The Effect of Health Shocks on Employment: Evidence From Accidents in Chile*

Vincent Pohl† — Queen's University

Christopher Neilson — Yale University

Francisco Parro — Ministerio de Hacienda de Chile

October 2013

Abstract

We use administrative employment data and hospital records to estimate the causal effect of health shocks on employment. External health shocks such as accidents serve as a source of exogenous variation. To control for employment trends, we match treatment and control groups on observables and employ a difference-in-differences strategy. Our estimates show that health shocks reduce employment by about three percentage points. Women, individuals with little education, and those with severe shocks experience a higher decrease in employment.

*We thank seminar and conference participants at ATINER, CEA, iHEA, and McGill 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 excellent research assistance.

†Contact: vincent.pohl@queensu.ca.

1 Introduction

Absenteeism due to sickness imposes large costs on firms and workers. While firms experience production loss, workers potentially suffer from lost earnings. Stewart et al. (2003) estimate, for example, that the cost of lost work due to pain conditions alone amounts to over $60 billion per year in the U.S. To devise policies that may reduce this burden it is important to understand the causal effect of health on productivity and earnings losses. While a large literature in health economics estimates the relationship between individuals' health and their labor market outcomes, a causal relationship is difficult to establish. The difficulty arises in part because individuals with low earnings or weak attachment to the labor force may not be able to afford high quality health care and therefore suffer more severe health shocks. In this case, causality may run from earnings to health. In addition, there may be unobserved individual characteristics, such as risk attitude, that are correlated with health and labor market outcomes.

To estimate the effect of health shocks on subsequent employment status, this paper uses administrative employment data combined with hospital records from Chile. We use accidents and other external health shocks that are orthogonal to unobserved determinants of health status and labour market outcomes. In addition, we use individual fixed effects to control for time invariant individual heterogeneity and match individuals who suffered a health shock to a healthy control group. Hence, our estimates have a causal interpretation. In addition to the overall effect of health shocks on employment, we estimate heterogeneous effects by education and health insurance status. These two variables affect both health and labor market outcomes, but they are also potentially endogenous, which makes identification of these effects difficult without using random health shocks.

Most studies on the effect of health on labor market outcomes are based on survey data and are therefore plagued by endogeneity of health measures and measurement error. A typical study uses health measures such as self-assessed health (SAH) and self-reported work limitations. Since survey respondents usually answer questions about their health and labor market status at one point in time it is often difficult to discern the correct timing of events. In this case, the researcher does not know if a health event precedes a change in earnings or vice versa. Etilé and Milcent (2006) find that SAH is related to income when conditioning on clinical health measures, for instance, which implies that SAH could be endogenous to labor market outcomes. In addition, there is evidence that SAH is not a reliable measure of health. For example, Crossley and Kennedy (2002) show that almost a third of respondents change their SAH when asked twice during the same interview. Hence, measurement error is also an issue when using subjective health measures.

Another reason why SAH may be endogenous to labor market outcomes is the so-called justification bias. Respondents use ill health to justify why they are not employed. This issue is particularly relevant in the early retirement literature (for example Bazzoli (1985)). Bound (1991) analyzes whether objective health measures such as mortality can alleviate this problem. He concludes that instrumenting SAH with objective measures yields unbiased estimates of health on labor market outcomes, but may lead to biased estimates of financial variables. Other studies use self-reported health measures deemed more objective than SAH, such as whether respondents have been diagnosed with a specific medical condition. For example, Au et al. (2005) find that the effect of health on employment of older workers is underestimated by using SAH compared to a more objective health index based on self-reports. However, Baker et al. (2004) find considerable measurement error in objective self-reported health measures when validating them with medical records, shedding doubt on the objectivity of specific self-reported health measures. Kalwij and Vermeulen (2008) add to this literature by arguing that instead of instrumenting SAH with more objective health measures the latter should be included alongside SAH to capture the multidimensional nature of health. On the other hand, Dwyer and Mitchell (1999) reject the justification hypothesis when comparing the impact of SAH and objective health measures on retirement plans. Overall, however, the existing literature does not sufficiently answer important policy-relevant questions about the causal relationship between health and labor market outcomes.

Recognizing these limitations, some authors use techniques to generate estimates that may have a causal interpretation. For example, García Gómez and López Nicolás (2006) use Spanish survey data and employ matching techniques to estimate the effect of changes SAH on employment transitions. Cai and Kalb (2006) jointly estimate health and labor supply equations and find that labor force participation has indeed feedback effect on health and health is therefore not exogenous. Another solution to account for the relationship between health and labor market outcomes is to estimate a structural model as done by French (2005) and Bound et al. (2010), who focus on (early) retirement, or Gallipoli and Turner (2011), who investigate the effect of disability on household labor supply.

One way of estimating the causal effect of health on labor market outcomes would be to randomly assign health shocks. This approach would be unethical, but it is possible to randomly assign other types of health interventions. For example, Thomas et al. (2006) provide experimental evidence on the effect of health on labor market outcomes using random assignment of iron supplementation in Indonesia. Although this approach yields causal effects its external validity is limited. In particular, these results do not inform on the effect of negative health shocks.

An alternative to random assignment is a natural experiment using health shocks that are likely exogenous to labor market outcomes. For example, Doyle (2005) exploits car accidents, but his study is concerned with the effect of health insurance status on treatment and subsequent health outcomes and does not consider labor market outcomes. Similarly, Mohanan (2013) uses bus accidents in India. This study estimates the effect of health shocks on financial outcomes such as consumption and debt, but does not consider employment directly. Although Mohanan (2013) provides clean estimates by matching accident victims with an unexposed control group, his study suffers from a small sample size and may also be of limited external validity due to the very specific nature of the health shock. In contrast to the latter paper, we use different types of external health shocks, including traffic accidents, but also other shocks such as injuries due to falling, assault, or fire. Our paper is also the first to use external health shocks to estimate the effect of health on employment.

In contrast to the majority of existing studies, which mostly use survey data, the present paper combines administrative data bases for both health shocks and labor market outcomes. Only a few other studies in this research area also use administrative data. For example, Jeon (2013) matches Canadian cancer registry to tax return data to analyze changes in employment after an initial cancer diagnosis and finds a drop in employment but no longterm effects on earnings conditional on working. In a paper that is probably closest to our own, Lundborg et al. (2011) estimate the effect of hospitalizations on labor market outcomes using Swedish administrative data. In contrast to our paper, they do not only consider accident related hospitalizations, but also health shocks that may be more predictable such as cardiovascular diseases. Moreover, they only have access to annual data, which makes it more difficult to assess the correct sequence of cause and effect. In particular, they cannot rule out a situation where an individual becomes unemployment in January and suffers a health shock in December that is related to the stress caused by the employment transition. By contrast, we observe monthly employment and are therefore able to match health and labor market outcomes much more precisely based on their timing.

There is a large literature on the relationship between health insurance, health, and labor market outcomes (see Currie and Madrian (1999) and Gruber (2000) for overviews). Again, the main problem in establishing causal effects is the potential endogeneity of health insurance because individuals who anticipate negative health shocks are more likely to obtain health insurance. In addition, systems where health insurance is tied to employment (such as in the U.S.) complicate the matter further. One way of dealing with this problem is by exploiting health shocks that are orthogonal to health insurance status as in Doyle (2005). We follow this idea by using external health shocks that are likely unrelated to health insurance status. Therefore we obtain estimates of how the effect of health shocks on employment depends on health insurance. Chile provides an ideal setting because it has a dual health insurance system with public and private insurance providers that differ in terms of quality of health care.

Education has also been linked to labor market outcomes and health. There is less evidence, however, on how education mitigates the effect of health shocks on labor market outcomes. Given the potential endogeneity of both education and health, this question is not an easy one to answer. Using a similar approach to Lundborg et al. (2011), we investigate this question by splitting our sample by education and estimating the effect of external health shocks on employment in each sample separately. Our hypothesis is that individuals with higher levels of education experience a smaller decrease in employment after a health shock. Their human capital likely allows them to find a different job more easily if they cannot work in their pre-shock occupation due an injury.

Our main contributions are the use of detailed administrative data and external health shocks as a source of exogenous variation. To control for employment trends we employ a difference-in-differences strategy. In addition, we match treatment and control groups using a new matching algorithm (Coarsened Exact Matching). These features allow us to estimate causal effects of health shocks on employment that can be used to devise policies aimed at reducing the cost of these shocks. To preview our results, we find substantial effects of external health shocks on employment in the long run. On average, employment decreases by almost three percentage points. Individuals with low schooling levels, women, and those with more severe health shocks experience higher reductions in employment.

The remainder of this paper proceeds as follows. Section 2 provides background on the Chilean health care system. In Section 3, we describe our two data sources, our sampling and matching approach, and provide summary statistics. The empirical strategy is covered in Section 4 and Section 5 contains graphical evidence for the effect of health shocks on employment as well as the regression results. Section 6 concludes.

2 The Chilean Health Care 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 2007, about 70 percent of the Chilean population were enrolled in FONASA and about 17 percent were members of an ISAPRE. Besides these two types of health insurance, there are other reimbursement sources for certain types of health care costs. Expenses resulting form work, school, and transport accidents are covered by respective compensation schemes. Since we use external health shocks, these types of coverage account for a sizable fraction of the health shocks in our sample.

Employees and retirees who are enrolled in FONASA contribute seven percent of their income to insure themselves and their dependents. In addition, FONASA covers uninsured pregnant women and poor or disabled individuals for free. 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. 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. Average contributions amount to 9.2 percent of income. The additional premium is voluntary and buys ISAPRE members additional benefits. In contrast to FONASA beneficiaries, ISAPRE members have access to private facilities but are often restricted to provider networks according to their particular ISAPRE plan. Another important difference between FONASA and ISAPRE are waiting times, which are significantly lower under the latter. Although this probably does not matter for health care obtained immediately after an accident or other external health shock, longer waiting times for rehabilitative services may affect recovery times and therefore employment status.

3 Data

3.1 Data Sources

Our employment 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 can 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 firm's industry. In addition, SC records employees' level of education, sex, year and month of birth, and the date they became affiliated with SC. We have access to the universe of SC records, which comprise 7.1 million individuals. In total, there are 285 million monthly records dating from October 2002 to December 2009.

The health shock data stem from hospital discharge 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, the exact dates of admission and discharge, and if a surgery was performed. The Chilean health ministry collects these records from all hospitals in the country. We classify a hospital stay as caused by an external health shock if the ICD-10 code starts with an S or T (Injury, poisoning and certain other consequences of external causes).[1] In addition to the primary diagnosis, we also observe the secondary diagnosis for most accident victims. These secondary ICD-10 codes start with V, W, X, or Y (External causes of morbidity) and denote the type of the accident or other event.[2] Appendix Tables A.1 to A.10 contain detailed distributions of primary and secondary diagnoses by sex, education, and health insurance provider and length of hospital stay by diagnosis.[3]

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.[4] Table 1 shows the sizes

[1] See http://www.icd10data.com/ICD10CM/Codes for a list of all ICD-10 codes. For example, the code S52 denotes "fractures of the forearm" and may be subdivided into several specific fracture locations within the forearm.

[2] The codes are very specific. Examples include V03.12 "Pedestrian on skateboard injured in collision with car, pick-up truck or van in traffic accident" or X00.2 "Injury due to collapse of burning building or structure in uncontrolled fire."

[3] The current version of this paper does not yet contain estimation results for different diagnoses, but these results will be added in a future version.

[4] We carried out all empirical analyses on a secure server at the Chilean finance ministry. The

of the initial and final estimation samples. Out of the 7.1 million individuals in the employment data, we select those who became affiliated with SC before 2004 and have at least 12 months of non missing earnings. This sample restriction ensures that we observe individuals before the hospital data begin and drop those who may have only held a short-term job. Moreover, we restrict the sample to individuals born between 1944 and 1983, so they are at most 65 and least 19 years old during the sample period. These restrictions leave us with 1.7 million individuals.

The hospital data contain records from 2.3 million individuals. For about 215,000 of them an external health shock is the cause for their first hospital stay during 2004 and 2007. We are able to match about 52,000 of them to SC records. These individuals constitute the treatment group.[5] That leaves 1.5 million individuals as potential control group members, i.e., those who did not have any hospital stay between 2004 and 2007. The following subsection describes how we select a control group from these individuals.

3.2 Sampling and Matching

There are two reasons why we do not simply use all individuals who did not have an accident or any other hospital stay between 2004 and 2007 as the control group. First, running regressions with 1.5 million individual fixed effects would be very time consuming. By randomly sampling a subset from the group of individuals without health shocks, we can decrease computational time without sacrificing a lot of precision. Second, we need a way to assign placebo shocks to control group members. That is, when comparing employment outcomes before and after the health shock, we need to construct a control group that has pre- and post-outcomes too, where the pre- and post-periods are defined by a placebo health shock.

Therefore, we construct treatment and control groups as follows. For each month from January 2004 to December 2007, we drop all individuals who were employed less than six months during the previous year. Then we select the individuals who had a hospital stay due to an external health shock during that month as part of the treatment group. To select members of the control group, we randomly sample 0.2 percent of individuals who did not have any health shock between 2004 and 2007.[6,7] We stratify this random sample by sex, age, education, and cumulative employment at the time of the placebo health shock. Cumulative employment is expressed as number of years employed prior to the placebo accident. The treatment and control groups consist of all individuals that are selected in each month. Our estimation sample, which consists of all treatment group members and the control group sample, includes about 136,000 individuals, and we have a total of 11 million

authors are not able to identify individuals from the matched data. The project was granted IRB approval by Queen's University.

[5]Besides individuals born before 1944 and after 1983 there are also patients in the hospital records who have never been affiliated with SC.

[6]Since there are 48 months in the sample period, sampling 0.2 percent for each month corresponds roughly to a ten percent sample overall.

[7]Since it is possible that the same individual is sampled for the control group in more than one month, we cluster standard errors in our regression on individual identifiers (RUT).

monthly observations (see Table 1).

To match treatment and control group individuals, we use Coarsened Exact Matching (CEM), a matching algorithm developed by Iacus et al. (2012). The basic idea of this algorithm is to assign matching weights that reflect differences in the distributions of observable characteristics between the treatment and control groups. To this end, observables are discretized into a small number of strata. In particular, the CEM weights are defined as follows:

$$w_i = \begin{cases} 1 & \text{if } i \in T^s \\ \frac{N_C}{N_T}\frac{N_T^s}{N_C^s} & \text{if } i \in C^s \end{cases}, \tag{1}$$

where T^s and C^s denote the sets of individuals who are in stratum s in the treatment and control group, respectively. N_T and N_C are the numbers of individuals in the treatment and control groups, respectively, and N_T^s and N_C^s are the numbers treatment and control observations in stratum s. In our case, strata are defined by the intersection of sex (two categories), education (five categories), cohort (eight categories with five cohorts each), and cumulative employment before the (placebo) health shock (six categories from less than one year to more than five years).

The weights in equation (1) apply to a situation where weights are assigned to an entire sample. Since we select a random sample from the control group, we have to adjust these weights accordingly.[8] Denoting the total (over all months) number of control group members sampled according to the process outlined above by n_C and the number of sampled control group members in stratum s by n_C^s, the adjusted CEM weights are

$$w_i = \begin{cases} 1 & \text{if } i \in T^s \\ \frac{N_C}{N_T}\frac{N_T^s}{n_C^s} & \text{if } i \in C^s \end{cases}. \tag{2}$$

Using these adjusted weights, the sum of w_i over all sampled individuals equals $N = N_T + N_C$ again.

3.3 Summary Statistics

We are particularly interested how individuals' outcomes differ by education and health insurance provider. First, Table 2 shows the average unweighted characteristics of treatment and control groups in the first two columns. Overall, men are overrepresented in our sample, even more so in the treatment group, which is not surprising since men are more exposed to factors that can cause accidents. Chilean workers do not have high levels of education. About half the sample does not have a high school degree and about ten percent have a degree from a technical school or university. Treatment group members are less educated than control group members, which reflects sorting into more dangerous jobs by education. The age distribution of individuals with and without accident is similar. The third column shows that our matching algorithm makes the distribution of observables more similar between treatment and control groups. The match is not exact, however, since we match on the month level and Table 2 shows averages over all sample months.

[8] Note that the sum of w_i over all i in equation (1) equals $N = N_T + N_C$.

Table 3 shows summary statistics for three month-level outcomes of interest by education for the whole sample[9] There is a clear education gradient for both employment and earnings. Individuals without a high school degree are employed 80 percent of all months while those with a high school or higher degree are employed in about 84 percent of all months. Monthly earnings range from 236,000 CLP for individuals without any degree to 772,000 CLP for those with a university degree. Note that these averages contain zero earnings for months when individuals where not employed.

The education gradient is also reflected in the first four rows of Table 4 that show employment and earnings prior to the accident for members of the treatment group. Moreover, individuals with higher levels of education are more likely to be enrolled in ISAPRE and less likely to be enrolled in FONASA. In particular, individuals with a university degree are more than ten time more likely to have ISAPRE coverage than individuals without any degree. The propensity that work, school, or transport accident insurance cover a hospital stay is similar across education levels. The severity of accidents, as measured by the length of stay is highest for those with the least amount of education. However, technical school and university graduates are more likely to undergo surgery. These relationships could also reflect the shorter average hospital stays for ISAPRE patients (see below).

Table 5 contains summary statistics by insurance provider for the treatment group. The relationship between education and FONASA/ISPARE coverage is also apparent here. In addition, ISAPRE members are more likely to be employed and have higher earnings levels. Their average pre-accident monthly earnings are over three times as high as those of FONASA patients. As mentioned above, ISAPRE members have shorter average lengths of stay and are more likely to undergo surgery. The latter discrepancy between ISAPRE and FONASA patients reflect differences in access to health care. ISAPRE members receive better and more efficient care that allows them to leave the hospital sooner but also obtain more costly procedures that may affect subsequent outcomes.

We control for individuals' industry in the regressions below. The distribution of industry by education and insurance provider is shown in Appendix Tables A.11 and A.12.

4 Estimation Strategy

We estimate the long-run effect of external health shocks on employment using a difference-in-differences (DID) framework. In particular, we estimate the following regression:

$$E_{it} = \alpha \mathbf{1}\{t > s\} HS_{is} + X_{it}'\beta + \gamma_i + \delta_t + \epsilon_{it}, \tag{3}$$

where $E_{it} = \{0, 1\}$ is employment of individual i in month t, $HS_{is} = 1$ if i had an external health shock in month s, and $\mathbf{1}\{t > s\}$ is an indicator variable that equals one for time periods that are after the health shock. We include only months

[9] All summary statistics in the remainder of this section are weighted by the adjusted CEM weights shown in equation 2.

strictly after the health shock month in the post period because individuals may be employed in the month of the health shock before the shock occurred.[10] Each time period corresponds to a calendar month. γ_i and δ_t are individual and time fixed effect, respectively. Hence, α is the DID parameter of interest. X_{it} contains time varying variables such as industry and age. All regressions are weighted by the CEM weights w_i in equation (2). We also estimate equation (3) for subsamples defined by sex and education. We estimate all of these regressions as linear probability models, which is necessary due to the individual fixed effects. Standard errors in all regressions are clustered on the individual level to account for potentially dependent time-specific error terms.

In addition, we include interactions in the DID regression as follows:

$$E_{it} = \sum_{k=1}^{K} \alpha^k \mathbf{1}\{t > s\} HS_{is} D_i^k + X_{it}'\beta + \gamma_i + \delta_t + \epsilon_{it}, \tag{4}$$

where the D_i^k are indicator variables for health insurance status and length of the hospital stay (one day, up to one week, one to two weeks, more than two weeks), respectively. Hence, α^k is the effect of an external health shock on employment for individuals who have a specific health insurance provider or length of stay.

5 Results

5.1 Graphical Evidence

First, we present graphical evidence for the effect of accidents on employment. The following graphs plot average monthly employment for treatment and control group members for 12 months before and 36 months after the (placebo) health shock. We sample control group members as described in Section 3.2 and use the adjusted CEM weights to calculate average employment.

Figure 1 shows average employment of treatment and control groups by sex. Overall, there is a decreasing trend in employment for all individuals, although it is more pronounced for women than for men. This pattern is due to the nature of the employment data. Once employees become affiliated with SC they stay in the system even when they quit or lose their job. Individuals for whom no earnings are reported are not necessarily unemployed, however. Rather, they may work in the informal sector, which is sizable in Chile. Not knowing if individuals are unemployed or employed in the informal section is clearly not ideal since we do not know if an apparent decrease in employment after a health shock means that an individual does not have a job. We assume that the likelihood of working in the informal sector is independent of health shocks.

Keeping this caveat in mind, Figure 1 shows that external health shocks have an economically significant negative effect on employment for both men and women.

[10] Recall that we count individuals as employed who had positive earnings in a given month. Individuals who stop working after a health shock may still have positive earnings in the same month and therefore be classified as employed.

Average employment drops from almost 90 percent before to about 80 percent in the month following the health shock. Among women, employment keeps decreasing during the entire three-year follow-up period until it reaches less than 70 percent. Men's employment also decreases after a small rebound and then decreases at at lower rate than women's. Control group employment after the placebo health shock decreases at roughly the same rate but from a higher level. As shown in the graph, the difference in employment levels in the post-period is due to the decrease in employment in the month immediately after the health shock. This difference narrows slightly for men and stays constant or even widens for women. Hence, these two graphs provide clear evidence for a causal negative effect of health shocks on employment.[11]

Figure 2 shows the same employment plots by age groups. Again, average employment is slightly higher in the control group before the health shock, and there is a general downward trend in employment over time. Given these general patterns, external health shocks have a substantial negative effect on employment across all ages, but the impact increases with age. The initial effect for individual in their 50s is almost twice as large as for those aged 30 to 39. Since we do not have as many observations for individuals older than 60, the effects are not as clear among them. This finding shows that an accident or external health shock has more severe implications for older employees who may already have other health problems. It may also be more difficult for older workers to retrain for a different type of job if an injury makes working in their previous occupation impossible.

Next we investigate if different education levels impact the employment effect of health shocks. Figure 3 shows that higher levels of education are indeed associated with a smaller decrease in employment after a health shock. The drop in employment is particularly large for individuals without a high school degree. This finding confirms our hypothesis that higher levels of education have a protective effect. Two pathways seem plausible. First, individuals with higher levels of education have more general human capital and can therefore transition more easily into a different type of occupation if the health shocks makes employment in their previous job impossible.[12] Moreover, highly educated individuals are less likely to work in a job that requires physical effort and may therefore be affected less by an injury. Second, highly educated individuals tend to be healthier before the health shock, so they can cope better with injuries. As shown in Table 4, employees with higher degrees are also more likely to be insured by ISAPRE and have access to better health care. Finally, education is also related to behavior, such as following doctor's orders, that may be conducive to a faster recovery after an injury.

Finally, we compare the effect of external health shocks on employment across individuals with different health insurance providers. Since we obtain health insurance information from the hospital records, we cannot plot control group employment by insurance status. Since ISAPRE members have access to better health care than

[11] The pre-shock employment trends of treatment and control group do not match perfectly, which will be improved in future work through better matching weights.

[12] Since our data do not contain information on individuals' occupation, we cannot directly test this hypothesis.

FONASA beneficiaries, we hypothesize that the effects of a health shock on employment are more severe among the latter. Figure 4 backs this conjecture. Pre-shock employment is about 10 percentage points higher among ISAPRE members (see also Table 5), but the drop in employment is also significantly smaller than among those enrolled in FONASA. The decrease in employment among employees covered by work, school, or transport accident insurance ranges between the FONASA and ISAPRE effects. Overall, we conclude that access to better health care results in a smaller decrease in employment after a health shock.

5.2 Regression Results

We now discuss the regression results based on estimating equations (3) and (4). All regressions control for industry, second-order polynomial in age and the number of months since affiliation with SC, and indicator variables for each time period (month). We begin with the results from the DID regression using the whole sample, displayed in column (1) of Table 6. The coefficient of interest is the interaction between treatment status and the post-health shock indicator, which is estimated to be -0.028. In words, individuals are 2.8 percentage points less likely to be employed after an external health shock. This estimate is highly significant as are most other coefficients that we estimate. Comparing this estimate to the graphical evidence presented in the previous section, we note that accounting for observed characteristics and unobserved heterogeneity through fixed effects reduces the effects considerably. This difference is not surprising since both observed and unobserved individual characteristics may be correlated with the propensity of suffering an external health shock.

To investigate potential heterogeneity in the effect of health shocks on employment, we split the sample by sex and education. Columns (2) and (3) of Table 6 show that women's employment decreases more than men's after a health shock, confirming the findings from Figure 1. The difference amounts to 1.3 percentage points, which is statistically and economically significant. Women are less likely to suffer an external health shock, but if they do the impact is more severe. A possible explanation is that men are more likely to be the only breadwinner in the family, so they need to maintain an income even after suffering an injury.

The remaining columns of Table 6 split the sample by level of education. The estimated DID coefficients confirm the findings from Figure 3: higher levels of education lead to a smaller decrease in employment after the health shock. Individuals without a high school degree reduce their employment by 3.6 percentage points whereas the drop among university graduates is only 2.1 percentage points. Employees who attended technical schools do not show a significant reduction in employment at all.[13] It is surprising that university graduates decrease their employment more than those of technical school although the former have a more general form of human capital. It may be the case that graduates of technical schools hold jobs

[13] Since there are fewer individuals with high levels of education, this result could be due to a lack of power, but it is actually the smaller point estimate that leads to the insignificance of the parameter estimate.

that are easier to keep after suffering an injury.

We estimate heterogeneous effects by health insurance provider by including interactions between the DID indicator and dummies for insurance providers as shown in equation (4).[14] Table 7 reports the estimates for the insurance interactions. Focusing on FONASA and ISAPRE members, we see that the effect of a health shock on employment is larger among the latter (-3.6 and -5.0 percentage points, respectively). This result is surprising because it is the opposite of the graphical evidence shown in Figure 4. Without having information on health insurance coverage of the control group it is not clear how to address this discrepancy, but a further split of the sample, for example by education, may also shed light on this counterintuitive result.

Finally, we include interactions between the DID term and length of hospital stay in Table 8. Length of stay is a proxy for the severity of the injury sustained from an external health shock. The estimated pattern is as expected: the longer the hospital stay, the larger the negative impact of health shocks on employment. While the estimates range around -2 percentage points for individuals who stay in the hospital for up to one week, the decrease in employment amounts to 10 percentage points for those with stays of more than two weeks. This result clearly shows that mitigating the severity of health shocks can lead to much smaller degrees of employment reductions and absenteeism.

6 Conclusion

We use administrative data from Chile to estimate the causal effect of external health shocks such as accidents on employment status. Our findings show that individuals suffering from injuries experience a substantial decrease in employment. Subgroups that are particularly affected are women, individuals with low levels of schooling, and those having severe health shocks leading to long hospital stays. These results are policy relevant because they allow us to quantify the labor market effects of health. In contrast to most existing studies, our results have a causal interpretation due to the nature of our data and the health shocks. A potential application would be to evaluate the benefits of reducing the risks of certain accidents or of making the impact of accidents less severe.

The results by education subgroups are particularly interesting because they point to a type of return to education that is usually neglected. In addition to increasing employment and earnings, higher levels of education also decrease the negative effect of suffering a health shock. Hence, education reduces risk related to health events. Another policy implication of this result is that increasing education would further improve the welfare of risk averse individuals. Moreover, to the extent that a decrease in employment due to health shocks results in costs to society, investing in more education would have positive welfare effects due to this pathway.

To quantify the cost of employment changes due to health shocks, it is necessary

[14]Since we only observe the source of health insurance for members of the treatment group, we cannot split the sample by health insurance as we do for education.

to consider the effect on earnings. We have monthly earnings in our data and have done some preliminary analyses using earnings as the dependent variable in DID regressions. Since earnings seem to be noisy given the administrative source of our data, these regressions require some further work and results will be presented in a future version of this paper.

References

Au, D., Crossley, T., and Schellhorn, M. (2005). The effect of health changes and long-term health on the work activity of older Canadians. *Health Economics*, 14:999–1018.

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

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

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

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

Cai, L. and Kalb, G. (2006). Health status and labour force participation: evidence from Australia. *Health Economics*, 15(3):241–261.

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

Currie, J. and Madrian, B. C. (1999). Health, Health Insurance and the Labor Market. In Ashenfelter, O. C. and Card, D., editors, *Handbook of Labor Economics*, pages 3309–3416. Elsevier Science.

Doyle, J. (2005). Health Insurance, Treatment and Outcomes: Using Auto Accidents as Health Shocks. *Review of Economics and Statistics*, 87(2):256–270.

Dwyer, D. and Mitchell, O. (1999). Health problems as determinants of retirement: Are self-rated measures endogenous? *Journal of Health Economics*, 18(2):173–193.

Etilé, F. and Milcent, C. (2006). Income-related reporting heterogeneity in self-assessed health: evidence from France. *Health Economics*, 15(9):965–981.

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

Gallipoli, G. and Turner, L. (2011). Household Responses to Individual Shocks: Disability and Labor Supply.

García Gómez, P. and López Nicolás, A. (2006). Health shocks, employment and income in the Spanish labour market. *Health Economics*, 15(9):997–1009.

Gruber, J. (2000). Health Insurance and the Labor Market. In Culyer, A. J. and Newhouse, J. P., editors, *Handbook of Health Economics*, pages 645–706. Elsevier Science.

Iacus, S. M., King, G., and Porro, G. (2012). Causal Inference without Balance Checking: Coarsened Exact Matching. *Political Analysis*, 20(1):1–14.

Jeon, S.-H. (2013). The long-term effects of cancer on employment and earnings of cancer survivors.

Kalwij, A. and Vermeulen, F. (2008). Health and labour force participation of older people in Europe: What do objective health indicators add to the analysis? *Health Economics*, 17(5):619–638.

Lundborg, P., Nilsson, M., and Vikström, J. (2011). Socioeconomic Heterogeneity in the Effect of Health Shocks on Earnings: Evidence from Population-Wide Data on Swedish Workers.

Mohanan, M. (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.

Stewart, W. F., Ricci, J. A., Chee, E., Morganstein, D., and Lipton, R. (2003). Lost productive time and cost due to common pain conditions in the US workforce. *Journal of the American Medical Association*, 290(18):2443–2454.

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

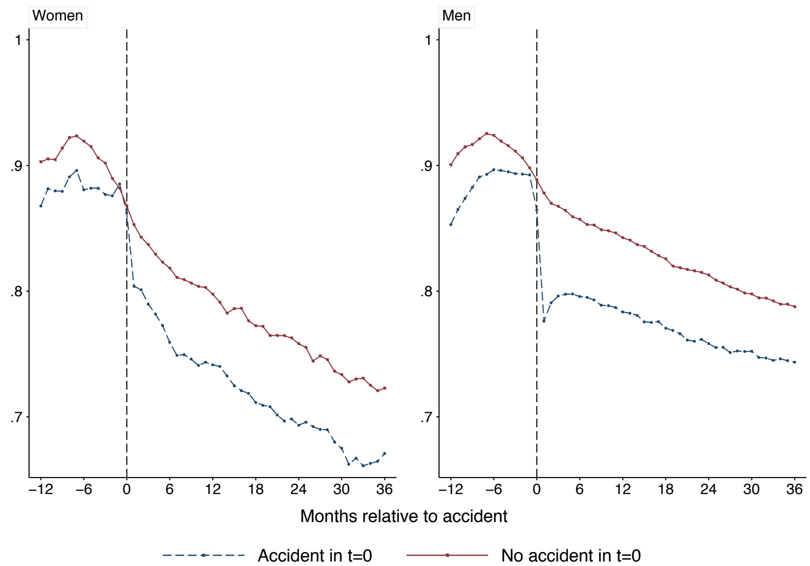

Figure 1: Employment of Treatment and Control Groups Before and After (Placebo) Accident by Sex

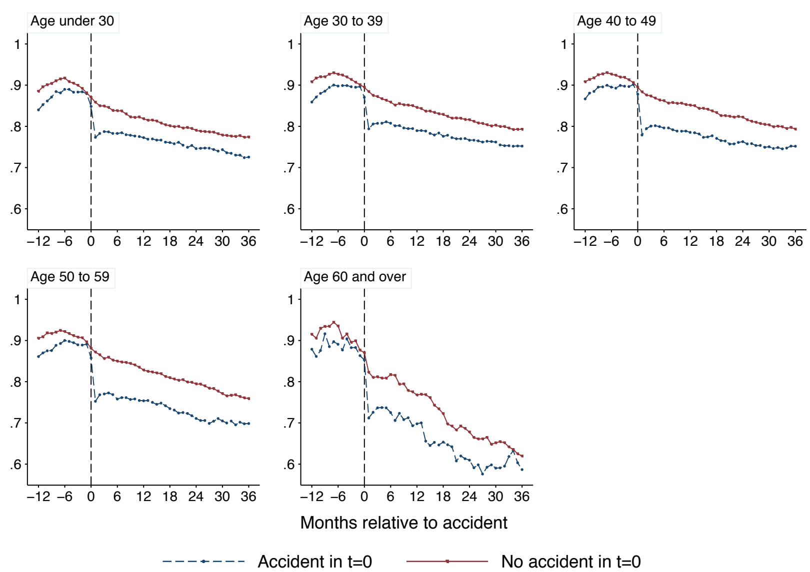

Figure 2: Employment of Treatment and Control Groups Before and After (Placebo) Accident by Age Group

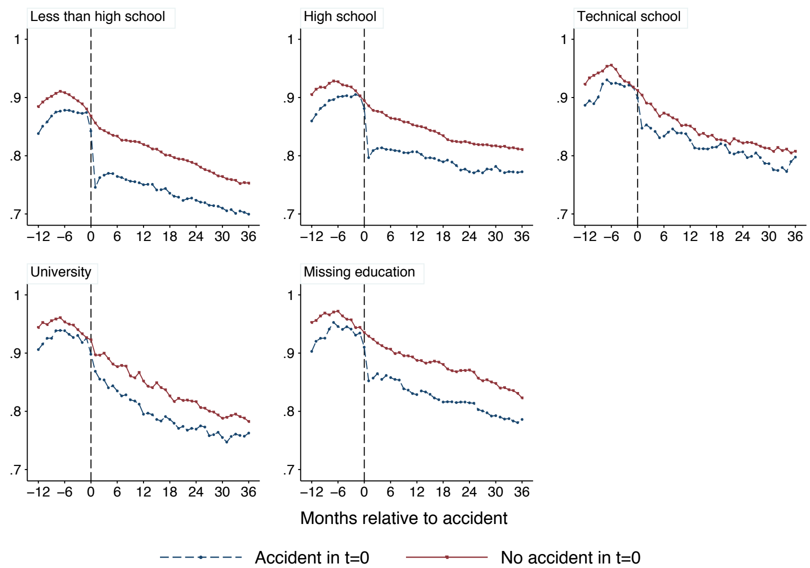

Figure 3: Employment of Treatment and Control Groups Before and After (Placebo) Accident by Education

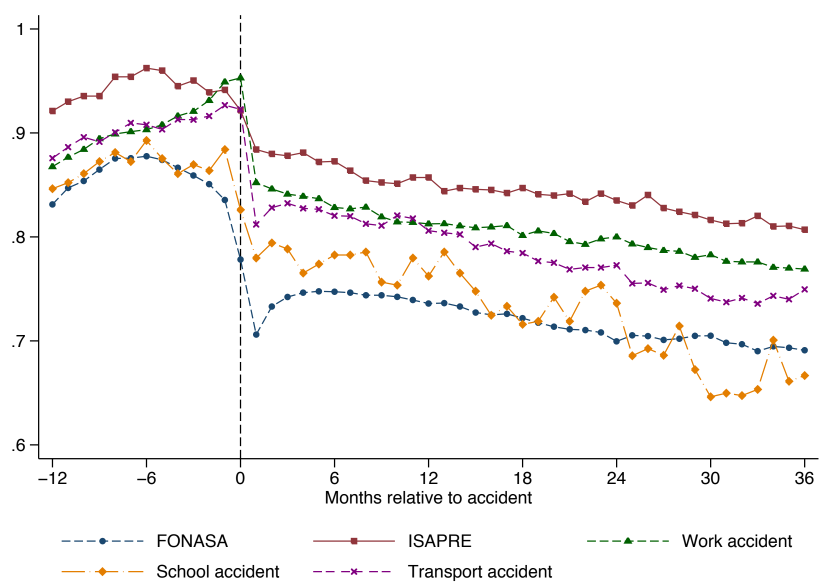

Figure 4: Employment of Treatment Group Before and After Accident by Health Insurance Provider

Table 1: Sample Sizes

Sample description	Sample size
Individuals in UI data	7,168,005
UI affiliation start date before 2004	2,176,560
At least 12 months of nonmissing earnings	1,867,970
Cohorts 1944 to 1983	1,749,629
Individuals with hospital stay between 2004 and 2007	2,263,548
Accident related hospital stay	215,703
Individuals with accident matched to earnings data (potential treatment group) [1]	51,871
Individuals in UI data without hospital stay (potential control group) [2]	1,474,445
Full sample [1] + [2]	1,526,316
Treatment group sample (at least six out of 12 months employed before health shock) [3]	36,823
Control group sample (at least six out of 12 months employed before placebo health shock and in random sample) [4]	99,394
Estimation sample [3] + [4]	136,217
Number of monthly observations	11,169,445

Table 2: Summary Statistics of Matching Variables

	Treatment	Control	
		unmatched	matched
Gender (1=male, 0=female)	0.886	0.770	0.885
	(0.318)	(0.421)	(0.319)
Education: less than high school	0.528	0.453	0.512
	(0.499)	(0.498)	(0.500)
Education: high school	0.311	0.309	0.318
	(0.463)	(0.462)	(0.466)
Education: technical school	0.044	0.063	0.046
	(0.206)	(0.243)	(0.209)
Education: university	0.046	0.074	0.052
	(0.210)	(0.261)	(0.221)
Education: missing	0.071	0.102	0.072
	(0.256)	(0.302)	(0.259)
Cohorts 1944 to 1948	0.028	0.026	0.028
	(0.165)	(0.160)	(0.165)
Cohorts 1949 to 1953	0.047	0.050	0.049
	(0.212)	(0.219)	(0.217)
Cohorts 1954 to 1958	0.083	0.088	0.084
	(0.276)	(0.284)	(0.277)
Cohorts 1959 to 1963	0.120	0.132	0.125
	(0.325)	(0.338)	(0.331)
Cohorts 1964 to 1968	0.159	0.155	0.163
	(0.366)	(0.361)	(0.369)
Cohorts 1969 to 1973	0.176	0.169	0.181
	(0.381)	(0.375)	(0.385)
Cohorts 1974 to 1978	0.184	0.188	0.181
	(0.388)	(0.391)	(0.385)
Cohorts 1979 to 1983	0.203	0.192	0.190
	(0.402)	(0.394)	(0.392)
Observations	36823	99394	87890

Table 3: Summary Statistics by Education for Whole Sample

	No high school	High school	Tech. school	University	Missing educ.
Employed	0.797	0.836	0.847	0.847	0.864
	(0.403)	(0.371)	(0.360)	(0.360)	(0.343)
Monthly earnings	235.8	299.8	466.1	771.8	422.3
	(233.1)	(284.5)	(438.5)	(639.0)	(458.7)
Accident	0.0121	0.0122	0.0122	0.0123	0.0125
	(0.109)	(0.110)	(0.110)	(0.110)	(0.111)
Observations	5149932	3284983	475394	535278	788514

Table 4: Summary Statistics by Education for Treatment Group

	No high school	High school	Tech. school	University	Missing educ.
Employment in pre-shock month	0.858	0.886	0.903	0.910	0.915
	(0.349)	(0.317)	(0.296)	(0.286)	(0.280)
Average pre-shock employment	0.789	0.814	0.835	0.859	0.845
	(0.194)	(0.188)	(0.191)	(0.186)	(0.206)
Earnings in pre-shock month	226.1	286.2	445.6	759.3	363.8
	(193.2)	(249.6)	(408.4)	(617.2)	(405.9)
Average pre-shock earnings	187.9	234.4	362.8	647.5	303.1
	(131.0)	(176.8)	(311.6)	(526.9)	(338.9)
FONASA	0.490	0.438	0.350	0.250	0.411
	(0.500)	(0.496)	(0.477)	(0.433)	(0.492)
Work accident	0.315	0.330	0.311	0.291	0.291
	(0.465)	(0.470)	(0.463)	(0.454)	(0.454)
School accident	0.00906	0.0115	0.00918	0.0188	0.0100
	(0.0947)	(0.107)	(0.0954)	(0.136)	(0.0996)
Transport accident	0.134	0.138	0.132	0.101	0.152
	(0.340)	(0.345)	(0.339)	(0.301)	(0.359)
ISAPRE	0.0223	0.0458	0.142	0.270	0.0990
	(0.148)	(0.209)	(0.349)	(0.444)	(0.299)
Other provider	0.0295	0.0358	0.0557	0.0699	0.0370
	(0.169)	(0.186)	(0.229)	(0.255)	(0.189)
Length of stay (days)	5.476	5.118	3.898	3.507	5.020
	(10.43)	(11.28)	(7.088)	(6.027)	(9.549)
Surgical intervention	0.414	0.449	0.498	0.547	0.508
	(0.493)	(0.497)	(0.500)	(0.498)	(0.500)
Observations	19430	11460	1634	1702	2597

Table 5: Summary Statistics by Insurance Provider for Treatment Group

	FONASA	Work acc.	School acc.	Transport acc.	ISAPRE	Other
Less than high school	0.573	0.525	0.462	0.523	0.228	0.445
	(0.495)	(0.499)	(0.499)	(0.500)	(0.419)	(0.497)
High school	0.302	0.324	0.346	0.320	0.275	0.318
	(0.459)	(0.468)	(0.476)	(0.466)	(0.447)	(0.466)
Technical school	0.0344	0.0435	0.0394	0.0435	0.122	0.0705
	(0.182)	(0.204)	(0.195)	(0.204)	(0.327)	(0.256)
University	0.0256	0.0424	0.0840	0.0346	0.241	0.0922
	(0.158)	(0.202)	(0.278)	(0.183)	(0.428)	(0.289)
Missing education	0.0642	0.0648	0.0682	0.0796	0.135	0.0744
	(0.245)	(0.246)	(0.252)	(0.271)	(0.342)	(0.263)
Employment in pre-shock month	0.813	0.939	0.866	0.916	0.930	0.867
	(0.390)	(0.240)	(0.341)	(0.277)	(0.256)	(0.340)
Average pre-shock employment	0.773	0.832	0.748	0.821	0.894	0.821
	(0.201)	(0.181)	(0.216)	(0.188)	(0.157)	(0.193)
Earnings in pre-shock month	208.8	319.6	339.2	302.4	699.9	368.3
	(198.9)	(284.4)	(369.9)	(258.2)	(581.6)	(409.8)
Average pre-shock earnings	181.8	252.8	258.0	237.8	610.4	314.8
	(128.1)	(219.7)	(259.8)	(192.4)	(502.3)	(327.8)
Length of stay (days)	5.329	4.722	4.703	6.734	3.200	4.253
	(9.632)	(10.99)	(6.546)	(12.65)	(6.742)	(8.465)
Surgical intervention	0.456	0.356	0.480	0.512	0.649	0.459
	(0.498)	(0.479)	(0.500)	(0.500)	(0.478)	(0.499)
Observations	16609	11672	381	4964	1907	1290

Table 6: Difference-in-Differences Regressions of Employment

	(1) All	(2) Men	(3) Women	(4) No HS	(5) High school	(6) Tech school	(7) University
Treatment × post	-0.0278^{***}	-0.0263^{***}	-0.0393^{***}	-0.0357^{***}	-0.0224^{***}	0.00774	-0.0213^{**}
	(0.00172)	(0.00180)	(0.00575)	(0.00242)	(0.00283)	(0.00833)	(0.00872)
Age	0.0262^{***}	0.0242^{***}	0.0371^{***}	0.0245^{***}	0.0260^{***}	0.0266^{***}	0.0431^{***}
	(0.00128)	(0.00133)	(0.00425)	(0.00181)	(0.00217)	(0.00767)	(0.00848)
Age squared	-0.000371^{***}	-0.000340^{***}	-0.000539^{***}	-0.000349^{***}	-0.000364^{***}	-0.000429^{***}	-0.000623^{***}
	(0.0000149)	(0.0000153)	(0.0000508)	(0.0000196)	(0.0000273)	(0.000109)	(0.000103)
Affiliation time trend	-0.00933^{***}	-0.00905^{***}	-0.0106^{***}	-0.0104^{***}	-0.00835^{***}	-0.0104^{***}	-0.0109^{***}
	(0.000243)	(0.000255)	(0.000791)	(0.000349)	(0.000418)	(0.00116)	(0.00134)
Affiliation time trend squared	0.00817^{***}	0.00789^{***}	0.00905^{***}	0.00923^{***}	0.00718^{***}	0.0101^{***}	0.0103^{***}
	(0.000287)	(0.000303)	(0.000891)	(0.000414)	(0.000489)	(0.00142)	(0.00153)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-month dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Within-R^2	0.0169	0.0154	0.0369	0.0186	0.0158	0.0206	0.0301
Number of individuals	124,422	105,093	19,329	62,362	39,935	5,798	6,559
Monthly observations	10,141,275	8,572,196	1,569,079	5,110,849	3,256,683	469,919	526,978

Notes: Standard errors clustered on individuals.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Difference-in-Differences Regression of Employment with Health Insurance Interactions

	(1)
Treatment × post × FONASA	-0.0357^{***}
	(0.00243)
Treatment × post × ISAPRE	-0.0492^{***}
	(0.00612)
Treatment × post × work acc.	-0.0200^{***}
	(0.00264)
Treatment × post × school acc.	0.0111
	(0.0148)
Treatment × post × transport acc.	-0.0116^{***}
	(0.00406)
Treatment × post × other prov.	-0.0406^{***}
	(0.00807)
Age	0.0262^{***}
	(0.00128)
Age squared	-0.000371^{***}
	(0.0000149)
Affiliation time trend	-0.00933^{***}
	(0.000243)
Affiliation time trend squared	0.00817^{***}
	(0.000287)
Individual FE	Yes
Industry dummies	Yes
Year-month dummies	Yes
Within-R^2	0.0169
Number of individuals	124,422
Monthly observations	10,141,275

Notes: Standard errors clustered on individuals.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Difference-in-Differences Regressions of Employment with Length of Stay Interactions

	(1)
Treatment × post × LOS 1 day	-0.0210^{***}
	(0.00271)
Treatment × post × LOS 1 week	-0.0190^{***}
	(0.00222)
Treatment × post × LOS 2 weeks	-0.0462^{***}
	(0.00458)
Treatment × post × LOS 2 weeks+	-0.0987^{***}
	(0.00635)
Age	0.0262^{***}
	(0.00128)
Age squared	-0.000371^{***}
	(0.0000149)
Affiliation time trend	-0.00933^{***}
	(0.000243)
Affiliation time trend squared	0.00817^{***}
	(0.000287)
Individual FE	Yes
Industry dummies	Yes
Year-month dummies	Yes
Within-R^2	0.0169
Number of individuals	124,422
Monthly observations	10,141,275

Notes: Standard errors clustered on individuals.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A Appendix Tables

Table A.1: Distribution of Primary Diagnosis

	Freq.	Perc.
Injuries to the head	10860	21.10
Injuries to the neck	766	1.49
Injuries to the thorax	2418	4.70
Injuries to the abdomen, lower back, etc.	3550	6.90
Injuries to the shoulder and upper arm	2158	4.19
Injuries to the elbow and forearm	2822	5.48
Injuries to the wrist, hand and fingers	6834	13.28
Injuries to the hip and thigh	1294	2.51
Injuries to the knee and lower leg	7330	14.24
Injuries to the ankle and foot	2174	4.22
Injuries involving multiple body regions	3923	7.62
Injury of unspecified body region	2104	4.09
Foreign body entering through natural orifice	265	0.51
Burns and corrosions of external body surface	1209	2.35
Poisoning by drugs, biological substances	1335	2.59
Toxic effects of substances chiefly nonmedicinal	1214	2.36
Other and unspecified effects of external causes	405	0.79
Certain early complications of trauma	52	0.10
Complications of surgical and medical care	753	1.46

Table A.2: Distribution of Primary Diagnosis by Sex (in Percent)

	Women	Men
Injuries to the head	18.49	21.46
Injuries to the neck	1.784	1.447
Injuries to the thorax	1.258	5.176
Injuries to the abdomen, lower back, etc.	4.778	7.192
Injuries to the shoulder and upper arm	2.612	4.413
Injuries to the elbow and forearm	5.335	5.504
Injuries to the wrist, hand and fingers	6.673	14.20
Injuries to the hip and thigh	1.577	2.644
Injuries to the knee and lower leg	13.04	14.41
Injuries to the ankle and foot	3.074	4.384
Injuries involving multiple body regions	6.832	7.732
Injury of unspecified body region	3.424	4.180
Foreign body entering through natural orifice	0.733	0.485
Burns and corrosions of external body surface	2.357	2.348
Poisoning by drugs, biological substances	12.88	1.164
Toxic effects of substances chiefly nonmedicinal	5.542	1.916
Other and unspecified effects of external causes	1.019	0.755
Certain early complications of trauma	0.0796	0.104
Complications of surgical and medical care	8.489	0.487

Table A.3: Distribution of Primary Diagnosis by Education (in Percent)

	No high school	High school	Tech. school	University	Missing
Injuries to the head	22.01	20.72	18.76	17.37	19.60
Injuries to the neck	1.360	1.580	2.242	1.778	1.486
Injuries to the thorax	5.128	4.647	3.411	2.325	3.906
Injuries to the abdomen, lower back, etc.	7.574	6.579	4.630	3.737	6.350
Injuries to the shoulder and upper arm	3.917	4.256	4.825	5.880	4.601
Injuries to the elbow and forearm	5.424	5.692	5.068	4.603	5.775
Injuries to the wrist, hand and fingers	13.11	14.20	12.38	9.480	13.44
Injuries to the hip and thigh	2.683	2.552	1.901	1.504	2.085
Injuries to the knee and lower leg	12.62	13.88	18.76	25.30	18.31
Injuries to the ankle and foot	4.490	3.955	3.411	4.011	3.954
Injuries involving multiple body regions	7.696	7.597	8.236	7.794	6.830
Injury of unspecified body region	4.252	3.929	3.899	3.646	3.906
Foreign body entering through natural orifice	0.498	0.568	0.536	0.410	0.479
Burns and corrosions of external body surface	2.160	2.683	2.485	2.051	2.468
Poisoning by drugs, biological substances	2.214	2.852	3.899	4.740	2.396
Toxic effects of substances chiefly nonmedicinal	2.604	2.049	2.973	2.051	1.725
Other and unspecified effects of external causes	0.685	0.907	0.877	1.185	0.767
Certain early complications of trauma	0.0974	0.0979	0.0975	0.0456	0.168
Complications of surgical and medical care	1.468	1.266	1.608	2.097	1.749

Table A.4: Distribution of Primary Diagnosis by Insurance Provider (in Percent)

	FONASA	Work acc.	School acc.	Transport	ISAPRE	Other
Injuries to the head	22.48	19.82	22.06	20.05	15.71	21.03
Injuries to the neck	1.327	1.901	0.829	1.329	1.353	1.581
Injuries to the thorax	6.009	3.289	3.814	2.998	3.099	4.586
Injuries to the abdomen, lower back, etc.	7.215	7.490	8.292	5.040	4.365	6.747
Injuries to the shoulder and upper arm	4.346	3.738	5.141	3.841	5.936	4.164
Injuries to the elbow and forearm	5.816	4.670	4.146	6.482	4.714	4.955
Injuries to the wrist, hand and fingers	10.29	17.27	15.42	17.26	11.52	13.86
Injuries to the hip and thigh	2.377	2.627	1.990	3.030	1.833	2.899
Injuries to the knee and lower leg	13.59	13.16	14.59	14.33	28.24	14.13
Injuries to the ankle and foot	3.016	5.902	2.819	6.077	3.448	4.006
Injuries involving multiple body regions	5.911	9.682	7.463	10.99	4.278	9.383
Injury of unspecified body region	4.040	4.322	4.975	3.452	4.496	4.323
Foreign body entering through natural orifice	0.707	0.335	0.166	0.146	0.611	0.369
Burns and corrosions of external body surface	2.105	2.663	1.493	3.095	1.615	2.161
Poisoning by drugs, biological substances	4.361	0.263	2.819	0.162	3.448	2.003
Toxic effects of substances chiefly nonmedicinal	3.061	1.744	1.990	0.875	2.575	1.792
Other and unspecified effects of external causes	0.850	0.826	0.995	0.373	0.829	0.843
Certain early complications of trauma	0.106	0.0641		0.146	0.0873	0.211
Complications of surgical and medical care	2.392	0.235	0.995	0.340	1.833	0.949

Table A.5: Length of Hospital Stay by Primary Diagnosis (in Days)

	Mean	Std.dev.
Injuries to the head	4.73	(8.91)
Injuries to the neck	5.41	(10.03)
Injuries to the thorax	5.12	(7.42)
Injuries to the abdomen, lower back, etc.	5.97	(11.27)
Injuries to the shoulder and upper arm	4.10	(5.45)
Injuries to the elbow and forearm	4.64	(6.07)
Injuries to the wrist, hand and fingers	3.83	(6.59)
Injuries to the hip and thigh	10.05	(13.33)
Injuries to the knee and lower leg	5.74	(12.08)
Injuries to the ankle and foot	5.75	(8.50)
Injuries involving multiple body regions	5.27	(11.85)
Injury of unspecified body region	7.05	(13.14)
Foreign body entering through natural orifice	2.83	(4.05)
Burns and corrosions of external body surface	11.15	(15.51)
Poisoning by drugs, biological substances	3.23	(4.64)
Toxic effects of substances chiefly nonmedicinal	3.86	(13.18)
Other and unspecified effects of external causes	2.66	(4.03)
Certain early complications of trauma	11.23	(13.09)
Complications of surgical and medical care	4.09	(5.82)

Table A.6: Distribution of Secondary Diagnosis

	Freq.	Perc.
Pedestrian	1086	2.18
Pedal cycle rider	966	1.94
Motorcycle rider	279	0.56
Occupant of three-wheeled motor vehicle	22	0.04
Car occupant	1071	2.15
Occupant of pick-up truck or van	141	0.28
Occupant of heavy transport vehicle	106	0.21
Bus occupant injured in transport accident	172	0.35
Other land transport accidents	1810	3.64
Water transport accidents	28	0.06
Other and unspecified transport accidents	277	0.56
Slipping, tripping, stumbling and falls	9329	18.76
Exposure to inanimate mechanical forces	6039	12.15
Exposure to animate mechanical forces	556	1.12
Accidental non-transport drowning and submersion	31	0.06
Exposure to electric current, radiation, etc.	200	0.40
Exposure to smoke, fire and flames	376	0.76
Contact with heat and hot substances	503	1.01
Contact with venomous animals and plants	354	0.71
Exposure to forces of nature	36	0.07
Accidental poisoning by noxious substances	578	1.16
Overexertion, travel and privation	772	1.55
Accidental exposure to other/unspecified factors	15242	30.66
Intentional self-harm	834	1.68
Assault	3867	7.78
Event of undetermined intent	4211	8.47
Legal intervention, operations of war	21	0.04
Complications of medical and surgical care	610	1.23
Sequelae of external causes	196	0.39

Table A.7: Distribution of Secondary Diagnosis by Sex (in Percent)

	Women	Men
Pedestrian	1.980	2.213
Pedal cycle rider	1.501	2.004
Motorcycle rider	0.247	0.605
Car occupant	2.739	2.073
Occupant of pick-up truck or van	0.297	0.282
Occupant of heavy transport vehicle	0.0660	0.234
Bus occupant injured in transport accident	0.544	0.318
Other land transport accidents	4.339	3.543
Water transport accidents	0.0165	0.0618
Other and unspecified transport accidents	0.792	0.525
Slipping, tripping, stumbling and falls	19.80	18.62
Exposure to inanimate mechanical forces	5.329	13.09
Exposure to animate mechanical forces	1.221	1.104
Accidental non-transport drowning and submersion	0.0495	0.0641
Exposure to electric current, radiation, etc.	0.198	0.431
Exposure to smoke, fire and flames	0.808	0.749
Contact with heat and hot substances	1.155	0.992
Contact with venomous animals and plants	1.782	0.563
Exposure to forces of nature	0.0495	0.0756
Accidental poisoning by noxious substances	4.009	0.767
Overexertion, travel and privation	1.353	1.580
Accidental exposure to other/unspecified factors	26.22	31.27
Intentional self-harm	7.392	0.884
Assault	2.145	8.560
Event of undetermined intent	8.909	8.409
Complications of medical and surgical care	6.484	0.497
Sequelae of external causes	0.544	0.373
Occupant of three-wheeled motor vehicle		0.0504
Legal intervention, operations of war		0.0481

Table A.8: Distribution of Secondary Diagnosis by Education (in Percent)

	No high school	High school	Tech. school	University	Missing
Pedestrian	2.168	2.183	2.121	1.962	2.443
Pedal cycle rider	2.220	1.797	0.808	0.887	1.752
Motorcycle rider	0.373	0.671	1.414	1.355	0.567
Occupant of three-wheeled motor vehicle	0.0522	0.0271	0.0505	0.0467	0.0493
Car occupant	1.810	2.197	3.384	3.877	2.763
Occupant of pick-up truck or van	0.231	0.305	0.354	0.420	0.444
Occupant of heavy transport vehicle	0.187	0.264	0.202	0.234	0.197
Bus occupant injured in transport accident	0.280	0.441	0.758	0.0934	0.370
Other land transport accidents	3.444	3.567	4.343	3.970	4.688
Water transport accidents	0.0597	0.0475	0.101	0.0934	0.0247
Other and unspecified transport accidents	0.466	0.617	0.556	0.934	0.740
Slipping, tripping, stumbling and falls	18.86	18.60	18.23	17.84	19.49
Exposure to inanimate mechanical forces	12.90	12.41	8.687	6.072	11.10
Exposure to animate mechanical forces	1.142	1.112	0.859	1.168	1.086
Accidental non-transport drowning and submersion	0.0634	0.0678		0.0467	0.0740
Exposure to electric current, radiation, etc.	0.369	0.522	0.404	0.327	0.222
Exposure to smoke, fire and flames	0.716	0.868	0.707	0.327	0.864
Contact with heat and hot substances	0.974	1.119	1.010	0.701	1.036
Contact with venomous animals and plants	0.709	0.705	0.657	0.887	0.691
Exposure to forces of nature	0.0784	0.0542	0.101	0.0467	0.0987
Accidental poisoning by noxious substances	1.112	1.180	1.717	1.635	0.913
Overexertion, travel and privation	1.377	1.444	2.172	3.129	1.974
Accidental exposure to other/unspecified factors	29.92	30.45	33.64	38.53	30.62
Intentional self-harm	1.508	1.824	2.222	2.849	1.382
Assault	8.564	7.804	5.657	3.783	5.625
Event of undetermined intent	8.751	8.272	7.828	6.305	8.784
Legal intervention, operations of war	0.0597	0.0271			0.0247
Complications of medical and surgical care	1.246	1.031	1.313	1.868	1.431
Sequelae of external causes	0.343	0.393	0.707	0.607	0.469

Table A.9: Distribution of Secondary Diagnosis by Insurance Provider (in Percent)

	FONASA	Work acc.	School acc.	Transport	ISAPRE	Other
Pedestrian	1.563	2.488	4.362	4.446	1.361	2.001
Pedal cycle rider	2.312	1.861	0.336	1.368	1.134	0.649
Motorcycle rider	0.398	0.685	0.168	1.044	0.499	0.649
Occupant of three-wheeled motor vehicle	0.0507	0.0216		0.0900		0.0541
Car occupant	1.392	1.947	6.040	5.851	2.178	1.893
Occupant of pick-up truck or van	0.160	0.188	0.168	1.080	0.272	0.379
Occupant of heavy transport vehicle	0.0858	0.368	0.168	0.522		0.162
Bus occupant injured in transport accident	0.121	0.663	0.168	0.612	0.363	0.324
Other land transport accidents	3.275	1.731	3.691	11.04	1.906	2.866
Water transport accidents	0.0624	0.0433	0.168	0.0540	0.0454	0.0541
Other and unspecified transport accidents	0.448	0.137	0.503	2.070	0.227	1.082
Slipping, tripping, stumbling and falls	20.55	17.50	15.10	16.33	16.20	14.93
Exposure to inanimate mechanical forces	8.573	18.55	9.396	17.05	6.216	6.923
Exposure to animate mechanical forces	1.618	0.663	0.503	0.108	1.089	0.865
Accidental non-transport drowning and submersion	0.0897	0.0288		0.0540		0.0541
Exposure to electric current, radiation, etc.	0.378	0.555	0.168	0.306	0.0454	0.379
Exposure to smoke, fire and flames	0.827	0.808	0.839	0.396	0.544	0.703
Contact with heat and hot substances	0.928	1.039	0.168	1.512	0.771	1.028
Contact with venomous animals and plants	1.068	0.281	0.671	0.216	0.771	0.433
Exposure to forces of nature	0.0546	0.115		0.0720	0.0454	0.0541
Accidental poisoning by noxious substances	1.599	0.656	0.336	0.198	1.906	1.190
Overexertion, travel and privation	0.269	3.476	0.336	1.512	3.766	2.812
Accidental exposure to other/unspecified factors	27.32	37.57	34.06	15.95	47.69	47.92
Intentional self-harm	2.932	0.0865	1.846	0.108	1.543	1.028
Assault	12.95	1.738	7.383	0.702	4.628	6.436
Event of undetermined intent	8.531	6.044	11.58	16.87	4.719	4.056
Legal intervention, operations of war	0.0624	0.0216	0.168			0.0541
Complications of medical and surgical care	1.953	0.317	1.174	0.162	1.543	0.811
Sequelae of external causes	0.405	0.418	0.503	0.288	0.499	0.216

Table A.10: Length of Hopsital Stay by Secondary Diagnosis (in Days)

	Mean	Std.dev.
Pedestrian	9.00	(17.81)
Pedal cycle rider	4.43	(5.58)
Motorcycle rider	6.99	(9.04)
Occupant of three-wheeled motor vehicle	8.68	(7.21)
Car occupant	6.49	(10.06)
Occupant of pick-up truck or van	3.12	(4.69)
Occupant of heavy transport vehicle	4.93	(8.14)
Bus occupant injured in transport accident	4.42	(6.27)
Other land transport accidents	7.40	(14.26)
Water transport accidents	5.89	(8.97)
Other and unspecified transport accidents	8.17	(11.41)
Slipping, tripping, stumbling and falls	5.14	(7.99)
Exposure to inanimate mechanical forces	4.96	(7.33)
Exposure to animate mechanical forces	4.33	(4.86)
Accidental non-transport drowning and submersion	5.65	(7.41)
Exposure to electric current, radiation, etc.	6.88	(12.78)
Exposure to smoke, fire and flames	11.02	(13.76)
Contact with heat and hot substances	10.35	(15.00)
Contact with venomous animals and plants	3.32	(4.00)
Exposure to forces of nature	4.36	(4.59)
Accidental poisoning by noxious substances	3.04	(6.48)
Overexertion, travel and privation	3.20	(4.13)
Accidental exposure to other/unspecified factors	4.75	(10.82)
Intentional self-harm	4.19	(12.89)
Assault	4.42	(6.09)
Event of undetermined intent	5.81	(12.07)
Legal intervention, operations of war	4.52	(3.46)
Complications of medical and surgical care	3.54	(5.04)
Sequelae of external causes	6.66	(14.05)

Table A.11: Distribution of Industry by Education for Treatment Group (in Percent)

	No high school	High school	Tech. school	University	Missing educ.
Agriculture	18.09	7.661	4.162	2.938	9.742
Fishing	1.441	1.483	1.285	1.528	1.001
Mining	1.137	1.536	2.326	3.114	1.579
Manufacturing - nonmetal	7.633	8.386	7.772	5.934	9.087
Manufacturing - metal	2.831	4.695	4.835	2.879	4.082
Utilities	0.211	0.323	0.673	0.705	0.809
Construction	29.34	25.63	15.06	12.69	8.972
Wholesale and retail	9.424	13.31	15.61	12.81	16.79
Hotels and restaurants	1.493	2.557	2.938	2.233	3.003
Transport; storage; communications	7.288	9.503	8.017	6.404	11.67
Finance	1.621	1.963	6.548	9.342	3.235
Real estate; renting; business	10.57	13.67	17.01	15.75	13.28
Public administration and defense	1.822	1.780	1.958	2.056	2.041
Education	0.556	0.838	2.632	9.166	3.697
Social and health services	0.659	0.812	2.387	2.703	2.079
Other community services	5.219	5.157	5.936	7.697	7.470
Buildings management	0.154	0.131	0.122		0.154

Table A.12: Distribution of Industry by Insurance Provider for Treatment Group (in Percent)

	FONASA	Work acc.	School acc.	Transport	ISAPRE	Other
Agriculture	15.37	12.08	6.299	12.25	4.195	6.822
Fishing	1.174	1.585	1.575	1.370	1.468	3.178
Mining	1.066	1.422	1.312	1.148	4.719	2.636
Manufacturing - nonmetal	7.231	8.165	5.512	10.29	7.079	6.744
Manufacturing - metal	2.974	3.932	4.462	4.754	3.304	4.109
Utilities	0.259	0.377	0.262	0.322	0.577	0.543
Construction	25.52	28.44	29.92	21.39	15.15	23.95
Wholesale and retail	11.84	10.64	6.562	11.78	15.57	11.63
Hotels and restaurants	2.330	1.576	2.100	2.015	2.412	1.705
Transport; storage; communications	6.960	9.330	11.55	9.690	6.869	11.40
Finance	1.626	2.399	4.462	1.894	9.806	3.256
Real estate; renting; business	12.11	11.55	16.01	12.73	15.52	12.64
Public administration and defense	2.625	1.028	1.575	1.773	0.839	0.930
Education	1.288	1.105	2.362	1.269	2.937	2.171
Social and health services	0.765	1.148	1.312	0.786	1.888	1.473
Other community services	5.997	4.541	4.462	5.761	6.240	6.124
Buildings management	0.199	0.0514	0.262	0.181	0.0524	0.0775