The Effect of Accidents on Employment: Evidence from Chile*

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July 2019

Abstract

Exploiting variation in the timing of accidents, we estimate the causal effect of this type of health shock on formal employment among Chilean men. We use administrative data on monthly employment and hospital discharges over a period spanning almost a decade. We find a large and persistent negative effect of accidents on employment. On average, employment in the formal sector declines by about 16 percentage points in the three years following an accident. The initial effect amounts to about −12 percentage points and it steadily increases in absolute value to a level of −20 to −25 percentage points in the third post-accident year. We also find some suggestive evidence that education and health care quality are factors that help mitigate the employment consequences of health shocks.

Keywords: Accidents; Health Shocks; Employment; Education; Health Insurance.
JEL codes: I10; I13; I15; J22.

*We are grateful to Daniel Avdic, Prashant Bharadwaj, Lukas Kauer and Josh Kinsler for providing valuable feedback, as well as to seminar and conference participants at the AEA, ASHEcon, ATINER, CEA, CHESG, University of Duisburg-Essen, EALE, ECHE, Essen Health Conference, iHEA, University of Maryland Baltimore County, McGill, McMaster, LMU Munich, University of Ottawa, SOLE and University of Toronto for their helpful comments. Pohl gratefully acknowledges financial support from the W.E. Upjohn Institute for Employment Research and the Queen's University Principal's Development Fund. We would also like to thank Loreto Reyes and Cristian Valencia for providing excellent research assistance. All remaining errors are our own.

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1 Introduction

Human capital is a fundamental determinant of labor market outcomes (for a review, see Card 1999). In addition to different forms of investments in education, individuals' health status can augment or reduce their stock of human capital, as highlighted by Becker (1964) and Grossman (1972). A strand of the literature provides evidence on positive effects of health on earnings and labor market attachment (for a review, see Currie and Madrian 1999). However, no consensus exists regarding the magnitude of these effects (Currie and Madrian 1999). In this paper, we use an event study approach to quantify the effects of accidents on employment among men in Chile. We find that employment in the formal sector, on average, declines by 16 percentage points following an accident. We also show evidence that suggests heterogeneous treatment effects across education, type of health insurance, and industry.

We combine administrative data from two sources. Specifically, we merge data on monthly employment 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 and hospitalizations for over 20,000 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 empirical strategy follows Fadlon and Nielsen (2017) and Dobkin et al. (2018) in exploiting the timing of health shocks. Specifically, our sample includes only men who had an accident between January 2004 and December 2007 but excludes a "traditional" control group consisting of individuals without a health shock. Instead, individuals who had an accident later during that time period serve as a control group for those who had an accident earlier. We also incorporate the methodological insights of Borusyak and Jaravel (2017) by estimating a semi-dynamic model that allows for time-varying 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 employment. 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] Another issue that arises when using survey data relates to the timing of changes in health status and labor market outcomes, that is, the difficulties involved in measuring which change occurred first. This problem persists even with panel data due to the existence of recall bias.

More recently, a growing number of studies have exploited exogenous variation coming 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 identification of causal effects is that these shocks are unexpected and, importantly, uncorrelated with any unobserved determinants of labor market outcomes.

In contrast to these studies, our health shock measure only includes hospitalization spells that are more likely to be exogenous, in particular, those due to accidents. This is in contrast to hospitalizations that may be more predictable, such as those due to cardiovascular conditions or cancer. Halla and Zweimüller (2013) use a similar strategy by focusing on commuting accidents in Austria as a source of identifying variation.[2] Accidents are unpredictable and exogenous to labor market outcomes.

In addition to using unpredictable health shocks, our sample only includes individuals who were hospitalized due to an accident during the study period. Halla and Zweimüller (2013), for example, compare individuals with and without accidents who may differ along some unobserved dimensions. In contrast, our identification strategy uses variation from the timing of hospitalizations as in Dobkin et al. (2018). More 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 the employment outcome. Therefore, in combination with using hospitalizations due only to external causes, we obtain a source of variation that is as close to random as feasible in an observational study.

The present study 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

[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).

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

data. In contrast to existing studies, which consider labor market outcomes at the annual or quarterly level, we use monthly employment data. This allows us to estimate dynamic effects immediately after the health shock and several years thereafter. Finally, the present study presents the first evidence in this area from an emerging economy, while the existing literature uses data from Europe and North America. Chile provides an interesting setting for studying the effect of health on labor market outcomes as a substantial fraction of the workforce is employed in jobs that require mostly manual tasks. Hence, the effect of disabling health events is likely more pronounced than in a setting where most workers have desk jobs.

We estimate a negative and statistically significant average treatment effect. Specifically, we find that employment in the formal sector declines by 16 percentage points following an accident. In addition, our dynamic treatment effect estimates show that, relative to the pre-accident period, formal employment falls by about 3 percentage points in the month of the accident, by about 12 percentage points in the following month, and by about 10 percentage points after two months. From there, we find that employment rates continuously decline, reaching a level 20 to 25 percentage points lower than before the health shock in the third year after the accident. Our main result is not sensitive to the sample restrictions we impose on our analysis.

We also provide evidence on heterogeneity in the treatment effects across individuals' observable characteristics. We show that, in our preferred specification, individuals with a higher educational attainment and with private health insurance (which provides access to higher-quality health care) exhibit a significantly smaller employment impact from accidents during the first months immediately after the health event. Finally, we find that the impact of health shocks is smaller in industries that rely less heavily on manual labor.

Overall, this paper uses an event study approach to provide causal evidence on the effect of accidents on employment in an emerging economy. Our evidence contributes to further assessing the magnitude of health effects on labor market activity and also suggests some mitigating factors for these effects. Following Currie and Madrian (1999), empirical evidence as that provided in this paper allows policymakers to better assess the cost effectiveness of interventions designed to prevent or cure diseases.

The remainder of this paper is organized as follows. Section 2 describes our data and Section 3 contains the empirical strategy. Section 4 presents and discusses our results while Section 5 concludes.

2 Data

In this section, we describe the data set used in the empirical analysis. We also discuss how we build the baseline sample to estimate the effects of accidents on formal employment. At the end of this section, we present summary statistics.

2.1 Data Sources

We combine administrative data on monthly employment and hospital stays from two sources. The employment data come from the Chilean unemployment insurance system, *Seguro de Cesantia* (SC). The Chilean government enacted the SC as an addition to the existing social protection safety net in 2002. Participation in the SC is mandatory for all workers who began a new employment relationship after October 2002. Monthly contributions amount to three percent of the employee's monthly salary. Thus, firms have to report their employees' salaries to the SC administration on a monthly basis. Our data consist of monthly observations of employment status (defined by strictly positive earnings) and the employer's industry. In addition, the SC records employees' educational attainment, sex, year and birth month, and the date they became affiliated with the SC. Our data set includes the universe of SC records from October 2002 to December 2011. There are about 4.2 million men in this data set.[3]

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 ICD-10 diagnosis code, the patient's health insurance provider (FONASA, ISAPRE or other), and the exact dates of admission and discharge.[4] The Ministry of Health of Chile collects these records from all hospitals in the country. We classify a hospital stay by major type of diagnosis according to the first letter of the ICD-10 code and retain hospital stays related to a diagnosis code that starts with S or T ("Injury, poisoning and certain other consequences of external causes").[5] The most common diagnoses are injuries to the head (21%); knee and lower leg (15%); wrist, hand, and fingers (14%); and involving multiple body regions (8%). We also observe the cause of each accident, using ICD-10 codes starting with V, W, X, or Y ("External causes of morbidity").[6] The most common causes are accidental exposure to other specified factors (30%); slipping, tripping, stumbling and falls (19%); exposure to inanimate mechanical forces

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

[4]FONASA is the public health insurance plan run by the Ministry of Health, whereas ISAPREs form the private insurance system. In Appendix Section A, we provide background information on the Chilean health care system.

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

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

(13%); and assault (8%). These accident causes highlight the fact that the health shocks considered in this paper are truly unanticipated events leading to a sudden decline in health status.

2.2 Sample Construction

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 individuals' monthly employment records to hospital records with RUT and sex.[7] We restrict the sample to men born between 1950 and 1980 who had an accident between January 2004 and December 2007, leading to an initial sample size of $N_1 = 77{,}112$. We exclude men who became affiliated with the SC after December 2003 to ensure that we can observe a sufficiently long employment history before the health shock ($N_2 = 42{,}879$). In addition, we drop men who were employed fewer than 18 months total before their accident to eliminate individuals with weak ties to the formal labor market, leading to our final sample size of $N_3 = 21{,}432$. To investigate the sensitivity of our results to these sample restrictions, we carry out robustness checks based on the full sample of 77,112 men in Section 4.3.

We follow these individuals for up to 36 months before and after their health shock. We observe the entire sample for 36 months after the accident. Before the health shock, we observe 61% of the sample for the full 36 months, 90% for at least 24 months, and the entire sample for at least 18 months by construction.

2.3 Summary Statistics

We display summary statistics in Table 1. Individuals (men) in our sample are, on average, about 38 years old at the time of the accident. Among them, 59% have no high school degree, 32% have completed high school, and 9% have at least some post-secondary education. Almost half the sample has public FONASA health insurance coverage, 5% are enrolled in a private ISAPRE plan, while the remaining individuals have alternative health insurance coverage. Since the hospital claims data do not specify the exact type of health insurance in these cases, we drop these individuals when considering heterogeneous treatment effects by insurance coverage; however, we include them in the main results. To summarize the sample distribution across industries, we consider the most common (modal) industry in which each individual is employed before the accident. Construction and transportation

[7] The data sets were merged on 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.

account for 41% of the sample while 50% are employed in agriculture/fishing, manufacturing, wholesale/retail/restaurant, or finance/real estate industries. Finally, we consider monthly employment rates, our outcome of interest. Before the accident, about 81% of men were employed in the formal sector each month whereas the employment rate drops to 76% after the health shock. This decline in employment is suggestive for the negative effect of accidents on employment, but the raw difference does not account for any observed or unobserved confounding factors.

3 Empirical Strategy

We estimate the causal effect of a health shock on employment in the formal sector using the sample of men who had an accident between January 2004 and December 2007. Specifically, we estimate the following regression:

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

where Y_{it} is an indicator that equals one if individual i is employed in the formal sector in year-month t and zero otherwise. That is, $Y_{it} = 0$ includes non-employment and employment in the informal sector. The variable K_{it} denotes the relative time that has 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. We also include 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. In this empirical setting, γ^k represents the causal effect of an accident on employment k months after the health shock. We cluster standard errors on the individual level.

Borusyak and Jaravel (2017) call specification (1) semi-dynamic in contrast to the fully dynamic specification that also includes $\sum_{k=-36}^{-1} \gamma^k \mathbf{1}\{K_{it} = k\}$, that is, the relative time prior to the accident. The absence of pre-treatment terms $\mathbf{1}\{K_{it} = k\}$ for $k < 0$ in the semi-dynamic specification relies on the assumption that the timing of treatment is unpredictable. 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 present our results from specification (1) by plotting the $\hat{\gamma}^k$ against time passed since the accident. We also provide robustness checks that include the pre-treatment terms, $\sum_{k=-36}^{-1} \gamma^k \mathbf{1}\{K_{it} = k\}$, in Section 4.3.

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 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

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

where $D_{it} = \mathbf{1}\{K_{it} \geq 0\}$, i.e. D_{it} is a post-accident indicator. Although specification (2) is widely used in the applied literature, γ does not represent the true effect of an accident on employment unless the dynamic treatment effects γ^k in regression (1) are equal for all $k \geq 0$. As Borusyak and Jaravel (2017) show, γ can be expressed as a weighted average of the γ^k 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 k can be obtained as the coefficient ω^k in the regression

$$\mathbf{1}\{K_{it} = k\} = \mu_i + \nu_t + \omega^k D_{it} + e_{it},$$

where μ_i is an individual fixed effect and ν_t is a year-month fixed effect. The variable D_{it} is the post-accident indicator included in model (2). In our data, we estimate $\hat{\omega}^0 = 0.119$, $\hat{\omega}^{12} = 0.0492$, $\hat{\omega}^{24} = -0.00420$, and $\hat{\omega}^{36} = -0.0412$, which clearly shows that regression (2) puts disproportionate weight on the month when the accident occurred and negative weights on effects two or more years after the health shock.[8] 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 $\hat{\bar{\gamma}} = \frac{1}{37}\sum_{k=0}^{36} \hat{\gamma}^k$ in practice. This aggregate

[8]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 does not become a problem unless data from dozens of years are available, which is rare.

treatment effect does not suffer from the negative weighting inherent in the canonical DiD estimator and is easily interpretable as the average employment effect of an accident during the first three years following the health shock. For comparison purposes, we report both $\hat{\gamma}$ estimated from regression (2) and $\hat{\hat{\gamma}}$ along with their clustered standard errors (obtained using the Delta method in the case of $\hat{\hat{\gamma}}$).

In the last part of our analysis, we investigate treatment effect heterogeneity. To do so, we estimate regressions (1) and (2) by education, type of health insurance, and industry groups. Specifically, we divide the sample into men who do and do not have a high school degree and those with FONASA and ISAPRE coverage. We determine industry by considering the individual's modal industry in pre-accident months and we aggregate industries into three categories: agriculture/fishing and mining; manufacturing and construction/transportation; and wholesale/retail/restaurants, finance/real estate, and education/health.

4 Results

In this section, we present and discuss our results. First, we estimate the dynamic and the average treatment effects using the full sample described in Section 2.2 Then, we assess the heterogeneity of the treatment effect across education, health insurance, and industry groups. Lastly, we perform robustness checks for our main results.

4.1 Results for the Full Sample

Figure 1 shows the estimated dynamic treatment effects $\hat{\gamma}^k$ from the semi-dynamic specification in regression (1).[9] Relative to the pre-accident period, we find a decline in formal employment by about 3 percentage points in the month of the accident and by about 12 percentage points in the following month. The small decline in the accident month is explained by the fact that most individuals who had an accident were employed during some portion of the month in which the accident occurred. After two months, the employment effect of an accident first increases slightly to about 10 percentage points and then starts to continuously decline for the remaining months of the three-year follow-up period. In the third year after the accident, employment is 20 to 25 percentage points lower than before the health shock. All dynamic treatment effects are highly statistically significant. Hence, we estimate a large and persistent negative effect of accidents on employment in the formal sector.

Next, we summarize the dynamic treatment effects shown in Figure 1 in a single average

[9]The graphs in this paper were generated using the scheme developed by Bischof (2017).

treatment effect. Panel (A) of Table 2 shows the average treatment effect estimated from the canonical DiD regression (2) in the first column. According to this estimate, employment in the formal sector declines by 7.5 percentage points following an accident. In contrast, the average of the dynamic treatment effects shows a reduction in employment by 15.7 percentage points. The large difference between these two estimates can be reconciled by the dynamic treatment effects in 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 the semi-dynamic specification represents the true model, the effects exhibited in column (2) are our preferred estimates for the average dynamic effect. Thus, we conclude that an accident leads approximately to a 16 percentage point reduction in employment in the formal sector, on average.

A comparison between our findings and existing estimates in the literature is not straightforward. The existing studies mostly rely on data from the U.S. or European countries, whereas ours uses information collected from an emerging economy. Garcia Gomez et al. (2013) find a decline of 6.5 percentage points in the employment rate among hospitalized Dutch men. Using U.S. data, Dobkin et al. (2018) find employment effects of hospitalizations that range between -10 and -13 percentage points. Therefore, our preferred estimate is somewhat larger in absolute value than the effects found for developed economies.

4.2 Treatment Effect Heterogeneity

We now investigate how the employment effect of accidents varies across individual characteristics. Specifically, we estimate the semi-dynamic specification (1) separately for (i) two educational categories, (ii) public and private health insurance, and (iii) three industry categories. Figure 2 shows the estimated dynamic treatment effects for individuals without a high school degree and for those who have completed high school or attained a higher degree. Note that most of the monthly estimates are not statistically different from each other. However, we observe a statistically different treatment effect across the education groups in the first three months after the accident. Specifically, we observe that the reduction in employment is smaller among more highly educated individuals during these months. This evidence suggests that human capital could have a mitigating effect on the labor market consequences of accidents, but this attenuating role of education only manifests for a short post-accident period.

Panel (B) of Table 2 summarizes the dynamic treatment effects exhibited in Figure 2. We confirm the slightly protective effect of education. Our preferred result, which averages the dynamic treatment effects, shows that employment declines by 16 percentage points among men with a high school degree and by 15.1 percentage points among more highly educated men (see columns 2 and 4); however, this difference is not statistically significant ($p = 0.157$). When considering the estimated average treatment effects from the canonical regressions in columns (1) and (3), we find estimates that are about half as large, but the difference between education categories is about the same in absolute terms and, in this case, it is statistically significant ($p = 0.0154$). Overall, our results are in line with the findings of 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. As we do not use exogenous variation in education here, we cannot interpret the educational gradient in a causal way. Nevertheless, our finding suggests a protective effect of education on the negative employment effect of a health shock.

Next, we consider differences by health insurance coverage. Figure 3 shows that the dynamic employment effects are smaller in absolute value among men with private health insurance (ISAPRE) than among those with public coverage (FONASA). This difference is most pronounced and statistically significant in the first few months after the accident, where we do not observe the initial larger drop in employment among ISAPRE enrollees. After about half a year, the differences become statistically insignificant, but the smaller employment decline for privately insured men persists.

Panel (C) of Table 2 shows the summary estimates by health insurance coverage. We find that employment declines by 14.9 and 12.9 percentage points among men with FONASA and ISAPRE coverage respectively, when averaging the dynamic treatment effects. This difference is not statistically significant ($p = 0.170$). The estimates from the canonical model are smaller in absolute value and their difference is statistically significant ($p = 0.0012$).[10] Hence, we find suggestive evidence that having access to privately provided higher-quality health care reduces the negative employment effect of a health shock, especially during the first months after the accident. Our result is in contrast to that in Dobkin et al. (2018) who find smaller employment effects among the publicly insured (Medicaid). The authors' explanation relies on the lower labor force attachment of Medicaid beneficiaries. However, our findings cannot be directly compared to those of Dobkin et al. (2018) due to the differences in the role of public insurance between Chile and the U.S. Again, we note that this gradient should not be interpreted in a causal way because of endogenous selection in public or private health insurance.

[10] We note that information on health insurance coverage is missing for many individuals or they have insurance other than FONASA or ISAPRE. The sample size for the ISAPRE subsample is relatively small with about 1,000 individuals and 70,000 monthly observations.

Lastly, we split the sample by the most common industry in which individuals were employed before the accident.[11] We aggregate industries into primary (agriculture, fishing, and mining), secondary (manufacturing, construction, and transportation), and tertiary (wholesale, retail, restaurant, finance, real estate, education, and health) industries. As the type of work, e.g., the degree of manual labor, varies across industries, the likelihood of remaining employed after an accident could be heterogeneous across industries. We plot the estimated dynamic treatment effects in Figure 4. Except for the first few months after the health shock, we do not find statistically significant differences between industries. In months 1, 2, and 3, individuals working in tertiary industries before the accident experience a significantly smaller decline in employment. For the remainder of the follow-up period, we find the largest decline in employment among men working in manufacturing, construction, and transportation, but the month-to-month differences compared to the other industry groups is not statistically significant.

We report the summary treatment effects in panel (D) of Table 2. For our preferred estimates (columns (2), (4), and (6)) we find an employment decline by 15.3, 16.4, and 15.0 percent for primary, secondary, and tertiary industries, respectively. These differences are not statistically different from each other ($p = 0.153$ for the difference between secondary and tertiary, and $p = 0.389$ for the difference between secondary and primary industries). As before, the canonical estimates are smaller in absolute value. The difference between the employment effects between the secondary and tertiary industries, and the secondary and primary industries are statistically significant ($p = 0.021$ and $p = 0.0048$, respectively). Hence, we find some evidence that the effect of an accident on subsequent employment is somewhat moderated in tertiary industries, where workers rely less on manual labor and are therefore less impacted by physical injuries.

Overall, we have shown suggestive evidence on the existence of heterogeneous effects of accidents on employment, especially during the months immediately after the health shock. We find that education, the type of health insurance, and the type of job could play a mitigating role on the employment effects of accidents during the first months after the accident. Specifically, individuals with greater human capital, with a private (higher-quality) health insurance, and those who work in service industries exhibit a smaller reduction in employment in the months immediately after the shock. This evidence is consistent with the educational gradient estimated in the employment effects for some specific diagnoses, and with a larger impact of accidents in jobs that rely more intensively on manual labor.

[11] By using the modal industry, we do not need to condition on employment in the month prior to the accident. As our sample is restricted to men with an employment history of at least 18 months, we observe industries for all sample members.

In addition, our evidence suggests that access to high-quality health care can attenuate the effects of health shocks on employment.

4.3 Robustness Checks

We conduct two robustness checks. First, Table 3 shows regression results that contain the pre-accident terms in the even columns, i.e., we make specification (1) fully dynamic by including the terms $\sum_{k=-36}^{-1} \gamma^k \mathbf{1}\{K_{it} = k\}$. Compared to our main result in Table 2, the average dynamic employment effect is slightly smaller in absolute value at -14.1 instead of -15.7 percentage points.[12] The heterogeneity results by education and industry are qualitatively similar to our main results with slightly smaller declines in employment throughout. However, the effects by health insurance coverage are reversed (see panel (C)). Individuals who have ISAPRE coverage experience a larger decline in employment, but the difference with those who have FONASA coverage is not statistically significant. Overall, the results do not change substantially when including the pre-accident terms, thereby providing further evidence that our identifying assumption of unexpected health shocks is plausible.

Second, we extend the sample to include individuals who became affiliated with the SC after December 2012 and who were employed at least 12 months before the accident, i.e., we impose weaker sample restrictions than in our main results. We thereby obtain a larger sample, but have fewer pre-accident time periods on average. Table 4 shows smaller effects from accidents on employment, with a decline of 4.5 percentage points in the canonical regression and 14 percentage points for the average dynamic treatment effect. These smaller effects are likely due to the fact that men in this sample are less attached to the labor force. When splitting the sample by education, we find a larger employment reduction for individuals without a high school degree in the canonical model but no difference in the average dynamic treatment effects (see panel (B)). In contrast to our main results, we also estimate a larger decline in employment among individuals with ISAPRE coverage in the dynamic treatment effect model but not in the canonical model (see panel (C)). Furthermore, we find the same pattern across industries as in our main results, with men in the secondary industry experiencing the largest decline in employment.

Overall, this robustness check suggests that our main finding is not very sensitive to the sample restrictions imposed in the baseline specification. However, our result regarding the mitigating effects of private insurance seems to be somewhat sensitive to sample restrictions.

[12] The results from the canonical specification are identical in Table 3 and Table 2, and are repeated for comparison purposes.

5 Conclusion

In this paper, we use an event study approach to quantify the effect of accidents on formal employment. Our dataset stems from Chilean administrative records on monthly employment and hospital discharges over a period spanning almost a decade. Using this data, we estimate dynamic and average treatment effects. We find that formal employment falls by about 3 percentage points in the month of the accident, relative to the pre-accident period. In the month following the accident, employment falls by about 12 percentage points. From there, we find that employment continuously declines, reaching a level of 20 to 25 percentage points lower than before the health shock in the third year after the accident. Based on these dynamic effects, we estimate an average treatment effect of 16 percentage points.

We also find evidence suggesting mitigating factors for the employment consequences of health shocks. Specifically, we show that more educated individuals and those with private (higher-quality) health insurance exhibit a significantly smaller employment impact from health shocks during the first months after the accident. However, the differentiated effect across groups is no longer significant during the months further removed from the date of the accident. We also show evidence suggesting that the impact of health shocks is smaller on employment in industries that rely less intensively on manual labor.

Our evidence contributes to further assessing the magnitude of health effects on labor market activity. It also suggests some alleviating factors for the employment consequences of health shocks. A further investigation of the channels through which the labor market impact of health shocks is mitigated or exacerbated constitutes an important avenue for future research.

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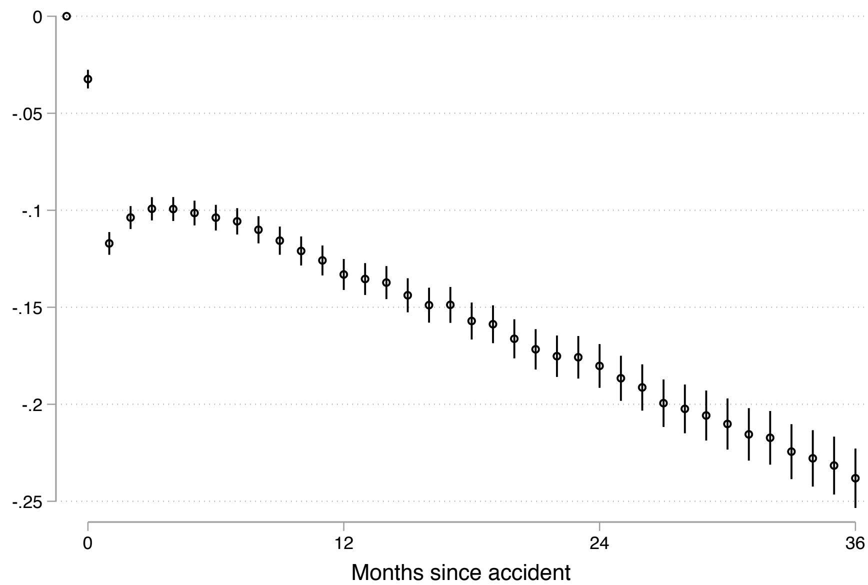
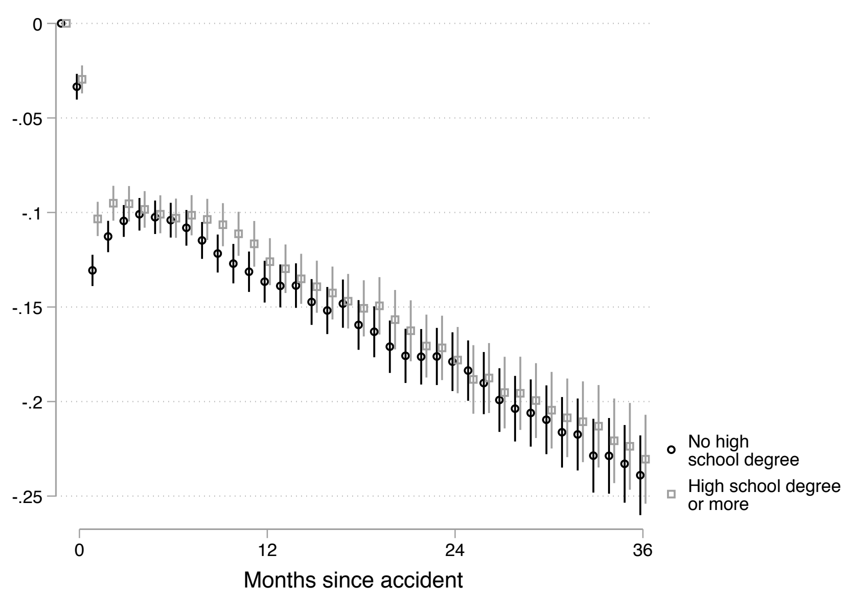


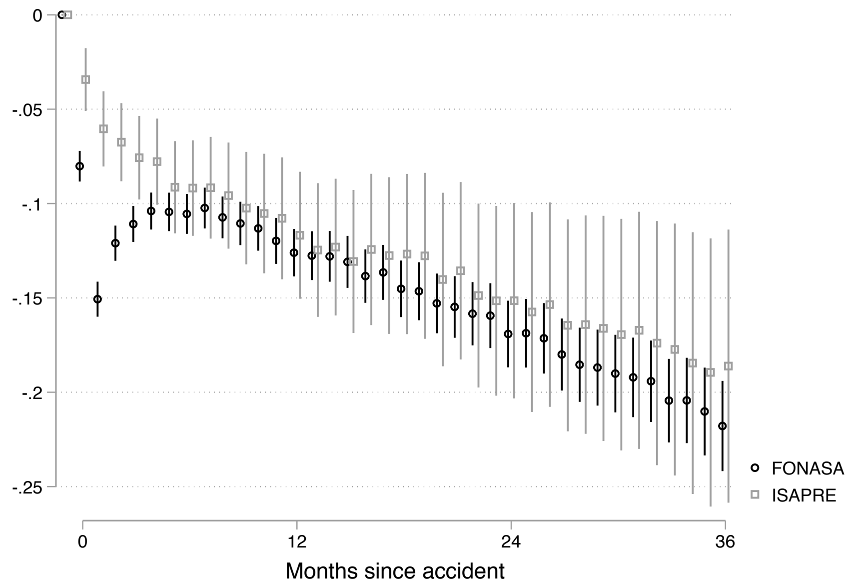
Note: The graph shows the estimated coefficients $\hat{\gamma}^k$ for $k = 0, \ldots, 36$ after the accident, from (1), for the full sample.

Figure 1: Employment Effects of Accidents, by Month After Accident



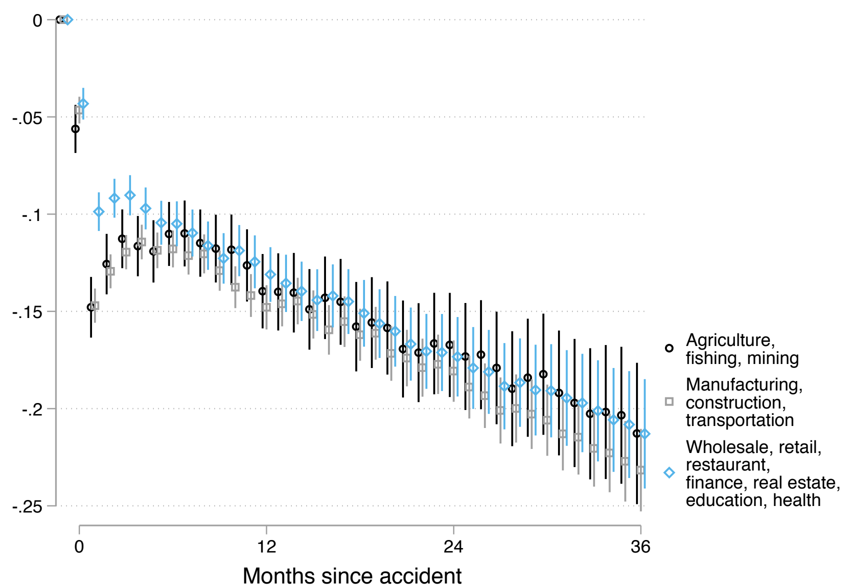
Note: The graph shows the estimated coefficients $\hat{\gamma}^k$ for $k = 0, \ldots, 36$ after the accident, from (1), by education.

Figure 2: Employment Effects of Accidents, by Month After Accident and by Education



Note: The graph shows the estimated coefficients $\hat{\gamma}^k$ for $k = 0, \ldots, 36$ after the accident, from regression (1), by health insurance coverage.

Figure 3: Employment Effects of Accidents, by Month After Accident and by Health Insurance Coverage



Note: The graph shows the estimated coefficients $\hat{\gamma}^k$ for $k = 0, \ldots, 36$ after the accident, from regression (1), by modal pre-accident industry.

Figure 4: Employment Effects of Accidents, by Month After Accident and by Industry

Table 1: Summary Statistics

	Mean	Std.dev.	Obs.
Age at accident	37.71	(7.653)	21,432
No high school degree	0.590	(0.492)	20,025
High school degree	0.316	(0.465)	20,025
Post-secondary education	0.0931	(0.291)	20,025
FONASA	0.468	(0.499)	21,268
ISAPRE	0.0493	(0.217)	21,268
Other health insurance	0.483	(0.500)	21,268
Agriculture, fishing	0.147	(0.354)	20,988
Mining	0.0144	(0.119)	20,988
Manufacturing	0.112	(0.315)	20,988
Construction, transportation	0.410	(0.492)	20,988
Wholesale, retail, restaurants	0.119	(0.323)	20,988
Finance, real estate	0.126	(0.332)	20,988
Education, health	0.0720	(0.258)	20,988
Employed before accident	0.813	(0.390)	699,613
Employed after accident	0.763	(0.425)	792,984

Notes: Age, education, and health insurance coverage at time of accident. Industry refers to modal pre-accident industry. Employment measured on the monthly level.

Table 2: Employment Effects of Accidents, Full Sample and Treatment Effect Heterogeneity

	(1) Canonical	(2) Avg. dyn.	(3) Canonical	(4) Avg. dyn.	(5) Canonical	(6) Avg. dyn.
	(A) Full Sample					
Post-accident	−0.0752***	−0.157***				
	(0.00236)	(0.00432)				
Observations	1,492,597	1,492,597				
	(B) By Education					
	No high school degree		High school degree or more			
Post-accident	−0.0807***	−0.160***	−0.0700***	−0.151***		
	(0.00327)	(0.00592)	(0.00372)	(0.00668)		
Observations	824,761	824,761	570,790	570,790		
	(C) By Health Insurance Coverage					
	FONASA		ISAPRE			
Post-accident	−0.0911***	−0.149***	−0.0605***	−0.129***		
	(0.00375)	(0.00678)	(0.00936)	(0.0198)		
Observations	700,597	700,597	70,782	70,782		
	(D) By Modal Pre-Accident Industry					
	Agriculture, fishing, mining		Manufacturing, construction, transportation		Wholesale, retail, restaurant, finance, real estate, education, health	
Post-accident	−0.0967***	−0.153***	−0.0970***	−0.164***	−0.0787***	−0.150***
	(0.00577)	(0.0101)	(0.00338)	(0.00595)	(0.00424)	(0.00779)
Observations	194,419	194,419	644,339	644,339	391,085	391,085

Notes: The table shows the average treatment effect of an accident on employment in the formal sector. Odd columns (labeled "Canonical") contain the estimate $\hat{\gamma}$ from regression (2) and even columns (labeled "Avg. dyn.") contain the estimate $\hat{\hat{\gamma}} = \frac{1}{37}\sum_{k=0}^{36}\hat{\gamma}^k$ with $\hat{\gamma}^k$s being the estimated dynamic treatment effects from regression (1). All regressions include individual and year-month fixed effects. Standard errors are clustered on the individual level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Employment Effects of Accidents, Full Sample and Treatment Effect Heterogeneity, Including Pre-Accident Time Effects

	(1) Canonical	(2) Avg. dyn.	(3) Canonical	(4) Avg. dyn.	(5) Canonical	(6) Avg. dyn.
	(A) Full Sample					
Post-accident	-0.0752^{***}	-0.141^{***}				
	(0.00236)	(0.00459)				
Observations	1,492,597	1,492,597				
	(B) By Education					
	No high school degree		High school degree or more			
Post-accident	-0.0807^{***}	-0.143^{***}	-0.0700^{***}	-0.137^{***}		
	(0.00327)	(0.00644)	(0.00372)	(0.00701)		
Observations	824,761	824,761	570,790	570,790		
	(C) By Health Insurance Coverage					
	FONASA		ISAPRE			
Post-accident	-0.0911^{***}	-0.0980^{***}	-0.0605^{***}	-0.112^{***}		
	(0.00375)	(0.00734)	(0.00936)	(0.0197)		
Observations	700,597	700,597	70,782	70,782		
	(D) By Modal Pre-Accident Industry					
	Agriculture, fishing, mining		Manufacturing, construction, transportation		Wholesale, retail, restaurant, finance, real estate, education, health	
Post-accident	-0.0967^{***}	-0.125^{***}	-0.0970^{***}	-0.144^{***}	-0.0787^{***}	-0.129^{***}
	(0.00577)	(0.0116)	(0.00338)	(0.00658)	(0.00424)	(0.00812)
Observations	194,419	194,419	644,339	644,339	391,085	391,085

Notes: The table shows the average treatment effect of an accident on employment in the formal sector. Odd columns (labeled "Canonical") contain the estimate $\hat{\gamma}$ from regression (2) and even columns (labeled "Avg. dyn.") contain the estimate $\hat{\hat{\gamma}} = \frac{1}{37}\sum_{k=0}^{36}\hat{\gamma}^k$ with $\hat{\gamma}^k$s being the estimated dynamic treatment effects from regression (1). All regressions include individual and year-month fixed effects. The regressions in even columns also include the terms $\sum_{k=-36}^{-1}\gamma^k\mathbf{1}\{K_{it}=k\}$. Standard errors are clustered on the individual level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Employment Effects of Accidents, Full Sample and Treatment Effect Heterogeneity, Robustness Check: Weaker Sample Restrictions

	(1) Canonical	(2) Avg. dyn.	(3) Canonical	(4) Avg. dyn.	(5) Canonical	(6) Avg. dyn.
	(A) Full Sample					
Post-accident	-0.0451^{***}	-0.140^{***}				
	(0.00186)	(0.00343)				
Observations	4,128,338	4,128,338				
	(B) By Education					
	No high school degree		High school degree or more			
Post-accident	-0.0642^{***}	-0.140^{***}	-0.0507^{***}	-0.140^{***}		
	(0.00266)	(0.00475)	(0.00301)	(0.00544)		
Observations	1,954,813	1,954,813	1,290,455	1,290,455		
	(C) By Health Insurance Coverage					
	FONASA		ISAPRE			
Post-accident	-0.0807^{***}	-0.125^{***}	-0.0500^{***}	-0.131^{***}		
	(0.00294)	(0.00534)	(0.00744)	(0.0161)		
Observations	2,052,299	2,052,299	161,702	161,702		
	(D) By Modal Pre-Accident Industry					
	Agriculture, fishing, mining		Manufacturing, construction, transportation		Wholesale, retail, restaurant, finance, real estate, education, health	
Post-accident	-0.0909^{***}	-0.151^{***}	-0.0967^{***}	-0.159^{***}	-0.0760^{***}	-0.151^{***}
	(0.00479)	(0.00823)	(0.00279)	(0.00481)	(0.00335)	(0.00569)
Observations	342,089	342,089	1,136,318	1,136,318	768,321	768,321

Notes: The table shows the average treatment effect of an accident on employment in the formal sector. Odd columns (labeled "Canonical") contain the estimate $\hat{\gamma}$ from regression (2) and even columns (labeled "Avg. dyn.") contain the estimate $\hat{\hat{\gamma}} = \frac{1}{37}\sum_{k=0}^{36}\hat{\gamma}^k$ with $\hat{\gamma}^k$s being the estimated dynamic treatment effects from regression (1). The sample includes men with an accident between 1/2004 and 12/2007 with at least 12 months of employment before the accident, independent of when they became affiliated with SC and their pre-accident employment history. All regressions include individual and year-month fixed effects. Standard errors are clustered on the individual level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A Appendix: The Chilean Health Care System

We briefly describe the Chilean health care system to provide some background information and to motivate one of the sample splits in our heterogeneous treatment effect results. Chile has a 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 insurance plans that act as alternatives to FONASA.[13] Employees are enrolled in the public FONASA system by default but can opt out and join an ISAPRE. In 2009, about 74% of the Chilean population was enrolled in the FONASA and about 16% were members of an ISAPRE.

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 the 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 in public facilities or private facilities that have an agreement with FONASA at these copayment levels.

Individuals who opt out of the FONASA can choose among 13 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 can 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 make hospital beds available to non-FONASA beneficiaries. Second, ISAPRE beneficiaries avoid using public providers, because they can obtain better quality and timelier health care through their regular coverage. Overall, ISAPRE plans are more expensive than FONASA plans but provide access to better health care with shorter waiting times. In our analysis, we therefore use ISAPRE membership as a proxy for better access and higher-quality health care.

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