The Effect of Accidents on Labor Market Outcomes: Evidence from Chile*

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Abstract

We estimate the causal effect of accidents on employment and earnings among Chilean men using event study methods and monthly administrative data. An accident of any type reduces the probability of being employed by 8.4 percentage points in the first year, by 11.2 percentage points in the second year, and by 14.8 percentage points in the third year after the accident. On average, over the three years after the accident, employment declines by 14%, relative to the pre-accident mean. In addition, accidents reduce monthly earnings by around 11% in the first year, 17% in the second year, and 22% in the third year after the accident. On average, monthly earnings fall by 16%, relative to the pre-accident average. Thus, we estimate persistent and increasing labor market effects of accidents over time. These effects vary by individuals' age, education, and industry and by severity of the accident. Our findings imply that the economic consequences of health shocks go beyond direct medical expenses.

Keywords: Health Shocks; Accidents; Labor Market Outcomes; Employment; Earnings.
JEL Codes: I10; I13; I15; J22.

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1 Introduction

Evidence regarding the effects of health on labor market activity is crucial for the evaluation of social programs designed to prevent or cure diseases (Currie and Madrian, 1999). An improved understanding of this subject may also guide health and redistribution policy (e.g., Deaton, 2002, 2013). For instance, a causal link between health and labor earnings would inform policymakers that policies aimed at reducing health inequality could also contribute to closing income gaps. Evidence on the effect of health on labor market outcomes is also relevant for broader economic issues such as economic growth, intergenerational transmission of human capital, and labor force participation. A positive relationship between health and labor market outcomes has been well documented. However, the identification of causal effects has proven to be difficult (Currie and Madrian, 1999).

In this paper, we use event study methods and monthly administrative data from Chile to quantify the causal effect of accidents on employment and earnings and thereby provide a credible estimate for the effect of health on these labor market outcomes. To estimate the causal effect of health shocks, we exploit the timing of accidents, similar to Dobkin et al. (2018). Our sample includes only individuals who had an accident and excludes a "traditional" control group consisting of individuals without a health shock.

We also incorporate the methodological insights of Borusyak and Jaravel (2017) by estimating a dynamic model that allows for time-varying pre- and post-treatment effects. Importantly, we show that the weighting implicit in a standard difference-in-differences (DD) approach underestimates the negative effect of accidents on labor market outcomes. Instead, we obtain average treatment effects that are manually aggregated from a dynamic specification, which avoids the bias due to the negative weighting inherent in standard DD.

Our empirical design addresses several of the methodological issues present in some existing studies. By using administrative data, we avoid the common problems found in the use of survey data, such as non-random measurement error, reverse causality, and justification bias, which can lead to endogenous health measures (e.g., Bound, 1991; Crossley and Kennedy, 2002; Baker, Stabile, and Deri, 2004).[1]

More recently, a growing number of studies have exploited exogenous variation from sudden changes in health status ("health shocks") to estimate the causal effect of health on labor market outcomes. For example, García-Gómez et al. (2013), Lundborg, Nilsson, and Vikström (2015), and Dobkin et al. (2018) use acute hospitalizations in the Netherlands,

[1] 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 for health reasons, it is also a socially acceptable reason and may be over-reported in surveys, as first noted by Bazzoli (1985).

Sweden, and the United States; and Heinesen and Kolodziejczyk (2013) and Jeon (2017) use cancer diagnoses in Denmark and Canada as specific health shocks. The underlying assumption that allows for the identification of causal effects is that these shocks are unexpected and, importantly, uncorrelated with any unobserved determinants of labor market outcomes.

Different from these studies, our health shock measure only includes hospitalizations that are truly unpredictable, in particular, those due to accidents. Individuals cannot change their labor supply in anticipation of an unpredictable event like an accident. In contrast, hospitalizations such as those due to cardiovascular conditions may be more predictable because individuals usually experience slowly deteriorating health conditions before being admitted to a hospital. Halla and Zweimüller (2013) use a similar strategy by focusing on commuting accidents in Austria as a source of identifying variation.[2] The drawback of using only health shocks due to accidents is the limited external validity due to focusing on a very specific type of health shock. We are willing to accept this restriction because it yields credible causal estimates.

Although accidents are unpredictable, the probability of having an accident at all may not be exogenous to labor market outcomes. For example, risk preferences may be correlated with the propensity to have an accident and the type of work chosen by an individual, thereby leading to omitted variable bias. To avoid this issue, our sample only includes individuals who were hospitalized due to an accident at least once during the study period. In contrast, for example, Halla and Zweimüller (2013) compare individuals with and without accidents who may differ along some unobserved dimensions.[3]

Instead, our identification strategy uses variation from the timing of hospitalizations as in Dobkin et al. (2018). Specifically, our empirical design relies on the identifying assumption that, conditional on having an accident during our observation window, the timing of the accident is uncorrelated with unobserved components of labor market outcomes. Thus, in combination with using hospitalizations due to external causes only, we obtain a source of variation that is as close to random as feasible in an observational study.

[2] Mohanan (2013) uses accidents in India as a source of exogenous variation, but focuses on consumption and debt instead of labor market outcomes.

[3] Other studies that build counterfactuals based on samples of non-injured individuals include Reville and Schoeni (2001), Dano (2005), and Crichton, Stillman, and Hyslop (2011). Reville and Schoeni (2001) use administrative data from California to estimate the earnings losses associated with workplace injuries that lead to permanent partial disability. The authors match injured workers to their co-workers with similar pre-injury earnings to estimate earnings losses following injury. Dano (2005) investigates the causal relationship between road injuries and labor market outcomes. The author relies on propensity score matching and a DD design to estimate counterfactuals. Crichton, Stillman, and Hyslop (2011) examine the impact of injuries on labor market outcomes by comparing injured individuals receiving earnings compensation from a state-run insurance system in New Zealand to samples of non-injured workers.

Our paper therefore makes methodological advances over previous work and, thus, offers a more credible estimate of the causal effect of health shocks on labor market outcomes. Specifically, we incorporate recent insights in DD settings by estimating a dynamic model that allows for time-varying pre- and post-treatment effects; and we check the no pre-trends assumption by estimating a restricted version of the fully dynamic model normalizing two pre-treatment indicators. Moreover, instead of comparing individuals with and without accidents, we build counterfactuals by exploiting the variation in accident timings. Lastly, we combine a highly plausible source of exogenous variation (timing of hospitalizations due to accidents) with high-frequency administrative data.[4]

We combine administrative data from two sources. Specifically, we merge monthly employment data on the universe of men affiliated with the Chilean unemployment insurance system from October 2002 to December 2011 to the universe of Chilean hospital discharge records for the years 2004 to 2007. Our baseline sample contains information on employment, earnings, and hospitalizations for nearly 13,000 men who were hospitalized due to an accident during our study period. By focusing on health shocks stemming from events such as slipping, tripping, stumbling, falls; exposure to inanimate mechanical forces; land transport accidents; and other events of similar nature, our identification strategy relies on truly unanticipated events.

Our estimates show that the impact of accidents on labor market outcomes occurs immediately after the health shock and persists in subsequent years. Specifically, we find that an accident reduces the probability of being employed by 8.4 percentage points in the first year, by 11.2 percentage points in the second year, and by 14.8 percentage points in the third year after the accident. These effects represent a 14% decline in employment on average, relative to the pre-accident mean. In addition, the estimates suggest that the decline in monthly earnings associated with accidents also grows over time, from US$60 in the first year to US$126 in the third year after the accident. On average, over the three years after the accident, monthly earnings fall by 16%, relative to the pre-accident average.

We also provide evidence that suggests treatment effect heterogeneity across individuals' observable characteristics. First, we show that older individuals exhibit a larger decline in employment and earnings after the accident than younger individuals. Second, we show that more educated individuals experience a slightly smaller decline in the probability of being employed than less educated individuals as consequence of an accident. We also find

[4] In addition, our study presents the first evidence in this area based on administrative data from an emerging economy, whereas the existing literature uses data from Europe and North America. In a related study, Mommaerts, Raza, and Zheng (2019) use survey data to estimate the effect of hospitalizations on labor market outcomes among older workers in China in addition to several developed countries.

significant differences in the earnings effect of accidents across education groups. However, more educated individuals exhibit higher pre-accident earnings than less educated individuals, which turns the difference in the percentage drop of earnings across education groups negligible. Third, we show evidence that points to a more pronounced impact of accidents in industries that rely more heavily on manual labor, that is, the primary and secondary sectors. Lastly, we find that more severe accidents, proxied by longer hospital stays, lead to a larger decline in both employment and earnings.

Overall, this paper uses an event study approach to provide causal evidence on the effect of accidents on labor market outcomes in an emerging economy. Our evidence contributes to further assessing the magnitude of health effects on labor market activity. Furthermore, empirical evidence as that provided in this paper allows policymakers to better assess the cost effectiveness of interventions designed to prevent or cure diseases (Currie and Madrian, 1999). In particular, our findings suggest that the economic consequences of health shocks could go beyond direct medical expenditures.

The remainder of this paper is organized as follows. Section 2 describes the institutional context of the labor market and health system in Chile. Section 3 describes the data. In Section 4, we discuss our empirical strategy. Section 5 presents our results and Section 6 discusses possible explanations for our findings. Section 7 concludes this paper.

2 Institutional Context

In this section, we briefly describe the Chilean labor market and health care system.[5] Our labor market data come from the Chilean unemployment insurance system (UIS). The Chilean government enacted the UIS as an addition to the existing social protection safety net in 2002. Participation in the system is mandatory for all workers hired or who signed new contracts after October 2002. Workers with work relationships established prior to this date may join the system on a voluntary basis. The UIS incorporates mandatory individual savings and a solidarity social security scheme. The savings component features an individual unemployment account financed through the contributions of workers and their employers for workers with open-ended contracts, and by employers only for workers with fixed-term or specific work/service contracts. The solidarity component, accessible through a solidarity unemployment fund, is co-financed by employers and the state.

The UIS includes employed workers over the age of 18 whose working conditions are regulated by the Labor Code. Workers under the age of 18, domestic workers, pensioners,

[5]Online Appendix B contains more detailed information on the safety net in Chile.

self-employed or own-account workers, and public sector employees are excluded from unemployment insurance. Workers covered by the UIS are called dependent workers and those excluded from the UIS are called independent workers.

About 15% of Chile's dependent workers are in the informal sector, according to the National Statistics Institute. Since we use the UIS to measure employment, we cannot distinguish between non-employment and employment in the independent or informal sectors. To avoid interpreting a switch from dependent to independent or informal work as a drop in employment, we restrict our sample to individuals with high levels of attachment to the dependent sector (see Section 3). Transitions between sectors are relatively rare. For example, 80% of individuals who were dependent workers in a particular quarter continue to be so in the following quarter. Most workers who ceased being dependent workers became inactive and did not move to another occupational category (Central Bank of Chile, 2018). Hence, we can be confident that most employment switches recorded in our data represent an actual change in employment status.

We now briefly describe Chile's dual health care system. The *Fondo Nacional de Salud* (FONASA) is the public health insurance system run by the Ministry of Health. In addition, there are several *Instituciones de Salud Previsional* (ISAPREs), which provide private health insurance plans that act as alternatives to FONASA.[6] Employees are enrolled in the public FONASA system by default but can opt out and join an ISAPRE. Currently, more than 80% of the Chilean population is enrolled in FONASA. In our hospital discharge data, almost all of the individuals with non-missing information on health insurance are enrolled in the FONASA system.[7]

FONASA beneficiaries are classified into four groups. Group A beneficiaries are individuals who lack resources or formal employment; these are individuals who 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 make any copayments to public providers. About 36% of FONASA beneficiaries are classified as group A. The remaining 64% are employees who contribute 7% of their salary to FONASA, up to a monthly salary ceiling. They are classified into groups B, C, and D according to their monthly income. These FONASA beneficiaries pay copayments for health care services that vary between 0% and 20% depending on their earnings relative to the minimum wage and their number of dependents. About 60% of our sample are enrolled in FONASA groups A or B and are not subject to any copayments.

[6] As a third option, some workers are enrolled in plans that are sponsored by firms or other special groups.

[7] Since we do not observe much variation in health insurance in our sample, we are unable to estimate heterogeneous treatment effects by insurance provider.

Beneficiaries can only obtain health care services in public facilities or private facilities that have an agreement with FONASA at these copayment levels.

Individuals who opt out of FONASA can choose among 12 ISAPRE plans which 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 plans but provide access to better health care. ISAPREs collect the mandatory contribution of 7%, but members may pay an additional premium amounting to 2.2% of their income on average. ISAPRE beneficiaries almost exclusively use private providers for two main reasons. First, by law, most public hospitals do not have hospital beds available for non-FONASA beneficiaries. Second, ISAPRE beneficiaries avoid using public providers because they can obtain better quality and timelier medical care from private providers who only serve ISAPRE enrollees.

Overall, FONASA plans are cheaper than ISAPRE plans but provide access to lower quality health care services with longer wait times. For instance, about 260,000 FONASA patients were on a waiting list for surgery in 2018, and 42% of them waited for more than a year. Furthermore, 1.6 million people are currently waiting to see a specialist, given the shortage of these doctors in the public system. In addition, absenteeism among state health officials is two times higher than the national average.[8]

3 Data and Summary Statistics

We combine administrative data on monthly earnings and hospital stays from two sources. The labor market data include the universe of UIS records from October 2002 to December 2011. We collect monthly observations on earnings, employment status (defined by strictly positive earnings), and the employer's industry. In addition, the UIS records employees' educational attainment, sex, year and birth month, and the date they became affiliated with the UIS. We deflate earnings using 2018 as the base year and express them in U.S. dollars; the approximate average exchange rate during our sample period is 600 Chilean pesos to 1 U.S. dollar.

We use the universe of Chilean hospital discharge records for the years 2004 to 2007 to measure health shocks. For each hospital stay we observe the International Classification of Diseases, 10th edition (ICD-10) diagnosis code, the patient's health insurance provider, and the exact dates of admission and discharge. The Ministry of Health of Chile collects these records from all hospitals in the country. We classify a hospital stay by type of major

[8] See https://colegioabogados.cl/reforma-a-fonasael-problema-de-la-salud-en-chile/.

diagnosis according to the first letter of the ICD-10 code and retain hospital stays related to a diagnosis code that starts with S or T ("Injury, poisoning, and certain other consequences of external causes").[9] We also observe the cause of each accident using ICD-10 codes starting with V, W, X, or Y ("External causes of morbidity").[10] For a cleaner analysis, we exclude accidents related to "Intentional self-harm" and "Complications from medical and surgical care," which are not completely exogenous events.[11]

The employment and hospital data contain each individual's *Rol Unico Tributario* (RUT) which acts as a unique identifier for tax and other official purposes in Chile. We match individual monthly employment records to hospital records by RUT numbers and sex.[12] Our baseline sample consists of dependent workers with strong ties to the labor market, which increases the likelihood that a post-accident change in employment recorded in the UIS reflects an actual change in the employment status of workers, and not movement to self-employment or the informal sector. Based on the latter criteria, we exclude women from the analysis because they exhibit a high rate of informality (Central Bank of Chile, 2018).

We restrict the sample to men, born between 1950 and 1980, who had an accident between January 2004 and December 2007. These men were between 22 and 61 years old during our study period. We also exclude men who became affiliated with the UIS after December 2003 to ensure that we can observe a sufficiently long employment history before the health shock. We include only individuals for whom we observe a balanced panel of 36 months before and after the accident. This avoids biased estimates due to attrition. It also implies that we only retain individuals who did not have a fatal accident. In addition, we drop men who were employed in the formal sector fewer than 18 months total before their accident to eliminate individuals with weak ties to the formal labor market, resulting in our final sample size of 12,885 individuals. To investigate the sensitivity of our results to these sample restrictions, we carry out robustness checks in Section 5.3.

Table 1 presents a description of the sample. Pre-accident, the average monthly employment rate is 78% and mean monthly earnings equal about US$570 (including zero earnings for months when individuals were not employed; the mean earnings equal US$730, conditional on employment). Individuals are, on average, about 38 years old at the time of the accident. Among them, 86% have at most a high school degree and almost 9% have some level of postsecondary education. In addition, 94% of the individuals who report a health

[9]See https://www.icd10data.com/ICD10CM/Codes/S00-T88

[10]See https://www.icd10data.com/ICD10CM/Codes/V00-Y99

[11]We thank the editor for pointing out this issue.

[12]The data sets were merged using a secure server at the Chilean Ministry of Finance, and only de-identified data were made available to the authors. This project was granted IRB approval by the General Research Ethics Board of Queen's University.

insurance provider were enrolled in FONASA at the time of the accident. We also observe that 13% were employed in the primary sector (agriculture, fishing, and mining), 42% in the secondary sector (manufacturing, construction, and transportation), and 24% in the tertiary sector (wholesale, retail, restaurant, finance, real estate, education, and health).[13] Distributions of education, industry, age, and earnings in our sample are roughly similar to Chile's overall population.[14] Finally, Table 1 shows that 6% of the individuals in our sample exhibit hospital stays lasting longer than two weeks.

Tables A1 and A2, in Online Appendix A, summarize diagnoses and accident causes. The most common diagnoses are injuries to the head (21%); knee and lower leg (17%); wrist, hand, and fingers (15%); and those involving multiple body regions (8%). The most common causes of accidents in our sample are slipping, tripping, stumbling and falls (21%); exposure to inanimate mechanical forces (14%); and assault (8%). These characteristics of accidents highlight the fact that the health shocks considered in this paper are truly unanticipated events leading to a sudden decline in health status. In addition, Tables A3 through A8, in Online Appendix A, present the distribution of accidents by age, education, and industry.

4 Empirical Strategy

We use an event study approach to quantify the effects of accidents on labor market outcomes. In particular, we specify the following empirical model:

$$Y_{it} = \alpha_i + \beta_t + \sum_{k=-36}^{36} \gamma_k \mathbf{1}\{K_{it} = k\} + u_{it}, \tag{1}$$

where Y_{it} is the relevant labor market outcome (employment status or monthly earnings) of individual i in month t and K_{it} denotes the relative time passed since the health shock, i.e., $K_{it} = 0$ in the month of the accident, $K_{it} = 1$ in the month following the accident, and so on. Model (1) also includes an individual fixed effect, α_i, and a calendar year-month fixed effect, β_t (e.g., for January 2005). Lastly, u_{it} is an i.i.d. error term.

Borusyak and Jaravel (2017) call specification (1) the *fully dynamic model*, and show that it suffers from a fundamental underidentification problem. Specifically, they show that a linear trend in the dynamic path of causal effects is not identified. In order to solve this identification problem, Borusyak and Jaravel (2017) propose starting from the fully dynamic regression (1) and dropping any two terms for $k < 0$ by setting the corresponding γ_k to zero.

[13] We classify individuals according to their modal pre-accident sector.

[14] See, for example, https://data.oecd.org/chile.htm.

Ideally, the omitted categories should be far apart. Here we select $k = -1$ and $k = -36$ as the omitted categories, i.e., we set $\gamma_{-36} = \gamma_{-1} = 0$.[15]

Once we have set these two restrictions, we check for pre-trends by plotting the path of $\hat{\gamma}_k$ before and after the treatment. We use this graph only to evaluate pre-trends, i.e., we ensure that the estimated $\hat{\gamma}_k$ for $k < 0$ are not statistically different from zero. In addition to a visual inspection of the pre-trends, we formally test the joint null hypothesis that the pre-treatment terms have no effect on the outcome—$H_0 : \gamma_k = 0, k = -35, \ldots, -2$—using an F-test.[16] Once we are comfortable with the assumption of no pre-trends, we set all γ_k for $k < 0$ to zero and proceed to estimate only the post-accident dynamic treatment effects, because this is more efficient than estimating the full model.

We consider two labor market outcomes: an indicator that equals one if individual i is registered as employed in the UIS data in year-month t and zero otherwise, and monthly earnings (including zero when the individual is not employed). In this empirical setting, γ_k represents the causal effect of an accident on employment or earnings k months after the accident. We cluster standard errors at the individual level.

Our empirical design relies on the identifying assumption that, conditional on having an accident during our observation window, the timing of the accident is uncorrelated with unobserved components of the labor market outcomes. As the individual does not know whether any given pre-treatment period corresponds to $K_{it} = -1$, $K_{it} = -2$, or any other period prior to the accident, the assumption of unpredictable treatment timing is plausible in this setting. That is, our identifying assumption relies on the fact that we consider "true" health shocks in contrast to hospital stays that may have been scheduled in advance or may be predictable due to a slowly worsening health condition. We check this assumption by visually and formally inspecting the existence of pre-trends as described above. In addition, the inclusion of individual fixed effects and the use of a balanced panel address potential bias due to attrition.[17]

[15]Linear pre-trends can never be detected in the data. However, as discussed by Borusyak and Jaravel (2017), if the event timing is indeed correlated with unobservables, it is unlikely that the pre-trends will be exactly linear. Setting the omitted categories far apart, makes it less likely that a linear pre-trend, perhaps not statistically significant, will be visible. Hence, we follow Borusyak and Jaravel (2017) and set the omitted categories far apart, which increases the usefulness of the graph used to check for pre-trends by focusing attention on nonlinearities. The F-statistic used to test for pre-trends is invariant to the choice of the omitted categories.

[16]Note that the F-statistic may exceed a conventional critical value for two reasons even if the estimation results do not exhibit clear pre-trends. First, the test is two-sided, so it may pick up positive and negative pre-treatment effects that nevertheless do not follow any systematic trend. Second, due to the large sample size and precisely measured labor market outcomes, we expect a relatively small p-value for this test regardless of any potential pre-trends.

[17]For instance, the individual fixed effect specification and the balanced panel address possible bias due

We only consider individuals who had an accident during the study period. Estimates relying on the comparison of individuals with and without accidents may be biased as a consequence of unobserved differences between those who are prone to having accidents and those who never had an accident, the control group. This research design is similar to the one used by Fadlon and Nielsen (2017), who construct a control group that had a health shock Δ years later than the treatment group. In their main specification, $\Delta = 5$.[18] Instead of using a specified difference between the timing of health shocks between treatment and control, we follow the generalized approach of Dobkin et al. (2018), which exploits the timing of health shocks in a *nonparametric event study* without explicitly designating individuals with later shocks as the control group.

To summarize the effect of an accident on employment, we also estimate the "canonical" DD regression

$$Y_{it} = \alpha_i + \beta_t + \gamma D_{it} + \varepsilon_{it}, \tag{2}$$

where $D_{it} = 1\{K_{it} \geq 0\}$, i.e., D_{it} is a post-accident indicator.[19] Although specification (2) is widely used in the applied literature, γ does not represent the true effect of an accident on labor market outcomes unless the dynamic treatment effects γ_k in regression (1) are equal for all $k \geq 0$.

As Borusyak and Jaravel (2017) show, γ in regression (2) can be expressed as a weighted average of the γ_k's in regression (1) with the weights decreasing with relative time post-accident and possibly becoming negative for large k. That is, the canonical DD estimator puts "too much" weight on dynamic treatment effects immediately after the treatment and "too little" or even negative weight on effects further in the future. Specifically, the weight for relative time period k can be obtained as the coefficient ω_k in the regression

$$\mathbf{1}\{K_{it} = k\} = \alpha_i + \beta_t + \omega_k D_{it} + e_{it},$$

where the variable D_{it} is the post-accident indicator included in model (2). In our data, we estimate $\hat{\omega}_0 = 0.1503$, $\hat{\omega}_{12} = 0.0342$, $\hat{\omega}_{24} = -0.0066$, and $\hat{\omega}_{36} = -0.0291$, which shows that regression (2) puts disproportionate weight on the month when the accident occurred

to a correlation between mortality and labor market outcomes. Another attraction of the balanced panel specification is that it allows us to examine the pattern of pre-trends and post-accident effects without concerns that they might be driven by compositional changes.

[18]The approach of Fadlon and Nielsen (2017) introduces the trade-off between having to choose between a more comparable control group (small Δ) and a longer follow-up period (large Δ).

[19]In contrast to the most commonly used DD model, specification (2) does not include a typical control group (individuals who never had an accident), but it rather uses the same comparison strategy as our dynamic specification (1), i.e., individuals who had an accident later during the study period serve as a control group for those who had an accident earlier.

and negative weight on effects two or more years after the health shock.[20] To avoid this weighting scheme, we obtain a second aggregate treatment effect by calculating the sample average of the dynamic treatment effects $\hat{\gamma}_k$. As we observe all individuals in the sample for the full 36 months after the accident, this sample average amounts to $\bar{\hat{\gamma}} = \frac{1}{37}\sum_{k=0}^{36}\hat{\gamma}_k$. This aggregate treatment effect does not suffer from the negative weighting inherent in the canonical DD estimator and is easily interpretable as the average employment or earnings effect of an accident during the first three years following the health shock. We also calculate year-specific treatment effects for years 1 to 3 as follows:

$$\bar{\hat{\gamma}}^1 = \frac{1}{13}\sum_{k=0}^{12}\hat{\gamma}_k, \quad \bar{\hat{\gamma}}^2 = \frac{1}{12}\sum_{k=13}^{24}\hat{\gamma}_k, \quad \text{and} \quad \bar{\hat{\gamma}}^3 = \frac{1}{12}\sum_{k=25}^{36}\hat{\gamma}_k. \tag{3}$$

For comparison purposes, we report both $\hat{\gamma}$ estimated from regression (2) and $\bar{\hat{\gamma}}$, $\bar{\hat{\gamma}}^1$, $\bar{\hat{\gamma}}^2$, and $\bar{\hat{\gamma}}^3$, with their clustered standard errors (obtained using the Delta method in the case of $\bar{\hat{\gamma}}$ etc.).

5 Results

We now present and discuss our results. First, we estimate dynamic and average treatment effects using the sample described in Section 3. Then, we assess treatment effect heterogeneity. Lastly, we perform robustness checks for our main results. We postpone a detailed interpretation of our results to Section 6, where we discuss potential explanations for our findings.

5.1 Dynamic Treatment Effects

We first check for pre-trends by plotting the path of the estimated dynamic treatment effects $\hat{\gamma}_k$ before and after the accident. Panel (a) of Figure 1 shows the estimated dynamic treatment effects on employment and panel (b) plots the analogous effects on monthly earnings. We observe that there is no evident pre-trend for both outcomes, which is consistent with our initial hypothesis that the type of health shocks considered here are truly unanticipated events. Although a few statistically significant pre-treatment effects on employment occur in year 3 before the accident, there is no discernible overall trend, and the effects within

[20]Negative weighting is a particularly important issue in our setting because we use monthly data and hence have a long panel with 36 post-accident periods. In contrast, most existing studies use yearly data where negative weighting may not become a problem unless data from many years are available.

two years before the accident are not statistically significant at the 5% level. For monthly earnings, none of the pre-treatment effects are statistically significant.

In addition, Table 2 shows the p-values for F-tests of the joint null hypothesis that the pre-accident effects $\{\hat{\gamma}_{-35}, \ldots, \hat{\gamma}_{-2}\}$ are equal to zero. We calculate the p-value for the three, two, and one years before the accident. The treatment effects in months 36 and 1 before the accident are excluded, see Section 4. The p-values range between 0.16 and 0.6, with the exception of the p-value for employment effects during the entire three years before the accident, which equals 0.021. However, as discussed in footnote 16, a p-value between 2% and 5% is not necessarily concerning in this context because the post-accident effects are very precisely estimated. Hence, overall, we fail to reject to null hypothesis of no pre-treatment effects.

In sum, the results shown in Figure 1 and Table 2 support the assumption of no pre-trends. In the following analyses, we set all pre-accident coefficients to zero and proceed to estimate only the dynamic post-accident treatment effects.[21] For comparison purposes, we also estimate the treatment effect derived from the canonical DD regression (2).

Column (1) of Table 3 shows the canonical DD treatment effect for employment. According to this estimate, employment declines by 5.9 percentage points or 8% following an accident. In contrast, the average of the dynamic treatment effects in column (2) of Table 3 indicates a reduction in employment by 11.2 percentage points or 14%. The large difference between these two estimates can be reconciled by the dynamic treatment effects in panel (a) of Figure 1. The initial effect of the accident is relatively small, but the canonical DD estimator puts the most weight on the accident month. Over time, the effects become larger in absolute value, but they are weighted by smaller and eventually negative weights. Given that treatment effects vary over time, the effects exhibited in column (2) of Table 3 are our preferred estimates for the average dynamic effect on employment. These and all other results in Table 3 are statistically significant at the 1% level.

In addition, we observe a persistent decline in employment over time in column (2) of Table 3. Employment falls by 7.6 percentage points or 10% in the sixth month after the accident, and by 16.7 percentage points or 21% at the end of the third year. On average, accidents reduce the probability of being employed by 8.4 percentage points (11%) in the first year of the post-accident period, by 11.2 percentage points in the second year (14%), and by 14.8 percentage points (19%) in the third year.

Next, we show the estimated effect of accidents on earnings in columns (3) and (4) of

[21] Tables A9 and A10 in the Online Appendix present the estimated parameters of the restricted fully dynamic model and semi-dynamic model, respectively.

Table 3. We observe again that the canonical DD average treatment effect is smaller than the one derived from the dynamic model; this divergence can be explained by the same reasons as stated above. Hereafter we focus the discussion on the treatment effects derived from the dynamic model. Individuals exposed to an accident exhibit a decline in earnings by US$93, which is equivalent to a 16% drop relative to the pre-accident average. In addition, the point estimates in column (4) of Table 3 suggest that the decline in monthly earnings associated with accidents grows over time. Specifically, we observe a decline in monthly earnings by US$65 or 12% in the sixth month after the accidents and by US$144 or 25% at the end of the third year. On average, monthly earnings decline by US$60 (11%) during the first year, US$95 (17%) during the second year, and US$126 (22%) during the third year after the accident.[22]

A comparison between our findings and existing estimates in the literature is not straightforward because existing studies mostly rely on data from the United States or European countries, whereas ours uses information collected from an emerging economy. Moreover, as discussed in the introductory section, existing studies vary regarding the frequency of the data used and/or the type of health shock considered. Dobkin et al. (2018), who rely on an empirical strategy similar to ours but use U.S. data, find that a hospital admission reduces the probability of being employed by 8.9 percentage points in the first year after admission, and by 11.1 percentage points in the third year after admission. This effect represents a 12–15% decline in employment relative to the pre-admission average. The authors estimate that annual earnings decline by 20% relative to the pre-admission average. Hence, their findings are consistent with ours. Interestingly, both Dobkin et al.'s (2018) evidence and ours suggest that the decline in employment and earnings following a health shock is persistent, or even increasing, over time. In Section 6, we discuss how idiosyncratic characteristics of the labor market and health system may contribute to understanding our findings from Chilean data.

Other studies estimate the causal effect of accidents on labor market outcomes by building counterfactuals based on samples of non-injured individuals. Halla and Zweimüller (2013) use an empirical design that combines matching and difference-in-differences approaches to estimate the labor market effects of accidents occurring on the way to and from work,

[22] In our baseline sample, 60% of the individuals enrolled in FONASA belong to groups A or B. Those individuals receive free health care from all the providers in the public network, as we explained in Section 2. In addition, 15% of FONASA beneficiaries are enrolled in group C and pay up to 10% of the health service provision, while the remaining 25% is enrolled in group D, who pay up to 20% of the health service provision. Then, in practice, private health expenditures of FONASA beneficiaries is insignificant and not directly related to the type of accident they face since copayments are strongly limited. Thus, the main economic consequences from having an accident for FONASA beneficiaries come from the labor market and not from out-of-pocket medical spending; the public health care system in Chile may not be very efficient, but people are insured against these types of financial losses.

which they interpret as a negative health shock. Using data from Austrian mandatory social accident insurance, they find persistent negative effects of health shocks on employment and earnings. This result is consistent with the evidence provided by Dobkin et al. for the U.S. and ours for Chile. However, Halla and Zweimüller (2013) report smaller effects than those we find using Chilean data. Crichton, Stillman, and Hyslop (2011) find similar results as in Halla and Zweimüller (2013) using data from the New Zealand health insurance system, which is similar to the Austrian system.

The large effects documented in our paper for Chile and in Dobkin et al. (2018) for the United States, relative to the effects found by Halla and Zweimüller (2013) for Austria and by Crichton, Stillman, and Hyslop (2011) for New Zealand, raise the interesting question of how institutional settings impact the labor market effect of health shocks. Indeed, as pointed out by García-Gómez (2011), differences in social security arrangements may help explain differences in the employment consequences of health shocks across countries. For example, in the Austrian health insurance system, basically every resident has access to free health-care utilization and rehabilitation. One may speculate that negative health shocks might have less detrimental effects in that type of system than in others where adequate health treatment is prevented by liquidity constraints (such as in the U.S. system) or by low-quality health care services (such as in the Chilean public system).

A further understanding on how different social security arrangements shape the trade-off between sufficient protection and the moral hazard problem inherent in granting generous benefits may guide the optimal design of social insurance policies. A stepping stone in that direction is Mommaerts, Raza, and Zheng (2019). The authors follow the event study design of Dobkin et al. (2018) to estimate the effect of hospitalizations on labor market outcomes in the United States, China, and 13 countries in Europe. They find that, in contrast to the U.S., where hospitalizations lead to large decreases in earnings, individuals in Northern and Southern Europe are largely protected from negative economic outcomes. Mommaerts et al.'s evidence strongly suggests that the institutional setting is a key determinant of the labor market consequences of health shocks.

Besides differences in institutional settings, studies relying on different empirical designs are not directly comparable to each other. We estimate the effects of accidents on labor market outcomes using an approach similar to Halla and Zweimüller (2013) and Crichton, Stillman, and Hyslop (2011) in Online Appendix C and find smaller effects than in our main results described above. However, the results in the Online Appendix do not represent credible causal effects because, as we explicitly show, the parallel trends assumption that is required for a DD approach is not met.

5.2 Treatment Effect Heterogeneity

We now investigate how the labor market effects of accidents vary across individual characteristics. Specifically, we estimate the dynamic specification for different age, education, and industry groups. We also asses how the effects of accidents vary according to the severity of the accident. We focus the discussion on the average dynamic treatment effects, but we also report the canonical DD estimates for comparison purposes.

We first study heterogeneity by age. We consider the impact of accidents for two age groups: individuals who are 22–49 years old at the time of accident, and individuals who are 50–61 years old. In panel (a) of Table 4, we observe a larger decline in employment and earnings for older individuals. The probability of employment falls by 10.6 percentage points in the younger group, which represents a 14% decline relative to the pre-accident average. Instead, individuals in the older group exhibit a decline in the probability of employment by 17.6 percentage points or 22% of the pre-accident average.

In addition, column (5) of Table 4 shows the effect of accidents on monthly earnings for the two age groups. We observe again that senior individuals experience a larger decline in earnings after the accident. Specifically, monthly earnings decline by US$93 in the younger group, whereas earnings fall by US$110 in the older group. Relative to the pre-accident means, these effects represent a decline in monthly earnings by 16% and 21% for the younger group and the older group, respectively.

We now assess heterogeneity across education groups. To do so, we consider two education categories: a high school degree or less education and at least some postsecondary education. We observe, in panel (b) of Table 4, that more educated individuals experience a slightly smaller decline in the probability of being employed after the accident. In addition, column (5) of Table 4 shows that the level of earnings loss after an accident is larger for more educated individuals than for those with less education (US$188 and US$75, respectively). However, in parallel, column (6) of Table 4 also shows that more educated individuals exhibit higher pre-accident earnings than less educated individuals. Overall, relative to the pre-accident mean, the decline in earnings in the two education groups is roughly the same, around 15%.[23]

Next, we study the differential impact of accidents across industries. In general, industries such as agriculture, fishing, mining, manufacturing, construction, and transportation are 'brawn-intensive,' whereas services in sectors such as retail, finance, health, and education are 'brain-intensive' (Ngai and Petrongolo, 2017). We use this fact to classify industries

[23]Interestingly, we observe that the canonical DD effect is larger (in absolute value) for the group with less education, which is the opposite conclusion reached by the dynamic model. The negative weighting problem inherent in the canonical DD (see Section 4) suggests that earning losses (in levels) are relatively larger for less educated individuals right after the accident, but this pattern is reversed later on.

into two groups: the primary/secondary sectors (agriculture, fishing, mining, manufacturing, construction, and transportation) and the tertiary or service sector (wholesale, retail, restaurant, finance and real estate, health, and education).

Panel (c), column (2) of Table 4 shows that workers attached to the primary/secondary sectors exhibit a larger fall in employment, compared to those participating in the tertiary sector (12.5 versus 11.2 percentage points or 15% versus 13%). We also observe in column (5) of Table 4, that individuals working in the primary/secondary sectors experience a 20% decline in monthly earnings relative to the pre-accident average, whereas the analogous figure for those attached to the tertiary sector is 16%. Hence, the labor market effects of accidents are less pronounced in industries related to the service sector.[24]

We next discuss the relation between accident severity and employment and earnings losses. We proxy accident severity by the number of days that individuals spend in the hospital following an accident, using two weeks as the cutoff criteria. Shorter stays are associated with a 10.8 percentage point (14%) drop in employment rates and a 16% earnings decline, whereas individuals with longer stays experience a reduction in employment by 19.3 percentage points or 26% and a reduction in earnings by 30% (see panel (d) of Table 4). Hence, individuals with longer hospital stays experience a larger decline in both the probability of employment and monthly earnings compared to those who stay less than two weeks.

The results of Table 4 suggest that the average dynamic treatment effect is heterogeneous across age, education, and industry groups. Table 4 also shows that individuals with longer hospital stays exhibit larger employment and earnings losses. We now provide a formal test of treatment effect heterogeneity. To do so, we estimate the semi-dynamic model including interaction terms between the treatment terms in regression (1) and the relevant group indicator:

$$Y_{it} = \alpha_i + \beta_t + \sum_{k=0}^{36} \gamma_k \mathbf{1}\{K_{it} = k\} + \sum_{k=0}^{36} \delta_k \mathbf{1}\{K_{it} = k\} Z_i + u_{it}, \tag{4}$$

where Z_i is an indicator for one of the two categories for each characteristic. For example, for heterogeneity by age, we include interactions with an indicator that equals one if the individual was 50–61 years old at the time of the accident. Then, we use an F-test to test the joint null hypothesis of no effect of the interaction terms: $H_0 : \delta_k = 0, k = 0, \dots, 36$. Panels (a) through (d) in Table 5 present the results for each characteristic. We reject the joint null hypothesis of no effect of the interaction terms for the employment regressions

[24]In addition, injured individuals do not exhibit evident adaptive behavior. Specifically, in our sample, 60% of individuals do not move to a different industry, whereas 85% do not move between the primary/secondary and tertiary sectors, after an accident.

that include the education, industry and accident severity indicators and for all earnings regressions at the 1% level. For the age indicator in the employment regression, the p-value equals 0.06 (see Table 5).

Overall, even though we cannot claim causality at this stage of the analysis, the results in this subsection suggest that older individuals and those who are attached to the primary and secondary sectors suffer larger labor market losses than other groups. We also find that the decline in employment following an accident is larger for less educated workers and individuals with more severe health shocks. In Section 6, we conjecture potential explanations for these findings.

5.3 Robustness

In this subsection, we conduct additional analyses that aim to assess the sensitivity of our results to different sample restrictions. We discuss our results in terms of the percentage of change relative to the pre-accident mean.[25] Table 6 presents the results.

In our main sample, the average number of accidents is 1.28 with a standard deviation of 0.75. Furthermore, 80% of individuals experienced only one accident during the period of analysis. Our first robustness analysis restricts the sample of individuals to those who experience only a single accident during the period of analysis (specification R1). The results in Table 6 show a decline in employment by 12% and a decline in monthly earnings by 14%, relative to the pre-accident mean. These values are slightly smaller than those derived from our baseline specification. This difference is indeed expected since individuals who suffer multiple accidents are less likely to quickly re-enter the labor force.

We next explore how our results change when we consider samples of individuals with different degrees of pre-accident labor market attachment, which differs from our baseline specification that included individuals who were employed at least 18 out of 36 pre-accident months. In particular, specification R2 includes individuals with full attachment during the pre-accident period, i.e., those who were employed in every pre-accident month; specification R3 includes those with at least 24 months of employment during the pre-accident period; specification R4 considers those with at least 6 months of employment during the pre-accident period; and specification R5 does not impose any sample restrictions. The results in Table 5 reveal a decline in employment between 7% (specification R5) and 15% (specification R3) and a decline in monthly earnings between 11% (specification R5) and 16% (specification R3), relative to the pre-accident average. Not surprisingly, individuals

[25]Different samples lead to different employment and earnings averages. Thus, the effects in levels are not necessarily comparable across different samples.

with lower labor market attachment experience a smaller relative decline in employment and earnings. Overall, selecting a sample with less labor market attachment before the accident reduces the effect of accidents, but our main conclusion stays.[26]

6 Discussion

We now discuss potential explanations for our results.[27] Our findings can be framed within a model of human capital with industry-specific skills and a differentiated impact of health events across the skill space. As discussed in Section 5.2, some industries are 'brawn-intensive,' whereas others are more 'brain-intensive.' Health events that affect mechanical skills should be more disabling for workers attached to 'brawn-intensive' industries. In contrast, health shocks affecting cognitive skills are likely more relevant for workers in 'brain-intensive' industries. Therefore, different types of accidents disable different types of skills, and thus, should trigger heterogenous labor market effects on workers at different jobs. Furthermore, in a given industry, the same accident could produce different health consequences on workers with different characteristics; for example, it may take longer for older workers to recover from a fall. Lastly, the consequences of an accident could be exacerbated or attenuated by the efficiency of the health care system. We next discuss these issues in light of the evidence reported in this paper.

Our analysis of treatment effect heterogeneity shows that the impact of accidents on both employment and earnings is less pronounced for workers employed in the tertiary sector; that is, the wholesale, retail, restaurant, finance and real estate, education, and health sectors. These industries rely less heavily on manual labor than, for instance, manufacturing and construction. This evidence suggests that the accidents considered in our analysis disable skills that are more intensively used in the primary and secondary sectors, that is, the 'brawn-intensive' industries. Indeed, we observe in Table A1 in the Online Appendix that most of the accidents in our sample injure parts of the body used to perform mechanical tasks; only 20% affect the head. In addition, Table 1 shows that almost 70% of the workers in our sample with an observed industry are employed in the primary or secondary sector. Furthermore, the distribution of accidents by diagnosis and causes does not seem to be different across

[26] Online Appendices B and C present two additional empirical exercises. In Online Appendix B, we build an alternative measure of income that includes simulated unemployment benefits. Then, we estimate the dynamic effects considering this alternative measure of income as the outcome. In Online Appendix C, we estimate the effect of accidents on labor market outcomes using a "traditional" control group consisting of individuals who did not have an accident.

[27] To be clear, this section is speculative and only aims to discuss a possible economic setting in which our main findings might be framed, but does not intent to present causal findings.

sectors, as we show in Online Appendix A. Hence, the composition of the workforce (in terms of the type of industry), the types of accidents we consider, and the suggestive evidence we present regarding the differentiated effects of accident across industries help understand the persistent and negative labor market effects of accidents.

In addition, Table 1 shows that, among non-missing records, almost the entire workforce is enrolled in FONASA, the public insurance system. As described in Section 2, FONASA is a deficient system with long waiting times. Thus, the majority of workers in our sample indeed lack effective health care that would facilitate a quick return to the labor force after an accident. The poor quality of the Chilean public health care system is another element that helps understand our findings.

Our analysis in Section 5.2 also suggests a larger effect for older workers. As observed in Online Appendix A, younger and older workers exhibit similar types of accidents. Thus, heterogeneity in the type of accident does not seem to be the main driver behind the differentiated effects across age that we report in Table 4. A more plausible explanation comes from the fact that older individuals are closer to the mandatory retirement age, which is 65 in Chile, and thus, they are more likely to advance their date of retirement after a health shock (however, we cannot observe retirement in our data). Another potential explanation is that older workers might take longer to recover from accidents that impact the mechanical functionality of the body, which are the main type of accidents in our sample.

Our findings also suggest that human capital could have a (small) mitigating effect on the employment consequences of accidents. We find some differences in diagnoses and accident causes across education groups. For instance, we observe that highly educated individuals are more likely to have accidents classified as "Accidental exposure to other/unspecified factors," a catch-all category for likely less severe accidents, whereas individuals with low levels of education are more likely to suffer "Exposure to inanimate mechanical forces," likely work accidents (see the Online Appendix). This suggestive evidence could provide context to findings such as in Heinesen and Kolodziejczyk (2013) and Jeon and Pohl (2019), who estimate an educational gradient in the employment effects of cancer diagnoses. However, we do not find evidence of a large differential effect of accidents on labor market outcomes across education groups. Hence, in the Chilean context, our findings only suggest that education has protective effects for the type of injury, a mechanism that likely operates through occupational choice. In addition, our results do not support the existence of significant wage penalties for highly skilled workers after a temporary exit from the labor market (Anderson, Binder, and Krause, 2002; Sasser, 2005; Bertrand, Goldin, and Katz, 2010).

Overall, our results suggest that accidents that mainly disable mechanical skills may be particularly harmful in labor markets that are populated by 'brawn-intensive' jobs, and where workers lack a high-quality health care system. Our results also suggest, that beyond the type of accident and job, some workers might exhibit more difficulties to attenuate the health consequences of an accident; i.e., older and less educated workers.

7 Conclusion

In this paper, we use an event study approach and monthly administrative data to estimate the causal effect of accidents on employment and earnings. Our data stem from Chilean administrative records on monthly earnings and hospital discharges over a period spanning almost a decade. Using this data, we estimate dynamic and average treatment effects. We find that employment falls by about 8.4 percentage points in the first year, by 11.2 percentage points in the second year, and by 14.8 percentage points in the third year after the accident. This represents an average decline in employment of 14%, relative to the pre-accident average. In addition, we find that individuals exposed to accidents experience a decline in monthly earnings by US$60 in the first year, by US$95 in the second year, and by US$126 in the third year after the accident. On average, over the three years after the accident, monthly earnings fall by 16%, relative to the pre-admission average.

We hypothesize that the type of health shock we study and the idiosyncratic characteristics of the Chilean labor market and health insurance system allow us to rationalize the negative and persistent effects of accidents found in this paper. We also find evidence suggesting that the labor market consequences are more severe for older individuals and those who are attached to industries that rely more intensively on manual labor. Our evidence also suggests that human capital could have a (small) mitigating effect on the employment consequences of accidents. Furthermore, we observe significant differences in the earnings effect of accidents across education groups, although this effect is negligible when pre-accident earnings are taken into account.

Our findings are policy-relevant. We show that economic consequences of health shocks could go beyond explicit medical expenses. The evidence provided in this paper supports the need for a more vigorous discussion on mechanisms to facilitate a quick return to work for individuals experiencing a sudden decline in health. As described in Section 3, Chile's public FONASA system provides health care services of a lower quality than the private ISAPRE plans, in which only a minority of workers are enrolled. Hence, our findings highlight the need for health care reform that improves the quality of the public health care system in Chile. Such a reform would reduce health inequalities in Chile which, in light of the results of this paper, might contribute to lowering the high levels of income inequality documented in the country (see, e.g., Núñez and Tartakowsky, 2011). Last but not least, further investigation of the factors that could exacerbate or mitigate the labor market consequences of health shocks is an important avenue that future research should address; in this regard, the social security system emerges as a first-order candidate to explore (García-Gómez, 2011).

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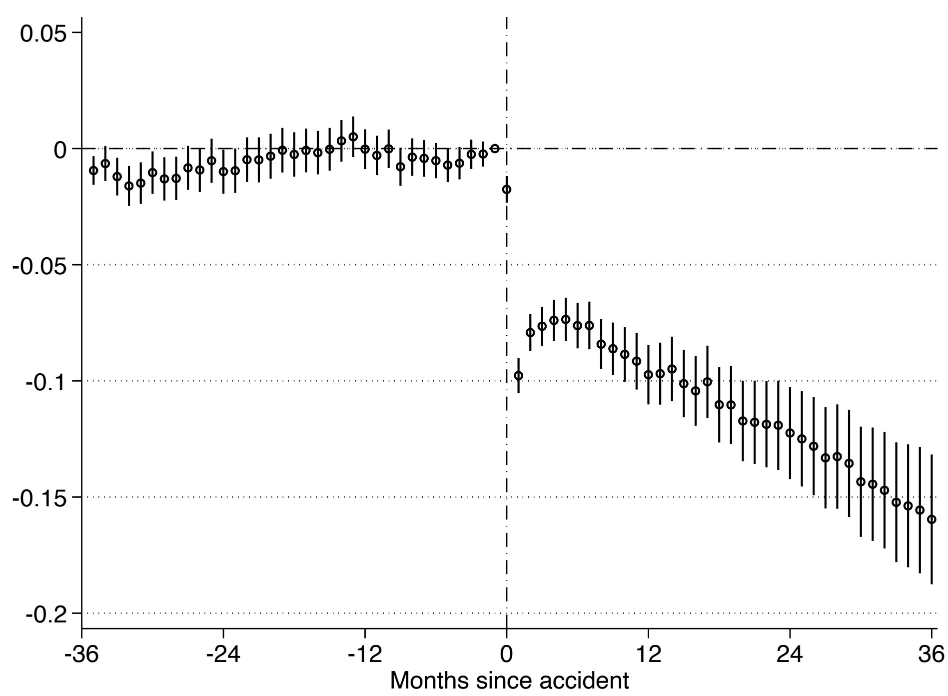
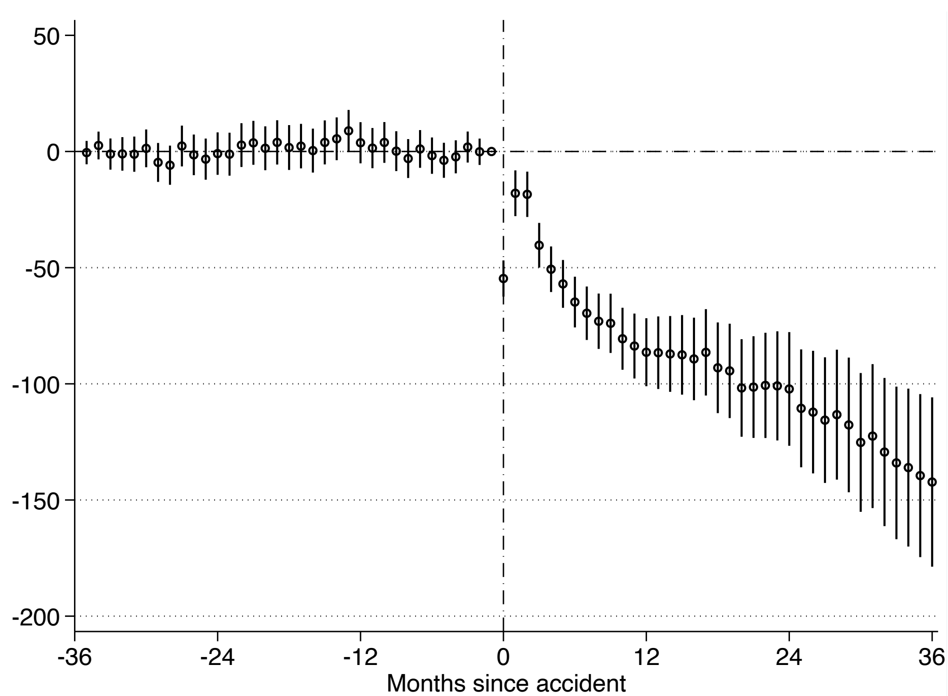
Figure 1: Estimated Treatment Effects from the Fully Dynamic Model

(a) Employment



(b) Monthly Earnings (in U.S. dollars)



Note: The graphs plot estimated dynamic treatment effects $\hat{\gamma}_k$ from regression (1) along with their 95% confidence intervals.

Table 1: Sample Characteristics

Labor market outcome	Mean (Std.dev.)
Pre-accident employment	0.780 (0.414)
Pre-accident monthly earnings	568.35 (653.15)
Age at time of accident	**Dist. (%)**
22–49	90.25
$\geq$ 50	9.75
Education	**Dist. (%)**
High school degree or lower	85.71
Post-secondary degree	8.41
Missing	5.88
Insurance	**Dist. (%)**
FONASA	51.79
ISAPRE	3.14
Missing	45.07
Industry	**Dist. (%)**
Agriculture and fishing	11.59
Mining	1.30
Manufacturing	8.93
Construction and transportation	32.91
Wholesale, retail, and restaurant	9.21
Finance and real estate	9.95
Education and health	5.30
Missing	20.81
Length of hospital stay	**Dist. (%)**
$\leq$ 7 days	84.12
8–14 days	10.00
$\geq$ 15 days	5.88

Notes: The age, education, and health insurance coverage variables are computed at the time of the accident. Industry refers to the pre-accident mode. Employment and earnings are measured at the monthly level. Monthly earnings are deflated using 2018 as the base year and expressed in U.S. dollars using an exchange rate of 600 Chilean pesos per 1 U.S. dollar.

Table 2: F-Tests for Pre-accident Effects

	Employment	Earnings
	(1)	(2)
F-test (p-value): γ_{-35} to γ_{-2}	0.0214	0.2340
F-test (p-value): γ_{-24} to γ_{-2}	0.1602	0.5983
F-test (p-value): γ_{-12} to γ_{-2}	0.2586	0.5390
Observations	940,605	940,605
Individuals	12,885	12,885

Notes: The dependent variables are an indicator of monthly employment and monthly earnings in U.S. dollars. $\hat{\gamma}_k$ is the dynamic treatment effect in month k relative to the accident. All regressions include individual and year-month fixed effects, see regression (1). The F-test (p-value) is the p-value of an F-test for the joint null hypothesis of no effect of the parameters γ_{-35} to γ_{-2} (corresponding to the three years before the accident), γ_{-24} to γ_{-2} (two years), and γ_{-12} to γ_{-2} (one year) on the outcome.

Table 3: Estimated Treatment Effects of Accidents on Employment and Monthly Earnings

	Employment		Earnings	
	Canonical (1)	Dynamic (2)	Canonical (3)	Dynamic (4)
Avg. post-accident	–0.0585***	–0.1120***	–38.3667***	–92.7604***
	(0.0035)	(0.0079)	(3.9903)	(9.7973)
6 months: $\hat{\gamma}_6$		–0.0761***		–65.3711***
		(0.0051)		(5.8223)
12 months: $\hat{\gamma}_{12}$		–0.0986***		–87.0286***
		(0.0067)		(7.9034)
24 months: $\hat{\gamma}_{24}$		–0.1266***		–103.1622***
		(0.0104)		(13.0582)
36 months: $\hat{\gamma}_{36}$		–0.1667***		–143.5367***
		(0.0145)		(19.2706)
First year (avg.): $\bar{\hat{\gamma}}^1$		–0.0835***		–60.3165***
		(0.0045)		(5.2051)
Second year (avg.): $\bar{\hat{\gamma}}^2$		–0.1123***		–95.0992***
		(0.0083)		(10.2668)
Third year (avg.): $\bar{\hat{\gamma}}^3$		–0.1483***		–125.9808***
		(0.0123)		(15.8904)
Individual fixed effects	Yes	Yes	Yes	Yes
Year-month fixed effects	Yes	Yes	Yes	Yes
Observations	940,605	940,605	940,605	940,605
Individuals	12,885	12,885	12,885	12,885

Notes: The dependent variables are an indicator of monthly employment and monthly earnings in U.S. dollars. The canonical average post-accident effect is the estimated coefficient $\hat{\gamma}$ from a DD regression that includes a single post-accident indicator, see regression (2). The dynamic average post-accident effect is the sample average of the dynamic treatment effects for the full 36 months after the accident: $\bar{\hat{\gamma}} = \frac{1}{37}\sum_{k=0}^{36}\hat{\gamma}_k$, where $\hat{\gamma}_k$ is the estimated effect k months after the accident, see regression (1). Coefficients $\hat{\gamma}_6$, $\hat{\gamma}_{12}$, $\hat{\gamma}_{24}$, and $\hat{\gamma}_{36}$ are the dynamic treatment effects 6, 12, 24, and 36 months after the accident. The average first year effect is the sample average of the dynamic treatment effects estimated for the first year after the accident: $\bar{\hat{\gamma}}^1 = \frac{1}{13}\sum_{k=0}^{12}\hat{\gamma}_k$. Analogously, the average second year effect is $\bar{\hat{\gamma}}^2 = \frac{1}{12}\sum_{k=13}^{24}\hat{\gamma}_k$, and the average third year effect is $\bar{\hat{\gamma}}^3 = \frac{1}{12}\sum_{k=25}^{36}\hat{\gamma}_k$. Standard errors clustered at the individual level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Treatment Effect Heterogeneity for the Estimated Effects of Accidents on Employment and Monthly Earnings

	Employment			Earnings				
	Canonical	Avg. dynamic	Mean (pre-accident)	Canonical	Avg. dynamic	Mean (pre-accident)	Obs.	Indiv.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(a) Age at time of accident								
22–49	–0.0559***	–0.1060***	0.7797	–36.7811***	–92.6012***	571.96	848,917	11,629
	(0.0036)	(0.0081)		(4.2117)	(10.3956)			
≥ 50	–0.0832***	–0.1761***	0.7864	–53.9271***	–110.2601 ***	534.94	91,688	1,256
	(0.0121)	(0.0273)		(12.4057)	(28.4470)			
(b) Education								
High school or lower	–0.0609***	–0.1095***	0.7722	–42.6808***	–75.2526***	486.32	806,285	11,045
	(0.0038)	(0.0085)		(3.7901)	(8.9329)			
Post-secondary	–0.0384***	–0.1016***	0.8307	–17.6309***	–187.7459***	1,282.45	79,059	1,083
	(0.0117)	(0.0277)		(25.8318)	(66.3957)			
(c) Industry								
Primary/secondary	–0.0818***	–0.1247***	0.8472	–63.9868***	–121.8737***	621.02	514,796	7,052
	(0.0043)	(0.0096)		(5.2640)	(12.7799)			
Tertiary sector	–0.0610***	–0.1116***	0.8765	–21.9584***	–112.2956***	710.93	230,096	3,152
	(0.0062)	(0.0142)		(8.8539)	(22.1610)			
(d) Length of hospital stay								
< 15 days	–0.0541***	–0.1075***	0.7822	–36.3752***	–90.4330***	574.81	885,344	12,128
	(0.0035)	(0.0080)		(4.1176)	(10.1461)			
≥ 15 days	–0.1314***	–0.1925***	0.7504	–71.5763***	–137.5215***	464.98	55,261	757
	(0.0180)	(0.0372)		(16.1350)	(35.8776)			

Notes: The dependent variables are an indicator of monthly employment and monthly earnings in U.S. dollars. The canonical average post-accident effect is the estimated coefficient $\hat{\gamma}$ from a DD regression that includes a single post-accident indicator, see regression (2). The average dynamic effect is the sample average of the dynamic treatment effects for the full 36 months after the accident: $\bar{\hat{\gamma}} = \frac{1}{37}\sum_{k=0}^{36}\hat{\gamma}_k$ where $\hat{\gamma}_k$ is the estimated effect k months after the accident, see regression (1). All regressions include individual and year-month fixed effects. Standard errors clustered at the individual level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: F-tests for Treatment Effect Heterogeneity in Semi-dynamic Models with Interactions

	Employment	Earnings	Obs.	Indiv.
	(1)	(2)	(3)	(4)
	(a) Age at time of accident			
F-test (p-value)	0.0604	< 0.0001	940,605	12,885
	(b) Education			
F-test (p-value)	0.0060	< 0.0001	885,344	12,128
	(c) Industry			
F-test (p-value)	0.0091	< 0.0001	744,892	10,204
	(d) Length of hospital stay			
F-test (p-value)	< 0.0001	< 0.0001	940,605	12,885

Notes: The dependent variables are an indicator of monthly employment and monthly earnings in U.S. dollars. All regressions include individual and year-month fixed effects. Each regression also includes the dynamic treatment terms $\mathbf{1}\{K_{it} = k\}$ interacted with an indicator for one of the categories for each characteristic, see regression (4). The F-test (p-value) is the p-value of an F-test for the joint null hypothesis of no effect of the interaction terms on the outcome.

Table 6: Estimated Treatment Effects of Accidents on Employment and Monthly Earnings, Robustness Checks

	Employment			Earnings				
	Canonical	Avg. dynamic	Mean (pre-accident)	Canonical	Avg. dynamic	Mean (pre-accident)	Obs.	Indiv.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Main specification	–0.0508***	–0.1120***	0.7800	–38.3667***	–92.7604***	568.35	904,605	12,885
	(0.0035)	(0.0079)		(3.9903)	(9,7973)			
Specification R1	–0.0508***	–0.0961***	0.7790	–33.4638***	–78.2825***	573.78	759,346	10,402
	(0.0037)	(0.0086)		(4.3690)	(11.0267)			
Specification R2	–0.0631***	–0.1055***	0.9223	–34.4355***	–97.1720***	763.19	505,379	6,923
	(0.0038)	(0.0087)		(5.9790)	(15.2629)			
Specification R3	–0.0659***	–0.1250***	0.8322	–41.7881***	–101.2243***	623.44	814,899	11,163
	(0.0035)	(0.0079)		(4.4061)	(10.8144)			
Specification R4	–0.0366***	–0.0689***	0.6770	–25.5751***	–62.4953***	479.75	1,153,765	15,805
	(0.0032)	(0.0075)		(3.4068)	(8.4352)			
Specification R5	–0.0270***	–0.0467***	0.6253	–20.3311***	–49.3601***	442.16	1,253,848	17,176
	(0.0030)	(0.0071)		(3.1747)	(7.8398)			

Notes: The dependent variables are an indicator of monthly employment and monthly earnings in U.S. dollars. The canonical average post-accident effect is the estimated coefficient $\hat{\gamma}$ from a DD regression that includes a single post-accident indicator, see regression (2). The average dynamic effect is the sample average of the dynamic treatment effects for the full 36 months after the accident: $\bar{\hat{\gamma}} = \frac{1}{37}\sum_{k=0}^{36}\hat{\gamma}_k$. All regressions include individual and year-month fixed effects. Specification R1: Individuals with one accident. R2: Individuals employed 36 months during the pre-accident period. R3: Individuals employed at least 24 months during the pre-accident period. R4: Individuals employed at least 6 months during the pre-accident period. R5: No sample restriction based on pre-accident employment. Standard errors clustered on the individual level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Online Appendix

A Additional Tables

Table A1: Distribution of Accidents by Diagnosis

Diagnosis	Percent
Injuries to the head	20.63
Injuries to the knee and lower leg	16.9
Injuries to the wrist, hand and fingers	15.19
Injuries involving multiple body regions	7.96
Injuries to the abdomen, lower back, lumbar spine, pelvis and external genitals	6.16
Injuries to the elbow and forearm	5.97
Injuries to the ankle and foot	4.67
Injuries to the thorax	4.59
Injuries to the shoulder and upper arm	4.54
Injury of unspecified body region	3.98
Burns and corrosion of external body surface, specified by site	2.53
Injuries to the hip and thigh	2.46
Toxic effects of substances chiefly nonmedicinal substances as the source	1.66
Injuries to the neck	1.11
Poisoning by, adverse effect of, and underdosing of, drugs medicaments and biological substances	0.56
Effects of foreign body entering through natural orifice	0.56
Other and unspecified effects from external causes	0.44
Certain early complications from trauma	0.09

Table A2: Distribution of Accidents by Cause

Cause of accident	Percent
Accidental exposure to other/unspecified factors	27.22
Slipping, tripping, stumbling and falls	20.5
Exposure to inanimate mechanical forces	14.36
Event of undetermined intent	11.36
Assault	7.69
Other land transport accidents	3.89
Car occupant injured in transport accident	2.19
Pedal cycle rider injured in transport accident	1.92
Pedestrian injured in transport accident	1.75
Overexertion, travel and privation	1.74
Exposure to animate mechanical forces	1.25
Contact with heat and hot substances	0.86
Exposure to smoke, fire and flames	0.75
Motorcycle rider injured in transport accident	0.7
Accidental poisoning by and exposure to noxious substances	0.64
Other and unspecified transport accidents	0.64
Contact with venomous animals and plants	0.59
Exposure to electric current, radiation and extreme ambient air temperature and pressure	0.43
Bus occupant injured in transport accident	0.39
Occupant of pick-up truck or van injured in transport accident	0.34
Occupant of heavy transport vehicle injured in transport accident	0.28
Sequelae of external causes of morbidity and mortality	0.25
Exposure to forces of nature	0.12
Water transport accidents	0.08
Occupant of three-wheeled motor vehicle injured in transport accident	0.04
Legal intervention, war operations	0.03
Accidental non-transport drowning and submersion	0.02

Table A3: Distribution of Accidents by Diagnosis and Age

Diagnosis	Percent	
	22–49	≥ 50
Injuries to the head	20.62	20.65
Injuries to the knee and lower leg	17.06	15.47
Injuries to the wrist, hand and fingers	15.31	14.04
Injuries involving multiple body regions	7.91	8.37
Injuries to the abdomen, lower back, lumbar spine, pelvis and external genitals	6.21	5.66
Injuries to the elbow and forearm	5.96	6.06
Injuries to the ankle and foot	4.55	5.74
Injuries to the thorax	4.51	5.34
Injuries to the shoulder and upper arm	4.51	4.86
Injury of unspecified body region	3.96	4.23
Burns and corrosion of external body surface, specified by site	2.53	2.47
Injuries to the hip and thigh	2.36	3.35
Toxic effects of substances chiefly nonmedicinal substances as the source	1.66	1.67
Injuries to the neck	1.16	0.72
Poisoning by, adverse effect of, and underdosing of, drugs medicaments and biological substances	0.62	0
Effects of foreign body entering through natural orifice	0.54	0.8
Other and unspecified effects from external causes	0.44	0.48
Certain early complications from trauma	0.09	0.08

Table A4: Distribution of Accidents by Cause and Age

Cause of accident	Percent	
	22–49	≥ 50
Accidental exposure to other/unspecified factors	27.48	24.78
Slipping, tripping, stumbling and falls	20.03	24.86
Exposure to inanimate mechanical forces	14.25	15.38
Event of undetermined intent	11.41	10.92
Assault	8.09	3.98
Other land transport accidents	3.84	4.38
Car occupant injured in transport accident	2.25	1.59
Pedal cycle rider injured in transport accident	1.83	2.71
Pedestrian injured in transport accident	1.71	2.07
Overexertion, travel and privation	1.77	1.43
Exposure to animate mechanical forces	1.25	1.27
Contact with heat and hot substances	0.82	1.27
Exposure to smoke, fire and flames	0.76	0.64
Motorcycle rider injured in transport accident	0.74	0.32
Accidental poisoning by and exposure to noxious substances	0.65	0.48
Other and unspecified transport accidents	0.64	0.72
Contact with venomous animals and plants	0.59	0.56
Exposure to electric current, radiation and extreme ambient air temperature and pressure	0.45	0.24
Bus occupant injured in transport accident	0.38	0.48
Occupant of pick-up truck or van injured in transport accident	0.33	0.48
Occupant of heavy transport vehicle injured in transport accident	0.22	0.8
Sequelae of external causes of morbidity and mortality	0.25	0.24
Exposure to forces of nature	0.1	0.24
Water transport accidents	0.09	0
Occupant of three-wheeled motor vehicle injured in transport accident	0.03	0.08
Legal intervention, war operations	0.03	0.08
Accidental non-transport drowning and submersion	0.02	0

Table A5: Distribution of Accidents by Diagnosis and Education

Diagnosis	Percent	
	High school or lower	Post-secondary
Injuries to the head	21.38	14.25
Injuries to the knee and lower leg	15.24	29.05
Injuries to the wrist, hand and fingers	15.25	13.87
Injuries involving multiple body regions	8.13	7.17
Injuries to the abdomen, lower back, lumbar spine, pelvis and external genitals	6.52	3.45
Injuries to the elbow and forearm	5.99	5.77
Injuries to the ankle and foot	4.62	5.03
Injuries to the thorax	4.76	3.82
Injuries to the shoulder and upper arm	4.45	5.68
Injury of unspecified body region	4.04	3.07
Burns and corrosion of external body surface, specified by site	2.52	2.33
Injuries to the hip and thigh	2.59	1.77
Toxic effects of substances chiefly nonmedicinal substances as the source	1.73	1.3
Injuries to the neck	1.08	1.77
Poisoning by, adverse effect of, and underdosing of, drugs medicaments and biological substances	0.55	0.84
Effects of foreign body entering through natural orifice	0.61	0.28
Other and unspecified effects from external causes	0.45	0.47
Certain early complications from trauma	0.09	0.09

Table A6: Distribution of Accidents by Cause and Education

Cause of accident	Percent	
	High school or lower	Post-secondary
Accidental exposure to other/unspecified factors	26.15	35.92
Slipping, tripping, stumbling and falls	20.72	19.76
Exposure to inanimate mechanical forces	14.91	9.14
Event of undetermined intent	11.55	9.33
Assault	8.14	5.17
Other land transport accidents	3.86	3.42
Car occupant injured in transport accident	2.06	1.94
Pedal cycle rider injured in transport accident	2.07	0.74
Pedestrian injured in transport accident	1.78	1.57
Overexertion, travel and privation	1.37	4.62
Exposure to animate mechanical forces	1.31	1.02
Contact with heat and hot substances	0.91	0.37
Exposure to smoke, fire and flames	0.75	0.65
Motorcycle rider injured in transport accident	0.57	2.22
Accidental poisoning by and exposure to noxious substances	0.68	0.37
Other and unspecified transport accidents	0.64	0.74
Contact with venomous animals and plants	0.59	0.46
Exposure to electric current, radiation and extreme ambient air temperature and pressure	0.44	0.55
Bus occupant injured in transport accident	0.36	0.55
Occupant of pick-up truck or van injured in transport accident	0.34	0.28
Occupant of heavy transport vehicle injured in transport accident	0.28	0.28
Sequelae of external causes of morbidity and mortality	0.21	0.55
Exposure to forces of nature	0.13	0.09
Water transport accidents	0.07	0.18
Occupant of three-wheeled motor vehicle injured in transport accident	0.04	0.09
Legal intervention, war operations	0.04	0
Accidental non-transport drowning and submersion	0.02	0

Table A7: Distribution of Accidents by Diagnosis and Industry

Diagnosis	Percent	
	Primary/ secondary	Tertiary
Injuries to the head	19.88	19.69
Injuries to the knee and lower leg	16.39	20.26
Injuries to the wrist, hand and fingers	15.3	15.7
Injuries involving multiple body regions	8.5	7.15
Injuries to the abdomen, lower back, lumbar spine, pelvis and external genitals	6.22	5.58
Injuries to the elbow and forearm	5.98	6.09
Injuries to the ankle and foot	5.11	4.69
Injuries to the thorax	4.94	3.35
Injuries to the shoulder and upper arm	4.34	5.14
Injury of unspecified body region	4.07	3.7
Burns and corrosion of external body surface, specified by site	2.49	2.62
Injuries to the hip and thigh	2.31	2.23
Toxic effects of substances chiefly nonmedicinal substances as the source	1.74	1.31
Injuries to the neck	1.18	0.7
Poisoning by, adverse effect of, and underdosing of, drugs medicaments and biological substances	0.46	0.83
Effects of foreign body entering through natural orifice	0.56	0.48
Other and unspecified effects from external causes	0.46	0.38
Certain early complications from trauma	0.06	0.1

Table A8: Distribution of Accidents by Cause and Industry

Cause of accident	Percent	
	Primary/ secondary	Tertiary
Accidental exposure to other/unspecified factors	27.22	28.59
Slipping, tripping, stumbling and falls	20.82	20.12
Exposure to inanimate mechanical forces	15.13	13.2
Event of undetermined intent	11.65	11.81
Assault	6.77	6.28
Other land transport accidents	3.62	4.28
Car occupant injured in transport accident	2.21	2.22
Pedal cycle rider injured in transport accident	2.01	1.59
Pedestrian injured in transport accident	1.46	1.94
Overexertion, travel and privation	1.79	2.35
Exposure to animate mechanical forces	1.21	1.27
Contact with heat and hot substances	0.96	0.76
Exposure to smoke, fire and flames	0.77	0.6
Motorcycle rider injured in transport accident	0.5	1.24
Accidental poisoning by and exposure to noxious substances	0.6	0.63
Other and unspecified transport accidents	0.57	0.79
Contact with venomous animals and plants	0.62	0.48
Exposure to electric current, radiation and extreme ambient air temperature and pressure	0.41	0.38
Bus occupant injured in transport accident	0.45	0.29
Occupant of pick-up truck or van injured in transport accident	0.41	0.29
Occupant of heavy transport vehicle injured in transport accident	0.33	0.29
Sequelae of external causes of morbidity and mortality	0.2	0.41
Exposure to forces of nature	0.13	0.06
Water transport accidents	0.11	0.06
Occupant of three-wheeled motor vehicle injured in transport accident	0.01	0.03
Legal intervention, war operations	0.04	0
Accidental non-transport drowning and submersion	0	0.03

Table A9: Estimated Coefficients from the Restricted Fully Dynamic Model

	Pre-accident			Post-accident	
	Employment	Earnings		Employment	Earnings
	(1)	(2)		(3)	(4)
$\hat{\gamma}_{-35}$	–0.0094***	–0.4697	$\hat{\gamma}_{0}$	–0.0176***	–54.6654***
$\hat{\gamma}_{-34}$	–0.0064*	2.5937	$\hat{\gamma}_{1}$	–0.0977***	–17.9790***
$\hat{\gamma}_{-33}$	–0.0120***	–1.0949	$\hat{\gamma}_{2}$	–0.0792***	–18.4233***
$\hat{\gamma}_{-32}$	–0.0161***	–1.0343	$\hat{\gamma}_{3}$	–0.0765***	–40.3457***
$\hat{\gamma}_{-31}$	–0.0149***	–1.1523	$\hat{\gamma}_{4}$	–0.0739***	–50.6722***
$\hat{\gamma}_{-30}$	–0.0103**	1.3580	$\hat{\gamma}_{5}$	–0.0735***	–56.9870***
$\hat{\gamma}_{-29}$	–0.0131***	–4.6957	$\hat{\gamma}_{6}$	–0.0762***	–64.7961***
$\hat{\gamma}_{-28}$	–0.0128***	–5.8920	$\hat{\gamma}_{7}$	–0.0761***	–69.6157***
$\hat{\gamma}_{-27}$	–0.0083*	2.3655	$\hat{\gamma}_{8}$	–0.0842***	–73.0587***
$\hat{\gamma}_{-26}$	–0.0092*	–1.4371	$\hat{\gamma}_{9}$	–0.0861***	–73.9474***
$\hat{\gamma}_{-25}$	–0.0052	–3.2809	$\hat{\gamma}_{10}$	–0.0886***	–80.5935***
$\hat{\gamma}_{-24}$	–0.0099**	–0.9040	$\hat{\gamma}_{11}$	–0.0915***	–83.7285***
$\hat{\gamma}_{-23}$	–0.0095*	–1.1549	$\hat{\gamma}_{12}$	–0.0973***	–86.4042***
$\hat{\gamma}_{-22}$	–0.0048	2.7596	$\hat{\gamma}_{13}$	–0.0969***	–86.6056***
$\hat{\gamma}_{-21}$	–0.0049	3.7374	$\hat{\gamma}_{14}$	–0.0949***	–87.1368***
$\hat{\gamma}_{-20}$	–0.0033	1.3899	$\hat{\gamma}_{15}$	–0.1012***	–87.5283***
$\hat{\gamma}_{-19}$	–0.0007	3.9305	$\hat{\gamma}_{16}$	–0.1043***	–89.2848***
$\hat{\gamma}_{-18}$	–0.0025	1.7089	$\hat{\gamma}_{17}$	–0.1004***	–86.4469***
$\hat{\gamma}_{-17}$	–0.0008	2.3191	$\hat{\gamma}_{18}$	–0.1103***	–93.0949***
$\hat{\gamma}_{-16}$	–0.0018	0.3962	$\hat{\gamma}_{19}$	–0.1103***	–94.4273***
$\hat{\gamma}_{-15}$	–0.0003	3.9207	$\hat{\gamma}_{20}$	–0.1172***	–101.7773***
$\hat{\gamma}_{-14}$	0.0033	5.4721	$\hat{\gamma}_{21}$	–0.1178***	–101.4046***
$\hat{\gamma}_{-13}$	0.0051	8.9011*	$\hat{\gamma}_{22}$	–0.1187***	–100.6710***
$\hat{\gamma}_{-12}$	–0.0003	3.7676	$\hat{\gamma}_{23}$	–0.1191***	–100.8795***
$\hat{\gamma}_{-11}$	–0.0030	1.4642	$\hat{\gamma}_{24}$	–0.1224***	–102.1989***
$\hat{\gamma}_{-10}$	–0.0001	3.8965	$\hat{\gamma}_{25}$	–0.1250***	–110.5523***
$\hat{\gamma}_{-9}$	–0.0078*	0.1504	$\hat{\gamma}_{26}$	–0.1281***	–112.1745***
$\hat{\gamma}_{-8}$	–0.0037	–3.0247	$\hat{\gamma}_{27}$	–0.1331***	–115.6246***
$\hat{\gamma}_{-7}$	–0.0042	1.1005	$\hat{\gamma}_{28}$	–0.1326***	–113.2623***
$\hat{\gamma}_{-6}$	–0.0052	–1.7821	$\hat{\gamma}_{29}$	–0.1355***	–117.6869***
$\hat{\gamma}_{-5}$	–0.0071*	–3.7816	$\hat{\gamma}_{30}$	–0.1434***	–125.2181***
$\hat{\gamma}_{-4}$	–0.0063*	–2.2875	$\hat{\gamma}_{31}$	–0.1445***	–122.4944***
$\hat{\gamma}_{-3}$	–0.0025	1.9239	$\hat{\gamma}_{32}$	–0.1471***	–129.3604***
$\hat{\gamma}_{-2}$	–0.0023	–0.1065	$\hat{\gamma}_{33}$	–0.1522***	–134.0353***
			$\hat{\gamma}_{34}$	–0.1538***	–136.0534***
			$\hat{\gamma}_{35}$	–0.1556***	–139.5027***
			$\hat{\gamma}_{36}$	–0.1596***	–142.2681***

Notes: The dependent variables are an indicator of monthly employment and monthly earnings in U.S. dollars. $\hat{\gamma}_k$ is the dynamic treatment effect k months before/after the accident. All regressions include individual and year-month fixed effects. Observations = 940,605; individuals = 12,885. Standard errors clustered at the individual level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A10: Estimated Coefficients from the Semi-dynamic Model

	Employment	Earnings
	(1)	(2)
$\hat{\gamma}_0$	–0.0164***	–55.3749***
$\hat{\gamma}_1$	–0.0967***	–18.6550***
$\hat{\gamma}_2$	–0.0784***	–19.0686***
$\hat{\gamma}_3$	–0.0758***	–40.9642***
$\hat{\gamma}_4$	–0.0735***	–51.2708***
$\hat{\gamma}_5$	–0.0732***	–57.5697***
$\hat{\gamma}_6$	–0.0761***	–65.3711***
$\hat{\gamma}_7$	–0.0762***	–70.1883***
$\hat{\gamma}_8$	–0.0845***	–73.6331***
$\hat{\gamma}_9$	–0.0867***	–74.5288***
$\hat{\gamma}_{10}$	–0.0893***	–81.1852***
$\hat{\gamma}_{11}$	–0.0925***	–84.3345***
$\hat{\gamma}_{12}$	–0.0986***	–87.0285***
$\hat{\gamma}_{13}$	–0.0984***	–87.2531***
$\hat{\gamma}_{14}$	–0.0966***	–87.8110***
$\hat{\gamma}_{15}$	–0.1032***	–88.2333***
$\hat{\gamma}_{16}$	–0.1065***	–90.0222***
$\hat{\gamma}_{17}$	–0.1029***	–87.2162***
$\hat{\gamma}_{18}$	–0.1130***	–93.8968***
$\hat{\gamma}_{19}$	–0.1133***	–95.2599***
$\hat{\gamma}_{20}$	–0.1204***	–102.6383***
$\hat{\gamma}_{21}$	–0.1213***	–102.2930***
$\hat{\gamma}_{22}$	–0.1224***	–101.5856***
$\hat{\gamma}_{23}$	–0.1230***	–101.8187***
$\hat{\gamma}_{24}$	–0.1266***	–103.1622***
$\hat{\gamma}_{25}$	–0.1294***	–111.5406***
$\hat{\gamma}_{26}$	–0.1327***	–113.1876***
$\hat{\gamma}_{27}$	–0.1380***	–116.6628***
$\hat{\gamma}_{28}$	–0.1377***	–114.3260***
$\hat{\gamma}_{29}$	–0.1409***	–118.7761***
$\hat{\gamma}_{30}$	–0.1490***	–126.3330***
$\hat{\gamma}_{31}$	–0.1503***	–123.6352****
$\hat{\gamma}_{32}$	–0.1532***	–130.5270***
$\hat{\gamma}_{33}$	–0.1586***	–135.2276***
$\hat{\gamma}_{34}$	–0.1604***	–137.2713***
$\hat{\gamma}_{35}$	–0.1624***	–140.7461***
$\hat{\gamma}_{36}$	–0.1667***	–143.5367***
Observations	940,605	940,605
Individuals	12,885	12,885

Notes: The dependent variables are an indicator of monthly employment and monthly earnings in U.S. dollars. $\hat{\gamma}_k$ is the dynamic treatment effect k months after the accident. All regressions include individual and year-month fixed effects. Standard errors clustered at the individual level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B Safety Net in Chile

This appendix discusses the main elements of the safety net for workers in Chile. First, we describe the benefits included in the UIS, which covers unemployment of dependent workers. Then, we describe disability subsidies, which protect workers from earning losses generated by a temporary labor disability. We conclude by estimating the effect of an accident on a broader predicted income measure that includes some of the safety net benefits described in this appendix.

B.1 UIS Benefits

The UIS is composed of individual saving accounts and a solidarity fund. To receive payments from the individual saving accounts, unemployed workers must register a minimum of 12 months of contributions (continuous or discontinuous) from the date of the last withdrawal (or from the affiliation date) in the case of indefinite contracts, and a minimum of 6 months of contributions in the case of fixed-term contracts. Individuals can make between 1 and 13 monthly withdrawals from their individual accounts, as long as they have enough funds. Monthly benefits are determined by the average monthly earnings for the last 12 months (6 months) for workers with an indefinite-term (fixed-term) contract as shown in Table B1.

The UIS solidarity fund is for workers with an insufficient balance in their individual accounts. Access to the solidarity fund requires a minimum of 12 months of contributions into the solidarity fund during the last 24 months before the request.[28] Importantly, benefits are always financed first with the individual account and then supplemented with the solidarity fund. Individuals can make up to 5 (indefinite-term contracts) or 3 (fixed-term contracts) monthly withdrawals from the solidarity fund. The benefits from the solidarity fund are based on the average monthly earnings for the last three months before the request as shown in Table B1.[29]

[28]The last three contributions must be continuous and with the same employer. Individuals must have made a maximum of 10 withdrawals from the solidarity fund during the last 5 years.

[29]Individuals can make two additional withdrawals in periods where the national unemployment rate exceeds the average rate of the last four years by one percentage point.

Table B1: UIS Benefits by Month of Unemployment Spell

	First	Second	Third	Fourth	Fifth	Sixth+
Withdrawal from individual account						
Any type of contract	70%	55%	45%	40%	35%	30%
Withdrawal from solidarity fund						
Indefinite contract	70%	55%	45%	40%	35%	0
Maximum (in US$)	1,078	847	693	616	539	
Minimum (in US$)	323	254	208	185	162	
Fixed-term contract	50%	40%	35%	0	0	0
Maximum (in US$)	770	616	239			
Minimum (in US$)	231	185	162			

B.2 Disability Subsidies

Work and professional accidents are ruled by Law 16.744, which is built on two main pillars. The first pillar covers workers who suffer an accident or illness that results in a permanent disability. The second pillar aims at building a safety net for workers who suffer a temporal illness or accident that keeps them off work for a limited period of time, but who are able to resume working afterwards.

The first pillar includes a disability pension for workers whose earnings generating capacity is undermined as a consequence of a work accident or a professional illness. Workers who experience a reduction in their earning generating capacity equal to or greater than 40%, but less than 70%, are considered partially disabled, whereas those who experience a reduction of 70% or more are considered fully disabled. A special medical committee is the entity in charge of estimating the earning losses suffered by injured workers. The monthly disability benefit is paid until the age of retirement and reaches 35% of base earnings, determined by law, for partially disabled workers, and 70% of base earnings for fully disabled workers.

The second pillar is composed of a labor incapability subsidy (LIS). The LIS includes a subsidy for workers that certify an authorized medical leave as a consequence of a temporary labor disability. A medical leave is considered legally valid if it is recognized by the employer and is authorized by a special medical committee. This type of subsidies is classified into two categories: those related to a common illness and those related to a work accident or professional illness.

A common illness is defined as one that has no relation to the profession or work executed, but which prevents workers from maintaining their regular duties. This subsidy consists of compensation equivalent to 100% of the average monthly earnings received during the last three months before the medical leave period. The benefits are extended during the entire medical leave period, which must be approved by a special medical committee. Work accidents are defined as all injuries suffered by workers while performing their job or commuting to and from the workplace, and professional illnesses are those directly or indirectly related to work duties. In the case of the latter type of accident, benefits are the same as those granted for common illnesses, but are extended for a maximum period of 52 weeks, which can be prolonged to 104 weeks.

B.3 Effect of Accidents on Income

We conclude this appendix by estimating the impact of accidents on an outcome that includes some of the elements of the Chilean safety net. We need to predict benefits because we do not observe actual benefits in our data. First, we explain how we construct this additional outcome. Employers must make legal contributions to the UIS, even for workers with medical leave. Hence, in our dataset, we cannot identify workers who are on medical leave, and thus, who are receiving disability subsidies.[30] Therefore, we focus the analysis on UIS benefits. Unemployment benefits are registered in a separate module of the UIS dataset, to which we do not have access. However, as described above, the use of the funds accumulated in the UIS follows well-defined rules. This fact allows us to simulate, with a reasonable degree of accuracy, the benefits received by unemployed individuals in our sample, and thus, to build a broader measure of income as an additional outcome.

In Chile, approximately, 15% of workers exhibit a fixed-term contract. Furthermore, our sample includes workers with strong labor market attachment, and thus, who are likely to be ruled by an indefinite employment relationship. Hence, we assume that all the individuals in our sample receive unemployment benefits according to the scheme described above for indefinite-term contracts. In addition, we assume that individuals only use resources from their individual accounts because we do not observe the balance of individual UIS accounts, and thus, we cannot identify who meets the requirements of access to the solidarity fund. Using the latter criteria, we calculate the potential unemployment benefits for the unemployed workers in our sample (defined by zero earnings). We then add these benefits to the observed earnings, generating a broader income measure, which we refer to simply as income. Mean

[30] We observe the earnings of workers with a medical leave just as we do for the rest of the employed workers.

predicted pre-accident income is US$604, which is about 6% higher than mean pre-accident earnings. It should be noted, however, that this income measure underestimates total income because it does not include potential disability subsidies.

Table B2 presents the dynamic effects of an accident on monthly income which, as previously explained, includes predicted unemployment benefits. On average, income declines by US$90, which is equivalent to a 15% drop relative to the pre-accident mean. Hence, the effect on income is only slightly smaller than the effect on earnings (16%). This difference is mostly explained by the buffering role played by unemployment benefits, especially during the months following the accident. Concretely, we observe in Table B2 that monthly income declines by 8% in the first year after the accident, by 16% during the second year, and by 21% during the third year. The analogous effects for the case of earnings are 11%, 17%, and 22%, respectively (see Table 3). Due to the limitation of unemployment benefits for 13 months, the estimates are nearly identical in later years. Overall, this analysis suggests that a social safety net may be a relevant factor to buffer the fall in income of workers that lose their jobs as a consequence of an accident, especially during the period immediately following the accident.

Table B2: Estimated Treatment Effects of Accidents on Monthly Income

	Dynamic model
Avg. post-accident	–90.4061***
	(9.1742)
6 months: $\hat{\gamma}_6$	–56.4482***
	(5.0552)
12 months: $\hat{\gamma}_{12}$	–79.5182***
	(7.1305)
24 months: $\hat{\gamma}_{24}$	–107.7319***
	(12.2000)
36 months: $\hat{\gamma}_{36}$	–147.4150***
	(18.2124)
First year (avg.): $\bar{\hat{\gamma}}^1$	–49.6090***
	(4.6235)
Second year (avg.): $\bar{\hat{\gamma}}^2$	–95.1702***
	(9.5459)
Third year (avg.): $\bar{\hat{\gamma}}^3$	–129.4068***
	(15.0135)
Individual fixed effects	Yes
Year-month fixed effects	Yes
Observations	940,605
Individuals	12,885
Mean (pre-accident)	603.89

Notes: The dependent variable is monthly income in U.S. dollars. The dynamic average post-accident effect is the sample average of the dynamic treatment effects for the full 36 months after the accident: $\bar{\hat{\gamma}} = \frac{1}{37}\sum_{k=0}^{36}\hat{\gamma}_k$, where $\hat{\gamma}_k$ is the estimated effect k months after the accident, see regression (1). Coefficients $\hat{\gamma}_6$, $\hat{\gamma}_{12}$, $\hat{\gamma}_{24}$, and $\hat{\gamma}_{36}$ are the dynamic treatment effects 6, 12, 24, and 36 months after the accident. The average first year effect is the sample average of the dynamic treatment effects estimated for the first year after the accident: $\bar{\hat{\gamma}}^1 = \frac{1}{13}\sum_{k=0}^{12}\hat{\gamma}_k$. Analogously, the average second year effect is $\bar{\hat{\gamma}}^2 = \frac{1}{12}\sum_{k=13}^{24}\hat{\gamma}_k$, and the average third year effect is $\bar{\hat{\gamma}}^3 = \frac{1}{12}\sum_{k=25}^{36}\hat{\gamma}_k$. Standard errors clustered at the individual level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C Control Group Without Accidents

Our preferred empirical strategy uses the variation from the timing of hospitalizations to estimate the causal effect of accidents on labor market outcomes (employment and earnings), as in Dobkin et al. (2018). As we discuss in the main text, it is not clear that never-hospitalized individuals are a suitable control group because they may differ from individuals with accidents along several unobserved dimensions. However, to facilitate a comparison of our findings with those existing in the literature, in this appendix, we provide an additional analysis that considers a "traditional" approach with a control group that includes individuals who did not have an accident during the study period. We use inverse propensity score weighting (IPSW) to account for observed differences between the treatment and control groups, but we cannot control for time-varying unobserved differences between these groups, as previously discussed.

First, we assess the balance between the treatment and control groups before re-weighting by the IPSW. Imbens (2015) stresses the importance of the design stage and, in particular, of the overlap in the support of the covariates included to build the propensity score. Following this insight, we check the overlap in the support of the covariates between the treatment group and the control group using normalized differences.[31] Column (3) in Table C1 shows the normalized differences for selected covariates. They are all well below the rule-of-thumb value of 0.25 suggested by Imbens and Wooldridge (2009). Therefore, the overlap between the treatment group and the control group is satisfactory; individuals with and without accidents do not appear to be very different in observables characteristics, even before weighting.[32]

[31] The use of normalized differences to check the overlap is preferred over the use of t-statistics, because the former is independent of the sample size. The normalized difference for the covariate Z_k is defined as $\left(\bar{Z}_k^T - \bar{Z}_k^C\right) / \left[0.5\left(S^2_{Z_k^T} + S^2_{Z_k^C}\right)\right]^{1/2}$, where $\bar{Z}_k^T$ and $\bar{Z}_k^C$ are the sample mean of Z_k in the treatment group and the control group, respectively, and $S^2_{Z_k^T}$ and $S^2_{Z_k^C}$ are the corresponding sample variances.

[32] To make the estimation that follows more manageable, we draw a 10% random sample of individuals without an accident to serve as the potential control group.

Table C1: Unweighted and Weighted Means and Standard Deviations and Normalized Differences of Propensity Score Covariates

	Unweighted		Normalized	IPSW-weighted	
	Treat.	Control	difference	Treat.	Control
	(1)	(2)	(3)	(4)	(5)
Age at health shock	38.15	39.20	−0.136	38.10	38.10
	(7.649)	(7.915)		(7.640)	(7.641)
Less than high school ed.	0.595	0.543	0.106	0.595	0.595
	(0.491)	(0.498)		(0.491)	(0.491)
High school degree	0.316	0.322	−0.0144	0.316	0.316
	(0.465)	(0.467)		(0.465)	(0.465)
Post-secondary education	0.0891	0.135	−0.146	0.0891	0.0891
	(0.285)	(0.342)		(0.285)	(0.285)
Agriculture, fishing	0.115	0.0878	0.0914	0.116	0.116
	(0.320)	(0.283)		(0.321)	(0.321)
Mining	0.0131	0.0126	0.00399	0.0127	0.0127
	(0.114)	(0.112)		(0.112)	(0.112)
Manufacturing	0.0891	0.0882	0.00344	0.0870	0.0870
	(0.285)	(0.284)		(0.282)	(0.282)
Construction, transportation	0.330	0.307	0.0477	0.336	0.336
	(0.470)	(0.461)		(0.473)	(0.472)
Wholesale, retail, restaurant	0.0921	0.117	−0.0801	0.0872	0.0872
	(0.289)	(0.321)		(0.282)	(0.282)
Finance, real estate	0.0993	0.119	−0.0636	0.0991	0.0991
	(0.299)	(0.324)		(0.299)	(0.299)
Education, health	0.0529	0.0679	−0.0629	0.0505	0.0505
	(0.224)	(0.252)		(0.219)	(0.219)
Missing industry	0.208	0.200	0.0199	0.211	0.211
	(0.406)	(0.400)		(0.408)	(0.408)
Observations	12,833	52,843		12,080	30,119

To estimate the propensity score of an accident, we use a logit regression that includes the set of covariates shown in Table C1 and the year and month of the (placebo) accident.[33] We trim the sample based on the estimated propensity score to improve the balance between the treatment and control groups. Specifically, we exclude treated individuals whose propensity score is below the minimum or above the maximum propensity score in the control group and vice versa. As the last row in Table C1 shows, we exclude about 800 individuals from the treatment group and 21,000 individuals from the control group. Finally, we use the estimated propensity score to calculate the IPSW as follows:

$$\hat{w}_i^{ATET} = T_i + (1 - T_i)\frac{\hat{p}(Z_i)}{1 - \hat{p}(Z_i)}, \tag{5}$$

where $\hat{p}(Z_i)$ is the estimated propensity score conditional on covariates Z_i and $T_i = \{0, 1\}$ is the treatment indicator for having any health shock.

We then estimate the equivalent of regression (1), but include a control group, on a sample that is re-weighted by the IPSW. Specifically, we estimate the following regression:

$$Y_{it} = \alpha_i + \beta_t + \sum_{k=-36}^{36} \gamma_k \mathbf{1}\{K_{it} = k\} + \sum_{k=-36}^{36} \delta_k Acc_i \mathbf{1}\{K_{it} = k\} + u_{it}, \tag{6}$$

where Acc_i is an indicator that equals one if individual i had an accident during the study period and the other variables are defined as in the main text. Hence, we are interested in the coefficients $\delta_k, k = -36, \ldots, 36$ where we normalize δ_{-1} to zero. The coefficients δ_0 to δ_{36} measure the estimated effect of accidents on the outcomes (employment and earnings), and we use δ_{-36} to δ_{-2} to assess the parallel trends assumption. That is, we test whether the latter coefficients are statistically significant.

Figure C1 plots the estimated coefficients with their 95% confidence intervals.[34] We find a significant decline in employment and earnings following an accident. For both outcomes, the largest drop is observed immediately after the accident and persists during the entire period of analysis. The average decline in employment is about 5 percentage points (6%), whereas earnings fall, on average, by US$40 (7%). These effects are substantially smaller than in Figure 1 in the main text. Specifically, our main results show that employment drops continuously after an accident and the decline reaches 15% after about 2.5 years, and earnings fall by up to US$150 (25%). Therefore, the type of control group used has important implications for the estimated effects of accidents.

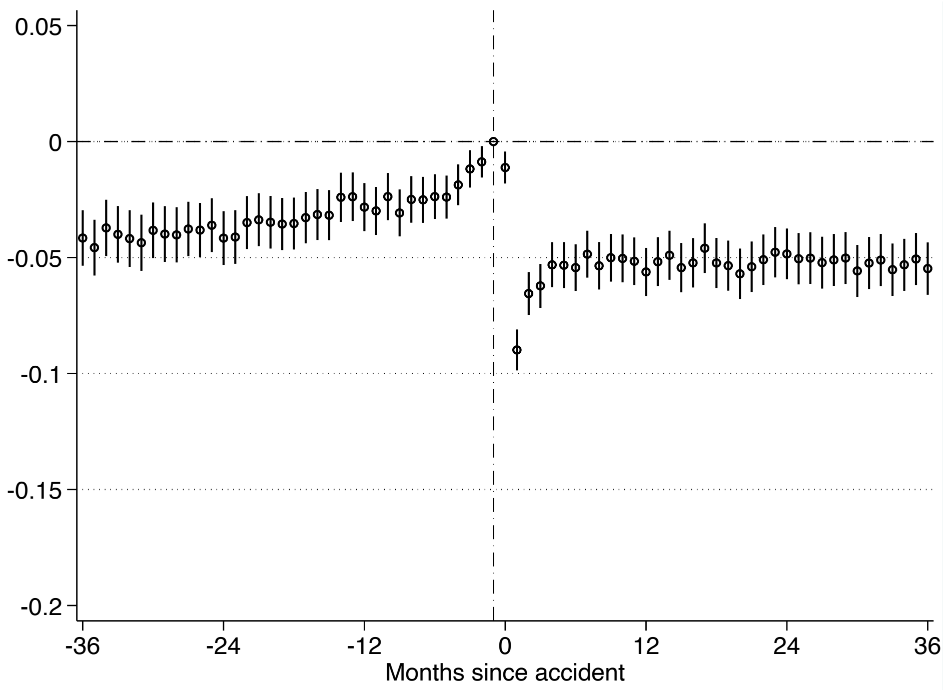
[33]We only observe a limited number of variables, which is typical for administrative data.

[34]We use the same scale as in Figure 1 to facilitate a comparison.

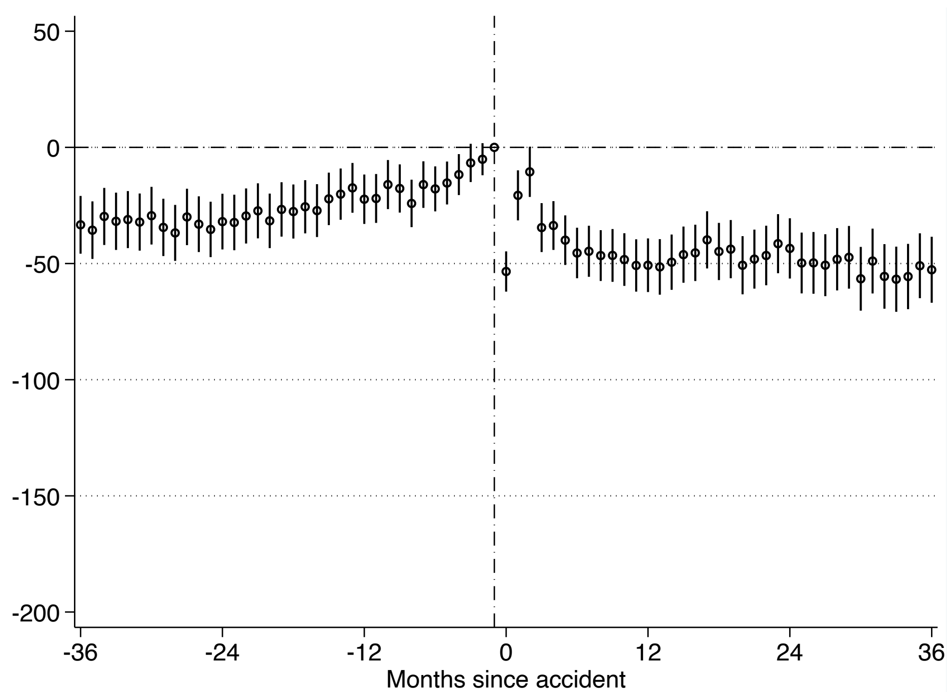
Turning to the effect leads (coefficients δ_{-36} to δ_{-2}) in Figure C1, we find that almost all of them are negative and statistically significantly different from zero. That is, the parallel trends assumption is clearly violated in this case. Even after re-weighting the control group by the IPSW and trimming the sample, the treatment and control groups appear to be different along some unobserved dimension, which makes it impossible to interpret these findings as causal effects of accidents. Due to the nature of our data, we can only control for a small set of observed characteristics, and it is possible that the (conditional) parallel trends assumption would be met if we had a richer set of covariates. We chose not to include pre-accident employment outcomes in the propensity score because of the concern that this could bias our estimates through regression to the mean (Daw and Hatfield, 2018). Given the weakness of the research design involving a control group without an accident, we strongly prefer our main findings. This implies that the negative labor market effects of accidents on Chilean men are substantially more severe than what the results in Figure C1 show.

Figure C1: Estimated Treatment Effects Using a No-Accident Control Group

(a) Employment



(b) Monthly Earnings (in U.S. dollars)



Note: The graphs plot estimated dynamic treatment effects $\hat{\delta}_k$ from regression (6) along with their 95% confidence intervals.