality rates for the ages of 40 to 76 years; (2) extrapolating mortality rates beyond the age of 76 years and calculating life expectancy; and (3) adjusting for differences in the proportion of racial and ethnic groups across percentiles. A complete description of these 3 steps appears in part II of the eAppendix in the Supplement. The entire analysis was conducted separately for men and women.

For individuals aged 63 years or younger, mortality rates were calculated based on income percentile 2 years earlier. The 2-year lag helps mitigate reverse causality arising from income changes near death.[9] Because of this 2-year lag, mortality rates were available from 2001 through 2014. Mortality rates conditional on income percentile 2 years prior are approximately equivalent to mortality rates conditional on income percentile at the age of 40 years because individuals' earnings are highly correlated over time between the ages of 40 years and 61 years (eFigure 2 and eTable 2 in the Supplement).

Earnings after the age of 62 years are less highly correlated with earnings at earlier ages because the rate of retirement increases sharply at 62 years of age, the earliest age of eligibility for Social Security benefits.[17] Therefore, income for individuals aged 63 years or older was measured at 61 years of age. Because 1999 is the earliest year in which income was observed and the mortality data end in 2014, mortality rates were calculated up to 76 years of age.

Beyond the age of 76 years, mortality rates were estimated using Gompertz models, in which mortality rates increase exponentially with age.[18,19] In a Gompertz model, the logarithm of the mortality rate is linear in age: $\log(m(age)) = \alpha + \beta age$. This Gompertz log-linear approximation fits NCHS data for mortality rates above 40 years of age with R^2 values of greater than 0.99 for both sexes (eFigure 3 in the Supplement). The log-linear approximation also fits mortality rates at specific income percentiles well (for example, $R^2 > 0.99$ at the 5th and 95th percentiles; Figure 3.1A and eFigure 4 in the Supplement).

The Gompertz parameters α (representing the intercept of the Gompertz model) and β (representing the slope) were estimated for each sex, income percentile, and year using maximum likelihood and modeling deaths at each age using a binomial distribution. When pooling all years, mortality rates up to the age of 76 years were used to estimate α and β. When computing year-specific estimates, α and β were estimated using data up to the age of 63 years, so that all years were treated symmetrically. Because the Gompertz model fits less well after the age of 90 years, all survivors at the age of 90 years were assigned sex-specific but income-independent mortality rates based on NCHS and SSA data.[20–22] The mortality rate estimates were used to construct survival curves for each income percentile (Figure 3.1B) and life expectancy was calculated as the area under the survival curve.

The life expectancy estimates were adjusted to control for differences in the racial and ethnic composition of income groups in 2 steps. Data from the National Longitudinal Mortality Study (NLMS) were used first to estimate mortality rates by age for black, Hispanic, and Asian individuals, relative to all other groups using Gompertz models (eFigure 5 in the Supplement). Log differences in mortality rates across races at a given age were assumed to be constant across income groups and areas, an approximation consistent with the NLMS data (eFigures 6 and 7 in the Supplement). US Census data were then used to estimate the share of black, Hispanic, and Asian individuals in each income percentile by sex and year. These data were combined to calculate the mean life expectancy that would prevail if each group had proportions of black, Hispanic, and Asian individuals corresponding to US means at the age of 40 years. In both the NLMS and the US Census, race and ethnicity are reported by individuals based on fixed categories.

3.2.2 National Trends in Life Expectancy by Income

Year-specific estimates of life expectancy were constructed by income quartile and ventile (5 percentile bins) to reduce estimation error. Trends in life expectancy were estimated using linear regressions of race- and ethnicity-adjusted life expectancy in each quartile or ventile by year.

3.2.3 Local Area Variation in Life Expectancy by Income

Individuals' locations were defined based on the zip code from which they filed tax returns or where their W-2 forms were mailed during the year their income was measured for nonfilers. Those individuals who moved after the age of 63 years (ie, after retirement age) were therefore classified as belonging to the location where they lived at the age of 61 years (where they worked).

The level of race- and ethnicity-adjusted life expectancy was estimated by income quartile and ventile for counties, commuting zones, and states, pooling data from 2001 through 2014. Commuting zones are geographic aggregations of counties based on commuting patterns in the 1990 Census that are widely used as measures of local labor markets. There are 741 commuting zones in the United States compared with more than 3 000 counties and more than 40 000 zip codes. The results reported are primarily for commuting zones because these zones constitute broad geographic units analogous to metropolitan statistical areas. However, unlike metropolitan statistical areas, commuting zones provide a complete partition of the country, including rural areas.

The amount of variation in life expectancy across areas was measured as the standard deviation of life expectancy across areas (weighted by population in the 2000 Census) after subtracting the variance across areas due to sampling error. Trends were estimated by regressions of year-specific race- and ethnicity-adjusted life expectancy estimates on calendar year separately in each area. Trend estimates were constructed by income quartile for the 100 most populated commuting zones (with populations >590 000) and for states.

3.2.4 Correlates of Local Area Variation in Life Expectancy

Theories for differences in life expectancy were evaluated by correlating commuting zone–level estimates for individuals in the bottom and top income quartiles with local area characteristics. Detailed definitions of these characteristics and sources appear in part III of the eAppendix and in eTable 3 in the Supplement.

Health behaviors (rates of current smoking, obesity [defined as body mass index {calculated as weight in kilograms divided by height in meters squared} ≥ 30], and exercise during the past month) were measured by income quartile from the 1996 through 2008 Behavioral Risk Factor Surveillance Surveys.

Measures of access to medical care included the fraction uninsured, risk-adjusted Medicare spending per enrollee, an index for the quality of inpatient care based on 30-day hospital mortality rates, and an index for the quality of primary and preventive care based on the fraction of people who visited primary care physicians and received routine care, such as mammograms, constructed using Medicare claims data.[23]

Residential income segregation was measured using the Reardon rank order index; higher numbers indicate greater segregation.[24] Income inequality was estimated with the Gini index using tax records; higher numbers indicate a more unequal income distribution. Social cohesion was estimated using a social capital index based on the methods of Putnam[25] and the share of the population that is religious. The percentage of black individuals was measured in the 2000 Census.

The following measures of local labor market conditions were used as proxies for the strength of local economies: the unemployment rate in 2000, population change between 1980 and 2000, and labor force change between 1980 and 2000.

Several other correlates were constructed using Census data and other sources, for example, population density, the fraction of college graduates, and median house values (a complete list appears in eTable 3 in the Supplement).[26]

3.2.5 Data Analysis and Availability

The raw data were collapsed into means by sex, age, income, year, and geographic area using SAS version 9.1.3 (SAS Institute Inc). The means by sex, age, income, year, and geographic area were analyzed using Stata version 13 (StataCorp). Tests of statistical significance were based on 2-sided tests with a significance threshold of .05. 95% confidence intervals for the race- and ethnicity-adjusted life expectancy estimates were calculated using a bootstrap resampling procedure (part II.E of the eAppendix in the Supplement). Correlation coefficients were calculated using Pearson correlation measures, weighted by population. Data sets containing life expectancy estimates by age, sex, year, and income group at the national, state, commuting zone, and county level are available at www.healthinequality.org.

3.3 Results

The sample consisted of 1 408 287 218 person-year observations from 1999 through 2014. The mean age at which people were analyzed was 53.0 years. Among individuals of working age (38-61 years), the median for household earnings was $61 175 per year and the mean for household earnings was $97 725 per year. Among those aged 40 to 76 years, there were 4 114 380 deaths from the SSA death files among men (mortality rate of 596.3 per 100 000) and 2 694 808 deaths among women (mortality rate of 375.1 per 100 000).

3.3.1 National Levels of Life Expectancy by Income

Figure 3.2 shows race and ethnicity-adjusted expected age at death by household income percentile, using pooled data from 2001-2014. Higher income was associated with longer life at all income levels. Men in the bottom 1% of the income distribution at the age of 40 years had an expected age of death of 72.7 years. Men in the top 1% of the income distribution had an expected age of death of 87.3 years, 14.6 years (95% CI, 14.4-14.8 years) higher than those in the bottom 1%. Women in the bottom 1% of the income distribution at the age of 40 years had an expected age of death of 78.8 years. Women in the top 1% had an expected age of death of 88.9 years, which is 10.1 years (95% CI, 9.9-10.3 years) higher than life expectancy for women in the bottom 1%.

The gap in life expectancy between men and women narrowed with increased income levels. In the bottom 1% of the income distribution, women lived 6.0 years (95% CI, 5.9-6.2 years) longer than men; in the top 1% of the income distribution, women lived only 1.5 years (95% CI, 1.3-1.8 years) longer than men.

The relationship between life expectancy and income percentile was approximately linear above the 2 lowest income percentiles. However, the relationship between life expectancy and dollar income amount was concave (eFigure 8 in the Supplement). That is, an increase in income of a given dollar amount was associated with smaller gains in life expectancy at higher income levels. For example, increases in income from $14k to $20k (the 15th vs. 20th income percentiles), $161k to $224k (the 90th vs. 95th percentiles), and $224k to $1.95 million (the 95th vs. 100th percentiles) were all associated with approximately the same difference in life expectancy (i.e. an increase of 0.7 to 0.9 years, averaging men and women).

Estimates of life expectancy grouping individuals based on individual earnings instead of household earnings were similar, as were estimates that used Gompertz extrapolations up to the age of 100 years instead of the age of 90 years (discussions of these and other sensitivity analyses appear in part IV of the eAppendix and in eFigure 9 in the Supplement).

3.3.2 National Trends in Life Expectancy by Income

The upper panels of Figure 3.3 show race- and ethnicity-adjusted life expectancy for men and women by income quartile for each year from 2001 through 2014. There was a larger increase in life expectancy for higher income groups during the 2000s. For men, the mean annual increase in life expectancy from 2001 through 2014 was 0.20 years in the highest income quartile compared with only 0.08 years in the lowest income quartile ($P < .001$). For women, the comparable changes were 0.23 years in the highest quartile and 0.10 years in the lowest quartile ($P < .001$). These differences persisted after controlling for the higher growth rate of income for individuals in the top quartile relative to the bottom quartile (eTable 4 in the Supplement).

The lower panels of Figure 3.3 show the annual increase in race-adjusted life expectancy by income ventiles. The annual increase in longevity was 0.18 years for men (which translates to an increase of 2.34 years from 2001-2014) and 0.22 years for women (2.91 years from 2001-2014) in the top 5% of the income distribution. In the bottom 5% of the income distribution, the annual increase in longevity was 0.02 years (0.32 years from 2001-2014) for men and 0.003 years for women (0.04 years from 2001-2014) ($P < .001$ for difference between top and bottom 5% of income distributions for both sexes).

3.3.3 Local Area Variation in Life Expectancy by Income

Levels of Life Expectancy by Commuting Zone

Life expectancy varied significantly across areas within the United States, especially for low-income individuals. Figure 3.4 shows life expectancy by income ventile for New York, San Francisco, Dallas, and Detroit. There was substantial variation across these areas for low-income individuals, but little variation for high-income individuals. Life expectancy ranged from 72.3 years to 78.6 years for men in the lowest income ventile across these 4 cities; the corresponding range for men in the top ventile was 86.5 years to 87.5 years.

The results in Figure 3.4 are representative of the variation across CZs more generally. The standard deviation of life expectancy across all commuting zones (weighted by population) was 1.39 years for men in the bottom income quartile vs 0.70 years in the top income quartile ($P < .001$). Life expectancy varied less across areas for women than men in the bottom income quartile, and the amount of variation across commuting zones also declined with income for women (eTable 5 in the Supplement).

Figure 3.5 shows maps of expected age at death by commuting zone for men and women for the bottom and top quartiles of the US income distribution (maps for the middle-income quartiles appear in eFigure 10 in the Supplement). For individuals in the bottom income quartile, life expectancy differed by about 5 years for men and 4 years for women between the lowest and highest longevity commuting zones ($P < .001$ for both sexes). A summary of standard errors by commuting zone appears in part V.C of the eAppendix and in eFigure 11 in the Supplement.

Nevada, Indiana, and Oklahoma had the lowest life expectancies (< 77.9 years) when men and women in the bottom income quartile were averaged. Of the 10 states with the lowest levels of life expectancy for individuals in the bottom income quartile, 8 formed a geographic belt from Michigan to Kansas (Michigan, Ohio, Indiana, Kentucky, Tennessee, Arkansas, Oklahoma, Kansas). The states with the highest life expectancies for individuals in the bottom income quartile (> 80.6 years) were California, New York, and Vermont. Life expectancy in the South was not significantly different from the US mean (−0.22 years, $P = .47$) for women but was lower (−0.96 years, $P = .03$) for men in the bottom income quartile. Individuals in the top income quartile had the lowest life expectancies (< 85.7 years) in Nevada, Hawaii, and Oklahoma and the highest life expectancies (> 87.6 years) in

Utah, Washington, DC, and Vermont.

Table 3.1 lists the top 10 and bottom 10 commuting zones in mean life expectancy (averaging men and women) among the 100 most populated commuting zones for individuals in the bottom and top income quartiles. The expected age at death for the bottom quartile ranged from 74.2 years for men and 80.7 years for women in Gary, Indiana, to 79.5 years for men and 84.0 years for women in New York, New York. The commuting zones with highest life expectancies were clustered in California (6 of the top 10), whereas the commuting zones with the lowest life expectancies were clustered in the industrial Midwest (5 of the bottom 10). The commuting zones with the highest life expectancies for those in the bottom income quartile also had the smallest gaps in life expectancy between the top and bottom quartiles ($r = -0.82$, $P < .001$). The expected age at death for the top income quartile ranged from 82.8 years for men and 85.3 years for women in Las Vegas, Nevada, to 86.6 years for men and 89.0 years for women in Salt Lake City, Utah. The areas with the highest and lowest life expectancies for those in the top income quartile were less clustered geographically; for example, California had commuting zones in both the top 10 and bottom 10 of the list.

The differences in life expectancy across commuting zones were similar in analyses with income measures adjusted for cost of living, controls for differences across areas in the income distribution within each quartile, or measures of loss in life years up to the age of 76 years that did not make use of Gompertz extrapolations (part IV.C of the eAppendix and eTable 6 in the Supplement). There was also considerable variation in life expectancy across counties within CZs (part V of the eAppendix, eFigure 12, and eTable 7 in the Supplement).

Trends in Life Expectancy

Similar to levels of life expectancy, temporal trends varied significantly across geographic areas. Figure 3.6 maps the annual change in life expectancy between 2001 and 2014 by state for men and women in the bottom income quartile in. Hawaii, Maine, and Massachusetts had the largest gains in life expectancy (gaining > 0.19 years annually) when men and women in the bottom income quartile were averaged. The states in which low-income individuals experienced the largest losses in life expectancy (losing > 0.09 years annually) were Alaska, Iowa, and Wyoming.

Table 3.2 lists the top 10 and bottom 10 commuting zones in terms of trends in life expectancy (when averaging men and women) among the 100 most populated CZs for individuals in the bottom and top income quartiles. The estimated trends for individuals in the bottom income quartile ranged from an annual gain of 0.38 years in Toms River, New Jersey, to an annual loss of 0.17 years in Tampa, Florida. Gaps in life expectancy between the bottom and top income quartiles generally declined or remained stable in areas in which the bottom income quartile experienced the largest gains in life expectancy, such as Toms River, New Jersey. In contrast, gaps in life expectancy between the top and bottom income quartiles increased by approximately 0.3 years annually in places such as Tampa, Florida.

Figure 3.7 shows race- and ethnicity-adjusted life expectancies by year for men and women in the bottom income quartile in 2 commuting zones in the top 10 (Birmingham, Alabama, and Cincinnati, Ohio) and 2 commuting zones in the bottom 10 (Knoxville, Tennessee, and Tampa, Florida). This Figure shows that trends in life expectancy across these areas diverged continuously throughout the 2000s. For example, life expectancy increased by approximately 3.2 years from 2001 through 2014 for men and women in Cincinnati, Ohio, but declined by approximately 2.2 years in Tampa, Florida.

3.3.4 Correlates of Local Area Variation in Life Expectancy

Figure 3.8 shows correlations of commuting zone-level estimates of race- and ethnicity-adjusted life expectancy for the bottom income quartile with local area characteristics. The correlations are divided into 6 groups: health behaviors, access to health care, environmental factors, income inequality and social cohesion, local labor market conditions, and other factors. Data for men and women are combined; correlations were similar by sex (eTable 8 in the Supplement). County-level correlations were also similar (eTable 9).

Health Behaviors

Life expectancy was negatively correlated with rates of smoking ($r = -0.69$, $P < .001$) and obesity ($r = -0.47$, $P < .001$) and positively correlated with exercise rates ($r = 0.32$, $P = .004$) among individuals in the bottom income quartile. The maps for rates of smoking, obesity, and exercise for low-income individuals were similar to those for life expectancy (eFigure 13 in the Supplement). Consistent with these findings, the NCHS data show that the majority of the variation in mortality rates across areas among individuals in the bottom income quartile was related to medical causes, such as heart disease and cancer, rather than external causes, such as vehicle crashes, suicide, and homicide (part V.E of the eAppendix and eTable 10 in the Supplement).

Access to Health Care

Measures of health insurance coverage and spending (the fraction of uninsured and risk-adjusted Medicare spending per enrollee) were not significantly associated with life expectancy for individuals in the bottom income quartile. Life expectancy was negatively correlated with hospital mortality rates ($r = -0.31$, $P < .001$), but was not significantly associated with the quality of primary care.

Environmental Factors and Residential Segregation

Theories that posit differences in mortality are driven by the physical environment (eg, exposure to air pollution or a lack of access to healthy food) suggest that the gap in life expectancy between the rich and poor should be larger in more residentially segregated cities. Empirically, in areas where rich and poor are more residentially segregated, differences in life expectancy between individuals in the top and bottom income quartile were smaller ($r = -0.23$, $P = .09$). Individuals in the bottom income quartile who live in more segregated commuting zones had higher levels of life expectancy ($r = 0.26$, $P = .04$).

Income Inequality and Social Cohesion

Life expectancy was not significantly associated with the Gini index of income inequality for individuals in the bottom quartile of the income distribution ($r = 0.20$, $P = .11$). Income inequality was more negatively correlated with life expectancy in the upper income quartiles (for the top quartile, $r = -0.37$, $P < .001$; Figure 3.9 and eFigure 14 in the Supplement). Life expectancy for individuals in the bottom quartile was negatively correlated with the social capital index ($r = -0.26$, $P = .05$) and not significantly associated with religiosity ($r = 0.12$, $P = .39$). There was no significant association between race- and ethnicity-adjusted life expectancy in the bottom income quartile and the fraction of black residents in the commuting zone ($r = -0.06$, $P = .62$).[27]

Local Labor Market Conditions

Unemployment rates, changes in population, and changes in the size of the labor force (all measures of local labor market conditions) were not significantly associated with life expectancy among individuals in the bottom income quartile.

Other Correlates

Associations between life expectancy for the bottom income quartile and 20 other factors were assessed (eTable 8 in the Supplement). The strongest correlates were the local area fraction of immigrants ($r = 0.72$, $P < .001$), median house values ($r = 0.66$, $P < .001$), local government expenditures per capita ($r = 0.57$, $P < .001$), population density ($r = 0.48$, $P < .001$), and the fraction of college graduates ($r = 0.42$, $P < .001$) (Figure 3.8). Population density and the fraction of college graduates were also significantly positively associated with trends in life expectancy across CZs for individuals in the bottom income quartile (eFigure 15 in the Supplement).

Similar to individuals in the bottom income quartile, small area variation in life expectancy for individuals in the top income quartile was strongly correlated with health behaviors (eg, for exercise rates, $r = 0.46$, $P < .001$) (Figure 3.9). Correlations with measures of health care access were more mixed; for example, life expectancy was negatively correlated with Medicare expenditures per capita ($r = -0.50$, $P < .001$) but positively associated with the index of preventive care ($r = 0.55$, $P < .001$). There was no significant correlation with residential segregation. Income inequality was negatively correlated with life expectancy for individuals in the top income quartile ($r = -0.37$, $P < .001$), as was the local unemployment rate ($r = -0.38$, $P < .001$). Among the factors most strongly correlated with life expectancy in the bottom income quartile, the fraction of immigrants was negatively correlated with life expectancy in the top income quartile ($r = -0.21$, $P = .02$), whereas the fraction of college graduates was positively correlated ($r = 0.41$, $P < .001$) with life expectancy.

3.4 Discussion

Addressing socioeconomic disparities in health is a major policy goal.[28–30] Yet the magnitude of socioeconomic gaps in life expectancy, how these gaps are changing over time, and their determinants remain debated. In this study, newly available data covering the US population were used to obtain more comprehensive and precise estimates of the relationship between income and life expectancy at the national level than was feasible in prior work. New local area estimates of life expectancy by income were calculated and factors that were correlated with higher life expectancy for individuals with low incomes were identified. The analysis yielded 4 major conclusions.

3.4.1 National Levels of Life Expectancy by Income

First, life expectancy increased continuously with income. There was no dividing line above or below which higher income was not associated with higher life expectancy. Between the top 1% and bottom 1% of the income distribution, life expectancy differed by 15 years for men and 10 years for women.

These differences are placed in perspective by comparing life expectancies at selected percentiles of the income distribution (among those with positive income) in the United States with mean life expectancies in other countries (eFigure 16 in the Supplement). For example, men in the bottom 1% of the income distribution at the age of 40 years in the United States have life expectancies similar to the mean life expectancy for 40-year-old men in Sudan and Pakistan, assuming that life expectancies in those countries are accurate. Men in the United States in the top 1% of the income distribution have higher life expectancies than the mean life expectancy for men in all countries.[31] The 10-year gap in life expectancy between women in the top 1% and bottom 1% of the US income distribution is equivalent to the decrement in longevity from lifetime smoking.[32]

3.4.2 National Trends in Life Expectancy by Income

Second, inequality in life expectancy increased in recent years. Between 2001 and 2014, individuals in the top 1% of income distribution gained around 3 years of life expectancy, whereas individuals in the bottom 1% experienced no gains. As a benchmark for this magnitude, the NCHS estimates that eliminating all cancer deaths would increase life expectancy at birth by 3.2 years.[33]

This finding of increasing gaps in longevity supports the conclusions of recent studies using smaller samples.[6,7,10,11,14] However, the finding that life expectancy for low-income women has not changed in recent years contrasts with the findings by Olshansky et al[11] that life expectancy has declined for women without a high school degree. The results in this study may differ because the group of people without a high school degree is an increasingly selected sample.[13]

Case and Deaton[34] and Gelman and Auerbach[35] also showed that age-adjusted mortality rates for white men aged 45 to 54 years were constant or increasing during the 2000s. Our finding of increasing life expectancy across most income groups differs from this result because our estimates incorporate declines in mortality rates at older ages, pool all races, and exclude individuals with 0 earnings at the age of 40 years. However, our finding of increasing inequality in life expectancy across income groups is consistent with the findings by Case and Deaton[34] that among whites, mortality rates increased most rapidly for individuals with low levels of education.

3.4.3 Local Area Variation in Life Expectancy by Income

Third, life expectancy varied substantially across local areas. Among individuals in the bottom quartile of the income distribution, life expectancy differed by about 4 years for women and 5 years for men between commuting zones with the lowest and highest longevity. Trends in life expectancy during the 2000s varied substantially across areas as well, ranging from gains of more than 4 years between 2001 and 2014 in some commuting zones to losses of more than 2 years in others. These small area differences suggest that the increasing inequality in health outcomes in the United States as a whole is not immutable.

Prior work documenting geographic variation in longevity has been unable to disaggregate the variability across areas by income.[36–38] Disaggregating by income is important. When pooling all income groups, life expectancy in the South was well below average (eFigure 17 in the Supplement).[36–41] However, among individuals in the bottom income quartile, life expectancy in the South was more similar to the US mean. The low level of life expectancy in the South documented in prior work is explained primarily by lower income levels rather than poorer health conditional on income. Among the population with low-income levels, the lowest life expectancy was found in Oklahoma and in cities in the rust belt, such as Gary, Indiana, and Toledo, Ohio. There was also a substantial difference in life expectancy between low-income individuals in Nevada and Utah, as first documented by Fuchs.[42]

3.4.4 Correlates of Local Area Variation in Life Expectancy

Fourth, the variation in life expectancy across small areas was used as a lens to evaluate theories for socioeconomic differences in longevity. Understanding the characteristics of areas where low-income individuals live longer may yield insights into the determinants of longevity for low-income populations more broadly. The differences in life expectancy across areas were highly correlated with health behaviors (eg, smoking, obesity, and exercise), suggesting that any theory for differences in life expectancy across areas must explain differences in health behaviors.

One such theory is that health and longevity are related to differences in medical care.[43–51] The present analysis provides limited support for this theory. Life expectancy for low income individuals was not significantly correlated with measures of the quantity and quality of medical care provided, such as the fraction insured and measures of preventive care. The lack of a change in the mortality rates of individuals in the lowest income quartile (Figure 3.1) when they become eligible for Medicare coverage at the age of 65 years further supports the conclusion that a lack of access to care is not the primary reason that low-income individuals have shorter life expectancies.[50,51]

A second theory is that physical aspects of the local environment affect health, for example through exposure to air pollution.[52–59] Such theories predict that income gaps in longevity should be greater in areas with greater residential segregation by income. This explanation also does not find strong empirical support. Life expectancy among individuals in the lowest income quartile was higher in more segregated areas—both in absolute terms and relative to individuals in the highest income quartile.

A third theory is that poor health is related to inequality or a lack of social cohesion, which may increase stress for low-income individuals.[60,61] Consistent with prior work, in the current study the Gini index of income inequality was negatively correlated with average life expectancy across commuting zones when pooling all income groups ($r = -0.36$; $P = .002$).[38] However, this correlation is partly driven by areas with more inequality having a larger share of individuals in low-income quartiles, which is associated with lower mean life expectancy because the relationship between income and longevity is concave (eFigure 7 in the Supplement).[62–64] Among individuals in the bottom income quartile, there was no association between inequality and life expectancy across areas, consistent with the findings of Lochner et al[65] based on multilevel data.

Inequality was more negatively correlated with life expectancy for individuals in the highest income quartile, contrary to the prediction that inequality has adverse effects on the health of low-income individuals. At the state level, the correlations between inequality and life expectancy were negative when pooling all income groups, consistent with evidence reviewed by Wilkinson and Pickett,[66] but the correlation with life expectancy in the bottom income quartile was positive. There was also no positive correlation between other measures of social cohesion (ie, social capital and participation in religious organizations) and life expectancy for individuals in the lowest income quartile.[25,67–71]

A fourth theory is that life expectancy is related to local labor market conditions.[72,73] Empirically, neither unemployment nor long-term population and labor force change were significantly associated with life expectancy for individuals in the lowest income quartile.

None of the 4 theories for low levels of life expectancy among low-income individuals were consistently supported by the data. Rather, the strongest pattern in the data was that low-income individuals tend to live longest (and have more healthful behaviors) in affluent cities with highly educated populations and high levels of government expenditures, such as New York, New York, and San Francisco, California. In these cities, life expectancy for individuals in the bottom 5% of the income distribution was approximately 80 years. In contrast, in less affluent cities, such as Gary, Indiana, and Detroit, Michigan, expected age at death for individuals in the bottom 5% of the income distribution was approximately 75 years. Low-income individuals living in cities with highly educated populations also experienced the largest gains in life expectancy during the 2000s.

There are many potential explanations for why low-income individuals who live in affluent, highly educated cities live longer. Such areas may have public policies that restrict smoking or greater funding for public services, consistent with the higher levels of local government expenditures in these areas. Low income individuals who live in high-income areas may also be influenced by living in the vicinity of other individuals who behave in healthier ways. Alternatively, the low-income population in such cities might have different characteristics, consistent with the larger share of immigrants in these areas. Testing between these theories is a key area for future research.

3.4.5 Implications for Practice and Policy

The small area variation in the association between life expectancy and income suggests that gaps in longevity may require local policy responses. For example, health professionals could make targeted efforts to improve health among low-income populations in cities, such as Las Vegas, Nevada, Tulsa, Oklahoma, and Oklahoma City, Oklahoma. The strong association between geographic variation in life expectancy and health behaviors suggests that policy interventions should focus on changing health behaviors among low-income individuals. Tax policies and other local public policies may play a role in inducing such changes. The publicly available data at www.healthinequality.org (listed in eTable 11 in the Supplement) provide a way to monitor local progress.

The findings also have implications for social insurance programs. The differences in life expectancy by income imply that the Social Security program is less redistributive than implied by its progressive benefit structure. Men and women in the top 1% of the income distribution can expect to claim Social Security and Medicare for 11.8 and 8.3 more years than men and women in the bottom 1% of the income distribution. Some have proposed indexing the age of eligibility for Medicare and full Social Security benefits to increases in life expectancy.[74] The differences in the increases in life expectancy across income groups and areas suggests that such a policy would have to be conditioned on income and location to maintain current levels of progressivity.[12]

3.4.6 Limitations

This study has several limitations. First, the life expectancy estimates relied on extrapolations of mortality rates after the age of 76 years (and the age of 63 years for the year-specific estimates). Although the geographic variation remains similar without extrapolating beyond the age of 76 years and the national NCHS data support these extrapolations, further work is needed to ensure their accuracy across income subgroups and geographic areas. The life expectancy estimates by year do not incorporate factors that may have affected mortality rates only after the age of 63 years, such as Medicare Part D in 2006.

Second, the relationships between income and life expectancy should not be interpreted as the causal effects of having more money because income is correlated with other attributes that directly affect health.[75] Because of such unmeasured confounding factors, the causal effects of income on life expectancy are likely to be smaller than the associations documented in this study. In addition, the local area variation need not reflect the causal effects of living in a particular area and may be driven by differences in the characteristics of the residents of each area. Although the correlational analysis in this study cannot establish causal mechanisms, it is a step toward determining which theories for disparities in longevity deserve further consideration.

Third, some of the measures used (eg, the percentage of religiosity to represent social cohesion) are constructed based on limited empirical data. However, we are unaware of better measures that could have been used as proxies for the various constructs of interest.

3.5 Conclusion

In the United States between 2001 and 2014, higher income was associated with greater longevity throughout the income distribution, and differences in life expectancy across income groups increased during this period. However, the association between life expectancy and income varied substantially across geographic areas; differences in longevity across income groups decreased in some areas and increased in others. The differences in life expectancy were correlated with health behaviors and local area characteristics.

Box: Key Messages

- Life expectancy increases continuously with income. At the age of 40 years, the gap in life expectancy between individuals in the top and bottom 1% of the income distribution in the United States is 15 years for men and 10 years for women.

- For individuals in the bottom income quartile, life expectancy at the age of 40 years differs by about 4.5 years between the commuting zones with the highest and lowest life expectancies. Adjusting for race and ethnicity, life expectancy for individuals with low incomes is lowest in Nevada, Indiana, and Oklahoma and highest in California, New York, and Vermont.

- Gaps in life expectancy by income increased between 2001 and 2014. Life expectancy did not change for individuals in the lowest quartile of the income distribution, whereas it increased by about 3 years for men and women in the top quartile of the income distribution. These changes varied significantly across geographic areas. The gap in life expectancy between the lowest and highest income quartiles decreased in some areas, such as areas within New Jersey and Alabama, but increased by more than 3 years in other areas, such as areas within Florida.

- Correlational analysis of the differences in life expectancy across geographic areas did not provide strong support for 4 leading explanations for socioeconomic differences in longevity: differences in access to medical care (as measured by health insurance coverage and proxies for the quality and quantity of primary care), environmental differences (as measured by residential segregation), adverse effects of inequality (as measured by Gini indices), and labor market conditions (as measured by unemployment rates). Rather, most of the variation in life expectancy across geographic areas was related to differences in health behaviors, including smoking, obesity, and exercise. Individuals in the lowest income quartile have more healthful behaviors and live longer in areas with more immigrants, higher home prices, and more college graduates.

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Tables and Figures

Table 3.1: Race- and Ethnicity-Adjusted Expected Age at Death by Commuting Zone and Income Quartile, 2001-2014

Race- and Ethnicity-Adjusted Expected Age at Death in Years, Bottom Income Quartile					
Rank	Commuting Zone	Bottom Income Quartile, Mean	Bottom Income Quartile, Men	Bottom Income Quartile, Women	Diff. Between Top and Bottom Income Quartiles, Mean
1	New York City, NY	81.8 (81.6, 82.0)	79.5 (79.3, 79.8)	84.0 (83.7, 84.4)	4.8 (4.5, 5.0)
2	Santa Barbara, CA	81.7 (81.3, 82.1)	79.4 (78.9, 79.9)	84.0 (83.4, 84.6)	5.8 (5.3, 6.4)
3	San Jose, CA	81.6 (81.2, 82.0)	79.5 (79.0, 79.9)	83.7 (83.1, 84.3)	4.7 (4.3, 5.0)
4	Miami, FL	81.2 (80.9, 81.6)	78.3 (77.8, 78.7)	84.2 (83.7, 84.8)	4.2 (3.9, 4.5)
5	Los Angeles, CA	81.1 (80.9, 81.4)	79.0 (78.7, 79.3)	83.2 (82.8, 83.6)	4.7 (4.5, 4.9)
6	San Diego, CA	81.1 (80.8, 81.4)	78.8 (78.5, 79.1)	83.4 (83.0, 83.8)	5.3 (5.0, 5.6)
7	San Francisco, CA	80.9 (80.6, 81.3)	78.8 (78.4, 79.2)	83.0 (82.5, 83.7)	5.2 (5.0, 5.4)
8	Santa Rosa, CA	80.8 (80.5, 81.2)	79.0 (78.6, 79.5)	82.6 (82.1, 83.1)	6.1 (5.6, 6.6)
9	Newark, NJ	80.7 (80.5, 80.9)	78.2 (78.0, 78.4)	83.2 (83.0, 83.6)	5.6 (5.3, 5.8)
10	Port St. Lucie, FL	80.7 (80.5, 80.9)	78.0 (77.8, 78.3)	83.3 (83.1, 83.7)	6.2 (5.9, 6.5)
...	*US Mean*	79.4 (79.4, 79.5)	76.7 (76.7, 76.8)	82.1 (82.1, 82.2)	7.0 (6.9, 7.1)
91	San Antonio, TX	78.0 (77.6, 78.4)	75.2 (74.7, 75.7)	80.8 (80.1, 81.5)	7.9 (7.4, 8.4)
92	Louisville, KY	77.9 (77.7, 78.2)	74.9 (74.6, 75.3)	80.9 (80.5, 81.3)	8.4 (8.0, 8.8)
93	Toledo, OH	77.9 (77.6, 78.2)	74.9 (74.6, 75.4)	80.8 (80.3, 81.3)	8.0 (7.5, 8.4)
94	Cincinnati, OH	77.9 (77.7, 78.1)	75.2 (74.9, 75.5)	80.5 (80.2, 80.9)	8.4 (8.0, 8.8)
95	Detroit, MI	77.7 (77.5, 77.8)	74.8 (74.6, 75.0)	80.5 (80.3, 80.8)	8.2 (8.0, 8.4)
96	Tulsa, OK	77.6 (77.4, 77.9)	74.9 (74.6, 75.3)	80.3 (79.9, 80.7)	8.2 (7.7, 8.6)
97	Indianapolis, IN	77.6 (77.4, 77.8)	74.6 (74.3, 75.0)	80.6 (80.2, 80.9)	8.5 (8.1, 8.8)
98	Oklahoma City, OK	77.6 (77.3, 77.8)	75.0 (74.7, 75.3)	80.2 (79.8, 80.5)	8.3 (7.9, 8.7)
99	Las Vegas, NV	77.6 (77.4, 77.8)	75.1 (74.9, 75.3)	80.0 (79.7, 80.3)	6.5 (6.2, 6.8)
100	Gary, IN	77.4 (77.1, 77.8)	74.2 (73.8, 74.6)	80.7 (80.2, 81.2)	7.2 (6.7, 7.8)
Race- and Ethnicity-Adjusted Expected Age at Death in Years, Top Income Quartile					
Rank	Commuting Zone	Top Income Quartile, Mean	Top Income Quartile, Men	Top Income Quartile, Women	Diff. Between Top and Bottom Income Quartiles, Mean
1	Salt Lake City, UT	87.8 (87.5, 88.1)	86.6 (86.2, 87.0)	89.0 (88.6, 89.4)	8.3 (7.9, 8.7)
2	Portland, ME	87.8 (87.3, 88.2)	86.8 (86.3, 87.5)	88.7 (88.0, 89.4)	7.4 (6.8, 7.9)
3	Spokane, WA	87.7 (87.2, 88.1)	86.1 (85.4, 86.8)	89.2 (88.7, 89.9)	7.7 (7.2, 8.3)
4	Santa Barbara, CA	87.5 (87.2, 87.9)	86.3 (85.8, 86.8)	88.7 (88.2, 89.3)	5.8 (5.3, 6.4)
5	Denver, CO	87.5 (87.3, 87.7)	86.6 (86.3, 86.9)	88.4 (88.1, 88.7)	7.9 (7.6, 8.2)
6	Minneapolis, MN	87.3 (87.1, 87.5)	86.4 (86.1, 86.7)	88.2 (88.0, 88.5)	7.7 (7.4, 8.0)
7	Grand Rapids, MI	87.3 (87.0, 87.6)	86.2 (85.7, 86.7)	88.4 (87.9, 88.9)	8.1 (7.7, 8.5)
8	Madison, WI	87.2 (86.8, 87.7)	86.1 (85.5, 86.7)	88.4 (87.9, 89.0)	8.1 (7.5, 8.7)
9	Eugene, OR	87.2 (86.9, 87.6)	86.3 (85.8, 86.9)	88.2 (87.7, 88.8)	7.3 (6.8, 7.8)
10	Springfield, MA	87.2 (86.8, 87.7)	86.3 (85.8, 86.9)	88.1 (87.5, 88.8)	7.2 (6.6, 7.9)
...	*US Mean*	86.4 (86.3, 86.5)	85.3 (85.2, 85.4)	87.5 (87.4, 87.6)	7.0 (6.9, 7.1)
91	Youngstown, OH	85.8 (85.3, 86.3)	84.6 (84.0, 85.3)	86.9 (86.2, 87.7)	6.7 (6.2, 7.3)
92	Los Angeles, CA	85.8 (85.5, 86.0)	84.9 (84.7, 85.2)	86.6 (86.2, 87.0)	4.7 (4.5, 4.9)
93	Lakeland, FL	85.8 (85.2, 86.3)	84.2 (83.4, 85.0)	87.3 (86.6, 88.2)	6.7 (6.1, 7.3)
94	Miami, FL	85.4 (85.1, 85.7)	84.3 (83.8, 84.7)	86.6 (86.1, 87.1)	4.2 (3.9, 4.5)
95	Bakersfield, CA	85.0 (84.5, 85.5)	84.1 (83.4, 84.8)	86.0 (85.2, 86.8)	6.1 (5.5, 6.8)
96	El Paso, TX	85.0 (84.4, 85.7)	83.2 (82.3, 84.2)	86.7 (85.9, 87.7)	5.9 (5.1, 6.7)
97	Brownsville, TX	84.8 (84.1, 85.7)	83.4 (82.4, 84.5)	86.3 (85.3, 87.6)	4.8 (3.9, 5.7)
98	Honolulu, HI	84.8 (83.8, 86.0)	84.2 (83.0, 85.5)	85.3 (83.8, 87.3)	6.6 (6.1, 7.2)
99	Gary, IN	84.6 (84.2, 85.1)	83.1 (82.5, 83.7)	86.1 (85.5, 86.8)	7.2 (6.7, 7.8)
100	Las Vegas, NV	84.1 (83.8, 84.4)	82.8 (82.4, 83.2)	85.3 (84.9, 85.8)	6.5 (6.2, 6.8)

Notes: Table shows levels of expected age at death for individuals in bottom income quartile (upper panel) and top income quartile (lower panel) of the national income distribution. Estimates are shown for CZs with the highest and lowest life expected age at death among the 100 most populous CZs. Column 1 reports means across genders; columns 2 and 3 report estimates by gender; column 4 reports longevity gaps (top income quartile minus bottom income quartile), pooling genders. 95% confidence intervals are shown in parentheses.

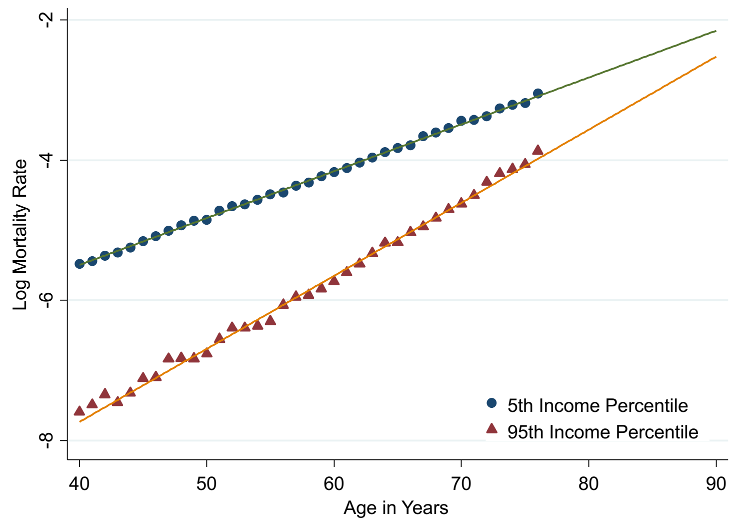
Table 3.2: Trends in Race- and Ethnicity-Adjusted Expected Age at Death by Commuting Zone and Income Quartile, 2001-2014

Annual Trend in Race- and Ethnicity-Adjusted Expected Age at Death in Years, Bottom Income Quartile					
Rank	Commuting Zone	Bottom Income Quartile, Mean	Bottom Income Quartile, Men	Bottom Income Quartile, Women	Diff. Between Top and Bottom Income Quartiles, Mean
1	Toms River, NJ	0.38 (0.24, 0.52)	0.45 (0.29, 0.63)	0.30 (0.08, 0.52)	-0.15 (-0.34, 0.05)
2	Birmingham, AL	0.29 (0.18, 0.41)	0.20 (0.07, 0.35)	0.37 (0.20, 0.55)	-0.43 (-0.66, -0.20)
3	Richmond, VA	0.26 (0.13, 0.39)	0.26 (0.11, 0.42)	0.26 (0.06, 0.45)	0.09 (-0.11, 0.32)
4	Syracuse, NY	0.25 (0.11, 0.40)	0.28 (0.13, 0.47)	0.21 (-0.01, 0.45)	-0.12 (-0.38, 0.14)
5	Cincinnati, OH	0.24 (0.15, 0.34)	0.27 (0.16, 0.39)	0.21 (0.07, 0.37)	0.09 (-0.08, 0.28)
6	Fayetteville, NC	0.24 (0.10, 0.38)	0.09 (-0.08, 0.25)	0.39 (0.19, 0.61)	-0.51 (-0.84, -0.20)
7	Springfield, MA	0.23 (0.06, 0.41)	0.22 (-0.00, 0.43)	0.25 (0.00, 0.53)	-0.12 (-0.42, 0.18)
8	Gary, IN	0.22 (0.08, 0.38)	0.24 (0.07, 0.41)	0.21 (-0.04, 0.46)	0.17 (-0.09, 0.49)
9	Scranton, PA	0.21 (0.08, 0.34)	0.10 (-0.04, 0.25)	0.32 (0.11, 0.54)	-0.03 (-0.28, 0.22)
10	Honolulu, HI	0.21 (0.05, 0.38)	0.04 (-0.17, 0.24)	0.38 (0.12, 0.66)	-0.18 (-0.50, 0.11)
...	*US Mean*	0.09 (0.07, 0.11)	0.08 (0.05, 0.11)	0.10 (0.06, 0.13)	0.13 (0.10, 0.16)
91	Cape Coral, FL	-0.07 (-0.21, 0.06)	0.05 (-0.13, 0.21)	-0.19 (-0.41, 0.02)	0.26 (0.01, 0.54)
92	Miami, FL	-0.07 (-0.14, -0.01)	-0.08 (-0.17, -0.01)	-0.06 (-0.16, 0.03)	0.39 (0.25, 0.54)
93	Tucson, AZ	-0.07 (-0.20, 0.05)	-0.08 (-0.24, 0.08)	-0.07 (-0.26, 0.13)	0.23 (-0.00, 0.50)
94	Albuquerque, NM	-0.08 (-0.22, 0.06)	-0.13 (-0.30, 0.05)	-0.03 (-0.26, 0.21)	0.20 (-0.08, 0.47)
95	Sarasota, FL	-0.08 (-0.20, 0.03)	-0.09 (-0.25, 0.06)	-0.08 (-0.26, 0.09)	0.27 (0.05, 0.51)
96	Des Moines, IA	-0.10 (-0.30, 0.08)	-0.02 (-0.25, 0.20)	-0.19 (-0.53, 0.08)	0.41 (0.11, 0.75)
97	Bakersfield, CA	-0.12 (-0.28, 0.03)	-0.22 (-0.42, -0.02)	-0.02 (-0.27, 0.21)	-0.01 (-0.33, 0.29)
98	Knoxville, TN	-0.12 (-0.26, 0.01)	-0.13 (-0.29, 0.03)	-0.11 (-0.33, 0.09)	0.23 (-0.01, 0.48)
99	Pensacola, FL	-0.15 (-0.30, -0.02)	-0.16 (-0.38, 0.02)	-0.15 (-0.40, 0.08)	0.41 (0.13, 0.70)
100	Tampa, FL	-0.17 (-0.25, -0.09)	-0.16 (-0.25, -0.07)	-0.18 (-0.30, -0.06)	0.28 (0.11, 0.46)
Annual Trend in Race- and Ethnicity-Adjusted Expected Age at Death in Years, Top Income Quartile					
Rank	Commuting Zone	Top Income Quartile, Mean	Top Income Quartile, Men	Top Income Quartile, Women	Diff. Between Top and Bottom Income Quartiles, Mean
1	El Paso, TX	0.48 (0.18, 0.84)	0.23 (-0.18, 0.66)	0.73 (0.33, 1.24)	0.50 (0.18, 0.89)
2	Poughkeepsie, NY	0.44 (0.27, 0.63)	0.35 (0.11, 0.62)	0.52 (0.28, 0.80)	0.31 (0.08, 0.58)
3	Gary, IN	0.40 (0.17, 0.67)	0.40 (0.12, 0.76)	0.39 (0.03, 0.75)	0.17 (-0.09, 0.49)
4	Portland, ME	0.39 (0.14, 0.65)	0.32 (0.02, 0.63)	0.45 (0.06, 0.89)	0.19 (-0.11, 0.52)
5	Youngstown, OH	0.38 (0.14, 0.70)	0.09 (-0.25, 0.45)	0.67 (0.33, 1.14)	0.33 (0.05, 0.67)
6	Buffalo, NY	0.38 (0.25, 0.51)	0.41 (0.22, 0.59)	0.35 (0.14, 0.54)	0.27 (0.11, 0.44)
7	Manchester, NH	0.36 (0.21, 0.55)	0.32 (0.11, 0.54)	0.41 (0.18, 0.67)	0.22 (0.03, 0.44)
8	Richmond, VA	0.35 (0.19, 0.54)	0.45 (0.22, 0.70)	0.24 (-0.02, 0.54)	0.09 (-0.11, 0.32)
9	Cincinnati, OH	0.34 (0.20, 0.49)	0.32 (0.14, 0.55)	0.35 (0.15, 0.56)	0.09 (-0.08, 0.28)
10	Chicago, IL	0.33 (0.26, 0.41)	0.27 (0.17, 0.36)	0.40 (0.30, 0.50)	0.19 (0.10, 0.28)
...	*US Mean*	0.22 (0.19, 0.24)	0.20 (0.17, 0.24)	0.23 (0.20, 0.25)	0.13 (0.10, 0.16)
91	Baton Rouge, LA	0.05 (-0.16, 0.24)	0.22 (-0.09, 0.51)	-0.12 (-0.41, 0.17)	0.09 (-0.16, 0.32)
92	Santa Rosa, CA	0.03 (-0.22, 0.27)	0.00 (-0.32, 0.32)	0.06 (-0.29, 0.43)	-0.05 (-0.36, 0.26)
93	Honolulu, HI	0.02 (-0.22, 0.27)	-0.03 (-0.32, 0.29)	0.08 (-0.29, 0.46)	-0.18 (-0.50, 0.11)
94	Salt Lake City, UT	0.01 (-0.13, 0.14)	-0.04 (-0.24, 0.15)	0.07 (-0.13, 0.24)	-0.14 (-0.33, 0.04)
95	Erie, PA	-0.00 (-0.35, 0.31)	0.26 (-0.18, 0.78)	-0.27 (-0.85, 0.09)	0.03 (-0.39, 0.36)
96	Rockford, IL	-0.03 (-0.33, 0.26)	0.06 (-0.25, 0.43)	-0.13 (-0.56, 0.33)	-0.06 (-0.37, 0.29)
97	Bakersfield, CA	-0.13 (-0.42, 0.12)	-0.18 (-0.57, 0.18)	-0.08 (-0.50, 0.30)	-0.01 (-0.33, 0.29)
98	Birmingham, AL	-0.15 (-0.34, 0.05)	-0.10 (-0.37, 0.16)	-0.19 (-0.47, 0.12)	-0.43 (-0.66, -0.20)
99	Fayetteville, NC	-0.27 (-0.57, -0.00)	-0.37 (-0.70, 0.01)	-0.18 (-0.63, 0.21)	-0.51 (-0.84, -0.20)
100	Lakeland, FL	-0.28 (-0.61, 0.00)	-0.33 (-0.79, -0.01)	-0.23 (-0.68, 0.22)	-0.29 (-0.64, 0.00)

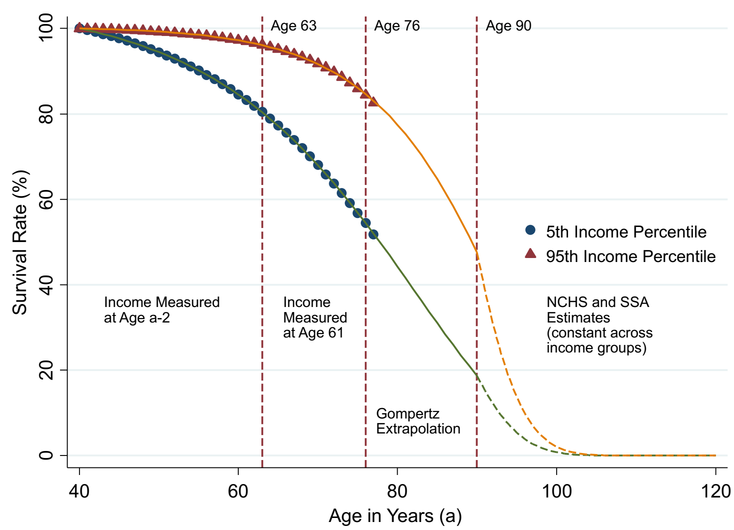
Notes: Table shows estimated annual trends in expected age at death for individuals in bottom income quartile (upper panel) and top income quartile (lower panel) of the national income distribution. Estimates are shown for CZs with the highest and lowest life trends among the 100 most populous CZs. Column 1 reports means across genders; columns 2 and 3 report estimates by gender; column 4 reports longevity gaps (top income quartile minus bottom income quartile), pooling genders. 95% confidence intervals are shown in parentheses.

Figure 3.1: Gompertz Approximations and Empirical Survival Curves for Men at 5th and 95th Income Percentiles, 2001–2014

(A) Log Mortality Rates

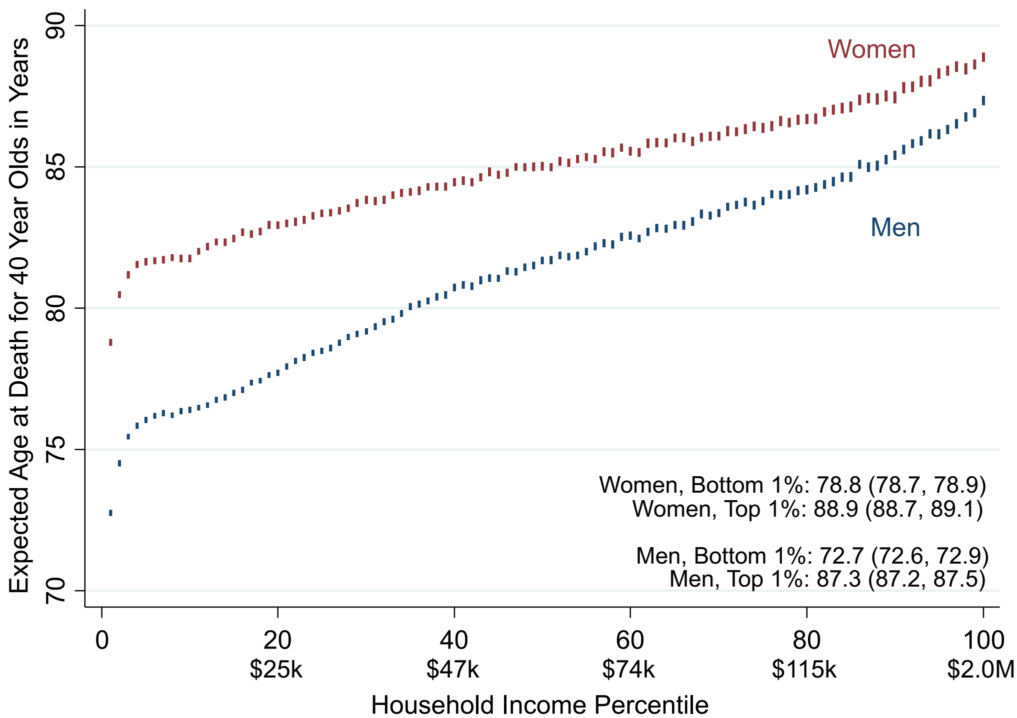


(B) Survival Curves



Notes: For panels A and B, the data for the scatter points were derived from cross-sectional mortality rates by age using income 2 years prior for men aged 40 to 62 years and cohort mortality rates by year using income observed at age 61 years for men aged 63 to 76 years. Empirical mortality rates were observed until the age of 76 years; therefore, empirical survival rates are observed until the age of 77 years. Solid lines show Gompertz extrapolations through the age of 90 years. In panel B, uniform mortality rates from the National Center for Health Statistics (NCHS) and the Social Security Administration (SSA) were used beyond the age of 90 years. Analogous results for women appear in eFigure 4 in the Supplement.

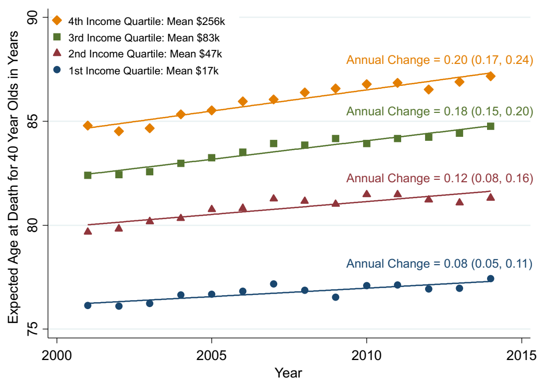
Figure 3.2: Race- and Ethnicity-Adjusted Life Expectancy for 40-Year-Olds by Household Income Percentile, 2001–2014



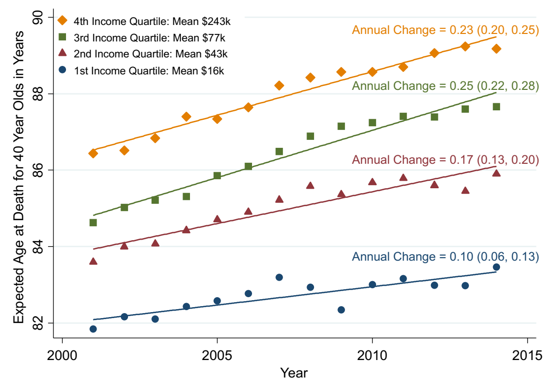
Notes: Life expectancies were calculated using survival curves analogous to those in Figure 3.1. The vertical height of each bar depicts the 95% confidence interval. The difference between expected age at death in the top and bottom income percentiles is 10.1 years (95% CI, 9.9–10.3 years) for women and 14.6 years (95% CI, 14.4–14.8 years) for men. To control for differences in life expectancies across racial and ethnic groups, race and ethnicity adjustments were calculated using data from the National Longitudinal Mortality Survey and estimates were reweighted so that each income percentile bin has the same fraction of black, Hispanic, and Asian adults.

Figure 3.3: Changes in Race- and Ethnicity-Adjusted Life Expectancy by Income Group, 2001–2014

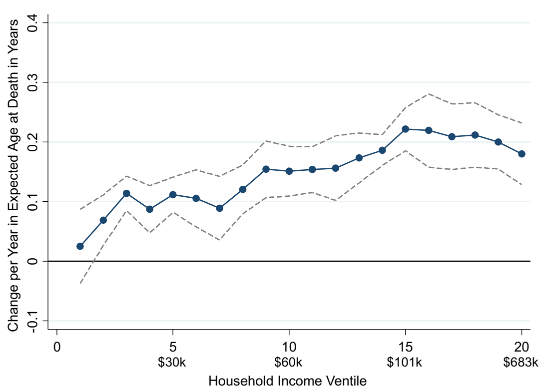
(A) Life Expectancy by Income Quartile by Year, Men



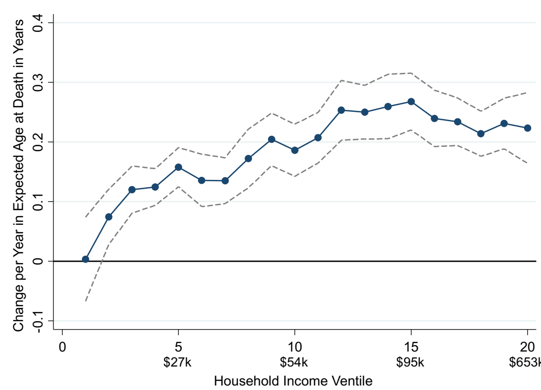
(B) Life Expectancy by Income Quartile by Year, Women



(C) Average Annual Change in Life Expectancy by Income Ventile from 2001-14, Men



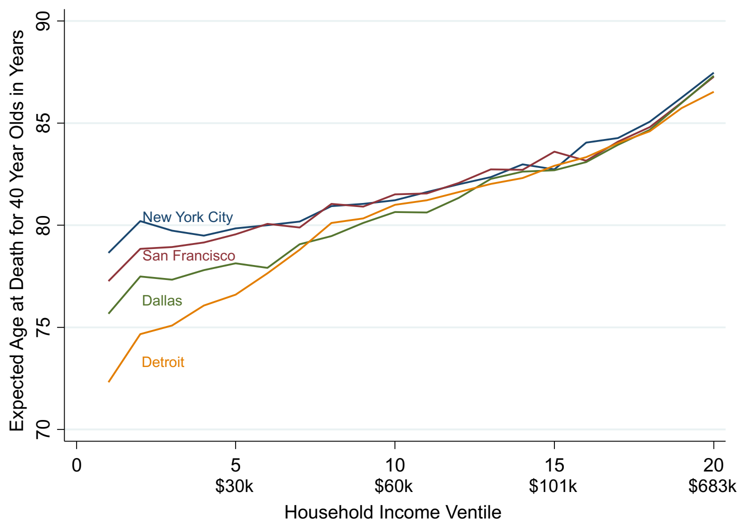
(D) Average Annual Change in Life Expectancy by Income Ventile from 2001-14, Women



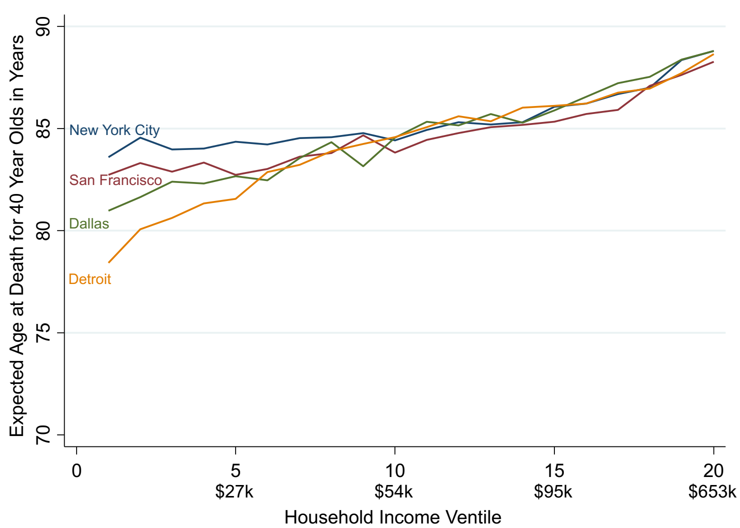
Notes: Scatter points in panels A and B show the race- and ethnicity-adjusted life expectancy estimates by year and household income quartile. Solid lines represent best fit lines estimated using ordinary least-squares regression. Panels C and D plot the slopes from analogous regressions estimated separately by income ventile (5 percentile point bins). Dashed lines show 95% confidence intervals.

Figure 3.4: Race- and Ethnicity-Adjusted Life Expectancy by Income Ventile in Selected Commuting Zones, 2001–2014

(A) Men



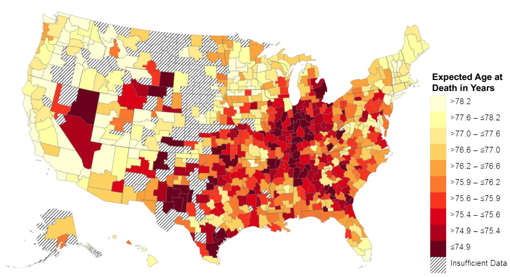
(B) Women



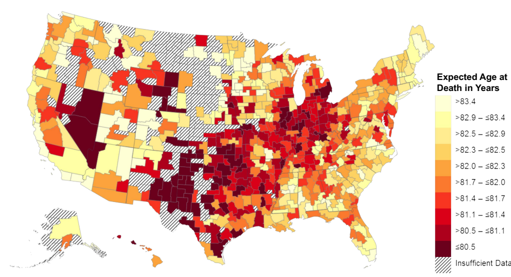
Notes: Estimates of race- and ethnicity-adjusted expected age at death for 40-year-olds computed by income ventile (5 percentile point bins).

Figure 3.5: Race- and Ethnicity-Adjusted Life Expectancy by Commuting Zone and Income Quartile, 2001–2014

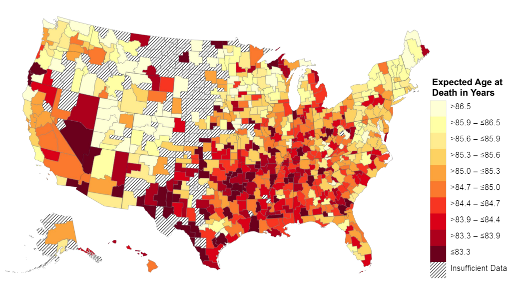
(A) Men, Bottom Quartile



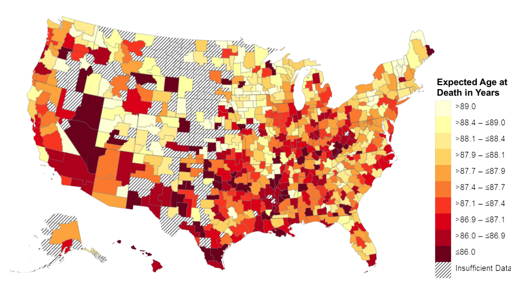
(B) Women, Bottom Quartile



(C) Men, Top Quartile



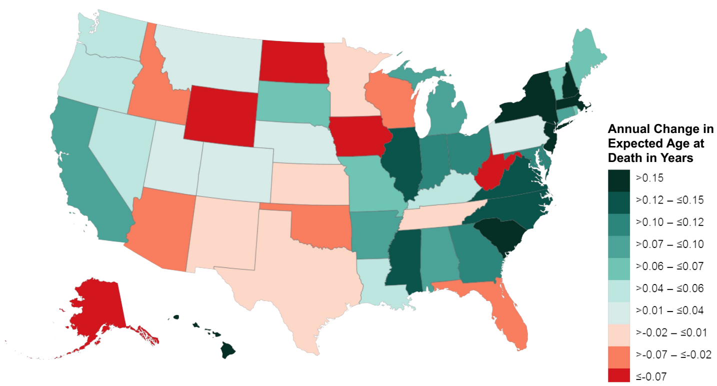
(D) Women, Top Quartile



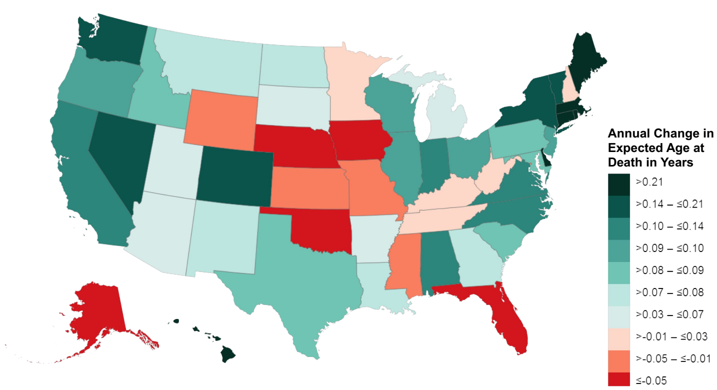
Notes: Estimates of race- and ethnicity-adjusted expected age at death for 40-year-olds computed by commuting zone. The 595 commuting zones with populations above 25 000 are grouped into deciles and colored from dark to light as expected age at death increases. The second and third quartiles appear in eFigure 10 in the Supplement.

Figure 3.6: Maps of Annual Change in Life Expectancy by State for Bottom Income Quartile, 2001–2014

(A) Men, Bottom Quartile

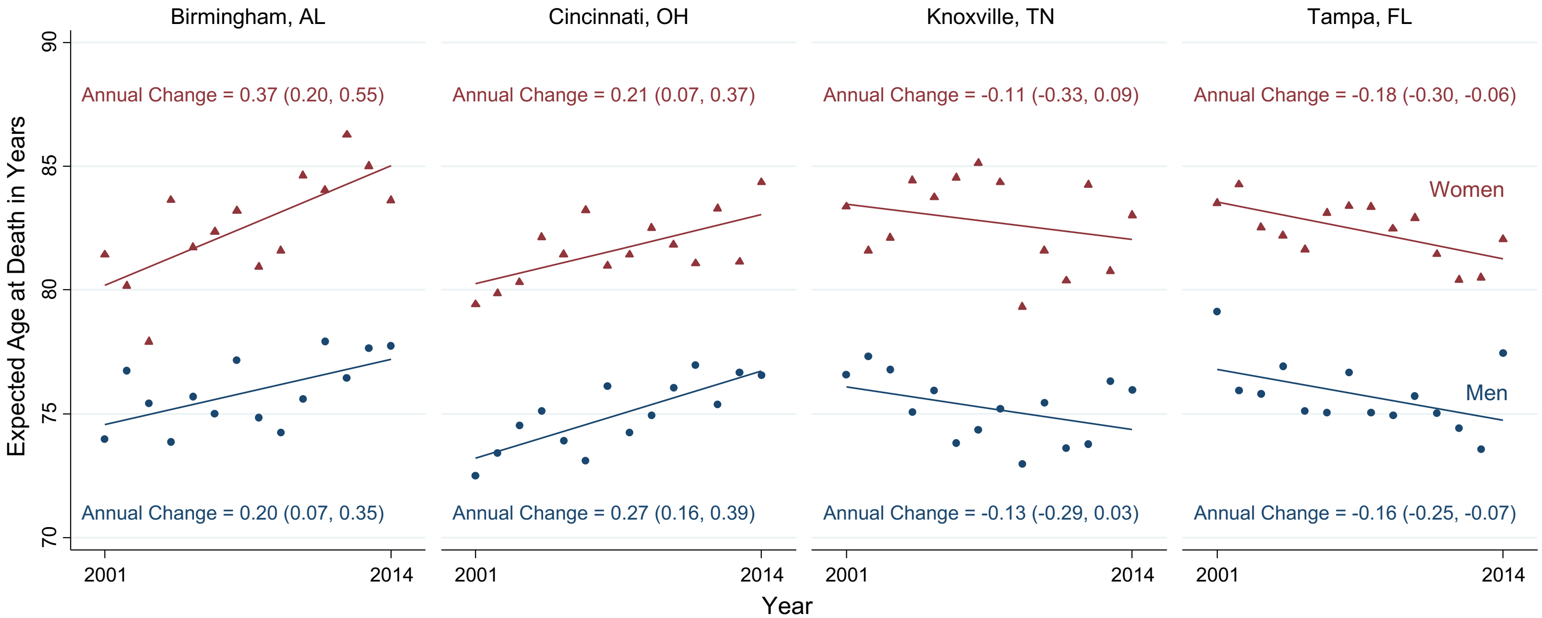


(B) Women, Bottom Quartile



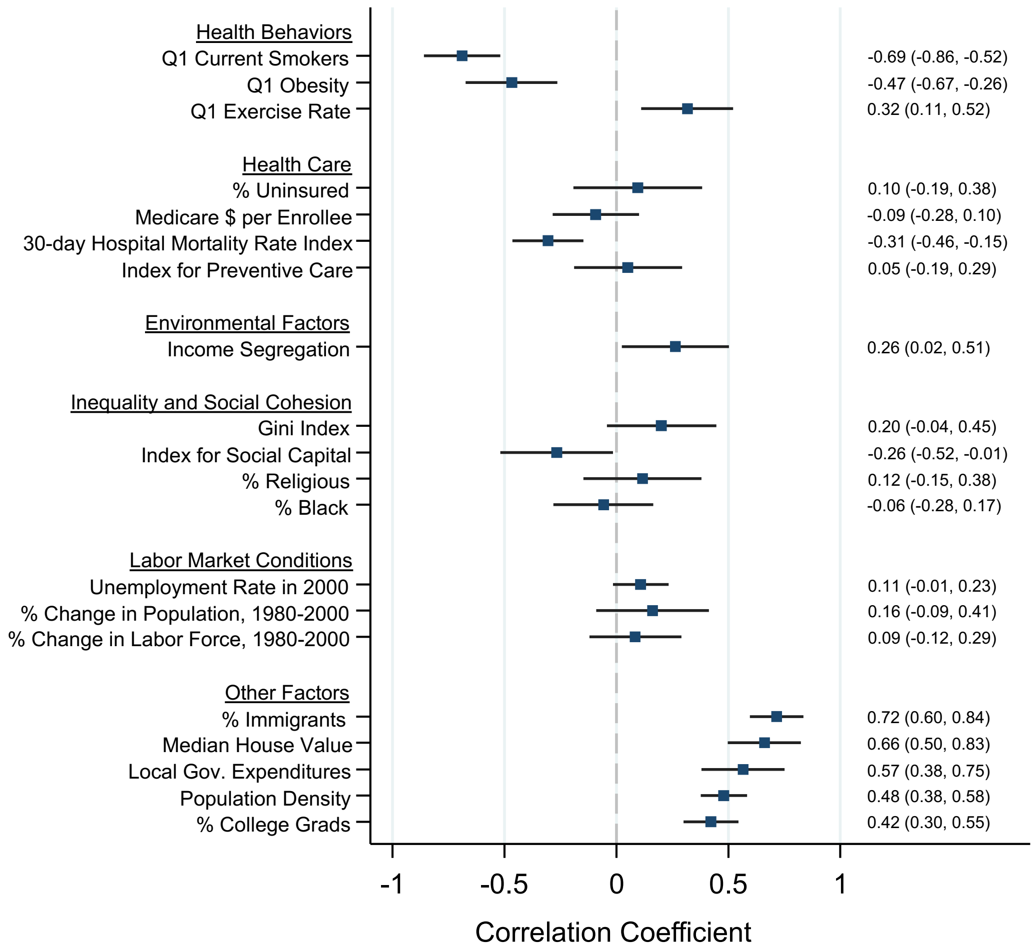
Notes: Annual changes estimated using ordinary least-squares regression of race- and ethnicity-adjusted expected age at death for 40-year-olds on calendar year by state. States are grouped into deciles and colored from red to turquoise as annual change in expected age at death increases.

Figure 3.7: Annual Change in Life Expectancy for Individuals in the Bottom Income Quartile Living in Selected Commuting Zones, 2001–2014



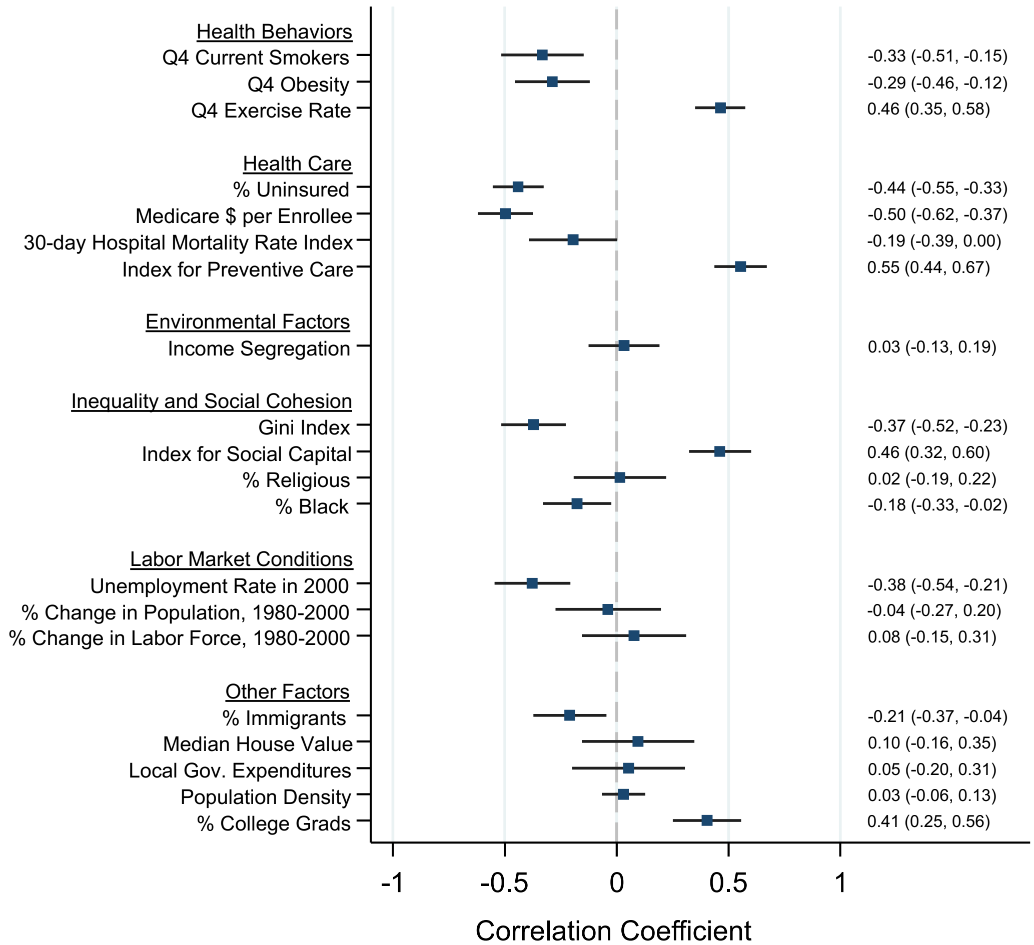
Notes: Solid lines indicate best linear fit, estimated using ordinary least-squares regression.

Figure 3.8: Correlations Between Life Expectancy in the Bottom Income Quartile and Local Area Characteristics, 2001–2014



Notes: Population-weighted univariate Pearson correlations estimated between local area characteristics and race- and ethnicity-adjusted expected age at death for 40-year-olds in the bottom income quartile. These correlations were computed at the commuting zone level after averaging life expectancy across sexes. The error bars indicate 95% confidence intervals with errors clustered by state. Definitions and sources of all variables appear in eTable 3 in the Supplement.

Figure 3.9: Correlations Between Life Expectancy in the Top Income Quartile and Local Area Characteristics, 2001–2014



Notes: Population-weighted univariate Pearson correlations estimated between local area characteristics and race- and ethnicity-adjusted expected age at death for 40-year-olds in the top income quartile. These correlations were computed at the commuting zone level after averaging life expectancy across sexes. The error bars indicate 95% confidence intervals with errors clustered by state. Definitions and sources of all variables appear in eTable 3 in the Supplement.