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

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Abstract

We study the causal effects of job loss and unemployment insurance (UI) on hospitalization and mortality for Brazilian workers. We construct a novel dataset that merges millions of individual-level administrative records on employment, hospital discharges, and mortality for a period of 17 years. Using a difference-in-differences research design that compares laid-off workers from firms that experienced mass layoffs to similar workers in firms that did not, we find that job loss causes a 30% increase in the probability of male in-patient admission to public hospitals, and a 34% increase in the risk of male mortality. Our estimates are driven primarily by external causes and apply to both older and younger male workers. We find no effects on female outcomes, but children of both male and female workers are subjected to higher risks of hospitalization following their parent's job dismissal. Using a regression-discontinuity design that exploits variation in UI eligibility following job loss, we show that UI largely offsets the risk of hospitalization for older male workers. Our results indicate that governmental labor market policies can effectively mitigate a substantial portion of the adverse health impacts of job loss.

Keywords: job loss, unemployment insurance, hospitalization, mortality

JEL Codes: I12, J63, J65

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1 Introduction

Understanding the relationship between unemployment and health has been a longstanding question in economics. Previous studies have shown that job dismissals and resulting losses in earnings impose substantial health and mortality risks on workers, particularly for middle-aged males (e.g., Sullivan and Wachter, 2009). However, as the evidence has been largely concentrated on high-income economies, little is known about potentially adverse implications that might prevail in places with high labor turnover, low public safety, and weak social insurance institutions.[1] In these contexts, for instance, effects may extend to other vulnerable groups including younger workers, who are more susceptible to engage in risk behavior (Bambra and Eikemo, 2018), or to cohabiting family members who are indirectly affected by higher mental and economic distress (McKee-Ryan and Maitoza, 2018). Furthermore, evidence on the effectiveness of public policy alternatives aimed at mitigating those impacts, such as income support programs, remains scant.

In this paper, we study the causal effect of job loss on hospitalization and mortality for dismissed Brazilian workers, and examine how these are affected by an exogenous variation in access to unemployment insurance after dismissal. We construct a novel, individual-level dataset that merges 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 total benefits claimed for each individual worker in our sample.

Our analysis is divided into two parts. In the first part, we estimate the dynamic treatment effect of job displacement on private health insurance enrollment, in-patient hospitalizations, and mortality. Our empirical strategy is based on a difference-in-differences design where we compare health outcomes from workers who were displaced by *mass layoffs* with those of a matched control group of workers who were not displaced in the same year. The main identification assumption is that the health trajectories of workers employed in firms that did not have mass layoffs serve as reliable counterfactuals to that of similar workers dismissed from firms that had mass layoffs. This assumption is supported by the absence of different trends in health outcomes between treatment and control groups prior to layoff. Reassuringly, this pattern holds both in our hospitalization outcomes using our main

[1] A consensus on the negative effects of unemployment, moreover, is still lacking. For instance, economic recessions and high unemployment rates have also been linked to *lower* mortality rates, possibly due to decreased opportunity costs of investing in health capital (Grossman, 1972; Ruhm, 2000).

difference-in-differences strategy and, in the case of mortality, using a novel intention-to-treat (ITT) strategy that relies on the timing of mass layoffs at the firm level. In addition, the size of our dataset (between 1 to 3 million observations in each sample) allows us to exactly match workers across both groups by a series of specific individual-, firm-, and regional-level characteristics, which further increases our cross-sample comparability.

We find that employment and average earnings of male workers decrease, respectively, by 14% and 29% up to four years after layoff, in comparison to the control group. In parallel to these effects, we find an average 30% increase in the probability of in-patient hospitalizations for male workers, and a 34% increase in the probability of death for this group, the latter concentrated mostly on the first year after layoff. Impacts are associated to both short- and long-term developments in the incidence of ischemic heart diseases, cerebrovascular diseases, respiratory diseases, and, to a smaller degree, neoplasms of respiratory organs. More noticeable, however, are the large increases in hospitalizations and deaths associated with *external causes*, such as injuries, transport vehicle accidents, and complications of trauma. Within that category we also find an increase in the probability of dying from involvement in assaults, or being a victim thereof. Altogether, results suggest that job loss cause an increase in the risk of diseases and conditions associated to anxiety, stress, and engagement in risk behavior. Finally, we show that hospitalizations and deaths associated to external causes are mostly driven by workers in younger cohorts, although effects are discernible across all age groups. Effects associated with specific diseases and medical conditions, in turn, are largely driven by workers in older cohorts.

We find no statistically significant effects of job loss on either hospitalization or mortality for female workers, although estimated impacts on their earnings are very similar to those found for male workers. However, using complementary data on individual family links, we find that *children* of both male and female workers are also subjected to higher risks of hospitalization from external causes following a dismissal of either one of their parents from their jobs. These findings give further support to our hypothesis that estimates reflect consequences from higher stress, which likely spill over to other family members and increase the overall mental burden in a household.

We also show that the probability of both male and female workers' enrollment status in private health insurance (HI) plans decrease, respectively, by 16% and 24%. These are insurance options (usually employer-sponsored) that cover medical services at a smaller market of private clinics and hospitals and are available to a fraction (about one-fourth) of Brazilian workers. Losing access to such private options of health care may thus represent an additional risk factor pushing up the adverse effects of job loss on health. Alternatively, as our data is based on admissions to *public* hospitals at the country's universal health care system,[2] it can also imply the possibility that hospitalization estimates simply reflect some workers substituting private for public health care options after layoff. However, our focus on non-elective, in-patient hospital admissions, and the proportional similarity between hospitalization and mortality estimates (the latter being a more direct and comprehensive measure of health shocks), provide little basis for that hypothesis. We also provide further supporting evidence with a mediation analysis showing that overall effects in the full sample of workers (i.e., those with or without HI at the time of layoff) are largely not explained by variations in patterns of health insurance enrollment before or after layoff.

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 countries, UI in Brazil is a federal program that provides financial benefits to workers laid off by their employers. Any worker dismissed not for cause 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 clean regression discontinuity design to a restricted sample of displaced workers and isolate the effect of income shocks.

Our results show that UI largely offsets the risk of hospitalization for male workers at the eligibility threshold. 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 fully concentrated on older workers (i.e., at or above 32 years old), while no significant effect is found for younger workers. Quantitatively, for the former group (older male workers eligible to UI) the probability of emergency hospitalization due to external causes decreases by 44% compared to ineligible workers in the same age range. Finally, we find no significant impacts of UI eligibility 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 health risks of job loss for individuals potentially more susceptible to those risks. In particular, we show that UI is an effective policy option that fully reverses the post-layoff increase in the probability of a common type of hospitalization for a particular group of workers, while compensating for only a portion of their lost incomes due to unemployment. We show, in fact, that the effects of UI are persistent and even outlast the average benefit window, as differential impacts on marginally eligible workers are measured as far as one year after layoff. Upon further investigation on potential mechanisms, we find that UI also discourages job search for treated individuals –

[2]Throughout the analysis we will use expressions such as "public-sector hospital admissions", "in-patient admissions to public hospitals" and "hospitalizations" interchangeably.

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 thus indicate that a substantial portion of the health impacts of job loss are likely associated with stress-induced consequences of higher liquidity constraints and with higher pressure to exit unemployment among the older – and more vulnerable – male population.

This paper relates more broadly to the extensive literature on the economic consequences of unemployment, while focusing, in particular, on the implications of job loss to the health of laid-off workers in both the short- and long-run. Our findings build on earlier works documenting the principal mechanisms that could 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 using 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.[3] 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 explore, at a singular level of detail, impacts by different causes of hospitalization and deaths, and the risk factors associated with different individual characteristics (such as age, gender, tenure and education level). We also tackle the less-explored problem of understanding indirect health impacts of job loss on workers' family members, providing novel evidence that children of laid-off workers are also at higher risk of being hospitalized following their parents' job dismissal.

Our analysis further expands in two important directions for public policy-making. First,

[3] 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.

despite the universality of public health care coverage in the country, the availability of optional, complementary private-sector options to a small share 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 clean 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. We provide the first causal evidence that UI benefits help reduce some of the health risks associated to job loss using direct measures of health – namely, individual in-patient hospitalizations due to external causes, which include accidents, injuries, and other consequences of risky activities in general.[4] 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 and details the merging procedure of our various data sources. Section 4 presents our main results on the impacts of job loss on health, and Section 5 investigates the mitigation effects of 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 Hospitalizations

As 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 recent years 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

[4]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 UI recipients.

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 categories listed in the vertical axis are the 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 the exposition, we group these blocks into *external causes*, containing all blocks related to injuries, accidents, self-harm, and assaults; and *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 ("assaults" being a notable exception, for which the number of recorded deaths is slightly higher than the number of hospitalizations).

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 nationwide public 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). The SUS provides medical services at all levels of complexity. It 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.

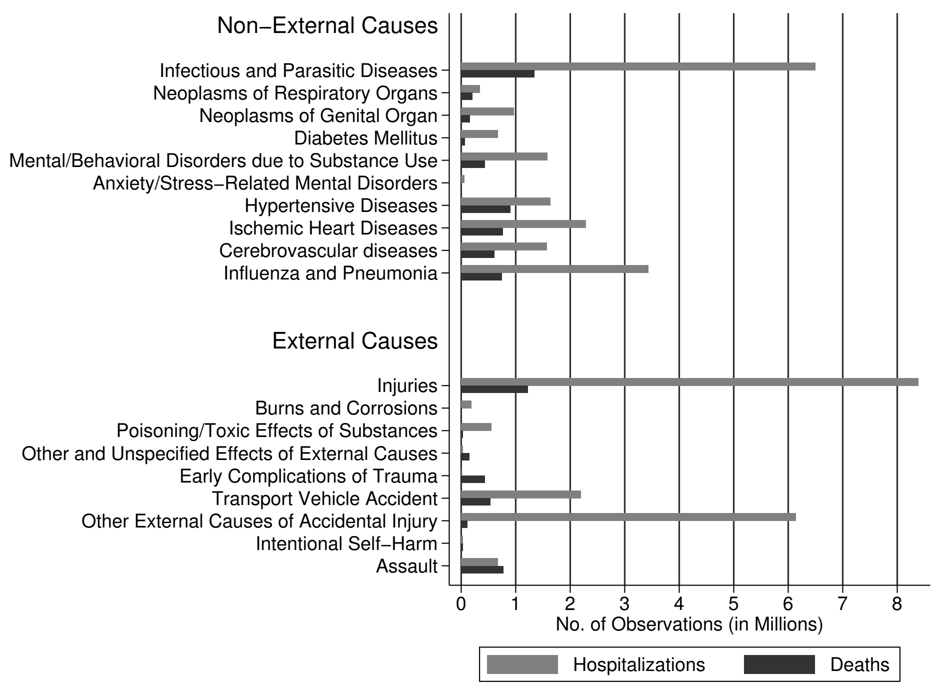
The records on hospital discharges used in this paper are from the SUS. However, in parallel with the public sector, a private, supplementary health care sector also operates in the country, in which autonomous physicians offer their medical services in privately-owned hospitals and clinics. Most of these services are financed indirectly through individual enrollment in private health insurance (HI) plans, which are commonly employer-sponsored (around 72% of all active plans in 2020). In 2019, HI plans covered supplementary medical services for about 24.25% of the Brazilian population (ANS, 2019).

There are important differences in health care usage across both systems that are worth

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.

considering, most notably: *(i)* the supplemental/private sector comprises a larger network of establishments for out-patient care (73% of the total in 2019), while the public sector has more establishments for in-patient and emergency care (54% of the total in 2019); *(ii)* non-elective care (including emergency care) corresponds to a third of inpatient hospital admissions in the private sector, while they correspond to about 82% of admissions in the public sector. Despite such differences, there is also evidence that some level of interaction exists between the two systems, with a share of hospital admissions in the public sector, for instance, being of patients also enrolled in private HI plans (see Appendix A.2). To mitigate potential concerns over confounding effects coming from this interaction (and also to narrow our scope to cases more likely associated to unforeseen shocks on individuals' health) we choose to focus on non-elective in-patient hospital admissions only. We also perform empirical exercises that investigate the extent to which our (public-sector) hospitalization estimates can be explained by variation in access to private health care (i.e., through individuals' enrollment in private HI plans).

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.[5] 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

[5] 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.

GDP (TN, 2019).

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 and discuss the procedures employed in merging information across individual observations at each of them.

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 Economy (The Ministry of Labor was incorporated in the current Brazilian Government). 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 that employers notify workers in advance in case of a dismissal. Therefore, throughout our analysis, we center all time units around the dismissal date registered at RAIS minus 30 days, which corresponds to the most conservative baseline for the advance notice period.[6] With this procedure, we aim to also capture the effect of learning about an imminent layoff, which in our context could be just as relevant as the dismissal itself.

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 collected 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

[6] 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.

of hospitalizations and deaths we also group all observations into two groups of roughly equal proportion: those that are 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 assaults and are thus comprised of events associated to some external circumstance, while the second group include different types of diseases specifically associated to some 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 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, his 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 Merging Records Across Datasets

All individuals in the labor (RAIS) and residential addresses (RFB) datasets contain unique tax-code identifier (CPF), which we use to merge observations across these two datasets. The merge 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 by their respective cell at each year. This still preserves 75.97% of the original observations in our sample. 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.[7] 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.[8]

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, comprehends the entire Brazilian population), the records corresponding to that same cell in both the health insurance data and the hospitalization data (which are also nationally comprehensive) are in fact associated to that single individual. In the end, 76.16% of all observations in the hospitalization data and 36.27% of those in the health insurance data are 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

[7]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.

[8]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.

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 the 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 of individuals who are marked as deceased but do not contain their dates of death nor their causes/circumstances). To test the extent that such measurement error impacts our estimates we readapt all exercises from the main analysis so that they rely only on the year the individual has died instead of their precise dates. Fortunately, they reveal that such impacts are negligible.

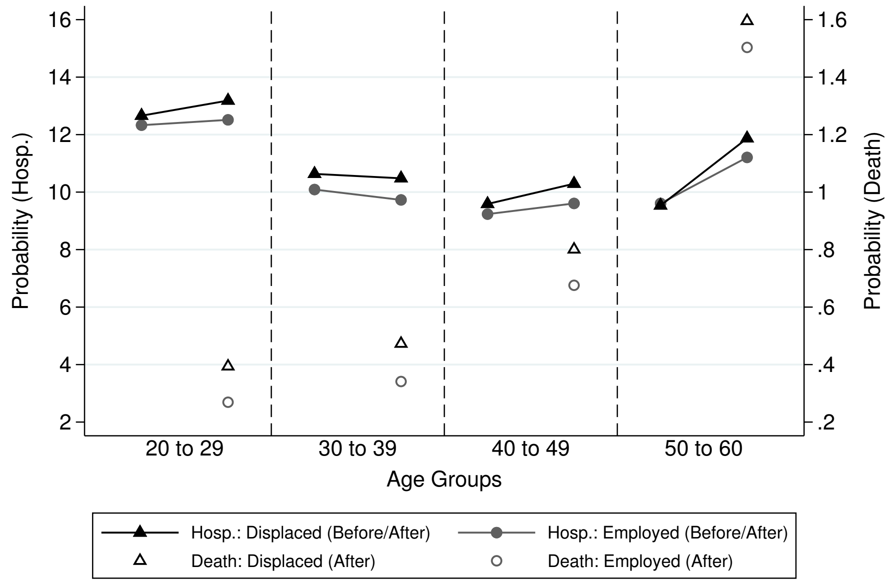
3.5 Descriptive Evidence

Figure 2, Panel (a) displays implied probabilites of hospitalization and deaths for workers with different employment status and in different age groups. They are computed from a sample that includes full-time workers, both male and female, employed in the non-agricultural, private sector between 2006 and 2014. Hospitalization probabilities are calculated both for three years before and three years after a worker is observed in the sample. For instance, for workers observed in 2010, "after" probabilities are based on their average number of hospitalitazions between years 2011 and 2013, while "before" probabilities are based on the same average between years 2008 and 2010. Mortality probabilities are calculated only for the "after" period (since, of course, workers must be alive at any given year in order to be observed in our sample). Each probability is reported separately for workers who were displaced or remained continuosly employed in the year they are observed. To improve visualization, hospitalization probabilities are referenced in the left-side axis of the graph, while those of mortality (magnified by a factor of 10 relative to the latter) are referenced in the right-side axis.

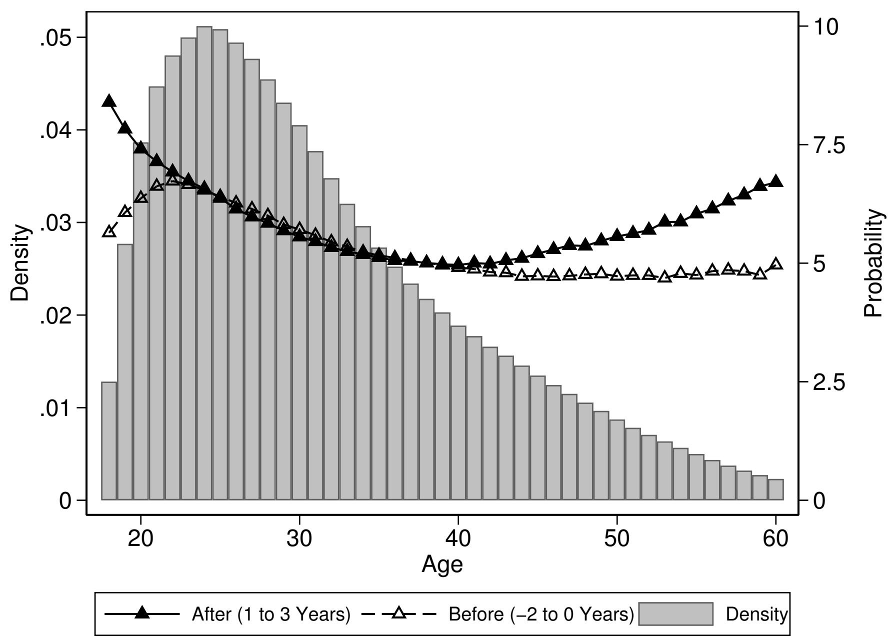
A few patterns emerge from the figure. Most notably, both the probabilities of hospitalization and of mortality in the "after" period are higher for dismissed workers in comparison to workers who remained employed in a same year. This difference is similar across every age group, with values ranging between .7-.8 percentage points for hospitalization and .13-.09 percentage points for mortality. Although small differences in hospitalization probabilites between displaced and non-displaced workers can also be seen in the "before" period (which

Figure 2: Mortality and Hospitalization by Age and Employment Status

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



(b) Prob. of Hospitalization (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.

should be attributed to non-random selection in the displacement of workers), such differences become more pronounced in the "after" period. This pattern, also visible for every age group, suggests a possible association of job displacement with higher health risks. Finally, hospitalization probabilities after displacement are highest both for younger (i.e., below 29 years old) and older workers (i.e., above 50 years old), while post-dismissal probabilities of mortality are increasingly higher for older cohorts.

Panel (b) in Figure 2 explores the age-driven patterns from Panel (a) in more detail. It displays the probability of hospitalization for displaced workers before and after layoff over a density plot of their ages when dismissed. Here it becomes clear that workers below 24 and those above 40 years old show higher risk of hospitalization after displacement, while this remain ambiguous for workers at the middle range of the age distribution.[9] Together these two figures imply not only the possibility of harmful health-related effects associated to job loss, but also that one's age at the time of dismissal may be a strong predictor for the magnitude of such impacts. They also suggest that risk factors associated with impacts at older and younger cohorts could be of different natures. Both possibilities will be explored more rigorously in the analysis that follows.

4 Job Loss, Public Hospitalization, 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 probabilities 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.[10] 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 not-for-cause displacement of more than 33% of a firm's workforce in a single calendar year. For the treatment group we select all workers displaced in mass layoffs

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

[10] 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.

between 2006 and 2014. Given the range of years in our dataset this allows us to estimate dynamic treatment effects for up to four years after displacement, as well as placebo effects up to three years before displacement. The pool of candidate workers for the control group includes all individuals employed in firms that did not experience mass layoffs during our period of analysis.

We first perform an exact matching procedure where we match each worker in the treated group with a single worker in the control group who *(i)* was 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 workers in the control group a placebo treatment date equal to the dismissal date of their matched counterpart in the treatment group, and compare outcomes for the two groups at different time intervals relative to the layoff date.[11]

To estimate the effect of job loss on employment, labor income, HI enrollment, and public hospitalization, we use the following event-study equation:

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

In this equation, all time subscripts t are based on years relative to each individual worker i's dismissal date (actual ones in the treatment group, and placebos in the control group), and are henceforth referred to as "event years". Y_{it} is the outcome of interest for worker i at year t. For labor income it takes the total value of earnings at years before or after treatment. For the remaining outcomes it represents probability values, taking value $Y_{it} = 1$ if the worker remained employed, remained enroleld in a HI plan, or was hospitalized by the end of that event year, and $Y_{it} = 0$ otherwise. $Treat_i$ is a dummy indicating that worker i belongs to the treatment group, and $Time_t$ is a dummy identifying the number of elapsed years since the worker's dismissal date. This means that $Time_0 = 1$ in the first 12 months after layoff,

[11] 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 B.4, where we test an alternative two-way fixed effects specification using a panel fully balanced in calendar years. We then formally apply Goodman-Bacon (2021)'s weight decomposition to our main estimates to evaluate the potential extent of such bias in our setting and also test the alternative estimator proposed by De Chaisemartin and d'Haultfoeuille (2020).

$Time_1 = 1$ the following 12 months, and so on; while $Time_{-1} = 1$ in the 12 months previous to layoff, $Time_{-2} = 1$ in the 12 months previous to that, and so on. The baseline omitted period is set at $t = -1$. 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 (i.e., $Post_t = 1$ for $t > 0$), and all other variables are defined as in (1). Standard errors in both equations are clustered at the firm level.

The biggest challenge with the empirical 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: $Y_{it} = \alpha + \beta \, Treat_i + \epsilon_{it}$.[12] The outcome of interest is the probability of mortality, so $Y_{it} = 1$ if individual i has died by the end of that event year and $Y_{it} = 0$ if not. Observations are ommited for all event years after an individual has died.

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 are firm-level shocks that most likely depend on broader economic conditions rather than on displaced workers' health status. We hope that by matching workers on several observable characteristics unrelated to individuals' health status, and by reporting estimates on pre-trends for most of our main

[12] In Appendix B.2 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.

outcomes, we confer sufficient credibility to the validity of our research design.[13]

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
Layoff Rate ($t = -1$)	0.17	0.17	-0.13	0.16	0.17	-0.13	0.16	0.17	-0.13
Layoff Rate ($t = -2$)	0.16	0.16	-0.06	0.16	0.16	-0.05	0.16	0.16	-0.06
Layoff Rate ($t = -3$)	0.15	0.16	-0.08	0.15	0.16	-0.09	0.15	0.16	-0.09

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

Table 1 presents summary statistics for treated and control units in each of our working samples described in Section 3. 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. Furthermore, it is worth noticing that reported values on all observable characteristics are not only balanced between treatment and control groups within samples, but also remarkably similar *across* samples. This reinforce the plausibility of the assumption that selecting individuals based on their being uniquely indentifiable at each cluster (reported in Table 1) is equivalent to selecting them randomly.[14]

[13] While reporting individual pre-trends on mortality is not possible using our main specification, we report dynamic estimates for this outcome over a 8-year period after layoff, which allows us to test whether effects dissipate over time – a useful sanity check – at the cost of sligtly higher imprecision in the latest periods due to sample imbalance. That reported pre-trends on the probability of hospitalization, and the close similarity between hospitalization and mortality patterns throughout the whole analysis, should also provide further support to the validity of our estimates.

[14] In this section we impose a final restriction on the two restricted samples, which is to only keep individuals who remain uniquely identifiable in their respective clusters for *all* years after/before their main event

4.2 Effects on Employment and Public Hospital Admissions

Our main results on labor market outcomes, private HI enrollment and admissions to public hospitals are displayed in Figure 3. Each graph shows the dynamic treatment effect of job loss on the indicated outcome, with estimates reported separately for men (dark gray) and women (light gray).[15] The indicated baselines for each group are the mean values of the outcome at $t = -1$ for treated individuals in that group.

The two upper figures show the estimated effects on subsequent employment and labor income. For male workers, the probability of employment decreases sharply by 24 percentage points (p.p.) in the first year after layoff, while total labor income decreases on average by 8 thousand Brazilian Reais (about 4 thousand dollars in 2012 values), or 56% relative to the baseline mean. Such impacts are also high for female workers, for whom we estimate relative reductions of approximately 30 p.p. in the probability of employment and 6.6 thousand Reais in total income (60% relative to baseline) in the first year. Such effects on employment and earnings appear to quickly diminish in the following years, but by the fourth year they still average at 14% and 17% reductions on the probability male and female employment, respectively, and reductions in total earning between 4,1 and 3,2 thousand Reais (28% relative to the baseline for male workers, and 29% for female workers). Point estimates for these average effects over four years after dismissal (in both absolute and relative values) are shown in Table 2.

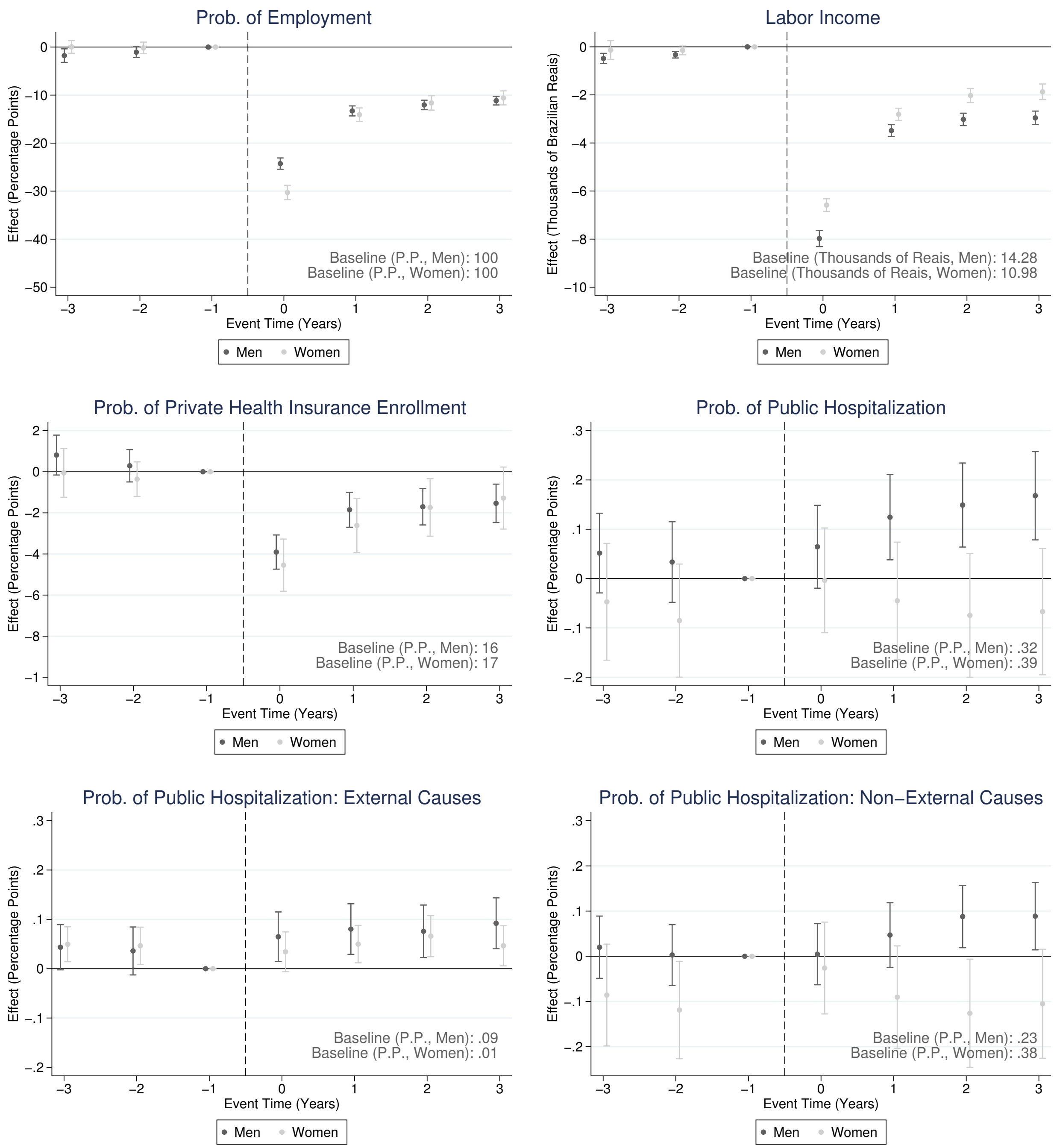
The middle-left graph in Figure 3 shows the estimated effect of job loss on the probability of enrollment in private HI plans. Impacts are very similar across both genders: we find that job loss causes a reduction between 3.9 and 4.5 p.p. in the probability of HI enrollment in the first year after layoff, corresponding to a 24% to 26% drop with respect to average baseline enrollments. By the fourth year, this impact is still maintained at around 8%. Average effects on overall enrollment after four years, reported in Table 2, suggest relative reductions in enrollment of 16% for male workers and 13% for female workers.

As discussed in Section 3, private HI plans in Brazil are primarily employer-sponsored (i.e., corporate), and so this big drop in enrollment is perhaps unsurprising. However, individual, non-employer-sponsored HI plans are available across many markets in the country, and dismissed workers who lost access to health insurance through their employers could, in principle, opt to purchase such plans as an alternative. The total effects on insurance enrollment from Figure 3 suggest, however, that such markets are unable to sufficiently ab-

year. This guarantees that outcomes in different years are not confounded by the possibility of individuals being identifiable in some years but not on others.

[15] All effects uncovered for male workers in this section and the next (effects on mortality) hold for the pooled sample of male and female workers.

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.

Table 2: Effect of Job Loss on Employment, HI Enrollment, and Hospitalization

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Labor Market Outcomes		**Prob. of Private HI Enrollment**			**Prob. of Public Hospitalization**		
	Prob. of Employment	Labor Income	All Plans	by Plan Type		Overall	by Cause	
				Corporate	Individual		External	Non-Ext.
	Panel A: Men							
Point Estimate	-14.2444***	-4088.25***	-2.6180***	-2.1760***	-0.0518	0.0981***	0.0517***	0.0494**
	(0.3630)	(127.12)	(0.3398)	(0.2988)	(0.0450)	(0.0250)	(0.0143)	(0.0206)
Baseline Mean (Treated, $t = -1$)	100	14285.46	16.2212	9.3796	0.4815	0.3238	0.092	0.236
Effect Relative to Baseline	-14%	-28%	-16%	-23%	-10%	30%	56%	20%
Implied Elasticity to Employment	-	-	1.14	1.64	0.71	-2.14	-4.00	-1.43
Implied Elasticity to Earnings	-	-	0.57	0.82	0.36	-1.07	-2.00	-0.71
Observations	2,007,355	2,007,355	623,322	623,322	623,322	1,284,864	1,284,864	1,284,864
	Panel B: Women							
Point Estimate	-16.5932***	-3228.19***	-2.4120***	-2.4017***	0.2544***	-0.0034	0.0171	-0.0186
	(0.6378)	(123.34)	(0.4600)	(0.4588)	(0.0833)	(0.0342)	(0.0108)	(0.0328)
Baseline Mean (Treated, $t = -1$)	100	10986.09	17.4267	9.8349	1.1352	0.3982	0.019	0.3811
Effect Relative to Baseline	-16%	-29%	-13%	-24%	22%	0%	89%	-4%
Implied Elasticity to Employment	-	-	0.81	1.50	-1.38	0	-5.57	0.25
Implied Elasticity to Earnings	-	-	0.45	0.83	-0.76	0	-3.07	0.14
Observations	1,119,552	1,119,552	334,768	334,768	334,768	693,658	693,658	693,658

Notes: This table shows the effect of job loss due to a mass layoff on labor market outcomes (column 1 and 2), private HI enrollment (columns 3 to 5) and public hospitalization (columns 6 to 8). Labor income (column 2) is measured in Brazilian Reais. Estimates were computed using the difference-in-differences equation (2). Dependent variables are indicated at the top of each column. The explanatory variable of interest is a dummy $Treat_i$ equal to 1 for treated workers, interacted with a dummy $Post_t$ equal to 1 for the period after displacement. All regressions include individual and year fixed effects. The sample includes a treatment group of workers displaced in mass layoffs and a matched control group of workers not displaced in the same year, working in firms that did not experience mass layoffs. Standard errors clustered at the firm level are indicated in parenthesis. All coefficients, standard errors, and baseline means representing probabilities have been scaled by 100, and effects are thus interpreted in terms of percentage points. ***, ** and * represent $p < 0.01$, $p < 0.05$ and $p < 0.1$ respectively.

sorb those displaced individuals. On Table 2 we 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, but for women we also quantify an increase of 23% in the probability of enrollment in the latter.[16]

The middle-right graph in Figure 3 shows the estimated effect of job loss on the probability of in-patient admission to a public hospital, up to 4 years after layoff. It reveals that this impact is positive and growing for male workers. Table 2 contains point estimates for the average effect quantified over four years after layoff and includes their 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.[17] The estimate in column (6) of Panel A reveal an increase of 0.1 p.p.

[16]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.

[17]We do *not* attach a causal interpretation to such elasticity however, as this would require that layoffs

in the probability of hospitalization for male workers, from a baseline probability of 0.32%. Relative impacts thus correspond to a total of 30% over the baseline average. Conversely, no statistically significant effect is found for female workers, as shown in column (3), Panel B.

Columns (7) and (8) in Table 2 shows separate estimates by causes of hospitalization (diagnoses), which are grouped as either external or non-eternal, respectively.[18] Estimates in Panel (a) show that the effects on male hospitalization are driven both by causes classified as external and non external. Impacts from external causes are quantified 56% relative to the average baseline for male workers, while non-external causes are quantified 20% relative to the baseline. The two bottom graphs from Figure 3 show the dynamic effects on hospitalization separately for these two diagnosis groups. They further reveal that (the probability of) hospitalizations due to external causes increase immediately in the year after layoff, and remain positive and statistically significant in all subsequent years. Hospitalizations from non-external causes, on the other hand, have a more controlled character and become apparent only in the long run (i.e., a few years after layoff). Both patterns precede closely to the related ones found in Subsection 4.4, where we explore impacts over a broader set of specific diagnoses. Finally, as before, we find no statistically significant effects for female workers.

4.3 Effects on Mortality

The left graph from Figure 4 shows the yearly effect of job loss on the risk of mortality up to ten years after layoff, using the matching-based specification adapted from equation (1) and discussed in Section 3. Two striking patterns emerge. First, we find that job loss sharply increases the risk of male mortality by 0.065 p.p. in the first year after layoff. This effect, over a baseline average of 0.08%,[19] amounts to a total increase of 81% in the probability of death in the first year. Female mortality also appear to increase marginally in the first year, but the estimated effect are statistically insignificant and much less evident in comparison to the one from their male counterparts. Second, effects rapidly decrease from the second year on, and seem to dissipate completely around the sixth year after layoff. Such patterns

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.

[18] As mentioned in Section 3, 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.

[19] As we are unable to replicate the procedure from the previous section and calculate the baseline as the average probability for treated workers in the year before layoff, we rely on untreated (i.e., non-dismissed) workers as a counterfactual and calculate their average baseline probability using all subsequent periods after treatment.

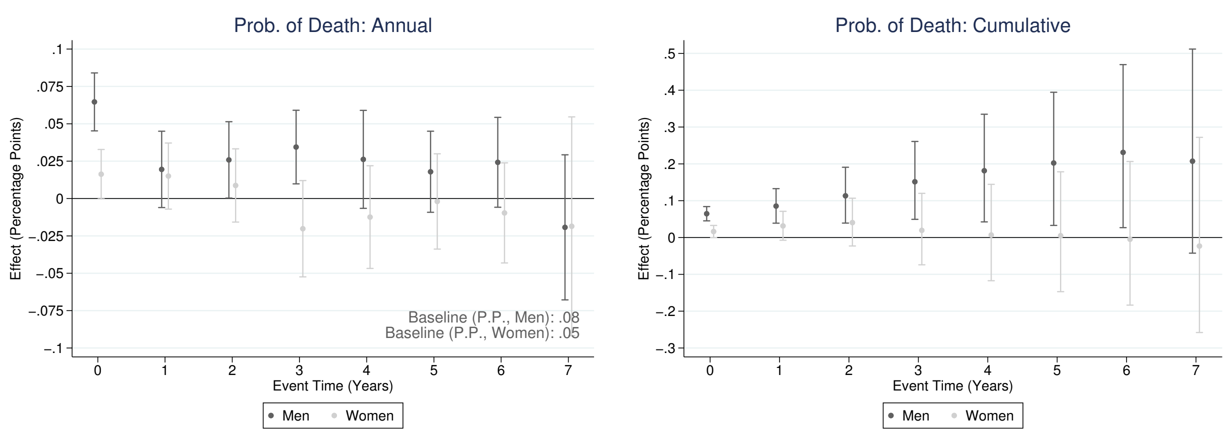
also agree with ones found elsewhere in the literature (e.g., Sullivan and Wachter, 2009).

The annual estimates from the left graph in Figure 4 can be used to calculate the cumulative effect of job loss on the probability of mortality for our sample of workers. Following the approach from Deryugina and Molitor (2019), at each year after job loss the change in cumulative mortality ΔM_t is given by

$$\Delta M_t = \prod_{t=0}^{T}(1 - m_t + \beta_t) - \prod_{t=0}^{T}(1 - m_t)$$

where β_t are the annual mortality effects of job loss and m_t is the empirical fraction of the laid-off workers who die in year t. Standard errors are analogously calculated using the estimates from the left-side graph. Results are shown in the right graph in Figure 4. A positive impact on cumulative mortality can be observed up until the last year in our window of analysis. After an initial surge, owing to the large effect quantified in the first year, it grows moderately until stabilizing in the latter years around a total of 0.2 p.p. By the last year, however, its confidence interval is wider and the estimated effect is statistically insignificant.

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. The left-side graph displays annual effects and the right-side graph displays cumulative effects. Outcomes are shown separately for both male (dark gray) and female workers (light gray). Estimates were computed using the matching-based 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.

A useful exercise to understand the social cost implied by the cumulative effects on mortality is to translate our estimates into subjective monetary figures using the concept of the Value of Statistical Life (VSL). Borrowing from Kniesner et al. (2012), whose VSL estimates range between US$4 to US$10 million, our cumulative mortality estimate suggests a total loss between US$8 to US$20 thousand per worker (between 16 and 40 thousand Brazilian Reais in 2012 values). Such numbers, even at their lower bound, surpass the total impacts on wages implied by Figure 4. For our selected sample of around 287 thousand male workers dismissed from mass layoffs between 2006 and 2014, they also imply aggregate losses between US$2 to US$6 billion.

Table 3: Effect of Job Loss on Mortality

	(1)	(2)	(3)
	Prob. of Death		
	Overall	by Cause	
		External	Non-Ext.
	Panel A: Men		
Point Estimate	0.0277***	0.0181***	0.0096***
	(0.0053)	(0.0038)	(0.0036)
Baseline Mean (Untreated, $t \geq 0$)	0.0821	0.0411	0.041
Effect Relative to Baseline	33%	44%	23%
plied Elasticity to Employment	2.36	3.14	1.64
Implied Elasticity to Earnings	1.18	1.57	0.82
Observations	1,889,691	1,889,691	1,889,691
	Panel B: Women		
Point Estimate	-0.0006	0.0017	-0.0024
	(0.0059)	(0.0026)	(0.0052)
Baseline Mean (Untreated, $t \geq 0$)	0.0514	0.0113	0.0401
Effect Relative to Baseline	-1%	15%	-5%
Implied Elasticity to Employment	0.06	-0.94	0.31
Implied Elasticity to Earnings	0.03	-0.52	0.17
Observations	1,022,529	1,022,529	1,022,529

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 $Treat_i$ 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. All coefficients, standard errors, and baseline means representing probabilities have been scaled by 100, and effects are thus interpreted in terms of percentage points. ***, ** and * represent $p < 0.01$, $p < 0.05$ and $p < 0.1$ respectively.

Point estimates for the average effects on mortality are shown in Table 3. 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 (33% increase over a baseline of 0.08 p.p.), and both on the risk of dying from non-external causes (23% increase over a baseline of 0.04 p.p.) and external causes (44% increase also over a baseline of 0.04 p.p.). Yet again, estimates for female workers are small and statistically insignificant.

Building on our findings from the previous subsection, the increase in deaths by non-external causes suggests that these may come as consequences from specific medical conditions that are likely associated with unemployment – higher stress and/or anxiety being examples of possible mediators. Conversely, an increase in deaths by external causes suggests the possibility of increased exposure to risky behavior. Both findings could also be directly or indirectly triggered by liquidity constraints. We explore these alternative possibilities in what follows.

4.4 Disaggregated Outcomes

In Panels (a) and (b) of Figure 5, respectively, we distinguish the effects on hospitalization and mortality for male workers by different causes according to standard ICD-10 classification.[20] As before, we group all events into either external causes or non-external causes. Following the patterns uncovered in the previous section, for each outcome in Figure 5 we also distinguish its short-term effect (i.e. that corresponding to the first year after layoff) from its 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:

$$\begin{aligned} Y_{it} = \alpha + \delta\, Treat_i + \beta_{ST}\, Treat_i \cdot Post_{(t=0)} + \lambda_{ST}\, Post_{(t=0)} \\ + \beta_{LT}\, Treat_i \cdot Post_{(t\geq 1)} + \lambda_{LT}\, Post_{(t\geq 1)} + \epsilon_{it} \end{aligned} \tag{3}$$

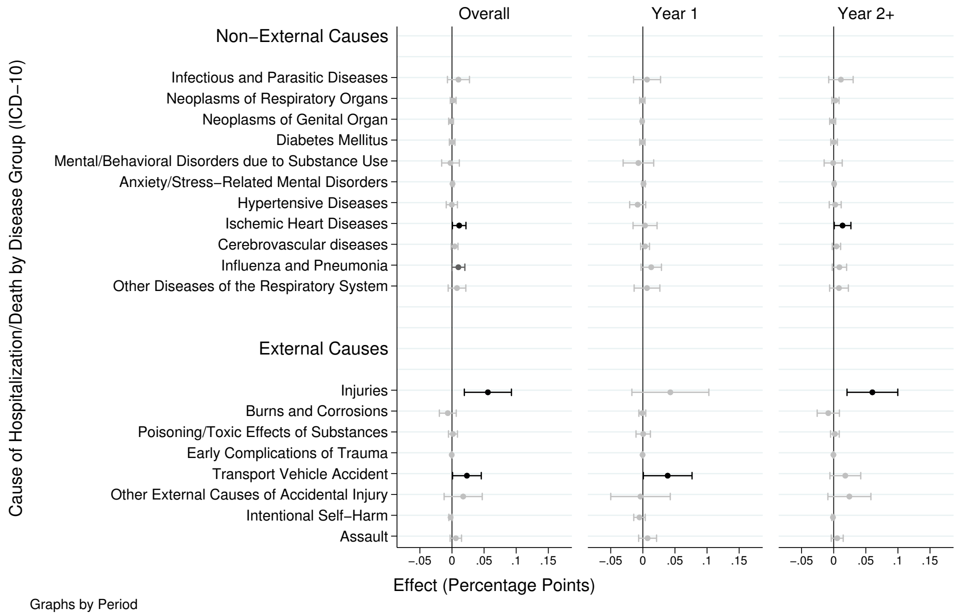
where the coefficients β_{ST} and β_{LT} represent short- and long-term effects, respectively. As in Section 4, we adapt equation (3) above to a matching-based equation to estimate the effects on mortality.

Our findings are twofold. First, we find positive and statistically significant effects for male workers on several disease groups categorized within non-external causes, namely, higher incidence of hospitalizations due to ischemic heart diseases, and influenza or pneumonia (p-value = 0.052). Breaking down the effects into long- and short-term ones, we find short-term impacts on mortality from ischemic heart diseases, cerebrovascular diseases, and other

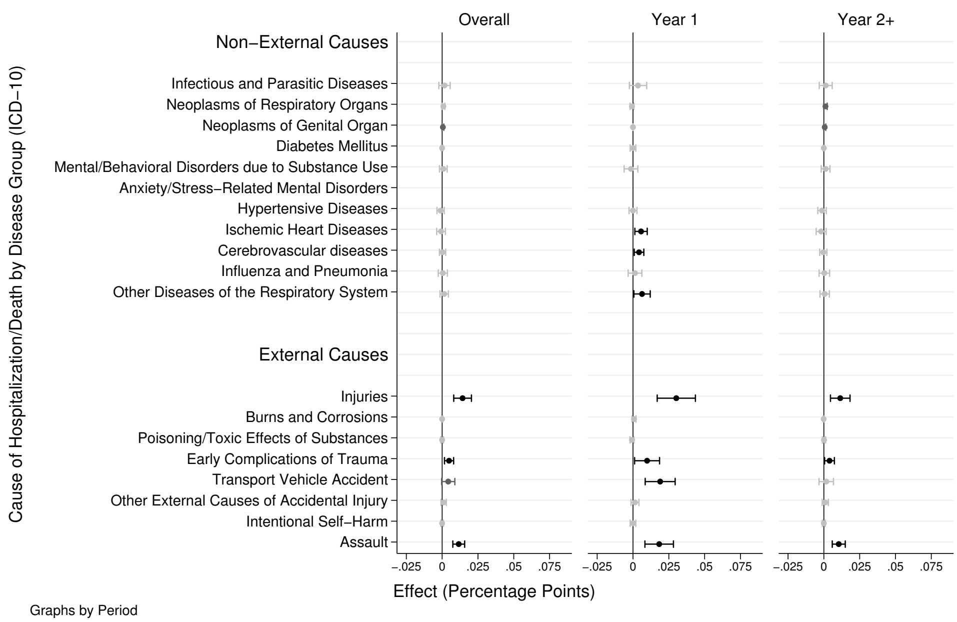
[20]In this section, as for the remainder of our analysis, we focus on male workers only since the impacts uncovered in the previous subsections are mostly for the sample of male workers. All exercises in the remainder of the paper were replicated for the sample of female workers. Unless explicitly reported, all results for female workers are either economically or statistically insignifficant.

Figure 5: 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). 95% confidence intervals are reported. All estimates and confidence intervals are computed using the sample for male workers. Estimates indicated in black are statistically significant at the 5% level, while those indicated in dark gray are statistically significant at the 10% level. Estimates in light gray are statistically insignificant. Estimates for hospitalizations are computed with the difference-in-differences equation (2) and estimates for mortality are computed with the matching-based equation adapted from equation (2).

diseases of the respiratory system. We also find very faint evidence of long-term increases in neoplasms of the genital organ (i.e., prostate cancer, p-value $= 0.096$) and of the respiratory organs (i.e., lung cancer, p-value $= 0.071$).[21] Specialized literature both in economics and medicine have previously connected such set of findings with the possibility of follow-up consequences of stress.[22] 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 large increases in hospitalizations and mortality of male workers due to external causes, as seen from the high estimates for injuries, early complications of trauma, transport vehicle accidents, and assaults in the two panels from Figure 5 (both in the short- and long-term). We find no evidence of an increase in suicides, which are 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 prone to accidents (e.g., reckless driving) or to situations involving higher risks to one's physical integrity (e.g., fighting and other forms of physical altercation).

4.5 Heterogeneity Analysis

Our next exercise is to investigate whether our previous results can be explained by 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.

Results are displayed in Figure 6. Each panel shows the average effects of job loss on the indicated outcome (i.e. probability of hospitalization or death) for different partitions following sample quartiles from each indicated characteristic. Each results is shown separately for external causes and non-external causes of hospitalization/death.

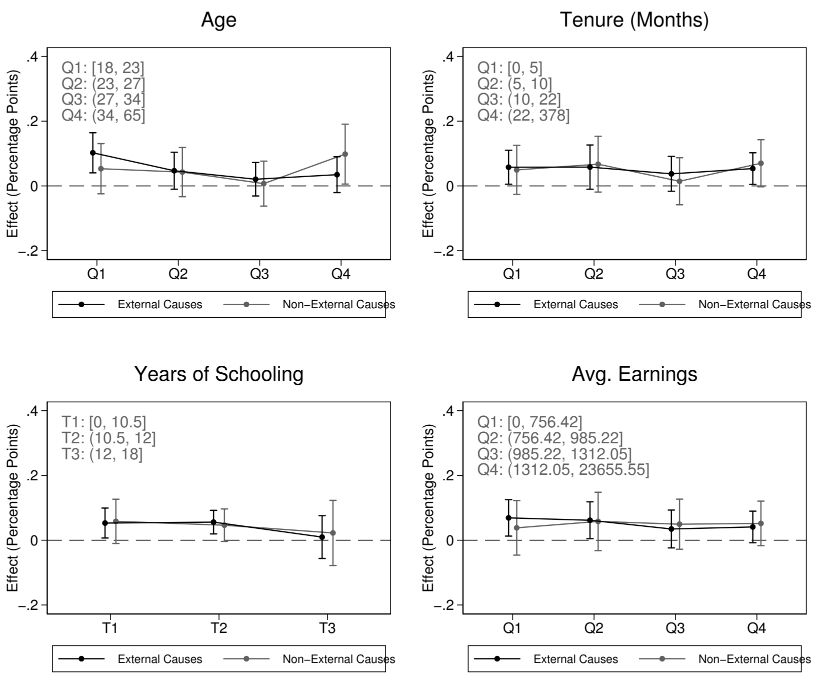
Estimates for hospitalization, shown in Panel (a), suggest that age is an important measure in explaining differences in effects across workers. We find that impacts on hospitalization due to external causes have a higher gradient towards younger cohorts. With the

[21] The latter of which could, in turn, be associated with later developments of unhealthy habits, such as smoking; see e.g. Lynge (1997).

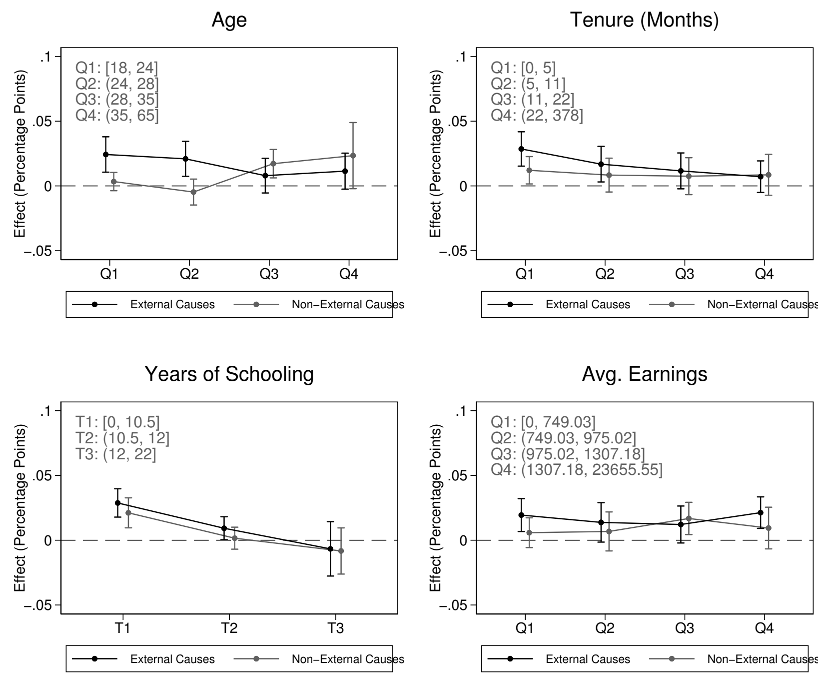
[22] An overview of possible mechanisms and supporting literature is provided in Eliason and Storrie (2009a).

Figure 6: 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. 95% confidence intervals are reported. All estimates and confidence intervals are computed using the sample for male workers. Estimates for hospitalizations are computed with the difference-in-differences equation (2) and estimates for mortality are computed with the matching-based equation adapted from equation (2).

partition by quartiles, results are statistically significant only for the group of workers in the lower quartile (18-23 years old) while also being statistically different from the point estimate in the third (28-34 years old) and the fourth quartiles (35-65 years old). Conversely, only the estimate for non-external causes in the fourth quartile is statistically different from zero. Panel (b) shows that estimates for mortality follow a similar pattern to those found for hospitalization. Effects on mortality from external causes are also mostly concentrated in younger cohorts (18-28 years old), 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 also 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 could, to some extent, be explained along the lines of different characteristics associated to either age group. Younger cohorts, for example, could be more likely to engage in risky behavior or in dealing with the frustration or financial constraints they face when losing a job,[23] while older cohorts may bear a larger share of the direct risks job displacement imposes on medical conditions, such as increased risk of heart attacks.

Table 4 reports point estimates on the probabilities of hospitalization and death by different age groups, following a more general division between older and younger workers. Panel A shows results for male workers at or above 32 years old (i.e., the upper sample tercile) and Panel B shows results for male workers below 32 years old. Results confirm that the risk of hospitalization and mortality from non-external causes is specific for the group of older workers. Hospitalization and mortality from external causes, however, are shown to be prevalent across both groups, further confirming that the health impacts of job loss are more clearly revealed through such causes (e.g., injuries and accidents). Effects relative to baseline means (as defined for each outcome) are also reported.

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.

[23]For example, hospitalization or deaths due to assaults, to the extent in which they driven by younger workers, could also be associated to criminal behavior – 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.

Table 4: Effects of Job Loss on Public Hospitalization (Male Workers), by Age Groups

	(1)	(2)	(3)	(4)	(5)	(6)
	Prob. of Public Hospitalization			**Prob. of Death**		
	Overall	by Cause		Overall	by Cause	
		External	Non-Ext.		External	Non-Ext.
	Panel A: Older Workers ($\geq$ 32 Years Old)					
Point Estimate	0.1304***	0.0518**	0.0810**	0.0355***	0.0155***	0.0199**
	(0.0452)	(0.0239)	(0.0379)	(0.0109)	(0.0056)	(0.0092)
Baseline Mean	0.3603	0.0811	0.2857	0.1206	0.0338	0.0867
Effect Relative to Baseline	36%	63%	28%	29%	45%	22%
Observations	416,836	416,836	416,836	633,414	633,414	633,414
	Panel B: Younger Workers (< 32 Years Old)					
Point Estimate	0.0822***	0.0518***	0.0337	0.0237***	0.0195***	0.0043
	(0.0293)	(0.0179)	(0.0235)	(0.0056)	(0.0049)	(0.0028)
Baseline Mean	0.3062	0.0973	0.212	0.0627	0.0447	0.018
Effect Relative to Baseline	26%	53%	15%	37%	43%	23%
Observations	868,028	868,028	868,028	1,256,277	1,256,277	1,256,277

Notes: This table shows the effect of job loss due to a mass layoff on the probability of hospitalization (column 1 to 3) and death (columns 4-6). Panel A reports estimates for older workers ($\geq$ 32 years old) and Panel A reports estimates for younger workers (< 32 years old). Hospitalization estimates were computed using the difference-in-differences equation (2) and mortality estimates using the matching-based equation adapted from equation (2). Dependent variables are indicated at the top of each column. 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. All coefficients, standard errors, and baseline means representing probabilities have been scaled by 100, and effects are thus interpreted in terms of percentage points. ***, ** and * represent $p < 0.01$, $p < 0.05$ and $p < 0.1$ respectively.

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 (e.g., Gathman et al., 2021). 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 a few 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 5 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. We find only a small spillover effect of job loss on spouses' labor outcomes, as estimates in Panel A.1 show that spouses of male workers suffer a 3% income decrease following their husband's

Table 5: 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						
Point Estimate	-0.5473	-259.03**	-0.4558	-0.0034	-0.0190	-0.0016	-0.0048
	(0.5758)	(109.79)	(0.5123)	(0.0224)	(0.0637)	(0.0037)	(0.0079)
Baseline Mean (Treated, $t = -1$)	45.0485	7362.60	12.6504	0.0377	0.3628	0.0047	0.0191
Effect Relative to Baseline	-1%	-3%	-3%	-8%	-5%	-33%	-24%
Observations	233,667	233,667	101,493	173,306	173,306	292,419	292,419
	Panel A.2: Children of Male Worker						
Point Estimate	-	-	-0.6375	0.0499**	-0.0060	0.0046	0.0055
	-	-	(0.7520)	(0.0251)	(0.0785)	(0.0033)	(0.0066)
Baseline Mean (Treated, $t = -1$)	-	-	17.5897	0.0353	0.5869	0	0.0054
Effect Relative to Baseline	-	-	-3%	141%	-1%	-	99%
Observations	-	-	132,433	195,020	195,020	255,966	255,966
	Panel B.1: Spouse of Female Worker						
Point Estimate	-0.0395	69.72	-1.3888**	-0.0697	0.0193	0.0169	0.0348
	(0.6577)	(270.17)	(0.06241)	(0.0500)	(0.0817)	(0.0110)	(0.0271)
Baseline Mean (Treated, $t = -1$)	64.6089	14252.92	10.9795	0.058	0.3776	0	0.0097
Effect Relative to Baseline	0%	0%	-12%	-119%	5%	-	355%
Observations	133,231	133,231	54,432	92,897	92,897	143,220	143,220
	Panel B.2: Children of Female Worker						
Point Estimate	-	-	-1.3011	0.0772*	0.0712	0.0073	0.0064
	-	-	(0.8050)	(0.0407)	(0.1091)	(0.0098)	(0.0060)
Baseline Mean (Treated, $t = -1$)	-	-	17.5031	0.0419	0.5039	0	0
Effect Relative to Baseline	-	-	-7%	183%	14%	-	-
Observations	-	-	64,820	97,797	97,797	128,475	128,475

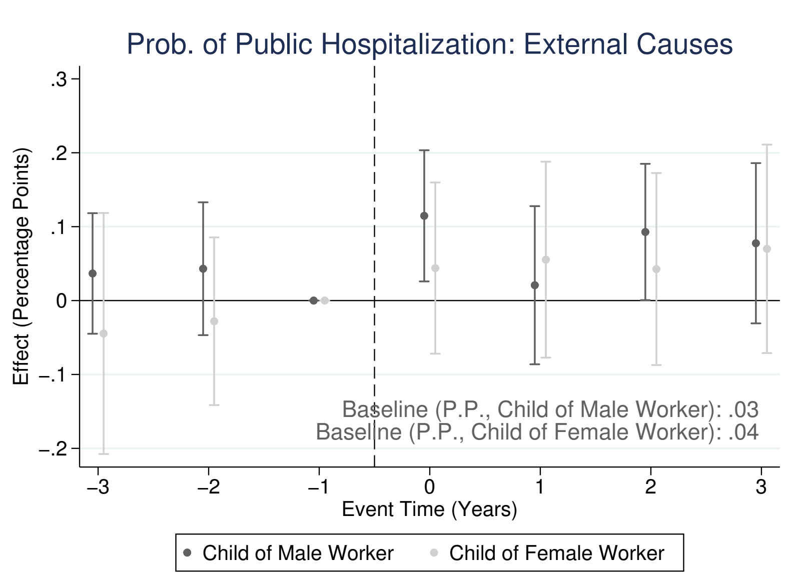
Notes: This table shows the effect of job loss due to a mass layoff on the probability of admission to a public hospital for children and spouses of dismissed workers. It includes estiamtes for labor market outcomes (columns 1-2, spouses only) health insurance enrolment (column 3), hospitalization due 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 $Treat_i$ equal to 1 for treated workers, interacted with a dummy $Post_t$ equal to 1 for the period after displacement. All regressions include individual and year fixed effects. The sample includes a treatment group of workers displaced in mass layoffs and a matched control group of workers not displaced in the same year, working in firms that did not experience mass layoffs. Standard errors clustered at the firm level are indicated in parenthesis. All coefficients, standard errors, and baseline means representing probabilities have been scaled by 100, and effects are thus interpreted in terms of percentage points. ***, ** and * represent $p < 0.01$, $p < 0.05$ and $p < 0.1$ respectively.

job dismissal. Panel B.1 also shows a 12% decrease in the probability of enrollment in HI for spouses of female workers, suggesting that a significant parcel of them could be enrolled in HI plans linked to their wife's employment. Although effects are economically marginal, they do seem to indicate some association between a worker's employment status and their spouse's. However, we do not find evidence on spillover effects of job loss on spouse's health for either gender, as estimates for hospitalization and mortality in Panels A.1 and B.1 are statistically insignificant.[24]

[24]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.

Panels A.2 and B.2 reports spillover estimates for children of male and female workers, respectively. We find large increases in the probability of children's hospitalization due to external causes, with proportional impacts measured at 141% for the children of male workers and 183% (p-value= 0.058) for children of female workers. Figure 7 shows the dynamic representation of these findings. We note from the figure that the effect on children of male workers is explained by a high impact on the probability of hospitalization in the first year after layoff, which remains positive but less discernible in the years thereafter. For children of female workers, dynamic effects are statistically insignificant. These results open 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 also reflecting an increase in domestic violence. Both possibilities, in fact, are consistent with recent findings by Fontes et al. (2022), who show that parental job loss worsens children's mental health screening results for a sample of children from two big Brazilian cities, and that such effects are potentially explained by exposure to abuse and neglect.

Figure 7: Effect of Job Loss on Children's Hospitalization (External Causes)



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

4.7 Alternative Specifications and 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 B.1 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.

In Appendix B.2 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 B.3 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 B.4 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 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 benefits in the latter. This last rule permits us build a clean regression-discontinuity (RD) design for the sub-sample of workers who are either barely eligible and barely ineligible to claim UI. More specifically, we estimate the following equation:

$$Y_{it} = \alpha + \beta D_i + f(X_i) + \epsilon_{it} \tag{4}$$

where Y_{it} is the outcome of interest (e.g., benefits claimed, hospitalization and mortality) measured up to one year after workers' layoff date; X_i, the running variable, is the difference between the most recent layoff date and the previous layoff date used to claim UI, normalized such that $X = 0$ at the cutoff required for eligibility (i.e., 16 months between the two most recent layoffs); $f(.)$ is a flexible polynomial function of the running variable; 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. 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 claimed UI benefits following a dismissal from a previous employment around 16 months earlier. We also restrict the sample to include only workers who claimed at least 3 months of UI benefits after their previous dismissal, as ineligible workers may still claim remaining benefits from a previous spell if these were not claimed in full. We keep our window of analysis between 2006 and 2014 and focus on male workers, since all health impacts uncovered in the previous fell on this group only.

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 employment rates at 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 are shown in Table 6, which shows its effect on program take-up, total amount of benefits claimed, and the probabilities of HI enrollment, hospitalization, and death, the latter two divided between effects by external and non-external causes. As in Subsection 4.4 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 older workers at or above 32 years old (the approximate median age in this sample), and Panel C for younger workers below 32 years old.

The first two columns in Table 6 reveal large positive impacts on both "first-stage" outcomes around the 16-months cutoff. Estimates in Panel A show that barely eligible workers are around 58% 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$1,665 (about US$850 in 2012 values) more in total benefits (column 2). Estimates remain quantitatively similar in

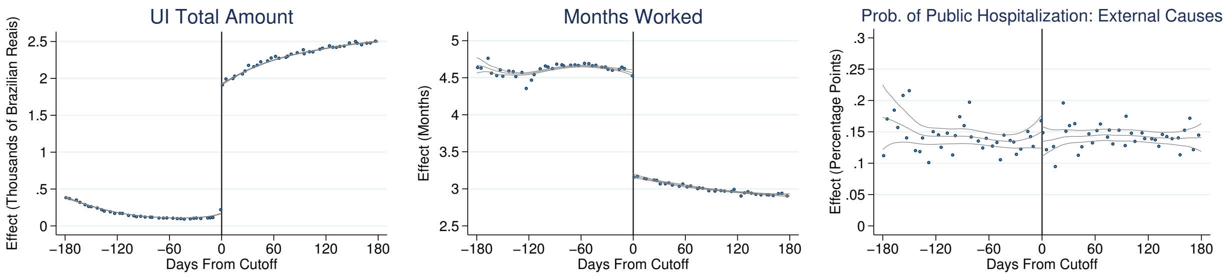
Table 6: Main Effects of UI Eligibility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Prob. of	Total Amount	Labor Market Outcomes		Prob. of HI	Prob. of Hospitalization		Prob. of Mortality	
	Take-Up		Months Worked	Labor Income	Enrollment	Ext. Causes	Non-Ext. Causes	Ext. Causes	Non-Ext. Causes
	Panel A: All Workers								
Point Estimate	58.1***	1,776***	-1.398***	-1,665***	-0.188**	0.0077	0.0104	-0.0208*	0.0036
	(0.164)	(5.536)	(0.0221)	(35.87)	(0.0956)	(0.0165)	(0.0234)	(0.0126)	(0.0094)
Mean Outcome (at Cutoff)	7.1383	114.16	4.6325	5860.18	4.9724	0.1345	0.2857	0.0864	0.0478
Effect Relative to the Mean	-	-	-30%	-28%	-3%	-5%	3%	-24%	7%
Observations	819,196	819,196	819,196	819,196	819,196	819,196	819,196	819,196	819,196
	Panel B: Older Workers ($\geq$ 32 Years Old)								
Point Estimate	57.6***	1,848***	-1.355***	-1,798***	-0.0305	-0.0529**	0.0105	-0.0182	0.0065
	(0.240)	(8.406)	(0.0334)	(60.66)	(0.137)	(0.0217)	(0.0359)	(0.0177)	(0.0182)
Mean Outcome (at Cutoff)	7.4474	126.94	4.4353	6181.63	4.8263	0.1186	0.3268	0.0751	0.0797
Effect Relative to the Mean	-	-	-30%	-29%	0%	-44%	3%	-24%	8%
Observations	390,690	390,690	390,690	390,690	390,690	390,690	390,690	390,690	390,690
	Panel C: Younger Workers (< 32 Years Old)								
Point Estimate	58.6***	1,712***	-1.440***	-1,540***	-0.333**	0.0331	0.0109	-0.0233	0.0013
	(0.226)	(7.285)	(0.0292)	(40.56)	(0.133)	(0.0246)	(0.0304)	(0.0178)	(0.0071)
Mean Outcome (at Cutoff)	6.8596	102.64	4.8104	5570.30	5.1041	0.1489	0.2487	0.0966	0.0191
Effect Relative to the Mean	-	-	-29%	-27%	-6%	22%	4%	-24%	6%
Observations	428,506	428,506	428,506	428,506	428,506	428,506	428,506	428,506	428,506

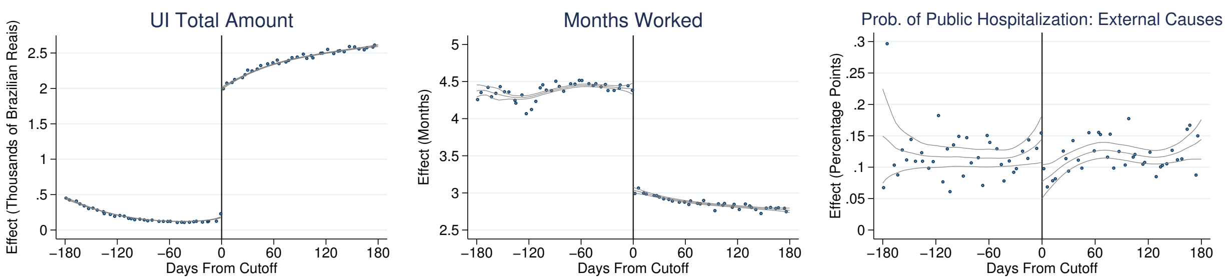
Notes: The first two columns in this table show the first-stage effects of UI eligibility on the probability of UI take-up (column 1) and the total amount of claimed benefits (column 2). The remaining columns show the second-stage effects of UI take-up on labor market outcomes (columns 3 and 4), on the 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. All coefficients, standard errors, and baseline means representing probabilities have been scaled by 100, and effects are thus interpreted in terms of percentage points. ***, ** and * represent $p < 0.01$, $p < 0.05$ and $p < 0.1$ respectively.

Figure 8: Main Effects of UI Eligibility

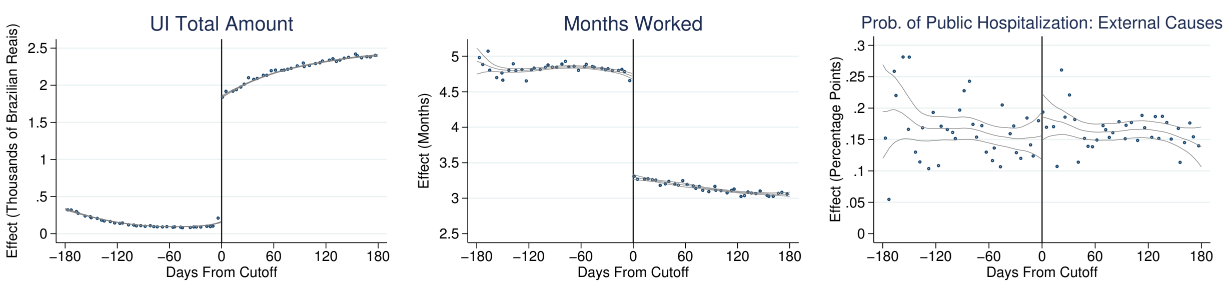
(a) All Ages



(b) Older Workers (≥ 32 Years Old)



(c) Younger Workers (< 32 Years Old)



Note: 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 older workers at or above 32 years old and Panel (c) for younger workers below 32 years old. 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.

the age-restricted samples from Panels B and C, with older workers slightly higher benefits than younger workers. Effects on benefits claimed for each sample are also displayed visually in the left-side RD plots shown in Figure 8.

Table 6 also includes estimates for the effects of UI eligibility on labor and health outcomes. 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

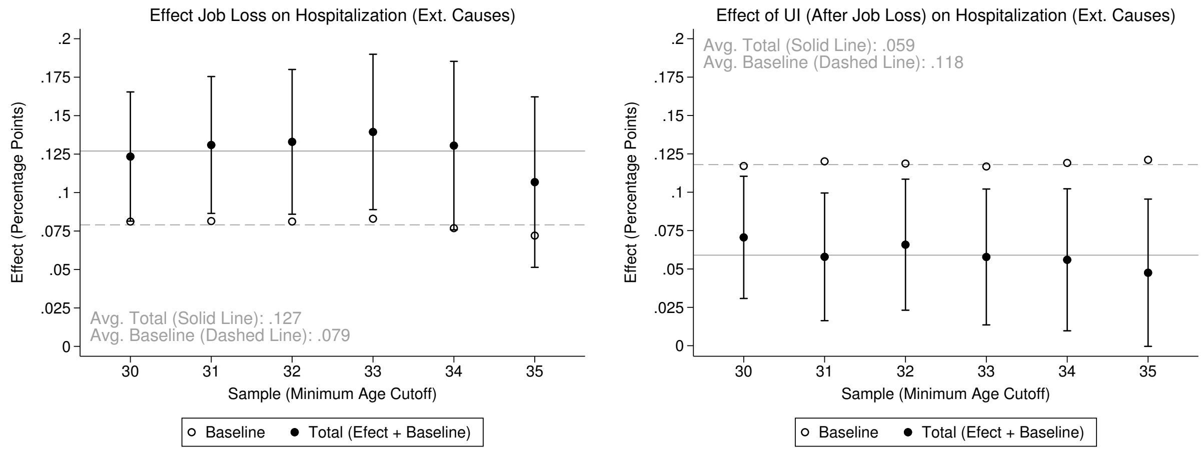
2.5 less months than ineligible workers in the year after layoff, and earn an average of R$2,980 less in labor income (about US$1,500 in 2012 values). The negative effect on labor income is even higher for older workers, who earn an average of R$3,330 less (about US$1,650 in 2012 values). This, in turn, 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. We also note that, for each group, forgone labor earnings are very closely compensated by total UI benefits received. Figure 8 shows the RD plots of the effect of UI eligibility on the number of months worked in a year for each age group.

Columns (5) through (9) show the impacts of UI take-up on the probabilities of enrollment in private HI plans, admissions to public hospitals, and deaths, up to one year after layoff. The two latter outcomes are divided between external and non-external causes. Results are as follows. First, we find no economically relevant increase in HI enrollment apart from a small, negative effect on younger workers (a 6% decrease in relation to the baseline of younger 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 HI enrollment suggests that non-employer-sponsored HI 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 eligibility on the probability of hospitalization of older workers from external causes (column 6). The point estimate in Panel B quantifies an negative impact of 0.053 p.p., equivalent to a 44% decrease in comparison to the baseline of ineligible workers. This effect is also robust to a range of different functional forms and specifications, as reported in Table D1. The correspondent RD plot is shown in the right-side graph from Figure 8, Panel B.

Figure 9 provides a visual connection between our findings on older workers' hospitalization from this section and the ones from the previous section. The leff-side graph plots the total estimated effects of job loss on hospitalization from external causes (black circles) over baseline means for the outcome variable before treatment (hollow circles). Estimates are shown for different choices of age thresholds defining the sample of older workers. From this graph we learn that the total probability of hospitalization from external causes (i.e., pre-treatment mean plus treatment effect) for older workers after losing their jobs is estimated at around 0.127%. This value is remarkably similar to the *baseline* value for this outcome (around 0.118%) in the subsample of dismissed older workers in the threshold of UI eligibility. This can be seen by comparing the solid gray line in the left-side graph with the dashed gray line in the right-side graph, which replicates this same exercise using our

Figure 9: Summary of Job Loss and UI Effects on Hospitalization (External Causes) for Older Cohorts, Robustness to Sample Cutoff Ages



Notes: This figure shows baselines point estimates for the effect of job loss on hospitalization from external causes (left-side graph) and for the effect of UI eligibility (after job loss) on the same outcome. Samples include older workers only, and estimates are shown for samples with different minimum age thresholds. At each graph, hollow circles represent the baseline mean for each sample and black circles are the point estimates for each sample added to their baseline values. 95% confidence intervals are also reported.

estimates from the present section. When accounting for the effect of UI, the estimated probabilities in the right-side graph (also shown for different choices of minimum age cutoffs when restricting the sample) also reveal that UI eligibility more than offsets the adverse effect of job loss quantified in the left-side graph. We see this by comparing the total probabilities (i.e., baseline plus effects) in the right graph (approx. 0.059%, on average) with pre-displacement baselines in the left graph (approx. 0.079%, on average).

Two different channels are likely to explain the effects of UI on health (Kuka, 2020). 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 13% and 16%), an increase in the probability of admission to public hospitals (30%) and an increase in the risk of mortality (33%), 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 impacts are stronger at both the lower and upper ends of our sample's age distribution.

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 hospital admissions associated with risk behavior (injuries and accidents) 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.

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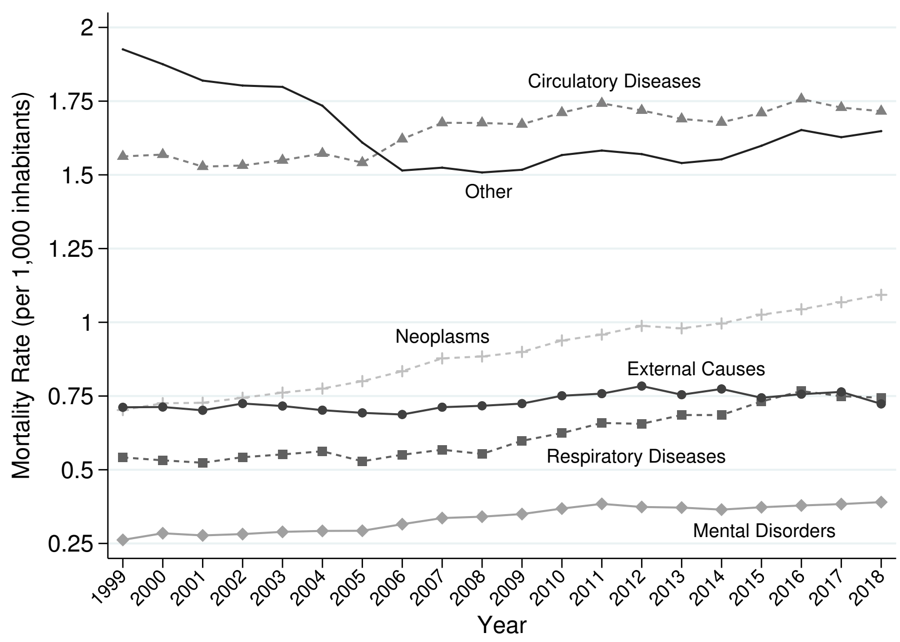
A Appendix to Section 2

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: Disaggregated Mortality Trends

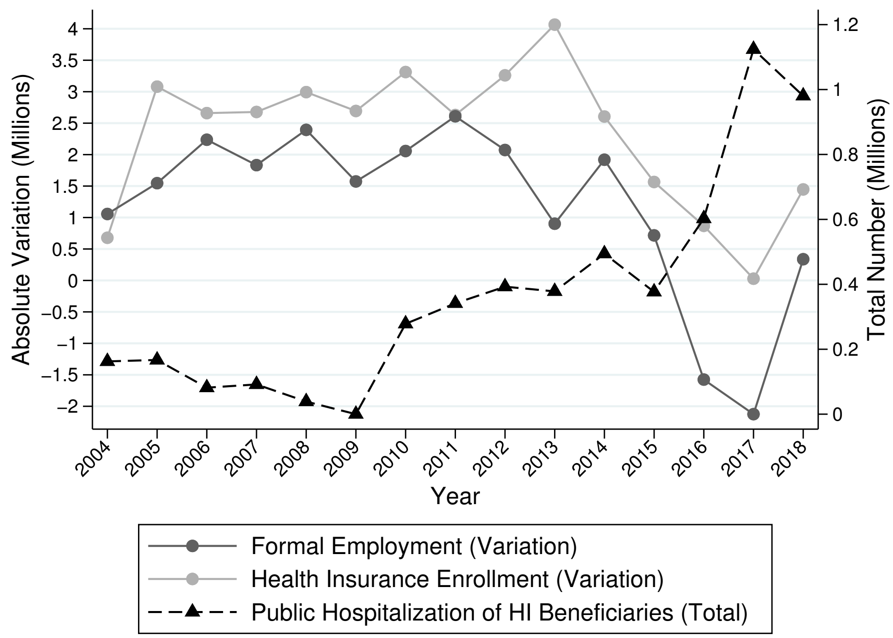


Notes: This figure the evolution in the adult mortality rate for the Brazilian population between 1999 and 2018 for the five leading causes of death according to ICD-10 disease chapters (all other chapters are grouped into "other").

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: 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).

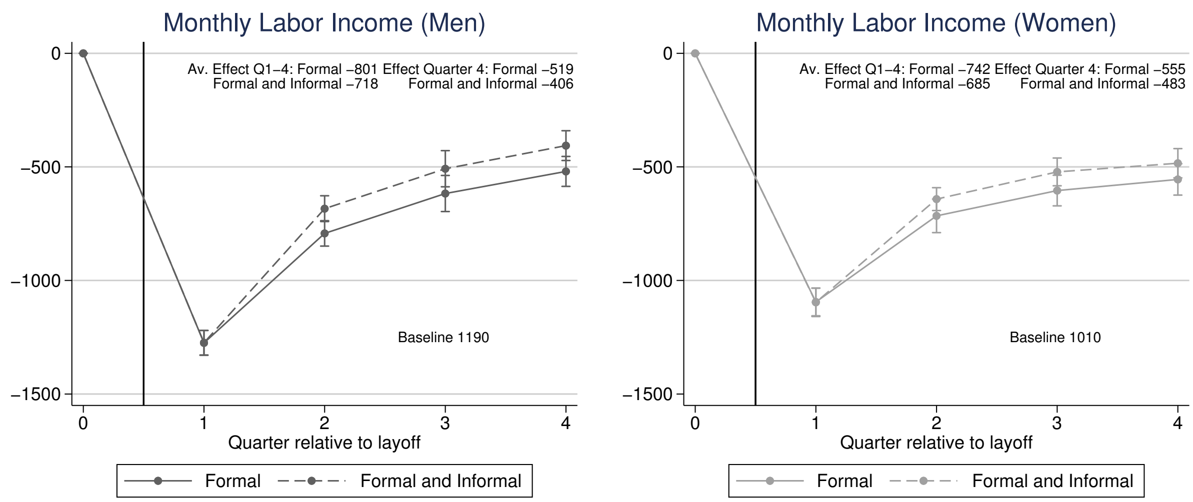
A.3 Differences in Formal and Informal Labor Earnings

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.

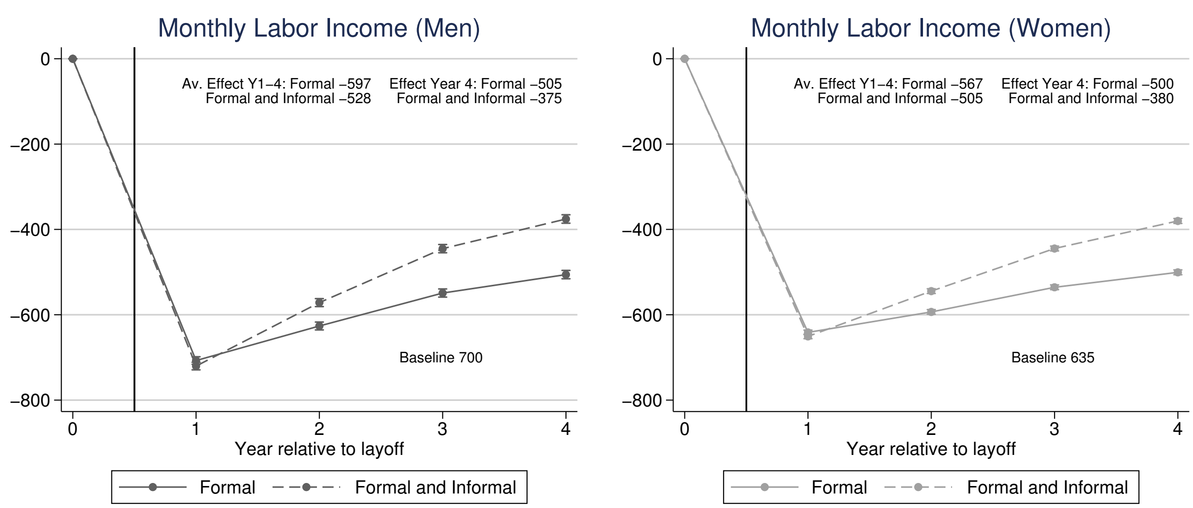
In Figure A3 we plot the average labor income trajectories of individuals employed at time $t = 0$ and were out of employment at time $t = 1$, separately for each gender and each different dataset. Panel (a) shows that in the representative sample, the effect on formal earnings in the last quarter (i.e. 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: 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.

B Appendix to Section 4

B.1 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).[25,26]

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:

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

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 $(\partial HI_t / \partial\,(Treat \cdot Time_t))$ and on public hospitalization $(dY_t / d\,(Treat \cdot Time_t))$, also

[25] 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.

[26] 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 3).

with equation (1):

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

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

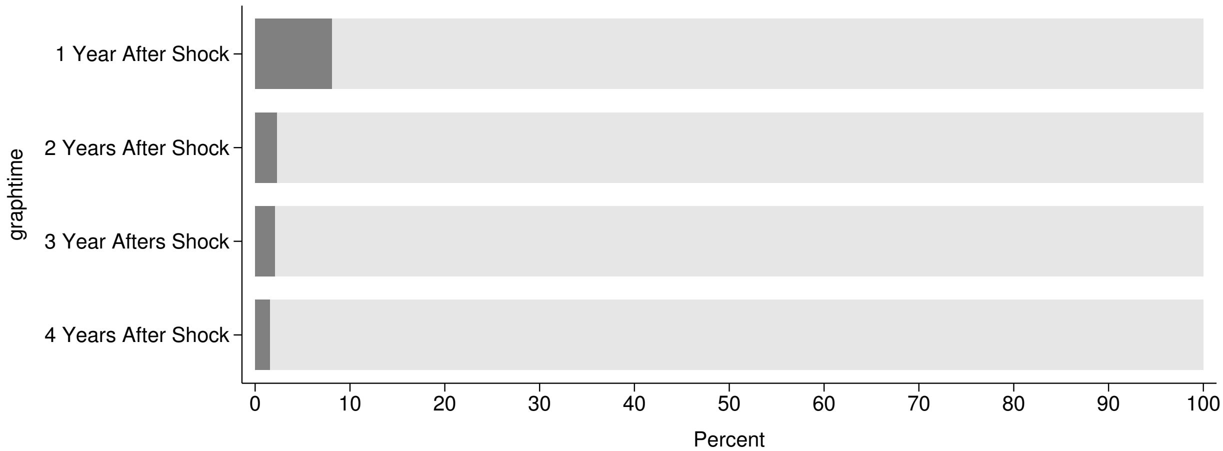
Finally, using expression (5) above, we calculate the relative contribution of HI_t to the impact of job loss at each subsequent period as the ratio $\frac{\phi \times \beta_t^{HI_2}}{\beta_t}$. The remaining unexplained part is analogously computed as $R_t = 1 - \frac{\phi \times \beta_t^{HI_2}}{\beta_t}$.

Results are displayed in Figure C1. 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,[27] they do not sufficiently explain the total impacts of job loss on public hospital admissions – which are thus more likely to reflect direct impacts of job loss on the health of individuals.

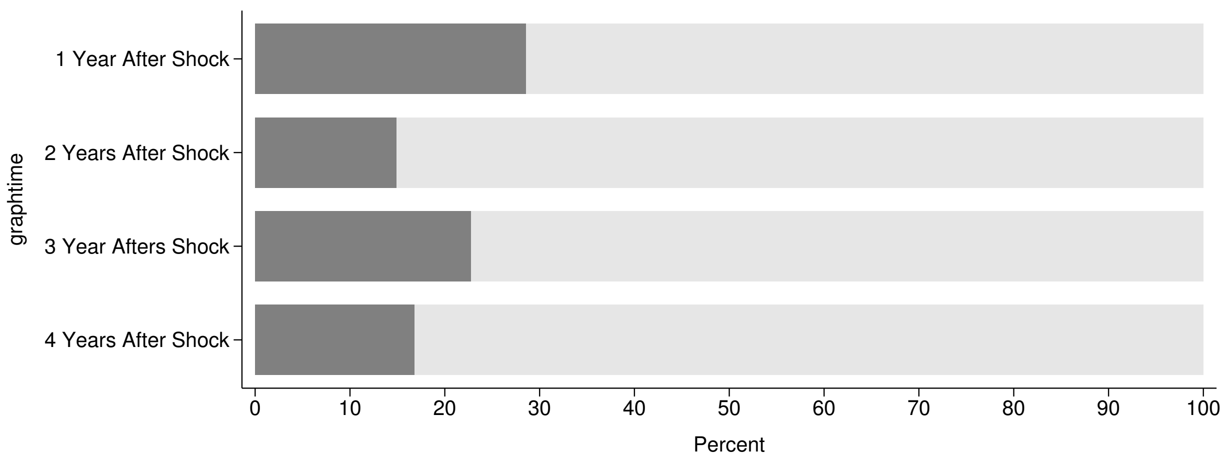
[27] This possibility also implicitly assumes that *all* effects on public hospital admissions mediated by changes in health insurance enrollment is due to individuals simply trading one type of care for the other. Such effects, of course, could also to some extent reflect an actual deterioration on 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.

Figure C1: 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 B.1. Results in Panel (a) are calculated using the full sample from the main analysis. Results in Panel (b) are calculated with a restricted sample of workers who were enrolled at a health insurance plan at time $t = 0$ (i.e. at the time of layoff). Dark gray bars show the ratio $\phi \times \beta_t^{HI_2} / \beta_t$ for each year following layoff. Light gray bars show the remaining values R_t.

B.2 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.

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

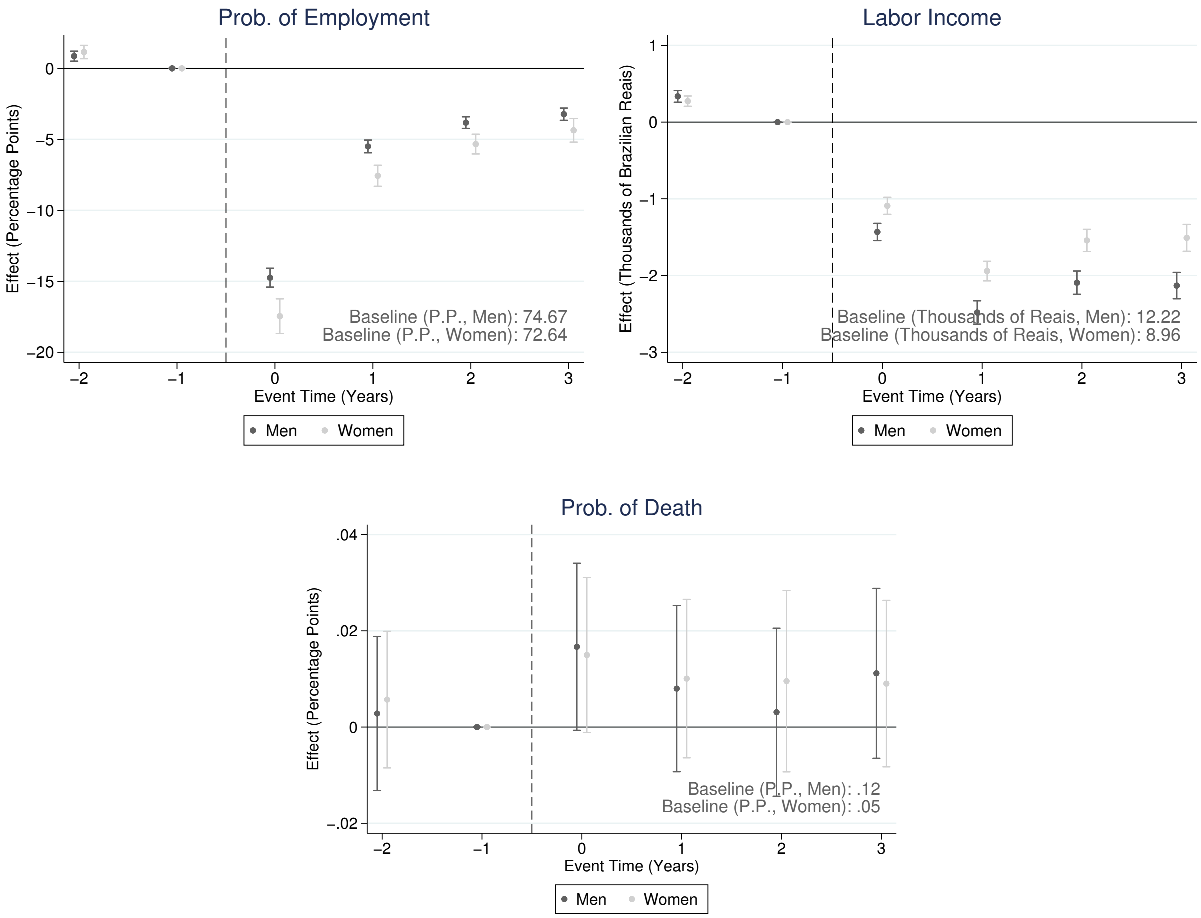
	(1)	(2)	(3)
	Labor Market Outcomes		Mortality
	Employment	Income	
	Panel A: Men		
Point Estimate ($t = 0$)	-15.1732***	-1599.79***	0.0153**
	(0.3143)	(56.49)	(0.0076)
Point Estimate ($t > 0$)	-4.6131***	-2402.65***	0.0060
	(0.1827)	(74.69)	(0.0056)
Baseline Mean (Treated, $t\bar{-}1$)	74.6766	12227.83	0.1262
Effect Relative to Baseline			
· at $t = 0$	20%	13%	12%
· at $t \geq 0$	6%	20%	5%
Observations	8,997,546	8,997,546	8,807,684
	Panel B: Women		
Point Estimate ($t = 0$)	-18.0341***	-1227.29***	0.0121*
	(0.5980)	(57.30)	(0.0070)
Point Estimate ($t > 0$)	-6.3295***	-1800.74***	0.0067
	(0.3425)	(74.47)	(0.0056)
Baseline Mean (Treated, $t\bar{-}1$)	72.6455	8961.63	0.0559
Effect Relative to Baseline			
· at $t = 0$	25%	14%	22%
· at $t \geq 0$	9%	20%	12%
Observations	4,683,804	4,683,804	4,640,713

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 $Treat_i$ equal to 1 for treated workers, interacted with a dummy $Post_t$ equal to 1 for the period after displacement. All regressions include individual and year fixed effects. The sample includes a treatment group of workers 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. All coefficients, standard errors, and baseline means representing probabilities have been scaled by 100, and effects are thus interpreted in terms of percentage points. ***, ** and * represent $p < 0.01$, $p < 0.05$ and $p < 0.1$ respectively.

We use equations (1) and (3), 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.

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 7 in the main analysis. Finally, in the lower graph of Figure C2, which shows the estimates for the dynamic effects, we see that estimated pre-trends are very close to zero.

Figure C2: 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.

B.3 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: Effects of Job Loss on Health Outcomes (Male Workers), Varying Mass Layoff Intensity

	(1)	(2)	(3)	(4)	(5)
	Panel A: HI Enrollment				
Point Estimate	-2.5835***	-1.5345**	-3.0253***	-3.2667***	-2.2970***
	(0.3390)	(0.6125)	(1.0637)	(0.5617)	(0.7923)
Mass Layoff Sample	> 33%	> 50%	closure	> 100 workers	> 250 workers
Observations	628,614	226,352	80,297	353,521	218,785
	Panel B: Hospitalization				
Point Estimate	0.0981***	0.0950**	0.0686	0.1100***	0.1088**
	(0.0250)	(0.0440)	(0.0645)	(0.0344)	(0.0445)
Mass Layoff Sample	> 33%	> 50%	closure	> 100 workers	> 250 workers
Observations	1,284,864	466,452	160,804	727,741	455,385
	Panel C: Mortality				
Point Estimate	0.0277***	0.0292***	0.0476***	0.0262***	0.0321***
	(0.0053)	(0.0091)	(0.0127)	(0.0077)	(0.0105)
Mass Layoff Sample	> 33%	> 50%	closure	> 100 workers	> 250 workers
Observations	1,889,691	690,751	213,059	1,060,321	650,030

Notes: This table shows the effect of job loss due to a mass layoff on emergency admissions to public hospitals. The sample is restricted to (1) mass layoffs of at least 33% of the workforce, (2) 50%, (3) plant closures, (4) at least 100 workers, and (5) at least 250 workers. HI and Hospitalization estimates were computed using the difference-in-differences equation (2) and mortality estimates using the matching-based equation adapted from equation (2). Dependent variables are indicated at the top of each column. The explanatory variable of interest is a dummy $Treat_i$ equal to 1 for treated workers, interacted with a dummy $Post_t$ equal to 1 for the period after displacement. 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. All coefficients, standard errors, and baseline means representing probabilities have been scaled by 100, and effects are thus interpreted in terms of percentage points. ***, ** and * represent $p < 0.01$, $p < 0.05$ and $p < 0.1$ respectively.

B.4 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.[28] A general consensus in

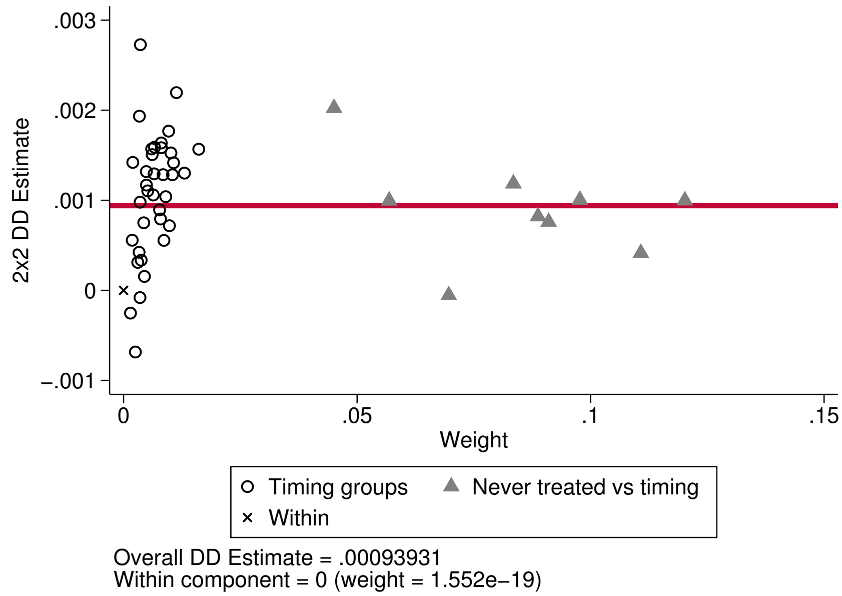
[28] See e.g., De Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2021), Goodman-Bacon (2021), Imai and Kim (2021), Sun and Abraham (2021), and Athey and Imbens (2022).

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 C3 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.[29] 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 d'Haultfoeuille (2020), which corrects the selection of comparison units across treatment and control groups. Results, shown in Panel (b) of Figure C3, also very similar to those from our baseline approach.

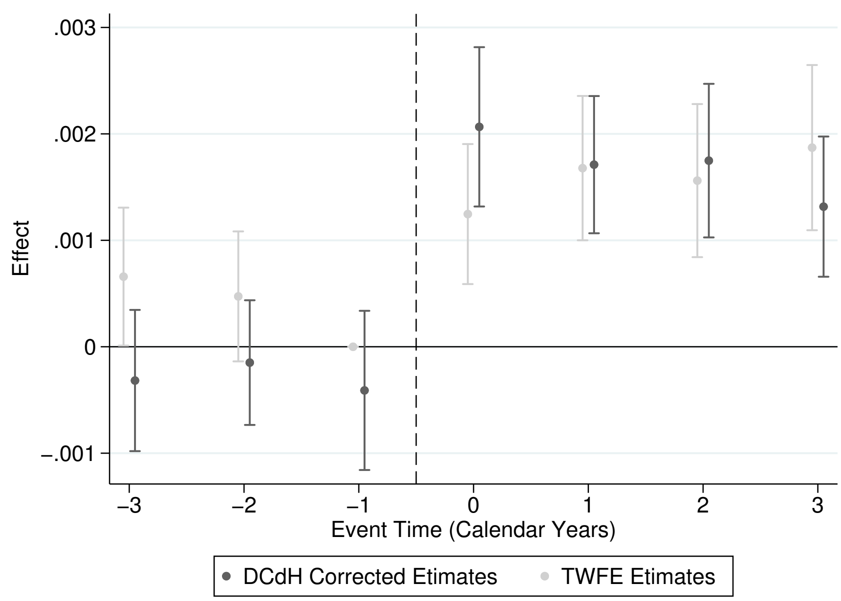
[29] In order to have a strongly balanced sample as required by the estimator, in this exercise we included only a subset of our full sample (treated and control units between 2012 and 2014). Estimated effects with this subsample using our main specification are very similar to those in the main analysis.

Figure C3: Effect of Job Loss on the Probability of Hospitalization, Alternative Estimators

(a) ATE Weight Decomposition



(b) TWFE Panel Estimates



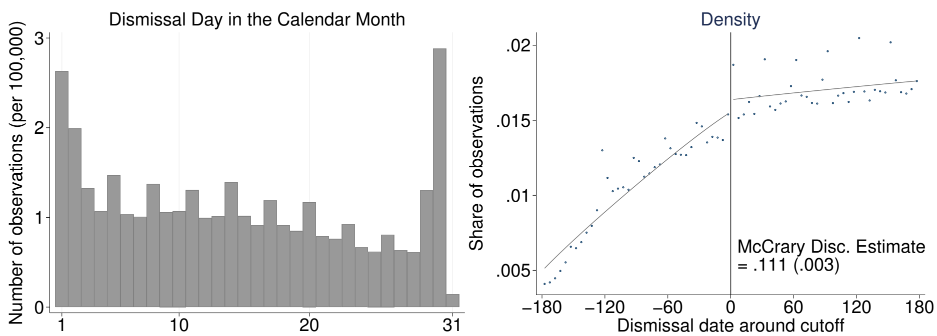
Notes: The figure in Panel (a) shows the weight decomposition of the average treatment effect (ATE) formed from each pairwise comparison between treatment units in the main ample, proposed in Goodman-Bacon (2021). The horizontal red line mark the estimated value from the original specification. The graph 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.

C Appendix to Section 5

C.1 Additional Robustness Tests

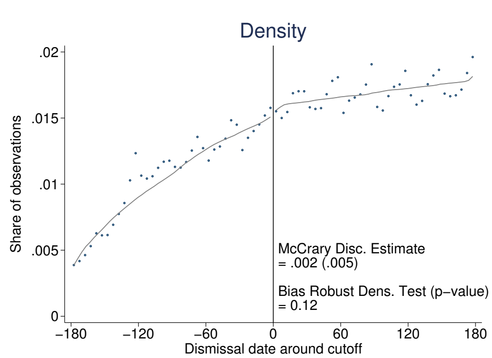
This section includes additional figures and tables that test the validity of the empirical design and the robustness of the main estimates. Figure D1 illustrates the problem of cyclicality with dismissal dates within calendar months, which causes a lack of smoothness around the cutoff in the untreated 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: Dismissal Dates Monthly Cycles



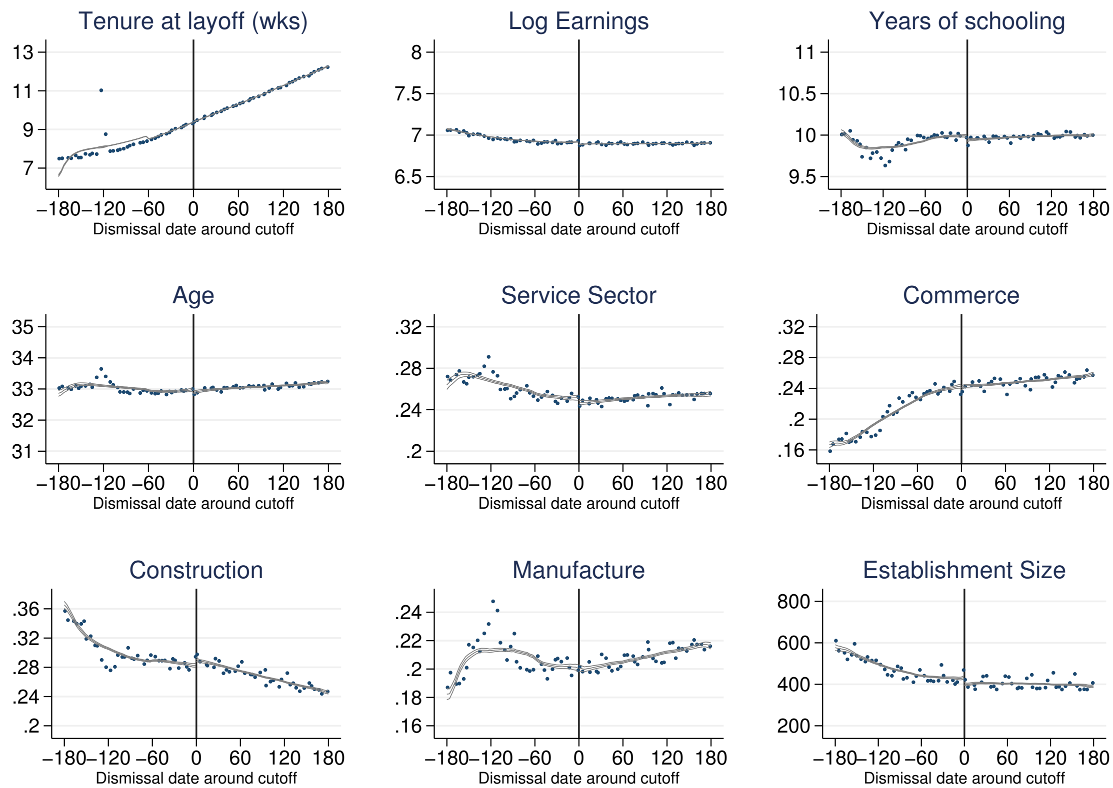
Notes: The left graph presents the distribution of dismissal dates by calendar day within each month. The right graph presents the running variable density function around the cutoff, based on an initial sample that includes all dismissal dates.

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.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: All Ages							
Point Estimate	-0.0145	-0.0217	-0.0162	-0.0066	-0.0248	-0.0093	-0.0073	-0.0247
	(0.0154)	(0.0269)	(0.0184)	(0.0150)	(0.0220)	(0.0171)	(0.0158)	(0.0240)
Bandwidths (Days)	CCT	30	60	90	CCT	150	180	CCT
Polynomial Order	0	1	1	1	1	2	2	2
Observations	2,212,153	2,212,153	2,212,153	2,212,153	2,212,153	2,212,153	2,212,153	2,212,153
	Panel B: Older Workers ($\geq$ 32 Years Old)							
Point Estimate	-0.0585**	-0.0694**	-0.0683***	-0.0525***	-0.0726***	-0.0645***	-0.0531**	-0.0798**
	(0.0234)	(0.0345)	(0.0238)	(0.0195)	(0.0280)	(0.0223)	(0.0206)	(0.0319)
Bandwidths (Days)	CCT	30	60	90	CCT	150	180	CCT
Polynomial Order	0	1	1	1	1	2	2	2
Observations	1,064,201	1,064,201	1,064,201	1,064,201	1,064,201	1,064,201	1,064,201	1,064,201
	Panel C: Younger Workers (< 32 Years Old)							
Point Estimate	0.0286	0.0204	0.0305	0.0349	0.0288	0.0403	0.0343	0.0241
	(0.0180)	(0.0406)	(0.0275)	(0.0224)	(0.0293)	(0.0256)	(0.0237)	(0.0348)
Bandwidths (Days)	CCT	30	60	90	CCT	150	180	CCT
Polynomial Order	0	1	1	1	1	2	2	2
Observations	1,147,952	1,147,952	1,147,952	1,147,952	1,147,952	1,147,952	1,147,952	1,147,952

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).

Figure D4: Effect of UI Take-Up on Public Hospitalization (External Causes), Permutation Test

Notes: Notes: The graphs compare t-statistics for the discontinuity estimates of the effect of UI take-up on hospital admissions at the true cutoff for UI eligibility (vertical black line) with the distribution of t-statistics obtained at all possible placebo cutoffs within 180 days away from the actual threshold. The dashed lines represent the 2.5, 5, 95 and 97.5 percentiles in the distribution of placebo cutoffs. Estimates are based on a local linear polynomial smoothing with a 60-day bandwidth, as in equation (4).