IZA Institute of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 16790

Job Loss, Unemployment Insurance, and Health: Evidence from Brazil

Guilherme Amorim
Diogo G. C. Britto
Alexandre Fonseca
Breno Sampaio

FEBRUARY 2024

IZA Institute of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 16790

Job Loss, Unemployment Insurance, and Health: Evidence from Brazil

Guilherme Amorim
Gies College of Business, University of Illinois at Urbana-Champaign

Diogo G. C. Britto
University of Milan-Bicocca, Bocconi University, CEPR, GAPPE/UFPE, and IZA

Alexandre Fonseca
Federal Revenue of Brazil, GAPPE/UFPE

Breno Sampaio
Federal University of Pernambuco, GAPPE/UFPE and IZA

FEBRUARY 2024

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Job Loss, Unemployment Insurance, and Health: Evidence from Brazil*

We study the effects of job loss and unemployment insurance (UI) on health among Brazilian workers. We construct a novel dataset linking individual-level administrative records on employment, hospital discharges, and mortality for a 17-year period, rarely available in the context of developing countries. Leveraging mass layoffs for identification, we find that job loss increases hospitalization (+33%) and mortality risks (+23%) for male workers, while women are not affected. These effects are pervasive over the distribution of age, tenure, income and education, and men's children are also negatively affected. Remarkably, about half of these impacts are driven by external causes associated with accidents and the violent Brazilian context. Using a regression discontinuity design, we show that access to UI partially mitigates the adverse effects of job loss on health. Our results indicate that the health costs of job loss are only partially explained by the income losses associated with job displacement.

JEL Classification: I12, J63, J65

Keywords: job loss, unemployment insurance, hospitalization, deaths

Corresponding author:
Guilherme Amorim
Gies College of Business
University of Illinois at Urbana-Champaign
1206 S 6th St
Champaign
IL 61820
USA
E-mail: gm10@illinois.edu

* We thank Richard Akresh, Bernard Black, Mark Borgschulte, Eliza Forsythe, Andy Garin, Marieke Kleemans, Kurt Lavetti, Audrey Light, Nolan Miller, David Molitor, Adam Osman, Paolo Pinotti, Kent Smetters, Rebecca Thornton, Russel Weinstein, and several seminar participants for comments. Amorim is indebted to Dan Bernhardt, Alex Bartik, Ben Marx, and Julian Reif for their support and advice.

Job loss is one of the most important economic shocks in modern societies, leading to large and persistent income losses (e.g., Bertheau et al., 2023; Couch and Placzek, 2010; Jacobson et al., 1993). Beyond its financial repercussions, it has been shown to adversely affect different aspects of workers' lives.[1] In particular, previous studies have underscored the substantial health and mortality risks imposed by job loss, particularly for relatively older, high-tenure men (e.g., Sullivan and Wachter, 2009). However, as the existing evidence is predominantly concentrated on high-income economies, much less is known about how these effects manifest in the context of developing countries. In such settings, the health costs of layoffs could be significantly larger due to widespread poverty and the lack of social insurance and adequate public health services. More importantly, evidence on the effectiveness of public policy alternatives aimed at mitigating these impacts remains scant both in developing and developed contexts.

In this paper, we investigate the causal effects of job loss on hospitalization and mortality, exploring how access to unemployment benefits may mitigate any effects. We focus on a large developing country, Brazil, and we build a uniquely comprehensive dataset, rarely available in similar settings. This dataset links individual-level administrative records on employment, hospital discharges, mortality, and access to unemployment benefits for the entire country from 2002 to 2018, along with information on family ties.

In the first part of the paper, we estimate the effects of job loss on health outcomes. Using a difference-in-differences design, we compare workers displaced in mass layoffs over time relative to similar control workers, defined via matching, who were not displaced in the same period. This strategy builds on the idea that mass layoffs should not depend on individual worker factors and has been widely used in previous work studying the effects of job loss on different outcomes (e.g., Couch and Placzek, 2010; Jacobson et al., 1993). In addition to providing evidence of parallel pre-trends, we address numerous potential identification concerns, such as those related to selection into treatment

[1] For impacts on other dimensions, see, for example, Britto et al. (2022); Khanna et al. (2021) for effects on crime and Rege et al. (2011) for effects on children's test scores.

and to mass layoffs spillover effects. We also develop a novel intent-to-treat (ITT) analysis that allows us to inspect pre-trends when analyzing mortality outcomes.[2] Specifically, we compare mortality rates over time for workers employed in firms that will experience a mass layoff in subsequent years – regardless of which individuals will actually be displaced – relative to similar workers in firms without mass layoffs. This allows us to show that mortality trends evolve in parallel across treated and control firms before the mass layoff event.

We begin by showing that job loss causes significant incomes losses for displaced workers. In particular, we show that labor income decreases by as much as 36% and 39% in the four-year period following layoff for men and women, respectively.[3]

Next, we show that job loss has adverse consequences for men's health. Specifically, the yearly probability of male hospitalization increases by .1 percentage points (.p.p) in the four-year period following layoff – a 33% increase relative to the pre-displacement mean. Their cumulative mortality is also 23% higher up to eight years after layoff. These effects emerge quickly in the first year after displacement and persist for several years. Interestingly, these impacts are not concentrated on high-tenure, older men. Rather, they are fairly pervasive over the distribution of workers' age, tenure, earnings, and education. We also find significant increases in hospitalization for the children of displaced men. Instead, effects on displaced women and their children are small and statistically insignificant, even though they experience similar income losses to men following job loss.

We also show that job loss reduces private health insurance (HI) coverage by about 2 p.p. for men and women. These plans supplement the Brazilian universal public health system which offers free health care to all citizens.

[2] We cannot do so in our main empirical design for mortality outcomes since, by construction, treated and control workers are alive in the pre-treatment period.

[3] Our main analysis is fully based on formal jobs which we can observe in the administrative data. Nevertheless, using survey data and a welfare registry, we provide evidence that informal employment compensates only a small fraction of the overall labor income losses. We also show that spousal labor supply does not significant compensate for these losses.

They are often employer-sponsored, typically covering high-income individuals seeking higher quality services (about 13% of our sample). The loss of private HI coverage, however, cannot explain the adverse effects on men's health, as we find similar effects for men without private HI coverage before layoff.

Next, we investigate to what extent the adverse effects on men's health are driven by different underlying reasons. Remarkably, about half of the effects on male hospitalization and mortality are driven by external causes, namely, by accidents and assaults. They line up well with the fact that external causes explain 15.2% of deaths in Brazil relative to the 7.6% in the world. In turn, 4.5% of deaths are associated with assaults relative to a 0.7% global average.[4] Thus, our results indicate that engagement in risky behavior and exposure to violence explain a meaningful part of the health toll of job loss in our context. The remaining half of the effects on men's health is linked to non-external causes, in particular by stress-related conditions such as ischemic heart diseases.

A relevant policy-related question is to what extent the effect of job loss on men's health is mediated by the income losses caused by job loss. For example, tighter financial constraints could cause more stress and drive worse health outcomes. If this is the case, policies focused on compensating these losses might have better chances of attenuating the health costs of job loss. At the same time, job loss is a stressful event *per se* which could lead to worse health outcomes, independently of the associated income losses.[5] To gain insight on these aspects, we compare the effects of job loss across workers who are predicted to experience different degrees of income losses upon job loss. We find that the adverse effects on men's health do not vary strongly across groups predicted to experience different levels of income losses, suggesting that income losses may not be the only mechanism at play.[6] However, the effects

[4] Source: `ourworldindata.org/causes-of-death-treemap`, retrieved on November 27th, 2023.

[5] Alternatively, the effects of job loss on health could also be explained by how workers allocate their time after layoff, which could be invested in activities which are more or less beneficial to their health.

[6] Specifically, we predict income losses due to job loss based on a array of workers characteristics (e.g., age, education, tenure, sector, and location). Then, we compare the effects

on hospitalization due to external causes are largest for workers in top quartile of the distribution of predicted income losses. The latter suggests that income may play a role in explaining the increase in externally-driven health incidents.

In the second part of the paper, we study the effects of access to unemployment insurance (UI) on the health of displaced men. This analysis is both relevant from a policy perspective and for shedding light on the role of income losses as a driver of the impacts of job loss on health. UI is the main policy providing income support to displaced workers in Brazil. It offers from three to five months of unemployment benefits at an average 81% replacement rate. To identify its effects, we leverage from a policy cutoff determining UI access based on small variations in dismissal dates using a clean regression discontinuity design.

Our results show that access to UI partially offsets the adverse effects of job loss on men's health outcomes. In particular, it largely offsets the increase in hospitalization risk due to external causes for older workers above age 35. This effect lasts during the first year after layoff, when UI transfers are paid out. We also find suggestive evidence that access to UI reduces the risk of mortality due to external causes in our full sample including men of all ages, although this effect is only statistically significant at the 10 percent confidence level. On the other hand, we find small and statistically insignificant impacts on hospitalization and mortality due to non-external causes.

Overall, the evidence on UI effects supports the idea that programs offering income transfers to displaced workers can partially mitigate the health costs of job loss. In terms of mechanisms, it suggests that income losses play a role in explaining the effects of job loss on incidents related to external causes. This interpretation is also supported by the fact that UI transfers in Brazil are close to a pure income transfer. For example, they are not conditional on job search requirements, participation in training programs, or follow-up meeting with caseworkers. In turn, these results indicate that the effects of job loss on non-external health events are less likely to be tied to the income losses associated with layoffs.

---

on health outcomes across workers predicted to experience different levels of income losses.

This paper contributes to an empirical literature studying the effects of job loss on health outcomes which has been overwhelmingly concentrated in advanced economies – see Picchio and Ubaldi (2022) for a recent review and meta analysis. A first strand of studies have used survey or aggregated data, finding mixed results – e.g., Black et al. (2015); Ruhm (2000); Salm (2009); Schaller and Stevens (2011, 2015) for Germany, Norway, and the US.[7] A second strand of research has relied on population administrative data and used mass layoffs or plant closures as a source of exogenous variation in the context of Austria, Scandinavian countries, and the US – see Bloemen et al. (2018); Browning and Heinesen (2012); Eliason and Storrie (2009); Kuhn et al. (2009); Sullivan and Wachter (2009). Overall, these studies tend to focus on relatively older, high-tenure workers, most of them finding increases in mortality risks after job loss.

To our knowledge, we provide the first evidence on the effect of job loss on health using high-quality administrative data for a large developing country, along with a credible source of exogenous variation based on mass layoffs.[8] Our paper complements earlier work studying the effects of job loss in Brazil on self-reported health with survey data (Giatti et al., 2008) and on children's mental health with a cohort study sample (Fontes et al., 2022). Our data and setting allow us to explore impacts on different causes of hospitalization and deaths at a singular level of detail, revealing the importance of external factors on health in our context. They also allow us to document heterogeneous effects over a rich set of individual characteristics and to explore the indirect health impacts of job loss on workers' family members.

Importantly, this paper offers insights into the effectiveness of transfer policies in alleviating the health costs of job loss. Using a clean regression discontinuity design, it provides novel evidence that access to unemployment benefits can mitigate some of the negative effects of job loss on health outcomes –

[7]While Salm (2009); Schaller and Stevens (2011) find no effects for layoffs driven by plant closures, Ruhm (2000) find a positive association between unemployment and health. In turn, Schaller and Stevens (2015) find that job loss leads to worse self-reported health.

[8]In terms of institutional setting and data, our work relates closely to Britto et al. (2022) who study the effects of job loss and unemployment benefits on crime.

namely, on hospitalization from external causes. This evidence is also informative to our understanding of mechanisms, suggesting that income losses associated with job loss are a driver of its impacts on externally-driven health events. The only prior study addressing these aspects is Kuka (2020), who explores state-level variation in UI generosity in the US. She shows that higher UI generosity in the US leads to higher HI coverage and utilization, along with improvements in *self-reported* health indicators.

The remainder of the paper is organized as follows. Section 1 provides background information on the Brazilian labor market, mortality trends, and health care in Brazil. Section 2 describes the data and details the merging procedure between our various data sources. Section 3 presents our main results on the impacts of job loss on health, and Section 4 investigates the mitigation effects of unemployment insurance. Section 5 concludes.

1 Institutional Background

1.1 Health Care in Brazil

Brazil is a large developing country, hosting nearly one-third of the population in Latin America. The country provides completely free and universal access to health care through its Unified Health System (*Sistema Único de Saúde* – SUS). The system is maintained with significant government investments, amounting to about 10% of the country's GDP (Azevedo et al., 2016). The SUS ensures public health provision at all levels of complexity. It has hospitals, emergency rooms, and community care centers operating in over 90% of Brazilian municipalities, and provides access to primary health care even in the most remote rural areas of the country (Bhalotra et al., 2019) – a substantial achievement for a developing country with more than 200 million inhabitants. Nevertheless, lack of supplies and long waiting lines are common issues in the system. In parallel to the SUS, privately owned hospitals and clinics offer supplementary health care in the country. These services are largely financed through individual enrollment in private health insurance (HI) plans, which are commonly employer-sponsored (72% of all active plans in 2020). Overall, private HI covers 24% of the population (ANS, 2019) and is strongly concentrated on high-income individuals seeking higher quality services.

Appendix Figure A1a shows that mortality rates have been strongly countercyclical with respect to the employment rate over the past decades, despite the large investments in public health. Appendix Figure A1b plots the main causes for adult mortality and in-patient admissions to public hospitals based on the ICD-10 (International Classification of Diseases, tenth revision). In line with worldwide patterns, heart, infectious and respiratory diseases are leading causes of mortality and hospitalization.[9] However, a distinctive feature of the Brazilian context is the large number of deaths and hospitalizations due to external causes. Overall, 15.2% of all deaths in Brazil are due to external factors relative to 7.6% worldwide, and 4.5% are directly associated with assaults (vs. 0.7% worldwide).[10] The country's violent background helps explaining the significant number of incidents due to assaults: Brazil displays the seventh-highest homicide rate in the World – 30.7 per one 100k inhabitants in 2017. It may also help explaining a large numbers of incidents associated with injuries that are not linked to a specific underlying cause (interpersonal violence, accidents, or self-harm).[11]

1.2 The Brazilian Labor Market

Labor relations in Brazil are regulated at the federal level. Firms are free to terminate workers unilaterally, without cause, upon the payment of termination costs.[12] Such terminations represent about 70% of (formal) job separations and are the focus of this paper.[13] In line with other developing countries, labor informality is high in Brazil, comprising an estimated 45% of all jobs in 2012. Job turnover is also high and there is substantial interaction between

[9] For worldwide statistics on mortality, see Dattani et al., 2023.

[10] Source: `ourworldindata.org/causes-of-death-treemap`, retrieved on November 27th, 2023.

[11] Hospitalization and mortality events are often associated with more than one ICD code which can refer both to an underlying medical condition (e.g., an injury to a specific part of the body), or an underlying reason of the event (e.g., different forms of interpersonal violence, accidents, or self-harm).

[12] Workers dismissed without cause receive roughly 1.3 monthly wages per tenure year in the form of a mandatory savings account and severance pay.

[13] Other terminations are mostly related to job quits.

the formal and informal sectors, with many workers moving frequently between the two. In addition, some firms maintain both formally- and informally-hired workers in their payroll (Ulyssea, 2018). Due to the lack of comprehensive data on informal jobs, our analysis focuses on workers leaving formal jobs. We will use survey data to assess how informal jobs affect the employment recovery of displaced workers and to study whether these jobs play any role as a mechanism explaining our findings.

The main program providing financial relief to displaced workers in Brazil is unemployment insurance, administered by the federal government. UI benefits last from three to five months, while the replacement rate starts at 100% for workers earning the minimum wage and declines smoothly to 67% at the benefit cap (2.65 times the minimum wage).[14] The only other form of income support at the national level is *Bolsa Familia* cash transfers, which cover roughly one-fourth of the population. However, this program targets very low-income families with per capita income below .1 minimum wages and the average transfer per family is only .16 minimum wages in the period studied.

2 Data

2.1 Data Sources

Our paper relies mainly on five core, individual-level data sources. First, we use employment data tracking the universe of formal jobs in Brazil for the 2000-2018 period from RAIS (*Relação Anual de Informações Sociais*). These data contain detailed contract-level information such as starting and termination dates, reason for termination, earnings, occupation, and workers' demographics such as date of birth, education and race. Additionally, it includes unique tax ids for both workers (CPF) and firms (CNPJ).

Second, we use data on the universe of admissions to public hospitals for the 2000-2018 period from SIH-SUS (*Sistema de Internações Hospitalares*). These data contain detailed information on each admission such as ICD-10 codes, procedure codes (if any), date of admission, length of stay, total value

[14] Government expenditure on the program is as large as 0.53% of the country's GDP (Tesouro Nacional, 2019).

charged and paid, the reason for admission and hospital identifiers. Our analysis focuses on emergency hospitalizations, which represent 82% of all admissions.[15,16] In addition, these data include detailed individual information such as date of birth, gender, municipality, and postal code of residence.

Third, we use population-wide mortality data for the 2000-2018 period from SIM-SUS (*Sistema de Informação sobre Mortalidade*), based on death certificates collected by the federal government. These data include the date of the event, along with ICD-10 codes on death causes, in addition to individual characteristics such as date of birth, gender, municipality and zip code of residence. Fourth, we use data on the take-up of private HI plans from ANS (*Agência Nacional de Saúde).* These data allow us to track the take-up of individual and employer-sponsored private HI plans over time, along with individual information on date of birth, gender, municipality and borough of residence.

Finally, we use a person registry covering the entire Brazilian population maintained by the Brazilian tax authority (*Cadastro Pessoa Física* – CPF). This dataset contains rich individual-level information such as gender, date of birth, mother's full name, year of death if deceased, a full history of residential addresses (including municipality, borough and postal code) and the individual's unique tax id.[17]

2.2 Individual Linkage Across Datasets

While individuals in the employment and person registries are identified by their tax ids, unique individual identifiers are not available in the health datasets. Therefore, the first step in our analysis is linking individuals in

[15]We identify these based on the reason for admission information. We pool together admissions classified as emergency (82% of admissions) and accidents (less than 2% of admissions) under a single emergency category for our analysis. Elective admissions cover virtually all of the remainder (about 18% of admissions).

[16]To avoid confounding our estimates with potential work-related injuries, we also exclude all events related to workplace accidents and those associated with commuting to/from work, both of which are coded separately in our database. These, nevertheless, correspond to a very small fraction of overall observations.

[17]In addition, for individuals filing tax from 2006 onwards, it also includes information on their spouses and dependents (including children).

the health datasets to their tax ids. Starting with the hospitalization data, we leverage the fact that a substantial share of individuals in the country can be uniquely identified by information available in the hospital records; namely, by their date of birth, gender, and postal code of residence. We call this group of variables a "linkage key".[18] Using the person registry, we verify that 76% of Brazilian residents can be uniquely identified by this linkage key. Hence, we associate individuals in the hospitalization data that can be uniquely identified by this linkage key with their tax ids, which then allows us to merge the hospitalization and the employment data at the individual level.[19] We repeat this same procedure for the private HI data using date of birth, gender and borough of residence as a linkage key – 57% of the population in the person registry can be uniquely identified by this information. Finally, we follow this procedure for the mortality data using year of death, date of birth, gender and municipality of residence as a linkage key – 64% of deceased individuals in the mortality data can be uniquely identified with this information.

When analyzing outcomes based on the hospitalization and private HI data, we focus on the subsample of residents who can be uniquely identified by the respective linkage key used to identify individuals with their tax ids.[20] Such restriction is not possible with mortality outcomes since the tax id linkage is done among deceased individuals. Therefore, we use the entire sample in our analysis of mortality. We will provide evidence that the different samples used to analyze different health outcomes are remarkably similar, suggesting that selection bias is highly unlikely. (Section 3.1, Appendix Table A1).

Overall, our procedure is designed to minimize measurement error, al-

[18] In large Brazilian municipalities, the postal code refers to specific streets, while smaller municipalities with less than 50k inhabitants usually have a single postal code.

[19] In other words, for individuals who can be uniquely identified in the person registry, covering the entire population, using the linkage key, we associate their unique tax id observed in that registry to their records in the hospitalization data. We then link each individual in the hospitalization data to their employment data using their tax id. We perform this procedure year by year so that it takes into account individuals moving across addresses.

[20] Namely, 76% (57%) of Brazilian residents who can be uniquely identified by their date of birth, gender and postal code of residence for hospitalization (private HI) outcomes. Thus, we exclude from the analysis individuals who we cannot possibly identify in the hospitalization and private HI data.

though some incorrect linkages may still occur for various reasons.[21] Nevertheless, we expect such errors to be quantitatively small, given the high quality of our administrative datasets. Any residual measurement errors in health outcomes are likely classical and should only result in slightly more imprecise estimates.[22] We will support this idea with a robustness exercise that emulates the same procedure to create employment outcomes (Section 3.8).

2.3 Descriptive Evidence

Appendix Figure A.1 illustrates the probability of hospitalization for displaced workers pre- and post-layoff, superimposed on a density plot of their ages at the time of dismissal. The figure highlights a higher risk of hospitalization for workers under 24 and those above 40, with no significant gap for workers in the middle age range.[23] This suggests potential health-related consequences of job loss, with age at dismissal being a potential predictor of the magnitude of impacts. The subsequent analysis will explore these possibilities more rigorously.

3 Job Loss, Public Hospitalization, and Mortality

3.1 Sample Selection and Empirical Strategy

In this section, we investigate the impacts of job loss on different health and labor market outcomes. We adopt a combined matching/difference-in-differences approach to identify the effects of job loss on health, using mass layoffs as a source of exogenous variation. Since mass layoffs are arguably unrelated to workers' individual behaviors, they have been widely used in earlier literature to estimate the effects of job loss on different outcomes.

[21] For example, despite the high quality of the health data sources, there might be some mistakes in the filling process undertaken by health units all over Brazil. Another possible source of measurement error is some lag in address updating in the person registry, which could affect the accuracy of our algorithm for identifying individuals based on their characteristics.

[22] This should not generate attenuation bias because such error affects only the dependent variable in our setting.

[23] Such U-shaped association is also reported as a statistically insignificant result in a meta-analytical study by Paul and Moser (2009), with youths and older adults close to retirement displaying more severe risks of distress from unemployment.

Our analysis focuses on full-time workers in the 18-65 age range with open-ended contracts in the non-agricultural, private sector.[24] The treatment group comprises workers displaced in a mass layoff during the 2006-2014 period.[25] This allows us to estimate dynamic treatment effects using a balanced panel for up to four years after displacement, as well as placebo effects up to three years before displacement.

We build the control group via exact matching. For each mass layoff year, the set of potential controls contains workers employed in non-mass layoff firms who were not displaced in the same calendar year.[26] We match each treated worker with a control worker based on individual-, firm- and regional-level characteristics. These are: gender, birth cohort, tenure, earnings (R$250/month bins), one-digit industrial sector (9 categories), firm size (quartiles), firm layoff rate in the three years prior to treatment (deciles), firm median tenure (years) and median wage (quartiles), municipal population (deciles), and state (27 categories). When treated workers are matched with multiple controls, a single control unit is randomly selected. We then assign to each control worker a placebo treatment date equal to the layoff date of their treated counterpart and compare outcomes for the two groups relative to this date. This stacking approach ensures that we compare treated workers with control units that have not been treated during our analysis period, addressing methodological concerns raised recently in the difference-in-differences literature.[27]

We estimate the following dynamic difference-in-differences equation:

[24] To avoid confounding retirement effects, we focus on women below age 60.

[25] We define mass layoff events by firms displacing (without cause) more than 33% of the workforce in a single calendar year – similarly to earlier studies (Britto et al., 2022; Couch and Placzek, 2010; Jacobson et al., 1993).

[26] We define non-mass layoff firms as those that do not experience such events during our period of analysis (2002-2017).

[27] For example, this approach is in line with methodological work by Dube et al. (2023) and follows recent work by Cengiz et al. (2019) and, in a similar setting to ours, Britto et al. (2022). In addition, we show in Appendix B.11 that our results are robust to the alternative estimator proposed by De Chaisemartin and d'Haultfoeuille (2020).

$$Y_{it} = \alpha + \delta\, Treat_i + \sum_{t=-P}^{T} \beta_t\, Treat_i \cdot Time_t + \sum_{t=-P}^{T} \lambda_t\, Time_t + \epsilon_{it}. \qquad (1)$$

Y_{it} is an outcome of interest for worker i at period t. Time periods t are set in yearly periods relative to each worker i's exact dismissal date (a placebo date in the case of control units). $Treat_i$ is a dummy indicating that worker i belongs to the treatment group, and $Time_t$ is a dummy identifying the number of elapsed years since the worker's dismissal date ($Time_1 = 1$ in the first 12 months since layoff, $Time_2 = 1$ in the following 12 months, and so on, while $Time_0 = 1$ in the 12 months before layoff, $Time_{-1} = 1$ in the 12 months previous to that, and so on). The baseline omitted period is set at $t = 0$. The coefficients $\{\beta_1, ..., \beta_T\}$ identify the dynamic treatment effects, while $\{\beta_{-P}, ..., \beta_{-1}\}$ identify any potential anticipation effects.[28] The average treatment effects over all periods are estimated using the equation:

$$Y_{it} = \alpha + \delta\, Treat_i + \beta\, Treat_i \cdot Post_t + \lambda\, Post_t + \epsilon_{it}, \qquad (2)$$

where the dummy $Post_t$ represents post-treatment periods. Standard errors in both equations are clustered at the firm level.

Since we use different samples for studying different outcomes, we repeat our matching procedure separately to build the treatment and control groups in each of these analyses – see Section 2.2.[29] Appendix Table A1 presents summary statistics for treated and control units in each of the three samples. First, the table shows that both the treatment and control groups are quite similar across samples (columns 1-2; 4-5; 7-8). This supports the idea that the sample restrictions we impose for linking different datasets do not raise strong

[28]Since we work with a perfectly balanced panel, comparing the same group of treated and control workers before and after treatment, this specification naturally absorbs individual fixed-effects. In fact, adding individual fixed-effects to this regression does not lead to any change in the coefficient estimates.

[29]Specifically, we use the full sample for studying employment and mortality outcomes, a sample restricted to individuals who can be uniquely identified in the country by their zip code/gender/date of birth for hospitalization outcomes, and by their borough/gender/date of birth for HI outcomes.

selection concerns. Second, the table shows that treatment and control groups are also balanced across a rich set of individual, regional and firm characteristics within samples (including characteristics that are not part of the matching process). The standardized difference is generally below or near the .20 cutoff for all characteristics (columns 3; 6; 9), indicating that differences in their underlying distributions are small (Cohen, 2013). Although our difference-in-differences design does not formally require the treatment and control groups to be the same in levels, these similarities offer additional support to our main identification assumption – that health outcomes would have followed parallel trends in the counterfactual scenario where treatment does not take place.

Nevertheless, the main threat to our empirical strategy is the possibility of dynamic selection of workers into treatment. Some third factors could lead to worse health conditions and simultaneously increase the likelihood of job loss.[30] Leveraging mass layoffs arguably driven by firm-specific factors mitigates these concerns but does not completely eliminate them, as firms still have some discretion in selecting whom to displace. In Appendix Section B.9, we provide different robustness checks addressing these concerns – e.g., we show that our results continue to hold when using more stringent mass layoff definitions, where the scope for selection into displacement is considerably reduced. We also address other identification concerns, such as local spillover effects of mass layoffs, and discuss the external validity of our analysis, since mass layoffs could, in principle, differ significantly from regular layoffs.

When estimating effects on mortality, we cannot use the pre-period in our baseline strategy because, by construction, all treated and control workers are alive before treatment. Given this limitation, we rely solely on the matching strategy to estimate impacts on mortality. We estimate differences in mortality across groups in the post-treatment period using the following equation: $Y_{it} = \sum_{t=0}^{T} \beta_t Treat_i \cdot Time_t + \sum_{t=0}^{T} \lambda_t Time_t + \epsilon_{it}$. Analogously, the average treatment effect is estimated using the following simple equation:

[30] For example, family shocks such as divorce or the loss of a close relative could lead to higher stress and worse health. At the same time, these events might increase the likelihood of job displacement, even within the context of mass layoffs.

$Y_{it} = \beta Treat_i + \epsilon_{it}$. In Appendix B.10 we develop a novel intent-to-treat (ITT) approach which combines firm-level variations in mass layoffs with a difference-in-difference strategy. Specifically, we compare mortality rates over time for workers employed in firms anticipating a future mass layoff – independently of who will and who will not be displaced in that event – relative to similar workers in non-mass layoff firms. This allows us to inspect mortality trends across treated and control firms before the mass layoff. We show that mortality rates in treated and control firms follow similar trends before treatment, yielding results consistent with our baseline strategy.

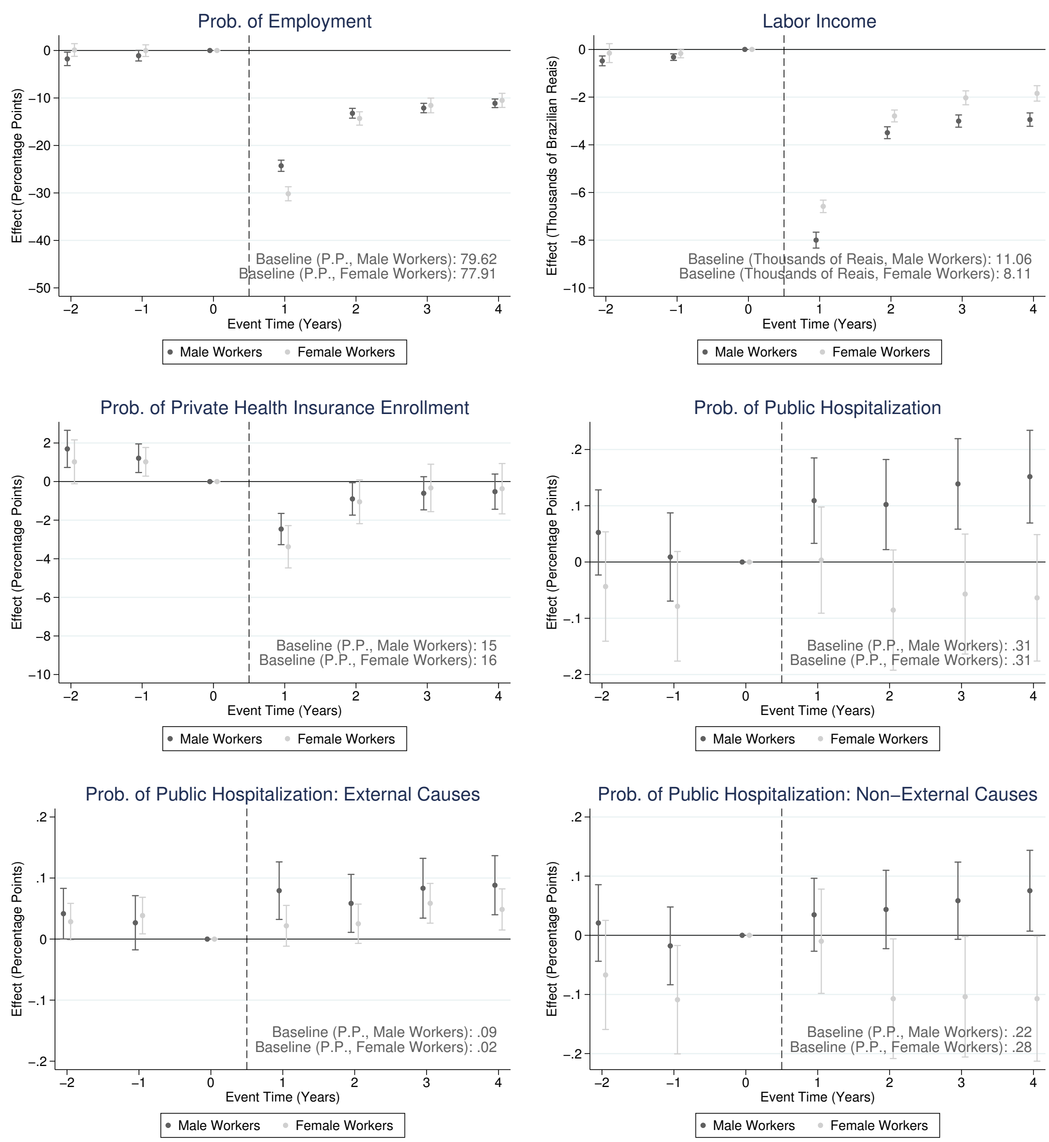
3.2 Effects on Employment, Private Health Insurance Coverage, and Hospitalization

Our main results on labor market outcomes, private HI enrollment and admissions to public hospitals are displayed in Figure 1. The graphs show the dynamic treatment effect of job loss on different outcomes based on equation (1), reporting separate estimates for male (dark gray) and female workers (light gray). The baseline for each group is the mean outcome value for the treated group in all pre-treatment years.

The two upper figures show large negative effects on employment and labor income. In the first year after layoff, the probability of employment decreases by 24 and 30 percentage points (p.p.) for male and female workers, respectively. Their labor income in the first year after layoff decreases by 8 and 6.6 thousand Brazilian Reais (BRL), respectively – a 56% and 60% drop relative to the baseline. These large negative effects quickly diminish in the following years, but they remain sizable even four years after layoff. On average, labor income decreases by 36-39% for men and women in the four years after layoff – see Table 1, based on eq. (2). In Appendix B.2, we provide evidence that labor income losses remain sizable even after taking into account that some workers enter informal jobs after layoff.[31] We also show that spousal labor supply changes little after layoff, revealing that added worker effects are small

[31] In particular, we show that absolute labor income losses decrease by 10-20% once we consider informal jobs in the analysis.

Figure 1: Dynamic Effects of Job Loss on Employment, Income, HI Enrollment, and Hospitalization



Notes: This figure shows the dynamic treatment effects of job loss due to a mass layoff on formal employment, labor income, private health insurance enrollment and emergency admissions to public hospitals. Outcomes are shown separately for both male (dark gray) and female workers (light gray). Estimates were computed using the difference-in-differences equation (1). Each sample includes a treatment group of workers displaced in mass layoffs and a matched control group of workers not displaced in the same year, working in firms that did not experience mass layoffs. 95% confidence intervals are also reported. Income variables are measured in BRL.

in our context – see Appendix B.6. Overall, job loss entails sizable and persistent employment and income losses – which are also large in comparison to estimates in the context of developed countries (see, e.g., Bertheau et al., 2023).

Table 1: Average Effects of Job Loss on Employment, HI Enrollment, and Hospitalization

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Labor Market Outcomes		**Prob. of Private HI Enrollment**			**Prob. of Public Hospitalization**		
	Prob. of Employment	Labor Income	All Plans	by Plan Type: Corporate	Individual	Overall	by Cause: External	Non-Ext.
	Panel A: Male Workers							
Point Estimate	-14.2351	-4,092.2441	-2.0895	-1.8238	-0.0333	0.1049	0.0545	0.0521
	(0.3634)	(127.0389)	(0.3366)	(0.2956)	(0.0429)	(0.0230)	(0.0134)	(0.0189)
Baseline Mean (Treated, $t \leq 0$)	79.6208	11066.8167	12.7311	6.8832	.4393	.3123	.0909	.2294
Effect Relative to Baseline	-17%	-36%	-16%	-26%	-7%	33%	59%	22%
Implied Elasticity to Employment	-	-	0.94	1.52	0.41	-1.94	-3.47	-1.29
Implied Elasticity to Earnings	-	-	0.44	0.72	0.19	-0.91	-1.63	-0.61
Observations	2,017,162	2,017,162	700,014	700,014	700,014	1,411,942	1,411,942	1,411,942
	Panel B: Female Workers							
Point Estimate	-16.6660	-3,204.0571	-1.9634	-1.9365	0.1507	-0.0099	0.0162	-0.0235
	(0.6599)	(123.6602)	(0.4121)	(0.4327)	(0.0824)	(0.0308)	(0.0091)	(0.0294)
Baseline Mean (Treated, $t \leq 0$)	77.9164	8116.9586	13.8542	7.08	.9741	.3105	.0268	.2842
Effect Relative to Baseline	-21%	-39%	-14%	-27%	15%	-3%	60%	-8%
Implied Elasticity to Employment	-	-	0.66	1.28	-0.71	0.14	-2.85	0.38
Implied Elasticity to Earnings	-	-	0.35	0.69	-0.38	0.07	-1.53	0.20
Observations	1,121,064	1,121,064	421,120	421,120	421,120	835,772	835,772	835,772

Notes: This table shows the effect of job loss due to a mass layoff on labor market outcomes (column 1 and 2), private HI enrollment (columns 3 to 5) and public hospitalization (columns 6 to 8). Labor income (column 2) is measured in BRL. Estimates were computed using the difference-in-differences equation (2). Dependent variables are indicated at the top of each column. The explanatory variable of interest is a dummy $Treat_i$ equal to 1 for treated workers, interacted with a dummy $Post_t$ equal to 1 for the period after displacement. The sample includes a treatment group of workers displaced in mass layoffs and a matched control group of workers not displaced in the same year, working in firms that did not experience mass layoffs. Standard errors clustered at the firm level are indicated in parenthesis. All coefficients, standard errors, and baseline means representing probabilities have been scaled by 100, and effects are thus interpreted in terms of percentage points.

Next, we show that enrollment in private HI decreases between 2.5 and 3.4 p.p. one year after layoff for men and women, corresponding to a 20% to 25% drop relative to their baselines (middle-left-side graph, Figure 1). The initial drop is explained by the fact that about 72% of these plans are employer-sponsored. In Table 1, we show that only women increase the take-up of individual plans, but that the effect is quantitatively small relative to the loss in employer-sponsored coverage (columns 4-5). Although formal employment is still 10 p.p. lower four years after layoff, the negative effects on private HI coverage vanish four years after the layoff for men and women. This suggests a potential mechanism where the recovery in employment is biased towards firms sponsoring private HI plans.

Next, we investigate the implications of job loss on workers' health outcomes. Figure 1 shows the effects of job loss on public hospitalization – as measured by the probability of emergency, in-patient admissions covered by the SUS (middle-right-side graph). We find a strong positive effect on hospitalization for men, which increases on average by .1 p.p., or 33% relative to the baseline – see Table 1. This implies a sizable elasticity of hospitalization to employment and labor income of -1.94 and -.91.[32] This effect is persistent, remaining sizable for at least four years, which is in line with the persistent employment losses. Interestingly, hospitalizations for external causes explain half of the total effects, increasing by .0545 p.p., while non-external causes explain the remaining part, increasing by .0521 p.p. – see Panel A Table 1 (columns 7-8), and Figure 1 (bottom graphs). Hence, a substantial portion of the impacts on men can be explained by external factors – e.g., related to accidents and interpersonal violence.

In turn, the overall impact on the probability of hospitalization for women is quantitatively small and statistically insignificant (column 6 of Panel B, Table 1). Though most earlier studies using mass layoffs focus exclusively on men (e.g., see Bloemen et al., 2018; Browning and Heinesen, 2012; Sullivan and Wachter, 2009), the lack of impacts on female workers are in line with earlier evidence for Austria and Sweden – see Eliason and Storrie (2009); Kuhn et al. (2009).

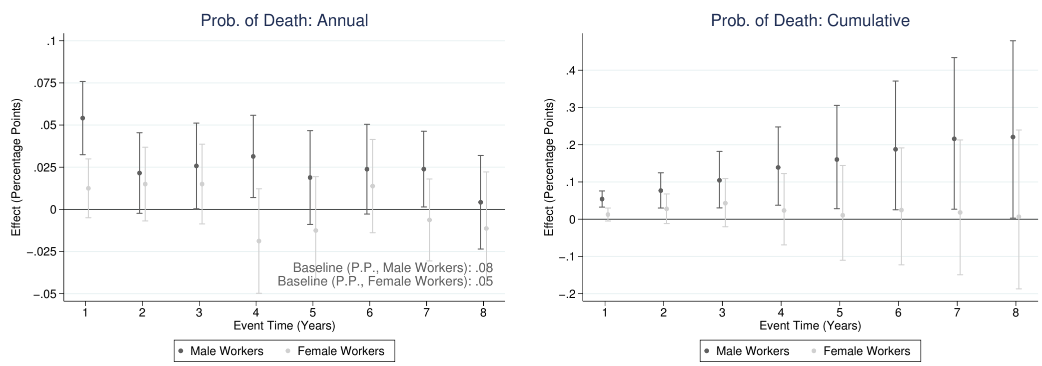
3.3 Effects on Mortality

We proceed by studying the effects of job loss on mortality. Since we cannot rely on pre-post differences in our main design, our specification compares mortality rates across displaced workers and their matched control group – using the specification adapted from equation (1) and discussed in Section 3.1.

[32]We do *not* attach a causal interpretation to these elasticities as this would require that layoffs affect hospitalization only through either one of these variables. This is likely not the case, as the effects could arise through different mechanisms other than employment and earnings – see Section 3.7 for a discussion on mechanisms.

Figure 2 shows the effect on yearly and cumulative mortality rates – left and right graphs, respectively.[33] Despite some indication of higher mortality rates in the first years after layoff, the effects on female mortality are not statistically significant and the accumulated effect on mortality is close to zero a few years after the layoff. Thus, both the analyses on female hospitalization and mortality do not indicate meaningful impacts on women's health.

Figure 2: Dynamic Effects of Job Loss on Mortality



Notes: This figure shows the dynamic treatment effects of job loss due to a mass layoff on the probability of death. The left-side graph displays annual effects and the right-side graph displays cumulative effects. Outcomes are shown separately for both male (dark gray) and female workers (light gray). Estimates were computed using the matching-based equation adapted from equation (1). Each sample includes a treatment group of workers displaced in mass layoffs and a matched control group of workers not displaced in the same year, working in firms that did not experience mass layoffs. 95% confidence intervals are also reported.

On the other hand, two striking patterns emerge for displaced men. First, male mortality risk increases by 0.054 p.p. in the first year after layoff, equivalent to a 54% increase relative to the baseline risk in the control group.[34] The effect strongly diminishes from the second year on and slowly vanishes over time. In line with the results for hospitalization, external causes explain a sizable part of the effect on male mortality – as much as 62% of the effect. The probability of deaths due to external causes increases by .0149 p.p., while

[33] In line with Deryugina and Molitor (2019), we define the change in cumulative mortality ΔM_t at each year after job loss as $\Delta M_t = \prod_{t=0}^{T}(1 - m_t + \beta_t) - \prod_{t=0}^{T}(1 - m_t)$ where β_t are the annual mortality effects of job loss and m_t is the empirical fraction of the laid-off workers who die at year t.

[34] We set the baseline using the average mortality rate in the control group during the respective post-treatment period. We cannot follow the procedure used for other outcomes based on pre-treatment periods because, by construction, all units are alive prior to the layoff.

deaths due to non-external reasons increase by .009 p.p – see columns 2-3 of Panel A, Appendix Table B1.

The large increase in male deaths and hospitalization by external causes suggests an increase in risk behavior by workers following job loss and also a link with the violent Brazilian context. Conversely, an increase in deaths by non-external causes suggests that these may come as consequences from specific medical conditions that are likely associated with unemployment – higher stress and/or anxiety being examples of possible mediators. We explore these alternative possibilities in what follows.

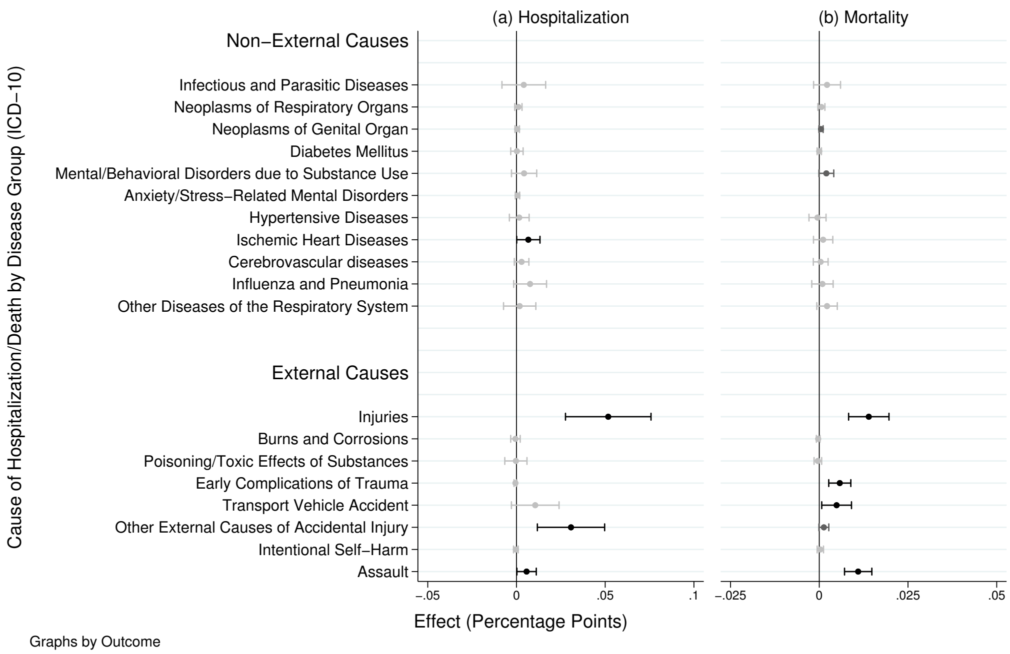
3.4 Effects by Specific Causes

We now estimate the effects of job loss on hospitalization and mortality by different causes according to the ICD-10 – see Figure 3. For the remainder of the paper we restrict our attention to male workers, since we do not find significant effects of job loss on women's health outcomes (Sections 3.2-3.3).

Our findings are twofold. First, we find positive and statistically significant effects for male workers on some disease groups within non-external causes. We estimate a .0067 p.p. increase in the incidence of hospitalizations due to ischemic heart diseases, which increase by 113% relative to the baseline. We also find mild evidence of increased mortality from mental/behavioral disorders related to substance use (p-value = 0.061). Both these factors might be related to higher stress and anxiety following job loss. In addition, we find some evidence of increases in neoplasms of the genital organ (p-value = 0.095). The latter might signal higher engagement in unhealthy behaviors such as smoking, which has been associated with several types of cancer.

Second, and most noticeably, we find robust evidence of large increases in male hospitalization and mortality due to external causes. Hospitalization and mortality due to injuries increase by 64% and 26%, respectively. ICD codes also offer insight into the underlying causes behind these effects. The same incident may be associated with several ICD codes which are informative on the objective health issue (e.g., trauma on the head, or a burn) and the underlying reason of the incident (e.g., transport vehicle accidents, or an assault).

Figure 3: Average Effects of Job Loss, by Diagnosis Groups (Male Workers)



Notes: This figure shows the estimated effects (and confidence intervals) of job loss on public-sector hospitalizations for different diagnoses (Panel A) and on mortality for different causes of death (Panel B), as defined in the International Classification of Diseases (ICD-10). 95% confidence intervals are reported. All estimates and confidence intervals are computed using the sample for male workers. Estimates indicated in black are statistically significant at the 5% level, while those indicated in dark gray are statistically significant at the 10% level. Estimates in light gray are statistically insignificant. Estimates for hospitalizations are computed with the difference-in-differences equation (2) and estimates for mortality are computed with the matching-based equation adapted from equation (2).

The results in Figure 3 reveal that they are driven by accidents and interpersonal violence, while effects on intentional self-harm are small and statistically insignificant. Hospitalization and mortality due to transport vehicle accidents increase by 49% (p-value 0.12) and 30%, while other causes of accidental injury increase by 60% and 76%. In turn, job loss leads to 172% higher hospitalization and 34% higher mortality due to assaults. Overall, these results indicate that higher engagement in risk behavior (e.g., heavy driving leading to accidental injuries, or reckless driving) and exposure to interpersonal violence play a role in explaining our findings.[35]

[35]The increase in assaults could also be related to the decision to commit a crime. The latter would be in line with earlier evidence for Brazil showing increases in criminal activity

3.5 Family Spillovers

One important direction for understanding the health costs of job loss is assessing its potential spillovers on the household. Clearly, family members are also exposed to the financial constraints and potential stress caused by job loss, and may also be affected by the associated losses in private HI coverage. In order to estimate these effects, we identify the spouses and children of workers in our sample using depend claims data provided by the Brazilian tax authority for the 2006-2019 period and the Cadunico welfare register for the 2011-2020 period.[36] Since many of these links are based on records for children who are alive in the post-treatment period, we focus on hospitalization outcomes rather than mortality ones.

In Table 2, we estimate the impacts on the children of workers displaced in mass layoffs, aged between 1 and 18 at the time of the layoff. We distinguish between the impacts on the first year after layoff, which were shown to be stronger for displaced workers, and the subsequent periods. We find large increases in hospitalization due to external causes for children in the first year after father's job loss (Panel A, column 2), which increase by 186% relative to the baseline – see Appendix Figure B3 for the yearly dynamic effects. The same probability also increases by 86% in subsequent periods, but this effect is not statistically significant. The same table shows that these effects cannot be explained by variation in private HI coverage, which is not significantly affected (Panel A, column 1). In turn, estimates on hospitalization for non-external causes are small and statistically insignificant (Panel A, column 3). Similarly, the effects on children's health are statistically insignificant following mother's job loss (Panel B, column 2-3). Overall, these results show that parental job loss leads to worse health outcomes by children following fathers' but not mothers' displacement. They line up well with our main results showing that

by displaced workers in Brazil (Britto et al., 2022).

[36] Depend claims data mainly cover individuals in the upper side of the income distribution who file taxes. In turn, Cadunico mainly covers low- and middle-income individuals who are targeted by federal welfare programs. The combination of the two results in a reasonably balanced dataset on family links for individuals in different parts of the income distribution.

male workers' health is strongly affected by job loss, while effects on women are muted.

In Appendix B.6, we use the same data to link couples and study impacts on spouses. Overall, we do not find much evidence of effects on spouses' health or private HI coverage.

Table 2: Average Effects of Job Loss on Workers' Children

	(1)	(2)	(3)
	HI	Hospitalization	
	Enrollment	Ext. Causes	Non-Ext. Causes
	Panel A.2: Children of Male Workers		
Point Estimate ($t = 1$)	-0.4995	0.0727	0.0156
	(0.8028)	(0.0310)	(0.1136)
Point Estimate ($t > 1$)	0.1854	0.0337	0.0104
	(0.8108)	(0.0283)	(0.0744)
Baseline Mean (Treated, $t \leq 0$)	13.7889	.0389	.5374
Effect ($t = 1$) Relative to Baseline	-3%	186%	2%
Effect ($t > 1$) Relative to Baseline	1%	86%	1%
Observations	123,326	179,760	179,760
	Panel B.2: Children of Female Workers		
Point Estimate ($t = 1$)	-2.0516	-0.0231	0.0500
	(0.6814)	(0.0599)	(0.1339)
Point Estimate ($t > 1$)	-0.0634	-0.0192	0.0461
	(0.7350)	(0.0528)	(0.0968)
Baseline Mean (Treated, $t \leq 0$)	11.7969	.0577	.5765
Effect ($t = 1$) Relative to Baseline	-17%	-39%	8%
Effect ($t > 1$) Relative to Baseline	0%	-33%	8%
Observations	80,976	121,422	121,422

Notes: This table shows the effect of job loss due to a mass layoff on the probability of public hospitalization for children of dismissed workers. It includes estimates for health insurance enrolment (column 1) and hospitalization due to external and non-external causes (columns 2-3). Estimates were computed using the difference-in-differences equation (2). Dependent variables are indicated at the top of each column. The explanatory variable of interest is a dummy $Treat_i$ equal to 1 for treated workers, interacted with a dummy $Post_t$ equal to 1 for the period after displacement. The sample includes a treatment group of workers displaced in mass layoffs and a matched control group of workers not displaced in the same year, working in firms that did not experience mass layoffs. Standard errors clustered at the firm level are indicated in parentheses. All coefficients, standard errors, and baseline means representing probabilities have been scaled by 100, and effects are thus interpreted in terms of percentage points.

3.6 Heterogeneity Analysis

Our next exercise is investigating heterogeneous treatment effects after splitting the sample over quartiles of selected individual covariates.[37] Overall, the

[37]Thresholds are defined based on observable characteristics from workers in the treatment group, who are then assigned to a given quartile together with their respective pairs in the control group. Our matching strategy, described in Section 3.1, ensures that treatment

results show that the effects of job loss on hospitalization and mortality are fairly pervasive over key individual characteristics – see Figure 4 showing average effects for different groups. The effects do not vary strongly over age, tenure, education and (pre-displacement) earnings.[38] However, one notable exception is college educated workers, with 14 or more years of education, who represent a small share of the treatment group (7%). For them, the effects on hospitalizations are relatively small in magnitude and statistically insignificant, while the effects on mortality turn out to be negative. In Appendix Figure B2, we show that percentage losses in labor income do not vary much over the same characteristics, including education.[39] Therefore, despite experiencing comparable income losses to other workers, highly educated individuals appear to handle job loss more effectively both financially and emotionally, and may even experience health benefits.

Another interesting pattern is that effects on mortality seem to decrease with tenure, being statistically insignificant for workers in the upper tenure quartile. The latter group has higher liquidity at displacement, since the value of mandatory severance payment increases with tenure (see Section 1.2). Although these heterogeneous effects could be explained by other differences between high- and low-tenure workers, these results suggest that (a lack of) income liquidity may be a mechanism linking job loss and mortality.

3.7 Discussion on Mechanisms

Job loss may negatively affect men's health through several, non-exclusive mechanisms, which we group into three broad categories. First, job loss may lead to worse health through its large adverse impacts on labor income. Financial constraints may lead to stress and anxiety, which in turn are associated

---

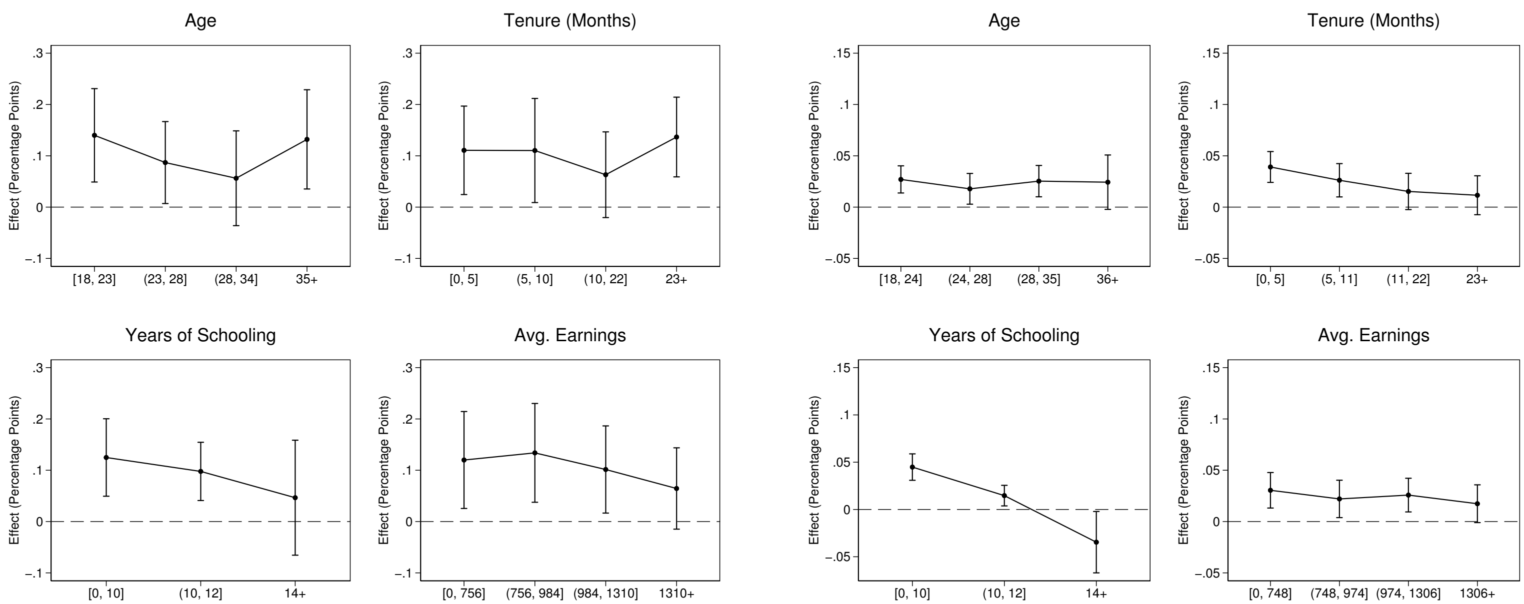
and control groups will remain similar in the characteristics used to perform the matching, regardless of the way that our sample is partitioned.

[38] Effects on hospitalization are more pronounced in the lowest and highest age quartiles, mirroring the pattern in Appendix Figure A.1, but they do not differ statistically from other age groups.

[39] When performing such comparisons, we consider income losses relative to previous income. This offers a more reasonable comparison across workers with different baseline incomes, since absolute dollar losses may have very different implications for low- and high-income workers.

Figure 4: Average Effects of Job Loss, by Individual Demographic Quartiles (Male Workers)

(a) Hospitalization **(b) Mortality**



Notes: This figure shows the estimated effects (and confidence intervals) of job loss on public-sector hospitalizations (Panel A) and mortality (Panel B) for different quartiles of each indicated individual characteristic. 95% confidence intervals are reported. All estimates and confidence intervals are computed using the sample for male workers. Estimates for hospitalizations are computed with the difference-in-differences equation (2) and estimates for mortality are computed with the matching-based equation adapted from equation (2).

with several diseases and could also trigger engagement in risky behavior – e.g., smoking, drinking, driving under the influence, and so on. In addition, the lack of income may result in lower health care investments or lead to less healthy consumption patterns (e.g., eating less healthy food). Second, independently of the effects on income, job loss is arguably a stressful event *per se*, leading to uncertainty, anxiety, and loss of self-esteem, which could then result in adverse health effects. This would also be in line with evidence that employment has a psychological value that goes beyond wages (Hussam et al., 2022).[40] Third, job loss has a significant impact on how workers allocate their time. Following job loss, they could spend more time in activities that are relatively more detrimental to their health.

While we cannot pinpoint each of these nuanced factors, our analyses provide insights into the role of these three broad categories of mechanisms. A particularly pertinent question is the extent to which adverse health effects are influenced by an income mechanism. This inquiry can guide whether policies should prioritize financial assistance to mitigate the health costs of job loss or explore alternative strategies for greater efficacy.

Our evidence in Section 3.6 offers some indication that income losses brought by job loss may play a role in explaining the adverse effects on men's health. First, we showed that the effects of job loss on men's health are fairly pervasive over various individual characteristics, lining up with consistent income losses over those same dimensions. Second, high-tenure workers experience lower impacts on mortality (though not on hospitalization). Since these workers receive large sums of money in the form of severance payments and have higher access to unemployment benefits, this indicates that income at displacement might matter. On the other hand, we find muted impacts for the 7% of men who have a college degree in our sample, although they experience similar income losses as other workers. This suggests that income losses due to job displacement may not be the only mechanism driving the impacts on men's

[40] Both the first and second mechanisms are related to stress and anxiety. They are consistent with the evidence presented in Section 3.6 showing increases in hospitalization and mortality causes associated with such factors.

health.[41]

To gain further insight into the role of income, we compare workers who are predicted to experience varying levels of labor income reductions following job loss.[42] We predicted such losses based on a rich set of pre-determined characteristics, such as job location, occupation, and education.[43] Figure 5 plots the effects of job loss by groups of workers predicted to experience different levels of income losses. The left graph shows that labor income decreases by as much as 47% for workers in the top quartile of predicted losses, while workers in the bottom quartile experience on average 25% lower income. In turn, the center and right graphs show that hospitalizations for non-external and external causes do not vary strongly over the same dimension – indicating that income is unlikely to be the only mechanism at play. However, the results on hospitalization for external causes show stronger effects for workers in the top quartile of predicted income losses – the effect for them is 72% larger than for those in the bottom quartile (0.46 p.p. vs. 0.79 p.p.). This suggests that income factors might play a stronger role in explaining the effects of job loss on externally-driven events.

Overall, we interpret these evidence as indicative that income losses might not be the only driver of the effects found on men's health outcomes, and that such mechanism may play a more important role in explaining the effects on health outcomes driven by external causes. In Section 4, we will study the impacts of access to unemployment benefits and will provide further evidence on the role of income.

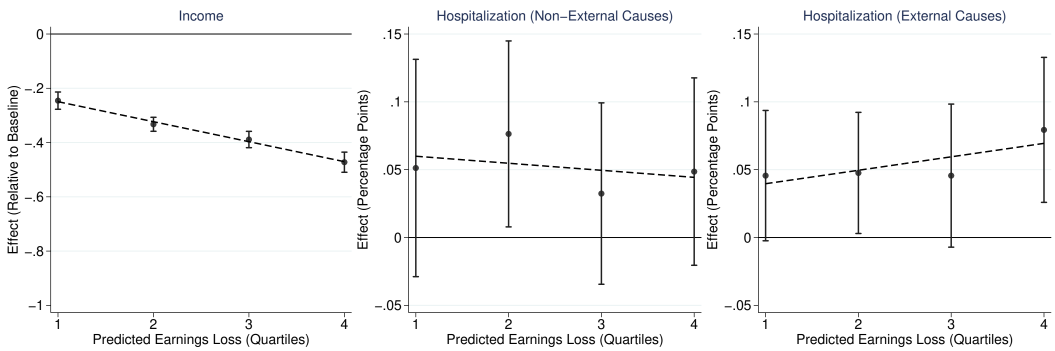
Finally, we also consider the possibility that our main results could be

[41]In turn, our findings that job loss by fathers leads to higher hospitalization by children also offer some insight into mechanisms (Section 3.5). They indicate that time substitution following job loss is unlikely to be the only driver of the effects, since children's time allocation is not directly affected by parental layoff.

[42]This exercise largely follows Hilger (2016) who investigate the impacts of parental job loss on college enrollment in the US.

[43]Specifically, we estimate the effects of job loss on labor income for each (male) worker in our sample relative to his matched counterpart based on (2). Then, we regress these estimates on a rich set of pre-determined characteristics (income, tenure, age dummies, schooling, gender, occupation, and municipality-industry fixed effects) and predict labor income losses for different groups of workers.

Figure 5: Predicted income losses and the effects of job loss on hospitalization



Notes: The figure shows the effect of job loss on labor income (left graph) and public hospitalization for non-external (center graph) and external causes (right graph), after splitting the sample by predicted income losses, as estimated from equation (2), along with 95% confidence intervals. Predicted income losses are computed after regressing individual job loss effects on labor income on a set of characteristics: income, tenure, age dummies, schooling, gender, occupation, and municipality-industry fixed effects. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same year. The effects on income for each group are re-scaled by the mean outcome in the treatment group in the pre-treatment period. Standard errors are clustered at the firm level.

explained by workers relocating to the informal sector. Such jobs could be riskier or more stressful, which could potentially explain the worse health conditions after (a formal) layoff. However, the evidence provided in Appendix B.2 shows that informal jobs compensate only a small part of the employment losses. More importantly, in Appendix B.7, we show that our effects are similar across workers with very different levels of exposure to labor informality. Hence, we do not find much support for the idea that the take-up of informal jobs is a main driver of our findings.

3.8 Robustness Analyses

We perform a series of robustness exercises to address several potential concerns related to our main empirical results. First, as we measure hospitalization by hospital admissions covered by the public health care system, increases in hospitalization after job loss could be potentially driven by substitution from the private to the public system. However, in Appendix B.8 we show that our estimates change little when excluding workers without access to private HI before job loss, thus indicating that private-public substitution does not drive our main finding on hospitalization. This is not surprising to the extent that private HI covers only a relatively small fraction of the population. In addition, using a mediation analysis, we provide evidence that only about 8% of the effects on hospitalization can be explained by lower private HI coverage caused by job loss in our main sample.

Second, we address concerns related to selection into treatment, even within mass layoffs. In Appendix B.9, we show that our main results do not change much when progressively focusing on mass layoffs where a larger share (or number) of workers are displaced, up to the case of plant closures. This mitigates concerns that our results could be driven by firms selectively choosing to displace workers with worsening health conditions. This analysis also addresses concerns that the effects of mass layoffs could largely differ from the effects of regular layoffs due to spillover effects – e.g., across displaced coworkers, or in the local areas where mass layoffs take place. In this case, we would expect to find much stronger impacts when using stricter mass layoff definitions. Instead, we find that our main results do not change much when varying the intensity of mass layoffs, which supports the external validity of our analysis.

Third, we develop a novel intent-to-treat (ITT) analysis that allows us to inspect pre-trends when analyzing mortality outcomes. Instead of taking workers displaced in mass layoffs as the treatment group, we consider as treated all workers employed in mass layoff firms two years before the event takes place. The control group is built by matching such treated workers to similar workers in non-mass layoff firms, following our baseline approach. As a result, we are able to compare the evolution of mortality rates for workers in firms that will experience mass layoff in the future, relative to other similar workers. In Appendix B.10, we show that ITT estimates lead to similar implied elasticities of mortality to employment and labor income, supporting our main findings. Reassuringly, we find that mortality trends before treatment do not differ across workers in mass layoff firms and similar control workers in non-mass layoff firms. Moreover, the ITT approach also addresses selection issues due to firm discretion in firing decisions, since it considers all workers employed in such firms two years before the layoffs as treated. Hence, it provides yet another piece of evidence that our findings are not driven by selection in displacements.

Fourth, in Appendix B.11 we show that our findings are robust to using estimators proposed in the recent methodological literature on difference-in-differences with staggered treatment timing. Finally, in Appendix B.12 we provide evidence on the validity of our data linkage procedure for our various health outcomes. In particular, we replicate the same procedure for linking employment outcomes in our main analysis as though we lacked unique person identifiers in the employment data. We show that the results obtained are extremely similar to the ones obtained by linking employment outcomes based on unique person identifiers.

4 Attenuating Effects of Unemployment Insurance

In the previous section, we documented that job loss leads to higher hospitalization and mortality for men, both from external and non-external causes. Now, we explore whether UI transfers provide any attenuation to these adverse effects. As shown in Section 3.2, job loss leads to strong and persistent income losses. Studying the effects of UI transfers can be informative on the role of financial constraints as a mechanism driving the effects of job loss on health. In addition, this analysis will shed light on the effectiveness of transfer policies in mitigating the health toll of job loss.

4.1 Research Design

Unemployment insurance in Brazil is a federal program providing income support to displaced workers in the formal sector. Eligible workers are entitled to 3-5 months of benefits replacing on average 80% of their pre-displacement earnings. To be eligible, workers must have been continuously employed in the last 6 months prior to layoff. Moreover, for repeated claimants, there must be at least 16 months separating the worker's (current) layoff date and the previous layoff used to claim UI benefits in the past. We leverage this last rule to identify the effect of UI eligibility using a regression discontinuity (RD) design. Specifically, we compare barely eligible and ineligible workers due to the 16-month rule using the following equation:

$$Y_{it} = \alpha + \beta D_i + f(X_i) + \epsilon_{it} \tag{3}$$

where Y_{it} is the outcome of interest and X_i, the running variable, is the difference between the most recent layoff date and the previous layoff date used to claim UI, normalized such that $X = 0$ at the 16-month eligibility cutoff. In addition, $f(.)$ is a flexible polynomial spline of the running variable, D_i is a dummy indicating that the worker satisfies the 16-month rule (i.e., $D = 1(X_i \geq 0)$), and ϵ_{it} is the error term. β is the coefficient of interest identifying the intention-to-treat effect of UI eligibility.

Our baseline estimates are based on a local linear model with a narrow bandwidth of 60 days at both sides of the cutoff. We test the robustness of this specification with several sensitivity checks using different polynomial orders and bandwidth choices (including the optimal range proposed by Calonico et al., 2014), and with permutation tests, which compare our mains estimates with a range of placebo effects at different cutoff points.

4.2 Sample Selection and Balance Tests

This analysis is based on workers displaced for a second time during 2006-2014 around 16 months after a prior layoff, with those above the 16-month mark becoming entitled to 3-5 months of unemployment benefits. We restrict our attention to workers displaced with at least six months of tenure, so that the 16-month eligibility rule is binding. We focus on male workers (who experience worse health outcomes due to job loss – see Section 3.1) in the 18-65 age range leaving open-ended contracts in the private sector. Since layoffs follow monthly cycles and are more likely to take place near the turn of each month, we drop from our sample workers whose 16-month cutoff date is within 3 days from the start or the end of the month.[44] This prevents our RD cutoff from coinciding with such dismissal cycles, which generate mild discontinuities in the density of layoffs near the turn of any calendar month – see Appendix Figure C3.

Appendix Figures C4 and C5 show that the running variable's density func-

[44]This restriction is based on the initial layoff date giving rise to the initial UI claim that determines the RD cutoff date. This date is not endogenous to the variation in the subsequent layoff date used in RD the analysis, defining UI eligibility according to the 16-month rule.

tion and a rich set of pre-determined worker characteristics (including tenure, earnings, educational level, age, and employment at different industry sectors) are balanced around the cutoff in our final working sample. Together, these results support the identifying assumption of our RD design that treatment assignment is as good as random near the cutoff.

4.3 Results

Appendix Table C1 presents the effects of UI eligibility on program take-up and labor market outcomes, based on equation (3). It shows that workers barely meeting the 16-month eligibility cutoff are 58 p.p. more likely to receive unemployment benefits, corresponding to a R$ 1,776 increase in benefit amount (columns 1-2). In line with extensive earlier literature, the same table shows that UI eligibility reduces labor supply. Eligible individuals work .66 months less and earn R$ 741 less in the first year after layoff (about 15% less relative to the mean for both measures). Appendix Figure C1 presents the graphical evidence showing clearly visible discontinuities for total benefit amount and months worked around the cutoff.[45]

Table 3 presents our main results for the impact of UI eligibility on health-related outcomes during the first year after layoff – the period when employment losses from job loss are largest and when UI benefits are paid. We also present the results by age, as the effects of income on health could largely differ between younger and older workers. In particular, younger individuals could be more likely to use the additional income to engage in unhealthy behavior, e.g., smoking and alcohol consumption. First, we find that UI eligibility does not affect the probability of private HI enrollment (column 1). Point estimates on this measure are statistically insignificant and quantitatively small (also relative to the baseline). Hence, we do not find evidence that displaced workers invest UI benefits directly in health care.

Second, we turn to the impacts on hospitalization. We find evidence that UI eligibility reduces the risk of hospitalization from external causes by .1

[45] A small share of workers to the left of the cutoff are able to collect residual benefits from their previous UI claim – e.g., workers who were entitled to five months but who have only collected four months before finding a new job.

Table 3: Local Average Effects of UI Eligibility on HI Enrollment, Hospitalization, and Mortality

	(1)	(2)	(3)	(4)	(5)
	Prob. of HI	Prob. of Hospitalization		Prob. of Mortality	
	Enrollment	Ext. Causes	Non-Ext. Causes	Ext. Causes	Non-Ext. Causes
	Panel A: All Workers				
Point Estimate	-0.1487	-0.0077	0.0128	-0.0209	0.0035
	(0.1711)	(0.0242)	(0.0339)	(0.0126)	(0.0094)
Baseline Mean (at Cutoff)	7.1251	.1975	.4134	.0864	.0478
Effect Relative to the Mean	-2%	-3%	3%	-24%	7%
Observations	360,185	558,810	558,810	819,198	819,198
	Panel B: Older Workers ($\geq$ 35 Years Old)				
Point Estimate	0.1920	-0.1094	-0.0007	-0.0184	0.0065
	(0.2688)	(0.0365)	(0.0639)	(0.0177)	(0.0182)
Baseline Mean (at Cutoff)	6.2957	.1816	.545	.0751	.0796
Effect Relative to the Mean	3%	-60%	0%	-24%	8%
Observations	130,691	201,538	201,538	390,706	390,706
	Panel C: Younger Workers ($<$ 35 Years Old)				
Point Estimate	-0.3536	0.0494	0.0220	-0.0233	0.0013
	(0.2207)	(0.0318)	(0.0389)	(0.0178)	(0.0071)
Mean Outcome (at Cutoff)	7.5943	.2064	.3397	.0966	.0191
Effect Relative to the Mean	-4%	23%	6%	-24%	6%
Observations	229,494	357,272	357,272	428,492	428,492

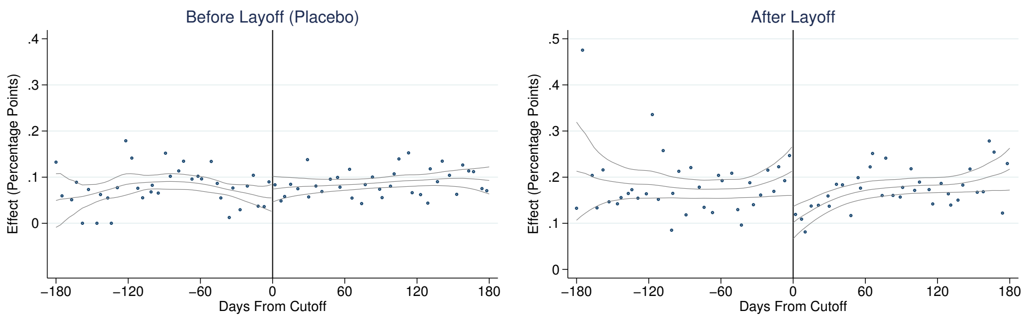
Notes: The columns in this table show the effect of UI eligibility on the probability of enrollment in private health insurance plans (column 1), and on the probability of public hospitalization (columns 2 and 3) and death (columns 4 and 5), the latter two divided between external and non-external causes. Each probability is calculated considering a window of one year after layoff. The sample includes displaced male workers with at least 6 months of continuous employment prior to layoff who are displaced within a symmetric bandwidth of 60 days around the cutoff required for eligibility to unemployment benefits – namely, 16 months since the previous layoff resulting in UI claims. The local linear regression includes a dummy capturing eligibility for UI benefits (i.e., the main variable of interest), time since the cutoff date for eligibility, and a term for the interaction between the two. Standard errors clustered at the worker level are indicated in parentheses. All coefficients, standard errors, and baseline means representing probabilities have been scaled by 100, and effects are thus to be interpreted as percentage points.

p.p. for older workers, equivalent to a substantial 60% reduction relative to the mean (column 2, Panel B). Figure 6 presents the graphical evidence for this measure (right panel). In addition, we include a placebo analysis showing that the same measure is balanced prior to displacement (left panel), offering compelling evidence in favor of a causal interpretation for this result. This effect is also robust to a range of different functional forms and specifications, and to permutation tests – see Appendix Table C2 and Figure C6.[46] In turn,

[46]The permutation tests compare our main estimates for these outcomes with the distribution of estimates at placebo cutoff points, unrelated to changes in UI eligibility.

the same estimates for younger workers are not statistically significant (column 2, Panel C). Similarly, impacts on hospitalization from non-external causes are small in magnitude and not statistically significant (column 3).

Figure 6: Local Average Effects of UI Eligibility on Hospitalization (External Causes) for Older Male Workers



Note: The graphs plot the averages around the eligibility cutoff for the probability of public hospitalization due to external causes up to one year before and after layoff for older workers, with 35 years old or more. The sample includes displaced male workers with at least 6 months of continuous employment prior to layoff. Dots represent averages based on 5-day bins. The lines are based on a local linear polynomial smoothing with a 60-day bandwidth with 95% confidence intervals.

Finally, we analyze the impacts on mortality. We find some evidence that UI eligibility may lead to lower mortality due to external causes. Eligibility reduces mortality due to external causes by .02 p.p., a 24% drop relative to the baseline, though the effect is only significant at the 10% level (column 4, Panel A). Point estimates in the sample of older and younger workers both indicate a 24% reduction in externally driven mortality but are not statistically significant. In line with the results for hospitalization outcomes, the effects on mortality due to non-external causes are also small in magnitude and not statistically significant.

Overall, these results indicate that access to UI benefits mitigates some of the adverse effects of job loss on hospitalization and mortality due to external causes. In particular, we find a large and robust 60% reduction in hospitalization from external causes for older workers. They also offer some suggestive evidence that mortality due to external factors may decrease by as much as 24% for both younger and older workers, though these results should be interpreted with caution as estimates are not very precise. Lastly, we do not find evidence of effects on private HI enrollment, hospitalization from non-external causes, or mortality from non-external causes.

4.4 Discussion on mechanisms

In principle, access to UI benefits may affect health through various mechanisms that could either be beneficial or detrimental to health. First, individuals might allocate their additional income to healthcare or health-promoting activities – e.g., doing sports or going to the gym. The fact that we find no impacts on private HI enrollment or adverse health events unrelated to external causes does not lend support to these explanations. Second, UI transfers could be spent on unhealthy activities such as drinking and smoking. Additionally, UI benefits might encourage engagement in risky activities, such as driving under the influence or exposure to violence in bars or nightlife. Our findings do not align with these explanations; instead, they indicate a reduction in adverse health events related to external causes.

Finally, UI benefits may alleviate financial constraints, which, in turn, could lead to lower stress and lower engagement in risky activities. This is consistent with our findings that UI eligibility reduces the probability of health events driven by external factors. This interpretation is further supported by the fact that UI in Brazil closely resembles a pure income transfer, free from conditionalities such as job search requirements, participation in training programs, or mandatory meetings with caseworkers. The impact of UI transfers is more pronounced for older workers, for whom an income mechanism is likely more relevant, as they potentially bear greater financial responsibility in the household (e.g., breadwinner status, or childcare costs). Relatedly, UI transfers alleviate financial constraints, allowing workers to take longer periods of time to find new jobs. It also potentially mitigates the stress associated with job search, reinforcing this mechanism. Appendix Figure C2 shows the dynamic effects of UI eligibility on labor supply for several semesters after job loss. The impact is concentrated in the first year after layoff, coinciding with the time window chosen in Section 4.3 to estimate improvements in health outcomes.

More generally, our finding that UI reduces health incidents driven by external factors is in line with our previous results on the effects of job loss. In particular, we show that the effects of job loss on hospitalizations from external causes are stronger for workers who are predicted to experience large income losses from job loss (Section 3.7, Figure 5). Overall, these results support the idea that the income mechanism plays a role in explaining the impacts of job loss on health incidents due external causes. They also suggest that impacts on non-external causes are more likely to be driven by mechanisms other than income – such as the direct impacts of job loss on stress and anxiety.

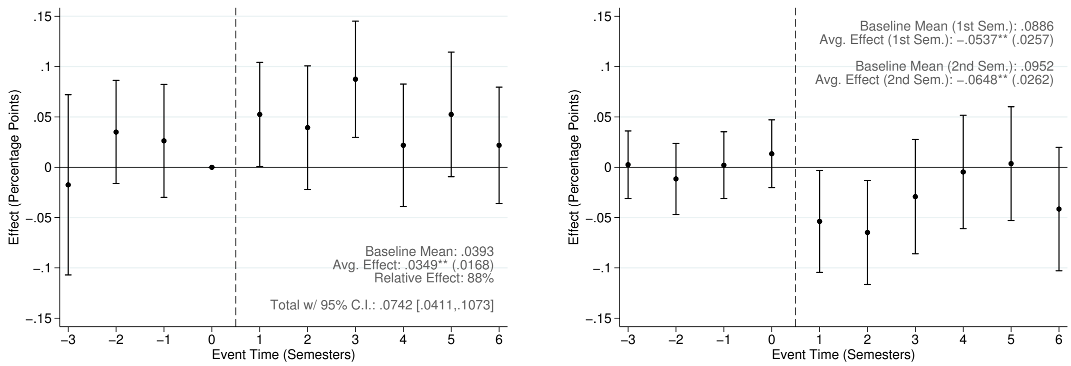
Independently of the role of each potential mechanism, our results show that UI policies may be effective at mitigating part of the adverse impacts of job loss on men's health. For older workers, the strong reductions in hospitalization from external causes are comparable in magnitude with the increase in that same measure caused by job loss. In Figure 7, we provide a comparison between the two effects at several semesters before and after the layoff. To improve comparability, we estimate the effects of job loss for older male workers using a more similar sample to the UI analysis, restricting attention to workers with at least 6 months of tenure at displacement (left graph). The right panel shows the effects of UI eligibility based on our main RD analysis. Although confidence intervals are somewhat large, point estimates suggest that UI eligibility roughly offsets the entire adverse effect of job loss on hospitalization due to external causes during the first two semesters after layoff. This aligns with the period in which benefits are paid out and is consistent with our previous findings that UI also influences labor supply during the first year after layoff.

5 Conclusion

We construct a novel dataset that combines detailed, individual-level information on employment spells for the universe of Brazilian workers with their hospitalization and mortality records across a 17-year time span. Using these data, we conduct a comprehensive causal analysis of the health impacts of job loss in the context of a developing economy with mixed (i.e., public and private) systems of healthcare provision. We document that job loss leads to

Figure 7: Comparison of Job Loss and UI Effects on Hospitalization (External Causes) for Older Male Workers ($\geq$ 35 years old)

(a) Job Loss (Dismissed vs. Non-Dismissed) **(b) UI (RD Estimates, Dismissed Only)**



Notes: Panel (a) shows the dynamic treatment effects of job loss due to a mass layoff on public hospitalization due to external causes for older male workers ($\geq$ 35 years old). Estimates were computed using the difference-in-differences equation (1), with event times measured in 6-moths periods. Panel (b) shows the RD estimates of UI eligibility on the probability of public hospitalization due to external causes for older male workers dismissed from a job. Estimated probabilities are restricted to the specific time window (semester) indicated in the horizontal axis (event time), which indicate time relative to dismissal analogously to the description in section 3.1. 95% confidence intervals are displayed.

large and persistent negative effects on health outcomes and that such costs are entirely concentrated on male workers. Specifically, for this group, we find an average 30% increase in the probability of hospitalization and a 23% increase in mortality risk. These effects are not concentrated in specific groups of workers. Rather, they are fairly pervasive over the distribution of income, age, tenure and education. The effects on hospitalization and mortality are driven both by external and non-external causes, which similarly contribute to the overall effects. The increase in non-external causes is mainly associated with conditions that can be linked to stress, such as ischemic heart diseases. In turn, the increase in external causes can be attributed to incidents involving accidents and violence.

Our analysis underscores the importance of designing policies that offer assistance to displaced workers. In this regard, we provide novel evidence that unemployment benefits can effectively mitigate some of the adverse health effects of job loss. In particular, UI transfers seem particularly effective in reducing health risks associated with external causes for older workers. Moreover, these mitigating effects are concentrated in the first two semesters following the layoff. These results indicate that transfer policies following job displacement may be an effective tool for addressing the health consequences of job loss. However, our findings also highlight the necessity of complementary policy tools, considering that UI transfers are not effective across all groups of workers and all types of health issues.

References

ANS, "Caderno de Informação da Saúde Suplementar," *Technical Report*, 2019.

Athey, S. and G. W. Imbens, "Design-based analysis in difference-in-differences settings with staggered adoption," *Journal of Econometrics*, 2022, *226* (1), 62–79.

Azevedo, P. F., S. F. Almeida, N. C. Ito, V. Boarati, C. R. Moron, W. Inhasz, and F. Rousset, "A cadeia de saúde suplementar no Brasil: avaliação de falhas de mercado e propostas de políticas," *Technical Report*, 2016.

Bertheau, Antoine, Edoardo Maria Acabbi, Cristina Barceló, Andreas Gulyas, Stefano Lombardi, and Raffaele Saggio, "The unequal consequences of job loss across countries," *American Economic Review: Insights*, 2023, *5* (3), 393–408.

Bhalotra, Sonia, Rudi Rocha, Rodrigo R Soares et al., "Can universalization of health work? evidence from health systems restructuring and expansion in brazil," *IZA DP*, 2019, (12111).

Black, Sandra E, Paul J Devereux, and Kjell G Salvanes, "Losing heart? The effect of job displacement on health," *ILR Review*, 2015, *68* (4), 833–861.

Bloemen, H., S. Hochguertel, and J. Zweerink, "Job loss, firm-level heterogeneity and mortality: Evidence from administrative data," *Journal of Health Economics*, 2018, *59*, 78–90.

Breivik, A.-L. and A. Costa-Ramón, "The career costs of children's health shocks," *Working Paper*, 2022.

Britto, D. G. C., P. Pinotti, and B. Sampaio, "The effect of job loss and unemployment insurance on crime in Brazil," *Econometrica*, 2022.

Browning, M. and E. Heinesen, "Effect of job loss due to plant closure on mortality and hospitalization," *Journal of Health Economics*, 2012, *31* (4), 599–616.

Callaway, B. and P. H. Sant'Anna, "Difference-in-differences with multiple time periods," *Journal of Econometrics*, 2021, *225* (2), 200–230.

Calonico, S., M. D. Cattaneo, and R. Titiunik, "Robust nonparametric confidence intervals for regression-discontinuity designs," *Econometrica*, 2014, *82* (6), 2295–2326.

Cattaneo, M. D., M. Jansson, and X. Ma, "Manipulation testing based on density discontinuity," *The Stata Journal*, 2018, *18* (1), 234–261.

_ , _ , and _ , "Simple local polynomial density estimators," *Journal of the American Statistical Association*, 2020, *115* (531), 1449–1455.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer, "The effect of minimum wages on low-wage jobs," *The Quarterly Journal of Economics*, 2019, *134* (3), 1405–1454.

Chaisemartin, C. De and X. d'Haultfoeuille, "Two-way fixed effects estimators with heterogeneous treatment effects.," *American Economic Review*, 2020, *110* (9), 2964–96.

Cohen, Jacob, *Statistical power analysis for the behavioral sciences*, Routledge, 2013.

Couch, K. and D. W. Placzek, "Earnings Losses of Displaced Workers Revisited," *American Economic Review*, 2010, *100* (1), 572–89.

Dattani, Saloni, Fiona Spooner, Hannah Ritchie, and Max Roser, "Causes of Death," *Our World in Data*, 2023. https://ourworldindata.org/causes-of-death.

Deryugina, T. and D. Molitor, "Does When You Die Depend on Where You Live? Evidence from Hurricane Katrina," *American Economic Review*, 2019, *110* (11), 3602–33.

Dube, Arindrajit, Daniele Girardi, Oscar Jorda, and Alan M Taylor, "A local projections approach to difference-in-differences event studies," Technical Report, National Bureau of Economic Research 2023.

Eliason, M. and D. Storrie, "Job loss is bad for your health–Swedish evidence on cause-specific hospitalization following involuntary job loss," *Social Science Medicine*, 2009, *68* (8), 1396–1406.

Fontes, L. F., M. Mrejen, B. Rache, and R. Rocha, "Economic distress and children's mental health: evidence from the Brazilian high risk cohort study for mental conditions," *Working Paper*, 2022.

Gelbach, J. B., "When do covariates matter? And which ones, and how much?," *Journal of Labor Economics*, 2016, *34* (2), 509–543.

Giatti, Luana, Sandhi Maria Barreto, and Cibele Comini César, "Household context and self-rated health: the effect of unemployment and informal work," *Journal of Epidemiology & Community Health*, 2008.

Goodman-Bacon, A., "Difference-in-differences with variation in treatment timing," *Journal of Econometrics*, 2021, *225* (2), 254–277.

Hilger, Nathaniel G, "Parental job loss and children's long-term outcomes: Evidence from 7 million fathers' layoffs," *American Economic Journal: Applied Economics*, 2016, *8* (3), 247–283.

Hussam, Reshmaan, Erin M Kelley, Gregory Lane, and Fatima Zahra, "The psychosocial value of employment: Evidence from a refugee camp," *American Economic Review*, 2022, *112* (11), 3694–3724.

Imai, K. and I. S. Kim, "On the use of two-way fixed effects regression models for causal inference with panel data," *Political Analysis*, 2021, *29* (3), 405–415.

Jacobson, L. S., J. R. LaLonde, and D. G. Sullivan, "Earnings Losses of Displaced Workers," *American Economic Review*, 1993, pp. 685–709.

Khanna, Gaurav, Carlos Medina, Anant Nyshadham, Christian Posso, and Jorge Tamayo, "Job loss, credit, and crime in Colombia," *American Economic Review: Insights*, 2021, *3* (1), 97–114.

Kuhn, A., R. Lalive, and J. Zweimüller, "The public health costs of job loss," *Journal of Health Economics*, 2009, *28* (6), 1099–1115.

Kuka, E., "Quantifying the benefits of social insurance: unemployment insurance and health," *Review of Economics and Statistics*, 2020, *102* (3), 490–505.

Paul, K. I. and K. Moser, "Unemployment impairs mental health: Meta-analyses," *Journal of Vocational Behavior*, 2009, *74* (3), 264–282.

Picchio, Matteo and Michele Ubaldi, "Unemployment and health: A meta-analysis," *Journal of Economic Surveys*, 2022.

Rege, Mari, Kjetil Telle, and Mark Votruba, "Parental job loss and children's school performance," *The Review of Economic Studies*, 2011, *78* (4), 1462–1489.

Ruhm, C. J., "Are recessions good for your health?," *The Quarterly Journal of Economics*, 2000, *115* (2), 617–650.

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

Schaller, J. and A. H. Stevens, "Why are the unemployed in worse health? The causal effect of unemployment on health," *Labour Economics*, 2011, *18* (1), 71–78.

_ and _ , "Short-run effects of job loss on health conditions, health insurance, and health care utilization," *Journal of Health Economics*, 2015, *43*, 190–203.

Sorrenti, G., U. Zölitz, D. Ribeaud, and M. Eisner, "The causal impact of socio-emotional skills training on educational success," *Working Paper*, 2020.

Sullivan, D. and V. Wachter, "Job Displacement and Mortality: An Analysis using Administrative Data," *Quarterly Journal of Economics*, 2009, *124* (3), 1265–1306.

Sun, L. and S. Abraham, "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects," *Journal of Econometrics*, 2021, *2* (225), 175–199.

Tesouro Nacional, "Aspectos Fiscais do Seguro-Desemprego e do Abono Salarial," *Technical Report*, 2019.

Ulyssea, G., "Firms, informality, and development: Theory and evidence from Brazil," *American Economic Review*, 2018, *108* (8), 2015–47.

Online Appendix to “Job Loss, Unemployment Insurance, and Health: Evidence from Brazil”

For Online Publication

Guilherme Amorim, Diogo G. C. Britto, Alexandre Fonseca, and Breno Sampaio

January 23, 2024

A Appendix to Section 2 **1**
A.1 Descriptive Graphs 1

B Appendix to Section 3 **3**
B.1 Summary Statistics 3
B.2 Differences in Formal and Informal Labor Earnings 3
B.3 Average Effects on Mortality 6
B.4 Heterogeneous Effects on Labor Income 7
B.5 Dynamic Effects on Workers’ Children 8
B.6 Spillover Effects on Workers’ Spouses 8
B.7 Heterogeneity of Local Labor Informality 10
B.8 Mediation Analysis of Private Health Insurance 11
B.9 Tests of Selection into Treatment 15
B.10 Mortality Estimates using an ITT Approach 16
B.11 Methodological Concerns with Staggered Treatment Timings . 18
B.12 Differences Between Data Merging Methods 19

C Appendix to Section 4 **21**
C.1 Additional Results on the Effects of UI 21
C.2 Additional Robustness Tests 23

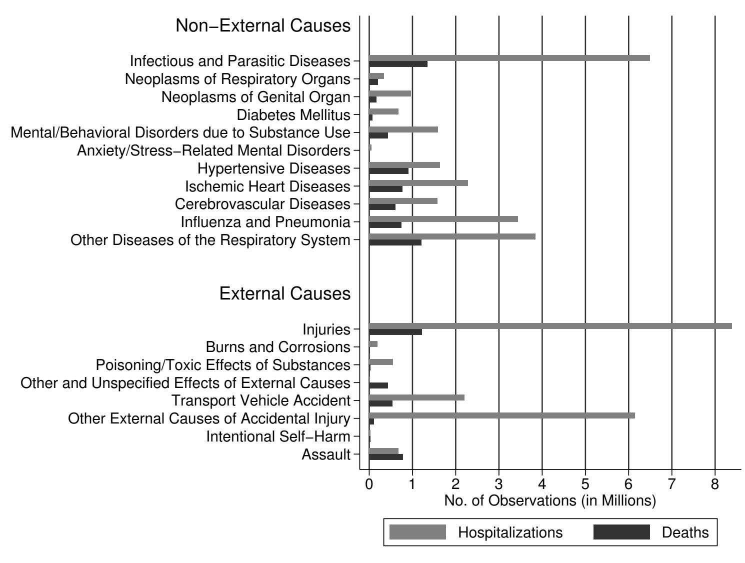
A Appendix to Section 2

A.1 Descriptive Graphs

Figure A1: Summary Statistics on Employment, Hospitalization and Mortality

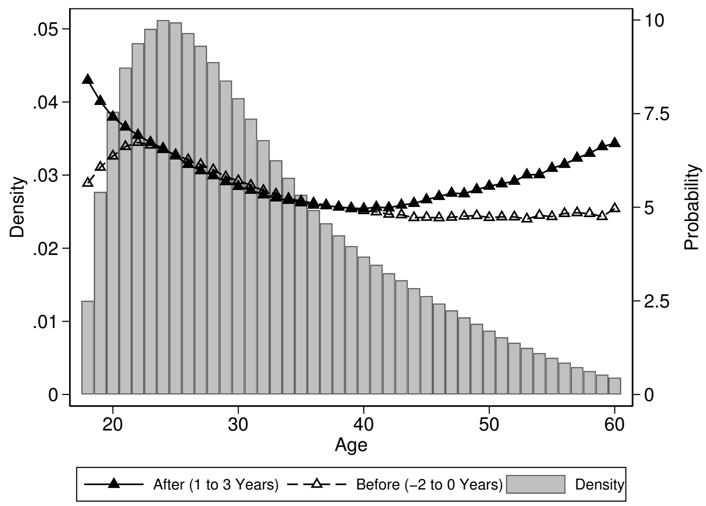
(a) Employment and Mortality Rates by Year

(b) Hospitalization and Deaths by Cause



Notes: Panel (a) shows the evolution of the adult mortality and employment rates for the Brazilian population between 1997 and 2018. Panel (b) shows the total number of hospitalizations in the public health system and deaths for adults aged 18 to 65 years old in Brazil between 2002 and 2018. These are decomposed by the leading causes according to the ICD-10 classification.

Figure A2: Probability of Hospitalization for Displaced Workers



Notes: This figure shows the probabilities of hospitalization for different age groups in the years between 2006 and 2014. The sample includes both male and female, full-time workers in the non-agricultural, private sector. The gray bars represent the proportion of workers in each age group. The triangles display the cumulative probabilities of admission to public hospitals for displaced workers. Black triangles are for the three years following dismissal and hollow triangles are for the two years before dismissal.

B Appendix to Section 3

B.1 Summary Statistics

Table A1: Summary Statistics, Treated vs. Non-Treated Observations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	No Restrictions			Unique Zip Code/Gend./D.o.B.			Unique Borough/Gend./D.o.B.		
	Treated	Non-Treated	Std. Diff.	Treated	Non-Treated	Std. Diff.	Treated	Non-Treated	Std. Diff.
Individual Characteristics									
Age	30.22	30.22	0.00	29.86	29.86	0.00	29.45	29.45	0.00
Tenure (Months)	16.85	16.86	-0.00	16.54	16.52	0.00	15.99	15.97	0.00
Educational Level (Years)	10.85	10.88	-0.01	10.92	10.99	-0.03	10.86	10.87	-0.01
Income	1,046.72	1,037.78	0.01	1,046.31	1,037.50	0.01	1,024.09	1,015.22	0.02
Municipality Characteristics									
Population	3,526,534	3,590,280	-0.01	3,791,669	3,859,904	-0.01	3,482,639	3,523,825	-0.01
GDP	32.51	32.92	-0.02	33.31	33.63	-0.02	32.99	33.09	-0.01
Gini Index	0.65	0.65	0.00	0.66	0.66	0.00	0.65	0.65	0.01
Informality Rate	0.34	0.34	0.03	0.33	0.33	0.02	0.33	0.33	0.03
Homicide Rate	21.03	21.42	-0.03	20.15	20.53	-0.03	18.60	19.06	-0.04
Firm Characteristics									
Mean Age	33.99	34.06	-0.02	33.94	33.96	-0.00	33.83	33.90	-0.02
Mean Tenure (Months)	33.10	29.22	0.23	32.95	29.05	0.23	32.85	29.00	0.23
Mean Educational Level	10.82	10.88	-0.03	10.88	10.96	-0.05	10.82	10.86	-0.02
Mean Income	1,361.78	1,379.23	-0.02	1,376.74	1,396.20	-0.02	1,360.42	1,376.96	-0.02
Firm Size	836.35	997.84	-0.07	901.99	1,068.75	-0.07	941.72	974.70	-0.01
Layoff Rate ($t = -1$)	0.17	0.17	-0.13	0.16	0.17	-0.13	0.16	0.17	-0.13
Layoff Rate ($t = -2$)	0.16	0.16	-0.06	0.16	0.16	-0.05	0.16	0.16	-0.06
Layoff Rate ($t = -3$)	0.15	0.16	-0.08	0.15	0.16	-0.09	0.15	0.16	-0.09

Notes: This table reports the average characteristics of treated (i.e. displaced in mass layoffs) and non-treated workers, together with the standardized difference between the two groups, for each working sample used in the main analysis. These are, respectively, a non-restricted sample (columns 1 to 3); a sample of workers who are uniquely identified in each zip code/gender/date-of-birth cluster (columns 4 to 6); and a sample of workers who are uniquely identified in each borough/gender/date-of-birth cluster (columns 7 to 9).

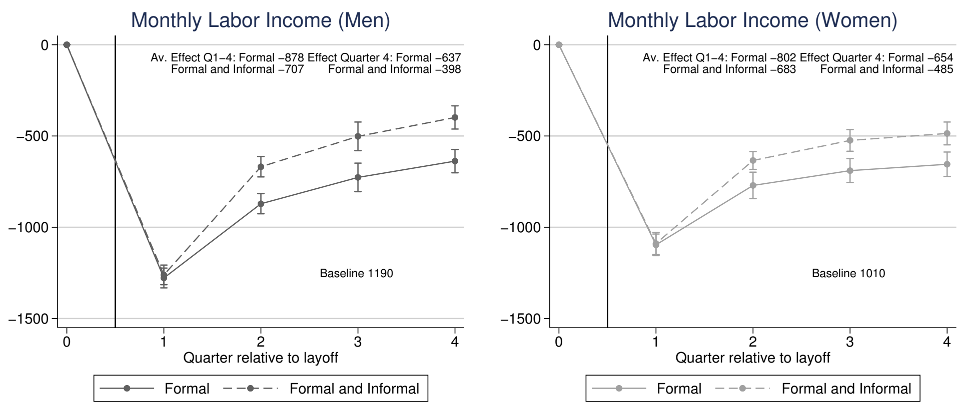
B.2 Differences in Formal and Informal Labor Earnings

In our main analysis of job loss in Section 3, we leverage mass layoffs in the formal labor market to estimate the effect of job loss on hospitalization and mortality. However, the high levels of labor informality in Brazil (see Section 1) imply that the estimated drop in employment could, in reality, be smaller, insofar as displaced workers can migrate to jobs in the informal market. In order to evaluate to what extent this may impact our main estimates, we use survey-based data from two different sources containing information on individuals' participation in both formal and informal labor markets. The first dataset is PNAD (*Pesquisa Nacional por Amostra de Domicílios*), a nationally representative survey conducted yearly by the Brazilian Institute of Geography and Statistics (*Instituto Brasileiro de Geografia e Estatística* – IBGE) to construct many of the official socioeconomic indicators published by the federal government (including the ones on labor informality). Although it does not contain individual identifiers, since 2012 the survey has included a longitudinal component that tracks a substantial portion of the interviewed households for five consecutive quarters. The second dataset is the *Cadastro Único* welfare register (Cadunico). It contains detailed information on individuals targeted by federal welfare programs, covering about two-thirds of the Brazilian population. Cadunico is thus representative of the low- and middle-income strata of the population – i.e., of those individuals who are more likely to transition to the informal labor market following job loss.

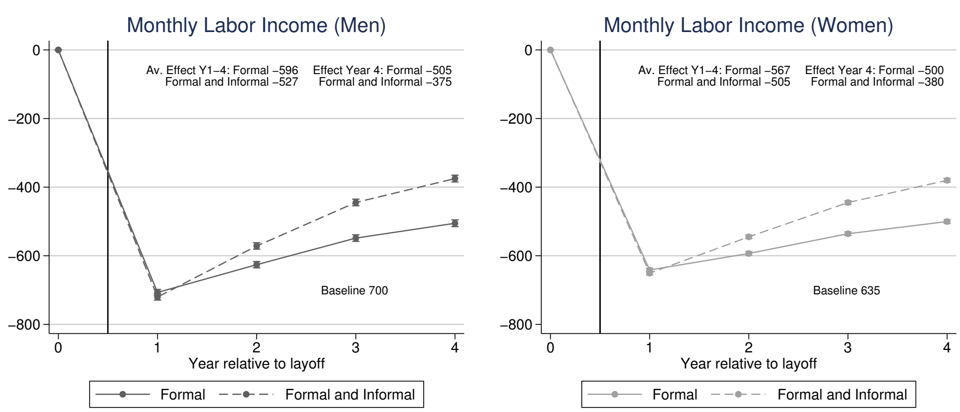
Using PNAD, we implement a different-in-differences design where the treatment group comprises individuals who are initially formally employed in interview quarter 1 and who were displaced in quarter 2, whereas the control group comprises workers employed in both quarters. We replicate the same procedure using Cadunico but at the yearly level. To keep the analyses as close as possible to our main analysis, we follow similar sample restrictions, focusing on private sector workers in the age range 18-65 years old. Differently from our main analysis, these data do not allow us to leverage mass layoffs. However, it offers insight into the role played by informal jobs in the employment recovery following job loss. Figure B1, Panel (a), shows that average formal labor income decreases by 878 and 637 BRL for men and women in the first year after layoff, respectively (equivalent to an 80% and 85% drop relative to the baseline, respectively). In turn, labor income losses reduce to 707 BRL and 603 BRL for men and women once we consider both formal and informal jobs. Hence, informal earnings close 15-20% of the labor income gap following job displacement. Figure B1, Panel (b), shows results based on Cadunico data, which offers better coverage for low- and middle-income workers. In line with the previous results, considering informal earnings reduces the labor income gap by roughly 11% for men or women. Overall, we conclude that employment losses due to job loss remain large, despite the availability of informal jobs which only partially mitigates the shock.

Figure B1: Effect of Job Loss on Formal and Informal Labor Market Outcomes

(a) PNAD Data



(b) Cadunico Data



Notes: This figure shows the effect of job loss on formal and informal labor income, along with 95% confidence intervals. Panel (a) is based on PNAD longitudinal household survey data following workers for up to five quarterly interviews. Panel (b) is based on CadUn registries of individuals claiming cash welfare benefits from the federal government at different years. The treatment group is defined as workers who are employed in the first period and out of employment in the second period; the control group is composed of workers who are employed in both the first and second periods. Earnings are measured in BRL. Baseline average values for the treated group at $t = 0$ are also reported.

B.3 Average Effects on Mortality

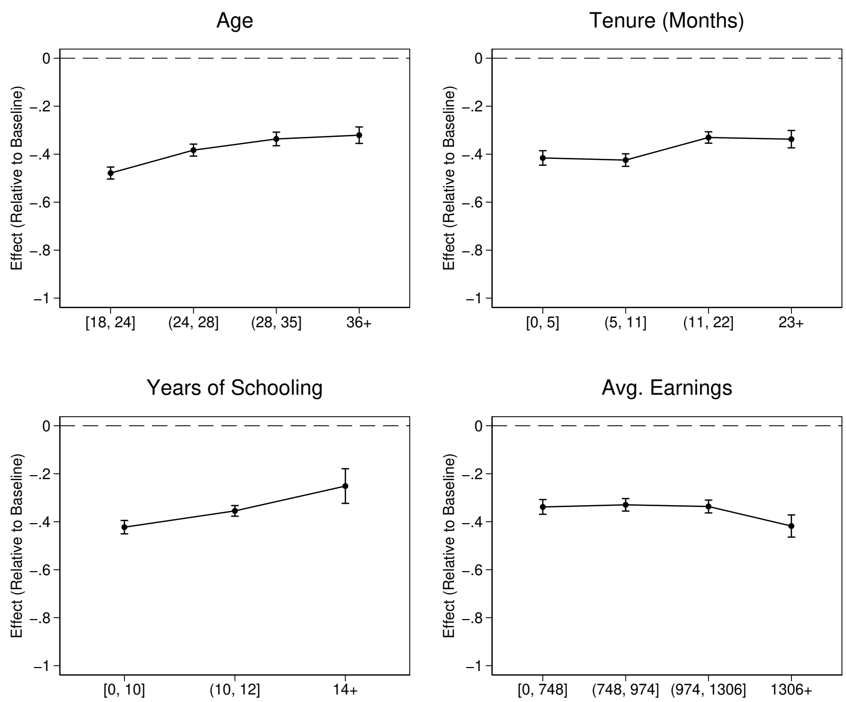
Table B1: Average Effect of Job Loss on Mortality

	(1)	(2)	(3)
	Prob. of Death		
	Overall	by Cause	
		External	Non-Ext.
	Panel A: Male Workers		
Point Estimate	0.0239	0.0149	0.0090
	(0.0046)	(0.0030)	(0.0033)
Baseline Mean (Untreated, $t > 0$)	.1027	.0659	.0368
Effect Relative to Baseline	23%	22%	24%
Implied Elasticity to Employment	-1.64	-1.57	-1.71
Implied Elasticity to Earnings	-0.82	-0.78	-0.85
Observations	2,574,349	2,574,349	2,574,349
	Panel B: Female Workers		
Point Estimate	-0.0036	-0.0014	-0.0022
	(0.0056)	(0.0023)	(0.0051)
Baseline Mean (Untreated, $t > 0$)	.0362	.0125	.0237
Effect Relative to Baseline	-9%	-11%	-9%
Implied Elasticity to Employment	0.56	0.68	0.56
Implied Elasticity to Earnings	0.31	0.37	0.31
Observations	1,435,819	1,435,819	1,435,819

Notes: This table shows the effect of job loss due to a mass layoff on the probability of death, both overall (column 1) and separately for each diagnosis group (columns 2-3). Estimates were computed using the matching-based equation adapted from equation (2). Dependent variables are indicated at the top of each column. The explanatory variable of interest is a dummy $Treat_i$ equal to 1 for treated workers. The sample includes a treatment group of workers displaced in mass layoffs and a matched control group of workers not displaced in the same year, working in firms that did not experience mass layoffs. Standard errors clustered at the firm level are indicated in parentheses. All coefficients, standard errors, and baseline means representing probabilities have been scaled by 100, and effects are thus interpreted in terms of percentage points.

B.4 Heterogeneous Effects on Labor Income

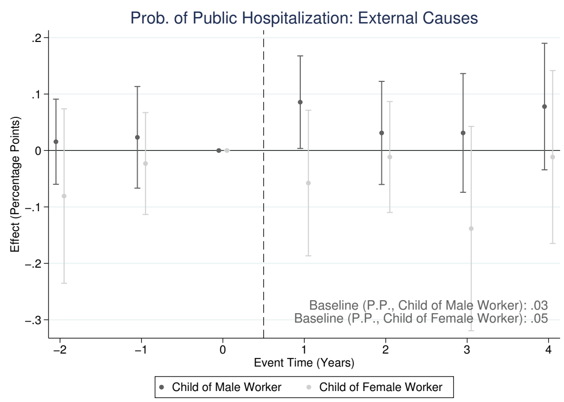
Figure B2: Average Effects of Job Loss on Labor Income, by Individual Demographic Quartiles (Male Workers)



Notes: This figure shows the estimated effects (and confidence intervals) of job loss on labor income for different quartiles of each indicated individual characteristic. 95% confidence intervals are reported. All estimates and confidence intervals are computed using the sample for male workers. Estimates are based on the difference-in-differences equation (2) and standard errors are clustered at the firm level.

B.5 Dynamic Effects on Workers' Children

Figure B3: Dynamic Effects of Job Loss on Workers' Children



Notes: This figure shows the dynamic treatment effects of parental job loss on children's hospitalization from external causes. Outcomes are shown separately for children of male workers (dark gray) and children of female workers (light gray). Baseline values are the outcome's average at pre-treatment years ($t \leq 0$) for each treatment group. Estimates were computed using the difference-in-differences equation (1). The sample includes children from a treatment group of workers displaced in mass layoffs and a matched control group of workers not displaced in the same year, working in firms that did not experience mass layoffs. 95% confidence intervals are also reported.

B.6 Spillover Effects on Workers' Spouses

In this section, we estimate the impact of job loss on labor and health outcomes for workers' spouses (or partners). We link couples using depend claims data provided by the Brazilian tax authority for the 2006-2019 period and the Cadunico welfare register for the 2011-2020 period. We focus on individuals who were recorded as spouses only to another single individual, and for whom this link was recorded prior to the worker's layoff. As in Section 3.5, average effects are distinguished between short-term effects (at $t = 1$) and long-term effects (at $t \geq 2$) – see Table B2 for results.

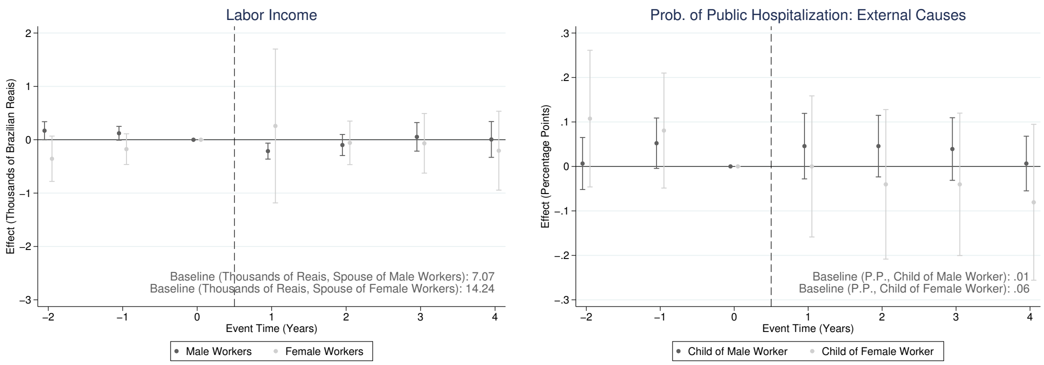
First, we do not find any economically meaningful effects on spousal labor supply, which vary by less than 4% relative to the baseline – columns 1-2, Table B2. Hence, added worker effects do not significantly compensate for the labor income losses following job loss. Second, effects on private health coverage and hospitalization are generally small and statistically insignificant – columns 3-5. The only exception is a statistically significant reduction in hospitalization due to external causes for spouses of female workers subsequently to the first year after layoff, which decreases by .11 p.p. However, a closer inspection of the dynamic estimates shown in Figure B4 reveals that this is most likely a spurious effect due to noisy estimates.

Table B2: Effects of Job Loss on Workers' Spouses

	(1)	(2)	(3)	(4)	(5)
	Labor Market Outcomes		HI	Hospitalization	
	Employment	Income	Enrollment	Ext. Causes	Non-Ext. Causes
	Panel A.1: Spouse of Male Workers				
Point Estimate ($t = 1$)	-1.0057	-310.3938	-0.8910	0.0261	-0.0935
	(0.5452)	(87.4108)	(0.4625)	(0.0299)	(0.0704)
Point Estimate ($t > 1$)	0.4454	-110.1135	-0.1968	0.0109	-0.0196
	(0.6457)	(130.5177)	(0.5028)	(0.0174)	(0.0574)
Baseline Mean (Treated, $t \leq 0$)	41.6577	6293.7154	10.2093	.0217	.3479
Effect ($t = 1$) Relative to Baseline	-2%	-4%	-8%	119%	-26%
Effect ($t > 1$) Relative to Baseline	1%	-1%	-1%	49%	-5%
Observations	235,578	235,578	130,410	214,648	214,648
	Panel B.2: Spouse of Female Workers				
Point Estimate ($t = 1$)	-0.4241	435.7254	-0.3944	-0.0627	-0.0403
	(0.6932)	(741.9176)	(0.5158)	(0.0736)	(0.1082)
Point Estimate ($t > 1$)	-0.2476	67.2127	-1.0618	-0.1165	-0.0358
	(0.6951)	(287.5815)	(0.6365)	(0.0533)	(0.0806)
Baseline Mean (Treated, $t = 0$)	62.7674	12909.5326	9.9052	.112	.3495
Effect ($t = 1$) Relative to Baseline	0%	3%	-3%	-55%	-11%
Effect ($t > 1$) Relative to Baseline	0%	0%	-10%	-103%	-10%
Observations	134,365	134,365	61,530	104,160	104,160

Notes: This table shows the effect of job loss due to a mass layoff on the probability of admission to a public hospital for spouses of dismissed workers. It includes estimates for labor market outcomes (columns 1-2) health insurance enrolment (column 3), and hospitalization due to external and non-external causes (columns 4-5). Estimates were computed using the difference-in-differences equation (2). Dependent variables are indicated at the top of each column. The explanatory variable of interest is a dummy $Treat_i$ equal to 1 for treated workers, interacted with a dummy $Post_t$ equal to 1 for the period after displacement. The sample includes a treatment group of workers displaced in mass layoffs and a matched control group of workers not displaced in the same year, working in firms that did not experience mass layoffs. Standard errors clustered at the firm level are indicated in parentheses. All coefficients, standard errors, and baseline means representing probabilities have been scaled by 100, and effects are thus interpreted in terms of percentage points.

Figure B4: Effect of Job Loss on Spouse's Outcomes



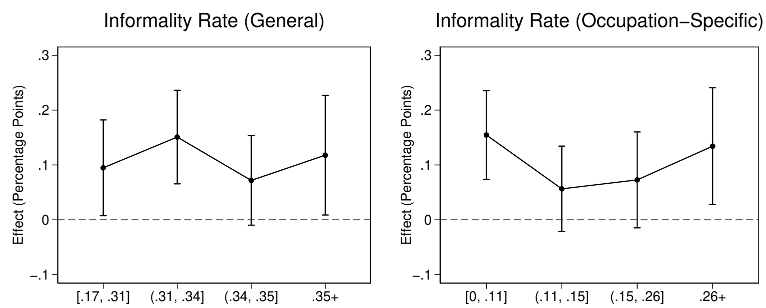
Notes: This figure shows the dynamic treatment effects of job loss on spouses' labor income hospitalization from external causes. Outcomes are shown separately for spouses of male workers (dark gray) and spouses of female workers (light gray). Baseline values are the outcome's average at pre-treatment years ($t \leq 0$) for each treatment group. Estimates were computed using the difference-in-differences equation (1). The sample includes spouses from a treatment group of workers displaced in mass layoffs and a matched control group of workers not displaced in the same year, working in firms that did not experience mass layoffs. 95% confidence intervals are also reported.

B.7 Heterogeneity of Local Labor Informality

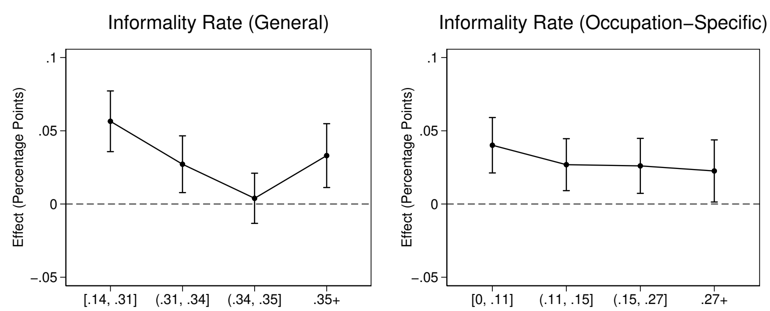
In this section, we investigate whether the take-up of informal jobs can explain the adverse effects of job loss on health outcomes – e.g., as informal jobs might be more risky or stressful. To do so, we compare job loss effects across workers with lower and higher exposure to labor informality – see Figure B5, Panels (a) and (b) for hospitalization and mortality outcomes, respectively. For each outcome, we split the sample across workers based on informality rates at the municipality level (left-side graphs) and at the (2-digit) occupation-municipality level (right-side graphs). Both informality measures are calculated using the 2010 Brazilian Population Census. Overall, results indicate that the adverse effects of job loss on hospitalization and mortality do not vary strongly over labor informality, and continue to hold for workers exposed to low levels of labor informality. Thus, these results do not support the idea that informal work plays a significant role in explaining our main findings.

Figure B5: Effect of Job Loss on Formal and Informal Labor Market Outcomes

(a) Hospitalization



(b) Mortality



Notes: This figure shows the estimated effects of job loss on public-sector hospitalizations (Panel A) and mortality (Panel B) for different quartiles of local-level informality. 95% confidence intervals are reported. All estimates and confidence intervals are computed using the sample for male workers. Estimates for hospitalizations are computed with the difference-in-differences equation (2) and estimates for mortality are computed with the matching-based equation adapted from equation (2).

B.8 Mediation Analysis of Private Health Insurance

The mixed character of public and private health care provision in Brazil raises the possibility that the effect of job loss on public hospital admissions is partially confounded by workers' substitution from private to public care. Around 15% of individuals in our sample have access to private HI before job loss, and we show that job loss reduces private HI coverage by 2 percentage points (Table 1, Section 3.1).

We re-estimate the effects of job loss on hospitalization after splitting the sample across workers with and without private HI coverage in the year before treatment – see Table B3. The table shows that the increase in hospitalization is stronger for male workers without access to private HI before job loss. This suggests that substitution from the private to the public system may indeed play some role in explaining our findings. However, the effects on hospitalization among male workers without HI at the baseline period are extremely similar to the effects in our main sample. The probability of hospitalization increases by .1365 p.p. (Table B3, Panel A, column 4-6) relative to .1049 in our main sample (Table 1, Panel A, column 6, in Section 3.1). Hence, although private-public substitution may play some role in explaining our findings, it cannot explain the effects on the largest portion of our main sample, composed of workers without previous private HI coverage. For these workers, we find similar impacts relative to our main analysis.

Table B3: Effect of Job Loss on Public Hospitalization for male workers, by HI Status

	(1)	(2)	(3)	(4)	(5)	(6)
	HI at $t = 0$			**No HI at $t = 0$**		
	Overall	by Cause		Overall	by Cause	
		External	Non-Ext.		External	Non-Ext.
Point Estimate	0.2752	0.1529	0.1070	0.1365	0.0599	0.0806
	(0.1077)	(0.0673)	(0.0876)	(0.0413)	(0.0227)	(0.0348)
Baseline Mean	.1733	.0612	.1223	.4135	.1212	.3029
Effect Relative to Baseline	158%	250%	87%	33%	49%	26%
Observations	45,780	45,780	45,780	531,524	531,524	531,524

Notes: This table shows the effect of job loss due to a mass layoff on public hospitalizations, both for individuals enrolled at a HI plan at the beginning of the layoff year (columns 1 to 3), and for those without a HI plan at the beginning of the layoff year (columns 4 to 6). Estimates were computed using the difference-in-differences equation (2). Dependent variables are indicated at the top of each column. The explanatory variable of interest is a dummy $Treat_i$ equal to 1 for treated workers, interacted with a dummy $Post_t$ equal to 1 for the period after displacement. All regressions include individual and year fixed effects. The sample includes a treatment group of workers displaced in mass layoffs and a matched control group of workers not displaced in the same year, working in firms that did not experience mass layoffs. Standard errors clustered at the firm level are indicated in parentheses. All coefficients, standard errors, and baseline means representing probabilities have been scaled by 100, and effects are thus interpreted in terms of percentage points.

We complement these results with a mediation analysis to quantify to what extent variations in private HI coverage can explain the impacts on hospitalization outcomes – for details on our implementation of the mediation analysis, see Breivik and Costa-Ramón (2022); Gelbach (2016); Sorrenti et al. (2020).[47,48] The indirect effect of job loss on public hospitalizations through the loss of (private) health insurance is first obtained by decomposing the unconditional treatment effects β_t, $t \in \{1, 2, 3, 4\}$ in equation (2) as follows:

$$\frac{dY_t}{d\left(Treat \cdot Time_t\right)} = \frac{\partial Y_t}{\partial HI_t} \cdot \frac{\partial HI_t}{\partial \left(Treat \cdot Time_t\right)} + R_t, \tag{4}$$

where Y_t is the outcome of interest (emergency public hospitalization), HI_t is a dummy for being enrolled in a health insurance plan at time t (the "mediator"), R_t is the unexplained fraction of the treatment's impact, and the remaining terms are defined as before. From the expression above, we estimate $\partial Y_t / \partial HI_t$ with equation (1) by adding the mediator term HI_t into its right-hand side:

$$Y_{it} = \alpha + \delta\, Treat_i + \sum_{t=-P}^{T} \beta_t^{HI_1}\, Treat_i \cdot Time_t + \sum_{t=-P}^{T} \lambda_t\, Time_t + \phi HI_{it} + \epsilon_{it}.$$

Then, as in section, we re-estimate the (total) effects of job loss on health insurance enrollment ($\partial HI_t / \partial \left(Treat \cdot Time_t\right)$) and on public hospitalization ($dY_t / d\left(Treat \cdot Time_t\right)$), also with equation (1):

$$HI_{it} = \alpha + \delta\, Treat_i + \sum_{t=-P}^{T} \beta_t^{HI_2}\, Treat_i \cdot Time_t + \sum_{t=-P}^{T} \lambda_t\, Time_t + \epsilon_{it},$$
$$Y_{it} = \alpha + \delta\, Treat_i + \sum_{t=-P}^{T} \beta_t\, Treat_i \cdot Time_t + \sum_{t=-P}^{T} \lambda_t\, Time_t + \epsilon_{it}.$$

[47] An ideal setting in such analysis would be one where we have a second source of exogenous variation in health insurance enrollment (to estimate the component ϕ in the expression that follows). Since we rely solely on variation that comes through the impact of job loss, the following results should be interpreted with a certain caution. We believe, nonetheless, that this exercise is informative about the relative magnitudes of the aforementioned direct and substitution effects.

[48] In what follows we focus on male workers only (as the hospitalization effects on female workers are statistically insignificant) and rely on the sample restricted by single observation in each date-of-birth/gender/district cluster (see Section 2).

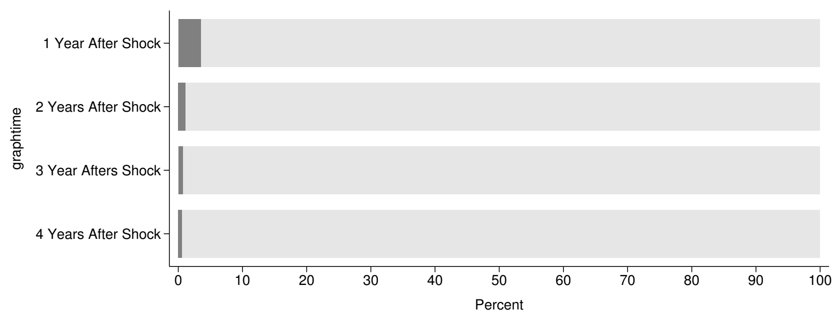
Finally, using expression (4) above, we calculate the relative contribution of HI_t to the impact of job loss at each subsequent period as the ratio $\frac{\phi \times \beta_t^{HI_2}}{\beta_t}$. The remaining unexplained part is analogously computed as $R_t = 1 - \frac{\phi \times \beta_t^{HI_2}}{\beta_t}$.

Results are displayed in Figure B6. In Panel (a) we report the percentages of the effects on public hospitalization that are explained by the impacts on health insurance, using the full sample of male workers. We find that only about 4% of the effect is possibly mediated by the concurrent impact on access to private health insurance, while in subsequent periods this share decreases to below 1%. Panel (b) reports the same percentages on a restricted sample of treated workers (together with their matched counterparts in the control group) who, at the time of layoff, were enrolled in a private health insurance plan. For this sub-sample the mediating impact of health insurance is much higher: about 51% in the first year, then falling to an average of 27% in subsequent years. In sum, these findings suggest that although substitution effects are relevant to the small share of workers who had access to private care prior to layoff,[49] they do not sufficiently explain the total impacts of job loss on public hospital admissions – which are thus more likely to reflect direct impacts of job loss on individuals' health.

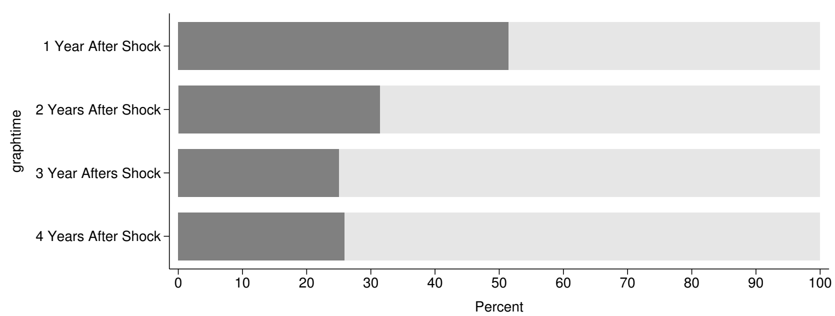
[49]This possibility also implicitly assumes that *all* effects on public hospital admissions mediated by changes in health insurance enrollment is due to individuals simply trading one type of care for the other. Such effects, of course, could also to some extent reflect an actual deterioration of their health due to the very fact that they lost access to private (and possibly higher-quality) care. Although this reinforces our argument that public hospitalizations more likely reflect direct impacts on individual health, we do not, however, explore this more nuanced mechanism.

Figure B6: Mediation Analysis of the Effect of Private Health Insurance on Public Hospitalization (Emergency), Male Workers

(a) Full Sample



(b) Health Insurance at $t = 0$



Notes: This figure shows the results of the mediation analysis of the total effect on public hospitalizations for male workers, as described in Section B.8. Results in Panel (a) are calculated using the full sample from the main analysis. Results in Panel (b) are calculated with a restricted sample of workers who were enrolled at a health insurance plan at time $t = 0$ (i.e., at the time of layoff). Dark gray bars show the ratio $\phi \times \beta_t^{HI_2}/\beta_t$ for each year following layoff. Light gray bars show the remaining values R_t.

B.9 Tests of Selection into Treatment

One potential concern with our identification strategy is that firms choose to displace workers with worsening health conditions, even within mass layoffs. We address these concerns by studying how the effects vary when using a more strict definition of mass layoffs, i.e., when a large share or number of workers are displaced at the same time. More strict definitions should reduce the space for such type of selection. Results are shown in Table B4. Although some estimates become more imprecise because sample size shrinks with more strict definitions, estimates do not strongly change across specifications. Overall, they do not support the idea that selection into treatment drives our results.

Table B4: Effects of Job Loss on Health Outcomes (Male Workers), Varying Mass Layoff Intensity

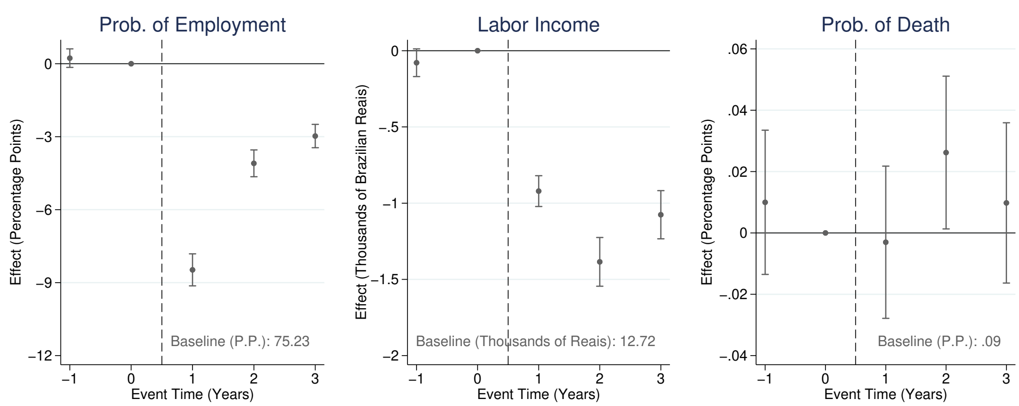
	(1)	(2)	(3)	(4)	(5)
	Panel A: HI Enrollment				
Point Estimate	-1.2739	-0.2807	-0.1862	-2.5530	-1.4510
	(0.5706)	(0.6725)	(0.8551)	(0.5335)	(0.7421)
Mass Layoff Sample	> 50%	> 66%	closure	> 100 workers	> 250 workers
Observations	252,224	153,762	92,120	376,922	236,222
	Panel B: Hospitalization				
Point Estimate	0.1338	0.1494	0.0419	0.1366	0.1077
	(0.0400)	(0.0527)	(0.0641)	(0.0325)	(0.0415)
Mass Layoff Sample	> 50%	> 66%	closure	> 100 workers	> 250 workers
Observations	513,604	306,978	183,722	766,080	484,232
	Panel C: Mortality				
Point Estimate	0.0294	0.0347	0.0271	0.0290	0.0355
	(0.0086)	(0.0108)	(0.0152)	(0.0074)	(0.0098)
Mass Layoff Sample	> 50%	> 66%	closure	> 100 workers	> 250 workers
Observation	735,920	438,722	259,517	1,086,921	678,285

Notes: This table shows the effect of job loss due to a mass layoff on emergency admissions to public hospitals. The sample is restricted to (1) mass layoffs of at least 33% of the workforce, (2) 50%, (3) plant closures, (4) at least 100 workers, and (5) at least 250 workers. HI and Hospitalization estimates were computed using the difference-in-differences equation (2) and mortality estimates using the matching-based equation adapted from equation (2). Dependent variables are indicated at the top of each column. The explanatory variable of interest is a dummy $Treat_i$ equal to 1 for treated workers, interacted with a dummy $Post_t$ equal to 1 for the period after displacement. The sample includes a treatment group of workers displaced in mass layoffs and a matched control group of workers not displaced in the same year, working in firms that did not experience mass layoffs. Standard errors clustered at the firm level are indicated in parentheses. All coefficients, standard errors, and baseline means representing probabilities have been scaled by 100, and effects are thus interpreted in terms of percentage points.

B.10 Mortality Estimates using an ITT Approach

We provide an intention-to-treat variation of our main design that allows us to test for differential pre-trends in mortality outcomes. Specifically, we set *all workers* employed in mass layoff firms two years before such events take place as the treatment group. Then, we build the control group via exact matching as in our main analysis. Figure B7 shows the dynamic effects based on equation (1). Relative to our main analysis, the effects on employment and labor income are smaller because not all workers in the treatment group are displaced during the mass layoffs. The mass layoffs take place during $t = 1$, the calendar year when mass layoffs unfold in treated firms. The impacts on labor income are largest in the subsequent year ($t = 2$), when labor income drops by 1.3 thousand BRL for men, a 10% reduction relative to the baseline. In the same year, we find a positive and statistically significant effect on mortality, which increases by .25 p.p., or 27% relative to the baseline. Importantly, the analysis does not show any differential pre-trends in mortality rates, supporting the causal interpretation of our finding that job loss leads to higher male mortality.

Figure B7: Dynamic ITT Effects of Job Loss on Labor Market Outcomes and Mortality

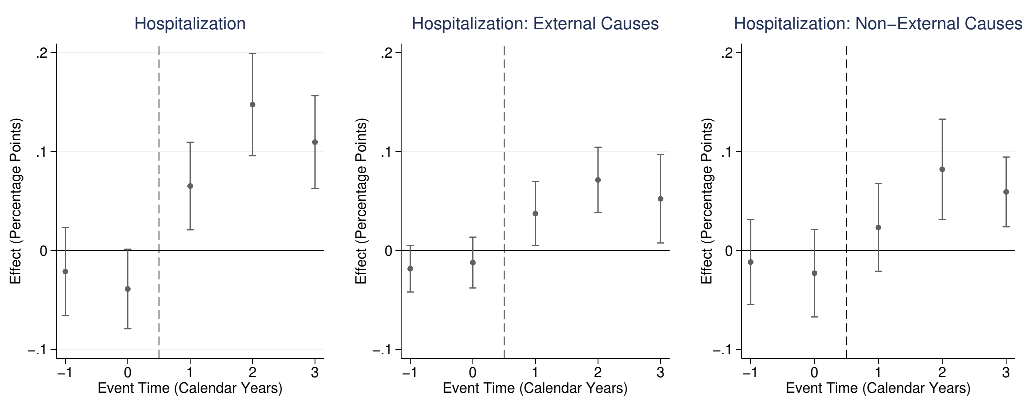


Notes: This figure shows the dynamic intention-to-treat (ITT) effects of job loss due to a mass layoff on formal employment, labor income, and mortality. Estimates were computed using the difference-in-differences equation (1). The sample includes a treatment group of workers employed at $t = 0$ in a firm that suffers a mass layoff at the end of that period, and a matched control group of workers employed at $t = 0$ in a firm that does not suffer a mass layoff in the period of analysis. The event time is measured in calendar years. 95% confidence intervals are also reported. Income variables are measured in BRL.

B.11 Methodological Concerns with Staggered Treatment Timings

Several recent studies have raised methodological concerns about using two-way fixed effects estimators in difference-in-differences designs with "staggered" treatment – that is, when treatment timing varies across observations – e.g., see De Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2021), Goodman-Bacon (2021), Imai and Kim (2021), Sun and Abraham (2021), and Athey and Imbens (2022). Our stacking approach re-centering time around the timing of treatment and using never-treated workers fully addresses such issues – see Dube et al. (2023) – and is in line with recent work (Britto et al., 2022; Cengiz et al., 2019). Nevertheless, we show that our results remain robust when using the estimator proposed by De Chaisemartin and d'Haultfoeuille (2020). This estimator follows a similar approach to ours to the extent that it selects not yet treated units for estimating each dynamic treatment effect. The results for our main hospitalization outcome are displayed in Figure B8. They indicate that hospitalization probabilities for men increase by roughly .1 p.p. – in line with our main estimates in Figure 1 (Section 3.2).

Figure B8: Effect of Job Loss on the Probability of Hospitalization, Alternative Estimators



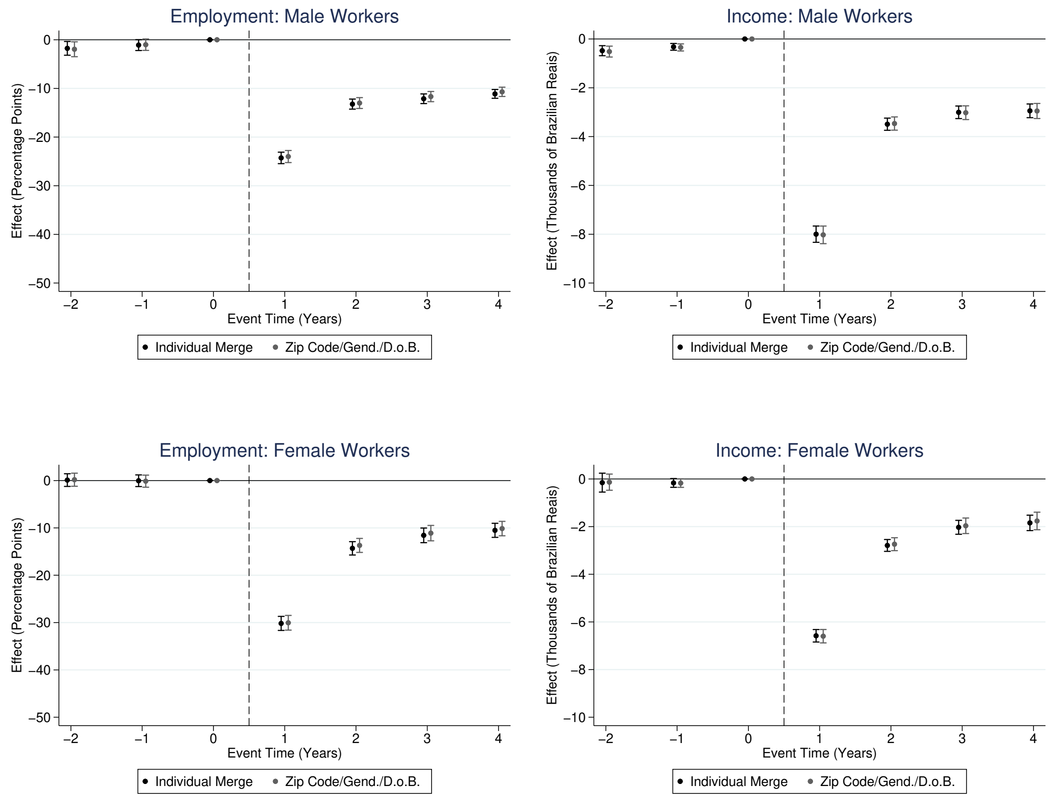
Notes: These graphs show two-way fixed effects (TWFE) panel estimates for the effects of job loss on hospitalization for male workers, with the correction proposed in De Chaisemartin and d'Haultfoeuille (2020). 95% confidence intervals are also reported.

B.12 Differences Between Data Merging Methods

We provide a validation exercise for the data linkage procedure used for building health outcomes. Namely, we identify individuals in different health datasets with their tax codes when they can be uniquely identified in the entire country based on a given set of characteristics – see Section 2.2 for the details. For robustness, we replicate the same procedure for creating employment outcomes in our main estimation sample, as if we could not link employment outcomes based on individual tax codes. We use the same linkage key used for linking hospitalization outcomes: postal code of residence, date of birth and gender.[50] Then, we compare these results with our main results on employment outcomes created with individual tax codes. The results show that our linkage procedure based on fine individual characteristics leads to extremely similar results relative to our baseline – see Figure B9. They support the idea that such a procedure leads only to classical measurement error in the dependent variable which should not bias the estimates. In addition, the comparison shows that such noise is likely very small, as confidence intervals remain small for the outcomes based on our linkage procedure.

[50] Although the employment data (RAIS) does not include postal code residence, we recover such information based on our person registry available for the entire population.

Figure B9: Dynamic Effects of Job Loss on Income, Different Data Merging Methods



Notes: This figure shows the dynamic treatment effects of job loss due to a mass layoff on formal employment and labor income for male (upper panel) and female workers (lower panel). Results are shown separately for outcomes merged at the individual level (dark gray) and by unique zip-code/gender/date-of-birth clusters (light gray). Estimates were computed using the difference-in-differences equation (1). Each sample includes a treatment group of workers displaced in mass layoffs and a matched control group of workers not displaced in the same year, working in firms that did not experience mass layoffs. 95% confidence intervals are also reported. Income variables are measured in BRL.

C Appendix to Section 4

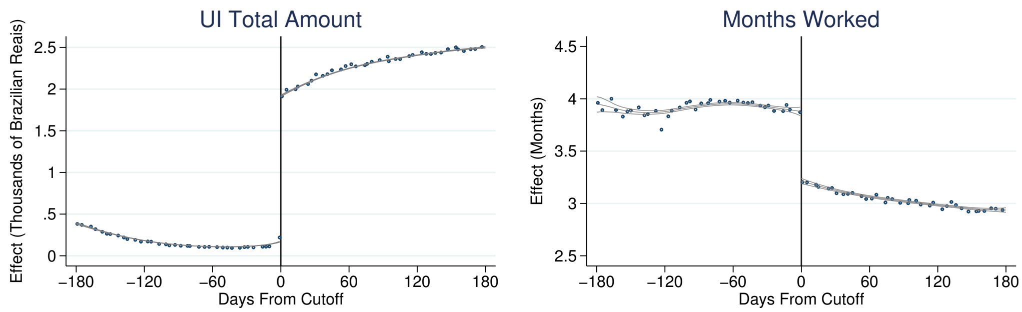
C.1 Additional Results on the Effects of UI

Table C1: Local Average Effects of UI Eligibility on UI Take-Up, Benefits Claimed, and Labor Market Outcomes

	(1)	(2)	(3)	(4)
	Prob. of Take-Up	Total Amount	Labor Market Outcomes	
			Months Worked	Labor Income
Point Estimate	58.1500	1,776.2823	-0.6631	-741.9075
	(0.1645)	(5.5358)	(0.0201)	(34.4737)
Baseline Mean (at Cutoff)	7.1397	114.1965	3.9258	5015.0061
Effect Relative to the Mean	-	-	-16%	-14%
Observations	819,198	819,198	819,198	819,198

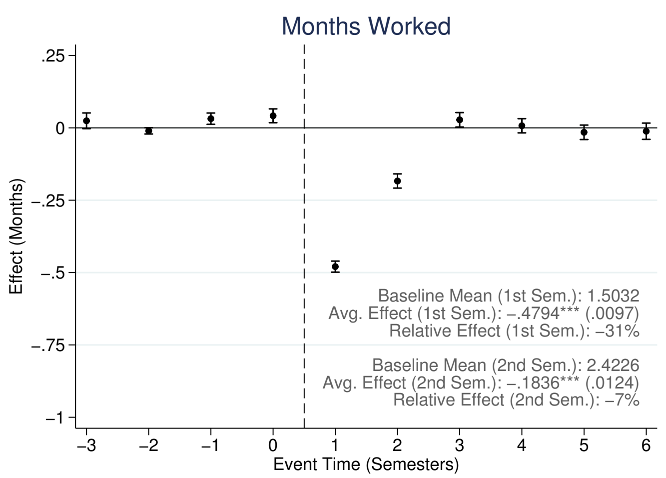
Notes: The first two columns in this table show the first-stage effects of UI eligibility on the probability of UI take-up (column 1) and the total amount of claimed benefits (column 2). The sample includes displaced male workers with at least 6 months of continuous employment prior to layoff who are displaced within a symmetric bandwidth of 60 days around the cutoff required for eligibility to unemployment benefits – namely, 16 months since the previous layoff resulting in UI claims. The local linear regression includes a dummy capturing eligibility for UI benefits (i.e., the main variable of interest), time since the cutoff date for eligibility, and a term for the interaction between the two. Standard errors clustered at the worker level are indicated in parentheses. All coefficients, standard errors, and baseline means representing probabilities have been scaled by 100, and effects are thus interpreted in terms of percentage points.

Figure C1: Local Average Effects of UI Eligibility on Benefits Claimed and Number of Months Worked



Note: The graphs plot the averages around the eligibility cutoff for the total amount of claimed benefits and the total number of months worked up to one year after layoff. The sample includes displaced male workers with at least 6 months of continuous employment prior to layoff. Dots represent averages based on 5-day bins. The lines are based on a local linear polynomial smoothing with a 60-day bandwidth with 95% confidence intervals.

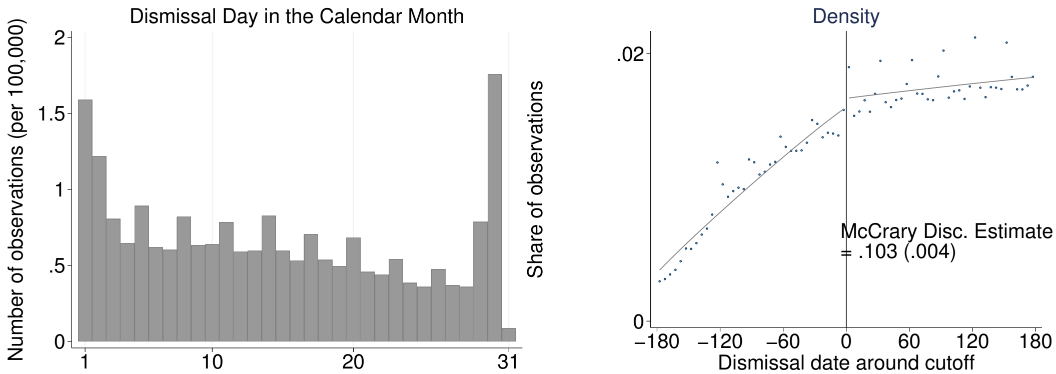
Figure C2: Local Average Effects of UI Eligibility on Number of Months Worked, by Semester



Note: This figure shows the RD estimates of UI eligibility on the number of months worked for different 6-months periods before and after dismissal. The event time on the horizontal axis indicates time relative to dismissal analogously to the description in section 3.1. 95% confidence intervals are displayed.

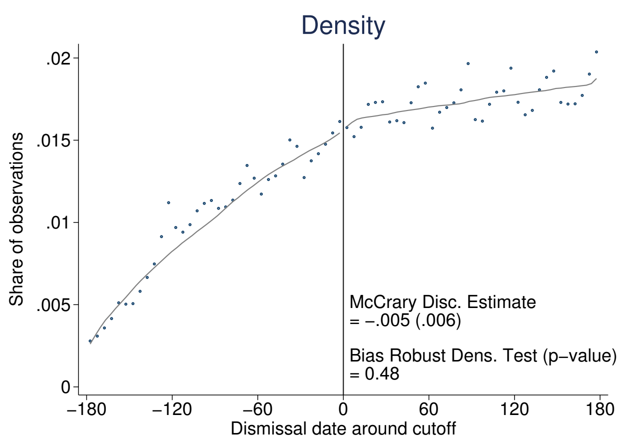
C.2 Additional Robustness Tests

Figure C3: Dismissal Dates Monthly Cycles



Notes: The left graph presents the distribution of dismissal dates by calendar day within each month. The right graph presents the running variable density function around the cutoff, based on an initial sample that includes all dismissal dates.

Figure C4: Effect of UI Eligibility, Density Function



Notes: This figure shows the density of dismissal dates around the cutoff date for eligibility for unemployment benefits (i.e., 16 months since the previous layoff date in the past) in our main working sample. The sample includes displaced parents with at least 6 months of continuous employment prior to layoff. The results of McCrary density test and the bias robust test proposed by Cattaneo et al. (2018, 2020) are also reported.

Figure C5: Effect of UI Eligibility, Balance on Covariates

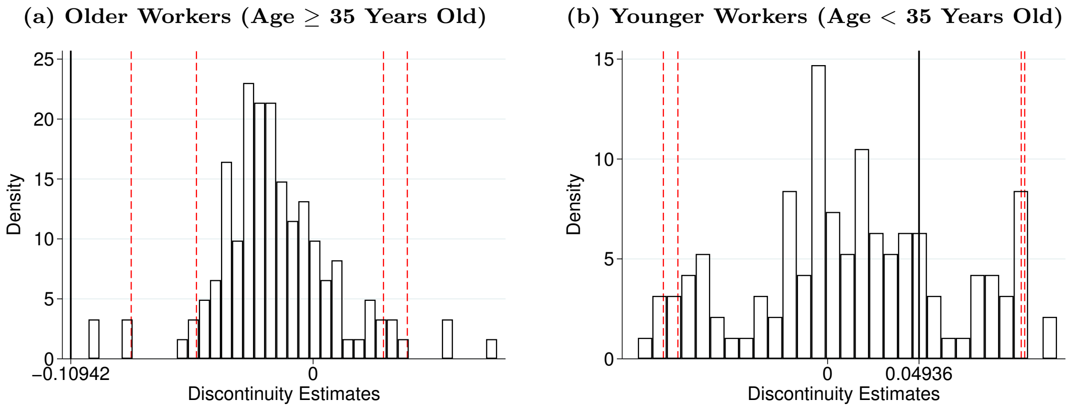
Notes: The graphs show the balance of pre-determined covariates around the cutoff for eligibility for unemployment benefits. The sample includes displaced parents with at least 6 months of continuous employment prior to layoff. Dots represent averages based on 5-day bins. The lines are based on a local linear polynomial smoothing with a 60-day bandwidth with 95% confidence intervals.

Table C2: Effect of UI Eligibility on Public Hospitalization (External Causes, Older Workers), Robustness to Different Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Point Estimate	-0.0585	-0.0694	-0.0683	-0.0525	-0.0726	-0.0645	-0.0531	-0.0798
	(0.0234)	(0.0345)	(0.0238)	(0.0195)	(0.0280)	(0.0223)	(0.0206)	(0.0319)
Bandwidths (Days)	CCT	30	60	90	CCT	150	180	CCT
Polynomial Order	0	1	1	1	1	2	2	2
Observations	1,064,201	1,064,201	1,064,201	1,064,201	1,064,201	1,064,201	1,064,201	1,064,201

Notes: This table replicates the regression discontinuity analysis in Table 3 for different specifications of the polynomial regression and different bandwidths (indicated on bottom of the table). CCT denotes the optimal bandwidth according to Calonico et al. (2014).

Figure C6: Effect of UI Take-Up on Public Hospitalization (External Causes), Permutation Test



Notes: The graphs compare t-statistics for the discontinuity estimates of the effect of UI take-up on hospital admissions at the true cutoff for UI eligibility (vertical black line) with the distribution of t-statistics obtained at all possible placebo cutoffs within 180 days away from the actual threshold. The dashed lines represent the 2.5, 5, 95 and 97.5 percentiles in the distribution of placebo cutoffs. Estimates are based on a local linear polynomial smoothing with a 60-day bandwidth, as in equation (3).