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 in the Chilean labor market using event study methods and monthly data. An accident reduces the probability of being employed by 8.3 percentage points in the first year, by 11.1 percentage points in the second year, and by 14.5 percentage points in the third year after the accident. On average, employment declines by 14%, relative to the pre-accident mean. In addition, accidents reduce monthly earnings by around US$60 in the first year, US$94 in the second year, and US$125 in the third year after the accident. On average, monthly earnings fall by 16%, relative to the pre-accident average. Thus, we estimate a persistent, or even increasing, labor market effect from accidents over time. 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

A strand of the literature provides evidence on the positive effects of health on earnings and labor market attachment, but no consensus exists regarding the magnitude of these effects (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. We find that individuals exposed to accidents reduce their employment rate by 14% and experience a 16% decline in monthly earnings, relative to pre-accident averages. The declines in employment and earnings are persistent and growing over time.

To estimate the causal effect of health shocks, we exploit the timing of accidents. Specifically, our sample includes only individuals who had an accident and excludes a "traditional" control group consisting of individuals without a health shock. Instead, individuals who had an accident later during the study period serve as a control group for those who had an accident earlier (Fadlon and Nielsen, 2017; Dobkin et al., 2018).

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 (DiD) 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 bias due to the negative weighting inherent in standard DiD.

Our empirical design addresses several of the methodological issues that are 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, Garcia Gomez et al. (2013), Lundborg, Nilsson, and Vikström (2015), and Dobkin et al. (2018) use acute hospitalizations in the Netherlands, 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

[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).

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 for 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. 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.

Our paper therefore contributes to the literature that estimates the causal effect of health on labor market outcomes by combining a highly plausible source of exogenous variation (timing of hospitalizations due to accidents) with high-frequency administrative data. In contrast to existing studies, which consider labor market outcomes at the annual or quarterly level, we use monthly labor market data. This allows us to estimate dynamic effects immediately after the health shock and several years thereafter. Finally, our study presents the first piece of 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 (forthcoming) use survey data to estimate the effect

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

of hospitalizations on labor market outcomes among older workers in China in addition to several developed countries.

We combine administrative data from two sources. Specifically, we merge monthly employment data of 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 over 13,000 men who were hospitalized due to an accident during our study period. By focusing on health shocks stemming from events such as accidental exposure, slipping, tripping, stumbling, falls, exposure to inanimate mechanical forces, assault, 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 8.3 percentage points in the first year, by 11.1 percentage points in the second year, and by 14.5 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$125 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 on treatment effect heterogeneity across the observable characteristics of individuals. 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. However, we do not find significant differences in the earnings effect of accidents across education groups. 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 the individual's number of days hospitalized, lead to a larger decline in both employment and earnings. We discuss the possible explanations for these findings in Section 6.

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. 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 each worker and his/her employer, for workers with open-ended contracts, and by the employer 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, self-employed or own-account workers, and public sector employees are excluded from unemployment insurance. Workers covered by unemployment insurance 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.[3] 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.[4]

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. 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

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

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

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.[5]

3 Data and Summary Statistics

We combine administrative data on monthly earnings and hospital stays for Chilean men from two sources.[6] 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 USD.

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 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").[7] We also observe the cause of each accident using ICD-10 codes starting with V, W, X, or Y ("External causes of morbidity").[8]

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 with RUT numbers and sex.[9] Our baseline sample consists of dependent workers with strong ties to the labor market,

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

[6]We do not include women in the analysis because of their lower rate of labor market attachment and higher rate of informality (Central Bank of Chile, 2018).

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

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

[9]The data sets were merged using a secure server from 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.

which increases the likelihood that a post-accident change in employment recorded in the UIS reflects an actual changes in the employment status of workers, and not movement to self-employment or the informal sector.

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 13,113 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, 85% have at most a high school degree, at most, and almost 9% have some level of postsecondary education. In addition, 94% of the individuals who report a health 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 25% in the tertiary sector (wholesale, retail, restaurant, finance, real estate, education, and health).[10] Finally, Table 1 shows that 6% of the individuals in our sample exhibit hospital stays lasting longer than two weeks.

We next characterize accidents in terms of their diagnoses and causes. We observe, in Table 2, that the most common diagnoses are injuries to the head (20%); knee and lower leg (17%); wrist, hand, and fingers (15%); and those involving multiple body regions (8%). In addition, Table 3 shows that one of the most common causes of accidents in our sample are slipping, tripping, stumbling and falls (20%); exposure to inanimate mechanical forces (14%); and assault (8%). These causes 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.

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

4 Empirical Strategy

We use an event study approach to quantify the effects of accidents on labor market outcomes. In partiacular, 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 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. Ideally, the omitted categories should be far apart. Here we select $k = -1$ and $k = -36$ as the omitted categories, i.e., we set $\gamma_{-36} = \gamma_{-1} = 0$. 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. 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 the post-accident dynamic treatment effects only, 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.[11]

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. Instead, conditional on having an accident between 2004 and 2007, individuals who had an accident later during that time period serve as a control group for those who had an accident earlier during that same time period. This research design is similar to the ones used by Fadlon and Nielsen (2017) and Dobkin et al. (2018).

To summarize the effect of an accident on employment, we first estimate the "canonical" DiD 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.[12] 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 in regression (1) with the weights decreasing with relative time and possibly becoming negative for large k. That is, the canonical DiD 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 ω^k can be obtained as the coefficient ω_k in the regression

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

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

[12]In contrast to the most commonly used DiD 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.

where the variable D_{it} is the post-accident indicator included in model (2). In our data, we estimate $\hat{\omega}_0 = 0.1504$, $\hat{\omega}_{12} = 0.0341$, $\hat{\omega}_{24} = -0.0066$, and $\hat{\omega}_{36} = -0.0291$, which shows that regression (2) puts disproportionate weight on the month when the accident occurred and negative weight on effects two or more years after the health shock.[13] 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 $\bar{\hat{\gamma}} = \frac{1}{37}\sum_{k=0}^{36} \hat{\gamma}_k$ in practice. This aggregate treatment effect does not suffer from the negative weighting inherent in the canonical DiD 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 treatment. 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

[13] 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, which is rare.

initial hypothesis that the type of health shocks considered in this paper 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 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. Therefore, the results shown in Figure 1 make us comfortable with 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, as discussed in Section 4. For comparison purposes, we also estimate the treatment effect derived from the canonical DiD regression (2).

Column (1) of Table 4 shows the canonical DiD treatment effect for employment. According to this estimate, employment declines by 5.9 percentage points or 7.6% following an accident. In contrast, the average of the dynamic treatment effects in column (2) of Table 4 indicates a reduction in employment by 11.1 percentage points or 14.2%. 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 DiD 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 4 are our preferred estimates for the average dynamic effect on employment. These and all other results in this paper are statistically significant at least at the 1% level.

In addition, we observe a persistent decline in employment over time in column (2) of Table 4. Employment falls by 7.6 percentage points or 10% in the sixth month after the accident, and by 16.3 percentage points or 21% at the end of the third year. On average, accidents reduce the probability of being employed by 8.3 percentage points (11%) in the first year of the post-accident period, by 11.1 percentage points in the second year (14%), and by 14.5 percentage points (19%) in the third year.

Next, we show the estimated effect of accidents on earnings in columns (3) and (4) of Table 4. We observe again that the canonical DiD model computes an average treatment effect that 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$92, which is equivalent to a 16% drop relative to the pre-accident average. In addition, the point estimates in column (4) of Table 4 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 11% in the sixth month after the accidents and by US$143 or 25% at the end of the third year. On average, the monthly earnings decline equals US$60 (11%) during the first year, US$94 (17%) during the second year, and US$125 (22%) during the third year after the accident.

A comparison between our findings and existing estimates in the literature is not straightforward since 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 they use and/or the type of health shock they consider. 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 despite the different setting. 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.

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 DiD estimates for comparison purposes.

We first study heterogeneity by age. We consider the impact of accidents for two age groups: the first group includes individuals who are 22–49 years old at the time of accident, and the second group includes older individuals who are 50-61 years old. In panel (a) of Table 5, we observe a larger decline in employment and earnings for older individuals. The probability of employment falls by 10.5 percentage points in the younger group, which represents a 13% decline relative to the pre-accident average. Instead, individuals in the older group exhibit a decline in the probability of employment by 17.3 percentage points or 22% of the pre-accident average.

In addition, column (5) of Table 5 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$92 in the younger group, whereas earnings fall by US$109 in the older group. Relative to the pre-accident mean, these effects represent a decline in monthly earnings by 16% and 20% for the younger group and the older group, respectively.

We now assess heterogeneity across education groups. To do so, we consider two education categories: the first category includes individuals with a high school degree or less education, and the second category contains those with at least some postsecondary education. We observe, in panel (b) of Table 5, that more educated individuals experience a slightly smaller decline in the probability of being employed after the accident. In addition, column (5) of Table 5 shows that the level of earnings loss after an accident is larger for more educated individuals than for those with less education (US$195 and US$74, respectively). However, in parallel, column (6) of Table 5 also shows that more educated individuals exhibit higher pre-accident earnings than less educated individuals. Both elements imply that, relative to the pre-accident mean, the decline in earnings in the two education groups is roughly the same, around 15%.[14]

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 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 5 shows that workers attached to the primary/secondary sectors exhibit a larger fall in employment, compared to those participating in the tertiary sector (12.4 versus 11 percentage points or 14.6% versus 11.5%). We also observe in column (5) of Table 5, that individuals working in the primary/secondary sectors experience a 19% 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.

We conclude the analysis of this section by assessing the relation between accident severity

[14]Interestingly, we observe that the canonical DiD 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 DiD (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.

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 10.1 percentage point (13%) drop in employment rates and a 16% earnings decline, whereas individuals with longer stays experience a reduction in employment by 13 percentage points or 17% and a reduction in earnings by 28% (see panel (d) of Table 5). 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.

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.[15] Table 6 presents the results.

In our main sample, the average number of accidents is 1.28 with a standard deviation of 0.75. 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 13%, 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

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

period; and specification R5 does not place any sample restrictions. The results in Table 6 reveal a decline in employment between 7% (specification R5) and 15% (specification R3) and a decline in monthly earnings between 11% (specification R5) and 15% (specification R3), relative to the pre-accident average. Overall, selecting a sample with less labor market attachment before the accident reduces the effect of accidents, but our main conclusion stays.

6 Discussion

We now discuss potential explanations for our results. 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. For instance, 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. Hence, in this economic environment, the employment possibilities of an injured worker would be severely undermined when the health event disables specific skills used in the industry in which the worker is participating. Furthermore, the consequences of an accident could be exacerbated even more in the absence of effective medical care provided through an efficient health care system.

Then, we hypothesize that the combination of two factors explains the persistent and negative labor market effects of accidents: first, the type of health shocks we consider, and second, the idiosyncratic characteristics of the labor market and health insurance system in Chile. As discussed in Section 3, the health shocks that we study are accidents that mainly injure parts of the body used to perform mechanical tasks (see Table 2). Moreover, a substantial fraction of the Chilean workforce is employed in jobs that require mostly manual tasks. For instance, among non-missing records, Table 1 shows that almost 70% of the individuals in our sample are employed in the primary or secondary sectors. The economic framework that we have sketched above suggests that the effect of disabling health accidents is more pronounced in this setting than in an environment where most workers have desk jobs.

Furthermore, as shown in Table 1, 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 the workers in our sample indeed lack effective health care that would facilitate a quick return to the labor force after an accident. Hence, a combination of the type of health shocks that we consider, the composition of the Chilean workforce, and a public insurance system that does not operate at the highest level of efficiency rationalizes our findings.

Our analysis of treatment effect heterogeneity shows suggestive evidence on the effects of accidents across different groups of individuals. The analysis of heterogeneity by age groups suggest that the largest effect is observed for senior individuals. A possible explanation for this result is 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, in comparison to younger workers.

In addition, we find that, after an accident, more educated individuals experience a slightly smaller decline in the probability of being employed than less educated workers. This result is in line with the findings described in the existing literature. For example, Heinesen and Kolodziejczyk (2013) and Jeon and Pohl (2019) estimate an educational gradient in the employment effects of cancer diagnoses. Our evidence suggests that human capital could have a (small) mitigating effect on the employment consequences of accidents. In the case for earnings, we do not find evidence of a differentiated effect across education groups. Our results thus 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).

Finally, we also show that the impact of accidents on both employment and earnings is less pronounced for workers attached to the tertiary sector; that is, the wholesale, retail, restaurant, finance and real estate, education, and health sectors. These sectors rely less heavily on manual labor than, for instance, manufacturing and construction. This last set of findings is also consistent with the model sketched above, where health shocks that disable specific skills produce larger effects for workers that rely on injured skills more intensively.

7 Conclusion

In this paper, we use an event study approach and monthly adminstrative 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 8.3 percentage points in the first year, by 11.1 percentage points in the second year, and by 14.5 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$94 in the second year, and by US$125 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.

Our findings contribute to further assessing the magnitude of health effects on labor market activity. We show that economic consequences of health shocks could go far beyond explicit medical expenses. A further investigation of the factors that could exacerbate or mitigate the labor market consequences of health shocks constitutes an important avenue for future research.

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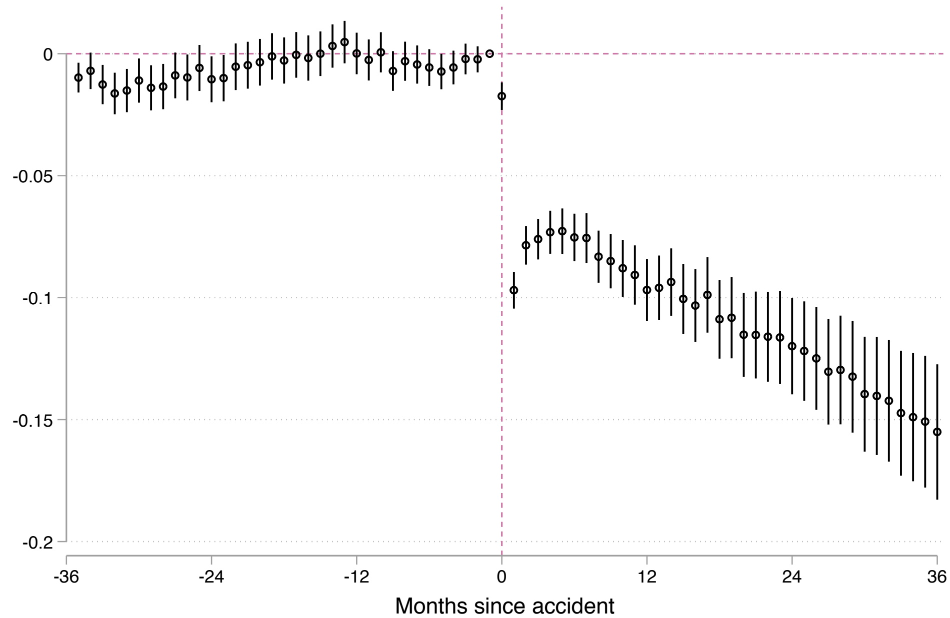
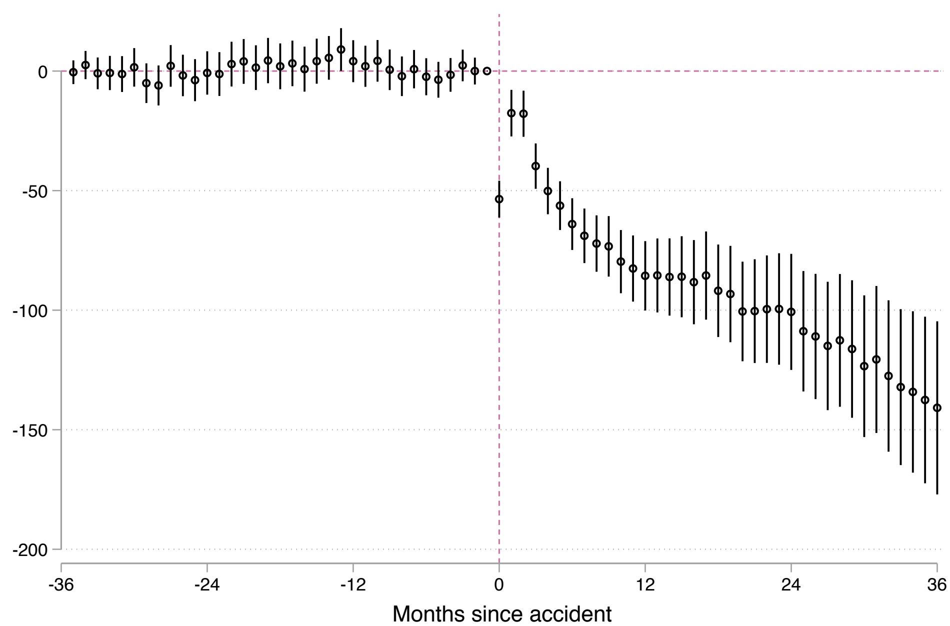


(a) Employment



(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.

Figure 1: Estimated Treatment Effects from the Fully Dynamic Model

Table 1: Sample Characteristics

Labor market outcome	Mean (Std.dev.)
Pre-accident employment	0.780 (0.414)
Pre-accident monthly earnings	567.49 (653.40)
Age at time of accident	**Dist. (%)**
22–49	90.31
$\geq$ 50	9.69
Education	**Dist. (%)**
High school degree or lower	85.66
Post-secondary degree	8.49
Missing	5.86
Insurance	**Dist. (%)**
FONASA	52.26
ISAPRE	3.12
Missing	44.61
Industry	**Dist. (%)**
Agriculture and fishing	11.51
Mining	1.28
Manufacturing	8.90
Construction and transportation	32.87
Wholesale, retail, and restaurant	9.27
Finance and real estate	9.95
Education and health	5.36
Missing	20.86
Length of hospital stay	**Dist. (%)**
$\leq$ 7 days	84.11
8–14 days	10.05
$\geq$ 15 days	5.84

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 is 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: Distribution of Accidents by Diagnosis

	Dist. (%)
Injuries to the head	20.27
Injuries to the knee and lower leg	16.62
Injuries to the wrist, hand and fingers	15.03
Injuries involving multiple body regions	7.83
Injuries to the abdomen, lower back, lumbar spine, pelvis and external genitals	6.07
Injuries to the elbow and forearm	5.89
Injuries to the ankle and foot	4.6
Injuries to the thorax	4.52
Injuries to the shoulder and upper arm	4.47
Injury of unspecified body region	3.92
Burns and corrosion of external body surface, specified by site	2.5
Injuries to the hip and thigh	2.42
Toxic effects of substances chiefly nonmedicinal substances as the source	1.86
Poisoning by, adverse effect of, and underdosing of, drugs medicaments and biological substances	1.23
Injuries to the neck	1.1
Other and unspecified effects from external causes	0.6
Effects of foreign body entering through natural orifice	0.55
Complications from surgical and medical care, not elsewhere classified	0.44
Certain early complications from trauma	0.09

Table 3: Distribution of Accidents by Cause

	Dist. (%)
Accidental exposure to other/unspecified factors	26.79
Slipping, tripping, stumbling and falls	20.16
Exposure to inanimate mechanical forces	14.14
Event of undetermined intent	11.21
Assault	7.56
Other land transport accidents	3.82
Car occupant injured in transport accident	2.15
Pedal cycle rider injured in transport accident	1.88
Pedestrian injured in transport accident	1.72
Overexertion, travel and privation	1.71
Exposure to animate mechanical forces	1.23
Intentional self-harm	1.04
Contact with heat and hot substances	0.85
Exposure to smoke, fire and flames	0.73
Motorcycle rider injured in transport accident	0.69
Accidental poisoning by and exposure to noxious substances	0.63
Other and unspecified transport accidents	0.63
Contact with venomous animals and plants	0.58
Complications from medical and surgical care	0.53
Exposure to electric current, radiation and extreme ambient air temperature and pressure	0.42
Bus occupant injured in transport accident	0.38
Occupant of pick-up truck or van injured in transport accident	0.34
Occupant of heavy transport vehicle injured in transport accident	0.27
Sequelae of external causes of morbidity and mortality	0.26
Exposure to forces of nature	0.11
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 4: Estimated Treatment Effects of Accidents on Employment and Monthly Earnings

	Employment		Earnings	
	Canonical (1)	Dynamic (2)	Canonical (3)	Dynamic (4)
Avg. post-accident	–0.0589***	–0.1106***	–38.0738***	–92.0538***
	(0.0034)	(0.0078)	(3.9576)	(9.7212)
6 months: $\hat{\gamma}_6$		–0.0757***		–64.8987***
		(0.0051)		(5.7721)
12 months: $\hat{\gamma}_{12}$		–0.0987***		–86.6331***
		(0.0067)		(7.8368)
24 months: $\hat{\gamma}_{24}$		–0.1249***		–102.1617***
		(0.0103)		(12.9621)
36 months: $\hat{\gamma}_{36}$		–0.1632***		–142.7109***
		(0.0144)		(19.1335)
First year (avg.): $\bar{\hat{\gamma}}^1$		–0.0832***		–59.9291***
		(0.0045)		(5.1605)
Second year (avg.): $\bar{\hat{\gamma}}^2$		–0.1112***		–94.3498***
		(0.0082)		(10.1854)
Third year (avg.): $\bar{\hat{\gamma}}^3$		–0.1453***		–125.0067***
		(0.0122)		(15.7747)
Individual fixed effects	Yes	Yes	Yes	Yes
Year-month fixed effects	Yes	Yes	Yes	Yes
Observations	957,249	957,249	957,249	957,249

Notes: The dependent variables are an indicator of monthly employment and monthly earnings in U.S. dollars. See equations (1) and (3) for a definition of the estimated coefficients. Standard errors clustered at the individual level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: 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.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(a) Age at time of accident							
22–49	–0.0564***	–0.1048***	0.7793	–36.4042***	–91.8169***	571.12	864,466
	(0.0036)	(0.0081)		(4.1755)	(10.3121)		
≥ 50	–0.0823***	–0.1726***	0.7869	–54.5883***	–108.7395***	533.68	92,783
	(0.0120)	(0.0271)		(12.3525)	(28.2432)		
(b) Education							
High school or lower	–0.0613***	–0.1084***	0.7720	–42.4065***	–73.9766***	484.72	819,936
	(0.0038)	(0.0084)		(3.7540)	(8.8344)		
Post-secondary	–0.0403***	–0.0984***	0.8300	–17.9621***	–194.6005***	1282.82	81,249
	(0.0116)	(0.0274)		(25.5259)	(65.9607)		
(c) Industry							
Primary/secondary	–0.0824***	–0.1240***	0.8470	–64.1449***	–119.1856***	619.12	522,242
	(0.0043)	(0.0095)		(5.2227)	(12.6397)		
Tertiary sector	–0.0623***	–0.1097***	0.8763	–21.8481***	–116.2506***	712.82	235,279
	(0.0062)	(0.0141)		(8.8007)	(22.1410)		
(d) Length of hospital stay							
< 15 days	–0.0546***	–0.1066***	0.7818	–36.1316***	–90.0640***	573.90	901,331
	(0.0035)	(0.0079)		(4.0840)	(10.0670)		
≥ 15 days	–0.1303***	–0.1829***	0.7512	–70.6491***	–131.0667***	464.27	55,918
	(0.0178)	(0.0372)		(15.9817)	(35.5914)		

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. Standard errors clustered at the individual level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Main specification	–0.0589***	–0.1106***	0.7800	–38.0738***	–92.0538***	567.49	957,249
	(0.0034)	(0.0078)		(3.9576)	(9.7212)		
Specification R1	–0.0513***	–0.0947***	0.7589	–33.2787***	–77.5778***	616.99	773,143
	(0.0037)	(0.0085)		(4.3335)	(10.9392)		
Specification R2	–0.0638***	–0.1050***	0.8743	–33.9084***	–96.8015***	798.69	513,701
	(0.0038)	(0.0087)		(5.9377)	(15.1094)		
Specification R3	–0.0666***	–0.1236***	0.7995	–41.6217***	–100.4681***	661.89	829,572
	(0.0035)	(0.0078)		(4.3704)	(10.7261)		
Specification R4	–0.0369***	–0.0673***	0.6710	–25.1807***	–62.0279***	523.67	1,175,008
	(0.0032)	(0.0074)		(3.3792)	(8.3903)		
Specification R5	–0.0272***	–0.0453***	0.6244	–19.9874***	–48.9536***	440.97	1,277,646
	(0.0030)	(0.0071)		(3.1467)	(7.7932)		

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. 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.