Health Shocks, Education, and Labor Market Outcomes*

Vincent Pohl[†] Queen's University

Christopher Neilson New York University

Francisco Parro Universidad Adolfo Ibanez

November 2014

Abstract

The effects of health and education on labor market outcomes have received much attention. In this paper, we contribute to this literature by analyzing two particular roles of education. First, we measure how it affects the frequency and severity of health shocks, and second, we estimate how labor market effects of health shocks vary by educational attainment. We answer these questions using administrative earnings data and hospital records from Chile and find that education has a strong protective effect in this context. Moreover, differences in health shock characteristics and work conditions only explain a part of this education gradient.

*We thank Daniel Avdic, Prashant Bharadwaj, and seminar and conference participants at AEA, ATINER, CEA, CHESG, ECHE, iHEA, McGill, McMaster, LMU Munich, SOLE, and Toronto for helpful comments. Pohl gratefully acknowledges financial support from the W.E. Upjohn Institute for Employment Research and the Queen's University Principal's Development Fund. Loreto Reyes provided research assistance.

†Industrial Relations Program and Economics Department, email: vincent.pohl@queensu.ca.

1 Introduction

There is ample evidence that education, health, and labor market outcomes are correlated. Overall, economists agree on the positive effects of education on earnings and health (Card, 1999; Lochner, 2011) and of health on labor market outcomes (Currie and Madrian, 1999), but the exact causal channels are not completely understood. In a recent study, Heckman et al. (2014) use rich panel data including personality traits to analyze one of these channels, the causal effect of education on health status, health behavior, and wages. Understanding other potential pathways is important for policymakers interested in the causal effects of education and health on labor market outcomes. If there is such an effect, it may be possible to devise policies to reduce the cost imposed by health-related absenteeism. Moreover, if higher levels of education lead to a reduction in the negative effect of health on employment and earnings, it may be optimal to increase school enrollment, particularly in countries where educational attainment is low.

In this paper, we analyze another channel for the benefits of education. Instead of modeling education decisions as in Heckman et al. (2014), we take the final education level of adults as given, but rather focus on the role of health. While most existing studies only observe measures of health status and treat changes in health as a black box, we proxy changes in health status by sudden health events that require hospitalization. We then show how education affects 1) the likelihood, type, and severity of such health shocks and 2) the way in which health shocks result in changes in labor market outcomes. Exploiting detailed administrative earnings and hospital data from Chile, we use propensity score weighting combined with difference-in-differences and fixed effects strategies to uncover the role of education in the labor market effects of health shocks. While we do not directly estimate causal effects of education on health or labor market outcomes, our estimates show how the causal effect of health shocks on labor market outcomes varies by educational attainment.

Education may affect the frequency of adverse health events such as accidents, heart attacks, or cancer diagnoses for two main reasons. First, better educated individuals tend to engage in fewer unhealthy behaviors such as smoking (e.g., Grimard and Parent, 2007; de Walque, 2007; Jürges, Reinhold, and Salm, 2011), behaviors leading to obesity (e.g., Webbink, Martin, and Visscher, 2010; Brunello, Fabbri, and Fort, 2013), binge drinking (e.g., Naimi et al., 2003), and risky driving or not wearing a seatbelt (e.g., Leigh, 1990).[1] Second, individuals with higher levels of education possess the necessary qualifications to work in white-collar occupations (e.g., Autor and Handel, 2013; Speer, 2014). Occupations that require more manual tasks are associated with adverse health events, including workplace ac-

[1]See Cutler and Lleras-Muney (2010) for an overview of possible pathways between education and health behaviors.

cidents and exposure to unsafe conditions (e.g., Guardado and Ziebarth, 2013).[2] Hence, there is a clear association between education and the risk of health shocks through occupational choice.

Independent of education, health is directly associated with labor market outcomes. Individuals with lower levels of self-assessed health status report lower wages, work fewer hours, are less likely to participate in the labor force, and retire early. Currie and Madrian (1999) provide a review of the earlier literature.[3]

While the effects of education on health and of health on labor market outcomes are well documented, evidence on how education mitigates the negative consequences of health shocks for employment and earnings is limited. Our paper therefore contributes to understanding how education and health interact in their effects on labor market outcomes.

When estimating the effect of health status on labor market outcomes using survey data, nonrandom measurement error, reverse causality, and justification bias are common problems that can lead to endogenous health measures (e.g., Bound, 1991; Crossley and Kennedy, 2002; Baker, Stabile, and Deri, 2004).[4] Another issue that arises when using survey data is the timing of changes in health status and labor market outcomes. It is often difficult to measure which change occurred first. Even with panel data this problem persists due to recall bias.

To address the endogeneity of health measures, French (2005), Bound, Stinebrickner, and Waidmann (2010), and Gallipoli and Turner (2011) impose structure on the relationship between health and labor market outcomes in order to estimate causal parameters. On the other end of the methodological spectrum, Thomas et al. (2006), and Mohanan (2013) use experimental and quasi-experimental variation, respectively, to estimate the reduced-form effect of health on labor supply and household finances. Other studies that use sudden health events to estimate the effects of health on labor market outcomes include Dano (2005), Lundborg, Nilsson, and Vikström (2011), Halla and Zweimüller (2013), and Jeon (2013).

To establish causal pathways from health to labor market outcomes, it is important to observe the exact timing of changes in both variables, as noted above. Even when using administrative data, it may be difficult to do so. For example, Dano (2005), Lundborg, Nilsson, and Vikström (2011), and Jeon (2013) use annual earnings reported on tax returns as a measure for the intensive labor supply margin. This type of data complicates the timing of events because lower earnings in a given year may be due to a labor market shock that precedes a change in health status. In this case, reverse causality cannot be ruled out. In

[2]Workplace related stress may also contribute to lower health status. While stress may not necessarily be associated with blue-collar occupations, Johnson et al. (2005) find that stress is overall more prevalent in occupations that require lower levels of education.

[3]Another large literature documents the reverse effects of income shocks, for example due to recessions or unemployment, on individuals health (e.g., Ruhm, 2000; Salm, 2009).

[4]Justification bias refers to the bias introduced when respondents list their health as the reason for labor market outcomes such as early retirement. While some individuals retire due to health reasons, it is also a socially acceptable reason and may therefore be overreported in surveys as first noted by Bazzoli (1985).

contrast, our paper is the first to use monthly earnings data in this context. Therefore, we are able rule out most cases of reverse causality because we also know the exact date when a health shock occurred.

Dano (2005), Mohanan (2013), and Halla and Zweimüller (2013) only consider changes in health status due to accidents. While these external health shocks have the benefit of being unpredictable by individuals, the propensity of having an accident is not necessarily exogenous. For example, individuals who are not very risk averse may select into more dangerous jobs and also drive less safely. In contrast, we use all types of health shocks that require a hospital stay, including shocks that may be predictable due to underlying chronic conditions (e.g., heart attacks and complications of diabetes). To deal with potentially endogenous changes in health, we combine three strategies. First, we only include individuals in the treatment group who were hospitalized but did not have a hospitalization in the preceding year. Hence, we are confident that they did not anticipate the health shock and did therefore not change their labor supply before the shock occurred. Second, we use propensity score weighing in order to equalize pre-shock employment trends between treatment and control group. Third, we account for time invariant unobservable differences between individuals with and without health shocks by using difference-in-differences and fixed effects strategies. Hence, we are able to a analyze the labor market effects of a diverse range of health shocks while ensuring that observable and unobservable heterogeneity does not bias our results.

With the exception of Mohanan (2013), who uses a very small sample from one Indian village, our paper is the first to estimate the effect of health shocks on economic outcomes in an emerging or developing economy. Moreover, our sample is not only representative of the entire formal Chilean workforce, but we observe the universe of employees and hospital admissions. Therefore, we can estimate treatment effects that are relevant for the whole population. As many similar countries, Chile also provides an interesting setting for studying the effect of health and education on labor market outcomes for two reasons. First, a substantial fraction of the workforce is employed in jobs that require mostly manual tasks. The effect of disabling health events is therefore more pronounced than in a setting where most workers have desk jobs. Second, education levels are low with half the workforce not having a high school degree. Therefore the potential gains from increasing education are large if better educated individuals can cope more easily with health shocks.

In sum, our contributions to the literature are fourfold. First, we establish the role of education in the link between health and labor market outcomes. Second, by using detailed administrative data from Chile, we are able to time changes in health status and labor market outcomes very precisely to avoid reverse causality. Third, we combine empirical methods and sample restrictions to estimate effects of a wide variety of health shocks that are not biased due to observed or unobserved heterogeneity. Finally, our results are particularly policy relevant because they are the first that apply to the general workforce of an emerging economy.

The rest of this paper is organized as follows. In Section 2, we provide a brief background on the Chilean health care system and describe the data used in this paper. We also present summary statistics and some descriptive evidence on the effects of health shocks on labor market outcomes. Section 3 discusses our empirical methodology including propensity score weighting and outcome regressions. We then present the results in Section 4. Our findings show a reduction in labor supply at the extensive margin following a health shock. Education has substantial protective effect. Individuals with post-secondary eduction reduce their employment rates by over two percentage points less than those without a high school degree. We then investigate to what extent these education effects are due to different health shock and job characteristics. In particular the latter explain the protective effect of education to some extent. Finally, we conclude in Section 5.

2 Background and Data

2.1 The Chilean Healthcare System

Chile has a dual health care system. The *Fondo Nacional de Salud* (FONASA) is the public health insurance plan run by the Health Ministry. In addition, there are several *Instituciones de Salud Previsional* (ISAPRE), which are private plans that act as alternatives to FONASA. Employees are enrolled in the public FONASA system by default but can opt out and join an ISAPRE. In 2009, about 74 percent of the Chilean population were enrolled in FONASA and about 16 percent were members of an ISAPRE.

FONASA beneficiaries are classified in four groups. Group A beneficiaries are individuals who lack resources or formal employment, receive welfare or government pensions, pregnant women, and children under six years of age. Group A beneficiaries obtain free health care from all providers in the public network. They do not have to pay a premium for enrollment or any copayments to public providers. About 36 percent of FONASA beneficiaries are classified in group A. The remaining 64 percent are employees who contribute seven percent of their salary to the insurer, up to a monthly salary ceiling. They are classified into groups B, C, and D according to their monthly income. FONASA members pay copayments for health care services that vary between zero and 20 percent depending on their earnings relative to the minimum wage and the number of dependents. Beneficiaries can only obtain health care in public facilities or private facilities that have an agreement with FONASA at these copayment levels. If FONASA members want to avoid this limitation and choose a private health care provider instead, they pay higher copayments that depend the private facility's pricing level.

Individuals who opt out of FONASA can choose among 13 ISAPRE plans that are run by private insurance providers. Each plan offers different levels of coverage and different treatment options with different premiums. ISAPRE plans are more expensive than FONASA but provide access to better health care. The ISAPRE collect the mandatory contribution of seven percent, but members can pay an additional premium amounting to 2.2 percent of income on average. ISAPRE beneficiaries almost exclusive use private providers. There are two main reasons. First, by law, public hospitals mostly do not make hospital beds available to non-FONASA beneficiaries. Second, ISAPRE beneficiaries avoid using public providers, because they can obtain better-quality and more timely care through their regular coverage. Hence, ISAPRE plans are more expensive than FONASA but provide access to better health care with shorter waiting times.

2.2 Data Sources and Summary Statistics

We use administrative data on monthly earnings and hospital stays from two sources. The earnings data come from the Chilean unemployment insurance system, *Seguro de Cesantía* (SC). The Chilean government enacted it as an addition to the existing social protection net in 2002. Participation in SC is mandatory for all workers who have begun a new employment relationship after October 2002. Employees in existing jobs could elect to join SC. Monthly contributions amount to three percent of the employee's salary. Firms therefore report their employees' salaries to the SC administration on a monthly basis. Our data consist of monthly observations of individual earnings, employment (nonzero earnings), and the employer's industry. In addition, SC records employees' educational attainment, sex, year and month of birth, and the date they became affiliated with SC. We have access to the universe of SC records from October 2002 to December 2011. Monthly earnings are deflated with 2009 as the base year and expressed in 1,000 Chilean Pesos (CLP).[5] There are about 4.2 million men in this data set.[6]

The health shock data stem from hospital records. We have access to the universe of Chilean hospital discharge records for the years 2004 to 2007. For each hospital stay we observe the ICD-10 diagnosis code, the patient's health insurance provider, and the exact dates of admission and discharge. The Chilean health ministry collects these records from all hospitals in the country. We classify a hospital stay by major type of diagnosis according to the first letter of the ICD-10 code.[7] For estimation purposes, we select the ten most frequent types of diagnosis and lump the remaining ones into an "other" category. The appendix contains tables showing the distribution of these diagnoses. There are about 1.4 million men in this data set.

[5] 1,000 CLP equal roughly 2 US dollars.

[6] Since the earnings data stem from SC records, only employees in the formal workforce are included. There are about 5.6 million men aged 15 to 64 in Chile, so we capture the majority of the population.

[7] See http://www.icd10data.com/ICD10CM/Codes for a list of all ICD-10 codes.

Both data sets contain individuals' *Rol Único Tributario* (RUT) that acts as a unique identifier for tax and other purposes in Chile. We match individuals' monthly employment records to hospital records on RUT and sex.[8] We restrict the sample to men born between 1950 and 1980 and exclude men who became affiliated with SC after December 2003 to ensure that we can observe a sufficiently long employment history before the health shock. In addition, we drop men that were employed fewer than 24 months between 2002 and 2011 to eliminate individuals with weak ties to the formal labor market. Men who had a hospital stay in 2004 are dropped from the sample. This restriction implies that a hospitalization is a true health shock in the sense that an individual experienced no severe health events in the previous year. To make the estimation more manageable, we draw a random 25 percent sample from the potential control group, i.e. men without a hospital stay. The final estimation sample consists of 46,485 men with a hospital stay and 138,199 control group members. To compare treatment and control group individuals before and after a health shock in a difference-in-differences strategy, we have to assign a placebo health shock month to members of the control group. We do so by randomly assigning a number between one and 36 to each man without a hospital stay, which corresponds to the months between January 2005 and December 2007.

We now describe the data we use for estimation. Columns (1) and (2) of Table 1 show summary statistics for individual characteristics by treatment status. The two groups are roughly similar with the treatment group being slightly older and less educated. As in other emerging economies, the levels of education are low in Chile, with over half of the sample not having a high school degree. Labor market status in the month before the health shock ($t = -1$) does not differ substantially between treatment and control group either. In addition to employment in $t = -1$ we categorize employees by "blue collar" and "white collar" industry and within-industry earnings tercile.[9]

Next, we turn to a more detailed look at health shock frequencies and characteristics by education. Panel A in Table 2 shows the fraction of men in our sample who were hospitalized between 2005 and 2007, both overall and by industry in $t = -1$. There is a clear gradient by education with 8.2 percent of men without a high school degree, 7.4 percent of high school graduates, and 6.8 percent of those with post-secondary education experiencing a health shock. This gradient mostly persists when conditioning on industry.

Panel B in Table 2 displays the distribution of health shock characteristics by education. External health shocks such as car and workplace accidents are more common among the less

[8] We carried out all empirical analyses on a secure server at the Chilean finance ministry. The authors are not able to identify individuals from the matched data. The project was granted IRB approval by Queen's University.

[9] We proxy occupation, which we do not observe directly, by assigning each employee to an industry that tends to have more blue collar or white collar jobs. In addition, we use earnings tercile within the two industry categories to capture different types of occupations.

educated while those with post-secondary education are more likely to suffer from diseases of the circulatory system and cancer. The severity of hospital stays, measured in days spent in the hospital, is higher for men with lower educational attainment. The proportion of men staying more than two weeks is double for those without a high school degree compared to the post-secondary education group. This pattern applies to the initial hospital stay and to the sum of days spent in the hospital for the same diagnosis within one year of the initial health shock. Finally, we report the fraction of men enrolled in FONASA and ISAPRE. While most men without a high school degree are enrolled in FONASA A or B due to their low income, almost half of those with post-secondary education are ISAPRE members. Hence, there is a strong positive correlation between education and better health insurance coverage. In the regressions that follow, we control for health insurance provider in order to avoid attributing the effect of better health care to education.

Overall, Table 2 shows that higher education levels are associated both with fewer and less severe health shocks. Hence, our first conclusion based on this descriptive evidence is that education has a protective effect as it lowers the propensity of health shocks, and in particular more severe shocks. In the analysis in Section 4, we investigate if higher educational attainment also leads to better labor market outcomes following a health shock.

Finally, we describe the outcome variables to provide a sense for their magnitudes and some preliminary evidence for the effect of health shocks. We are interested in the effects of health shocks on both the extensive and intensive labor supply margins. To measure the extensive margin, we use an indicator for whether an individual was employed in a given month. Since the data do not contain hours worked, we use monthly earnings to capture the intensive margin. In particular, we take log-earnings, so the earnings regressions are conditional on employment (non-zero earnings). Last, we investigate the aggregate effect of health shocks on the individual's financial situation by summing up monthly earnings during the one or two years before and after the health shock, respectively, and dividing by average monthly earnings in the same group. Formally, they are defined as

$$\%W_i^{S^+} = \frac{\sum_{s=1}^{S} W_{i,s}}{\bar{W}^C_{g(i),0}} \quad \text{and} \quad \%W_i^{S^-} = \frac{\sum_{s=-S}^{-1} W_{i,s}}{\bar{W}^C_{g(i),0}}, \tag{1}$$

where $W_{i,s}$ represents monthly earnings of person i in month s before or after the health shock, $\bar{W}^C_{g(i),0}$ is average monthly earnings in the month of the placebo health shock for control individuals in the same group $g(i)$ as person i, and $S = \{12, 24\}$. Groups are defined by age, education, and industry. Hence, we interpret $\%W_i^{S^+}$ and $\%W_i^{S^-}$ as the number of months worth of earnings within one or two years compared to the average person in his group.[10]

[10]For example, $\%W_i^{S^+} = 10$ would indicate that this person earns 10 months worth of average monthly

Table 3 shows average employment and earnings for treatment and control groups before and after the health shock. Overall, between 70 and 75 percent of men are employed in any given month and average monthly earnings are around 250,000 and 350,000 pesos. Using the summary statistics in this table, we can construct an unconditional difference-in-differences estimate. The effect of a health shock an employment is about -6.6 percentage points and the effect on monthly earnings equals $-19{,}000$ pesos or about 40 US dollars. These are only rough estimates that do not account for other factors, but they provide some first evidence that health shocks reduce employment and earnings. The average earnings measures defined in equation (1) also indicate a financial loss due to health shocks within one and two years. Specifically, the average health shocks leads to a loss of 0.4 monthly earnings within the first and of about 0.75 monthly earnings within two years after the health shock.

3 Methodology

The main challenge in estimating the causal effect of health shocks on labor market outcomes is the fact that these shocks are not randomly assigned. Instead, individuals who suffer a health shock may differ observably and unobservably from the control group. We combine three strategies that deal with heterogeneity and therefore allow us to interpret our estimates as causal effects. First, we weight the data using estimated propensity scores to make treatment and control groups observably similar.

Second, we use a difference-in-differences (DID) strategy and individual fixed effects (FE) to account for time-invariant unobserved heterogeneity. The DID approach is only valid under the common trends assumption. Here, the common trend refers to labor market outcomes before the health shock. Since we observe these outcomes for up to five years we can easily test this assumption. In addition to DID regressions we also include individual FE. This approach can be seen as a generalization of DID where the constant term in the regression does not only differ between treatment and control group but rather by individual. A crucial assumption for the FE estimates to be valid is strict exogeneity of the health shock. While we cannot completely rule out that individuals anticipate health shocks and therefore adjust their labor supply, we use the third strategy to exclude health shocks that may be anticipated.

The third strategy consists of excluding individuals from our sample who were hospitalized in 2004, the first year of the hospital records. Hence, each treated individual was relatively healthy for at least one year prior to being admitted to a hospital.[11] To the extent that a hospital stay, which does not follow an earlier inpatient visit, represents new informa-

earnings in his group during the year following a health shock.

[11] Ideally, we would also like to observe their outpatient doctor visits and other health care use, but our data are limited to hospital stays, so we can rule out severe health events during the year before the hospitalization that we use as a health shock.

tion about a person's health, these health shocks are not anticipated and therefore strictly exogenous.

3.1 Propensity Score Weighting

In this section, we describe our weighting procedure using the propensity score. The goal is to make treatment and control groups similar based on observable covariates that are predetermined at the time of the (placebo) health shock. Using monthly earnings data, we can flexibly account for labor market outcomes before the health shock. Hence, in addition to individual characteristics such as age and education, we also include employment, earnings, and industry up to one year before the health shock in the propensity score covariates.

Imbens (2014) stresses the importance of the design stage and in particular of the overlap of the covariates that enter the propensity score. We follow his suggestion and check the overlap of covariates between treatment and control groups using normalized differences.[12] The normalized difference for covariate Z_k is defined as

$$\Delta Z_k = \frac{\bar{Z}_k^T - \bar{Z}_k^C}{\sqrt{0.5(S^2_{Z_k^T} + S^2_{Z_k^C})}},$$

where $\bar{Z}_k^T$ and $\bar{Z}_k^C$ is the sample mean of Z_k in the treatment and control group, respectively, and $S^2_{Z_k^T}$ and $S^2_{Z_k^C}$ are the corresponding sample variances. Column (3) in Table 1 shows the normalized differences for select covariates including labor market status in the month preceding the health shock.[13] All of them are below 0.1 and hence well below the rule of thumb value of 0.25 suggested by Imbens and Wooldridge (2009). Hence, the overlap between treatment and control group is good. In other words, individuals with and without health shocks are not very different even before weighting, including pre-treatment outcomes. This similarity is reassuring as it implies that we do not have to rely solely on propensity score weighting to equalize observables between the two groups. Moreover, it lends support to the common trends assumption that is necessary for our DID strategy.

To estimate the propensity score of a health shock we use a logit regression that includes the covariates from Table 1 plus employment status for 12 months before the health shock and detailed industry in the month preceding the health shock.[14] After estimating the propensity score we also check overlap between treatment and control group by examining the distribution of the estimated propensity scores by treatment status. Figure 1, which displays kernel density estimates of the propensity score distribution, shows that the overlap is very good.

[12] Using normalized differences to check the overlap is preferred to t-statistics because the former are independent of sample size.

[13] The appendix contains the normalized differences for all covariates that enter the propensity score, including employment status for 12 months before the health shock and detailed industry in month $t = -1$.

[14] The appendix contains the estimation results.

In other words, the distribution of the estimated propensity of a health shock conditional on observables is almost identical in the treatment and control groups. The good overlap is further illustrate when we trim the sample to exclude treated individuals whose propensity score is below the minimum or above the maximum propensity score in the control group and vice versa. We only exclude 18 individuals out of over 180,000 due to this restriction.

While there are many possibilities to match treatment and control group members based on the propensity score, Busso, DiNardo, and McCrary (2014) show that using inverse propensity score weights (IPSW) leads to a relatively small bias if the overlap between treatment and control group is good. We therefore use the estimated propensity score to calculate the IPSW as follows:

$$\hat{w}_i^{ATE} = \frac{T_i}{\hat{p}_i(Z_i)} + \frac{1 - T_i}{1 - \hat{p}_i(Z_i)}, \tag{2}$$

where $\hat{p}_i(Z_i)$ is the estimated propensity score conditional on covariates Z_i and $T_i = \{0, 1\}$ is the treatment indicator for having any health shock. When weighting the data by $\hat{w}_i^{ATE}$, the difference between treatment and control outcomes corresponds to average treatment effects (ATE) (**?**). We weight the regressions described in the following section by the ISPW.

3.2 Outcome Regressions

We estimate the effect of health shocks on the labor market outcomes employment, monthly log-earnings, and the total earnings measure defined in equation (1) in Section 2.2 above. To analyze the effect of educational attainment on the labor market effects of health shocks, we first estimate DID regressions as follows:

$$\begin{aligned} Y_{it} = \beta_1 T_i + \sum_k \beta_2^k T_i E_i^k + \beta_3 P_t + \sum_k \beta_4^k P_t E_i^k \\ + \beta_5 T_i P_t + \sum_k \beta_6^k T_i P_t E_i^k + X_i' \gamma + \delta_t + u_{it}, \end{aligned} \tag{3}$$

where Y_{it} is the labor market outcome of individual i in month t. $T_i = \mathbf{1}\{H_i = 1\}$ is the treatment indicator for any health shock, $P_t = \mathbf{1}\{t \geq 0\}$ is the indicator for months after the (placebo) health shock, and $E_i^k = \mathbf{1}\{E_i = k\}$ denotes that individual i has a high school degree ($k = 2$) or some post-secondary education ($k = 3$). The vector of control variables X_i contains age and age squared at the time of the health shock, educational attainment, labor market status and industry in the month before the health shock ($t = -1$), employment status for the each of the 12 months before the health shock, health insurance provider for the treatment group, and indicators for year and month when the health shock occurred. Finally, we include a dummy variable for each year-month of our sample period (δ_t). The parameters of interest are the βs for the treatment-post interactions: β_5 for individuals without a high school degree and $\beta_6^k, k = 2, 3$ for men with a complete high school or some post-secondary education, respectively. These coefficients correspond to ATEs since we weight the data by the IPSW defined in equation (2). We estimate regression (3) using OLS and FE. In the latter case, we exclude the time-invariant variables T_i, $T_iE_i^k$, and X_i.

By testing the hypothesis $H_0 : \beta_6^k = 0, k = 2, 3$ in regression (3), we can determine if education reduces the negative effect of health shocks on labor market outcomes. That is, we expect $\beta_5 < 0$ and $\beta_6^k > 0, k = 2, 3$. However, these results do not tell us if the positive effect of education is due to educational attainment per se or to different health shock characteristics (see Table 2). Therefore, we also interact the treatment, post-shock, and education variables in regression (3) with the type of diagnosis and the length of stay.[15] Now the treatment variable is defined as $H_i^j = \mathbf{1}\{H_i = j\}$, where j denotes health shock categories (four length of stay ranges and ten major diagnoses). Hence we estimate

$$\begin{aligned} Y_{it} = &\sum_j \beta_1^j H_i^j + \sum_{j,k} \beta_2^{j,k} H_i^j E_i^k + \beta_3 P_t + \sum_k \beta_4^k P_t E_i^k \\ &+ \sum_j \beta_5 H_i^j P_t + \sum_{j,k} \beta_6^{j,k} H_i^j P_t E_i^k + X_i'\gamma + \delta_t + u_{it}. \end{aligned} \tag{4}$$

In addition to health shock characteristics, better educated individuals may also experience smaller decreases in employment and earnings because they hold different types of jobs. We proxy occupation by whether an employee works in a blue collar or white collar industry and by the within industry earnings tercile.[16] We estimate the following regression:

$$\begin{aligned} Y_{it} = &\sum_j \beta_1^j T_i S_i^j + \sum_{j,k} \beta_2^{j,k} T_i S_i^j E_i^k + \sum_j \beta_3^j P_t S_i^j + \sum_{j,k} \beta_4^{j,k} P_t S_i^j E_i^k \\ &+ \sum_j \beta_5^j T_i S_i^j P_t + \sum_{j,k} \beta_6^{j,k} T_i S_i^j P_t E_i^k + X_i'\gamma + \delta_t + u_{it}, \end{aligned} \tag{5}$$

where $S_i^j = \mathbf{1}\{S_i = j\}$ is an indicator for labor market status (i.e. industry-earnings tercile category) j. We also estimate these regressions using OLS and FE. Again, we are interested in the DID parameters β_5^j and $\beta_6^{j,k}$ in regressions (4) and (5). We can then determine if the mitigating effect of eduction in the relationship between health shocks and labor market outcomes disappears when interacted with health shock or job characteristics. In particular, if education has no effect other than leading to less severe health shocks and allowing individuals to hold jobs where they can deal with health problems more easily, we expect that the coefficients $\hat{\beta}_6^{j,k}$ become insignificant.

Regressions (3), (4) and (5) constrain the treatment effects to be constant over time. It is likely, however, that the labor market effects of health shocks change over time. Making

[15] We use the total length of stay in a hospital during one year following the initial health shock for stays that are due to the same diagnosis as the initial hospitalization.

[16] See Table 1 for a list of these labor market status categories.

use of our monthly earnings data, we can estimate such time-varying treatment effects over the short and long run. In particular, we let treatment effects vary on the monthly level during the first year after the health shock and annually for subsequent years. We replace the post-shock indicator P_t by health shock-lag variables L_t accordingly, and our baseline regression becomes

$$\begin{aligned} Y_{it} = \beta_1 T_i + \sum_k \beta_2^k T_i E_i^k + \sum_s \beta_3^s L_t^s + \sum_{s,k} \beta_4^{s,k} L_t^s E_i^k \\ + \sum_s \beta_5^s T_i L_t^s + \sum_{s,k} \beta_6^{s,k} T_i L_t^s E_i^k + X_i' \gamma + \delta_t + u_{it}, \end{aligned} \tag{6}$$

where $s = \{0, 1, 2, \ldots, 12, [13, 24], [24, 36], [37, 48], [49, 60], [61, 83]\}$ denotes the number of months that have passed between the health shock and month t and $L_t^s = \mathbf{1}\{L_t = s\}$.[17] Similarly, we estimate regressions with interactions by health shock characteristics and labor market status as follows:

$$\begin{aligned} Y_{it} = \sum_j \beta_1^j H_i^j + \sum_{j,k} \beta_2^{j,k} H_i^j E_i^k + \sum_s \beta_3^s L_t^s + \sum_{s,k} \beta_4^{s,k} L_t^s E_i^k \\ + \sum_{j,s} \beta_5^s H_i^j L_t^s + \sum_{j,s,k} \beta_6^{j,s,k} H_i^j L_t^s E_i^k + X_i' \gamma + \delta_t + u_{it} \end{aligned} \tag{7}$$

and

$$\begin{aligned} Y_{it} = \sum_j \beta_1^j H_i S_i^j + \sum_{j,k} \beta_2^{j,k} H_i S_i^j E_i^k + \sum_{j,s} \beta_3^{j,s} L_t^s S_i^j + \sum_{j,s,k} \beta_4^{j,s,k} L_t^s S_i^j E_i^k \\ + \sum_{j,s} \beta_5^{j,s} H_i S_i^j L_t^s + \sum_{j,s,k} \beta_6^{j,s,k} H_i S_i^j L_t^s E_i^k + X_i' \gamma + \delta_t + u_{it}. \end{aligned} \tag{8}$$

In the results section below we report the estimated ATEs $\hat{\beta}_5^{j,s}$ and $\hat{\beta}_6^{j,s,k}$ by time passed since the health shock as well as by educational attainment and health shock or labor market categories.

4 Results

4.1 Graphical Analysis

Before discussing the regression results, we provide graphical evidence for how the effect of health shocks on labor market outcomes varies by educational attainment. Figure 2 plots monthly employment rates relative to the month of the (placebo) health shock for treatment

[17] We follow individuals until December 2011, so the longest follow-up period for someone who incurred a health shock in January 2005 is 83 months.

and control group. The employment data are weighted by the IPSW defined in equation (2), so we can interpret the vertical difference between employment rates of treatment and control group as the time-varying ATE of a health shock.[18] First, we note that the propensity score weighting works very well, so pre-treatment employment rates of treatment and control group are identical for each education category. Second, we find that health shocks have an immediate and substantial effect on employment rates. This negative effect decreases a little over time, but even four years after the health shock, men in the treatment group are substantially less likely to be employed. Finally, we can provide a first answer to our research question about the role of education in the relationship between health shocks and labor market outcomes. Men without a high school degree decrease their employment by about six percentage points immediately after the health shock while high school graduates and those with post-secondary education see their employment fall by four and two percentage points, respectively. Hence, there is clear evidence that education has a protective effect. In the following subsections, we explore to what extent this education differential can be explained by other factors such as health shock characteristics.

In Figure 3, we plot monthly log-earnings by treatment status and education attainment. Hence, these graphs show the effect of health shocks on the intensive labor supply margin conditional on employment. In contrast to the extensive margin effects shown in Figure 2, the effects for all education groups are much smaller. There is an earnings decrease of about 20 percent in the month of the health shock for men without a high school degree, but in the following months, earnings in the treatment group catch up with earnings in the control group. Overall, there does not seem to be a significant longterm effect of health shock on earnings conditional on employment. For individuals with a high school degree or post-secondary education the initial drop in earnings is even smaller.[19]

Given the evidence presented in Figures 2 and 3, we can already draw two important preliminary conclusions. First, men adjust their labor supply mostly at the extensive margin in response to a health shock. This finding suggests that they either stop working completely after a hospitalization and return to work very slowly, or they remain employed and do not reduce their labor supply at least after the first month. In the regressions below, we can investigate if this pattern persists when we control for health shock characteristics. Second, we find strong evidence for a protective effect of education. In other words, the higher an individual's educational attainment, the less he reduces his labor supply at the extensive margin. We cannot attribute this effect to education per se or to differential health shock and job characteristics between education groups yet, but we are able to do so in the regression

[18] Here we aggregate over all types of diagnoses, but the regression results below treat health shocks as heterogeneous based on diagnosis and severity.

[19] The pre-treatment earnings of men with post-secondary education are not perfectly matched between treatment and control group, which is due to the fact that we do not include earnings but only employment in the propensity score. The common trends assumption is still satisfied, however.

analysis below.

4.2 Difference-in-Difference Regressions

We now turn to the results from estimating the DID regressions (3), (4), and (5), i.e. the long-run effects of health shocks. All regressions are weighted by the IPSW in equation (2) and control for a quadratic in age at the (placebo) health shock, education, labor market status (earnings tercile by blue/white collar industry), detailed industry in the pre-shock month, employment for the 12 months before the health shock, health insurance provider (FONASA A, B, C, D, and ISAPRE) for the treatment group, health shock calendar month and year dummies, and a flexible time trend (dummies for each sample month). Standard errors in all regressions to follow are clustered on the individual level. We focus on the ATE estimates, i.e. the coefficients β_5 for the ATE for men without an educational degree and β_6^2 and β_6^3 for high school graduates and men with post-secondary education, respectively, in the regressions above. When investigating if the effect of education can be explained by heterogeneity in health shock or job characteristics, these coefficients are indexed accordingly.

Table 4 contains our baseline results for all labor market outcomes considered here. For completeness, we report not only the ATE estimates, but also the coefficients on treatment and post-shock dummies as well as on the education interactions. The first column shows the results from estimating regression (3) using OLS. The ATE estimates confirm the graphical evidence discussed above. First, there is a significant reduction in employment for all educational groups. These estimates are all significant at a p-value below 0.001. Second, men without a high school degree reduce their employment by 4.6 percentage points after the health shock while those with a high school degree and post-secondary eduction reduce employment by 2.9 and 2.3 percentage points, respectively.[20] Hence, the regression result confirms the protective effect of education. The second column in Table 4 reports estimates for the same regression obtained using individual fixed effects. The estimates are virtually identical to those obtained via OLS. This similarity is reassuring because it implies that we do not have to rely on individual fixed effects and the associated assumption to obtain valid estimates for the causal effect of health shocks on labor market outcomes.[21]

While there is strong evidence for the negative effect of health shocks on labor supply at the extensive margin, column (3) in Table 4, which contains the regression results for monthly log-earnings, shows that the effect at the intensive margin is smaller, confirming the finding from Figure 3. The ATE for the no high school group amounts to a 1.3 percent reduction in monthly earnings due to health shocks. While the point estimates for the difference in ATE

[20] The difference between the no high school group and high school graduates and men with post-secondary education, respectively, is statistically significant, but the difference between the two latter groups is not.

[21] Due to this similarity we do not report FE estimates for the earnings outcomes.

between the higher education groups and the baseline are positive, indicating a protective effect of educational attainment, they are not statistically significant.

In the last two columns of Table 4, we are interested in the total financial effect of health shocks over the one and two years following the health shock. This effect combines the extensive and intensive margins and quantifies the overall health shock effect on earnings. The outcome variable here is the number of months worth of average group earnings defined in equation (1). Hence, we interpret the ATE estimates as the number of months worth of earnings that men lose due to a health shock within the one or two years after the shock. For example, for men without a high school degree, we find a significant effect of about -0.5, implying that these men lose on average half a month worth of earnings during the year after the shock. Similarly, the they lose 1.2 months during the two-year interval. Men with higher levels of education lose significantly fewer months of earnings.[22] Those with post-secondary education, for instance, lose only 0.4 months worth of earnings during the two years after a health shock. Hence, when considering the overall earnings loss due to health shock we see a strong protective effect of education. However, combined with the results on employment and monthly log-earnings, it is clear that this loss mostly comes from men being less likely to work at all after a health shock.

Given the evidence that individuals with higher levels of education experience a lower reduction in employment and total earnings after a health shock, we now ask if this protective effect of education may be due to the fact that highly educated men suffer less severe shocks or work in jobs where they can deal with the effects of health shocks more easily. In other words, we test if the positive effect of education disappears when conditioning on health shock and job characteristics. First, we consider heterogeneity by diagnosis. We divide hospital stays into the ten most common broad diagnosis categories. Table 5 contains the estimation results for employment, monthly log-earnings, and total earnings. There is considerable heterogeneity in the ATEs of different health shocks on employment independent of education. For example, a hospital stays for treatment of neoplasms (cancer and other tumors) leads to a 17 percentage point reduction in employment rates among men without a high school degree while a health shock due to external causes (for example accidents) only reduces employment by 2.7 percentage points for this group. For roughly half of the diagnoses categories, we still find a significant protective effect of higher education levels. Among men that are hospitalized for cancer treatments, those with a high school degree and post-secondary education reduce their employment by 8.4 and 4.6 percentage points, respectively. Hence, while the education gradient in the employment effects of health shocks diminishes to some extent when conditioning on diagnosis, it is still present particularly for more severe types of health shocks. We draw a similar conclusion for the earnings outcomes considered in Table 5.

[22] Recall that the earnings months are relative to average earnings in a comparison group that is defined by education among other variables.

The second health shock characteristic we consider in this context is the shock severity proxied by the total length of stay (in days) for the same diagnosis as the initial hospital stay within 12 months following the begin of the first hospitalization.[23] Table 6 shows the estimation results for four length of stay categories. First, we find larger negative effects on employment and earnings with longer hospital stays. For example, the decrease in employment rates for men without a high school degree range from three to 13 percentage points and monthly earnings decrease by zero to seven percent depending on length of stay. Second, we still see significant effects of education. While men with more educational attainment have shorter hospital stays (see Table 2), this relationship does not explain the protective effect of education. In contrast, for men who stay more than two weeks, having post-secondary education leads to a reduction in employment rates that is eight percentage points lower than among men without a high school degree. Again, these effects are constrained to the extensive margin as we do not see significant effects in the monthly log-earnings regression. Overall, we find that differences in health shock characteristics by educational attainment do not explain the fact that highly educated men reduce their labor supply less than men with lower education following a health shock.

Another explanation for the observed protective effect of education is a potential difference in job characteristics by education. For example, men who work in highly paid jobs in a "white collar" industry may cope with a health shock more easily. These men can work in their occupations if they are limited by an adverse health event and they may also have the necessary means to purchase medical services that improve or speed up the healing process after a health shock. To test this hypothesis, we condition on earnings tercile within blue and white collar industries. Table 7 reports the estimates that correspond to regression (5) above. Among men without a high school degree, the effects of a health shock vary by industry and earnings tercile. In particular, for men working in white collar industries, the decrease in employment rates is smaller for those earning higher earnings before the health shock. However, for men working in blue collar industries, this relationship is reversed. To answer the question how education and job characteristics interact, we focus on the $\hat{\beta}_6$-coefficients. These estimates are mostly insignificant in column (1) of Table 7. This finding holds in particular for men who work in blue collar industries across all pre-shock earnings terciles. On the other hand, education has a significant effect among men employed in white collar industries. For example, men who are in the lowest pre-shock earnings tercile in white collar industries reduce the employment the most if they do not have a high school degree compared to all other job-education cells (by 6.4 percentage points). If they work in the same job category and have higher educational attainment, they reduce their employment significantly less (by 3.5 percentage points for high school graduates and not at all for men with post-

[23] We also restricted length of stay to the initial hospitalization, and the results are similar to ones reported here.

secondary education). A possible explanation is that men who work in a low paying manual job in a white collar industry have fewer opportunities to move to another occupation within the same firm or industry that can accommodate their health problems unless they have higher levels of eduction.[24] With regards to earnings, we find that higher levels of education reduce the decrease due to health shocks for some but not all job categories. Hence, we can explain the protective effect of education to some extent by occupational sorting.

In sum, the estimation results from the DID regressions show a clear picture. First, men with higher levels of education reduce their long-run employment by less than those without educational attainment. This protective effect of education mostly plays a role at the extensive labor supply margin. The financial implications of this effect are substantial with highly educated men losing about half a month worth of earnings on average in the two years after a health shock compared to 1.2 months for those without a high school degree. Second, when we condition on health shock characteristics, we find that the protective effect mostly remains, implying that it is not the differences in health shocks by education that drive the results. On the other hand, job characteristics explain the education effects to some extent, but not completely. These results indicate that higher levels of education allow individuals to work in "better" jobs where they can cope with the effects of health shocks more easily, but different types and severity levels of health shock do not explain the protective effect of education.

4.3 Employment Regressions With Health Shock Lags

After the long-run effects estimated by simple DID regressions, we now turn to dynamic treatment effects by estimating regressions (6), (7), and (8). These results are important because they show how individuals change their labor supply over time after suffering a health shock. We present the results in this section in graphical form to ease interpretation. Figure 4 shows the estimated effects of health shock lags on employment by educational attainment. We estimate monthly effects for the first year after the shock and annual effects for the following four years. The indicated significant differences in the graphs for high school graduates and men with post-secondary education refer to $\hat{\beta}_6^{s,k}$ in regression (6) being statistically significant. That is, for the indicated lags, education leads to different effects of health shocks on employment. The results in Figure 4 confirm the DID results discussed above and provide additional insights about the timing of the health shock effects. In particular, higher levels of education lead to smaller reductions in employment for all time periods after the health shock. Another interesting finding is the fact that the large initial drop in

[24]Ideally, we would like to observe occupations before and after the health shock, but this information does not exist in the monthly earnings data.

employment is missing for men with post-secondary education. Hence, these individuals cope with health shocks much more easily especially in the first few months.

Following the reasoning above, we now check if conditioning on health shock and job characteristics affects this education gradient. Figures 5 and 6 plot ATEs by education and diagnosis and total length of stay, respectively. We find a similar shape for the time-varying ATEs as before with more educated men having smaller absolute employment reductions. We also find that the differences between ATEs by education are not significant for most of the diagnoses and some length of stay categories. For labor market status in $t = -1$, shown in Figure 7, we find similar patterns. Hence, when considering dynamic treatment effects, we find that the role of education clearly matters, but there is mixed evidence about the actual pathways. To some extent, education per se acts in a protective manner, but there is also evidence that this effect operates through less severe health shocks and more favorable work conditions associated with higher educational attainment.

5 Conclusion

In this paper, we investigate the role education in determining labor market effects of health shocks. Using administrative earnings and hospital data from Chile, we find that men with higher levels of education reduce their employment less after a health shock than their less educated counterparts. Highly educated individuals have lower total earnings losses after a health shock, but the latter effect is mostly due to a decrease in labor supply at the extensive margin. Finally, we find that differential health shock and job characteristics explain only a part of the observed education gradient.

These result are relevant because they illustrate the potential gains that can be realized in an emerging economy such as Chile when education levels are increased. Through higher levels of education, individuals do not only obtain higher wages, but are also able to deal with health shocks more easily. This protective effect operates through various channels. Highly educated individuals qualify for jobs that expose them to fewer unsafe working conditions, thereby lowering the frequency and severity of health shocks. Moreover, they can afford better health care should a health shock occur, and finally, they possess the general human capital that enables them to switch jobs if necessary after a health shock.

While we provide evidence for these protective effects of education, it will be interesting to further investigate these channels in more detail in future work. In particular, in adding data on outpatient healthcare, it would be possible to account for healthcare utilization outside of hospitals. For example, it is possible that better educated individuals use more preventive care. Another potential extensions is the analysis of the role of eduction and health in occupational choice.

References

Autor, David H and Michael J Handel. 2013. "Putting Tasks to the Test: Human Capital, Job Tasks, and Wages." *Journal of Labor Economics* 31 (2):S59–S96.

Baker, M, M Stabile, and C Deri. 2004. "What do self-reported, objective, measures of health measure?" *Journal of Human Resources* 39 (4):1067–1093.

Bazzoli, GJ. 1985. "The early retirement decision: new empirical evidence on the influence of health." *Journal of Human Resources* 20 (2):214–234.

Bound, John. 1991. "Self-Reported Versus Objective Measures of Health in Retirement Models." *Journal of Human Resources* 26 (1):106–138.

Bound, John, Todd Stinebrickner, and Timothy Waidmann. 2010. "Health, economic resources and the work decisions of older men." *Journal of Econometrics* 156 (1):106–129.

Brunello, Giorgio, Daniele Fabbri, and Margherita Fort. 2013. "The Causal Effect of Education on Body Mass: Evidence from Europe." *Journal of Labor Economics* 31 (1):195–223.

Busso, Matias, John DiNardo, and Justin McCrary. 2014. "New Evidence on the Finite Sample Properties of Propensity Score Reweighting and Matching Estimators." *Review of Economics and Statistics* .

Card, David. 1999. "The causal effect of education on earnings." *Handbook of labor economics* 3:1801–1863.

Crossley, TF and S Kennedy. 2002. "The reliability of self-assessed health status." *Journal of Health Economics* 21 (4):643–658.

Currie, Janet and Brigitte C. Madrian. 1999. "Health, Health Insurance and the Labor Market." In *Handbook of Labor Economics*, edited by Orley C. Ashenfelter and David Card. Elsevier Science, 3309–3416.

Cutler, David M. and Adriana Lleras-Muney. 2010. "Understanding differences in health behaviors by education." *Journal of Health Economics* 29 (1):1–28.

Dano, Anne Moller. 2005. "Road injuries and long-run effects on income and employment." *Health Economics* 14 (9):955–970.

de Walque, Damien. 2007. "Does education affect smoking behaviors?" *Journal of Health Economics* 26 (5):877–895.

French, Eric. 2005. "The Effects of Health, Wealth, and Wages on Labour Supply and Retirement Behaviour." *Review of Economic Studies* 72 (2):395–427.

Gallipoli, Giovanni and Laura Turner. 2011. "Household Responses to Individual Shocks: Disability and Labor Supply."

Grimard, Franque and Daniel Parent. 2007. "Education and smoking: Were Vietnam war draft avoiders also more likely to avoid smoking?" *Journal of Health Economics* 26 (5):896–926.

Guardado, José R and Nicolas R Ziebarth. 2013. "A Model of Worker Investment in Safety and Its Effects on Accidents and Wages." :1–44.

Halla, Martin and Martina Zweimüller. 2013. "The Effect of Health on Earnings: Quasi-experimental Evidence From Commuting Accidents." *Labour Economics* 24 (C):23–38.

Heckman, James J, John Eric Humphries, Gregory Veramendi, and Sergio Urzua. 2014. "Education, Health and Wages." :1–60.

Imbens, Guido W. 2014. "Matching Estimators in Practice: Three Examples." *NBER Working Paper* :1–66.

Imbens, Guido W and Jeffrey M. Wooldridge. 2009. "Recent Developments in the Econometrics of Program Evaluation." *Journal of Economic Literature* 47 (1):5–86.

Jeon, Sung-Hee. 2013. "The long-term effects of cancer on employment and earnings of cancer survivors."

Johnson, Sheena, Cary Cooper, Sue Cartwright, Ian Donald, Paul Taylor, and Clare Millet. 2005. "The experience of work-related stress across occupations." *Journal of Managerial Psychology* 20 (2):178–187.

Jürges, Hendrik, Steffen Reinhold, and Martin Salm. 2011. "Economics of Education Review." *Economics of Education Review* 30 (5):862–872.

Leigh, J Paul. 1990. "Schooling and seat belt use." *Southern Economic Journal* :195–207.

Lochner, Lance. 2011. "Non-production benefits of education: Crime, health, and good citizenship." .

Lundborg, Petter, Martin Nilsson, and Johan Vikström. 2011. "Socioeconomic Heterogeneity in the Effect of Health Shocks on Earnings: Evidence from Population-Wide Data on Swedish Workers."

Mohanan, Manoj. 2013. "Causal Effects of Health Shocks on Consumption and Debt: Quasi-experimental Evidence from Bus Accidents Injuries." *Review of Economics and Statistics* 95 (2):673–681.

Naimi, Timothy S, Robert D Brewer, Ali Mokdad, Clark Denny, Mary K Serdula, and James S Marks. 2003. "Binge drinking among US adults." *JAMA: The Journal of the American Medical Association* 289 (1):70–75.

Ruhm, Christopher J. 2000. "Are Recessions Good For Your Health?" *Quarterly Journal of Economics* 115 (2):617–650.

Salm, Martin. 2009. "Does job loss cause ill health?" *Health Economics* 18 (9):1075–1089.

Speer, Jamin D. 2014. "Pre-Market Skills, Occupational Choice, and Career Progression." :1–86.

Thomas, D, E Frankenberg, J Friedman, JP Habicht, M Hakimi, and N Ingwersen. 2006. "Causal effect of health on labor market outcomes: Experimental evidence."

Webbink, Dinand, Nicholas G Martin, and Peter M Visscher. 2010. "Does education reduce the probability of being overweight?" *Journal of Health Economics* 29 (1):29–38.

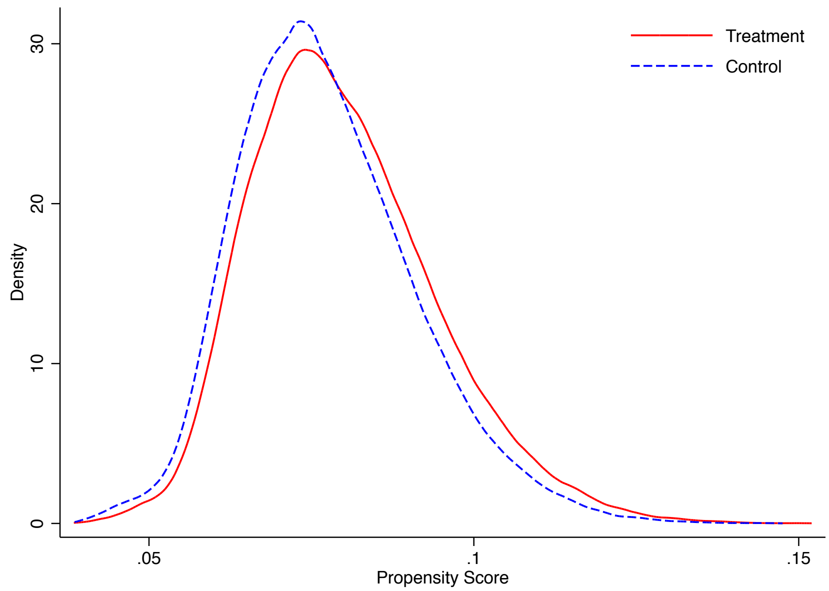

Figure 1: Density Plots of the Propensity Score Distribution For Treatment and Control Groups

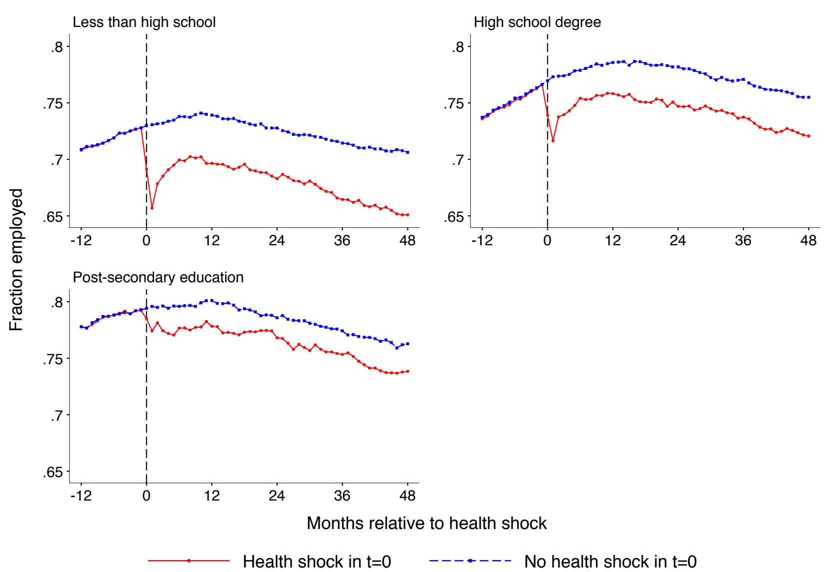

Figure 2: Average Employment over Time Relative to Health Shock by Treatment Status and Education – Weighted by ATE Inverse Propensity Score Weight

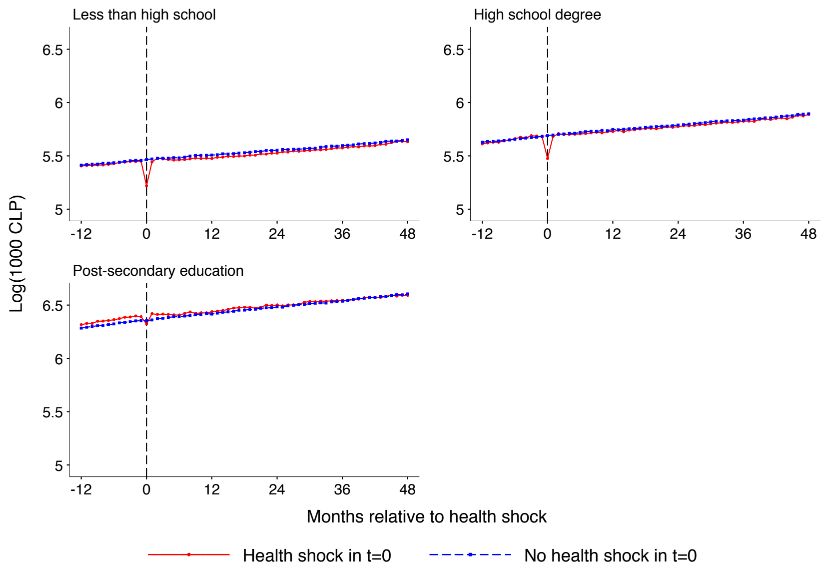

Figure 3: Average Monthly Log-Earnings over Time Relative to Health Shock by Treatment Status and Education – Weighted by ATE Inverse Propensity Score Weight

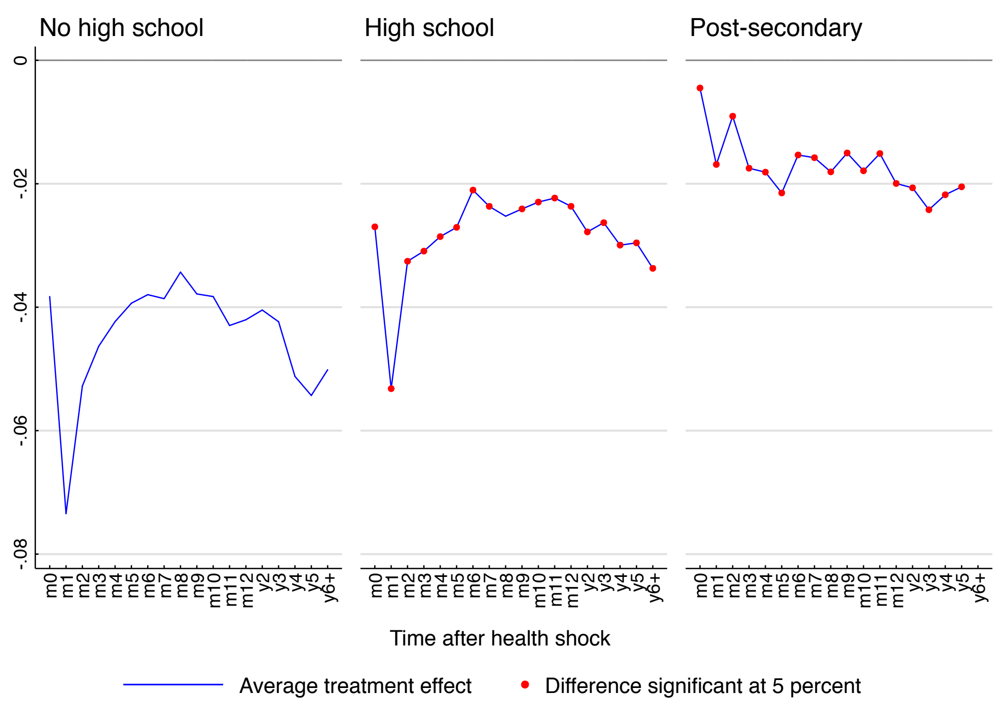

Figure 4: Time-Varying Average Treatment Effects by Education

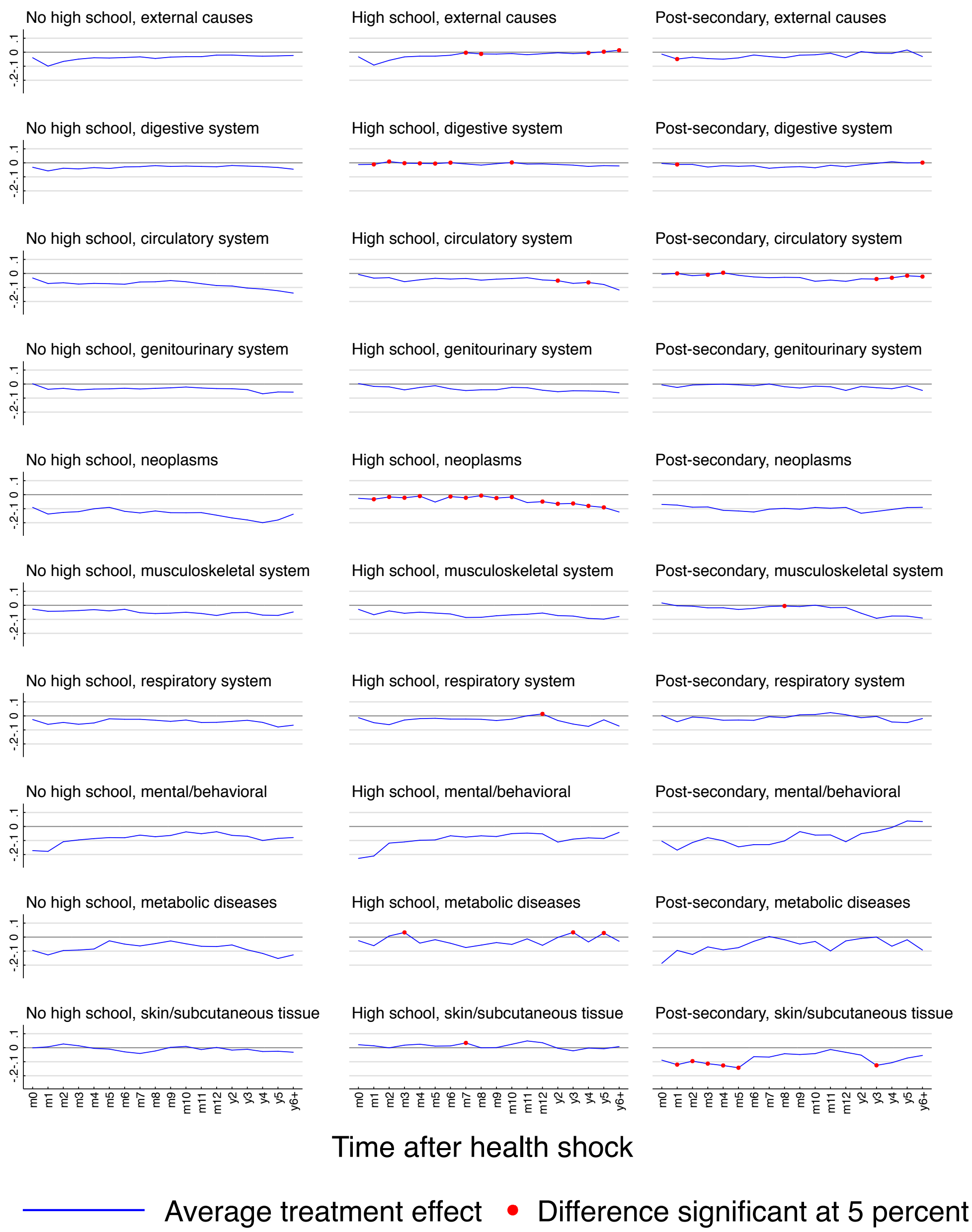

Figure 5: Time-Varying Average Treatment Effects by Education and Diagnosis

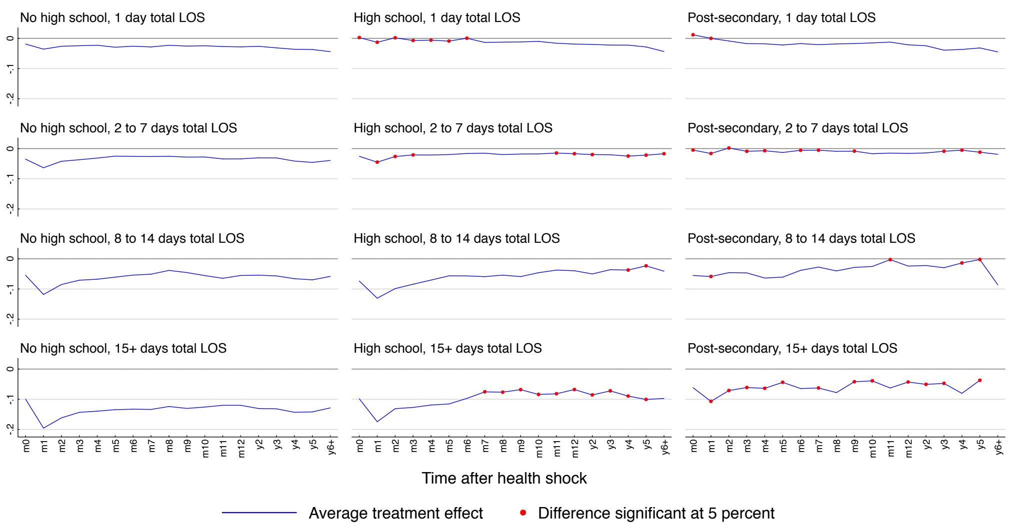

Figure 6: Time-Varying Average Treatment Effects by Education and Total Length of Stay

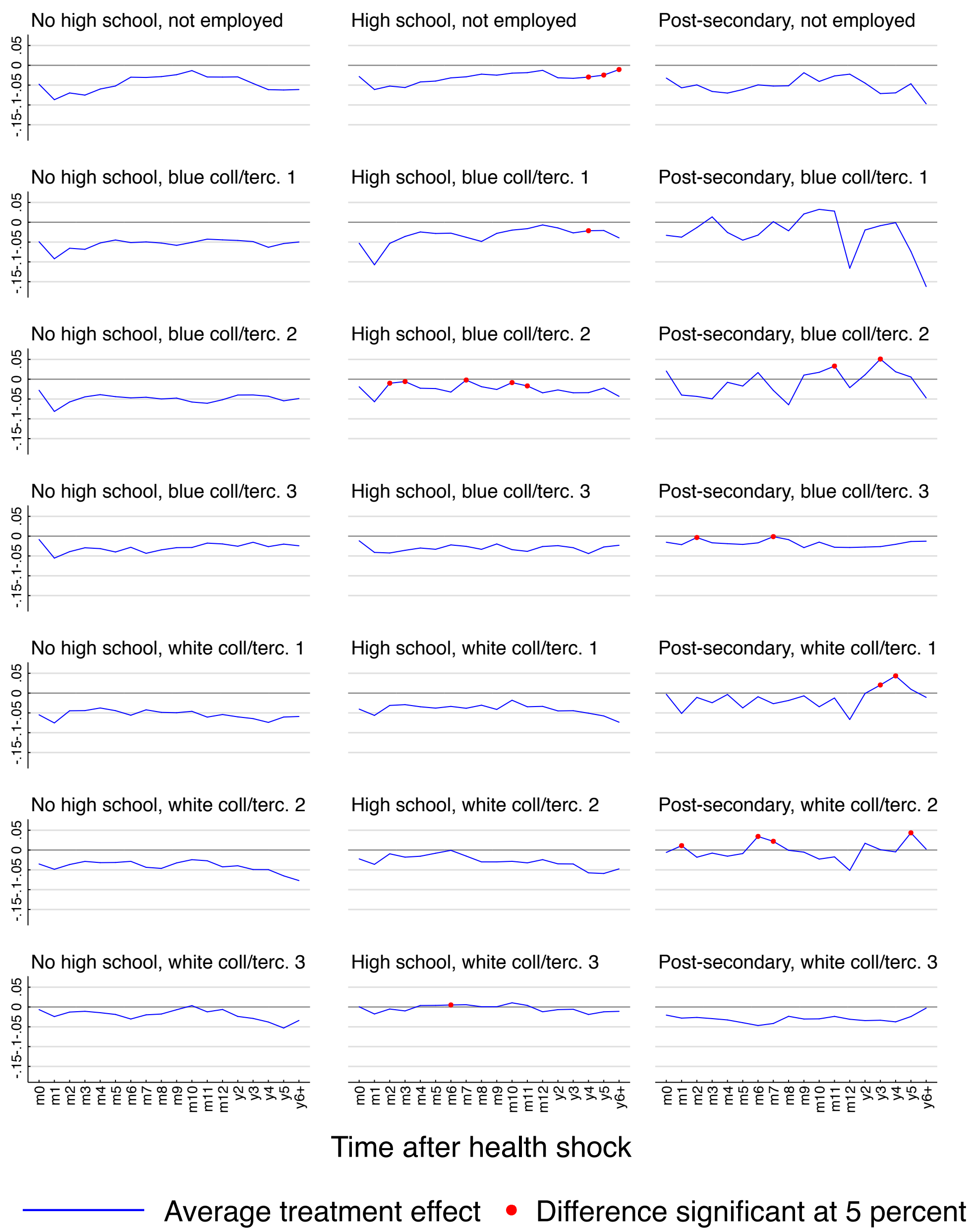

Figure 7: Time-Varying Average Treatment Effects by Education and Labor Market Status in $t = -1$

Table 1: Unweighted and Weighted Means and Normalized Differences of Select Propensity Score Covariates

	(1)	(2)	(3)	(4)	(5)
	Unweighted		Normalized	ATE-weighted	
	Treat.	Control	difference	Treat.	Control
Age at health shock	38.72	37.94	0.0977	37.98	38.00
	(8.088)	(7.856)		(7.876)	(7.877)
Less than high school	0.571	0.539	0.0652	0.542	0.541
	(0.495)	(0.498)		(0.498)	(0.498)
High school degree	0.312	0.327	−0.0321	0.326	0.326
	(0.463)	(0.469)		(0.469)	(0.469)
Post-secondary education	0.117	0.134	−0.0525	0.132	0.133
	(0.321)	(0.341)		(0.338)	(0.340)
Not employed in $t = -1$	0.255	0.251	0.0106	0.251	0.251
	(0.436)	(0.433)		(0.434)	(0.434)
Blue collar industry, earnings tercile 1 in $t = -1$	0.135	0.127	0.0261	0.128	0.127
	(0.342)	(0.333)		(0.334)	(0.333)
Blue collar industry, earnings tercile 2 in $t = -1$	0.122	0.129	−0.0209	0.129	0.128
	(0.327)	(0.335)		(0.335)	(0.335)
Blue collar industry, earnings tercile 3 in $t = -1$	0.120	0.125	−0.0163	0.125	0.125
	(0.325)	(0.331)		(0.330)	(0.331)
Blue collar industry, earnings tercile 1 in $t = -1$	0.128	0.116	0.0354	0.117	0.117
	(0.334)	(0.321)		(0.322)	(0.322)
Blue collar industry, earnings tercile 2 in $t = -1$	0.106	0.118	−0.0380	0.117	0.117
	(0.308)	(0.323)		(0.322)	(0.322)
Blue collar industry, earnings tercile 3 in $t = -1$	0.112	0.113	−0.00199	0.112	0.113
	(0.316)	(0.316)		(0.315)	(0.316)
Fraction of months employed up to $t = -1$	0.721	0.728	−0.0249	0.726	0.727
	(0.264)	(0.262)		(0.262)	(0.263)
Observations	46,485	138,199	184,684	46,485	138,187

Table 2: Health Shock Frequencies and Characteristics by Educational Attainment

	No high school	High school degree	Post-secondary
A. Fraction of men with health shock			
Total	0.0818	0.0743	0.0682
By industry in $t = -1$			
Not employed	0.0866	0.0725	0.0550
Agriculture, fishing	0.0892	0.0798	0.0839
Mining	0.0998	0.103	0.102
Manufacturing	0.0765	0.0756	0.0681
Construction, transportation	0.0760	0.0722	0.0712
Wholesale, retail, restaurants	0.0747	0.0702	0.0682
Finance, real estate	0.0770	0.0720	0.0700
Education, health	0.0962	0.0894	0.0728
Missing industry	0.0811	0.0761	0.0694
Observations	101,025	59,635	24,024
B. Health shock characteteristics			
Diagnosis			
External causes	0.289	0.249	0.176
Digestive system	0.243	0.266	0.250
Circulatory system	0.0711	0.0787	0.0850
Genitourinary system	0.0562	0.0644	0.0850
Neoplasms	0.0275	0.0306	0.0462
Musculoskeletal system	0.0606	0.0753	0.0923
Respiratory system	0.0553	0.0523	0.0614
Mental/behavioral	0.0381	0.0340	0.0289
Endocrine/metabolic	0.0127	0.0113	0.0151
Skin/subcutaneous	0.0326	0.0276	0.0259
Other	0.114	0.111	0.134
Length of stay of first hospitalization			
1 day	0.300	0.351	0.441
2 to 7 days	0.529	0.504	0.473
8 to 14 days	0.112	0.0978	0.0570
15+ days	0.0584	0.0476	0.0289
Length of stay of all hospitalizations for same diagnosis within one year			
1 day	0.263	0.316	0.405
2 to 7 days	0.529	0.503	0.483
8 to 14 days	0.126	0.113	0.0717
15+ days	0.0819	0.0680	0.0406
Health insurance provider			
FONASA A	0.279	0.204	0.0928
FONASA B	0.342	0.275	0.129
FONASA C	0.157	0.183	0.101
FONASA D	0.179	0.232	0.192
ISAPRE	0.0434	0.106	0.486
Observations	26,555	14,489	5,441

Notes: Panel A. shows the fraction of men who had at least one hospitalization between 2005 and 2007 (the treatment group) by education attainment overall and the same fraction separately for each industry, in which they were employed in the month prior to the health shock. Panel B. shows the distribution of health shock characteristics along several dimensions for men who had at least one hospitalization, by educational attainment.

Table 3: Labor Market Outcomes by Treatment and Control Group Before and After the Health Shock

	Treatment		Control	
	Before	After	Before	After
Employed	0.719	0.701	0.733	0.745
	(0.449)	(0.458)	(0.442)	(0.436)
Monthly earnings	241.8	318.8	278.3	336.3
	(312.2)	(417.4)	(347.0)	(401.3)
Months worth of earnings within ±1 year	10.71	11.30	10.42	11.40
	(8.064)	(8.729)	(7.703)	(8.248)
Months worth of earnings within ±2 years	19.86	22.38	19.56	22.84
	(15.02)	(17.30)	(14.50)	(16.55)
Observations	1817999	3122527	10346908	9189052

Notes: Months worth of earnings are defined in equation (1) in the text.

Table 4: Difference-in-Differences Employment and Earnings Regressions With Education Interactions

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment		Monthly Earnings		Total Earnings	
					1 year	2 years
	OLS	FE	OLS	FE	OLS	OLS
High school	0.00700***		0.105***		−1.306***	−2.376***
	(0.00112)		(0.00260)		(0.0392)	(0.0810)
Post-secondary	0.0244***		0.544***		−4.897***	−8.969***
	(0.00169)		(0.00554)		(0.0695)	(0.143)
Treatment	0.0222***		0.430***		5.888***	11.46***
	(0.00312)		(0.00874)		(0.182)	(0.379)
Treat × High school	−0.00684**		−0.0383***		−0.451***	−1.000***
	(0.00214)		(0.00485)		(0.0755)	(0.155)
Treat × Post-secondary	−0.0157***		−0.188***		−2.275***	−4.569***
	(0.00342)		(0.0102)		(0.138)	(0.283)
Post-shock	−0.000136	0.0244***	−0.0280***	0.00314	0.902***	3.127***
	(0.00203)	(0.00183)	(0.00285)	(0.00476)	(0.0239)	(0.0516)
Post-shock × High school	0.0193***	0.0188***	0.0489***	0.127***	0.246***	0.697***
	(0.00170)	(0.00168)	(0.00255)	(0.00423)	(0.0337)	(0.0734)
Post-shock × Post-secondary	−0.0170***	−0.0160***	0.0957***	0.0590***	0.0176	0.205*
	(0.00256)	(0.00252)	(0.00427)	(0.00714)	(0.0408)	(0.0946)
Treat × Post-shock	−0.0464***	−0.0477***	−0.0131***	−0.105***	−0.497***	−1.204***
	(0.00219)	(0.00218)	(0.00306)	(0.00599)	(0.0453)	(0.0976)
Treat × Post-shock × High school	0.0176***	0.0179***	0.00341	0.0474***	0.273***	0.732***
	(0.00354)	(0.00351)	(0.00516)	(0.00925)	(0.0722)	(0.156)
Treat × Post-shock × Post-secondary	0.0238***	0.0240***	0.0123	0.0487**	0.312***	0.797***
	(0.00555)	(0.00554)	(0.00876)	(0.0164)	(0.0884)	(0.201)
Individual controls	Yes	No	Yes	No	Yes	Yes
Year-month dummies	Yes	No	Yes	No	Yes	Yes

Continued on next page

Table 4 – continued from previous page

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment		Monthly Earnings		Total Earnings	
					±1 year	±2 years
	OLS	FE	OLS	FE	OLS	OLS
Cubic time trend	No	Yes	No	Yes	No	No
Individual fixed effects	No	Yes	No	Yes	No	No
(Within-)R^2	0.122	0.00183	0.392	0.00585	0.467	0.429
Number of individuals	184,672	184,672	184,672	184,672	137,057	127,347
Person-month observations	19,611,591	19,611,591	14,333,205	14,333,205	274,114	254,694

Notes: Data are weighted by the inverse propensity score weight w_i^{ATE}, so the displayed coefficients are the diff-in-diff parameters $\hat{\beta}_5^j$ and $\hat{\beta}_6^{j,k}$ (see equation (3)), which correspond to the ATE for the indicated subgroup. Individual controls include age and age squared at time of health shock, education, industry and earnings tercile in the month prior to the health shock, and dummies for year and month of the health shock. Standard errors in parentheses clustered on the individual level. Significance levels: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 5: Difference-in-Differences Estimates From Employment and Earnings Regressions With Education and Diagnosis Interactions

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment		Monthly Earnings		Total Earnings	
					1 year	2 years
	OLS	FE	OLS	FE	OLS	OLS
External causes	-0.0270^{***}	-0.0297^{***}	-0.0109^{*}	-0.0692^{***}	-0.370^{***}	-1.041^{***}
	(0.00367)	(0.00365)	(0.00534)	(0.0105)	(0.0763)	(0.163)
External causes × High school	0.0186^{**}	0.0196^{**}	-0.00421	0.0367^{*}	0.103	0.442
	(0.00618)	(0.00613)	(0.00936)	(0.0171)	(0.131)	(0.277)
External causes × Post-secondary	0.0280^{*}	0.0295^{**}	0.0146	0.0615^{+}	0.369^{*}	1.134^{**}
	(0.0114)	(0.0113)	(0.0184)	(0.0345)	(0.181)	(0.400)
Digestive system	-0.0277^{***}	-0.0272^{***}	0.00000623	-0.0368^{***}	-0.265^{***}	-0.329^{+}
	(0.00382)	(0.00377)	(0.00532)	(0.00988)	(0.0771)	(0.168)
Digestive system × High school	0.0109^{+}	0.0115^{+}	0.00446	0.0215	0.308^{*}	0.460^{+}
	(0.00618)	(0.00607)	(0.00898)	(0.0155)	(0.124)	(0.270)
Digestive system × Post-secondary	0.0216^{*}	0.0189^{+}	0.0165	0.0157	0.00134	-0.287
	(0.0103)	(0.0103)	(0.0155)	(0.0304)	(0.156)	(0.362)
Circulatory system	-0.101^{***}	-0.102^{***}	-0.0260^{*}	-0.233^{***}	-0.722^{***}	-2.103^{***}
	(0.00778)	(0.00777)	(0.0104)	(0.0220)	(0.154)	(0.341)
Circulatory system × High school	0.0385^{**}	0.0391^{***}	-0.00120	0.108^{***}	0.187	0.835^{+}
	(0.0119)	(0.0119)	(0.0163)	(0.0317)	(0.234)	(0.492)
Circulatory system × Post-secondary	0.0574^{**}	0.0575^{**}	-0.0147	0.0939^{+}	0.685^{*}	1.937^{**}
	(0.0175)	(0.0176)	(0.0280)	(0.0549)	(0.293)	(0.633)
Genitourinary system	-0.0473^{***}	-0.0446^{***}	-0.0107	-0.0887^{***}	-0.960^{***}	-1.632^{***}
	(0.00785)	(0.00777)	(0.0111)	(0.0214)	(0.163)	(0.355)
Genitourinary system × High school	0.000173	-0.00154	0.0184	0.0273	0.607^{*}	1.132^{*}
	(0.0125)	(0.0124)	(0.0182)	(0.0321)	(0.245)	(0.542)
Genitourinary system × Post-secondary	0.00184	0.00283	0.0312	0.0487	0.462^{+}	1.138^{+}

Continued on next page

Table 5 – continued from previous page

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment		Monthly Earnings		Total Earnings	
					±1 year	±2 years
	OLS	FE	OLS	FE	OLS	OLS
	(0.0171)	(0.0170)	(0.0280)	(0.0499)	(0.279)	(0.643)
Neoplasms	-0.175^{***}	-0.180^{***}	-0.0118	-0.504^{***}	-1.112^{***}	-3.353^{***}
	(0.0148)	(0.0151)	(0.0167)	(0.0476)	(0.297)	(0.643)
Neoplasms × High school	0.0915^{***}	0.0957^{***}	-0.0211	0.308^{***}	0.972^{*}	2.014^{*}
	(0.0213)	(0.0214)	(0.0275)	(0.0638)	(0.456)	(0.996)
Neoplasms × Post-secondary	0.129^{***}	0.139^{***}	-0.0115	0.421^{***}	1.136^{**}	2.953^{**}
	(0.0257)	(0.0257)	(0.0389)	(0.0794)	(0.420)	(0.941)
Musculoskeletal system	-0.0584^{***}	-0.0604^{***}	-0.0221^{*}	-0.0909^{***}	-1.034^{***}	-2.276^{***}
	(0.00744)	(0.00730)	(0.0105)	(0.0194)	(0.197)	(0.384)
Musculoskeletal system × High school	-0.0150	-0.0132	-0.00163	0.00601	0.517^{+}	1.633^{**}
	(0.0112)	(0.0111)	(0.0162)	(0.0275)	(0.281)	(0.576)
Musculoskeletal system × Post-secondary	0.0142	0.0145	0.0197	-0.00574	1.025^{***}	2.341^{***}
	(0.0160)	(0.0160)	(0.0251)	(0.0468)	(0.300)	(0.670)
Respiratory system	-0.0561^{***}	-0.0582^{***}	-0.0179	-0.121^{***}	-0.501^{**}	-0.912^{*}
	(0.00836)	(0.00838)	(0.0117)	(0.0235)	(0.171)	(0.372)
Respiratory system × High school	0.0299^{*}	0.0287^{*}	0.0319^{+}	0.0557	0.0596	0.465
	(0.0138)	(0.0137)	(0.0190)	(0.0362)	(0.269)	(0.604)
Respiratory system × Post-secondary	0.0463^{*}	0.0495^{*}	0.0529^{+}	0.112^{*}	0.144	0.400
	(0.0198)	(0.0198)	(0.0293)	(0.0559)	(0.355)	(0.779)
Mental/behavioural	-0.0748^{***}	-0.0802^{***}	-0.0678^{***}	-0.259^{***}	-1.417^{***}	-3.492^{***}
	(0.0103)	(0.0104)	(0.0155)	(0.0363)	(0.247)	(0.534)
Mental/behavioural × High school	-0.00858	-0.0105	-0.0458^{+}	-0.0594	-0.310	-0.724
	(0.0172)	(0.0172)	(0.0276)	(0.0582)	(0.406)	(0.791)
Mental/behavioural × Post-secondary	0.0357	0.0330	-0.0132	0.138	0.335	1.154
	(0.0302)	(0.0305)	(0.0486)	(0.0896)	(0.502)	(1.046)

Continued on next page

Table 5 – continued from previous page

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment		Monthly Earnings		Total Earnings	
					±1 year	±2 years
	OLS	FE	OLS	FE	OLS	OLS
Endocrine/metabolic	−0.129***	−0.128***	−0.0613*	−0.407***	−0.814*	−2.107**
	(0.0198)	(0.0198)	(0.0251)	(0.0627)	(0.367)	(0.763)
Endocrine/metabolic × High school	0.128***	0.116***	0.0891*	0.330***	−0.348	−0.0615
	(0.0337)	(0.0344)	(0.0432)	(0.0935)	(0.522)	(1.216)
Endocrine/metabolic × Post-secondary	0.115**	0.111**	0.0439	0.479***	0.691	2.435*
	(0.0360)	(0.0365)	(0.0672)	(0.109)	(0.543)	(1.148)
Skin/subcutaneous	−0.0123	−0.0129	0.000801	−0.0207	0.0406	−0.466
	(0.0103)	(0.0100)	(0.0139)	(0.0259)	(0.184)	(0.451)
Skin/subcutaneous × High school	0.0102	0.0110	0.0185	0.0366	0.271	0.567
	(0.0167)	(0.0165)	(0.0265)	(0.0426)	(0.338)	(0.791)
Skin/subcutaneous × Post-secondary	−0.0490	−0.0492	−0.0163	−0.199+	−0.390	−0.891
	(0.0342)	(0.0351)	(0.0471)	(0.118)	(0.375)	(0.898)
Other diagnosis	−0.0531***	−0.0537***	−0.0118	−0.127***	−0.376**	−1.343***
	(0.00581)	(0.00579)	(0.00755)	(0.0162)	(0.115)	(0.251)
Other diagnosis × High school	0.0322***	0.0313**	0.00564	0.0748**	0.437*	1.392***
	(0.00961)	(0.00965)	(0.0136)	(0.0251)	(0.184)	(0.408)
Other diagnosis × Post-secondary	0.0190	0.0176	−0.00190	0.0268	0.274	0.745
	(0.0140)	(0.0141)	(0.0209)	(0.0411)	(0.220)	(0.514)
Individual controls	Yes	No	Yes	No	Yes	Yes
Year-month dummies	Yes	No	Yes	No	Yes	Yes
Cubic time trend	No	Yes	No	Yes	No	No
Individual fixed effects	No	Yes	No	Yes	No	No
(Within-)R^2	0.124	0.00282	0.394	0.00671	0.470	0.432
Number of individuals	184,672	184,672	184,672	184,672	137,057	127,347
Person-month observations	19,611,591	19,611,591	14,333,205	14,333,205	274,114	254,694

Continued on next page

Table 5 – continued from previous page

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment		Monthly Earnings		Total Earnings	
					±1 year	±2 years
	OLS	FE	OLS	FE	OLS	OLS

Notes: See notes on Table 4.

Table 6: Difference-in-Differences Estimates From Employment and Earnings Regressions With Education and Total Length of Stay Interactions

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment		Monthly Earnings		Total Earnings	
					1 year	2 years
	OLS	FE	OLS	FE	OLS	OLS
1 day total LOS	-0.0326^{***}	-0.0329^{***}	-0.00612	-0.0560^{***}	-0.464^{***}	-0.778^{***}
	(0.00375)	(0.00371)	(0.00531)	(0.00992)	(0.0825)	(0.180)
1 day total LOS × High school	0.0100^{+}	0.0112^{*}	0.00677	0.0376^{*}	0.278^{*}	0.550^{*}
	(0.00578)	(0.00569)	(0.00860)	(0.0147)	(0.124)	(0.270)
1 day total LOS × Post-secondary	0.00120	0.00219	0.00924	0.00718	0.120	0.312
	(0.00834)	(0.00831)	(0.0127)	(0.0240)	(0.138)	(0.323)
2–7 days total LOS	-0.0364^{***}	-0.0374^{***}	-0.0100^{*}	-0.0741^{***}	-0.367^{***}	-0.964^{***}
	(0.00276)	(0.00274)	(0.00390)	(0.00749)	(0.0567)	(0.123)
2–7 days total LOS × High school	0.0153^{***}	0.0142^{**}	0.00260	0.0304^{*}	0.207^{*}	0.736^{***}
	(0.00456)	(0.00453)	(0.00667)	(0.0118)	(0.0933)	(0.204)
2–7 days total LOS × Post-secondary	0.0249^{***}	0.0237^{**}	0.0181	0.0455^{*}	0.231^{+}	0.613^{*}
	(0.00740)	(0.00739)	(0.0117)	(0.0219)	(0.119)	(0.263)
8–14 days total LOS	-0.0612^{***}	-0.0649^{***}	-0.0144^{+}	-0.147^{***}	-0.677^{***}	-1.643^{***}
	(0.00566)	(0.00566)	(0.00770)	(0.0165)	(0.115)	(0.239)
8–14 days total LOS × High school	0.0178^{+}	0.0197^{*}	-0.0158	0.0218	0.151	-0.148
	(0.00948)	(0.00948)	(0.0136)	(0.0264)	(0.190)	(0.385)
8–14 days total LOS × Post-secondary	0.0310^{+}	0.0348^{+}	-0.0425	-0.0137	0.973^{***}	1.178^{*}
	(0.0184)	(0.0184)	(0.0318)	(0.0588)	(0.263)	(0.567)
15+ days total LOS	-0.136^{***}	-0.139^{***}	-0.0684^{***}	-0.453^{***}	-1.323^{***}	-4.014^{***}
	(0.00824)	(0.00825)	(0.0103)	(0.0277)	(0.160)	(0.330)
15+ days total LOS × High school	0.0450^{**}	0.0472^{***}	0.0163	0.190^{***}	0.854^{**}	2.290^{***}
	(0.0138)	(0.0139)	(0.0182)	(0.0435)	(0.262)	(0.524)
15+ days total LOS × Post-secondary	0.0827^{**}	0.0769^{**}	-0.00641	0.146	1.520^{***}	3.578^{***}

Continued on next page

Table 6 – continued from previous page

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment		Monthly Earnings		Total Earnings	
					±1 year	±2 years
	OLS	FE	OLS	FE	OLS	OLS
	(0.0266)	(0.0269)	(0.0444)	(0.0916)	(0.344)	(0.762)
Individual controls	Yes	No	Yes	No	Yes	Yes
Year-month dummies	Yes	No	Yes	No	Yes	Yes
Cubic time trend	No	Yes	No	Yes	No	No
Individual fixed effects	No	Yes	No	Yes	No	No
(Within-)R^2	0.123	0.00251	0.393	0.00678	0.468	0.431
Number of individuals	184,672	184,672	184,672	184,672	137,057	127,347
Person-month observations	19,611,591	19,611,591	14,333,205	14,333,205	274,114	254,694

Notes: See notes on Table 4.

Table 7: Difference-in-Differences Estimates From Employment and Earnings Regressions With Education and Previous Labor Market Status Interactions

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment		Monthly Earnings		Total Earnings	
					1 year	2 years
	OLS	FE	OLS	FE	OLS	OLS
Not employed	-0.0472^{***}	-0.0493^{***}	0.0433^{***}	-0.163^{***}	-0.312^{+}	-0.275
	(0.00467)	(0.00482)	(0.00794)	(0.0202)	(0.180)	(0.365)
Not employed × High school	0.0215^{**}	0.0115	-0.0885^{***}	0.0411	-1.066^{**}	-2.260^{***}
	(0.00801)	(0.00853)	(0.0142)	(0.0355)	(0.330)	(0.665)
Not employed × Post-secondary	0.0111	0.0359^{*}	-0.209^{***}	0.139^{+}	-3.763^{***}	-7.882^{***}
	(0.0153)	(0.0164)	(0.0331)	(0.0780)	(0.543)	(1.117)
Blue collar, earn. terc. 1	-0.0499^{***}	-0.0530^{***}	-0.0173^{**}	-0.102^{***}	-0.213^{**}	-0.654^{***}
	(0.00498)	(0.00498)	(0.00655)	(0.0122)	(0.0733)	(0.151)
Blue collar, earn. terc. 1 × High school	0.00964	0.0110	0.00925	0.0277	-0.468^{**}	-0.546^{+}
	(0.00961)	(0.00986)	(0.0147)	(0.0240)	(0.155)	(0.326)
Blue collar, earn. terc. 1 × Post-secondary	0.0370	0.0342	0.219^{***}	-0.0394	-2.190^{***}	-3.898^{***}
	(0.0285)	(0.0298)	(0.0601)	(0.0860)	(0.320)	(0.614)
Blue collar, earn. terc. 2	-0.0415^{***}	-0.0439^{***}	-0.0475^{***}	-0.0948^{***}	-0.553^{***}	-1.262^{***}
	(0.00513)	(0.00510)	(0.00654)	(0.0117)	(0.0892)	(0.187)
Blue collar, earn. terc. 2 × High school	0.0102	0.0173^{*}	0.0502^{***}	0.0464^{*}	0.450^{**}	1.163^{***}
	(0.00864)	(0.00869)	(0.0119)	(0.0191)	(0.145)	(0.307)
Blue collar, earn. terc. 2 × Post-secondary	0.0301	0.0142	0.353^{***}	0.0587	0.396	0.957^{+}
	(0.0226)	(0.0224)	(0.0345)	(0.0564)	(0.241)	(0.518)
Blue collar, earn. terc. 3	-0.0219^{***}	-0.0262^{***}	0.0494^{***}	-0.0462^{**}	-0.552^{**}	-1.125^{**}
	(0.00594)	(0.00589)	(0.0102)	(0.0149)	(0.192)	(0.404)
Blue collar, earn. terc. 3 × High school	-0.00708	0.00131	-0.0559^{***}	0.00998	0.270	0.606
	(0.00800)	(0.00801)	(0.0148)	(0.0198)	(0.269)	(0.562)
Blue collar, earn. terc. 3 × Post-secondary	0.00768	0.0110	-0.153^{***}	-0.00682	0.108	0.208

Continued on next page

Table 7 – continued from previous page

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment		Monthly Earnings		Total Earnings	
					±1 year	±2 years
	OLS	FE	OLS	FE	OLS	OLS
	(0.0103)	(0.0102)	(0.0172)	(0.0261)	(0.264)	(0.574)
White collar, earn. terc. 1	-0.0635^{***}	-0.0631^{***}	-0.0527^{***}	-0.115^{***}	0.0524	-0.390^{*}
	(0.00628)	(0.00625)	(0.00816)	(0.0145)	(0.0768)	(0.175)
White collar, earn. terc. 1 × High school	0.0284^{**}	0.0294^{**}	0.0368^{**}	0.0703^{**}	-0.269^{*}	-0.0199
	(0.00994)	(0.0102)	(0.0141)	(0.0228)	(0.134)	(0.299)
White collar, earn. terc. 1 × Post-secondary	0.0643^{**}	0.0524^{*}	0.404^{***}	0.0945	-1.519^{***}	-1.786^{***}
	(0.0214)	(0.0218)	(0.0372)	(0.0601)	(0.201)	(0.476)
White collar, earn. terc. 2	-0.0547^{***}	-0.0455^{***}	-0.0847^{***}	-0.0895^{***}	-0.509^{***}	-1.446^{***}
	(0.00665)	(0.00647)	(0.00823)	(0.0143)	(0.105)	(0.231)
White collar, earn. terc. 2 × High school	0.0291^{**}	0.0206^{*}	0.0622^{***}	0.0526^{**}	0.616^{***}	1.411^{***}
	(0.00938)	(0.00939)	(0.0123)	(0.0200)	(0.154)	(0.336)
White collar, earn. terc. 2 × Post-secondary	0.0670^{***}	0.0265	0.343^{***}	0.0314	0.472^{*}	1.614^{***}
	(0.0170)	(0.0172)	(0.0245)	(0.0416)	(0.189)	(0.430)
White collar, earn. terc. 3	-0.0385^{***}	-0.0438^{***}	0.0258^{+}	-0.0861^{***}	-1.784^{***}	-4.220^{***}
	(0.00796)	(0.00794)	(0.0133)	(0.0201)	(0.261)	(0.583)
White collar, earn. terc. 3 × High school	0.0151	0.0258^{*}	0.00138	0.0481^{+}	1.899^{***}	4.026^{***}
	(0.0105)	(0.0106)	(0.0178)	(0.0255)	(0.352)	(0.778)
White collar, earn. terc. 3 × Post-secondary	0.00110	0.00593	-0.0802^{***}	0.0209	2.399^{***}	5.195^{***}
	(0.0107)	(0.0108)	(0.0180)	(0.0279)	(0.308)	(0.686)
Individual controls	Yes	No	Yes	No	Yes	Yes
Year-month dummies	Yes	No	Yes	No	Yes	Yes
Cubic time trend	No	Yes	No	Yes	No	No
Individual fixed effects	No	Yes	No	Yes	No	No
(Within-)R^2	0.123	0.00297	0.397	0.00636	0.475	0.437
Number of individuals	184,672	184,672	184,672	184,672	137,057	127,347

Continued on next page

Table 7 – continued from previous page

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment		Monthly Earnings		Total Earnings	
					±1 year	±2 years
	OLS	FE	OLS	FE	OLS	OLS
Person-month observations	19,611,591	19,611,591	14,333,205	14,333,205	274,114	254,694

Notes: See notes on Table 4.