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

Guilherme Amorim† Diogo Britto‡ Alexandre Fonseca§
Breno Sampaio¶

August 2, 2022

[CLICK HERE FOR LATEST VERSION]

Abstract

We study the causal effect of job loss on hospitalization and mortality for the universe of Brazilian workers, and investigate how these are impacted by exogenously-defined variation in access to unemployment insurance (UI) after dismissal. We construct a novel, individual-level dataset that matches observations across several administrative, nationally-comprehensive records on employment, hospital discharges, and mortality in Brazil for a period of 17 years. Using a combined matching and difference-in-differences approach based on firms' mass layoffs, estimates show that, for male workers, involuntary unemployment causes an increase of 15% in the probability of in-patient admission to public hospitals, and an increase of 37% in the risk of mortality. Such impacts are mostly driven by external causes of hospitalization/deaths, such as injuries, accidents, and assaults. Using a regression-discontinuity design that explores a sharp change in UI eligibility based on workers' dismissal dates between successive employment spells, we further show that UI take-up largely offsets the risk of hospitalization for older workers, and that such mitigating effect lasts beyond the program's benefit window. Our results suggest that job displacement programs can play an important role in mitigating some of the adverse health risks of job loss for potentially susceptible individuals.

Keywords: job loss, unemployment insurance, hospitalization, mortality

JEL Codes: I12, J63, J65

---

*We thank Rishard Akresh, Alex Bartik, Dan Bernhardt, Andy Garin, Ben Marx, Nolan Miller, David Molitor, Adam Osman, Julian Reif, and Rebecca Thornton for all the insightful comments and support.

†**Corresponding Author**. University of Illinois at Urbana-Champaign. Address: 214 David Kinley Hall, 1407 West Gregory Drive, Urbana, IL 61801, USA. E-mail: gm10@illinois.edu

‡Bocconi University, BAFFI-CAREFIN, CLEAN Center for the Economic Analysis of Crime, GAPPE/UFPE, IZA. Address: Via Roentgen 1, 20136, Milan, Italy. E-mail: diogo.britto@unibocconi.it.

§Federal Revenue of Brazil, GAPPE/UFPE. Address: Rua dos Economistas, Cidade Universitária, 50740-590, Recife, Pernambuco, Brazil. E-mail: alexandre.fonseca@rfb.gov.br

¶Federal University of Pernambuco, GAPPE/UFPE. Address: Rua dos Economistas, Cidade Universitária, 50740-590, Recife, Pernambuco, Brazil. E-mail: brenosampaio@ufpe.br.

1 Introduction

The effects of unemployment on health has for long been a topic of interest to economists and policy makers. However, the assessment of such causal link and the mechanisms through which it operates is fraught with many challenges, from the scarcity of credible sources of external variation to the difficulty of measuring individual health outcomes in large populations. While a few recent studies have managed to overcome some of these challenges, evidence is still primarily focused on high-income economies such as European countries or U.S. states. Yet very different – and potentially worse – implications may be uncovered in less developed regions, often characterized by more fragile public health conditions, higher labor turnover, and weaker social insurance institutions.

Besides our poor understanding of the health impacts of unemployment, evidence on the efficacy of public policy alternatives aimed at mitigating such impacts, in a single empirical setting, is still scant. Understanding the effectiveness of alternative policy remedies is not only important from a policy perspective but may shed more light on the mechanisms driving the effects of job loss on health.

In this paper, we use a unified framework to study the causal effects of job loss on hospitalization and mortality for dismissed Brazilian workers, and how these are impacted by exogenously-defined variation in access to unemployment insurance after dismissal. To do so, we construct a novel, individual-level dataset that matches observations across several administrative, nationally-comprehensive records on employment, hospital discharges, and mortality in Brazil for the 17-year period between 2002 and 2018. This rich dataset allows us to observe, for the universe of workers employed in the formal sector, detailed information on all their employment spells and earnings, their hospitalization and death records, and their enrollment in private-sector health insurance plans. We also observe unemployment insurance take-up and benefit amounts for each individual claimant in our sample.

Our analysis is divided into two main parts. In the first part, we estimate the dynamic treatment effect of job displacement on public hospital admissions[1] and mortality (both overall and by each cause of hospitalization/death). Our empirical strategy is based on comparing health outcomes of workers who were displaced by *mass layoffs* with those of a matched control group of workers who were not displaced in the same year (and were employed in firms that did not experience mass layoffs during all panel years). Identification relies on the assumption that mass layoffs are orthogonal to workers' health and to any other shock that might jointly affect their employment and their health. This assumption

[1] Unless otherwise noted, we will use expressions such as "public-sector hospital admissions", "in-patient admissions to public hospitals" and "hospitalizations" interchangeably throughout the paper.

is supported by the absence of different trends in health outcomes between treatment and control groups prior to layoff. In addition, the dimension of our dataset (between 1 to 3 million observation in each sample) allows us to exactly match workers across both groups by a series of specific individual-, firm-, and regional-level characteristics, further increasing our cross-sample comparability.

We find that employment and average earnings of male workers decreases, respectively, by 18% and 37% up four years after layoff, in comparison with the control group. We then document that the probability of admission to public hospitals increase, on average, by 15%, and that the probability of death increases by 37%, the latter effect being mostly concentrated on the first year after layoff. When analyzing average effects disaggregated by medical diagnosis groups and/or causes of death according to the International Classification of Diseases (ICD-10), effects appear concentrated on either short- or long-term developments in ischemic heart diseases, cerebrovascular diseases, neoplasm of respiratory organs, and mental and behavioral disorders due to substance use. Most strikingly, however, are the larger increases in both hospitalizations and deaths due to *external causes*, such as different types of injuries (i.e., to the head and thorax, and other accidental injuries), early complications of trauma, and assaults. They are also indicative of circumstances classified as either accidents or (attempted) homicides at their time of registry. Our estimates indicate an increase of 33% in hospitalizations and a staggering 276% increase in the probability of death (albeit over a very small baseline probability) due to such causes. Taken together, results point to the direction of an increase in the incidence of diseases associated with higher stress, and, in general, with the engagement of activities involving risky behavior.

We find no economically significant effects on either hospitalization or mortality for female workers. However, their labor market effects are very similar to those found for male workers, and their mortality estimates, though imprecise, suggest similar impacts in this group as well (in particular for the first year). Furthermore, using data on individual family links which allows us to match many of the laid-off workers in our sample with their respective spouses and children, we find that the children of both male and female workers are also subjected to higher risks of hospitalization due to external causes following their parent's dismissal from their jobs. These findings also support the hypothesis that our estimates reflect consequences of higher stress at the household level following an employment shock.

To evaluate if there is any significant heterogeneity in our results across regions and worker characteristics, we perform a series of triple-differences exercises as an extension to our empirical specification. They reveal that our main effects on hospitalization and mortality, more specifically those associated with external causes, are mostly driven by younger workers (i.e., those with ages below the median age in our sample). Effects on mortality due to non-external causes, in turn, are mostly driven by workers in older cohorts (i.e., those with ages above the median age). An additional exercise exploring the heterogeneity in workers' enrollment status in private health insurance plans (HIs, henceforth – insurance options which are usually employer-sponsored and cover services at a smaller market of *private* hospitals) further shows that the effects on hospital admissions (at *public* hospitals in the country's universal health care system) are largely *not* explained by differential access to HIs before or after layoff. In other words, while we find some small degree of *substitution* between private and public health care options after layoff, specially for those workers already enrolled in HIs in their year of layoff, our results are still mostly driven by a *direct* impact on public hospital admissions unmediated by enrollment status in private insurance.

In the second part of our analysis, we evaluate the contribution of unemployment insurance (UI) as a means to mitigate the adverse health impacts of layoffs. As in many other countries, UI in Brazil is a federal program that provides financial benefits to workers laid off by their companies. Any worker fired (not for cause) by their employer becomes eligible to UI provided they meet specific requirements on job tenure and on the number of months separating any previous layoff date from the current one, in case the former was already used to claim UI. This latter rule allows us to apply a (fuzzy) regression discontinuity design to a restricted sample of displaced workers and isolate the effect of income shocks, as well as relate it with to other potential mechanisms.

Our results show that the increase in UI take-up at the margin of the eligibility threshold largely offsets the risk of hospitalization for male workers documented in the first part. In line with our previous findings, we find that this attenuating effect of UI is concentrated at the same hospitalization categories mostly affected by job loss, namely, in-patient admissions due to external causes. Furthermore, we observe that these effect are driven by workers at age groups above the median age in our sample (i.e., around 30 years old), while no significant effect is found for workers below the median age. Quantitatively, for the former group (older male workers eligible to UI) the probability of emergency hospitalization due to external causes decreases by 85% compared to uneligible workers in the same age range. Finally, we find no significant impacts of UI eligibility/take-up on the risk of mortality for laid-off workers.

Our findings in this second part support the idea that job displacement programs can play an important role in mitigating some of the adverse risks of job loss for individuals potentially more susceptible to these risks. In fact, we show that impacts are persistent and that they outlast the average benefit window, as differential impacts on marginally eligible workers are observed as far as one year after layoff. Upon further investigation on potential mechanisms, we also show that UI in Brazil discourages job search for treated individuals – a result that is in line with previous literature on the labor market effects of UI (see e.g., Katz and Meyer, 1990, Lalive, 2007, and Card et al., 2007). Taken together, results are thus possibly associated both with stress-induced consequences of higher liquidity constraints and higher pressure for exiting unemployment among the older, more vulnerable, male population.

This paper contributes to the extensive literature on the economic consequences of job loss, in particular on its implications to the health of laid-off workers in the short- and long-run, and more broadly on the impacts of economic downturns on public health (e.g. Ruhm, 2000). Our findings build on earlier works documenting the principal mechanisms that might establish an association between job displacement and poor health, such as reductions in earnings (see e.g. Ruhm, 1991 and Jacobson et al., 1993) and other social, psychological and behavioral consequences (see Eliason and Storrie, 2009b for an overview). They also align with more recent work that utilize records on mortality and hospitalization at the worker level. Examples include Sullivan and Wachter (2009), who document increases in mortality rates among male workers in the U.S. state of Pennsylvania; Kuhn et al. (2009), who find increased expenditures on hospitalizations due to mental health problems for male workers in Austria; and Browning and Heinesen (2012), who find increases in the risk of circulatory diseases, abuses in alcohol consumption, suicide and suicide attempts, and traffic accidents for Danish workers.[2] In terms of institutional setting, our work relates more closely to Britto et al. (2022), who also use Brazilian data to investigate how job loss drives up the incidence of criminal activity in the country.

To the best of our knowledge, our study is the first comprehensive investigation of the effects of involuntary job loss on the risk of hospitalization and mortality for a large developing country, combining state-of-the-art econometric techniques with administrative, worker-level data on formal jobs for the whole population over an almost two-decade time span. Such features permit us to explore, at an unprecedented level of detail, the impacts by different causes of hospitalization and deaths, the risk factors associated with different individual characteristics (such as age, gender, tenure and education level), and their variations by different local socioeconomic characteristics. Our analysis also expands in two important directions for public policy-making. First, despite the universality of public health care coverage in the country, the availability of optional, complementary private-sector options to a small share

[2]Still other contributions include Salm (2009) and Schaller and Stevens (2011), who find no effect of job loss on various measures of physical and mental health based on samples of American and German workers, respectively; Schaller and Stevens (2015), who find negative impacts in self-reported health and mental health, and reductions in insurance coverage (but no impact on health care utilization) for a sample of American workers; and Bloemen et al. (2018), who find sizeable increases in the probability of death for a sample of Dutch workers.

of the population allows us to study how the estimated impacts change with varying levels of access to either public or private health care by laid-off workers. Second, the post-layoff variation in access to UI that comes from its specific eligibility rules allows us to evaluate its efficacy as a policy alternative to tackle the adverse impacts of job loss on workers' health. Our findings in this part differ from those in Kuka (2020), for example, who find that higher UI generosity in the U.S. increases health insurance coverage and utilization, but have no measurable effects on risky behaviors (such as smoking or alcohol consumption) or health conditions of recipients. In contrast, we find *no* effect on private health insurance enrollment (which, again, is very likely associated to the ubiquitousness of public health care coverage in the country) and a *decrease* in public-sector hospitalization due external causes, which include accidents, injuries, and other consequences of risky activities in general. These results, coupled with the persistence of the program's effect, suggests that temporary financial incentives can be a useful policy tool to help alleviate some of the health risks associated with job displacement.

The remainder of the paper is organized as follows. Section 2 provides background information on the Brazilian labor market, mortality trends, and health care in Brazil. Section 3 describes the data, while Appendix B details the merging procedure of our various data sources. Section 4 presents results for both male and female workers, and Section 5 investigates mitigation with unemployment insurance. Section 6 concludes.

2 Institutional Background

This section gives an overview of the main facts about health and mortality in Brazil and discuss some features of labor regulations that are relevant to our study.

2.1 Facts on Mortality and Hospitalization

As in other developing countries, Brazil underwent several economic, demographic, and epidemiological transitions during the second part of the 20th century that shaped the current state of its public health (Paim et al., 2011). Indicators for more recent years also show that health trends in the country have been strongly countercyclical, with periods of economic downturns (manifested, for instance, in higher rates of unemployment and lower access to credit) also matching those of higher morbidity and mortality rates. This pattern can be visualized in in Panel (a) of Figure 1, which shows the evolution in the adult mortality rate for the Brazilian population between 1997 and 2018 superposed with the evolution in the rate of total employment during the same period. Mortality trends disaggregated by the leading causes of death are discussed in Appendix A.1.

Panel (b) of Figure 1 shows the total quantities of cause-specific deaths (black bars) and in-patient admissions to public hospitals (gray bars) in Brazil between 2002 and 2018, for adults aged between 18 and 65 years old. The listed categories in the vertical axis correspond to specific disease blocks within ICD-10 chapters that correspond to the leading causes of hospitalization/deaths in the country during this time period. To facilitate our exposition, we further group these blocks into either *external causes*, containing all blocks related to injuries, accidents, self-harm and assaults, or *non-external causes*, containing all remaining blocks (see Subection 3.2 for more details on this classification). The figure suggests a close correspondence between the number of public-sector hospitalizations and deaths in the country, with similar leadings causes being featured across both measures. In terms of proportion, the number of deaths in each category is roughly around 10% to 50% of the corresponding number for hospitalizations (a notable exception being assaults, for which the number of recorded deaths is higher than the number of hospitalizations).

[Figure 1 about here.]

2.2 Health Care in Brazil

Access to health care in Brazil is enshrined in its 1988 Constitution and institutionalized in the country's Unified Health System (*Sistema Único de Saúde* – SUS), a nationalized system of health care that provides universal coverage for all citizens, free at the point of service. Such universality is maintained with significant investments from all levels of government, whose total expenses with health care reached R$ 557 billions in 2014, or 10,1% of the country's GDP (Azevedo et al., 2016). Today the SUS provides a range of medical services of all levels of complexity, has hospitals, emergency rooms and community care centers operating in more than 90% of all municipalities, and provides access to primary health care even in the most remote, rural areas of the country (Bhalotra et al., 2016) – a substantial achievement for a country with more than 200 million inhabitants.

The hospitalization records used in this paper are from the SUS. In parallel with the public sector, a private, supplementary sector of health care also operates in Brazil, with autonomous physicians offering medical services in privately-owned hospitals and clinics. Most of these services are financed through enrollment in health insurance plans (which are, in turn, mostly employer-sponsored) and in 2019 they covered about 24.25% of the Brazilian population, in addition to the coverage provided in the SUS (ANS, 2019).[3] There

[3]There are important differences in usage across both systems, most notably: *(i)* the supplemental/private sector comprises a larger network of establishments for outpatient care (73% of the total in

are two ways in which this might affect the interpretation of our estimates: at the one hand we might be underestimating the real effects on the probability of hospitalization by not covering the private sector, but at the other hand, part of the effects might be accounted for by workers substituting one type of care (private) for another (public) upon losing access to private health insurance as a consequence of job dismissal. We use individual-level data on health insurance enrollment and aggregated data on private sector hospitalizations to study this interaction between the public and the private sector, and perform additional exercises to quantify the extent to which our estimates could be explained by any such substitution effects.

2.3 The Brazilian Labor Market

Labor relations in Brazil are governed by federal regulations and are based on at-will employment. Firms are free to terminate workers unilaterally, for no specific reason and without the latter's prior approval (or to use the legal expression employed, without a just cause). Such terminations are the focus of this paper, to which we will refer to as either "dismissals" or "layoffs". They correspond to about two thirds of all job separations in our period of analysis, while the remaining third are mostly voluntary quits. We also focus on open-ended, full contracts in the private sector, which amount to 73% of all contracts in our period of analysis.

The main assistance program providing financial relief to dismissed workers in Brazil is unemployment insurance, administered by the federal government.[4] Eligibility rules are defined by certain conditions on worker's tenure and whether (and when) they have claimed the same benefit in previous dismissals. We provide further details on such rules in Section 5. The duration of benefits may last from three to five months depending on the length of employment. The replacement rate starts at 100% for workers earning a minimum wage and declines smoothly to 67% at the benefit cap (2.65 times the minimum wage). In 2018, total federal expenditures in the program amount to 36.3 billion Reais, or 0.53% of the country's GDP (TN, 2019).

2019), while the public sector has more establishments for inpatient and emergency care (54% of the total in 2019); *(ii)* emergency care corresponds to a third of all inpatient hospital admissions in the private sector, while in the public sector it corresponds to about 80% of admissions. Moreover, there is evidence of high interaction between the two systems, with a share of hospital admissions in the public sector, for instance, being from patients needing the same treatments they are covered by in the private sector (see Appendix A.2).

[4] Upon dismissals workers can also access funds from a mandatory savings account to which they contribute with 8% of their monthly wages during their employment spell. They are also entitled to severance payments that further compensates them with an amount equivalent to 40% of that account's balance. Taken together, both provisions give workers an approximate total of 1.36 times a month's worth of wages per tenure year at the time of layoff.

Labor informality is high in the country, comprising an estimated 45% of all jobs in 2012. Job turnover is also high and there is substantial interaction between the formal and informal sectors, with many workers moving frequently between the two. In addition, some firms even maintain both formally- and informally-hired workers in their payroll (Ulyssea, 2018). Due to the lack of comprehensive data on informal jobs, our analysis focus on formal jobs only. However, we use survey data to calculate the approximate rate at which workers in the formal sector migrate to informality each year, and show that the implied impacts on our estimates should be quantitatively small (see Appendix A.3). We also perform heterogeneity exercises to investigate how our estimates are impacted by the degree of labor informality in a region. Finally, we notice that the presence of an informal sector suggests our estimates are lower bounds to the real elasticities of employment and income on health.

3 Data

In this section we present the main administrative datasets used in this paper. The procedures implemented to merge information across individual observations in each sample are discussed in Appendix B.

3.1 Formal Labor Force

Information on matched employer-employee relationships comes from the Annual Social Information Report (*Relação Anual de Informações Sociais* – RAIS), administered by the Ministry of Labor. This comprehensive dataset is the main source of information about formal employment in Brazil, covering the universe of formally employed wage earners in both the public and private sectors, although it does not include the informal sector. It is collected annually and it contains detailed worker-level information such as the start and end dates of each contract, the location of each job, standardized codes for each occupation and industry sector, and worker's education and earnings. It also includes worker identifiers that allow us to keep track of labor force movement between different companies and across different years. Our working sample covers the years from 2000 to 2018.

Labor regulation in Brazil requires employers to notify workers in advance in case of a dismissal. This advance notice period starts at a minimum of 30 days for workers employed for up to one year, and increases proportionally to each extra year of tenure (3 extra days for each year, capped at a maximum of 90 days in total). In our sample, more than one-third of workers were dismissed before reaching one full year of employment, more than two-thirds before two years, and more than 90% before three years, meaning that most notice periods were within a window of 30 to 39 days. Throughout our analysis, we center all time units around the dismissal date registered at RAIS minus the 30 days corresponding to the most conservative baseline for the advance notice period. With this we aim to capture also the effects of learning about a job loss, which in our context could be just as relevant as the actual dismissal.

3.2 Health Insurance, Public Hospital Admissions, and Deaths

Information on public health care and public hospital admissions is from the National Health Database (*Base de Informações de Saúde* – DATASUS). This system is the government's official registry of all data collected from the country's network of public hospitals and healthcare services, the SUS. It is managed by the Brazilian Ministry of Health and it covers virtually all of the country's territory. Our sample comprehends all years between 2000 and 2018.

Individual-level data on admissions to public hospitals is from a subsection in this database called the Hospital Admissions System (*Sistema de Internações Hospitalares* – SIH-SUS). It includes information on individual characteristics such as age, sex, municipality and zip code of residence, and descriptive information on the hospital admissions such as diagnostic, procedure (if any), date of admission, length of stay, total value charged and paid, and hospital code. It also includes variables that characterize the general circumstance of each hospital admission. Although there is some overlapping in each description, it is possible to classify each admission into one of the following broad categories: elective, emergency, accidents, high-complex, and other causes. In practice, the first two of these categories comprise respectively 16% and 82% of all observations. In our analysis we add all observations under the "accidents" category into "emergency", and the remaining ones are classified simply as "elective".

Data on individual deaths is also from a subsection of DATASUS, the Mortality Information System (*Sistema de Informação sobre Mortalidade* – SIM-SUS). This dataset is generated annually from death certificates issued by the federal government, and it covers all deaths occurred within the Brazilian territory. It includes many of the same individual characteristics available at the SIH-SUS supplemented with additional information such as date and cause of death.

The use of a same standard of classification for all different causes of hospitalizations and deaths (i.e. ICD-10 blocks and chapters) allows us to draw further correspondences across both measures. As hinted at Subection 2.1, besides focusing on the main specific causes of hospitalizations and deaths we also group all observations into two groups of roughly equal proportion: those due to external causes (including all diagnoses grouped under chapters 19 and 20 in ICD-10) and those due to non-external causes (including all observations from other chapters). The first group contains those causes of hospitalization/mortality classified as injuries, accidents and homicide/suicide attempts and are thus informative about the *circumstance* of each event, while the second group include different types of diseases specifically associated to medical conditions.

Finally, enrollment data on private health insurance plans comes from the National Health Agency (*Agência Nacional de Saúde – ANS*). This agency is the primary government entity responsible for regulating the private (i.e. supplementary) healthcare market in the country. The dataset is also made available through the DATASUS platform, and it contains many of the same individual-level information mentioned in the other datasets for every insurance enrollee, plus other relevant information such as whether each plan is employer-sponsored (i.e. corporate) of was purchased in the individual market. It is likewise a nationally comprehensive dataset and contains records on all years covered in the other datasets.

3.3 Individual Registry

The last dataset we employ is the individual registry (*Cadastro Pessoa Física* – CPF) of the Federal Revenue of Brazil (*Receita Federal do Brasil* – RFB), the Brazilian federal bureau of tax administration. This dataset dates back to 1965 and contains the registry of the entire Brazilian population. It reports for each individual a unique code identifier, their full name, gender, date of birth, full residential address (including the history of all previous addresses) and their mother's full name. It also includes the year of death for all individuals who were deceased up until 2018. Finally, for those who have filled taxes at any year after 2006, it also includes records of their declared spouses and/or dependents.

3.4 Descriptive Evidence

Figure 2 shows the probabilities of hospitalization and death for different age and tenure groups, for the years between 2006 and 2014. The sample includes both male and female, full-time workers in the non-agricultural, private sector. Panel (a) displays the cumulative probability of death in the three years following the current one, both for workers who were displaced and those who remained continuously employed at each year. Panel (b) replicates this same exercise for the cumulative probability of admission to public hospitals. It focuses on displaced workers only and it also reports that same probability for the years between the current year of dismissal and the two years before dismissal.

[Figure 2 about here.]

Some interesting patterns emerge from the figures. Within every age group between 18 and 60 years old, the probability of dying in the next three years is consistently higher for workers displaced in a given year than for those who remained continuously employed in that same year. Similarly, among displaced workers the probability of public hospital admission is higher after displacement both for younger ($\leq$ 24 years old) and older ($\geq$ 40 years old) cohorts in the sample. This is consistent with the hypothesis that job loss may be specially detrimental to the health of those individuals at each tail of the working age distribution in a population. For the age groups in between, however, the evidence seems mixed.

Within the different job tenure groups the variations in the probabilities of mortality and hospitalization mirror one another. Displaced workers with or less than 24 months of continuous employment (which comprehends more than half of individuals in the sample) show higher values for both probabilities, while the pattern seems to reverse at higher levels of tenure. This seems consistent with the possibility of lower tenure groups being, on average, associated with industries with high turnover and low job security, which may likely potentialize the adverse health impacts of job loss.[5]

4 Job Loss, Public Hospital Admissions and Mortality

In this section we present our framework and findings for the first part of our analysis, in which we estimate the impacts of job loss on the probability of admission to public hospitals and of mortality.

4.1 Sample Selection and Empirical Strategy

In our analysis we focus on full-time workers (i.e. working a minimum of 30 hours per week) in the 18-65 age range, male and female, with open-ended contracts in the non-agricultural, private sector.[6] We adopt a combined matching/difference-in-differences approach as identification strategy and use *mass layoffs* as a source of exogenous variation.

Following Jacobson et al. (1993) and Couch and Placzek (2010), our baseline definition of mass layoff is the at will displacement of more than 33% of a firm's workforce in a given calendar year. As treatment group we select all workers displaced in mass layoffs between 2006 and 2014, which allows us to estimate dynamic treatment effects for up to four years

[5] In conducting the exercises in this subsection we noticed no prior distinctions in patterns between males and females (not shown).

[6] More specifically, male workers until 65 years old and female workers until 60 years old. This sample selection aims to avoid confounding effects of retirement, whose minimal ages were set at these values during the analyzed period.

after displacement, as well as placebo effects up to three years before displacement. The pool of candidate control workers includes all individuals employed in firms that did not experience mass layoffs during our period of analysis.

We perform an exact matching procedure where we match each worker in the treated group with a single worker in the control group who *(i)* is not displaced in the same calendar year, and *(ii)* belongs to the same category as the treated worker in accordance to selected individual-, firm- and regional-level characteristics. These are: birth cohort, tenure, earnings category (by 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, one single control unit is randomly selected. We then assign to control workers a placebo dismissal date equal to the layoff date of the matched treated worker, and compare outcomes for the two groups at different time intervals relative to the layoff date.[7]

To estimate the effect of job loss on employment and income, health insurance enrollment, and public hospital admission, we use the following event-study equation:

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

where Y_{it} is the parameter of interest for worker i at year t, $Treat_i$ 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 layoff date. This means that $Time_t = 0$ represents the first 12 months after layoff, $Time_t = 1$ the following 12 months, and so on; while $Time_t = -1$ represents the 12 months previous to layoff, $Time_t = -2$ the 12 months previous to that, and so on. The baseline omitted period is set at $Time_t = -1$. The coefficients $\{\beta_0, ..., \beta_T\}$ identify the dynamic treatment effects, while $\{\beta_{-P}, ..., \beta_{-2}\}$ identify any potential anticipation effects. The average treatment effects over all periods are estimated using the equation

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

where the dummy $Post_t$ represents all time periods after the layoff date, and all other

[7] With this procedure, half our sample is by construction formed by "never-treated" observations. This helps appease some of the methodological concerns raised recently on the validity of difference-in-differences designs with multiple treatment timings – namely, the bias coming from pairwise comparisons between observations treated at different points in time. We discuss this further at Appendix C.5, where we also formally apply Goodman-Bacon (2021)'s weight decomposition to our main estimates to evaluate the extent of such bias in our setting and also test the alternative estimator proposed by De Chaisemartin and d'Haultfoeuille (2020).

variables are defined as in (1).

The biggest challenge with the approach just described involves estimating the effect of job loss on mortality. Any worker in the treatment group displaced at a given year (and their corresponding matched pair in the control group) must be alive by that same year in order to become treated, thus any anticipation effects identified by (1) are mechanically set to zero. Given this limitation, to identify changes in the risk of mortality we rely solely on the matching strategy with differential estimates for the post-treatment years: $Y_{it} = \alpha + \sum_{t=0}^{T} \beta_t \, Treat_i \cdot Time_t + \sum_{t=0}^{T} \lambda_t \, Time_t + \epsilon_{it}$. Analogously, the average treatment effect is estimated using the following simple equation: $Y_{it} = \alpha + \beta \, Treat_i + \epsilon_{it}$.[8]

Our main identifying assumption is that of parallel trajectories in the rates of hospitalization and mortality across treatment and control groups in the absence of treatment (job dismissal). The main threat to this assumption is the possibility of dynamic selection of workers into treatment. For example, frail workers with weaker health and/or at higher risk of dying from illness may be ones that are targeted by employers for dismissal. Our focus on mass layoffs aims to minimize such concern, as mass layoffs most likely depend on firm-level shocks rather than on displaced workers' health status. By matching workers on several observable characteristics not related to individuals' health and by reporting estimates on pre-trends for most of our outcomes we also hope to provide further support to our identifying assumption.[9]

Table 1 presents summary statistics for treated and control units in each of our working samples (see Appendix B). All three samples are balanced across the series of observable individual-, firm- and regional-level characteristics, even those not included in the matching process mentioned above. The standardized difference between the two groups is below the threshold of 0.20 suggested by Imbens and Rubin (2015) for all variables – the one exception being mean tenure at the firm level, whose standardized difference is slightly higher at 0.23 in all samples. More importantly, our samples are also balanced in the firm-level pre-treatment

[8] In Appendix C.3 we use an intent-to-treat (ITT) strategy where individuals are selected based on the likelihood of dismissal two years into the future (i.e., when the firm they currently work will do a mass layoff). This allows us to calculate individual-level mortality pre-trends two years before treatment, and to compare the magnitudes of the estimates from our main specification with those from this alternative strategy. Reassuringly, ITT estimates for the mortality pre-trends are very close to zero, and the estimated impacts are reasonably comparable across both strategies.

[9] For our mortality estimates we extend the post-treatment effects on the maximum number of years permitted in our panel (i.e. 12 years, as our first treatment year is 2006 and the last year in our panel is 2018). Estimates for 4 or more years after treatment are thus slightly more imprecise but provide us with plausible evidence on their sanity, such as their pattern of dissipation over time. Other features found in our estimates for mortality (e.g. higher effects in the first year after displacement) also agree with similar patterns found elsewhere in the literature (see e.g. Sullivan and Wachter, 2009). Finally, reported pre-trends on the estimated probability of hospitalization give support to the assumption that workers are not selected into treatment based on their health.

layoff and mortality rates up to three years before treatment. It is worth noticing that while the former is used in the matching process, the latter is not, and the fact that pre-treatment mortality rates are strongly balanced gives further support to the assumption of orthogonality between mass layoffs and workers' mortality risks.

[Table 1 about here.]

4.2 Effects on Employment and Public Hospital Admissions

Our main results on labor market outcomes, private health insurance enrollment and admissions to public hospital are displayed in Figure 3. Each graph report the dynamic treatment effects of job loss on a separate outcome for both men (dark gray) and women (light gray), re-scaled by the baseline outcome for each group (i.e. the estimated effect in the respective treatment group at $t < 0$). In all graphs, the difference in outcomes between treatment and control groups is stable in the pre-displacement period, supporting the validity of our common-trends assumption.

[Figure 3 about here.]

The two upper figures show the estimated effects on subsequent employment and labor income. For male workers, the probability of employment decreases sharply by 30% relative to their matched control group in the first year after layoff, while total labor income decreases by as much as 70%. Such impacts are even higher for women, for whom we estimate a reduction of 40% and 80% in each respective outcome. These effects on employment and earnings appear to quickly diminish throughout the years, but by the fourth year after layoff they are still around 15% and 25% for each gender group, respectively. Average effects over the four years after dismissal (in absolute values and percentage points) are shown in Table 2.

[Table 2 about here.]

The bottom-left graph in Figure 3 reports the estimated effect of job loss on the probability of enrollment in private health insurance plans. As discussed in Section 3, private health insurance plans in Brazil are primarily employer-sponsored (i.e. corporate), so, perhaps unsurprisingly, we estimate that job loss causes a 30% reduction in health insurance enrollment by the first year after layoff, and by the fourth year this reduction is still around 10%. Estimates are remarkably similar across both genders. Average effects on overall enrollment after four years, reported in Table 3, show a 20% reduction for men and a 16% reduction for women.

Individual, non-employer-sponsored health insurance plans are available across many markets in the country, so dismissed workers who lost access to health insurance through their employers could, in principle, opt to purchase such plans. The total effects on insurance enrollment from Figure 3 shows us, however, that such markets are unable to sufficiently absorb those displaced individuals. On Table 3 we also report separate estimates on the effects of displacement on both corporate and individual health insurance enrollment. For both men and women, we quantify an average reduction of 31% in the probability of enrolment in the former, while for women we also quantify an increase of 23% in the probability of enrolment in the latter.[10]

[Table 3 about here.]

The bottom-right graph in Figure 3 shows the estimated effect of job loss on the probability of admission to public hospitals, up to 4 years after layoff. Estimates shown in this graph reveal a positive and growing impact of job loss on emergency hospital admissions for male workers. This impact is quantified in Table 4, which shows average 4-year estimates for the impacts on hospitalization and includes implied elasticities with respect to labor market outcomes. This is done by taking the calculated effects on percentage terms for hospitalization and dividing them by the percentage effects on employment and earnings.[11] Estimates in column (1) reveal an increase of 15% in this probability for male workers, from a baseline probability of 0.0059. Column (2) reveals that this effect is mostly explained by an increase in emergency admissions, whose probability is increased in 30% (from a baseline of 0.0034). These are mostly characterized by inpatient hospital admissions that involve any type of non-elective medical care, tipically in situations that would warrant a hospital visit without prior notice (but not only restricted to admissions through the emergency room of a hospital). As mentioned in Section 2, these correspond to about 80% of all hospital admissions in the public healthcare system. Conversely, no statistically significant effect is found for non-emergency admissions, as shown in column (3).

Preceding the fuller disaggregation by diagnoses we perform in Subsection 4.5, columns (4) and (5) in Table 4 shows separate estimates by causes of hospitalization (diagnoses),

[10] We again caution that, due to measurement error on plan characteristics pertaining to the sample matching procedure described in Subsection 4.1, baseline pre-displacement values on corporate and individual health insurance do not add up to the overall baseline value. In reality, all health insurance plans are classified as either one of these types.

[11] We do *not* attach a causal interpretation to such elasticity however, as this would require that layoffs affect hospitalization only through either one of these variables. This is certainly not the case, as the effects could arise through different competing mechanisms other than employment and earnings (such as psychological stress, to name one example). Nevertheless, we take it as an useful exercise to compare effects across different samples and specifications.

which are grouped as either external or non-eternal, respectively.[12] Estimates show that the effects on hospitalization are mostly driven by causes classified as *external*, corresponding to a 33% increase in relation to the baseline for male workers. Effects are also positive and statistically significant for female workers, revealing an increase of 78% with respect to their baseline.[13]

[Table 4 about here.]

4.3 Effects on Mortality

The big question following from what is shown above is whether an increase in hospitalization through emergency room admissions reveal actual adverse effects to workers' health (e.g., through higher risk of stress or higher propensity to risky behavior). Figure 4 shows the yearly effect of job loss on the risk of mortality up to ten years after layoff, using the matching-only specification adapted from equation (1). Two striking patterns emerge. First, that layoffs sharply increase the risk of male mortality in as much as 80%, from a baseline probability of 0.00076. Female mortality is also positive and statistically significant in the first year, with an increase of 40% from a baseline probability of 0.00048. Second, both effects rapidly decrease from the second year on. For females it turns statistically insignificant on all subsequent years, but for males the effect appears to linger up around the sixth year after layoff. Such patterns also correspond to ones found elsewhere in the literature (e.g., Sullivan and Wachter, 2009).

[Figure 4 about here.]

Point estimates for the average effects on mortality are shown in Table 5. In this table we also include separate estimates for the effects on deaths by causes: external and non-external. Results for male workers show that effects are positive and significant for all deaths (column 1) and across both groups of diagnoses (columns 2 and 3). More specifically, we find substantial increases in the overall probability of dying (37% increase over a baseline of 0.0008), and both on the risk of dying from non-external causes (15% increase over a baseline

[12] As mentioned in Subsection 3.2, external causes include all diagnoses grouped under chapters 19 (injury, poisoning and certain other consequences of external causes) and 20 (external causes of morbidity and mortality) at ICD-10.

[13] We urge caution in the comparison of effects between genders in this case, as that the baseline value for female workers is quantitatively less than 1/3 of the one from male workers. With that in mind, we also note that suggested elasticities of employment and labor earnings for females in Table 4 are larger than for males.

of 0.0006) and external causes – a striking 276% increase over a baseline of 0.00006.[14] For female workers, effects are largelly statistically insignificant, but they also point to an relative increase in deaths by external causes (53% over a baseline of 0.00005).

[Table 5 about here.]

What do these estimates tell us? On the one hand, the increase in deaths by non-external causes suggests that these may come as consequences from speficic medical conditions that are likely associated with unemployment – higher stress and/or anxiety being examples of possible mediators. An increase in deaths by external causes, on the other hand, also suggests some type of increased engagement, voluntary or not, in risky activities – an interpretation also supported by the increase in hospitalizations for this category, as shown in Subsection 4.2. Such activities could be directly associated with unemployment *per se* (e.g., higher risk-taking behavior triggered by non-external medical causes, or motivated by financial constraints), but could also in part reflect specific features from the socioeconomic background of Brazilian workers. We further explore the specific causes of hospitalizations/deaths in the Subsection 4.5.

4.4 Heterogeneity Analysis

Our next exercise is to investigate whether our previous results can be explained by any patterns in workers' heterogeneity. More specifically, we check if the impacts of job loss on workers' hospitalization and mortality outcomes vary by different quartiles of selected individual characteristics. Thresholds are defined based on observable characteristics of workers in the treatment group, who are assigned into a given quartile together with their respective pairs in the control group. Our matching strategy, described in Section 4.1, ensures us that treatment and control groups will remain similar in the characteristics used to perform the matching, regardless of the way our sample is partitioned. Also, in this section, as for the remainder of our analysis, we focus on male workers only since the impacts uncovered in the previous subsections are mostly for the sample of male workers.[15]

Results are displayed in Figure 5. Each panel shows the average effects of job loss on the indicated outcome (i.e. probability of hospitalization or death) for different partitions according to sample quartiles of each indicated characteristic. Each results is shown separately for external causes and non-external causes of hospitalization/death.

[14]We note, however, that this large percentual increase is quantified over a baseline ten times smaller than that of non-external causes. Point estimates indicate that the total contribution of deaths by non-external causes to the overall estimate is of 37%.

[15]All exercises in the remainder of the paper were replicated for the sample of female workers. Unless explicitly reported, all results for female workers are either economically or statistically insignifficant.

[Figure 5 about here.]

Estimates for hospitalization, shown in Panel (a), suggest that age is an important measure in explaining differences in effects across workers. We find that impacts on hospitalization due to external causes have a higher gradient towards younger cohorts. With the partition by quartiles, results are statistically significant for the group of workers in the two lower quartiles (18-27 years old), and are also statistically different from the point estimate in the third quartile (28-33 years old). We also find more nuanced evidence that differences in workers' tenure, education, and earnings also affect the likelihood of hospitalization due to external causes, but differences across quartiles are mostly statistically insignificant.

Panel (b) shows that estimates for mortality follow a similar pattern to those found for hospitalization. The most interesting result, perhaps, is that the effects on mortality from external causes are also mostly concentrated in younger cohorts (18-28 years old), partially mirroring the results shown in Panel (a), and that older cohorts in the third (29-34 years old) and fourth (35-60 years old) age quartiles have a higher risk of death from non-external causes. This may suggest that the adverse effects of job loss on health through the channels of stress and risk behavior (see Subsection 4.5) could, to some extent, be explained along the lines of different characteristics associated to either age group. Hospitalization or deaths due to being a victim of an assault, for example, is likely to be driven primarily by younger workers – in alignment with Britto et al. (2022), who show that a positive effect of job loss on criminal behavior is concentrated on younger cohorts only. Older cohorts, on the other hand, may bear a larger share of the direct risks job displacement imposes on medical conditions, such as increased risk of heart attacks.

Other dimensions of worker characteristics that seem important to explain patterns on mortality from external causes are job tenure and years of schooling, which show more concentrated effects on their lower quartiles. This is consistent with the hypothesis that the negative impact of job loss is more prevalent in those occupations with higher turnover and that require lower specialized skills. As with hospitalizations, differential patterns on mortality from external causes by earning quartiles are faint, but estimates appear to be slightly more precise at the highest and lower quartiles of worker earnings, perhaps reflecting some correlation with the channel of different age groups discussed above.

4.5 Disaggregated Outcomes

In Panels (a) and (b) of Figure 6, respectively, we distinguish the effects on hospitalization and mortality for male workers by different causes according to standard ICD-10 classification. As before, we group all events into either external causes or non-external causes.

Following the patterns uncovered in the previous section, each panel shows the estimated effects using the full sample and the subsamples of workers both above and below the median age (i.e., around 28 years of age).

[Figure 6 about here.]

Our findings are twofold. First, we find positive and statistically significant effects for male workers on several disease groups categorized within non-external causes. These include higher incidence of hospitalizations due to ischemic heart diseases (p-value = 0.037), and higher mortality due to mental/behavioral disorders due to substance abuse (p-value = 0.024) and to neoplasms of respiratory organs (p-value = 0.032). Considering the pattern in mortality observed in Figure 4, in Appendix C.1 we also break down estimates by short-term and long-term effects – that is, between effects observed in the first year after layoff versus the three subsequent years. Figure C1 shows higher short-term effects on mortality from ischemic heart diseases and cerebrovascular diseases, and the positive effect on neoplasms of respiratory organs found previously appears here as a long-term effect only (which could in turn be associated with later developments of unhealthy habits, such as smoking; see e.g. Lynge, 1997). Specialized literature both in economics and medicine have previously connected such set of findings with the possibility of follow-up consequences of stress.[16] In our context they are thus likely to be linked to the higher levels of stress usually associated with the event of unexpectedly loosing one's job.

Second, and most strikingly, we find robust evidence of a large increase in emergency hospitalizations and mortality of male workers due to external causes, as seen from the high estimates for injuries and assaults in the two panels from Figure 6. Assaults include only events classified either as homicides or homicide attempts, while injuries (e.g. on the head, thorax, and on multiple body regions) may also be associated to events classified as accidents.[17] We also find positive, statistically significant effects for transport vehicle accidents (Figure 6, Panel (b)) and other undefined events of external causes that are also classified as accidents. We find no evidence of an increase in suicides, which are all included under the category of intentional self-harm. Taken together, these findings are in line with the hypothesis that job loss, being a stressful event that can negatively impact one's health, may also increase the propensity of engagement with risk-taking behavior, such as exposing oneself to activities that are prone to accidents (e.g., reckless driving) or to situations and places that may increase one's risk of becoming a victim of homicide (e.g., fighting, committing robberies or going to dangerous locations).

[16] An overview of possible mechanisms and supporting literature is provided in Eliason and Storrie (2009a)

[17] These different classifications are taken from ICD-10.

More patterns emerge when partitioning the sample between workers above and below the median age. We find that hospitalizations caused by ischemic heart diseases are mostly concentrated at older cohorts, and that both cohorts are at similar risk of mortality caused by substance use. These results are in accordance to the intuitions discussed above. Likewise, in line with our findings in Subsection 4.4, we find that the increments in the probabilities of hospitalization and mortality due to external causes (more specifically, injuries, complications of trauma, and assaults) are higher for workers below the median age. However, estimates in Figure 6 further reveal that such impacts from external causes are also significant for workers above the median age, albeit at smaller magnitudes. In connection with the results from the previous subsection, this suggests that while the large effects on external causes of hospitalization/mortality may be driven more by the younger workers in our sample (and correspondingly by those with lower tenure and educational levels), they are also generally pervasive across different cohorts and other worker characteristics.

4.6 Family Spillovers

One important direction in exploring the effects of job loss on health is assessing their impacts on other members of a same family. The same mechanisms discussed previously can operate similarly on spouses and children of displaced workers, who could likewise become subject to increased financial insecurity and/or reduced access to health care, to name some possibilities. In order to estimate these effects, we construct a representative sample of Brazilian families using the individual links to dependents (children and spouses) that are listed in the individual registry (RFB) dataset, discussed in Section 3 and in the CU dataset discussed in Appendix A.3. In this sample we included individuals who were recorded as spouse only to another single individual, and for whom this link was recorded prior to the worker's layoff. We also only included children aged between 1 to 18 years old at the time of layoff.

Table 6 shows the estimated impacts of job loss on spouses and children of both male and female workers, for the main outcomes discussed in the previous sections. Estimates from Panels A.1 and B.2 give evidence of small spillover effects of job loss on spouses' employment and income for both genders. We find a 5% income decrease on spouses of male workers, and a 1% decrease on the employment probability of spouses of female workers (p-value= 0.096). The probability of enrollment in health insurance is also affected for spouses of female workers (10% decrease). Although effects are economically marginal, they do seem to indicate some level of dependency between a worker's employment status and their spouse's. An interesting finding is that, similarly to the results for male workers found in Subsection 4.3, the probability of mortality due to external causes for spouses of female workers increase significantly by 192% over a baseline probability of about 0.0001 following their spouse's layoff. This is in line with our previous argument on stress-related health consequences from a shock to a household's financial stability, which likely happens when one's spouse loses a job.[18]

Panels A.2 and B.2 reports spillover estimates for children of male and female workers, respectively. We find increase in the probability of hospitalization due to external causes for children of both male and female workers following their layoffs, respectively of 93% (p-value= 0.088) and 191%. This opens up a few possible interpretations. Once losing a job, parents may end up sharing part of their psychological burden with other members of their household, including their children, who in turn may become prone to negative health shocks themselves. Another more extreme possibility is that such estimates could be reflecting an increase of altercations or domestic violence occurred within a same household. Our analysis, however, do not allow us to endorse or rule out either one of these possibilities.

[Table 6 about here.]

4.7 Robustness

We perform a series of robustness exercises with the objective of testing different specifications from the ones used in the main analysis, and also to learn more about the underlying mechanisms behind the estimated impacts from this first part. In Appendix C.2 we perform a mediation analysis to measure to what degree the differential patterns of enrollment in private health insurance plans (before and after job loss) influence our estimates of the impacts on public hospitalization. We find that such patterns explain very little of the overall impacts on public hospital admissions (around 8% in the first year, and below 3% in subsequent ones). This allows us to rule out the hypothesis that these impacts would mostly reflect workers substituting private for public care after losing their jobs, which would then undermine the assumption that public hospital admissions reflect real, direct impacts on workers' health. On the other hand, those patterns do seem to explain a large share of hospitalizations for the small subset of workers who were enrolled in private health insurance plans at the time of layoff: almost one-third of the effects in the first year and an average of 17% of the effects in subsequent years. This suggests that such substitution effects, although negligible in the full sample, could be important for the subset of workers who chose to purchase private coverage when employed.

[18]It is worth mentioning that mortality outcomes in this exercise are not subject to the empirical restrictions from Subsection 4.3, given that spouses of workers can become deceased prior to a worker's layoff. We therefore employ the standard difference-in-differences strategy discussed in Subsection 4.1.

In Appendix C.3 we describe and implement a dynamic intent-to-treat (ITT) analysis to re-estimate the impacts of job loss on mortality and compare these estimates with the ones presented in Section 4.3. While the ITT approach weakens the impact of the treatment shock by construction, it allows us to build a event-study framework for the impacts on mortality much similar to the one used to estimate impacts on all other outcomes, while also giving us the possibility to look at pre-trends on mortality for a couple of years before treatment. Reassuringly, the ITT estimates imply very similar impacts to the ones we find using our main specification, and the estimated pre-trends behave smoothly. Finally, in Appendix C.4 we show that our estimates are robust to a different specifications for our definition of mass layoffs (e.g., different layoff shares, plant closures) and in Appendix C.5 we address some of the methodological concerns raised by the expanding literature on differences-in-differences designs with staggered treatment timings.

5 Attenuating Effects of Unemployment Insurance

In the previous section we document how job displacement significantly impacts workers' health by increasing their likelihood of hospitalization and mortality from causes related to stress and high-risk behavior. In this section, we explore whether unemployment insurance provides any attenuation to those adverse effects. As shown in Section 4.2, job displacement is linked to a negative and persistent shock on the incomes of displaced workers. A positive income shock (in the form of unemployment insurance) thus helps us understand to what extent our results can be explained by the mechanism of financial distress, while also shedding light on the role of public policy in mitigating those same effects.

5.1 Research Design

Unemployment insurance (UI) in Brazil is a centralized, government-sponsored program providing income support to displaced workers in the formal sector. Eligible workers are entitled to 3-5 months of benefits corresponding to up to 80% of their pre-displacement salaries. To become eligible, workers must have remained continuously employed in the same firm for the last 6 months prior to layoff; and a minimum 16-month period must have elapsed between the worker's (current) layoff date and the date of any previous layoff they were subjected to, in case they claimed UI in the latter. This last rule permits us build a (fuzzy) regression-discontinuity (RD) design with a first-stage sharply defined by the sub-sample of workers who are both barely eligible and barely ineligible to claim UI. More specifically, we estimate the following equation:

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

where Y_{it} is the outcome of interest (e.g. take-up and benefits claimed in the first stage, and hospitalization and mortality in the second stage); X_i is the difference between the most recent layoff date and the previous layoff date used to claim UI, standardized such that $X = 0$ at the cutoff required for eligibility (i.e. 16 months between the two most recent layoffs); $f(.)$ is a flexible polynomial function of the running variable (i.e. X_i); D_i is a dummy indicating eligibility to UI in the first stage (i.e. $D = 1(X_i \geq 0)$); and ϵ_{it} is the error term. Finally, β is the coefficient of interest identifying the impact of UI take-up.

Our 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, comparing our mains estimates with a range of placebo effects at different cutoff points.

5.2 Sample Selection and Balance Tests

We restrict our initial sample described in Section 4.1 (i.e. full-time workers in the 18-65 age range with open-ended contracts in the private sector) to include only workers binded by the UI eligibility rules at the time of layoff – that is, workers with at least 6 months of tenure in their current employment who were dismissed for a second time around the 16-month cutoff rule. We keep our window of analysis between 2006 and 2014 and focus only on male workers this time, since this group bared most of the health impacts shown in the previous section.

We also drop from our sample all workers whose layoff dates are within a 3-days distance from the start or the end of each month. This is to address the fact that a slightly higher number of dismissals (and hirings) occur in that specific interval, which can be empirically observed in Figure D1. This cyclical pattern thus creates small discontinuities around these days at each month, which are independent from but may coincide with the 16-month cutoff used in the RD design.

Figure D2 shows no evidence of discontinuity in the density of observations (displaced workers) around the 16-month cutoff using this restricted sample, which is further confirmed by computed statistics from the McCrary density test (McCrary, 2008) and the bias-robust test developed by Cattaneo et al. (2018, 2020). Additionally, Figure D3 shows that observations are balanced around the cutoff point for a rich set of pre-determined worker characteristics; including tenure, earnings, educantional level, age, and several industry sectors.

Together these results provide strong support to the assumption that, locally, our treatment assignment is as good as random.

5.3 Results

The main results on the impact of UI eligibility and take-up are shown in Table 7, which include the first-stage effects on program take-up and on the total amount of benefits claimed, and the second-stage effects of take-up on the probabilities of HI enrollment, hospitalization, and death, the latter two divided between effects by external and non-external causes. As in Subsection 4.5 we estimate each result separately for different age cohorts, which are here displayed in three different panels: Panel A shows results for workers of all ages, Panel B for workers below the median age (around 30 years old in this sample), and Panel C for workers above the median age.

[Table 7 about here.]

The first two columns in Table 7 reveal large positive impacts on both first-stage outcomes around the 16-months cutoff. Estimates in Panel A show that barely eligible workers are 55% more likely to claim UI at the time of layoff compared to barely ineligible workers (column 1), and that they receive an average of R$1,693 (about US$850 in 2012 values) more in total benefits (column 2).[19] Estimates remain quantitatively similar in the age-restricted samples from Panels B and C. Effects on benefits claimed for each sample are also displayed visually in the left-side RD plots shown in Figure 7.

[Figure 7 about here.]

Table 7 also presents our main estimates for the second-stage effects of UI take-up. Columns (1) and (2) confirm, in our setting, the now standard result that UI discourages job search: for a window of up to one year after layoff, UI claimants work an average of 2.5 less months than ineligible workers in the year after layoff, and earn an average of R$2,980 less in labor income (about US$1,500 in 2012 values). The negative effect on labor income is even higher for older workers, who earn an average of R$3,330 less (about US$1,650 in 2012 values). This, in turn, can likely be a reflection of larger income shocks to workers dismissed from more advanced job posts, a situation likely to be more common among older workers. RD plots on the reduced-form effect of UI eligibility on the number of months worked in the year, for each age group, are also shown in Figure 7.

[19] Average benefits are non-zero for ineligible workers because these workers can still claim any remaining benefits liked to their previous layoff, in case they did not receive the full 5 months of benefits they ware entitled to (e.g., if they found a job less than 5 months after this first layoff).

Columns (5) through (9) show the impacts of UI take-up on the probabilities of enrollment in private health insurance plans, admissions to public hospitals, and deaths, up to one year after layoff. As mentioned, the two latter outcomes are divided between external and non-external causes causes. Our results are as follows. First, we find no economically relevant increase in health insurance enrollment: only a small negative effect on workers below 30 years of age, statistically significant at the 10% level (quantified as a 6% decrease in relation to the baseline of workers barely ineligible to claim UI). This result contrasts with the clear decrease in labor supply quantified in column (2), and suggests that the risk of potentially losing access to employer-sponsored health insurance is not a significant part of the labor-leisure tradeoff incurred by UI claimants in our sample. Furthermore, the lack of an increase in health insurance enrollment suggests that non-employer-sponsored health insurance options (such as those in the individual market) are not sensible the income effect of UI.

Second, we find robust evidence for a negative impact of UI take-up on the probability of hospital admissions due to external causes (column 6). The estimate in Panel A shows that, in the unrestricted sample, this effect is quantified as a 29% decrease (p-value$= 0.100$) in comparison to the baseline of ineligible workers. Estimates in Panel B are larger and more precisely estimated, showing an impact equivalent to a 85% decrease in comparison to the baseline of ineligible workers, thus suggesting that the overall effect is mostly driven by older workers in our sample. This effect is also robust to a range of different functional forms and specifications, as reported in Table D1. The correspondent reduced-form effects of UI eligibility are shown in Figure 7, with the discontinuity estimates represented at the right figures from panel Panel A and B, but more clearly visualized for the latter.

As pointed out in Kuka (2020), two different channels are likely to explain the possible effects of UI on health. First, an income effect could be observed whereby UI benefits prompt individuals to make personal investments related to their health, either to the support (e.g., enrollment in supplemental health insurance) or to the detriment of it (e.g., increased consumption of harmful substances, such as smoking products and alcohol). To the extent that any effects on health insurance enrollment give us some measure of such investments in the observable, extensive margin, our results do not give us evidence that effects are mediated by the first of these channels. Likewise, no support is found for the second channel, in the probability of hospitalizations do not increase following the positive income shock.

Second, UI may help relieve the stress of economic uncertainty that is associated with job loss, which in turn could lead to a decrease in the probability of hospitalization due to causes typically related with it. The estimated reduction in hospitalizations due to external causes, such as those that are associated with stress and risk behavior, makes this channel one of possibly higher relevance – specially as it seems to affect the older, male population, whose larger share is more likely to be in charge of the financial responsibilities of a household and, at the same time, is at higher risk of facing greater difficulties in finding new jobs than younger workers, on average. The estimated decrease in labor supply (i.e., the lower number of months worked in the year following a layoff) also points to this direction, as the temporary financial support provided by UI seems to disincentivize more strenuous (and possibly more stressful) job search efforts. At the same time it suggests that workers are not using their extra time off work to engage or get involved in activities that could be harmful to their personal health. Finally, the fact that this effect is discernible as much as one years after job loss suggests that the benefits window that immediately follows it (i.e. between 1 to 5 months) may be critical, and that providing laid-off workers with financial support during such interval can be an effective way to mitigate the longer-term health impacts of job loss.

6 Conclusion

We construct a novel dataset that combines detailed, individual-level information on employment spells for the universe of Brazilian workers with their hospitalization records in the nation's universal health care system across a 17-year time span. With this data we conduct a comprehensive causal analysis on the health impacts of job loss in the context of a developing economy with mixed (i.e. public and private) systems of health care provision. We document that losing a job causes a decrease in the probability of enrollment in private, employer-sponsored health insurance plans (between 16% and 20%), an increase in the probability of admission to public hospitals (15%) and an increase in the risk of mortality (37%), the latter two on male workers only. Estimates suggest that the effects on hospitalization and mortality are mostly driven by causes related to psychological stress and risk behavior, and that the increase in hospitalization is primarily due to direct effects on individual's health (rather than plain substitution from private to public health care options).

Turning to policy implications, we also show that unemployment benefits (UI), to a large extent, mitigates the adverse health effects of job loss for particular demographic groups. More specifically, we show that UI take-up at the eligibility margin reduces emergency admissions associated with risk behavior (injuries, accidents, and assaults) for older male workers. These effects are persistent and seem to last well beyond the benefits window, suggesting that temporary financial assistance to displaced workers in the first critical months of unemployment can co a long way in supporting those individuals most susceptible to health shocks associated with liquidity constraints.

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.

Bhalotra, S., R. Rocha, and R. R. Soares, “Does Universalization of Health Work? Evidence from Health Systems Restructuring and Maternal and Child Health in Brazil,” *WP*, 2016.

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.

Card, D., R. Chetty, and A. Weber, “The spike at benefit exhaustion: Leaving the unemployment system or starting a new job?,” *American Economic Review*, 2007, *97* (2), 113–118.

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.

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

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

Eliason, M. and D. Storrie, "Does job loss shorten life?," *Journal of Human Resources*, 2009, *44* (2), 227–302.

_ **and** _ , "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.

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

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

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.

Imbens, G. W. and D. B. Rubin, *Causal Inference in Statistics, Social, and Biomedical Sciences.*, Cambridge University Press, 2015.

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

Katz, L. F. and B. D. Meyer, *Unemployment insurance, recall expectations, and unemployment outcomes.*, Vol. 105 1990.

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.

Lalive, R., "Unemployment benefits, unemployment duration, and post-unemployment jobs: A regression discontinuity approach.," *American Economic Review*, 2007, *97* (2), 108–112.

Lynge, E., "Unemployment and cancer: a literature review.," *IARC Scientific Publications*, 1997, *138*, 343–351.

McCrary, J., "Manipulation of the running variable in the regression discontinuity design: A density test," *Journal of Econometrics*, 2008, *142* (2), 698–714.

Paim, J., C. Travassos, C. Almeida, L. Bahia, and J. Macinko, "The Brazilian health system: history, advances, and challenges.," *Lancet*, 2011, *377* (9779), 1778–1797.

Ruhm, C. J., "Are workers permanently scarred by job displacements?," *American Economic Review*, 1991, *81* (1), 319–324.

_ , "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.

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

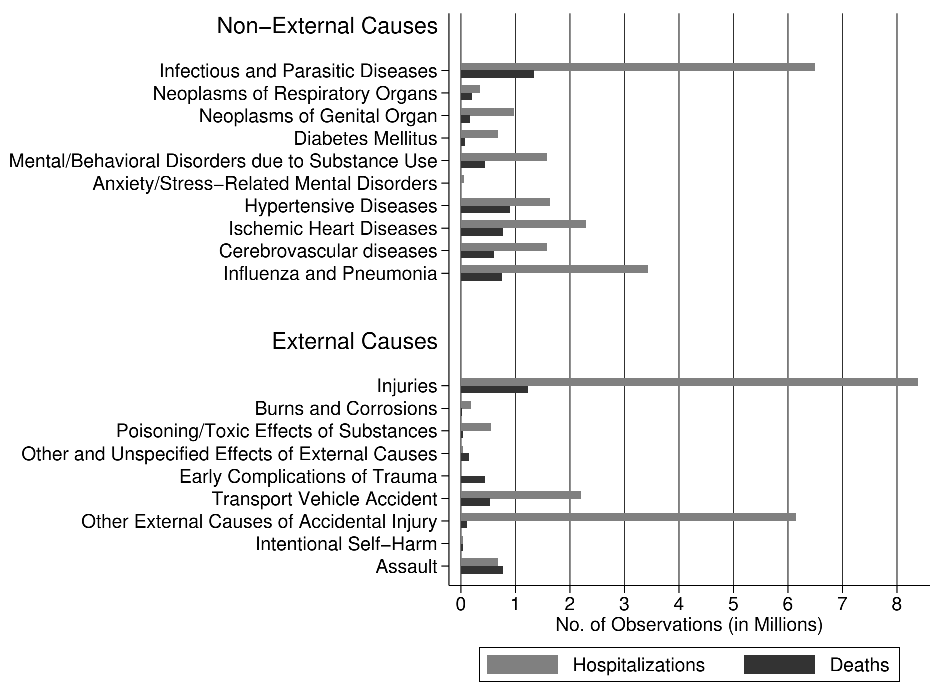
Ulyssea, G., "Firms, informality, and development: Theory and evidence from Brazil," *American Economic Review*, 2018, *108* (8), 2015–47.

WHO, "Noncommunicable Diseases: WHO Global NCD Action Plan 2013-2020," 2021.

Figure 1: Summary Statistics on Employment, Hospitalization and Mortality

(a) Employment and Mortality Rates by Year

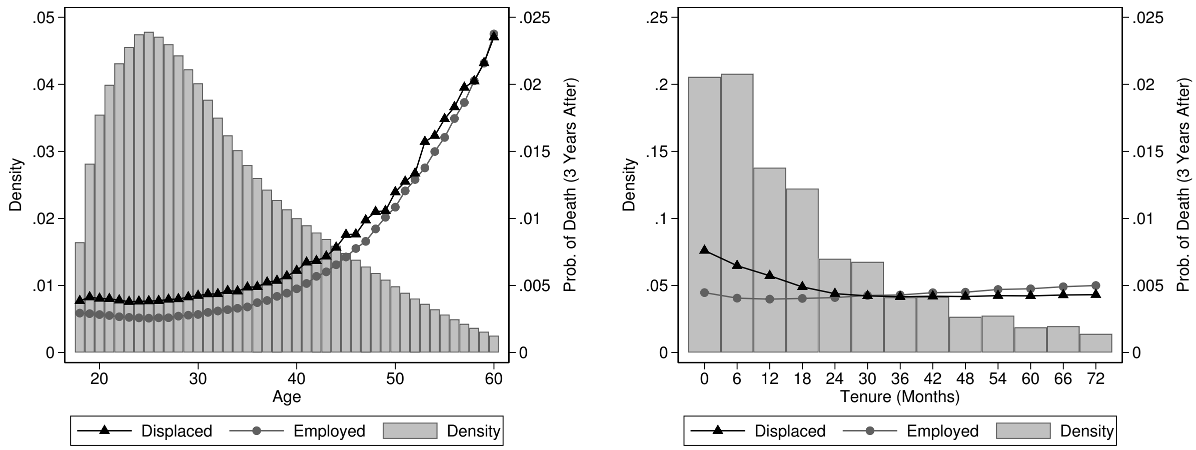
(b) Hospitalization and Deaths by Cause



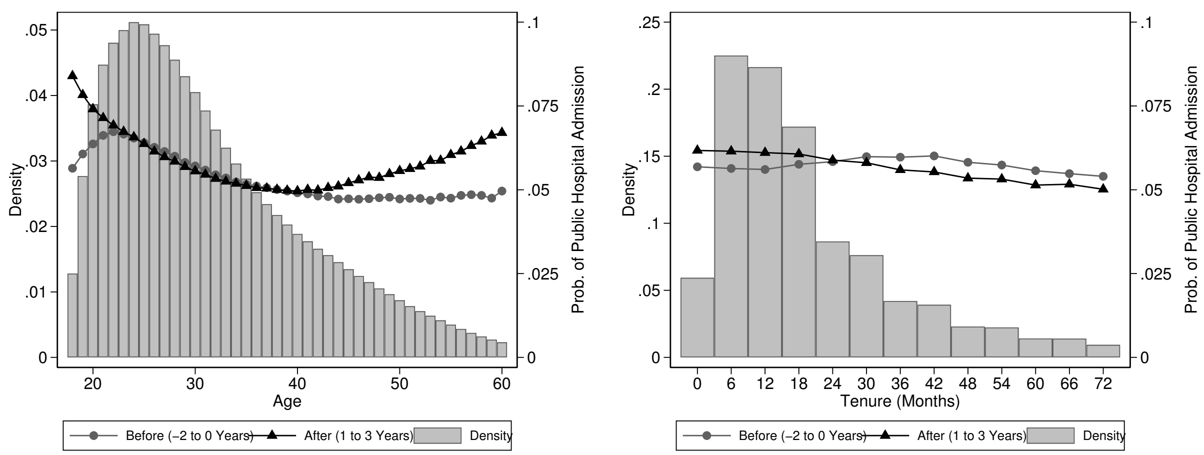
Notes: Panel (a) shows the evolution in the adult mortality rate for the Brazilian population between 1997 and 2018 superposed with the evolution in the rate of total employment during the same period. Panel (b) shows the total number of public-sector hospitalizations (gray bars) and deaths (black bars) of adults aged 18 to 65 years old in Brazil between 2002 and 2018. These are decomposed by the leading causes according to ICD-10 classification, which are grouped into either external or non-external causes.

Figure 2: Mortality and Hospitalization by Age, Tenure and Employment

(a) Prob. of Death (3 Years After), Displaced vs. Employed Workers

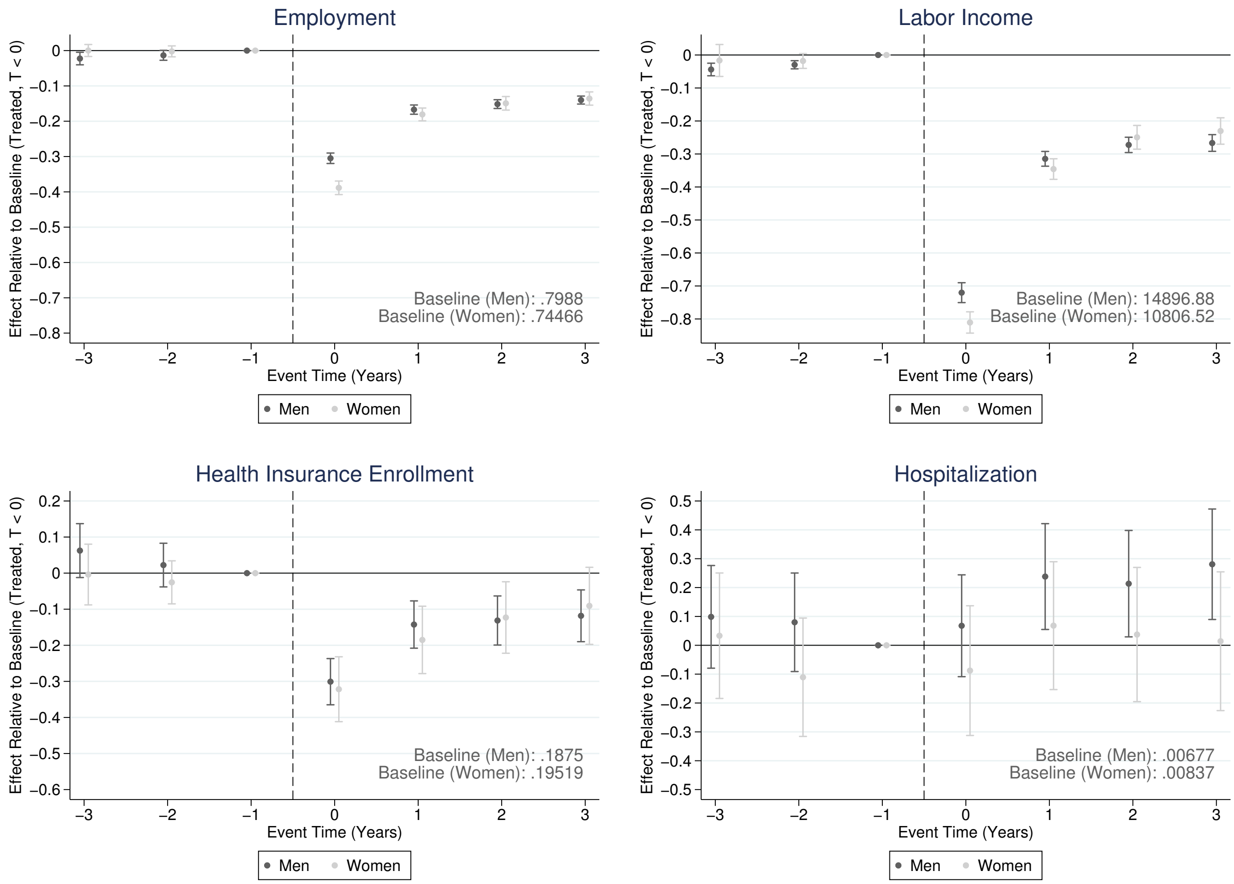


(b) Prob. of Public Hospital Admission (3 Years Before vs. After), Displaced Workers



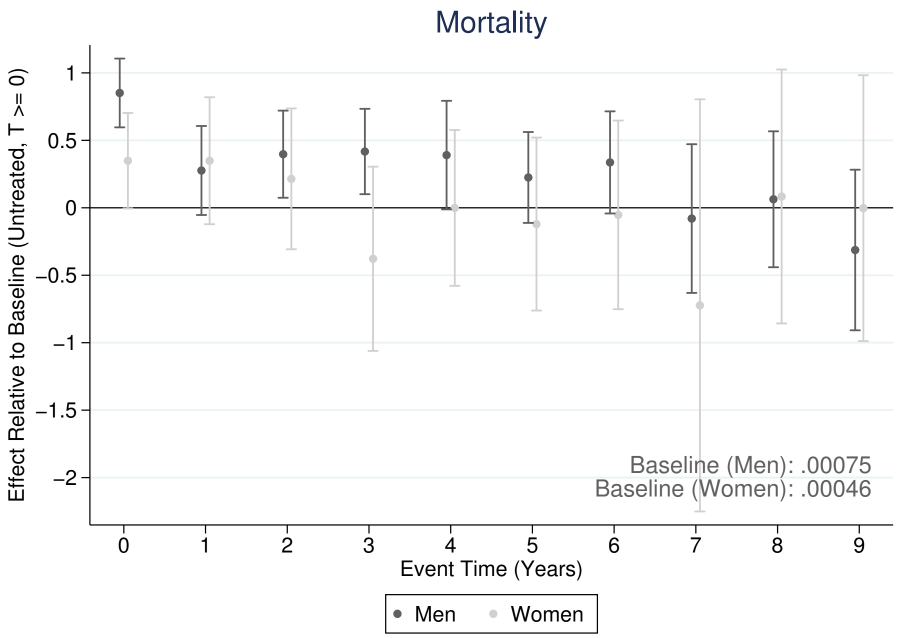
Notes: This figure shows the probabilities of hospitalization and death for different age and tenure groups, for the years between 2006 and 2014. The sample includes both male and female, full-time workers in the non-agricultural, private sector. Panel (a) displays the cumulative probabilities of death in the three following years, both for workers who were displaced at a given year and for those who remained employed at that same year. Panel (b) displays the cumulative probabilities of admission to public hospitals for displaced workers only, both for the three years following dismissal and the two years before dismissal.

Figure 3: Effect of Job Loss on Employment, Income, Health Insurance 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), and are re-scaled by the baseline outcome for each group (i.e. the estimated effect in the respective treatment group at $t < 0$). 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 Brazilian Reais.

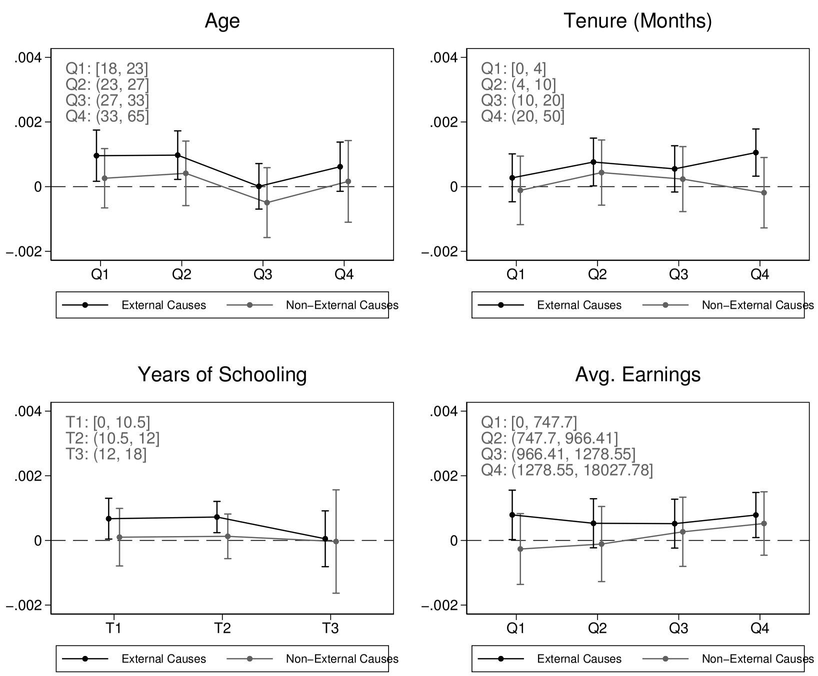
Figure 4: Effect of Job Loss on Mortality



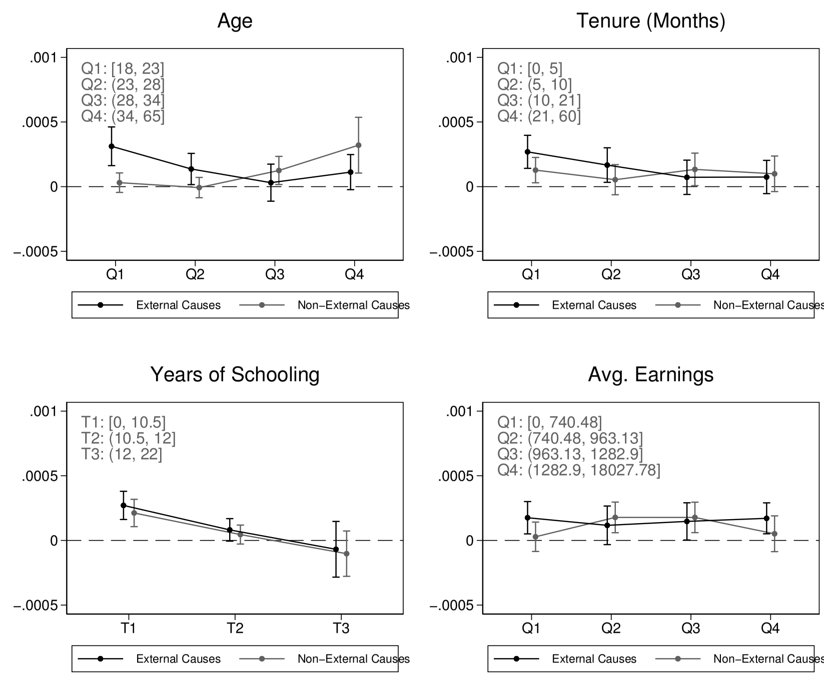
Notes: This figure shows the dynamic treatment effects of job loss due to a mass layoff on the probability of death. Outcomes are shown separately for both male (dark gray) and female workers (light gray), and are re-scaled by the baseline outcome for each group (i.e. the estimated effect in the respective treatment group at $t < 0$). Estimates were computed using the matching-based 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. Income variables are measured in Brazilian Reais.

Figure 5: Main Effects of Job Loss, by Individual Demographic Quartiles

(a) Hospitalization



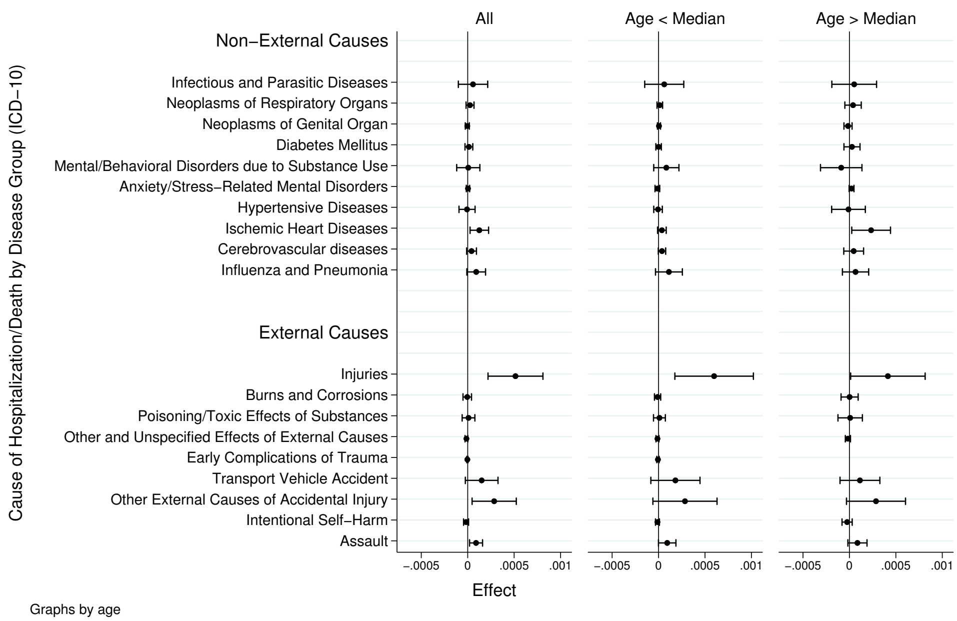
(b) Mortality



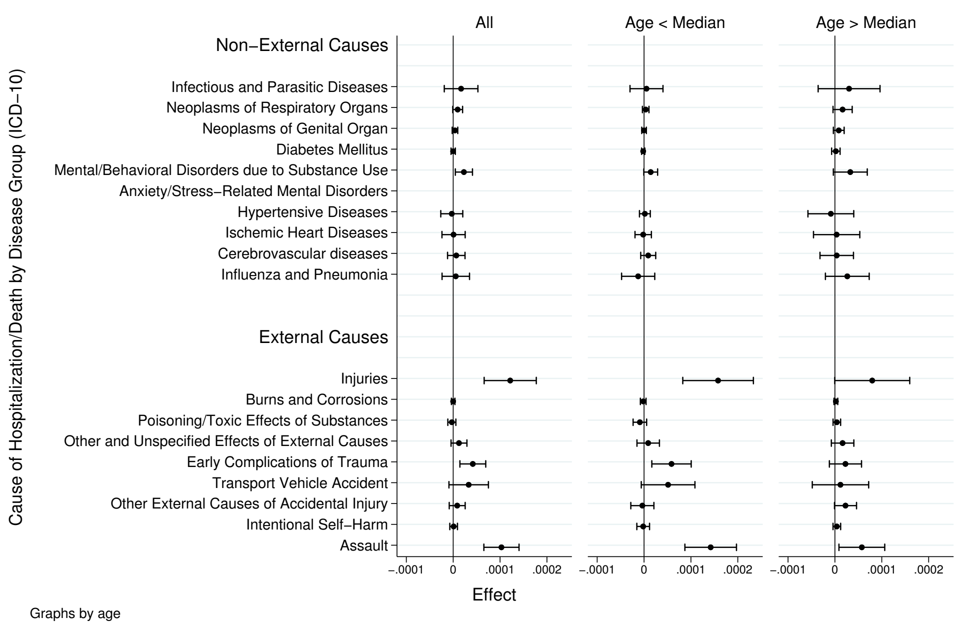
Notes: This figure shows the estimated effects (and confidence intervals) of job loss on public-sector hospitalizations (Panel A) and mortality (Panel B) for different quartiles of each indicated individual characteristic. All estimates and confidence intervals are computed using the sample for male 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).

Figure 6: Main Effects of Job Loss, by Diagnosis Groups

(a) Hospitalization



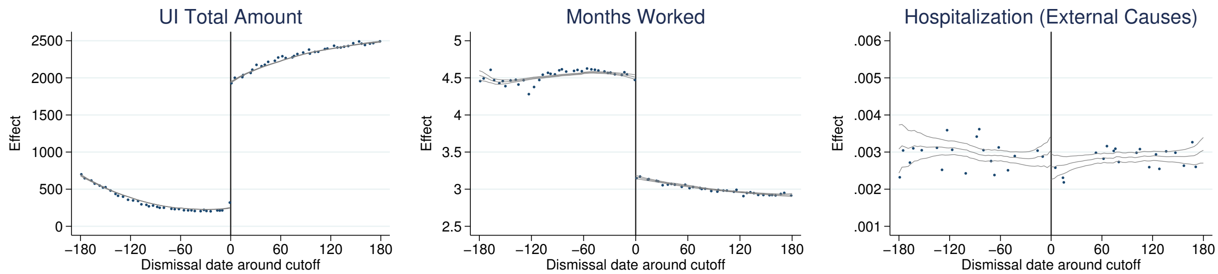
(b) Mortality



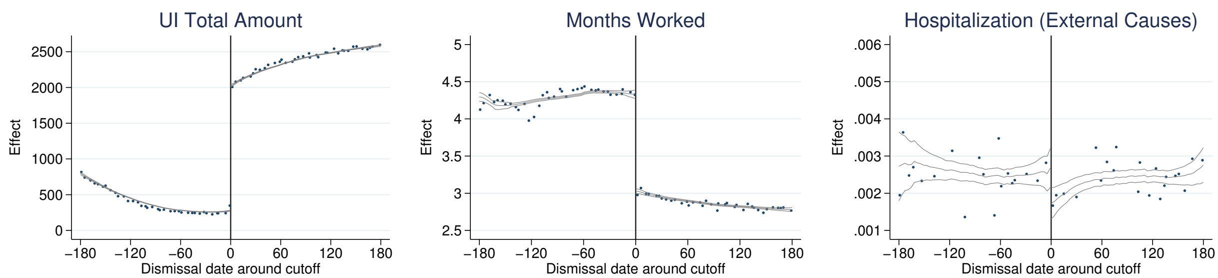
Notes: This figure shows the estimated effects (and confidence intervals) of job loss on public-sector hospitalizations for different diagnoses (Panel A) and on mortality for different causes of death (Panel B), as defined at the International Classification of Diseases (ICD-10). All estimates and confidence 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).

Figure 7: Main Effects of UI Eligibility

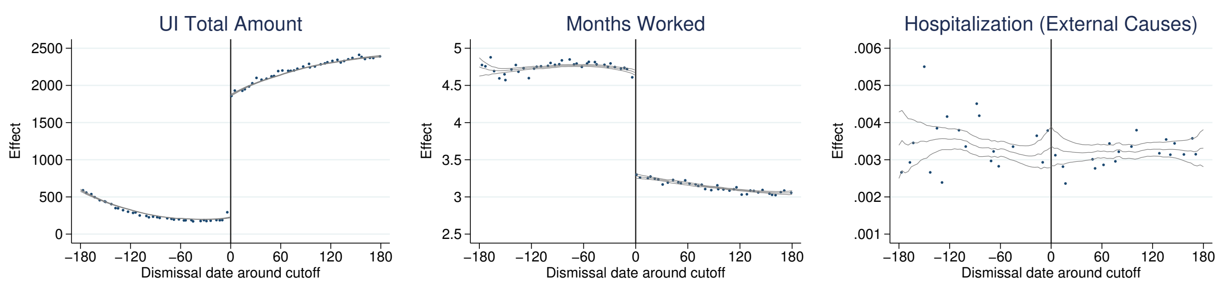
(a) All Ages



(b) Age > Sample Median



(c) Age ≤ Sample Median



The graphs plot the averages around the eligibility cutoff for: the total amount of claimed benefits, the total number of months worked up to one year after layoff, and the probability of hospitalization due to external causes up to one year after layoff. Panel (a) shows plots for all male workers in the sample, Panel (b) for those below 30 years of age (the sample median) and Panel (c) for those above 30 years of age. 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.

Table 1: 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
Firm Pre-Treatment Rates									
Layoff Rate ($=-1$)	0.17	0.17	-0.13	0.16	0.17	-0.13	0.16	0.17	-0.13
Layoff Rate ($=-2$)	0.16	0.16	-0.06	0.16	0.16	-0.05	0.16	0.16	-0.06
Layoff Rate ($=-3$)	0.15	0.16	-0.08	0.15	0.16	-0.09	0.15	0.16	-0.09
Mortality Rate ($=-1$)	0.000405	0.000015	0.03	0.000102	0.000015	0.01	0.000100	0.000015	0.01
Mortality Rate ($=-2$)	0.000016	0.000017	0.00	0.000015	0.000016	-0.01	0.000015	0.000016	-0.01
Mortality Rate ($=-3$)	0.000046	0.000030	0.01	0.000052	0.000028	0.01	0.000044	0.000028	0.02

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).

Table 2: Effects of Job Loss on Labor Market Outcomes

	(1)	(2)
	Employment	Earnings
	Panel A: Men	
$Treat_i \times Post_i$	-0.143175***	-4070.20***
	(0.003602)	(129.17)
Mean Outcome (Treated at $t < 0$)	0.7962	11,074.11
Effect Relative to the Mean	-18%	-37%
Observations	2,014,691	2,014,691
	Panel B: Women	
$Treat_i \times Post_i$	-0.166871***	-3214.42***
	(0.006563)	(126.56)
Mean Outcome (Treated at $t < 0$)	0.7792	8,121.63
Effect Relative to the Mean	-21%	-40%
Observations	1,120,350	1,120,350

Notes: This table shows the effect of job loss due to a mass layoff on employment (column 1) and labor earnings (column 2). 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 $_i$ equal to 1 for treated workers, interacted with a dummy $_t$ 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 parenthesis. ***, ** and * represent $p < 0.01$, $p < 0.05$ and $p < 0.1$ respectively.

Table 3: Effects of Job Loss on Health Insurance Enrollment

	(1)	(2)	(3)
	All Plans	Enrollment by Plan Type	
		Corporate	Individual
	Panel A: Men		
$Treat_i \times Post_i$	-0.025955***	-0.021841***	-0.000309
	(0.003378)	(0.002990)	(0.000447)
Mean Outcome (Treated at $t < 0$)	0.1294	0.0700	0.0045
Effect Relative to the Mean	-20%	-31%	-7%
Observations	649,236	649,236	649,236
	Panel B: Women		
$Treat_i \times Post_i$	-0.021813***	-0.022861***	0.002377***
	(0.004433)	(0.004517)	(0.000815)
Mean Outcome (Treated at $t < 0$)	0.1408	0.0729	0.0100
Effect Relative to the Mean	-16%	-31%	23%
Observations	348,740	348,740	348,740

Notes: This table shows the effect of job loss due to a mass layoff on enrollment in private health insurance plans, both overall (column 1), and separately for either corporate (i.e. employer-sponsored) and individual plans (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 $_i$ equal to 1 for treated workers, interacted with a dummy $_t$ 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 parenthesis. ***, ** and * represent $p < 0.01$, $p < 0.05$ and $p < 0.1$ respectively.

Table 4: Effects of Job Loss on Hospitalization

	(1)	(2)	(3)	(4)	(5)
	Overall	by Type of Admission		by Cause	
		Emergency	Non-Emerg.	External	Non-Ext.
	Panel A: Men				
$Treat_i \times Post_i$	0.000894***	0.001026***	-0.000057	0.000620***	0.000320
	(0.000319)	(0.000243)	(0.000216)	(0.000179)	(0.000266)
Mean Outcome (Treated at $t < 0$)	0.0059	0.0034	0.0028	0.0018	0.0043
Effect Relative to the Mean	15%	30%	-2%	33%	7%
Implied Elasticity to Employment	-0.83	-1.67	0.11	-1.83	-0.39
Implied Elasticity to Earnings	-0.41	-0.81	0.05	-0.89	-0.19
Observations	1,306,347	1,306,347	1,306,347	1,306,347	1,306,347
	Panel B: Women				
$Treat_i \times Post_i$	0.000246	0.000050	0.000243	0.000365***	-0.000001
	(0.000495)	(0.000380)	(0.000313)	(0.000139)	(0.000471)
Mean Outcome (Treated at $t < 0$)	0.0076	0.0045	0.0032	0.0005	0.0071
Effect Relative to the Mean	3%	1%	8%	78%	0%
Implied Elasticity to Employment	-0.14	-0.05	-0.38	-3.71	0
Implied Elasticity to Earnings	-0.08	-0.03	-0.2	-1.95	0
Observations	700,693	700,693	700,693	700,693	700,693

Notes: This table shows the effect of job loss due to a mass layoff on the probability of admission to a public hospital, both overall (column 1), and separately by type of admission (columns 2-3) and diagnoses groups (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 $_i$ equal to 1 for treated workers, interacted with a dummy $_t$ 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 parenthesis. ***, ** and * represent $p < 0.01$, $p < 0.05$ and $p < 0.1$ respectively.

Table 5: Effects of Job Loss on Mortality

	(1)	(2)	(3)
	Overall	by Cause	
		External	Non-Ext.
	Panel A: Men		
$Treat_i$	0.000257***	0.000165***	0.000092***
	(0.000048)	(0.000034)	(0.000033)
Mean Outcome (Untreated at $t \geq 0$)	0.0007635	0.0000598	0.0005936
Effect Relative to the Mean	37%	276%	15%
Implied Elasticity to Employment	-2.06	-15.33	-0.83
Implied Elasticity to Earnings	-1.00	-7.48	-0.41
Observations	2,097,017	2,097,017	2,097,017
	Panel B: Women		
$Treat_i$	-0.000007	0.000025*	-0.000033
	(0.000051)	(0.000015)	(0.000049)
Mean Outcome (Untreated at $t \geq 0$)	0.0004857	.0000469	0.0004388
Effect Relative to the Mean	-1%	53%	-8%
Implied Elasticity to Employment	0.05	-2.52	0.38
Implied Elasticity to Earnings	0.03	-1.33	0.2
Observations	1,107,874	1,107,874	1,107,874

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 diagnoses groups (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 $_i$ equal to 1 for treated workers. 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 parenthesis. ***, ** and * represent $p < 0.01$, $p < 0.05$ and $p < 0.1$ respectively.

Table 6: Effects of Job Loss on Workers' Spouses and Children

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Labor Market Outcomes		HI	Hospitalization		Mortality	
	Employment	Income	Enrollment	Ext. Causes	Non-Ext. Causes	Ext. Causes	Non-Ext. Causes
	Panel A.1: Spouse of Male Worker						
$Treat_i \times Post_i$	-0.004779	-283.45***	-0.004558	-0.000041	0.000263	0.000018	-0.000046
	(0.004994)	(102.517436)	(0.005123)	(0.000280)	(0.000999)	(0.000022)	(0.000064)
Mean Outcome (Untreated at $t \geq 0$)	0.411984	6,050.31	0.107341	0.000608	0.008691	0	0.000061
Effect Relative to the Mean	-1%	-5%	-4%	-7%	3%	-	75%
Observations	310,926	310,926	101,493	171,696	171,696	384,410	384,410
	Panel A.2: Children of Male Worker						
$Treat_i \times Post_i$	-	-	-0.006375	0.000598*	0.000790	0.000046	0.000055
	-	-	(0.007520)	(0.000350)	(0.000941)	(0.000033)	(0.000066)
Mean Outcome (Untreated at $t \geq 0$)	-	-	0.144368	0.000641	0.008996	0	0.000018
Effect Relative to the Mean	-	-	-4%	93%	9%	-	305%
Observations	-	-	132,433	193,487	193,487	256,098	256,098
	Panel B.1: Spouse of Female Worker						
$Treat_i \times Post_i$	-0.009103*	13	-0.013888**	-0.000725	0.000782	0.000181**	0.000067
	(0.005470)	(222.275030)	(0.006241)	(0.000671)	(0.001132)	(0.000090)	(0.000241)
Mean Outcome (Untreated at $t \geq 0$)	0.641568	13,765.29	0.134703	0.001886	0.007061	0.000094	0.000471
Effect Relative to the Mean	-1%	0%	10%	-38%	11%	192%	14%
Observations	176,827	176,827	54,432	93,044	93,044	186,842	186,842
	Panel B.2: Children of Female Worker						
$Treat_i \times Post_i$	-	-	-0.013011	0.001076**	0.000806	0.000073	0.000064
	-	-	(0.008050)	(0.000504)	(0.001302)	(0.000098)	(0.000060)
Mean Outcome (Untreated at $t \geq 0$)	-	-	0.149515	0.000564	0.007702	0	0
Effect Relative to the Mean	-	-	-8%	191%	105%	-	-
Observations	-	-	64,820	97,006	97,006	128,553	128,553

Notes: This table shows the effect of job loss due to a mass layoff on the probability of admission to a public hospital for children and spouses of dismissed workers. It includes estiamtes for labor market outcomes (columns 1-2, spouses only) health insurance enrolment (column 3), hospitalization due to external and non-external causes (columns 4-5), and mortality from external and non-external causes (columns 6-7). 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 $_i$ equal to 1 for treated workers, interacted with a dummy $_t$ 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 parenthesis. ***, ** and * represent $p < 0.01$, $p < 0.05$ and $p < 0.1$ respectively.

Table 7: Main Effects of UI Eligibility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	1st Stage		**2nd Stage**						
	Take-Up	Total Amount	Labor Market Outcomes		HI	Hospitalization		Mortality	
			Months Worked	Labor Income	Enrollment	Ext. Causes	Non-Ext. Causes	Ext. Causes	Non-Ext. Causes
	Panel A: All Ages								
Eligible to UI	0.545***	1,693***	-2.466***	-2,990***	-0.00363	-0.000801*	0.000405	-0.000345	0.000026
	(0.00164)	(5.484)	(0.0369)	(63.47)	(0.00301)	(0.000487)	(0.000712)	(0.000217)	(0.000160)
Mean Outcome (at Cutoff)	-	-	3.669	5,504.29	0.113314	0.002805	0.005973	0.000808	0.000311
Effect Relative to the Mean	-	-	-67%	-54%	-3%	29%	7%	-43%	8%
Observations	927,760	927,760	927,760	927,760	396,419	625,829	625,829	927,760	927,760
	Panel B: Age > Sample Median (30 Years Old)								
Eligible to UI	0.535***	1,747***	-2.427***	-3,332***	0.000724	-0.001807***	0.000887	-0.000317	0.000034
	(0.00241)	(8.380)	(0.0568)	(110.7)	(0.00435)	(0.000664)	(0.001166)	(0.000316)	(0.000312)
Mean Outcome (at Cutoff)	-	-	3.497	5,999.49	0.104064	0.002155	0.006897	0.000616	0.000431
Effect Relative to the Mean	-	-	-69%	-56%	-1%	84%	13%	-51%	8%
Observations	442,291	442,291	442,291	442,291	180,766	290,035	290,035	442,291	442,291
	Panel C: Age $\leq$ Sample Median (30 Years Old)								
Eligible to UI	0.555***	1,644***	-2.506***	-2,681***	-0.00723*	0.000023	0.000037	-0.000372	0.000029
	(0.00225)	(7.167)	(0.0482)	(69.01)	(0.00415)	(0.000700)	(0.000873)	(0.000299)	(0.000121)
Mean Outcome (at Cutoff)	-	-	3.84380	5,000.56	0.121831	0.003429	0.005085	0.000710	0.000118
Effect Relative to the Mean	-	-	-65%	-54%	-6%	1%	1%	-52%	25%
Observations	485,469	485,469	485,469	485,469	215,653	335,794	335,794	485,469	485,469

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 remaining columns show the second-stage effects of UI take-up on labor market outcomes (columns 3 and 4), on the probability of enrollment in private health insurance plans (column 5), and on the probability of hospitalization (columns 6 and 7) and death (columns 8 and 9), the latter two 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 for eligibility to 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 firm level are indicated in parenthesis. ***, ** and * represent $p < 0.01$, $p < 0.05$ and $p < 0.1$ respectively.

A Appendix to Section 2

A.1 Disaggregated Mortality Trends

The right panel in Figure A1 decomposes this yearly mortality rate for the five leading causes of death according to ICD-10 disease chapters, with all other chapters grouped into "other". Paralleling worldwide trends in low- and middle-income countries (WHO, 2021), circulatory diseases, including hypertension and ischemic diseases, account for the majority of deaths in the country, with 1.72 deaths per 1,000 individuals in 2018. These are followed by the various types of neoplasms (i.e., cancer) with 1.09 deaths per 1,000 individuals, and respiratory diseases with 0.74 deaths per 1,000 individuals. All three groups also demonstrated significant growth in their rates during the period of analysis, with the incidence of circulatory diseases increasing by 9.82%, that of neoplasms by 55,60%, and respiratory diseases by 37,38%. Other major causes of death include those classified as related to external causes (e.g., injuries, accidents, intentional self-harm and assault), with with 0.72 deaths per 1,000 individuals in 2018, and those related with mental disorders with with 0.39 deaths per 1,000 individuals (the latter representing a growth of 48,89% from 1999 figures).

[Figure A1 about here.]

A.2 Interaction between Public and Private Health Care

Figure A2 depicts descriptive evidence on two important features of the supplemental (i.e., private) health care sector in Brazil. First, it shows the year-to-year variations in formal employment and enrollment in private health insurance plans in the coutry bewteen 2004 and 2018. This is suggestive of the correlation between both variables and hints to the higher share of employer-sponsored insurance plans in the supplemental health care market. Second, it also plots on top of both these variables the total number of admissions in public hospitals by individuals who, at the time of admission, were also enrolled in private insurance plans. Its pattern demonstrates some level of interaction between both systems, possibly countercyclical with formal employment.

[Figure A2 about here.]

A.3 Differences in Formal and Informal Labor Earnings

In our main analysis of job loss in Section 4, we leverage from 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 2) 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 the National Survey by Household Sample (*Pesquisa Nacional por Amostra de Domicílios* – PNAD), 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 includes a longitudinal component that tracks a substantial portion of the interviewed households for five consecutive quarters. The second dataset is the Single Registry (*Cadastro Único* – CU). It contains detailed information on all participants of government financial assistanship programs in the country and is thus representative of the lowest-income strata of the population – the one more likely to transition to the informal labor market following a job loss.

In Figure A3 we plot the average labor income trajectories of individuals employed at time $t = 0$ and were out of employment at time $t = 1$, separately for each gender and each different dataset. Panel (a) shows that in the representative sample, the effect on formal earnings in the last quarter (i.e. approximately one year after the loss of employment) is smaller than the ones estimated in Section 4, at approximately 44% and 55% for males and females, respectively. When accounting for informal jobs, these effects are somewhat smaller for both genders (34% and 48%, respectively). This suggests that the difference between formal and informal labor earnings estimates in the first year is quantified only in about 7 to 10 percentage points. Results for the low-income sample shown in Panel (b) suggests a higher discrepancy between estimates in subsequent years, with formal earnings losses of 72% for males and 79% for females by the fourth year after the loss of employment, and of 53% for males and 60% for females when accounting for informal jobs. This suggests a difference between estimates of about 19 percentage points.

[Figure A3 about here.]

B Appendix to Section 3

B.1 Merging Records Across Datasets

All observations in both the labor (RAIS) and residential addresses (RF) datasets contain a unique tax-code identifier (CPF) for each individual in the sample, which we use to match them across these two different datasets. Such matching exercise performs very well in terms accuracy, with 99.09% of individuals in the labor data being matched to their yearly residential addresses. The hospitalization and mortality datasets from DATASUS, and the health insurance enrollment from ANS, however, are de-identified to preserve individual privacy and prevent the misuse of sensitive information. This poses a major challenge to our analysis, which the residential addresses data helps us overcome to a large extent. In what follows we describe the general procedure used to match individual workers' information with their hospitalization, health insurance, and mortality records.

We match workers with their hospitalization records by linking individual observations between the labor and hospitalization datasets. First, in the residential addresses data (linked to each worker in the labor data) we record how many individuals are contained within every separate cell uniquely defined by a given zip code, gender, and date of birth, for every year in our sample. Then we drop from the sample all individuals who share a same zip-code/gender/date-of-birth cell with another individual, thus keeping only those who are uniquely identified in the sample by their respective cell. This still preserves 75.97% of the original observations in this dataset. Finally, we match every hospitalization record from a given zip-code/gender/date-of-birth cell to the unique individual in that same cell in the residential addresses data, which in turn links to a single worker in the labor data.

Differently from the hospitalization data, the health insurance data does not contain individuals' zip-code of residence but their *borough* of residence as smallest geographical identifier.[20] In this sample we thus restrict observations to individuals who are uniquely identified by each borough/gender/date-of-birth cell, which preserves 57.13% of the observations in the original worker-addresses sample. All observations on health insurance enrollment corresponding to that same cell are then matched to the ones in the workers data.[21]

[20]Zip codes and boroughs vary in size depending on locality, and the former is always contained in the latter. In big urban centers, a zip code commonly identifies a single street, whereas a borough comprehends a collection of them. In rural areas, a whole municipality usually corresponds to one or two zip codes/boroughs only.

[21]A further drawback in the health insurance data is that the information on boroughs are based on manual inputs and thus contain several misspellings and missing records. Although no systematic patterns are observed, this feature of the data adds some degree of measurement error when combining it with other datasets. We overcome this by using a fuzzy matching algorithm to link these records to an official registry on boroughs from the national postal service of Brazil (*Correios*) before matching it with the labor data.

The main assumption behind these procedures is that since there is now only one individual for each constructed cell in the residential addresses data (which, as mentioned, encompasses about three-fourths of the Brazilian population), there is a high probability that the record corresponding to that same cell in both the health insurance data and the hospitalization data (which are nationally comprehensive) are associated to that single individual. Following this procedure, 76.16% of all observations in the hospitalization data and 36.27% of those in the health insurance data are then matched to the labor data.

We match workers with mortality records following a similar procedure. Out of the individuals marked as deceased in the residential addresses data, we select only those who are unique within a cell defined by a given municipality, gender, date of birth and year of death (the mortality data, mentioned next, does not contain records on individuals' zip code or borough of residence). We do the same with the mortality data, and match the remaining observations in the latter with the single individuals contained at each cell in the former. With this approach we are able to add vital information contained in the mortality database (e.g. the dates of death and their causes/circumstances) to 63.94% of deceased workers our labor data.

Differently from the construction of the other samples, here we do not drop observations that are not unique within a single cell, since each cell is defined only for a sub-section of the main sample (i.e. those who were deceased), but in doing so we also admit a higher probability of false-negatives in our empirical exercises (i.e. observations for individuals who are marked as deceased but do not contain their dates of death nor their causes/circumstances). Fortunately, in this case we are able to precisely quantify the extent of such measurement error, and conduct robustness exercises that rely only on the year the individual has died as our main outcome variable.

C Appendix to Section 4

C.1 Short- vs. Long-Term Effects of Job Loss

In view of the patterns observed in Figure 4, for each outcome in Figure 6 we also distinguish the short-term effect (i.e. that corresponding to the first year after layoff) from the long-term effect (i.e. that corresponding to all years after the first year following layoff). More specifically, we estimate each different effect by adapting equation (1) in the following way:

$$\begin{aligned} Y_{it} = \alpha + \delta\, Treat_i + \beta_{ST}\, Treat_i \cdot Post_{(t=0)} + \lambda_{ST}\, Post_{(t=0)} \\ + \beta_{LT}\, Treat_i \cdot Post_{(t\geq 1)} + \lambda_{LT}\, Post_{(t\geq 1)} + \epsilon_{it} \end{aligned} \tag{4}$$

where the coefficients β_{ST} and β_{LT} represent short- and long-term effects, respectively. As in Section 4, we adapt equation (4) above to a matching-based equation to estimate the effects on mortality. Results are shown in Figure C1.

[Figure C1 about here.]

C.2 Mediation Analysis of Private Health Insurance

The mixed character of public and private health care provision in Brazil raises the possibility that the direct effects of job loss on public hospital admissions is partially confounded by workers' substitution of private for public care. This is specially relevant in our context given that private care is most commonly acquired through employer-sponsored health insurance plans offered to a parcel of workers. In this section, we provide further insight into such mechanism with a mediation analysis inspired in Gelbach (2016) and as recently used in other empirical works (e.g., Sorrenti et al., 2020, Breivik and Costa-Ramón, 2022).[22,23]

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:

$$\frac{dY_t}{d\,(Treat \cdot Time_t)} = \frac{\partial Y_t}{\partial HI_t} \cdot \frac{\partial HI_t}{\partial\,(Treat \cdot Time_t)} + R_t, \tag{5}$$

[22] 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.

[23] 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 B).

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):

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

Finally, using expression (5) 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 C2. 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 8% of the effect is possibly mediated by the concurrent impact on access to private health insurance, while in subsequent periods this effects decrease to below 3%. Panel (b) reports the same percentages on a restricted sample of treated workers (together with their matched counterpart in the control group) who, at the time of layoff, were enrolled in a private health insurance plan. For this subsample the mediating impact of health insurance is much higher: about 29% in the first year, then falling to an average of 17% in subsequent years. In sum, these findings suggest that although substitution effects are likely relevant to the small share of workers who had access to private care prior to layoff,[24] they do not sufficiently explain the total impacts of job loss

[24] 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 on their health due to the very fact that they lost access to private (and possibly higher-quality) care. Although this reinforces our argument on public hospitalizations more likely reflecting direct impacts on individual health, we do not, however, explore this more nuanced mechanism.

on public hospital admissions – which are thus more likely to reflect direct impacts of job loss on the health of individuals.

[Figure C2 about here.]

C.3 Mortality Estimates using an ITT Approach

As discussed in Section 4.1, one of the main challenges in our identification approach, in which we compare individuals with different employment status after a mass layoff, is quantifying the individual-level risk of mortality prior to the employment shock (since, by construction, one must be alive at the time of layoff in order to be laid off). We circumvent this limitation with an intention-to-treat (ITT) approach, where we select individuals into treatment based on a higher probability of job displacement two years ahead (when the firm he or she works will suffer a mass layoff). More specifically, for each treated firm in our sample we assign a treatment dummy to *all* workers employed in that same firm two years *before* its mass layoff. In so doing, we are able to apply a differences-in-differences strategy (rather than rely solely on the matching strategy for post-treatment years) and show pre-trends on the individual probability of mortality for those same two years prior to treatment.

We use equations (1) and (4), respectively, to estimate the dynamic and average ITT effects here represented by the β-termed coefficients in those equations. To gain statistical power we slightly relax the choice of matching variables to include only birth cohort, tenure, and categories for earnings, industrial sector, firm size and state. We also impose a stricter definition of mass layoff, requiring that a firm dismisses at least 50% of its workforce in a same calendar year. Results on the average short- and long-term effects are shown in Table C1.

[Table C1 about here.]

As expected, the ITT impacts on both employment and earnings are lower than the average effects reported in Table 2. In the first year after the mass layoff we find an average reduction of 19% in the probability of employment for male workers, and of 22% for female workers. Both effects dissipate sharply in the three subsequent years, whose impacts are quantified in 3% and 5% for each gender group, respectively. Relative impacts on labor income are very similar across both time frames and genders (15% overall reduction). As in Section 4.3, we find positive effects on mortality on the first period for both genders, although the estimate is noisier for female workers. For male workers, more specifically, the implied elasticity to income in the first period is very close to -1, which is remarkably similar to the one suggested by Figures 3 and 4 in the main analysis. Finally, in the lower graph of Figure

C3, which shows the estimates for the dynamic effects, we see that estimated pre-trends are very close to zero.

[Figure C3 about here.]

C.4 Tests of Selection into Treatment

One potential concern with our identification strategy is the possibility that workers are being endogenously selected intro treatment (i.e. being dismissed by the firm) based on some unobservable, not-controlled-for personal characteristics, including ones related with frail health. Examples could include high absenteeism, lower productivity, higher propensity to work-related accidents, or higher spending on benefits such as disability insurance, workers compensation or subsidies to private health insurance. To address this concern we re-estimate the impacts of job loss on male workers while varying our definition of mass layoff to include different layoff percentages, minimum quantities of dismissed workers, and firm closures. Results are shown in Table C2. Reassuringly, all estimates are similar in value to the ones in our main analysis.

[Table C2 about here.]

C.5 Methodological Concerns with Staggered Treatment Timings

Several recent studies have raised concerns about the validity of difference-in-differences designs under settings where treatment is "staggered" – that is, when observations in the treated sample are assigned treatment at different points in time.[25] A general consensus in this literature is that pairwise comparisons between these observations (i.e., those treated at different points in time) can generate bias, which in turn can become severe if a larger weight is attributed to these comparisons in the calculation of the difference-in-differences estimates. However, in our setting, half our sample is by construction formed by "never-treated" observations, which *a priori* appeases some of these methodological concerns. Panel (a) in Figure C4 shows the plots of the weight decomposition proposed by Goodman-Bacon (2021) for the effect of job loss on hospitalizations using a two-way fixed effects panel model.[26] Results show that our estimate is largely not affected by such bias. As a further check, we re-estimate the same effect following the approach proposed by De Chaisemartin and

[25] See e.g., 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).

[26] In order to have a strongly balanced sample as required by the estimator, in this exercise we included only a subset of our full sample (treated and control units between 2012 and 2014). Estimated effects with this subsample using our main specification are very similar to those in the main analysis.

d'Haultfoeuille (2020), which corrects the selection of comparison units across treatment and control groups. Results, shown in Panel (b) of Figure C4, also very similar to those from our baseline approach.

[Figure C4 about here.]

D Appendix to Section 5

D.1 Additional Robustness Tests

This section includes additional figures and tables that test the validity of the empirical design and the robustness of the main estimates. Figure D1 illustrates the problem of cyclicality with dismissal dates within calendar months, which causes a lack of smoothness around the cutoff in the untreated sample. Figure D2 shows that the problem is corrected after dropping observations closer to both the beginning and end of each calendar month. Figure D3 further shows that the working sample is also balanced at the cutoff point across a series of workers' individual characteristics and industry sectors.

[Figure D1 about here.]

[Figure D2 about here.]

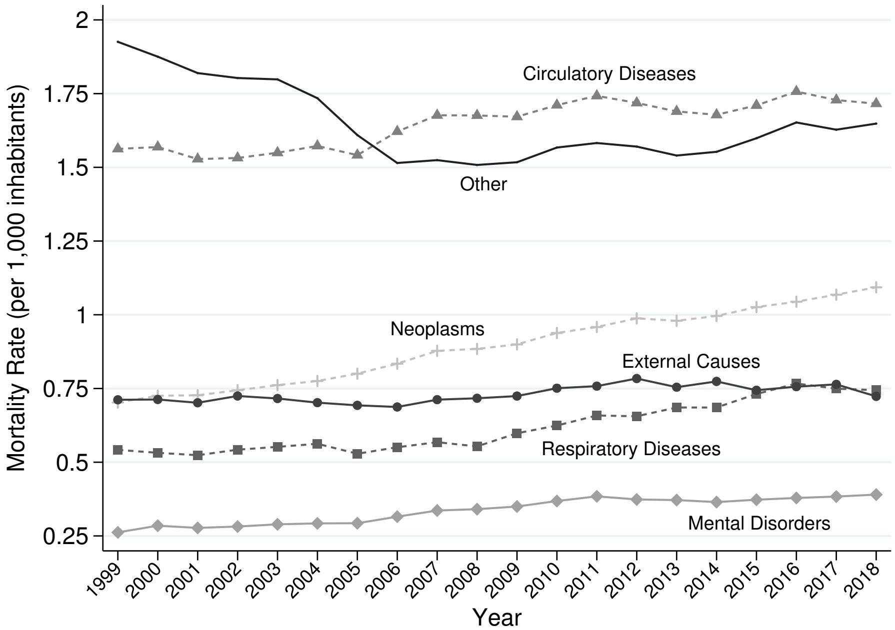
[Figure D3 about here.]

Table D1 shows that the main effects on hospitalizations from external causes are robust to different choices of polynomial orders and bandwiths, including the optimal bandiwths proposed by Calonico et al., 2014. Results remain quantitatively similar and with similar levels of significance across most specifications. They also remain particularly robust for older cohorts, as shown in Panel (b). Figure D4 in turn shows the results of permutation tests on the reduced-form effect of UI eligibility on the same outcome, both for the unrestricted sample of workers and for older workers only. This is done by assigning placebo cutoff dates around the real discontinuity and plotting the histogram of all placebo results, together with the real one. Panel (a) shows that the negative effect in the unrestricted sample falls slightly above the 5th percentile of the distribution of estimated coefficients. However, Panel (b) further confirms that the real estimated effect differs greatly from the placebo ones in the sample of older workers.

[Table D1 about here.]

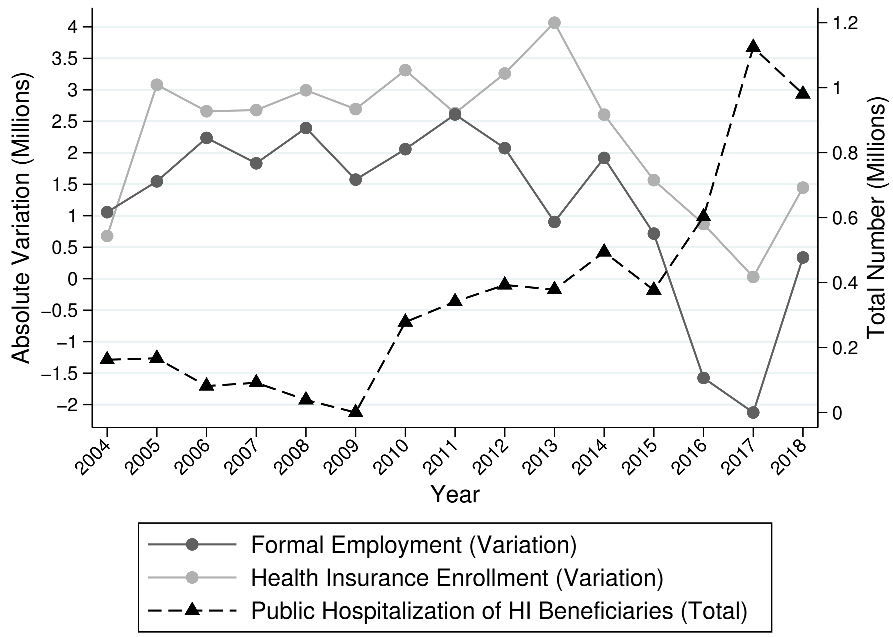
[Figure D4 about here.]

Figure A1: Disaggregated Mortality Trends



Notes: This figure the evolution in the adult mortality rate for the Brazilian population between 1999 and 2018 for the five leading causes of death according to ICD-10 disease chapters (all other chapters are grouped into "other").

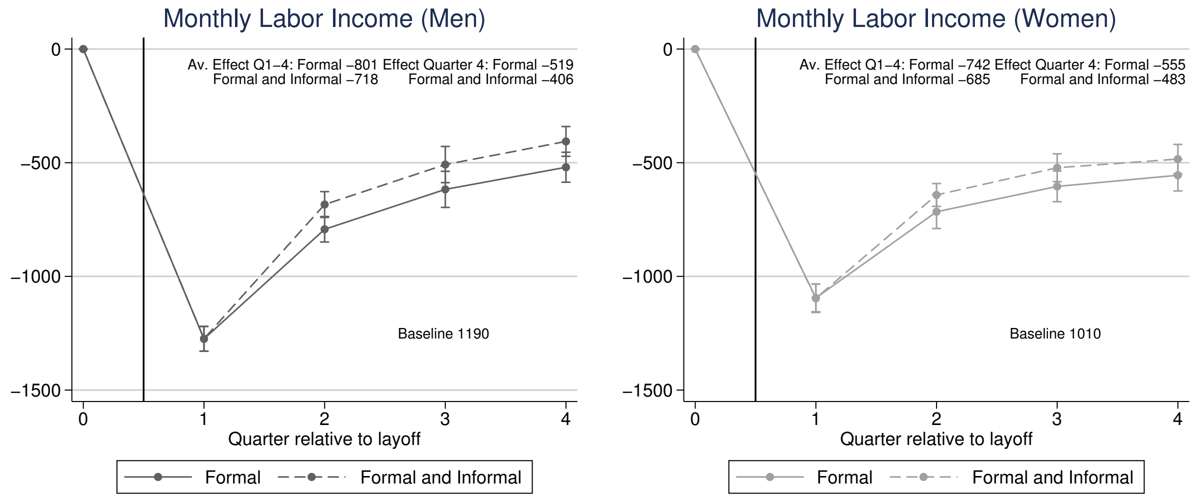
Figure A2: Dynamics in Public and Private Health Care



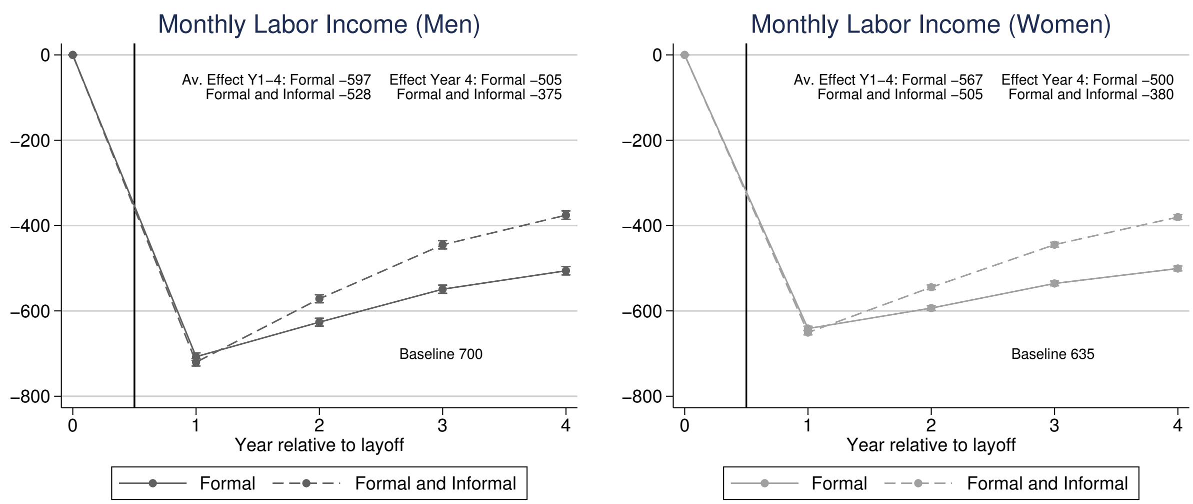
Notes: This figure plots the yearly variation in the number of formal employment and in the number of active private health insurance plans (in terms of units in the left-side axis); and the total number of public hospital admissions of individuals holding private health insurance plans (in terms of units in the right-side axis).

Figure A3: Effect of Job Loss on Formal and Informal Labor Market Outcomes

(a) PNAD Data



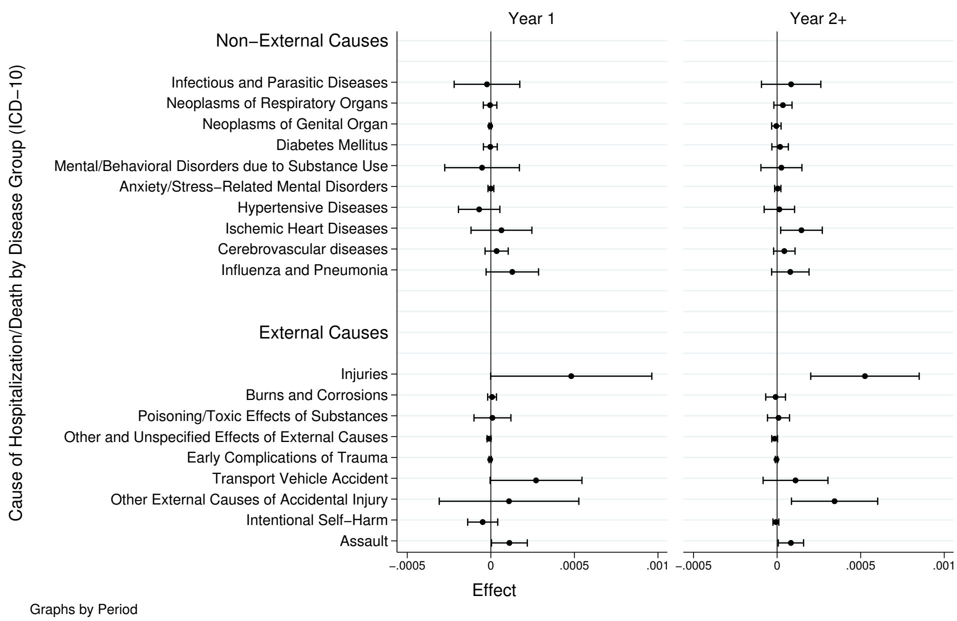
(b) CU Data



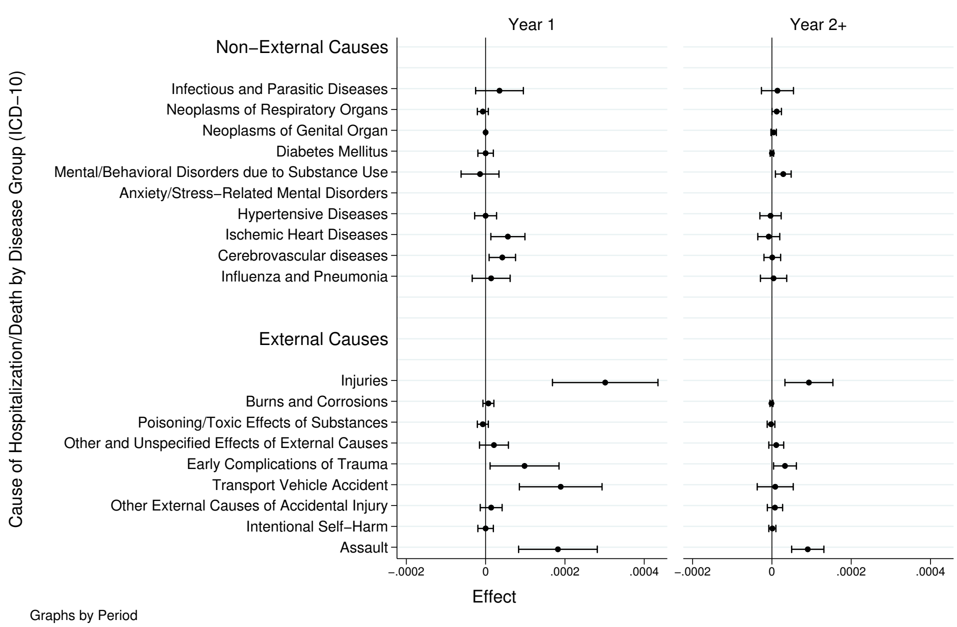
This figure shows the effect of job loss on formal and informal labor income,along with 95% confidence intervals. Panel (a) is based on PNAD longitudinal household survey data following workers for up to five quarterly interviews. Panel (b) is based on CadUn registries of individuals claiming cash welfare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

Figure C1: Short- vs. Long-Term Effects of Job Loss, by Diagnosis Groups

(a) Hospitalization



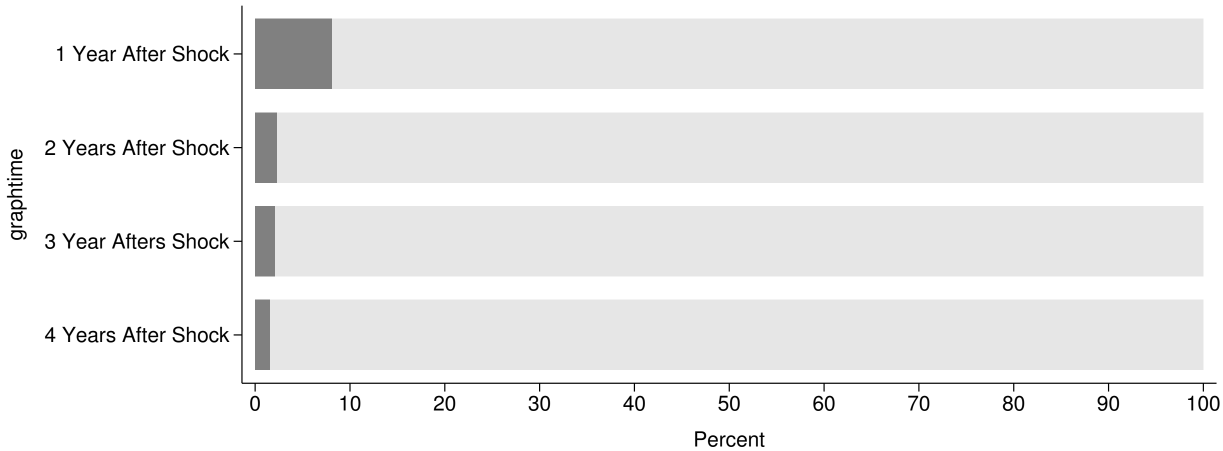
(b) Mortality



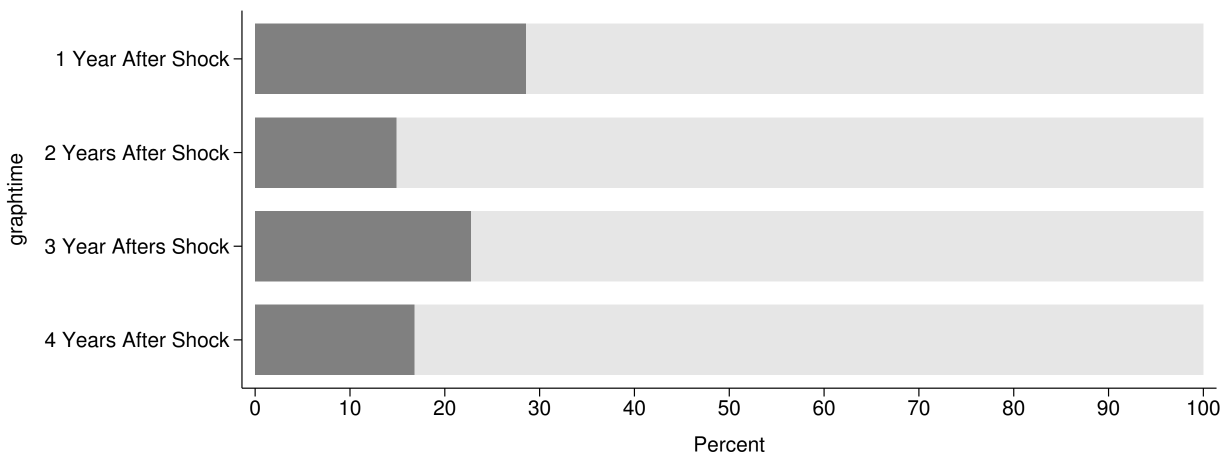
Notes: This figure shows the estimated effects (and confidence intervals) of job loss on public-sector hospitalizations for different diagnoses (Panel A) and on mortality for different causes of death (Panel B), as defined at the International Classification of Diseases (ICD-10). All estimates and confidence intervals are computed using the sample for male workers. Estimates for hospitalizations are computed with the difference-in-differences equation (4) and estimates for mortality are computed with the adapted matching-based equation.

Figure C2: Mediation Analysis of the Effect of Private Health Insurance on Public Hospitalization (Emergency), Male Workers

(a) Full Sample

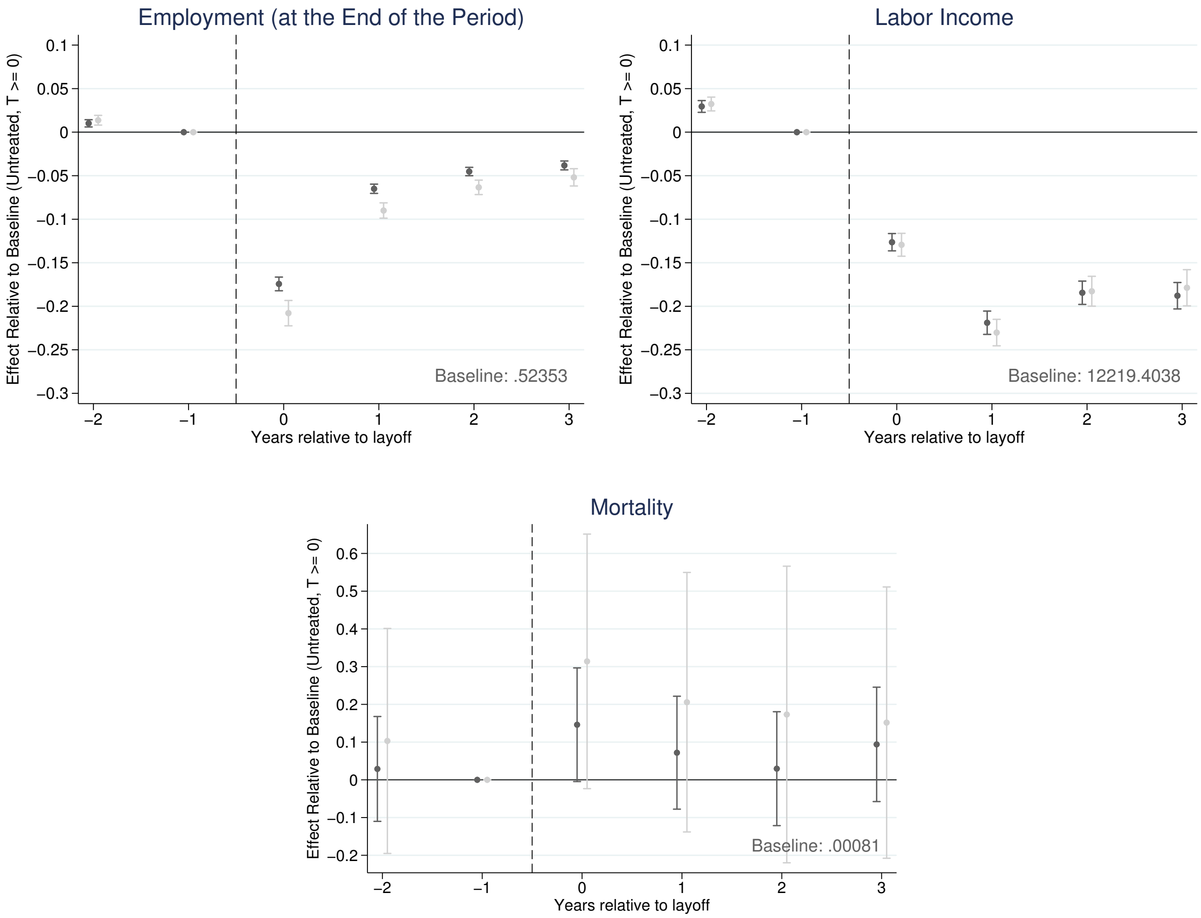


(b) Health Insurance at $t = 0$



Notes: This figure shows the results of the mediation analysis of the total effect on emergency hospitalizations for male workers, as described in Section C.2. 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.

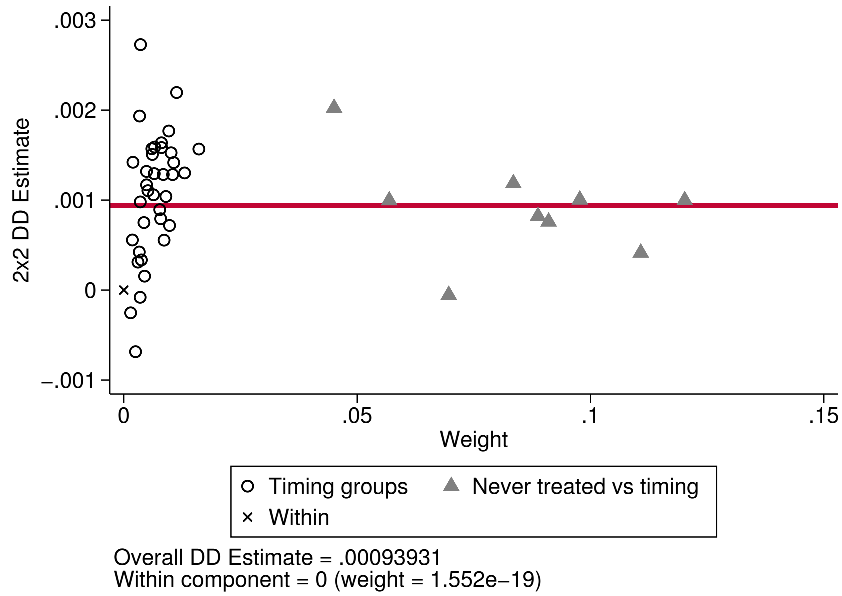
Figure C3: Dynamic ITT Effects of Job Loss on Labor Market Outcomes and Mortality



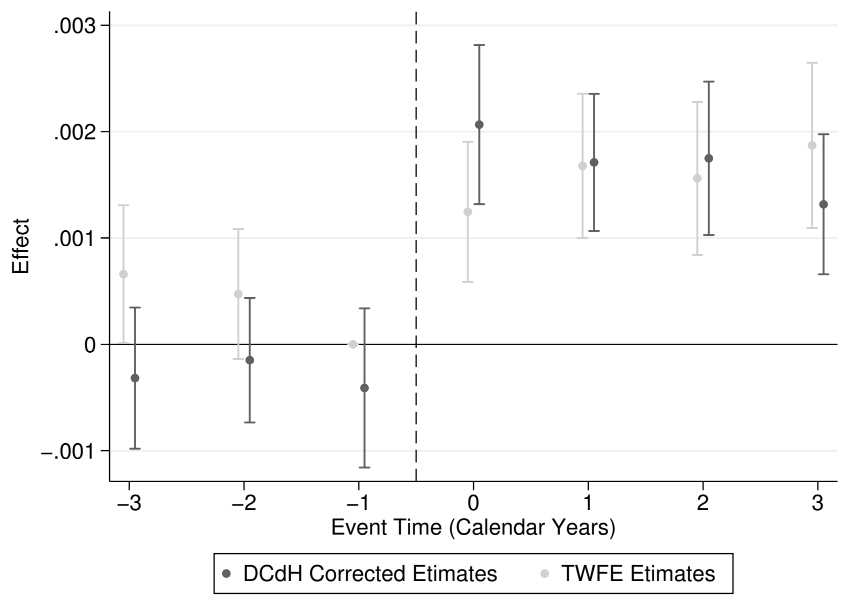
Notes: This figure shows the dynamic intention-to-treat (ITT) effects of job loss due to a mass layoff on formal employment, labor income, and mortality. Outcomes are shown separately for both male (dark gray) and female workers (light gray), and are re-scaled by the baseline outcome for each group (i.e. the estimated effect in the respective treatment group at $t < 0$). Estimates were computed using the difference-in-differences equation (1). The sample includes a treatment group of workers employed at $t = -2$ in a firm that suffers a mass layoff at $t = 0$, and a matched control group of workers employed at $t = -2$ in a firm that does not suffers a mass layoff in the period of analysis. Mass layoffs are defined as the displacement of more than 50% of the workforce in a same calendar year. 95% confidence intervals are also reported. Income variables are measured in Brazilian Reais.

Figure C4: Effect of Job Loss on the Probability of Hospitalization, Alternative Estimators

(a) ATE Weight Decomposition

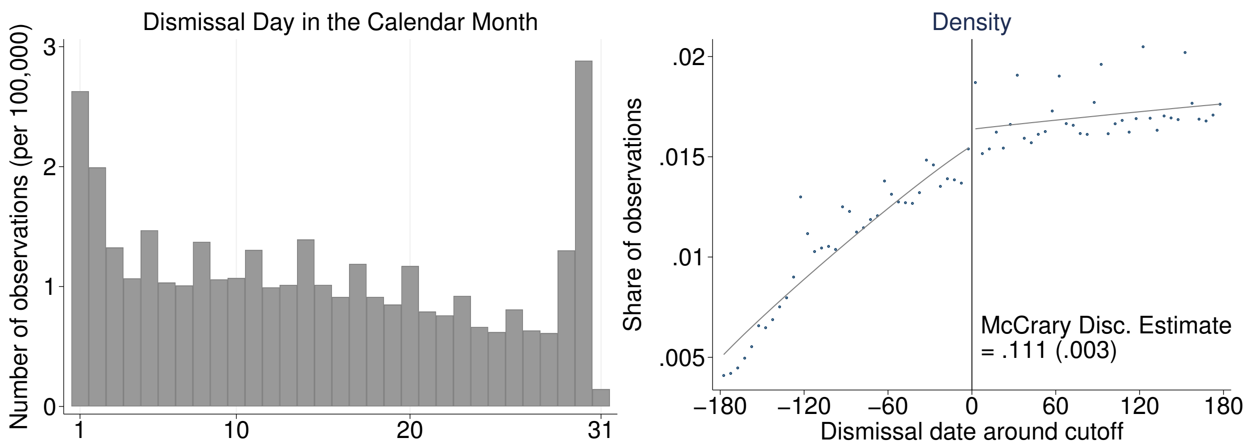


(b) TWFE Panel Estimates



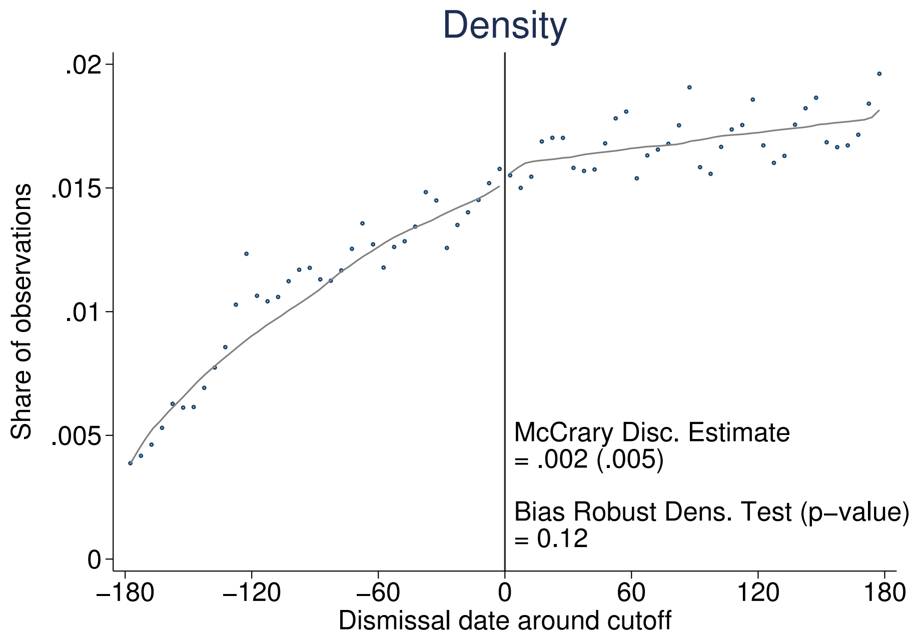
Notes: The figure in Panel (a) shows the weight decomposition of the average treatment effect (ATE) formed from each pairwise comparison between treatment units in the main ample, proposed in Goodman-Bacon (2021). The horizontal red line mark the estimated value from the original specification. The graph in Panel (b) shows two-way fixed effects (TWFE) panel estimates with the correction proposed in De Chaisemartin and d'Haultfoeuille (2020). 95% confidence intervals are also reported.

Figure D1: Dismissal Dates Monthly Cycles



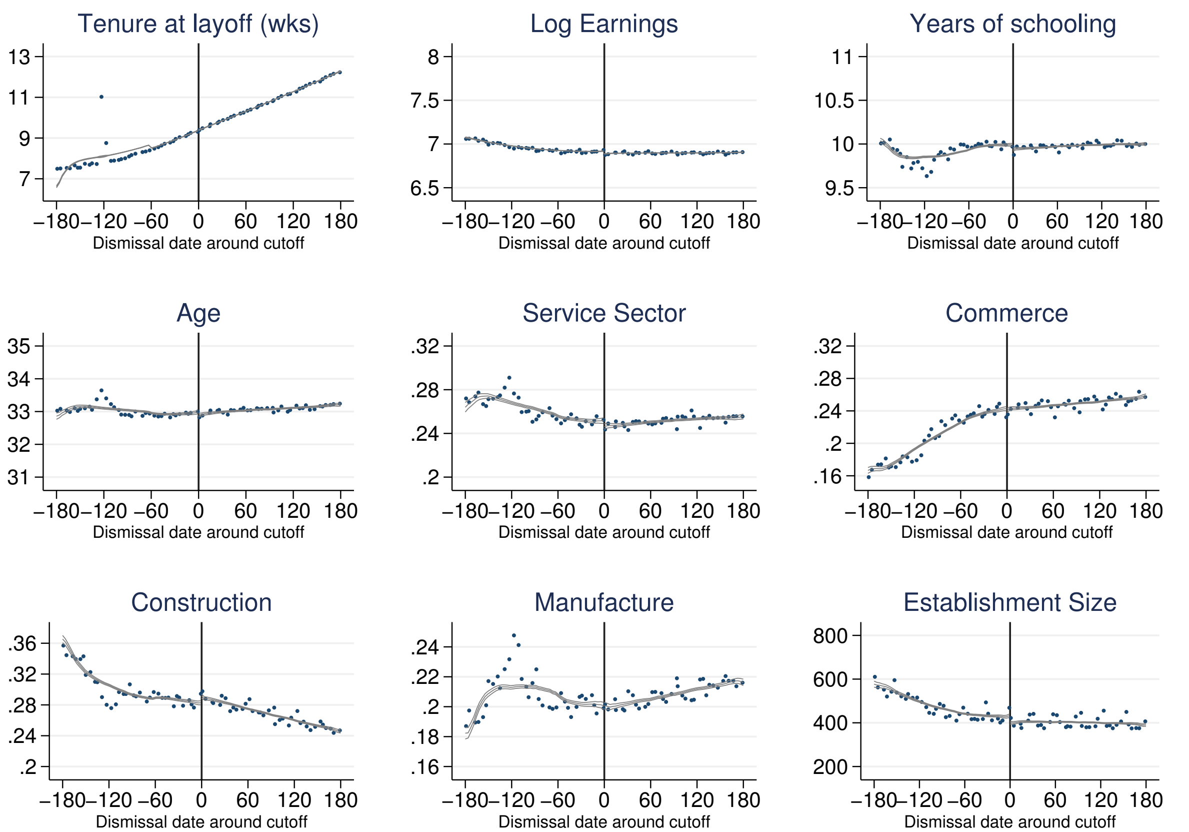
Notes: The left graph presents the distribution of dismissal dates by calendar day within each month. The right graph presents the running variable density function around the cutoff, based on an initial sample that includes all dismissal dates.

Figure D2: Effect of UI Eligibility, Density Function



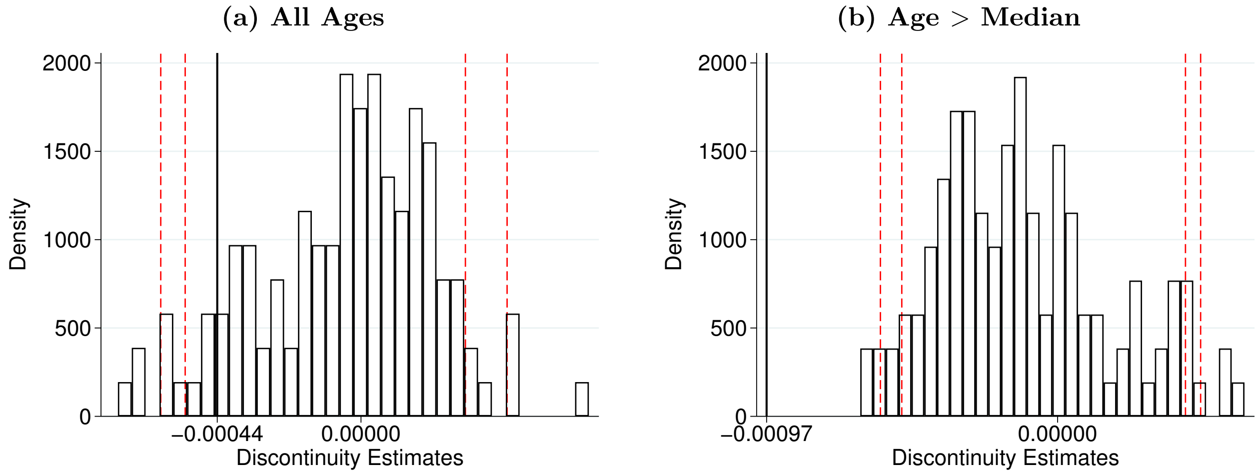
Notes: This figure shows the density of dismissal dates around the cutoff date for eligibility for unemployment benefits (i.e., 16 months since the previous layoff date in the past) in our main working sample. The sample includes displaced parents with at least 6 months of continuous employment prior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

Figure D3: 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.

Figure D4: Effect of UI Take-Up on Public Hospital Admissions (Emergency, External Causes): Age > Sample Median (30 Years Old), Permutation Tests



Notes: Notes: The graphs compare t-statistics for the discontinuity estimates of the effect of UI take-up on emergency 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).

Table C1: ITT Effects of Job Loss on Labor Market Outcomes and Mortality

	(1)	(2)	(3)
	Labor Market Outcomes		Mortality
	Employment	Income	
	Panel A: Men		
$Treat_i \times Time_{(t=0)}$	-0.161287***	-1,680.55***	0.000151**
	(0.003098)	(59.770446)	(0.000076)
$Treat_i \times Time_{(t>0)}$	-0.027411***	-1,735.59***	0.000058
	(0.003660)	(148.987121)	(0.000056)
Mean Outcome (Untreated at $t \geq 0$)	0.8461	11,318.92	0.001149
Effect Relative to the Mean ($t = 0$)			
· at $t = 0$	19%	15%	13%
· at $t \geq 0$	3%	15%	-
Observations	2,097,017	2,097,017	2,097,017
	Panel B: Women		
$Treat_i \times Time_{(t=0)}$	-0.186454***	-1,291.28***	0.000125*
	(0.005969)	(61.64)	(0.000069)
$Treat_i \times Time_{(t>0)}$	-0.038395***	-1,268.38***	0.000060
	(0.006108)	(173.59)	(0.000055)
Mean Outcome (Untreated at $t \geq 0$)	0.8401	8,427.98	0.000476
Effect Relative to the Mean			
· at $t = 0$	22%	15%	26%
· at $t \geq 0$	5%	15%	-
Observations	1,107,874	1,107,874	1,107,874

Notes: This table shows the intention-to-treat (ITT) effect of job loss due to a mass layoff on formal employment (column 1), labor income (column 2), and the probability of death (column 3), for both male (Panel A) and female workers (Panel B). 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 $_i$ equal to 1 for treated workers, interacted with a dummy $_t$ equal to 1 for the period after displacement. All regressions include individual and year fixed effects. The sample includes a treatment group of workers employed at $t = -2$ in a firm that suffers a mass layoff at $t = 0$, and a matched control group of workers employed at $t = -2$ in a firm that does not suffers a mass layoff in the period of analysis. Mass layoffs are defined as the displacement of more than 50% of the workforce in a same calendar year. Standard errors clustered at the firm level are indicated in parenthesis. ***, ** and * represent $p < 0.01$, $p < 0.05$ and $p < 0.1$ respectively.

Table C2: Effects of Job Loss on Health Outcomes (Male Workers), Varying Mass Layoff Intensity

	(1)	(2)	(3)	(4)	(5)
	Panel A: Health Insurance Enrollment				
$Treat_i \times Post_i$	-0.026172***	-0.015894***	-0.030590***	-0.032653***	-0.022945***
	(0.003398)	(0.00613)	(0.010500)	(0.005624)	(0.007935)
Mass Layoff Sample	> 33%	> 50%	closure	> 100 workers	> 250 workers
Observations	623,350	224,413	79,653	350,616	216,902
	Panel B: Hospitalization				
$Treat_i \times Post_i$	0.000843***	0.00107*	0.001176	0.001238***	0.001201**
	(0.000330)	(0.000578)	(0.000877)	(0.000453)	(0.0005902)
Mass Layoff Sample	> 33%	> 50%	closure	> 100 workers	> 250 workers
Observations	1,275,925	463,190	159,803	722,533	452,151
	Panel C: Mortality				
$Treat_i$	0.000262***	0.000255***	0.000434***	0.000280***	0.000317***
	(0.000048)	(0.000083)	(0.000116)	(0.000069)	(0.000095)
Mass Layoff Sample	> 33%	> 50%	closure	> 100 workers	> 250 workers
Observations	2,090,438	768,562	233,917	1,175,674	719,554

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. 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 $_i$ equal to 1 for treated workers, interacted with a dummy $_t$ 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 parenthesis. ***, ** and * represent $p < 0.01$, $p < 0.05$ and $p < 0.1$ respectively.

Table D1: Effect of UI Take-Up on Public Hospital Admissions (Emergency, External Causes), Robustness to Different Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: All Ages							
UI Take-Up	-0.000902*	-0.001395*	-0.001017*	-0.000696	-0.001011*	-0.000899*	-0.000717	-0.001284*
	(0.000482)	(0.000824)	(0.000548)	(0.000437)	(0.000545)	(0.000514)	(0.000472)	(0.000692)
Bandwidths (Days)	CCT	30	60	90	CCT	150	180	CCT
Polynomial Order	0	1	1	1	1	2	2	2
Observations	1,702,895	1,702,895	1,702,895	1,702,895	1,702,895	1,702,895	1,702,895	1,702,895
	Panel B: Age > Sample Median (30 Years Old)							
UI Take-Up	-0.001740***	-0.002331**	-0.002044***	-0.001653***	-0.002114***	-0.002072***	-0.001764***	-0.002437**
	(0.000653)	(0.001069)	(0.000734)	(0.000590)	(0.000806)	(0.000695)	(0.000640)	(0.001025)
Bandwidths (Days)	CCT	30	60	90	CCT	150	180	CCT
Polynomial Order	0	1	1	1	1	2	2	2
Observations	797,713	797,713	797,713	797,713	797,713	797,713	797,713	797,713
	Panel C: Age ≤ Sample Median (30 Years Old)							
UI Take-Up	-0.000181	-0.000662	-0.000183	0.000094	-0.000137	0.000052	0.000147	-0.000679
	(0.000634)	(0.001218)	(0.000795)	(0.000632)	(0.000769)	(0.000742)	(0.000682)	(0.001185)
Bandwidths (Days)	CCT	30	60	90	CCT	150	180	CCT
Polynomial Order	0	1	1	1	1	2	2	2
Observations	905,182	905,182	905,182	905,182	905,182	905,182	905,182	905,182

Notes: This table replicates the regression discontinuity analysis in Table 5 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).