Job Displacement Insurance and Consumption: Evidence from Brazil

François Gerard
Columbia University, CEPR & NBER

Joana Naritomi
London School of Economics & CEPR

January 11, 2019

***The latest version of the paper can be found here

Abstract

The most common forms of government-mandated job displacement insurance are Severance Pay (SP; lump-sum payments at layoff) and Unemployment Insurance (UI; periodic payments contingent on non-employment). While there is a vast literature on UI, SP programs have received much less attention, even though they are prevalent across countries and predominant in developing countries. In particular, little is known about the insurance value that they provide to displaced workers, which critically relies on workers' ability to optimally dissave their lump-sum amount after layoff. We follow a standard approach in the UI literature that evaluates workers' need for insurance and the insurance value of policies by studying workers' consumption profile after layoff. We exploit a rare combination of de-identified high-frequency expenditure data and matched employee-employer data in the state of São Paulo, Brazil, where displaced workers are eligible for both UI and SP. Our main finding is that workers *increase* consumption at layoff by 35% *despite* experiencing a long-term consumption loss of 17% when they stop receiving any benefits. We refer to "consumption" because these patterns are robust across expenditure categories, and are not driven by durable goods. Moreover, the excess sensitivity of consumption to cash-on-hand is present across a rich set of policy variation in UI benefits and SP amounts. We show that a simple dynamic model of job search and consumption with myopic, present-bias, or saving-constrained agents is consistent with both reemployment and consumption patterns in our data. We then use these models to illustrate the usual incentive-insurance trade-off under counterfactual job displacement insurance designs. Our findings highlight the importance of the difference between SP and UI in their *disbursement* policy when consumption is highly sensitive to the timing of benefit payment – beyond their different *contingency* policy – and shed new light on the need for job displacement insurance in a developing country context.

We would like to thank Oriana Bandiera, Lorenzo Casaburi, Raj Chetty, Stefano DellaVigna, Camille Landais, Attila Lindner, Matthew Notowidigdo, Johannes Schmieder, Jesse Shapiro, Johannes Spinnewijn, and participants at seminars and conferences. We would also like to thank the International Growth Centre, the National Science Foundation, and STICERD for their financial support. Rodrigo Candido, Cristiano Carvalho, Daniel Deibler, Dario Fonseca, Samira Noronha, Luiz Superti and a wonderful team of Columbia undergraduates provided outstanding research assistance. We are grateful to PRICED LTDA. This work uses de-identified data, and does not necessarily reflect the views of PRICED LTDA. François Gerard, Columbia University, e-mail: fgerard@columbia.edu. Joana Naritomi, London School of Economics, e-mail: J.Naritomi@lse.ac.uk

Governments around the world provide various forms of job displacement insurance to financially support laid-off workers. Figure 1 shows that the most common government-mandated policies are Severance Pay (SP) – lump-sum payments at layoff – and Unemployment Insurance (UI) – periodic payments contingent on non-employment (based on data from 132 countries).[1] Both programs are quite prevalent and often coexist, but while UI programs are mostly found in richer countries, SP programs exist across levels of development and are thus relatively more common in developing countries (Holzmann *et al.* , 2012).

The economic argument for the existence or co-existence of these two types of programs in the design of job displacement insurance dates back to Baily (1978), and mostly focuses on the *contingency* difference between the schemes. The usual incentive-insurance trade-off is between the welfare loss from distorting incentives to find a new job (*incentive effect*) and the welfare gain from insuring workers against the risk of displacement and the risk of remaining non-employed (*insurance value*). UI is arguably better at targeting as it can provide insurance against both risks; however, it may reduce job-search incentives through both a substitution effect and an income effect. SP can be better for incentives as it only affects job-search through an income effect, but it can only insure against the risk of displacement as it does not distinguish between workers who are immediately re-employed and those who remain non-employed for a longer period.

While there is a vast literature on UI schemes, SP programs have received less attention. There is evidence that SP programs delay reemployment to a lesser extent than UI programs (Card *et al.* , 2007), but we know little about their insurance value. Moreover, besides their different contingency policy, SP and UI also differ regarding when benefits are paid to workers. In particular, the insurance value of SP critically relies on workers' ability to optimally dissave their lump-sum amount after layoff. Despite a growing body of evidence of excess sensitivity to cash-on-hand from other policy contexts (e.g., Shapiro 2005, Olafsson & Pagel 2016, Kueng 2018), the role of *disbursement* policies in affecting the insurance value of job displacement insurance schemes has been largely overlooked. At the same time, it is not clear that we should observe the same excess sensitivity of consumption to income at layoff as the lump-sum is triggered by a negative shock.

This paper investigates the insurance value of UI and SP following a common approach in the UI literature that evaluates the need for insurance and the insurance value of various policies by studying workers' ability to smooth consumption after layoff (e.g., Gruber, 1997; Hendren, 2017; Kolsrud *et al.* , 2018; Ganong & Noel, 2018). We take advantage of a rare combination of de-identified high-frequency expenditure data and matched employee-employer data for more than 400,000 workers in the state of São Paulo, Brazil. This dataset allows us to track a wide range of expenditure categories before and after layoff in a context where displaced workers are eligible for both UI and SP, with variation in benefits across workers that we exploit.

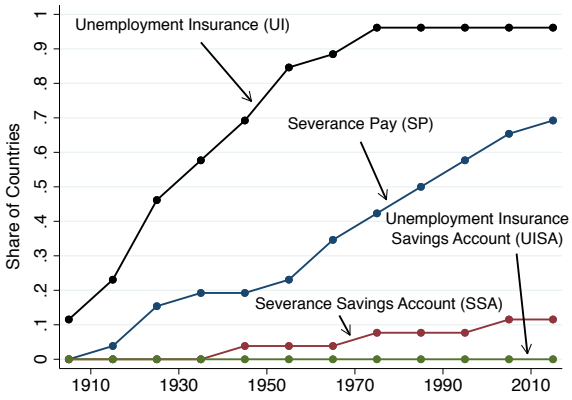
To the best of our knowledge, we provide the first evidence on the insurance value of a SP scheme. In addition, our analysis provides evidence on the need for job displacement insurance in a developing country context, where workers are often employed informally. Job displacement insurance can only cover *formal* workers (i.e., who are reported to the government) and payout schemes can only be made contingent on non-*formal*-employment. We know little about the need for and the insurance value of any job displacement insurance program in such contexts. Traditional means of self-insurance (e.g., formal credit) may be more limited (Chetty & Looney, 2007), but the need for insurance may be smaller if formal employment is not the usual state of the world or if informal jobs are easy to find and close substitutes for formal jobs.

Our main finding is that workers *increase* their consumption upon layoff by about 35% *despite* expe-

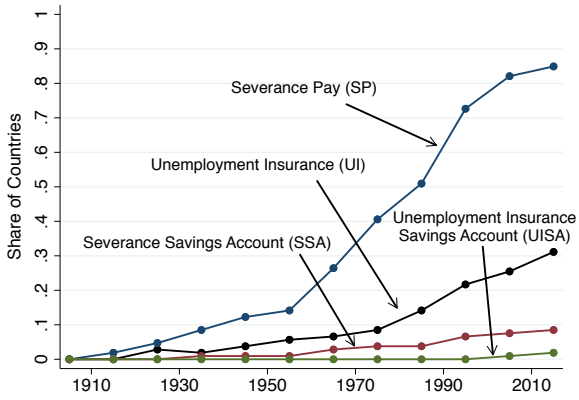
[1] This paper will focus on the *benefit payout schemes* – lump-sum vs. state-contingent. Job displacement insurance programs also differ in their *financing schemes* – insurance-based vs. savings-based (Parsons, 2016). According to this typology, SP and UI programs are typically insurance-based. *Severance Savings Accounts* (SSA) – forced savings accounts that workers can withdraw from at separation – are common in Latin American countries. *Unemployment Insurance Savings accounts* (UISA) – forced savings accounts from which workers can withdraw a fixed amount periodically, contingent on non-employment – are often discussed but remain rare in practice.

FIGURE 1: JOB DISPLACEMENT INSURANCE (GOVERNMENT-MANDATED) AROUND THE WORLD

(a) Western Europe, USA, CAN, AUS, NZ (26 countries)



(b) Africa, Asia, Rest of Americas (106 countries)



The figure displays the share of countries with government-mandated job displacement insurance programs by decade. We collected data for 132 countries in two regions of the world: Western Europe, USA, Canada, Australia, and New Zealand (panel a), and Africa, Asia and the rest of the Americas (panel b). The programs are categorized based on their *benefit payout schemes* – lump-sum vs. state-contingent – and *financing schemes* – insurance-based vs. savings-based (Parsons, 2016, see footnote 1), such that we display separate graphs for Unemployment Insurance (UI; state-contingent, insurance), Severance Pay (SP; lump-sum, insurance), Unemployment Insurance Savings Account (UISA; state-contingent, forced savings), Severance Savings Account (SSA; lump-sum, forced savings).

riencing a long-term consumption loss of about 17% when they stop receiving any benefits. We refer to *consumption* because all these patterns are robust across expenditure categories; in particular, they are not driven by durable goods. The long-term loss is comparable to other estimates from the UI literature, indicating a need for job displacement insurance in our setting, despite the higher labor market informality. In contrast, the increase in consumption at layoff triggered by the presence of SP is unique to this paper:[2] it is larger for workers eligible for higher SP amounts, it is robust across worker groups and realized non-formal-employment durations, but it is not found for workers ineligible for any benefits (they experience a sharp and persistent drop in consumption at layoff). Moreover, we document other instances of excess sensitivity to cash-on-hand. Using administrative data on UI payments, we find that workers spend 20% more in the week that they receive their monthly UI paycheck, and workers who are eligible for one additional month of UI consume significantly more only in the period during in which they draw the additional UI payment. Finally, workers fail to smooth consumption in anticipation of the (expected) drop in income at UI exhaustion, which is associated with a 10% drop in consumption, a result that helps rule out liquidity constraints as the explanation for the excess sensitivity of consumption to income in our data (Ganong & Noel 2018).

These results highlight the importance of the difference in *disbursement* policy between SP and UI, beyond their different *contingency* policy. The insurance value of SP may be lower than that of UI, not only because SP programs do not offer insurance against the risk of remaining non-(formally-)employed, but also because workers' consumption is over-sensitive to the timing of transfers. The periodic nature of UI payments could have an important role in helping displaced workers smooth consumption in that case. To make this point more concretely, we show that simple models with excessive impatience or savings constraints can rationalize both the consumption patterns and the reemployment patterns in our data (the latter are similar to those in Gerard & Gonzaga, 2016). We then use the models to simulate alternative policy designs and illustrate the role of disbursement policies for the incentive-insurance trade-off.

[2] Ganong & Noel (2018) document an increase in spending for a set of workers who happen to receive the first UI check on the same month as their last paycheck, which is consistent with our finding.

We begin by setting the stage for our empirical analysis. First, we lay out a standard dynamic model of job-search and consumption (Chetty, 2008) in order to highlight the incentive-insurance trade-offs with job displacement insurance programs and to motivate the approach that we follow in the empirical analysis.

Second, we describe our empirical setting. The state of São Paulo is the largest of the 27 Brazilian states in terms of population (42 million inhabitants) and economic activity (34% of Brazilian GDP). In Brazil, displaced formal workers are eligible for up to 5 months of UI after a 30-day waiting period. Employers are also required to pay a SP and workers are allowed to withdraw from a forced Severance Savings Account (SSA), which becomes liquid at layoff. As we focus on the *benefit payout schemes* – i.e., lump-sum vs. state-contingent – of job displacement insurance programs, we refer to both of these lump-sum programs under the term "SP" in the paper. For most displaced workers, the total SP amount exceeds the total UI benefit amount. Finally, workers laid off for cause are not eligible for any of these benefits.

Third, we present our data. In 2007, the state of São Paulo introduced monetary incentives for consumers to ask for receipts (*Nota Fiscal Paulista* program) in order to reduce the misreporting of sales subject to the state Value Added Tax (VAT). Consumers are incentivized to give their official ID number (*CPF*) to sellers at the time of purchase. Sellers are required to report details for all transactions to the state tax authority, including the consumer's ID number when provided. The tax authority then displays all the transactions for a given consumer in an individual online account that the consumer can open on the tax authority's website. This account allows the consumer to report any misreporting by sellers and to collect monetary rewards (Naritomi 2018). The take-up of this program was substantial: by 2015, more than 18 million individuals had signed up to an online account. Although the program did not aim at recording individual expenditures, its implementation linked receipts to individual ID numbers, creating a high frequency record of participants' reported expenditures. We obtained access to these expenditure data for about 400,000 de-identified users of a smartphone application that offer services to its users based on their account data, and we were able to match them with the Brazilian matched employee-employer dataset (*RAIS*) for the period 2010-2014, as well as with administrative UI records until 2012.[3] RAIS includes information on wages, hiring and separation months, and reason for separation, which allows us to calculate workers' statutory eligibility for SP and UI programs in all years. We show that displaced formal workers for which we have expenditure data are relatively comparable to the universe of displaced formal workers in São Paulo (based on observables). We also show that our expenditure data provide partial but meaningful coverage for purchases subject to the VAT (it excludes services and housing). Importantly, the data have the advantage of providing detailed sectoral information on the seller allowing us to categorize expenditures, of recording the exact date and amount for each transaction, and of including transactions irrespective of the amount or mode of payment.

Next, we turn to the empirics. In a first step, we present the results of a difference-in-differences analysis in which we estimate changes in expenditures for displaced formal workers in the 12 months before and after layoff, compared to a control group of workers employed over the whole 25-month period. Trends are similar prior to layoff, but expenditure levels increase sharply after layoff for displaced workers, implying an estimate of +35%. Expenditure levels decrease in subsequent months, implying a long-run estimate of -17% for workers who remain non-formally-employed 12 months after layoff. Our estimates are not driven by outliers – they are larger in absolute values at the median – or by composition effects – the spike at layoff is similar whether workers are reemployed immediately or remain non-formally-employed 12 months after layoff (about 30% of the sample; formal reemployment rates are low in Brazil). The effects are larger in relative terms for durables, but durables only constitute about 10% of total expenditures, and the effects are

[3]De-identified versions of the data were analyzed in a secure data room; no data was shared with the developers of the application.

large for non-durables as well, including for most sub-categories (e.g., food). The robustness of our results across various cuts of the data also suggest that they are not driven by reporting effects. We thus interpret our estimates for non-durable expenditures as capturing consumption responses. Additionally, consumption increases discontinuously upon reemployment but workers experience a long-run consumption loss even when reemployed, a typical finding in the literature (e.g., Stephens, 2001).

We link our estimates to variation in job displacement insurance benefits. We show that workers who are not eligible for any benefit (those laid off for cause) experience a sharp drop in consumption at layoff, which persists in subsequent months. In addition, workers eligible for higher SP amounts (controlling for UI; the variation comes mostly from tenure) have higher consumption levels in the first months after layoff; while workers eligible for higher UI replacement rates (controlling for SP; the variation comes mostly from wages) experience a smaller decrease in consumption in subsequent months while still eligible for UI.

The decrease in consumption accelerates in months 6 to 8 after layoff when most workers exhaust their UI benefits (most workers start drawing UI in months 1 to 3 after layoff), particularly for those with high replacement rates. This suggest that workers do not adjust consumption in anticipation of a foreseeable drop in income at UI exhaustion. In a second step of our analysis, we study changes in expenditures before and after UI payments more precisely using the UI payment dates. We document a systematic pattern within the UI spell: beneficiaries increase non-durable expenditures in the week following each UI payment by about 20%.[4] More importantly, non-durable expenditures are flat in the months before UI exhaustion, but start decreasing after the last UI payment, reaching a drop of 7% (unconditionally) to 12% (conditional on remaining non-formally-employed). As for the previous monthly-level analysis, we interpret this pattern as capturing a consumption response. We also present complementary findings from a RD analysis, which compares workers eligible for four vs. five monthly UI payments. The difference in consumption is only significant around the time workers eligible for an extra month of UI are more likely to draw UI.

The lack of anticipation before an expected drop in income is a useful result to shed light on the underlying models of behavior because it cannot be explained by liquidity constraints. It could be explained by models with biased beliefs or inattention regarding future income changes, but such models would not predict a sharp increase in consumption at layoff. In contrast, models of excess sensitivity to cash-on-hand, for instance from excessive impatience or savings constraints,[5] could rationalize both results. We make this point formally by introducing these features into the standard dynamic model laid out at the beginning of the paper and by showing that, in so doing, we can fit the consumption and reemployment patterns in our data. Surveys that we conducted with UI applicants in São Paulo support the relevance of these mechanisms in our context: when asked, more than 50% of respondents say that they would not want to receive all their UI benefits in a lump-sum fashion at layoff despite the clear financial advantage, and most of them mention the need "to control expenditures" or "to not spend it all at once" to explain their choice.

Finally, we use the calibrated models to illustrate the incentive-insurance trade-offs under counterfactual job displacement insurance designs and highlight the key role played by the disbursement policy. In particular, the insurance value of SP can be severely reduced when consumption is very sensitive to the timing of payments, which can undermine the advantage of SP in terms of incentive effects. Relatedly, our findings imply that the welfare gains from providing liquidity to workers at layoff, a policy also advocated on the

[4] We recognize that this specific result may not be a consumption response (most goods become durable at a high-enough frequency), but Shapiro (2005) shows that similar expenditure patterns for SNAP beneficiaries in the US correspond to consumption responses.

[5] One potential source of savings constraint is expenditure pressures from individuals' kin network (i.e., kinship taxation; Squires, 2018). However, surveys conducted with UI applicants in São Paulo suggest that this mechanism may not be first-order in our context. One potential explanation is that the transfer is triggered by a salient negative shock in our case (i.e., job loss), which may mitigate pressures from the kinship network.

ground that it mitigates incentive effects, may be limited unless it is provided in periodic installments.

This paper contributes to a large literature on job displacement insurance. There has been extensive work on UI programs in the context of developed countries, particularly since Baily (1978) was revisited by Chetty (2006, 2008). In contrast, the research on SP programs remains more limited, despite their prevalence and the fact that Baily (1978) discussed both programs.[6] This paper also contributes to the growing literature at the intersection of public economics and behavioral economics by shedding light on the importance of disbursement policies in aiding workers smooth consumption after layoff. In particular, the evidence bridges the literature on UI (e.g., DellaVigna *et al.* , 2017)) and household finance (e.g., Olafsson & Pagel, 2016)), and complements the recent work by Ganong & Noel (2018) on UI and consumer spending in the US using high frequency expenditure data to document a sharp drop in expenditures at UI exhaustion.

The paper also contributes to a growing but still relatively small literature on social insurance in developing countries. Even though São Paulo is a relatively rich and urban state, its informal sector remains important, accounting for about 35% of private-sector employment. Although developing countries spend much smaller shares of GDP on social insurance programs relative to developed countries (Chetty & Finkelstein 2013), the relevance of such programs increases over the development path: for instance, Figure 1 shows that the prevalence of job displacement insurance programs increased dramatically over the 20th century. Moreover, the higher prevalence of SP relative to UI in developing countries echo a common theme in the tax and development literature that lower state capacity may influence the choice of policy instruments (Gordon & Li, 2009). SP requires less administrative capacity as it shifts to firms the mandate to provide payments upon layoff, and does not require a government agency to monitor workers' reemployment. Our results show that these benefits may come at a cost as excess sensitivity to cash-on-hand may hinder the insurance value of SP programs. Considering these tradeoffs carefully is important as our results also show that the need for job displacement insurance can be sizable even in a context of high informality.

The paper is organized as follows. Section 1 lays out a standard conceptual framework that motivates the analysis. Section 2 presents our empirical setting and our data. Section 3 estimates changes in formal workers' expenditure profiles around displacement events, and Section 4 around UI payments and UI exhaustion. Section 5 discusses our results and shows that simple models with excessive impatience or savings constraints are able to rationalize our empirical findings. We also use the models to illustrate the implications of our findings for the design of job displacement insurance programs. Section 6 concludes.

1 Conceptual framework

We begin by laying out a simple dynamic model of job-search and consumption, as in Chetty (2006, 2008), to highlight the incentive-insurance trade-offs with job displacement insurance policies and extend the typical sufficient statistics formulas in the UI literature to the case of SP programs. The main purpose is to motivate the empirical analysis in the rest of the paper; a reader familiar with the literature can probably skip this section. The model will also later serve as a benchmark for our structural estimations and policy simulations. Below, we follow closely the notation and presentation in Kolsrud *et al.* (2018) and DellaVigna *et al.* (2017). We discuss at the end the case of a developing country context with labor market informality.

This paper focuses on the difference between SP and UI programs in terms of their benefit payout schemes: the SP amount is paid lump-sum at layoff, while UI benefits are paid periodically to displaced

[6]There is an extensive literature on SP programs in labor economics, in which they are studied mostly from the firms' perspective as creating firing costs. However, it is easy to show that there is nothing particular to the benefit payout scheme of SP programs in that respect from a conceptual point of view: perfectly-experienced UI programs would create similar firing costs.

workers who remain non-employed. Yet, their financing schemes may also differ. UI programs are typically funded out of a payroll tax and SP programs out of a firm's cash-flow. These financing schemes differ statutorily, but it is easy to show that they can be equivalent in practice, justifying our focus on the disbursement schemes. This is the case if the UI program is perfectly experienced-rated (i.e., the payroll tax rate is firm-specific and a function of a firm's layoff rate and of the UI utilization rate of its displaced workers), the economic incidence falls on workers, and firms are risk-neutral. The last two conditions are standard assumptions in the literature. The first condition is optimal for policy design (Blanchard & Tirole, 2006), and is often assumed implicitly in the literature (it justifies abstracting from the endogeneity of layoff decisions). The model below only deviates from Chetty (2008) to make this equivalence. The per-period tax funding a given expected UI outlay will then be the same as the per-period pay cut on workers' take-home pay that the firm will impose to fund an expected SP outlay of the same amount. The insurance embodied in such programs is that, for a given expected layoff rate, workers that are laid off late transfer resources towards those that are laid off early.[7]

1.1 Setup

We consider a partial equilibrium framework with a continuum of workers of mass 1. The model is in discrete time, starts in period $t = 0$ and ends in period $t = T$. Each worker i is initially employed at a firm that pays a wage w^e per period. With probability $1 - p$, the worker remains employed until period T. With probability p, she becomes non-employed in period 0 and remains non-employed until she finds a new job. The parameter p captures the expected layoff probability at the firm. Once reemployed, a displaced worker remains employed until period T, earning w^r. Before the start of the model, the government commits to a job displacement insurance policy. The policy specifies a UI program with a benefit level b paid in periods $t = 0, ..., P-1$ (where P is the potential UI duration) to displaced workers who remain non-employed, and a per-period tax τ^b levied on workers still employed at the firm. Assuming that the incidence falls on workers, the per-period take-home pay of these workers is reduced by τ^b. The policy also specifies a SP program mandating the firm to pay the amount f to each worker at layoff. Assuming that the firm is risk-neutral and that the incidence falls on workers, the per-period take-home pay of workers still employed at the firm is reduced by $\tau^f = \frac{p}{1-p} \cdot f$ in order to fund the expected SP outlay. With this setup, the two programs are actuarially fair contracts that the worker must enter into before the layoff uncertainty is resolved and that pay some compensation in case of layoff in exchange of a per-period premium when employed.

Job search. Each displaced worker i decides in each period t how much search effort $h_{i,t}$ to exert as long as she is non-employed. The effort level also determines her probability of reemployment in period $t + 1$, with $h_{i,t} \in [0, 1]$. The utility cost of effort $\psi_{i,t}(h_{i,t})$ is assumed to be separable, increasing, and strictly convex. We allow it to depend on the type of worker i, capturing heterogeneity in employability across workers, and on the time spent non-employed, capturing differences in employment prospects over a non-employment spell. The worker's probability to be non-employed in period t equals the survival probability $S_{i,t} \equiv \Pi_{t'=0}^{t-1}(1 - h_{i,t'})$, with $S_{i,0} = 1$. While we cannot observe a worker's specific survival probability, we can observe the population average $S_t \equiv \int S_{i,t}\, di$.

Inter-temporal consumption. Each worker i decides in each period t how much to borrow or save (at interest rate r) given her employment status. We consider time-separable preferences (with discount factor

[7] Real-world UI programs are rarely experienced-rated so that they also transfer resources from workers in firms with a low expected layoff rate to those with a high expected layoff rate. However, these transfers do not provide any insurance (they are redistributive) and they distort firms' layoff decisions (e.g., Carvalho *et al.* , 2018). The case for disregarding these transfers is particularly strong in a developing country context as UI recipients are not in the poorest segments of the population (they work in the formal sector).

δ) and assume that worker i's per-period utility from consumption $\nu_i(c)$ is increasing and strictly concave. A worker starts with some asset level $a_{i,0}$ in period 0 and borrowing constraints prevent her from running down her assets below $\overline{a}_i$ at any time. The worker's savings decisions determine her consumption level when remaining employed at the firm $c^e_{i,t}$, when remaining non-employed $c^n_{i,t}$, and when reemployed $c^r_{i,t}$, respectively. While we cannot observe a worker's contingent consumption plan, we can observe average levels of consumption, for example at different lengths of non-employment, $\overline{c}^n_t = \int \left(S_{i,t} \cdot c^n_{i,t} \right) \, di$.

Agents' problem. For a given government policy, each worker chooses how much to search and how much to consume in order to maximize her expected utility subject to the budget and borrowing constraints. Her choices depend on her assets and on the time spent non-employed in addition to the government policy. In particular, the value function $U_{i,t}$ for a worker who is non-employed in period t can be written as follows:

$$U_{i,t}(a_{i,t}) = \max_{h_{i,t}, a_{i,t+1}} \nu_i(c_{i,t}) - \psi_{i,t}(h_{i,t}) + \delta \cdot \left[h_{i,t} \cdot V_{i,t+1}(a_{i,t+1}) + (1 - h_{i,t}) \cdot U_{i,t+1}(a_{i,t+1}) \right] \tag{1}$$

where $a_{i,t}$ is the asset level at the start of period t and $V_{i,t+1}$ is the value function of being reemployed in the next period. The value function $V_{i,t}$ for a worker who is employed in period t is simply:

$$V_{i,t}(a_{i,t}) = \max_{a_{i,t+1}} \nu_i(c_{i,t}) + \delta \cdot V_{i,t+1}(a_{i,t+1}) \tag{2}$$

The consumption path must also satisfy the borrowing constraint $a_{i,t} > \overline{a}_i$, as well as the following inter-temporal budget constraints and budget balance conditions at the end of life:

$$\begin{aligned}
c^e_{i,t} &= a_{i,t} + (w^e - \tau^b - \tau^f) - \frac{a_{i,t+1}}{(1+r)} \text{ for } t = 0...T-1 \qquad & c^e_{i,t} &= a_{i,t} + w^e - \tau^b - \tau^f \text{ for } t = T \\
c^n_{i,t} &= a_{i,t} + (f_t + b_t) - \frac{a_{i,t+1}}{(1+r)} \text{ for } t = 0...T-1 \qquad & c^n_{i,t} &= a_{i,t} \text{ for } t = T \\
c^r_{i,t} &= a_{i,t} + (w^r) - \frac{a_{i,t+1}}{(1+r)} \text{ for } t = 1...T-1 \qquad & c^r_{i,t} &= a_{i,t} + w^r \text{ for } t = T
\end{aligned}$$

where $f_t = f \cdot \mathbb{1}\{t = 0\}$ and $b_t = b \cdot \mathbb{1}\{t < P\}$.

Budget and welfare. The UI budget depends on the expected UI benefits paid to displaced workers, and on the expected tax revenues levied on workers remaining employed at the firm. Perfect experienced-rating thus implies: $\tau^b = \frac{p}{1-p} \cdot b \cdot B$, where $B = \sum_{t=0}^{P-1} S_t$ is the *average paid UI duration* (as in other papers, we ignore time discounting for now, i.e., $1 + r = \delta = 1$).[8] This establishes the equivalence between the funding schemes of UI and SP programs. It is also irrelevant whether the programs are explicitly managed by the government or implicitly by the firm.[9] In this setup, the government's problem is to maximize welfare:

$$W = \int \left[p \cdot U_{i,0} + (1-p) \cdot V_{i,0} \right] \, di \tag{3}$$

We assume that this social welfare function is differentiable.

[8] Many papers with a similar setup in the UI literature assume that reemployed workers also pay the UI tax. As a result, the UI tax increases if workers remain non-employed longer even if they are not drawing any UI benefit anymore (because workers spend fewer periods post-layoff paying the tax). This effect does not appear with experienced-rating, so we do not make such an assumption.

[9] Firms may be more likely to default on their obligations at layoff. This issue does not appear to be important in our context: in a survey conducted with displaced workers in São Paulo, most respondents report that their firm paid all of what they were entitled to.

1.2 Sufficient statistics formulas

We now derive typical sufficient statistics formulas for the welfare effects of marginal changes in our three policy instruments, separately. In this section, we also make the usual benchmark assumption that agents are fully optimizing, taking all the impacts of their decisions into account, except on the government budget.

Consider first a marginal change in the level of the SP amount, f:

$$\begin{aligned}\frac{dW}{df} &= p \cdot \int \frac{\partial U_{i,0}}{\partial f}\, di + (1-p) \cdot \frac{d\tau^f}{df} \cdot \int \frac{\partial V_{i,0}}{\partial \tau^f}\, di + (1-p) \cdot \frac{d\tau^b}{df} \cdot \int \frac{\partial V_{i,0}}{\partial \tau^b}\, di \\ &= p \cdot \mathbb{E}\left[\frac{\partial \nu_i(c^n_{i,0})}{\partial c^n_{i,0}}\right] - p \cdot \mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right] - p \cdot \frac{d(b \cdot B)}{df} \cdot \mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right]\end{aligned}$$

where the first equality arises from the application of the envelope theorem. The two different expectations are simply the average marginal utility of consumption if non-employed in period 0 (when receiving the SP amount) and if remaining employed at the firm (across all periods paying for the SP and UI programs).

Dividing by $\mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right]$ to obtain a money metrics and by p to normalize by the mechanical effect of the reform, we obtain an expression for the welfare effect per \$1 spent on target beneficiaries ($d\widetilde{W}$):

$$df: \qquad d\widetilde{W} = \frac{\mathbb{E}\left[\frac{\partial \nu_i(c^n_{i,0})}{\partial c^n_{i,0}}\right] - \mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right]}{\mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right]} - \frac{d(b \cdot B)}{df} \tag{4}$$

Consider next a marginal change in the UI benefit level, b. Following the same steps as above, including the same normalization, we obtain the following welfare effect per \$1 spent on target beneficiaries:

$$db: \qquad d\widetilde{W} = \frac{\mathbb{E}^B\left[\frac{\partial \nu_i(c^n_{i,t})}{\partial c^n_{i,t}}\right] - \mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right]}{\mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right]} - \frac{1}{B} \cdot \frac{d(b \cdot B)}{db} \tag{5}$$

where the notation $\mathbb{E}^B$ specifies that the new expectation is taken over all the periods that workers would be observed drawing UI benefits in absence of the reform (the target beneficiaries for an increase db).

Finally, consider a marginal change in the potential UI duration, P, which is equivalent to a marginal change in b_P multiplied by b (Schmieder *et al.* , 2012). Following again the same steps as above, we obtain:

$$dP: \qquad d\widetilde{W} = \frac{\mathbb{E}\left[\frac{\partial \nu_i(c^n_{i,P})}{\partial c^n_{i,P}}\right] - \mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right]}{\mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right]} - \frac{1}{b \cdot S_P} \cdot \frac{d(b \cdot B)}{dP} \tag{6}$$

where the new expectation is taken over workers who remain non-employed at the end of the potential UI duration – i.e., in period P – in absence of the reform (the target beneficiaries for an increase dP).

The first term in equations (4)-(6) captures the potential welfare gain of the reforms, namely their insurance value. In equation (4), it depends on workers' gain from buying insurance against the risk of displacement, namely the consumption smoothing gain of transferring \$1 from the employed to the displaced state. In equations (5) and (6), it depends on workers' gain from buying insurance against the risk of re-

maining non-employed over the potential UI duration and after UI exhaustion, respectively. In all cases, the consumption smoothing gain depends on the marginal utility of consumption in the periods when workers receive additional resources, and not when these resources are actually spent. This is because workers are optimizing such that they are indifferent between increases in consumption or savings in all periods.

The second term captures the potential welfare loss, namely the efficiency cost from the incentive effect of the reforms. As is usual in the literature, it is captured by the fiscal externalities caused by behavioral responses to the reforms, and it takes the form of a ratio between the behavioral effect of the reforms – the increase in costs due to behavioral responses, i.e., $p \cdot \frac{d(b \cdot B)}{dx}$ for $x = \{f; b; P\}$ – and their mechanical effect – the increase in costs absent behavioral responses, i.e., $\{p; p \cdot B; p \cdot b \cdot S_P\}$ for $\{df; db; dP\}$, respectively. In the case of an increase in f, displaced workers may reduce job-search because of an income effect, potentially remaining non-employed longer (Card *et al.* , 2007). The only fiscal externalities are those on the UI budget. In particular, there would be no efficiency cost if the SP program was the only source of job displacement insurance. This is the intuition that lump-sum transfers do not create any distortion by themselves, only if there are pre-existing distortions, such as those induced by a UI program. For increases in b or P, displaced workers may reduce job-search because of an income effect and a substitution effect, as they face lower incentives to find a new job over the potential UI duration or before UI exhaustion, respectively.

Equations (4)-(6) highlight the tradeoffs between SP and UI programs. SP programs provide insurance against the risk of displacement, without discriminating among displaced workers based on their non-employment durations. UI programs are likely better targeted as they provide insurance against the risk of remaining non-employed, but they likely entail a larger efficiency cost. The difference between the two programs will thus depend on the size of the substitution effect and on the evolution of the marginal utility of consumption of non-employed workers over the potential UI duration.[10] Equations (4)-(6) also highlight the rationales for combining the two programs. A sizable substitution effect may limit the generosity of UI, constraining the government to rely on SP to provide additional insurance to displaced workers, even if it is in a coarse form (Baily, 1978). Moreover, a SP program could complement UI if there are negative consequences of displacement that workers experience even when reemployed. Concretely, a SP program could serve as an imperfect form of wage insurance (i.e., against $w^r < w^e$), provided that wage losses are not perfectly correlated with non-employment durations (Parsons, forthcoming).

1.3 Developing country context

An important feature of labor markets in developing countries is the large share of "informal" workers who escape government monitoring, which include informal employees and most self-employed workers. Job displacement insurance can only cover *formal* workers (we use "formal workers" and "formal employees" interchangeably), and the government cannot monitor beneficiaries working informally, so payout schemes can only be made contingent on non-*formal*-employment. Gerard & Gonzaga (2016) show that equations (5) and (6) still apply in that context. One must only redefine "employed" by "formally employed" and "non-employed" by "non-formally-employed." That the prevalence of informal jobs does not directly affect the formulas is another application of the envelope theorem (whether beneficiaries are non-employed or work informally makes no difference to the cost of a UI program). The same reasoning applies to equation (4).

This does not imply, however, that the trade-offs with job displacement insurance policies are unchanged

[10] Absent selection effects, the marginal utility of consumption will likely increase over the potential UI duration, as workers deplete their savings, but it may not increase quickly if workers are able to find other means of consumption smoothing over time. Moreover, there may be selection effects, as workers with different marginal utilities may be reemployed at different rates.

in a developing country context. First, a low administrative capacity may limit some countries to adopt SP programs, which do not require the government to keep track of formal workers or to make periodic payments to specific workers. Other countries may not be able to keep track of formal reemployment in a timely manner, such that they may not be able to exclude formally reemployed workers from drawing UI benefits. If the contingency is not binding, the difference between UI and SP programs is small: a UI program essentially becomes an unconditional transfer paid in installments, and the two programs only differ in terms of their disbursement policy, which is inconsequential in this framework.

Even if a contingent payout scheme can be meaningfully implemented, the relative size of the two sides of the trade-offs in equations (4)-(6) may differ in a developing country context. For instance, the availability of informal job opportunities may affect the efficiency cost of UI programs. On the one hand, displaced workers who would be formally reemployed otherwise can respond to UI incentives not only by remaining non-employed, but also also by working informally. On the other hand, formal reemployment rates may be low even in the absence of any incentive effect, e.g., if it is hard to find a new formal job ("exclusion" view) or if informal jobs are easy to find and close substitutes to formal jobs ("exit" view).[11] Gerard & Gonzaga (2016) show that the efficiency cost of a UI reform akin to the one in equation (6) is smaller in Brazil than in the US, and smaller in more informal labor markets within Brazil, because the second effect dominates: formal reemployment rates are lower in contexts of higher informality absent any incentive effect, which limits the possible size of the incentive effect. In settings with very low formal reemployment rates, UI programs with limited potential durations may in fact be close to unconditional transfers paid in installments.

That the efficiency cost of UI programs is not necessarily higher in a context of high informality is an argument in favor of UI so long as the need for insurance and the gain from discriminating between displaced formal workers based on their non-formal-employment durations is sizable. However, these may also differ in that context. The need for insurance may be large if it is hard to find a new formal job, but it may be small if informal jobs are easy to find and close substitutes for formal jobs (at least the difference between the insurance value of UI and SP programs will be small then). Even in the former case, the need for insurance may be small if formal employment is not the usual state of the world: the "shock" may be the formal hiring rather than the layoff event, and workers may simply save the excess earnings while lucky enough to have a formal job. At the same time, usual means of self-insurance (e.g., formal credit) may be more limited in developing countries, increasing the gains from social insurance (Chetty & Looney, 2007).

1.4 Informing the insurance value

There is a large empirical literature trying to evaluate the welfare effect of UI programs using formulas such as those in equations (5) and (6). A typical approach to inform their insurance value – the side of the tradeoff that is more challenging to assess – is the so-called "consumption approach." The difference in average marginal utilities in equation (5), for instance, can be written such that (Baily, 1978; Gruber, 1997):[12]

$$\frac{\mathbb{E}^B\left[\frac{\partial \nu_i(c^n_{i,t})}{c^n_{i,t}}\right] - \mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right]}{\mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right]} \simeq \gamma \frac{\mathbb{E}\left[c^e_{i,t}\right] - \mathbb{E}^B\left[c^n_{i,t}\right]}{\mathbb{E}\left[c^e_{i,t}\right]} \tag{7}$$

[11] These are the two main (complementary) views on the prevalence of informality in developing countries (Perry *et al.* , 2007).

[12] The decomposition assumes that the third-order derivative of the utility function is small (otherwise, the expression would include another term depending on the coefficient of relative prudence; Baily, 1978; Chetty, 2006), that the utility function is not state-dependent (the expression would include an additional term with state-dependent utility functions; Chetty & Finkelstein, 2013), and that workers share the same utility function (the expressions can be extended to this case and the selection issue that it implies; Kolsrud *et al.* , 2018).

where γ is an average coefficient of relative risk aversion and the ratio in equation (7) is the relative difference in average consumption when employed and when drawing UI benefits, namely the degree of consumption smoothing. A similar decomposition for the insurance value in equations (4) and (6) would simply replace the average consumption when drawing UI benefits by the average consumption in the month of SP receipt (irrespective of employment status) and in the month after UI exhaustion, respectively. Motivated by such a decomposition, a main strand in the literature investigates the size of the consumption drop after layoff to inform the insurance value of various policies. This type of study has not been carried out neither in settings with a SP program nor in a developing country context. Additionally, because existing studies focus on UI, they do not estimate the size of the consumption drop at layoff irrespective of reemployment status or UI takeup, which would inform the insurance value of introducing SP in this framework.[13]

A key advantage of this type of framework and of the expression in equation (7) is that it is "sufficient to obtain consumption-smoothing estimates for a single good (e.g., food), provided that the appropriate risk aversion parameter (e.g., curvature of utility over food) is used in conjunction with this estimate" (Chetty, 2006, p.1896). This is because workers are assumed to fully optimize across all choices, including all types of consumption, such that workers are indifferent between increases in any consumption category. As a result, one can inform the insurance value of job displacement insurance even with partial consumption data.

Similarly, it is not necessary to know *when* displaced workers spend their SP or UI benefits, as workers are assumed to be indifferent between increases in consumption or savings in all periods. This implication rests on assumptions that have been challenged in various contexts, such as the absence of savings constraints (e.g., from kinship taxation; Jakiela & Ozier, 2016), biased beliefs (Spinnewijn, 2015), or excessive impatience (DellaVigna & Paserman, 2005; Ganong & Noel, 2018). In the eventuality that these assumptions are violated, one would need to know when benefits are actually spent (rather than when they are received) to assess the consumption-smoothing gain from job displacement insurance policies, a question for which there is limited evidence in the literature. This lack of evidence is particularly critical for SP programs because their insurance value relies entirely on workers' ability to dis-save the SP amount optimally after layoff. Evidence from other contexts suggest that this should not be taken for granted (Shapiro, 2005).

One of the main reason why we know little about the consumption-smoothing gains of SP programs, or of any type of job displacement insurance in developing countries, is that combining the necessary high-frequency longitudinal data on consumption and (formal) employment is challenging. We address this limitation in the rest of the paper, by exploiting a rare combination of administrative datasets in an setting where we can exploit a rich set of policy variation.

2 Institutional background and data

Before delving into the empirical analysis, we present some relevant institutional background and our data.

2.1 Setting

The setting of our empirical analysis is the state of São Paulo, which is the largest of the 27 Brazilian states in terms of population (42 million inhabitants) and economic activity (34% of national GDP). As do many middle-income and developing countries, Brazil has a high rate of labor market informality (Perry *et al.* , 2007). In Brazil, every worker has a working card, and when an employer signs her working card, which is

[13]The sample in Ganong & Noel (2018) is conditional on UI takeup for instance.

required by law, her hiring is reported to the government, and she becomes a formal employee. Hiring an employee formally is costly, however;[14] firms face severe financial penalties for not complying with labor laws, including hiring workers informally, but the risk of detection is low (Almeida & Carneiro, 2012). As a result, informal workers account for about 50% of the non-farm labor force in Brazil. Even in São Paulo, a relatively rich and urban state, informal workers account for about 35% of private-sector employment.

2.2 Job displacement insurance programs

Displaced formal workers have three potential sources of job displacement insurance in Brazil.

Unemployment Insurance (UI). A worker who is involuntarily laid off from a private-sector formal job and who has at least six months of job tenure at layoff is eligible for UI benefits after a 30-day waiting period.[15] She must apply in person for UI within 120 days of her layoff date. If deemed eligible, her UI benefits are deposited every 30 days at a state bank, Caixa, as long as her name does not appear in a database where employers report new hiring monthly. The potential UI duration depends on her total tenure across all formal jobs in the 36 months prior to layoff. She is eligible for three, four, or five monthly UI payments if she has more than 6, 12, or 24 months of tenure, respectively. The benefit level depends on her average wage in the three months prior to layoff and ranges from 100% to 187% of the minimum wage. The replacement rate is thus particularly high at the bottom of the wage distribution, but it is down to about 60% for workers earning three times the minimum wage (the full schedule is in the Appendix). Finally, a peculiarity of the Brazilian UI program is that it is financed by a tax on firms' total sales (.65%) rather than by a payroll tax.

Severance Savings Account (FGTS). As in many Latin American countries, there is a mandatory SSA program in Brazil, the *Fundo de Garantia do Tempo de Serviço* (FGTS). Every month, employers must deposit 8% of their workers' monthly wage in an account under each worker's name at the same state bank, Caixa. There is a different account for every formal employment spell (i.e., every labor contract). The account is highly illiquid, but formal workers gain access to the full balance if they are involuntarily laid off.[16] The interest rate is notoriously low, barely catching up with inflation. Therefore, displaced workers have strong incentives to withdraw the full balance at layoff. Also, there is some red tape if workers delay withdrawal.[17]

Severance Pay (SP). Workers are entitled to a SP if they are involuntarily laid off. Employers must pay workers a monthly wage as "advance notice" of layoff and a "fine" equal to 40% of the amount deposited in the workers' FGTS account over the formal employment spell (another 10% is paid to the government).

Workers who are fired for cause (or quit) are not eligible for UI, do not gain access to their FGTS balance (the account remains illiquid until retirement), and are not entitled to any SP amount at separation.

Figure 2a displays the average *statutory* job displacement insurance benefits in our sample of analysis (*user sample*), which we describe below, and in a random sample from the universe of displaced formal worker in São Paulo (*random sample*). We restrict attention to laid-off workers and we apply the same selection criteria in the two samples; in particular, we focus on workers with more than 12 months of tenure at

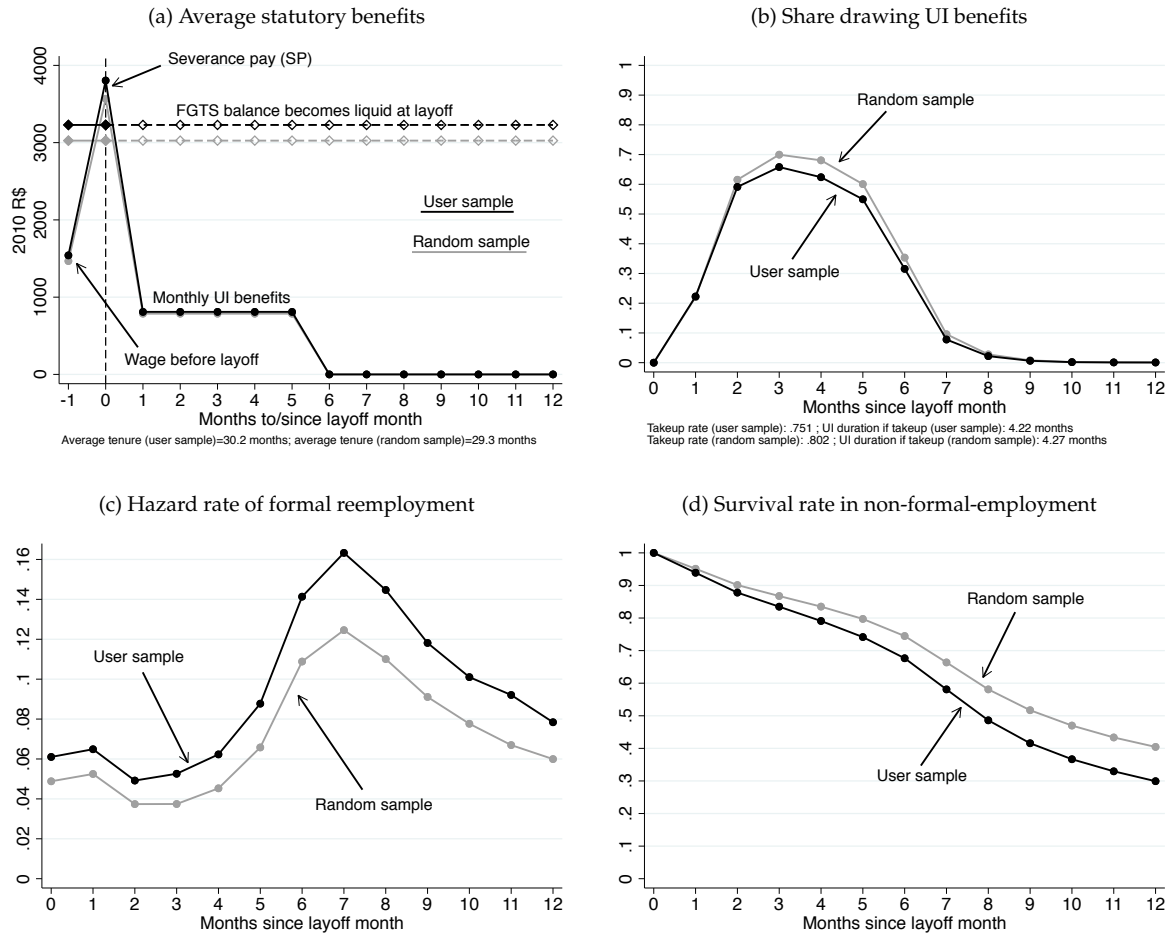
[14] Payroll taxes include 20% for Social Security and 7.8% for funding an array of programs (e.g., training, education). Formal employees are also entitled to the minimum wage, a 13th monthly wage, a 50% overtime rate for hours exceeding 44 hours/week, etc.

[15] There must also be at least 16 months between a worker's layoff date and the layoff date of her last successful application for UI. Note that the rules described in this paragraph were in place until February 2015, and thus for the whole period covered by our data.

[16] The only other events granting access to FGTS accounts are purchases of residential property, serious health shocks, natural disasters, and staying 3 years without a formal job. Any balance in the account is pensionable, and bequested in case of death. In surveys conducted with UI applicants in São Paulo, only 9% of respondents reported having accessed their FGTS balance before layoff. We emphasize that, at layoff, a worker can only access the FGTS balance for the job from which she is laid off, so a worker who quit job A and then was laid off from job B can only access the FGTS balance corresponding to job B (and not the FGTS balance from job A).

[17] Workers receive a code for withdrawal from the employer up to 5 business days after layoff that must be used within 90 days. After that workers are entitled to request a new code, but they would incur additional time and hassle costs.

FIGURE 2: BACKGROUND AND REPRESENTATIVITY OF DISPLACED FORMAL WORKERS IN THE USER SAMPLE



The figure provides some background on displaced formal workers (involuntarily laid off) in the setting of our empirical analysis and compares our *user sample* to a *random sample* from the universe of displaced formal workers in São Paulo. Section 2.4 provides details on the construction of the two samples. Panel (a) displays the average statutory job displacement insurance benefits of displaced formal workers in the two samples. Panels (b)-(d) display the share actually drawing UI benefits, the hazard rate of formal reemployment, and the survival rate in non-formal-employment in each month after layoff in the two samples. The samples in panel (b) are restricted to workers laid off in 2011 such that we observe their full UI spell in the UI data. We show in the Appendix that the patterns presented in panels (a), (c), and (d) are very similar when we impose the same restriction.

layoff (see below). The average FGTS balance, SP amount, and monthly UI benefit are 210%, 247%, and 53% of the average monthly wage at layoff in the user sample.[18] These figures are comparable in the random sample, although monetary values are slightly lower (see the discussion on the representativity of the user sample below).

We depict a potential UI duration of five months because the average tenure at layoff is above 24 months in both samples (see Table 1 below). In sum, Figure 2a shows that the average resources available to workers in a lump-sum fashion at layoff exceed the average UI benefits that they are eligible to draw. There is a lot of heterogeneity across workers, however, a feature that we exploit in the paper.

[18] All monetary values in the paper are in reais (R$) of 2010. The exchange rate was around US$1=R$2 at the time.

2.3 Data

Our empirical analysis relies on three main datasets.

Formal employment (RAIS). RAIS is a matched employee-employer dataset that covers the universe of formal employment spells in Brazil. Every year, all tax-registered firms must report all workers formally employed at any point during the previous year.[19] RAIS includes information on age, race, gender, education, sector of activity, establishment size and location, contracted hours, tenure, hiring and separation months, and reason for separation for every formal employment spell within a year. It also includes the average monthly wage over the spell and the December wage for spells that survive until December 31st.

UI registry. We are able to use data from the UI registry for the whole country from 2009 to 2012. The data include the application date, as well as the date and the benefit amount for all subsequent UI payments. We can merge workers in the formal employment and the UI registry data through a unique ID number.

We do not have data on **FGTS balances** or **workers' receipt of SP** for this project. However, because we are able to use RAIS data from 2005 to 2014, we can calculate the statutory job displacement insurance benefits for all workers displaced in 2010-2014 who had at most 72 months of tenure at layoff (90% of cases). We also conducted short surveys with 178 UI applicants in July and August 2018 to get a sense of firms' compliance with their obligations to displaced workers. Only 2% reported any issue in accessing their FGTS balance, 5% in receiving the 40% fine, and 13% in being given their advance notice. Moreover, conditional on having access to these resources, only 4% reported that they did not (intend to) withdraw the full FGTS balance and almost all made their withdrawal within 60 days after layoff (the median time is 15 days). Finally, the average amount available lump-sum at layoff was about 4.6 times the monthly wage, which is in line with the statutory amounts that we calculate in our sample.[20]

Expenditures. The main novelty of this paper is that we can match the formal employment and the UI registry data with administrative expenditure data from receipts that specify the consumers' ID numbers.

The *Nota Fiscal Paulista* (NFP) program was created by the state of São Paulo in October 2007 to reduce tax evasion of the state-level VAT and to foster a culture of tax compliance. In a nutshell, the program introduced the possibility of identifying the consumer's ID number on each receipt, and created a reward system of tax rebates and monthly lotteries so that final consumers have incentives to provide their ID number when making purchases and request it to be displayed on the receipts.[21] Sellers are required to report details for all transactions to the state tax authority, including the consumer's ID number when provided. The tax authority then displays all the transactions for a given consumer in an individual online account that the consumer can open on the tax authority's website. This account allows the consumer to report any misreporting by sellers and to collect the monetary rewards (see Naritomi, 2018, for more details).

Naritomi (2018) shows that the program has had a large impact on the volume of sales reported by retailers (an increase of about 20%). A key ingredient behind this impact is that consumer participation has been substantial, most likely because of the relatively low participation costs. Consumers do not need to send any receipt by themselves to the tax authority to get the rewards. No pre-registration is needed for consumers to start participating. Opening the online account is only necessary to start collecting the tax rebates – upon opening an account, the consumer can collect all tax rebates retroactively for a period of up to 5 years – and receiving lottery tickets for monthly cash prizes. Additionally, states do not tax personal income in Brazil (it is a federal tax) and their NFP participation cannot be used against consumers for the

[19] Compliance is high because of large penalties when the data are late or incomplete. The main purpose of RAIS is to administer a federal wage supplement. There are thus incentives for truthful reporting. It is also used by ministries to monitor formal job takeup.

[20] We are still cleaning the surveys, so these figures are subject to changes. Data and survey instruments will be available online.

[21] In the Appendix, we display a copy of a VAT receipt from São Paulo, which shows where the ID number is displayed.

enforcement of other tax bases. By 2014, more than 18 million people had opened an online account.

Unintendedly, these individual online accounts create a record of participants' reported expenditures. We obtained access to the data from a sample of de-identified accounts through a private company that created a smartphone application offering services to its users based on their account data, and we were able to match these data with the formal employment and the UI registry data using the individuals' ID number.[22] Our sample includes about 400,000 users who participated in the NFP program and had at least one formal employment spell in São Paulo between 2010 and 2014. The expenditure data include all purchases from 2010 to 2016 for which these users provided their ID number. For each purchase, the data include the date, the total value, the number of items, and the sector of activity of the seller. The latter is a 7-digit code (the same code as in RAIS), which allows us to classify expenditures in specific categories.[23]

By design, the expenditure data only include purchases for which users chose to provide their ID number, but they are not restricted by value or mode of payment. In particular, the data include cash transactions, which is critical in a developing country context. We show below that they provide meaningful coverage for purchases of goods (i.e., the VAT base, excluding services and rent) and that displaced formal workers in our user sample are relatively comparable to the universe of displaced formal workers in São Paulo.

2.4 Descriptive statistics, background, and representativity of the user sample

We present descriptive statistics for our main sample of analysis from the user sample (used in the next section) to provide some background information about our empirical setting. We also evaluate the representativity of our sample by comparing it to a random sample of displaced formal workers in São Paulo.

Our main sample of analysis is constructed as follows. From RAIS, we select all full-time private-sector formal employees in São Paulo who were involuntarily laid off between 2011 and 2013. This window allows use to have expenditure and employment data for at least one year before and after layoff for all workers in the user sample. We further restrict attention to workers who had at least 12 months of tenure at layoff, such that they have some prior attachment to the formal labor force, and at most 72 months of tenure, such that we can calculate their statutory SP amount and FGTS balance at layoff. We obtain a sample of 77,862 layoff events after keeping those events involving workers who appear in the user sample and had some purchases in the expenditure data both before and after the 25-month window centered around layoff (to make sure that they were consistent participants throughout the period). We obtain a sample of 156,021 layoff events after selecting 5% of the overall sample (not restricted to users) at random (*random sample*).

Table 1 displays the mean of a series of variables at or before layoff in the two samples, as well as their difference. All the differences are statistically significant at the 5% level, but they are modest in size. A first point to make is that our analysis takes place in an urban setting:[24] almost all workers were employed in an urban municipality (about half in the metropolitan area of the city of São Paulo) and the shares were only slightly larger in the user sample (97.4% vs. 97.1%). The shares of female (43.6% vs. 39.1%) and white workers (70.3% vs. 69.3%) were also larger in the user sample and the average worker was slightly older (32.82 years vs. 32.55 years). The most notable difference is that education levels were higher in the user sample, e.g., the share of workers with a high school degree was 7.5pp higher (72.8% vs. 65.3%). Accordingly, workers in the user sample were "positively selected," i.e., they had more tenure (30.20 months vs.

[22] We had only access to a de-identified version of the matched data from a secure data room; no data was shared with the developers of the application.

[23] For instance: 47 is Retail; 472 is Retail of food, beverages, tobacco; 4722-9 is Retail of meat and fish; 4722-9/01 is Retail of meat.

[24] In that respect, our setting differs from the setting of recent studies highlighting the usefulness of large lump-sum transfers rather than periodic payments in rural settings (e.g., Casaburi & Macchiavello, forthcoming).

29.22 months) and a higher monthly wage (R$1541 vs. R$1469) at layoff, and thus a lower UI replacement rate (68.3% vs. 69.8%). These differences are again relatively small, but they explain the differences in the average statutory job displacement insurance benefits in the two samples that are displayed in Figure 2a and reported in Table 1 as well. One explanation for the relative comparability of individuals in the user and random samples is that a large share of adults in São Paulo have smartphones (e.g., the share is 92% in our survey of UI applicants).

Figures 2b-2d also show that the two samples are relatively comparable in terms of UI and formal reemployment outcomes. The patterns in these figures also provide useful background information on displaced formal workers in São Paulo (Gerard & Gonzaga, 2016, document the same patterns for the whole country).

Figure 2b displays the share drawing UI benefits in each month after layoff. Because of the 30-day waiting period, the share is nil in the month of layoff. It remains small in the following month (about 20%), but it increases quickly afterward and peaks in month 3 after layoff, showing that displaced workers do not start drawing UI as soon as they become eligible. Overall, the UI takeup rate (i.e., the share drawing any UI benefit) is 75.1% in the user sample (80.2% in the random sample). The share drawing UI benefits remains more or less stable from months 2 to 5 and then decreases quickly in months 6 to 8. The share is essentially nil afterward. Workers in our samples are eligible for 4 to 5 months of UI and the patterns in Figure 2b implies that most UI takers exhaust their potential UI duration in our setting. In fact, the average UI duration reaches 4.23 months among UI takers in the user sample (4.27 months in the random sample).[25]

Most UI takers exhaust their potential UI duration in Brazil because formal reemployment rates are very low while workers are eligible for UI. This is shown in Figure 2c. The hazard rate of formal reemployment is slightly higher in the user sample, explaining the lower UI takeup rate and average UI duration in Figure 2b. However, the hazard rate is low in both samples: in the user sample, it is only around 6% in months 0 and 1, and it drops even lower when more workers start drawing UI benefits. As a result, Figure 2d, which displays survival rates in non-formal-employment, shows that about 75% of displaced workers in the user sample (80% in the random sample) remain without a formal job at the beginning of month 5 when workers start reaching the end of their potential UI duration. This pattern suggests that UI incentives depress formal reemployment. Accordingly, the hazard rate starts increasing in month 5 and peaks in month 6 and 7, after workers have exhausted their potential UI duration. Nevertheless, the hazard rate remains relatively low, reaching at most 16% per month, and it falls quickly afterward, such that formal reemployment rates are low even when UI does not create any substitution effect anymore. Consequently, a sizable share of workers remains without a formal job 12 months after layoff (30% in the user sample; 40% in the random sample).

Formal reemployment rates are low in São Paulo but they are higher than in the country as a whole: Gerard & Gonzaga (2016) show that formal reemployment rates are lower in labor markets with higher informality. They also show that formal reemployment rates are low, and decreasing in informality rates, for workers who are not eligible for any job displacement insurance benefits (workers with very low tenure or fired for cause). Therefore, although there are clear incentive effects, low formal reemployment rates are likely a feature of labor markets with high informality rather than a consequence of job displacement insurance policies.

[25] The samples in Figure 2b are restricted to workers laid off in 2011 such that we observe their full UI spell in the UI data. We show in the Appendix that the patterns presented in Figures 2a, 2c, and 2d are similar when we impose that same restriction. We also show in the Appendix the difference between the average statutory UI benefits and the average UI benefits actually drawn by these workers, which comes from the facts that workers do not take up UI as soon as they become eligible and that UI takeup is imperfect.

Table 1: Descriptive statistics and representativity of displaced formal workers in the user sample

	User sample (Treatment)	Random sample	Difference (1)-(2)		User sample (Control)	Difference (1)-(4)	
(mean at/before layoff)	(1)	(2)	(3)		(4)	(5)	
Share working in urban area	0.974	0.971	0.003**	(.000)	0.975	-.000**	(.000)
Share working in metropolitan area of São Paulo	0.556	0.534	0.022**	(.002)	0.57	-.014**	(.002)
Share female	0.436	0.392	0.044**	(.002)	0.394	.042**	(.002)
Share with high school degree	0.728	0.655	0.073**	(.002)	0.758	-.030**	(.002)
Share white	0.703	0.694	0.009**	(.002)	0.704	-.001	(.002)
Tenure (months)	30.20	29.26	0.95**	(.06)	32.83	-2.63**	(.07)
Age (years)	32.82	32.55	0.27**	(.04)	34.13	-1.31**	(.03)
Monthly wage (R$2010)	1,541	1,467	73.6**	(6.6)	1,802	-261.5**	(6.7)
Statutory UI benefit level (R$2010)	809.5	786.3	23.2**	(.8)	859	-49.55**	(.7)
Statutory SP amount (R$2010)	3,804	3,568	236.2**	(15.3)	4,743	-938.3**	(17.5)
Statutory FGTS balance (R$2010)	3,229	3,026	203.3**	(14.9)	4,305	-1076**	(18.6)
UI replacement rate (UI benefit/wage)	0.683	0.698	-0.015**	(.001)	0.637	0.046**	(.001)
SP "replacement rate" (SP amount/wage)	2.540	2.492	0.048***	(.003)	2.649	-0.109***	(.003)
FGTS "replacement rate" (FGTS balance/wage)	2.213	2.166	0.047***	(.005)	2.366	-0.153***	(.005)
Monthly expenditure (R$2010)	459.8	n.a.	n.a.	n.a.	538.2	-78.40**	(2.7)
Number of observations	77,862	156,021	233,883		220,160	298,022	

The table displays the mean of the variables in the left-hand side column at or before layoff for displaced formal workers in the user sample (column 1), which constitute the treatment group in our empirical analysis in Section 3. We assess the representativity of this sample by comparing it to a random sample from the universe of displaced formal workers in São Paulo (column 2). We also compare it to the "placebo" displaced workers from the user sample (column 4), which constitute the control group in Section 3. Monthly expenditures are averaged over the 12 months before layoff; the variable is only available for workers in the user sample. Sections 2.4 and 3.1 provide details on the construction of these samples. Columns (3) and (5) display the estimated coefficient from regressing these variables on an indicator for the treatment sample (standard errors in parentheses). Significance levels: 1%*** 5%**, 10%*.

2.5 Coverage of the expenditure data

We discuss here the coverage of the expenditure data and evidence supporting its quality for our purpose.

VAT receipts in São Paulo cover a rich set of expenditure categories and sellers are required by law to issue these receipts for every transaction, irrespective of the total value or mode of payment. Nevertheless, the VAT is only levied on goods, so the data cannot cover VAT-excluded items, such as services and rental costs. Moreover, among purchases taxed by the VAT, we unlikely observe all expenditures because users must voluntarily provide their ID number to the seller for it to appear on the receipt. Although incomplete, the last row in Table 1 presents some first evidence that the data cover a sizable share of users' purchases of goods. It shows that the mean monthly expenditure in our data in the 12 months before layoff is 29.8% of the mean wage prior to layoff, despite excluding services and rental costs (it is 30.9% using medians).

As discussed in Section 1.4, the consumption approach used in the literature to inform the insurance value of job displacement insurance policies does not require that one observes the total of all expenditure categories. Yet, it is key that the coverage of the data remains constant following relevant changes in the economic environment, such as income shocks following layoff (we would confound changes in expenditures and changes in coverage otherwise). This condition is difficult to evaluate in our case because there is no panel dataset of expenditures in Brazil to which we could benchmark our data (we would not need to use our data otherwise). We thus provide two supporting pieces of evidence.

First, we show that the coverage of our data is relatively constant when we look at a cross-section of income levels. Figure 3a displays binscatters of the correlation between log(expenditure) and log(wage) for

formal workers in our data and in the latest round of the Survey of Household Expenditures (POF), in which a representative cross-section of Brazilian households were surveyed by the Brazilian Institute of Geography and Statistics (IBGE) in 2009-2010 (we restrict attention to households in São Paulo). The correlations control for covariates available in both datasets (age, age squared, and dummy variables for being white, having a high school degree, and being female). The x-axis corresponds to the logarithm of the monthly wage of a worker formally employed at the time of the interview (survey sample) and on December 31st (user sample). The y-axis corresponds to the logarithm of the average monthly expenditure in the previous 12 months in the household (survey sample) and as reported by the user (user sample). For the sake of comparability, expenditures in the survey data are restricted to categories that could be reported in the user data, namely the purchase of goods; additionally, we restrict the user sample to the first year for which we know whether users were already participating in the previous year (2011) to focus on consistent participants.

Figure 3a shows that our data capture a sizable share of households' total expenditures for comparable categories in the survey data. Specifically, for a given wage level, the mean expenditure in our data is equal to 19% of the mean expenditure in the survey data on average. The difference may come from two factors: the user may not provide her ID number for all her purchases and the user may not be making all the households' purchases.[26] Importantly, the wage-expenditure gradient is similar in the two datasets indicating that the coverage of our data is relatively constant across income levels.[27]

Second, we show that the coverage of our data is relatively constant across income changes. Figure 3b displays a binscatter of the correlation between $\Delta log(expenditure)$ and $\Delta log(wage)$ in our data. We exploit changes in wages for workers who switch firms between 2010 and 2014, restricting attention to those who were employed on December 31st in at least two consecutive years in the origin and destination firm. These are sizable income changes that we can measure in our data and that are not purely transitory. We then regress $\Delta_{t-1}^{t+1} log(expenditure)$ on $\Delta_{t-1}^{t+1} log(wage)$ and year fixed effects, where t is the first year in the new job. We obtain a correlation of .269, which is comparable to the correlation between changes in spending and labor income in Baker (forthcoming), who uses data from a personal finance website in the US. Moreover, the relationship appears to be linear. In particular, it shows no sign that the slope differs for large positive changes in wage compared to the small or negative changes in wage that one would expect following layoff.

We provide further evidence in our analysis against what we believe are the three main possible threats for the *constant-coverage* assumption underlying our results: a change in the likelihood that a worker provides her ID number when making purchases after layoff, a change in the proportion of households' purchases made by the worker, and the possibility of users substituting purchases from formal to informal stores when experiencing a negative formal employment shock.

3 Expenditure profile around displacement

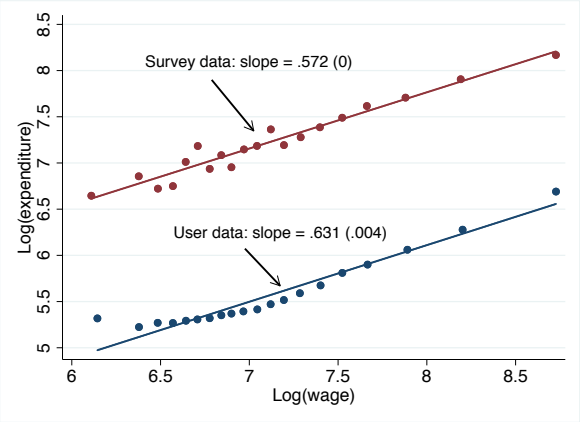
We now turn to the empirics. In a first step, we present the results of a difference-in-differences analysis in which we estimate changes in expenditures for displaced formal workers in the 12 months before and after layoff, compared to a control group of workers employed over the whole 25-month period. Our main finding is that workers increase their consumption upon layoff by about 35% despite experiencing a long-

[26] The NFP program creates incentives for households to provide the same ID number when making a purchase independently of the identity of the consumer: it gives participants one lottery ticket for each R$50 in total reported purchases.

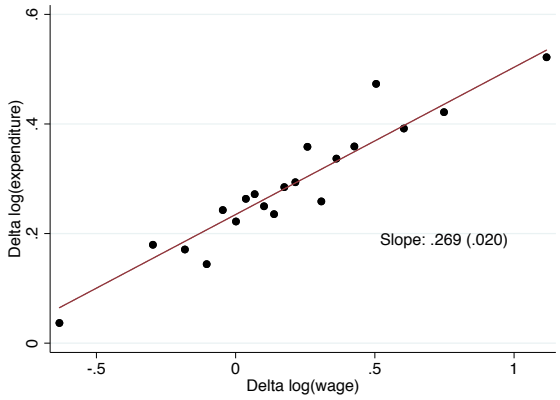
[27] Survey data are also imperfect and come with their own issues, but the fact that the wage-expenditure gradient is comparable to the one found in the survey indicates that we can replicate a relevant empirical regularity from the main Brazilian household expenditure survey in our data.

FIGURE 3: SUPPORTING THE QUALITY OF OUR EXPENDITURE DATA

(a) Wage-expenditure gradient in our data vs. survey data



(b) ΔWage and ΔExpenditure in our data



The figure provides two pieces of evidence supporting the quality of our de-identified expenditure data. Panel (a) displays binscatters of the correlation between log(expenditure) and log(wage) for formal workers in our data and in the latest round of the Survey of Household Expenditures (POF). The correlations control for age, age squared, and dummy variables for being white, having a high school degree, and being female. The x-axis corresponds to the logarithm of the monthly wage of a worker formally employed at the time of the interview (survey sample) and on December 31st (user sample). The y-axis corresponds to the logarithm of the average monthly expenditure in the previous 12 months in the household (survey sample) and as reported by the user (user sample). Expenditures in the survey data are restricted to categories that could be reported in the user data, namely goods. Panel (b) displays a binscatter of the correlation between $\Delta log(expenditure)$ and $\Delta log(wage)$ in our data (using all years). We exploit change in wages for workers who switch firms. We regress $\Delta_{t-1}^{t+1} log(expenditure)$ on $\Delta_{t-1}^{t+1} log(wage)$ and year fixed effects, where t is the first year in the new job.

run consumption loss of about 17% when they stop receiving any benefits. We use the word *consumption* because all these patterns are robust across expenditure categories; in particular, they are not driven by durable goods. We begin by laying out our empirical strategy. We then present our results, investigate relevant dimensions of heterogeneity, and link our results to the existing job displacement insurance policies.

3.1 Empirical strategy

We follow the literature and conduct a series of event analyses around job displacement. In particular, we estimate changes in expenditures in the 12 months before and after layoff for the sample of displaced formal workers described in Section 2.4 (see column 1 in Table 1). We use a control group of workers employed over the whole 25-month period to net out overall trends in the data in a difference-in-differences specification:

$$y_{itk} = \alpha_i + \alpha_k + \alpha_t + \sum_{k=-12}^{k=12} \beta_k \cdot Treat_i + \epsilon_{itk}, \tag{8}$$

where the α_i's, α_k's, and α_t's are fixed effects for each worker, each event time (i.e., months to/from the layoff month; negative values correspond to pre-event months), and each month (e.g., June 2012). The coefficients β_k's capture differential changes in monthly expenditure y_{ikt} in the months around the layoff event compared to a reference month (we choose $k = -6$; see below). We cluster standard errors by worker.

We construct our control group as follows. We select all months between 2011 and 2013 for which workers from the user sample are observed continuously employed in a given full-time private-sector job in the 25-month period centered around those months (workers have thus at least 12 months of tenure).[28] These

[28] In the few cases that a worker has more than one job in a given month, we select the job that pays the highest monthly wage.

worker-month pairs are candidate "placebo" displacement events. We then restrict attention to pairs for which the worker has at most 72 months of tenure, such that we can calculate placebo statutory SP amount and FGTS balance as if she was laid off in that month. We also require the worker to have some purchases in the expenditure data both before and after the 25-month period (again, to make sure that she was a consistent participant throughout the period). There can be several worker-month pairs for a given formal employment spell in the remaining sample. In that case, we randomly select one worker-month pair per year. Workers can thus serve as placebo displaced workers at most once a year, but they can do so in multiple years. We are left with a control group of 220,160 placebo displacement events in 2011-2013.

Table 1 shows that our treatment and control groups differ systematically. For instance, workers in the control group had higher wages, more tenure, and higher expenditure levels before the (placebo) layoff. Yet, Figure 4a shows that expenditure levels evolved similarly in the two groups prior to layoff, which supports the common-trend assumption underlying the specification in equation (8). Figure 4a displays the average monthly expenditure level in each of the 25 months around the event for the treatment and control groups separately (after netting out month fixed effects, α_t). The mean expenditure increases almost linearly in the control group. It is lower by about R$80 in the treatment group prior to layoff but it grows at a similar rate, with maybe the exception of the few months just before the event when workers might have learned already about their upcoming layoff (this is why we use $k = -6$ as reference month).[29] At layoff, the mean expenditure increases discontinuously by about 35% in the treatment group, exceeding the mean in the control group for two months. Afterward, expenditure levels decrease quickly, then appear to plateau, but drop again after month 5 when workers start exhausting their UI benefits. The pattern around UI exhaustion is fuzzy because workers exhaust UI benefits between months 5 and 7 after layoff (see Figure 2b). We thus present the results of an event analysis centered around UI exhaustion rather than displacement in Section 4. The mean expenditure in the treatment group is below its pre-layoff levels at the end of the 25-month window, implying a sizable long-run loss, especially compared to the trend in the control group. This is despite the fact that about 70% of workers in the treatment group had found a new formal job by then.

We use the specification in equation (8) to quantify the change in expenditures for displaced formal workers more precisely. Given the differences highlighted in Table 1, we re-weight the observations in the control group in all our regressions such that it compares better to the treatment group in terms of wage (and thus UI benefit), total lump-sum job displacement insurance benefit (SP+FGTS), and average expenditure prior to layoff (over $k = -12, ..., -6$).[30] We estimate regressions in levels but present our results in relative changes by dividing the $\widehat{\beta_k}$'s coefficients by the mean in the treatment group in the reference month.[31]

3.2 Main results

Figure 4b presents our first difference-in-differences results. The black line displays estimates using the whole treatment and controls groups as in Figure 4a (*unconditional* sample). The red line displays estimates restricting the treatment group to displaced workers who remain non-formally-employed in each month after layoff (*survival* sample; see, e.g., Kolsrud *et al.* , 2018). Specifically, the whole treatment group is included

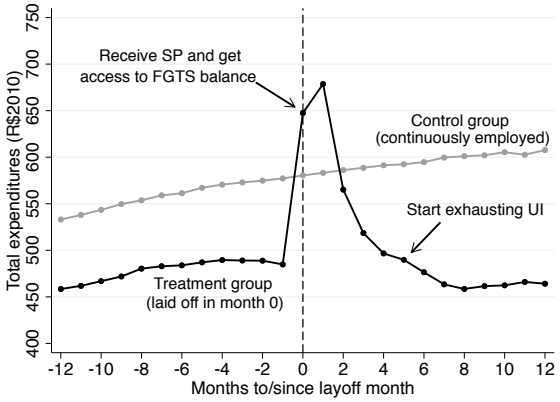
[29] In our survey of UI applicants, 60% of respondents report having learned about their upcoming layoff prior to the layoff month. Among those, the average worker (resp. median worker) learned about it 3 months (resp. 2 months) prior to the layoff month.

[30] We define cells by quintile of the wage, quintile of the total lump-sum amount at layoff (SP+FGTS), and decile of the mean expenditure between months $k = -12$ and $k = -6$ in the treatment group (250 cells). We then re-weight observations in each cell such that the cells have the exact same weight in the control group as in the treatment group (see, e.g., Naritomi, 2018). Unless mentioned otherwise, we always apply a similar reweighing when we present results for or with the control group in the rest of the paper.

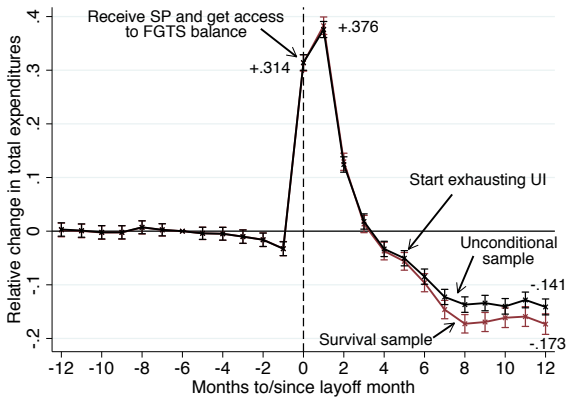
[31] We avoid using logs because users do not report positive purchases in all months. Percentage changes (both the increase at layoff and the long-run loss) are even larger using different transformations (e.g., inverse hyperbolic sine, see below and Appendix).

FIGURE 4: EXPENDITURE PROFILE AROUND DISPLACEMENT EVENT

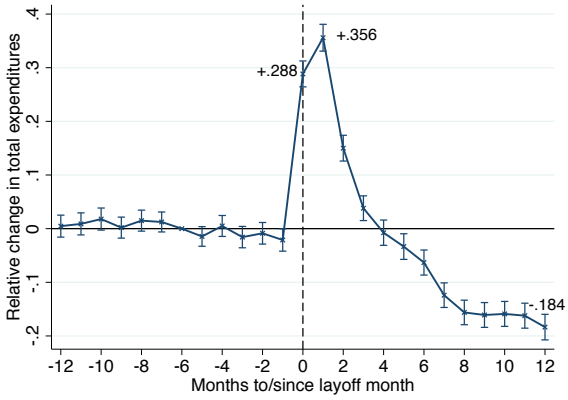
(a) Treatment vs. control (raw averages; unconditional sample)



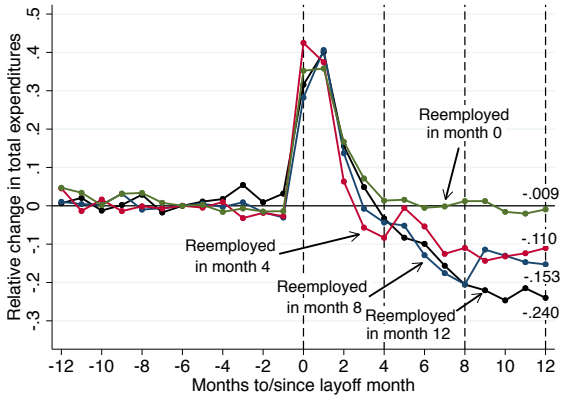
(b) DD estimates (unconditional and survival samples)



(c) DD estimates (sample conditional on no reemployment)



(d) DD estimates (samples reemployed in months 0, 4, 8, 12)



The figure presents our main results. Panel (a) displays the average monthly expenditure in each of the 25 months around the event for the treatment and control groups separately (raw data, we only netted out calendar month fixed effects). The vertical line indicates the displacement month. Panels (b)-(d) present difference-in-differences results (point estimates and 95% confidence intervals) using the specification in equation (8); we estimate regressions in levels but present our results in relative changes by dividing the estimated β_k's by the mean in the treatment group in the reference month ($k = -6$). The black line in panel (b) displays estimates using the whole treatment and controls groups (*unconditional* sample). The red line displays estimates restricting the treatment group to displaced workers who remain non-formally-employed in each month after layoff (*survival* sample; see text). Panel (c) presents similar results from a regression where we restrict the treatment group in all months to the subset of workers who remain non-formally-employed until at least the end of our event window (*conditional* sample). This sample is fixed over the 25-month period, thus avoiding composition effects. Panel (d) presents similar results for different (fixed) subsets of the treatment group, namely workers formally reemployed in months 0, 4, 8, and 12 after layoff, separately (we always use the full control group). In so doing, we show how expenditures change at formal reemployment. We omit confidence intervals in panel (d) for clarity, but all groups experience a significant increase in expenditures at layoff, and the long-run loss is significant for all groups but for those reemployed in month 0.

prior to layoff (the black and red lines coincide), but afterward, the sample drops workers as soon as they are reemployed. In other words, the sample only includes workers who "survive" without a formal job in each month after layoff, whose share in each month is the survival rate in Figure 2d. This is the sample that could receive job displacement insurance benefits contingent on non-formal-employment.[32]

Estimates are very close to 0 for most of the pre-layoff period, supporting our common-trend assumption. They become negative in the two months before layoff, at -1.6% and -3.3%, respectively. Workers' wages may grow slower just before layoff (we do not have high-frequency wage data), workers may be asked or choose to work fewer hours (we only observe contracted hours), or they may postpone some expenditures until they receive their SP and get access to their FGTS balance.[33] We do not focus on this specific pattern because it is not always robust across samples and subcategories of expenditures (see below).

The most striking pattern is that expenditures increase by 31.4% and 37.6% in the month of layoff and the subsequent month, respectively. The effect decreases quickly but remains positive in months 2 and 3 after layoff. Estimates are negative and more stable in months 4 and 5, at -3.3% and -5%, respectively. They then decrease again faster in months 6 to 8, when workers exhaust their UI benefits. The estimates also start diverging between the unconditional and the survival samples in those months. Hazard rates are higher in those months (see Figure 2c), increasing more rapidly the share of the unconditional sample that is formally reemployed, while workers in the survival sample remain by definition non-formally-employed (and with no UI benefits after UI exhaustion). Estimates are stable in months 8 to 12 and imply a long-run loss of -14.1% and -17.3% in the unconditional and survival samples, respectively, 12 months after layoff.

Composition effects. Figure 4c shows that the patterns in Figure 4b are not due to a composition effect, e.g., workers quickly reemployed driving the spike at layoff. It presents similar results from a regression where we restrict the treatment group in all months to the subset of workers who remain non-formally-employed until at least the end of our event window (*conditional* sample).[34] This sample is fixed over the 25-month period, and thus avoids composition effects. There is no significant drop in expenditures before layoff in the conditional sample and the sharp increase in expenditures is only slightly smaller than in the unconditional sample, at 28.8% and 35.6% in months 0 and 1, respectively. The patterns are generally similar as for the survival sample afterward, with a long-run loss of -18.4%.[35]

Reemployment. Figure 4d presents similar results for different (fixed) subsets of the treatment group, namely workers formally reemployed in months 0, 4, 8, and 12 after layoff, separately (we always use the full control group). In so doing, we address again concerns of composition effects and we show how expenditures change at formal reemployment. We omit standard errors in the graph for clarity. There are four lessons in Figure 4d. First, some groups do not experience a drop in expenditures before layoff, but all of them experience a sharp and significant increase in expenditures at layoff. In particular, the increase at layoff is unlikely due to a complementarity between leisure (i.e., time) and recorded expenditures in our data, as we find the same increase for workers reemployed immediately.[36] Second, workers reemployed immediately are the only group that does not experience any significant long-run loss. A likely reason for

[32] Given that the survival sample changes over time, we divide the $\widehat{\beta_k}$'s coefficients by the mean in the reference month for workers that still belong to the sample in month k. We also re-weight the observations in the survival sample such that the 250 cells (wage $\times$ lump-sum $\times$ pre-layoff expenditure) have the same weight in each month as in the unconditional sample (and the control group).

[33] In our survey of UI applicants, 12% of respondents experienced a slower growth in monthly wages in the months before layoff.

[34] We also re-weight observations in the conditional sample such that the 250 cells (wage $\times$ lump-sum $\times$ pre-layoff expenditure) have the same weight as in the unconditional sample (and the control group). Unless mentioned otherwise, we always follow the same reweighing strategy in the rest of the paper when we use a subset of the treatment group in our regressions.

[35] In month 12 after layoff, the survival and conditional samples coincide, but our estimated effect differ slightly between the results for each of the two samples in month 12 because of the inclusion of calendar months fixed effects, α_t, in the two separate regressions.

[36] Such complementarity could arise, e.g., through an increase in the likelihood that a worker provides her ID number when making purchases, in the proportion of households' purchases made by the worker, or in the time available to make purchases.

the long-run loss in the other samples, despite formal reemployment, is that the wage growth for formally reemployed workers in the treatment group is 10.8% lower than in the control group.[37] Third, expenditure levels systematically increase in the month following reemployment compared to workers reemployed in later months; the increase at reemployment is sharper in the period after month 8 when workers have access to no aditional benefits. To complement this finding, we present results from an event analysis centered around reemployment in the Appendix: expenditure levels increase by about 5% if we use workers reemployed in months 0 to 10 and by about 10% if we focus on those reemployed after all job displacement insurance benefits have been exhausted. Thus, there appears to be a real gain from formal reemployment. Fourth, the long-run loss is larger (-24%) for the sample of workers formally reemployed in month 12 than for the conditional sample, which is composed of workers reemployed only after month 12 (or never observed reemployed in our data). It could be the case that workers experiencing larger drop in expenditures search harder for a new job or that workers remaining non-formally-employed for a long period have selected into the informal sector (earning informal income).

Robustness checks. The patterns in Figure 4 are not due to outliers, to the fact that we use a specification in levels, or to the fact that some users are not reporting positive purchases in all months. We show similar patterns in the Appendix for the median, using the logarithm of the outcome plus 10, using an inverse hyperbolic sine transformation of the outcome, or restricting treatment and control groups to users who report positive purchases in all months prior to layoff. Moreover, we obtain similar patterns in the Appendix if we restrict the treatment group to workers laid off from a downsizing firm, which lost at least 30% of its workforce in the year of the layoff.[38] This latter result addresses possible concerns of endogenous layoffs, for instance of workers triggering their layoff to access the FGTS balance.

In sum, we find a large increase in expenditures at layoff despite a sizable long-run loss. Interestingly, if we integrate changes in expenditures over the whole year after layoff, we obtain an average drop in monthly expenditures of only 1%-2.5% (depending on the sample used). This highlights the benefits from having access to high-frequency data. With yearly data, we might have concluded that formal workers were relatively well insured against the consequences of job loss. However, as we show in Figure 4, this average drop would have masked a very non-smooth consumption pattern across months, which matters given that the average marginal utility of consumption and the marginal utility of average consumption can differ substantially with concave utility functions. The long-run loss that we estimate is comparable to existing estimates in the literature , despite the prevalence of informal job opportunities in our context. In contrast, the increase at layoff is unique to this paper: existing studies find a drop in expenditures at layoff, but they do not study a context where workers receive lump-sum job displacement insurance benefits. Comparing the average increase in expenditures to the average increase in financial resources, and assuming that we observe 19% of household expenditures on goods (see Figure 3), we obtain a marginal propensity to spend of .64 over the first three months after layoff.

3.3 Heterogeneity by expenditure category

We interpret the findings in Figure 4 as capturing changes in consumption because these findings are robust across expenditure categories and, as we show here, are not driven by purchases of durables.

[37] We use the specification: $DeltaLogWage_{it} = \alpha_t + \beta \cdot Treat_i + \epsilon_{it}$, where $DeltaLogWage_{it}$ is the difference between the log(wage) in the new job vs. before displacement (after vs. before placebo layoff in the control group). The regression sample includes one observation per worker in the control group and per worker formally reemployed by month 12 in the treatment group.

[38] We restrict attention to firms (i) with at least 10 employees 12 months before layoff, (ii) that had not been downsizing in the year prior to the year of layoff, and (iii) that remained smaller in the year following layoff.

Figure 5a displays the composition of expenditures before layoff (average expenditures from $k = -12$ to $k = -6$) in the treatment group. The composition of expenditures was very similar in the control group (we reproduce Figure 5a for the control group in the Appendix), the main categories being food (mostly groceries; about 39.5%), other non-durables (mostly personal goods; about 37%), and durables (about 10%).[39]

Figure 5b displays difference-in-differences results *in levels* (without rescaling the estimated $\widehat{\beta_k}$'s) for each of the categories in panel (a), separately (using the unconditional sample); we aggregate all non-durables together to make our point more clearly (we present results for food and other non-durable expenditures below and in the Appendix). It shows that non-durables are driving most of the increase in expenditures at layoff, and most of the long-run loss, although the pattern is visible for durable expenditures as well. In fact, the increase at layoff and the long-run loss are larger in relative terms for durables (+71.9% to +76.7%; -35.1% to -25.3%) than for non-durables (+21.3% to +24%; -18.8% to -13.7%). This is shown in Figures 5c and 5d, which display results for non-durables and durables in relative terms for the unconditional, survival, and conditional samples (as in Figure 4). Yet, because durables account for only 10% of expenditures (vs. more than 75% for non-durables), non-durables drive a much larger part of the changes in total expenditures.

The finding that durables do not drive our main results is important for interpreting them as consumption responses (Attanasio & Pistaferri, 2016) and as first evidence that liquidity constraints are unlikely to explain our results (liquidity constraints are more relevant for investments). We reinforce these points by displaying results for two subcategories of non-durables in Figures 5e and 5f, namely food and entertainment expenditures.[40] We find again a large increase in expenditures at layoff despite a sizable long-run loss in both cases, but the overall pattern is smaller in absolute values for food expenditures (+17.7% to +19%; -13.8% to -9.6%) than entertainment (+31.78% to +47%; -21.5% to -20.4%). This is consistent with a standard optimization of spendings within a month, as necessities are less likely to respond strongly to variations in income. In other words, the coefficient of relative risk aversion γ is likely larger for food expenditures than entertainment. We show similar results for grocery expenditures separately in the Appendix. The increase at layoff is slightly larger than for food expenditures as a whole (food away is partly a work-related expenditure) and the long-run loss is slightly smaller (groceries are more likely to include necessities).

A remaining concern for interpreting our results as consumption responses is the possibility of users substituting purchases from formal to informal stores when experiencing a negative formal employment shock. In this case, we might underestimate the increase at layoff and overestimate the long-run loss.[41] We cannot address this concern directly given that we do not measure informal expenditures in our data. However, we can address it indirectly by considering pharmacy expenditures (see Appendix). Pharmacies in Brazil are comparable to those in the UK: they do not sell the same wide range of products as in the US, but they do not sell only drugs as in some continental European countries. Interestingly, the same survey data used in Figure 3a reveal that 98% of the types of goods bought in a pharmacy are actually bought in pharmacies, such that the informality margin is mostly irrelevant. Products sold in pharmacies are less likely to be "bingeable," so we should not expect a large increase at layoff. We still observe a significant

[39] Durables (about 10%) include personal electronics, home appliances, furniture, vehicles and vehicle parts. Non-durables excluding food (about 37%) include temptation goods (less than 1%; liquor and tobacco stores, gambling, sugar-related foods such as ice-cream and candy stores), non-essential personal goods (about 30%; apparel, cosmetics, entertainment, travels, decoration), pharmacy and health-related expenditures (about 3.5%; mostly pharmacy expenditures), and other non-durables (about 3%; e.g., office supply, pet stores). Food (about 39.5%) includes groceries (about 36%) and food away (about 3%). Home improvement (about 4%) includes expenditures related to building materials, renovations, and home purchases. Transportation (about 2.5%) includes mostly gasoline. "Other" (about 2.5%) are sector codes that are not easily classified and "missing" (about 4%) are cases where the sector code is unknown.

[40] This category includes expenditures related to bookstores, toys, sport, movies, DVDs and video games, music, and theme parks.

[41] In contrast, we might underestimate the increase at layoff and the long-run loss if workers are less likely (resp. more likely) to provide their ID number to sellers at layoff (resp. in the long-run) because of income effects.

increase of about 4%. Moreover, the long-run loss is even larger than for total expenditures (around 35%).

In the rest of the paper, we focus on non-durables and refer to our results as consumption responses. Comparing again the increase in non-durable expenditures to the increase in financial resources, as we did above for total expenditures, we then obtain a marginal propensity to consume of .4 over the first three months after layoff.

3.4 Linking our results to job displacement insurance benefits

We link our results to job displacement insurance policies by exploiting existing variation across workers.

Laid-off vs. fired. Figure 6a compares difference-in-differences results for workers who were laid off (our treatment group in all results so far) and for workers who were fired for cause (*fired* hereafter) and were not eligible for any job displacement insurance at separation. The control group is the same in both cases. We construct the sample of fired workers following the same steps as for laid off workers (see Section 2.4; the "reason for separation" variable has different codes for workers who are laid off vs. fired). We show results for the unconditional samples only because being fired is rare, so the sample is rather small (4,041 events). As a result, estimates are less precise, particularly when we re-weight the sample of fired workers in the same way as we re-weight the control group to improve its comparability with our treatment group.

Estimates are close to 0 for both laid off and fired workers in the months prior to displacement, but they diverge dramatically upon displacement. Fired workers experienced a *drop* in consumption, which reached -26.1% in month 1. The sharp difference in consumption profiles between the two groups indicates that job displacement insurance benefits are driving the increase at layoff in the previous figures. The estimates for fired workers remain low in the months following displacement such that they remain below the estimates for laid off workers, even though the share of workers formally reemployed is higher among fired than laid off workers (this is consistent with incentive effects; see Appendix). The estimates converge at the end of the 25-month window; by then, survival rates in non-formal-employment are also similar in the two samples.

Variation in UI replacement rates. Figure 6b compares difference-in-differences results for workers who were eligible for different UI replacement rates. We use variation in wages, which is the only source of variation in UI benefit levels. We compare workers in the top and bottom quartiles of the wage distribution in the treatment group, after restricting attention to workers who had more than 24 months of tenure at layoff, such that they were all eligible for 5 months of potential UI duration.[42] The average UI replacement rates in the top and bottom wage quartiles are 41% and 80%, respectively. Tenure levels are also generally higher in the top wage quartile, implying that the total SP and FGTS amount is higher in the top wage quartile in proportion to their wage. We thus control flexibly for tenure levels at layoff to net out variations in the replacement rate from the total SP and FGTS amount. In particular, we use the following specification:

$$\begin{aligned} y_{itk} =& \alpha_i + \alpha_t + \sum_{q=\{1,4\}} \sum_{k=-12}^{k=12} (\alpha_{kq} \cdot \mathbb{1}\{Wage_i \in Q_q\} + \beta_{kq} \cdot Treat_i \cdot \mathbb{1}\{Wage_i \in Q_q\} \\ &+ \sum_{k=-12}^{k=12} (\delta_k \cdot f\left(Tenure_i\right) + \eta_k \cdot Treat_i \cdot f\left(Tenure_i\right)) + \epsilon_{itk}, \end{aligned} \tag{9}$$

where the sets Q_q's correspond to the quartiles of the wage distribution in the treatment group. The coefficients α_{kq}'s capture event time fixed effects for each wage quartile (we also assign workers in the control group to the quartile corresponding to their wage at "placebo" layoff); the coefficients β_{kq}'s capture differ-

[42] We exploit the variation in potential UI duration through a RD design in the next section (the sample size is relatively small).

ential changes in monthly expenditure in the treatment group compared to the reference month, separately for each quartile. The coefficients δ_k's and η_k's control for tenure effects that are allowed to differ in the treatment and control groups; in practice, we use a third-order polynomial in tenure for the function $f(\cdot)$.[43]

Figure 6b displays the estimated β_{kq}'s (divided by the mean in the reference month for treatment workers in the respective quartile) using the survival sample to focus on workers meeting the UI contingency requirement.[44] The increase at layoff is slightly larger for workers with higher UI replacement rates. Estimates are then similar in the two quartiles in months 2 and 3, when they essentially return to 0. The most interesting difference appears in the following months. While consumption continues to decrease in the top wage quartile, it remains flat for workers with higher UI replacement rates. The drop in consumption when they start exhausting their UI benefits is then much steeper for these workers, such that estimates for the two quartiles converge by month 8. In later months, lower-wage workers fare again better in relative terms, perhaps because informal work opportunities are closer substitutes to formal jobs for lower-skilled workers (Perry *et al.* , 2007)).

The differential pattern in Figure 6b links more clearly the consumption drop around UI exhaustion in previous figures to the UI program. The lack of smoothing among workers with high UI replacement rates in anticipation of the negative income shock at UI exhaustion (in months 3-5) is also particularly striking. This pattern will become clearer in Section 4, where we present the analysis of event studies centered around UI exhaustion. These results already suggest that displaced workers' consumption choices are not easily rationalized by liquidity constraints, which is in line with evidence for the US (Ganong & Noel, 2018).

Variation in SP and FGTS. We follow a similar approach to compare difference-in-differences results for workers who were eligible for different replacement rates from the total SP and FGTS amount. We compare workers in the top and bottom quartiles of the tenure distribution in the treatment group, after we restrict attention to workers with more than 24 months of tenure at layoff to net out variations in potential UI durations, and we control flexibly for wages to net out variations in UI replacement rates. The specification is the same as in equation (9), replacing $Tenure_i$ by $Wage_i$ and vice-versa. On average, the SP and FGTS amounts reach 428% and 670% of the pre-layoff wage in the bottom and top tenure quartiles, respectively.

Figure 6c displays the estimated $\widehat{\beta_{kq}}$'s (divided by the mean in the reference month for treatment workers in the respective quartile) using again the survival sample.[45] The estimates in months 0 and 1 are large in the bottom tenure quartile (+26% and +32.4%), but they are much larger in the top tenure quartile (+30.1% and +42.8%). This differential pattern links more clearly the increase in consumption at layoff in previous figures to the lump-sum job displacement insurance benefits. The estimates converge in the following month.

Overall, following a similar approach as above but now comparing the differential change in consumption and the difference in lump-sum amount for the average worker in the two quartiles, we obtain a marginal propensity to consume of .22, most of it coming from the first few months after layoff. One would expect the marginal propensity to consume to decrease once workers already receive a large lump-sum amount (as in the bottom quartile). Workers also likely increase other spending categories that are not captured in our data. For instance, in our survey of UI applicants, 26.5% of respondents report planning to use their SP and FGTS amounts to repay debts accumulated in previous months.

[43] We do not use weights when estimating the specification in equation (9) given that we exploit variation in wages and control for the other source of variation in SP and FGTS amounts, directly.

[44] Results for the unconditional sample, as well as differences in survival rates, are presented in the Appendix.

[45] Results for the unconditional sample, as well as differences in survival rates, are presented in the Appendix.

3.5 Heterogeneity by worker characteristics

Finally, Figure 7 presents some heterogeneity in our results by worker characteristics. Each line in each panel comes from a separate regression for non-durable expenditures, using the specification in equation (8) and re-weighting the respective subsample such that it compares better to the overall treatment group.

Panel (a) shows that consumption profiles around displacement are similar for female and male workers. This is useful to the extent that the proportion of households' purchases made by the worker is less likely to vary for women than for men. The larger long-run loss for female workers is likely due to the fact that they take longer to find a new formal job (we display survival rates by worker characteristics in the Appendix).

Panels (b) and (c) compare workers with less than vs. at least a high school degree and workers with less vs. more than the median age in the treatment group, respectively. The overall patterns are again similar, but present some interesting differences. There is a general view in the literature that informal job opportunities may be easier to find and may constitute better substitutes for formal jobs among lower-skilled and younger workers (Perry *et al.* , 2007). Accordingly, the long-run loss is smaller and almost identical in the unconditional and the survival samples for lower-educated workers. The long-run loss is also almost identical in the unconditional and the survival samples for younger workers. This indicates that, although their need for job displacement insurance may be sizable, the gains from making benefits contingent on non-formal-employment may be limited for lower-skilled and younger displaced workers. On average, older workers are able to maintain higher consumption levels after layoff (despite the fact that they take longer to find a new formal job), but those who remain without a formal job at the end of the 12-month window have the same long-run loss as younger workers. Older workers may thus be better prepared to cope with income shocks but they may particularly suffer from leaving the formal sector.

Finally, panel (d) shows that workers with previous experience with the consequences of job displacement appear able to maintain higher consumption levels after layoff. Specifically, the dashed lines display difference-in-differences results for workers who experienced another layoff from a formal job in the previous 5 years, had at least 12 months of tenure at that time, and remained at least 9 months without a formal job then (but less than 12 months to avoid including workers who transitioned into the informal sector).

4 Expenditure profile around UI payments and UI exhaustion

The evidence so far shows that consumption increases sharply at layoff, and that workers do not seem to smooth consumption in anticipation of the negative income shock at UI exhaustion, despite the steeper consumption drop afterward. The pattern around UI exhaustion in the previous figures is fuzzy, however, because workers in our sample exhaust UI benefits at different points in time between months 5 and 7 after layoff. We thus present here the results of an event analysis centered around UI exhaustion. We end the section by presenting the results of a Regression Discontinuity (RD) analysis, which compares the consumption and reemployment patterns of workers who were eligible for different potential UI durations.

4.1 Empirical strategy

The empirical strategy builds on the analysis in the previous section. We select all cases in the treatment group for which workers were eligible for 5 months of UI – they had more than 24 months of tenure at layoff – and were observed drawing 5 monthly UI payment in the data (we have UI payment data until 2012). For each of these 15,774 UI spells, we know the precise date and value of each monthly UI payments.

We then fetch all the daily expenditures (by category) in the 8 months before and the 5 months after the last UI payment. Next, we use observations in the control group with the same (placebo) layoff months to net out overall trends in the data. Specifically, for each category, we calculate the growth in average expenditure levels in the control group in each month after layoff compared to the layoff month. We do this separately for each of the 250 cells used in the re-weighting described in Section 3.1. Within each cell and by category, we then subtract this counterfactual growth from the expenditure levels of workers in the treatment group. Finally, we use these de-trended outcome variables in a standard event analysis specification:

$$y_{itk} = \alpha_i + \alpha_t + \sum_{k=\underline{k}}^{k=\overline{k}} \beta_k + \epsilon_{itk}, \tag{10}$$

where α_i's and α_t's are fixed effects for each worker and each month. The coefficients β_k's capture changes in de-trended expenditure levels y_{ikt} in the time periods k around a UI payment event (between a first period $\underline{k}$ and a last period $\overline{k}$) compared to a reference period. We define our time periods k carefully because UI payment dates can occur on any day within a month and because, as we show below, expenditure levels within a month are sensitive to the timing of UI payments. Finally, we cluster standard errors by worker.

4.2 Expenditure profile around UI payment dates within a month

Figure 8a presents the results of an event analysis based on the specification in equation (10), in which we investigate how non-durable expenditures change around UI payment dates within a month. Our window of analysis starts at the first UI payment date. We then divide the time between two payment dates – usually around 30 days – into four *quarter-month* periods: three 7-day periods spanning the first 21 days since a UI payment date (the first period includes the payment date) and a fourth period including the remaining days until the next UI payment date. To investigate how expenditure levels evolve after UI exhaustion in the same analysis, we construct comparable time periods after the last UI payment. We randomly assign dates for 5 additional (hypothetical) UI payments after UI exhaustion, using the actual distribution of days between UI payment dates in the data, and we divide again the time between hypothetical payment dates into similar quarter-month periods. Finally, we average expenditures at the worker-by-period level and we use the time period preceding the last UI payment as reference period.[46]

Figure 8a displays results in relative changes by dividing the estimated $\widehat{\beta_k}$'s by the mean in the treatment group in the reference time period. It shows that non-durable expenditure levels follow a systematic pattern over the UI spell: expenditure levels increase by 15%-20% in the 7 days following a UI payment and then decrease quickly in subsequent periods until they increase again following the next UI payment. Relatedly, we show in the Appendix that we also obtain similar patterns around paydays for formal employees in our data, using the fact that monthly salaries must be paid before the fifth business day of each month in Brazil.

These results further demonstrate that expenditure levels are very sensitive to cash-on-hand. To our knowledge, this is the first evidence of "payday effect" in the context of UI benefits, and it is consistent with other findings in the literature that expenditure levels within a month are very sensitive to the timing of various types of payments, e.g., food stamps in the US (Hastings & Washington, 2010) or paydays in Iceland (Olafsson & Pagel, 2016). However, we recognize that these results may not reflect consumption responses, as most goods effectively become durable at a high-enough frequency. Yet, Shapiro (2005) shows that similar expenditure patterns for food stamp beneficiaries in the US correspond to consumption responses. If

[46] The month fixed effects are based on the month of the first day in each time period.

we interpret such variation as consumption in our context, all the consumption gains from UI would be concentrated around the week when benefits are disbursed as consumption in weeks when no benefits are paid are as low as when all benefits are exhausted.

Figure 8a also shows that expenditure levels are generally decreasing over the first time periods in our window of analysis, but appear relatively constant in the time periods leading to the last UI payment. This is consistent with the findings that expenditure levels decrease quickly after month 1 in previous figures (the first month after layoff), but that the drop appears to plateau in months 3 to 5. Figure 8a reinforces our interpretation of this latter finding as suggesting a lack of anticipation of the negative income shock at UI exhaustion: the sharp increase in the 7 days after the *last* UI payment date is as large as increases following previous payment dates in the UI spell. Finally, our estimates in subsequent periods are centered around 0, implying that workers spend about the same in these periods as they did in the period just before a UI payment date during the UI spell, even though many UI exhaustees find a new formal job in these periods.

In theory, the patterns around and after UI exhaustion in Figure 8a could be affected by composition effects because the hazard rates of formal reemployment increase after UI exhaustion (see Figure 2): an increase in consumption among formally reemployed workers could be mitigating a reduction in consumption among those who remain without a formal job. We thus present the results from a similar analysis in Figure 8b for the subset of workers who remain without a formal job until the end of the time window considered in this analysis. The increase in expenditure levels in the 7 days following the last UI payment date is again as large as increases following previous payment dates in the UI spell. In following periods, expenditure levels drop below 0, which is consistent with the larger long-term loss in consumption in previous figures for workers who remain without a formal job.

4.3 Expenditure profile around UI exhaustion

Our estimates for the time periods leading to and following the last UI payment in Figures 8a and 8b imply that we would find a sharp and persistent drop in expenditure levels if we were to aggregate the data in 30-day periods around UI exhaustion. Figures 8c-8d show the results from such an analysis, for a window of 90 days before and 150 days after the last UI payment date. We aggregate the data in 30-day periods centered around UI exhaustion (period 0 is the 30-day period just after, and including, the last UI payment date) rather than in months because expenditure levels are not smooth between UI payment dates (as shown in Figures 8a and 8b) and because payment dates can fall on any day within a month.

Figures 8c and 8d use the same samples of UI exhaustees as in Figures 8a and 8b, respectively. Our estimates are around 0 in the periods leading to the last UI payment (the 30-day period prior to the last UI payment is the reference period), confirming the lack of smoothing in anticipation of UI exhaustion. We find no evidence that workers decrease their consumption even in the 30 days after their last UI payment in Figure 8c, despite the 5%-7% drop that they experience in subsequent periods (at this level of aggregation, we interpret again our estimates as capturing consumption responses). In Figure 8d, there is a slight decrease in consumption starting in period 0, although our estimates are only statistically significant from period 1 onward. It thus seems that workers who would likely benefit the most from starting to smooth their consumption earlier – the consumption drop after UI exhaustion reaches 12% in this sample – start reducing their consumption in the 30 days after their last UI payment. The spike for the 7-day period after the last UI payment in Figure 8b indicates that this takes place towards the end of the 30-day period, however.

We show similar patterns in the Appendix for the median, using the logarithm of the outcome plus 10, or using an inverse hyperbolic sine transformation. In all cases, we find no evidence of consumption

smoothing in anticipation of UI exhaustion, despite a sizable drop in consumption in subsequent periods.

The results in this section suggest that a high degree of myopia or severe saving constraints[47] may best rationalize the consumption profile of displaced formal workers in our context. We make this point more formally in the next section by introducing myopia and saving constraints into the model laid out in Section 1 and by showing that, in so doing, we can fit the consumption and reemployment patterns in our data. Before proceeding, we present the results of a RD analysis, which compares the consumption and reemployment patterns of workers eligible for four vs. five monthly UI payments. Even though the sample size for this exercise is small, the results complement our findings and provide out-of-sample moments to evaluate the predictive power of our estimated models.

4.4 Regression discontinuity analysis

Our analysis exploits the same RD design as in Gerard & Gonzaga (2016). They provide more in-depth discussion and description of the institutional details. Below, we explain the sample restriction and specification choices used to exploit the same RD variation for different outcomes.

Sample. Displaced formal employees were eligible for three, four, or five months of UI if they had more than 6, 12, or 24 months of accumulated tenure in the 36 months prior to layoff. In this section, we exploit the discontinuity at the 24-month cutoff for workers who had a single formal job in the previous 36 months. We focus on workers who had a single formal job in the previous 36 months because of a data limitation: the measure of tenure in RAIS underestimates the measure used by the UI agency by up to two months for every job spell. We thus measure the running variable, and consequently UI eligibility, more precisely for workers who had a single formal job spell in the previous 36 months. We focus on the 24-month cutoff because the usual conditions supporting the validity of a RD design appear to hold at this cutoff: the layoff density is smooth and there is no difference in sample composition at the cutoff (see below). In contrast, the layoff density is not smooth around the other eligibility cutoffs (see Gerard & Gonzaga, 2016).

Our main sample of analysis includes the subset of displaced workers in the user sample who had a single full-time private formal job in the previous 36 months. Workers with less than 22 months (resp. more than 24 months) of tenure at layoff were eligible for four (resp. five) monthly UI payments. Workers with 22 to 24 months of tenure were eligible for either four or five monthly UI payments because of the noise in our measure of the running variable. We restrict the sample to workers who had between 16 and 30 months of tenure such that we have a six-month window on each side of the cutoff for which we assess workers' potential UI duration precisely. The final sample includes 8,844 layoff events for which we have expenditure and formal employment data and 2,364 for which we also have UI data covering their entire UI spell.

Empirical strategy. Let T_i be the normalized tenure of worker i, such that $T_i = 0$ at the 24-month cutoff. Without noise in our measure of the running variable, we would simply regress an outcome y_i on a constant, an indicator for tenure levels above 24 months $1\left(T_i \geq 0\right)$, and a control function in tenure $f\left(T_i\right)$:

$$y_i = \alpha + \beta \cdot 1\left(T_i \geq 0\right) + f\left(T_i\right) + \epsilon_i, \tag{11}$$

The average treatment effect at the cutoff is captured by β under a continuity assumption for the control function at $T_i = 0$. In practice, we approximate the control function with local linear functions on each side of the cutoff using a triangular kernel. We then address the ambiguous eligibility of workers with 22

[47] Although this population has a savings technology (i.e., bank accounts), it may still be the case that saving is very costly. For instance, in developing countries there are non-trivial pressures to share income, often referred to as "kinship taxation" (Squires, 2018), which could effectively generate negative interest rates on savings.

to 24 months of tenure by excluding them from our regressions (we are left with 7,613 observations; 2,028 observations have UI data). With this "donut hole" approach, which we illustrate in Figure 9 (see below), the value of the outcome at the cutoff from the left is thus estimated by a local linear function fitted only to observations below 22 months. Finally, we cluster standard errors at the tenure level.

We provide evidence supporting the validity of our RD design in the Appendix. In particular, we show that there is no visible or significant change in the distribution and composition of our sample around the cutoff, by presenting a series of RD graphs and RD estimates using the specification in equation (11).

Results. First, we present results for UI outcomes, using the subsample with UI data. Figure 9a plots the share of workers who drew five monthly UI payments by tenure levels. The share is close to zero for workers with less than 22 months of tenure. It then increases between 22 and 24 months of tenure, as more workers are eligible for five UI payments,[48] and reaches around 60% above 24 months of tenure. The change in potential UI duration around the cutoff thus impacted the actual UI duration. The lines in the figure illustrate our empirical strategy. The line on the left of the cutoff (resp. right of the cutoff) is estimated using observations in a bandwidth of six months below 22 months (resp. above 24 months) of tenure. The treatment effect is the difference between the two lines at the cutoff, a significant increase of 58.2pp.[49]

Figure 9b shows that the increase in UI duration is concentrated in months 5 and 6 after layoff. It displays RD estimates (with their 95% confidence intervals) from using the specification in equation (11) in each month since layoff, separately; the outcome is a dummy variable for drawing UI benefits in that month. Workers eligible for five monthly UI payments were 22.3pp and 31.8pp more likely to draw UI benefits in months 5 and 6, respectively. The estimates are small and insignificant in adjacent months.

Next, we present results for formal reemployment outcomes, using the full sample to increase precision. Figure 9c shows that workers eligible for a longer potential UI duration delayed formal reemployment. It displays RD estimates in each month since layoff using a dummy variable for remaining without a formal job *at the beginning of that month* as outcome. The estimated effects, which capture differences in the survival rates in non-formal-employment at the beginning of each month, are increasing over time. The difference is marginally significant (at the 10% level) in month 5 (+3.9pp), indicating that workers with different potential UI durations had different formal job finding rates prior to any difference in their eligibility to draw additional UI benefits. The differences in survival rates in the first few months after layoff are more precise in Gerard & Gonzaga (2016) and become significant earlier in their case. There is thus some effect on formal reemployment in anticipation of UI exhaustion in our context, a pattern that has been documented in other contexts as well (see, e.g., Card & Levine, 2000, for the US). The difference in survival rates increases to 7.9pp and is statistically significant at conventional levels in month 6, implying that workers on the right of the cutoff further reduced formal job finding in month 5, when they were more likely to draw UI benefits. The estimated effects then remain relatively stable until the beginning of month 8, and only start decreasing once workers eligible for five monthly UI payments were not drawing any UI benefits anymore.

Finally, we present results for relative changes in non-durable expenditures, using again the full sample to increase precision. Figure 9d displays RD estimates in each month since layoff *for the median*.[50] The estimated effects are close to 0 before and just after layoff, but they become positive precisely around the time workers on the right of the cutoff are more likely to draw UI benefits. The increase in consumption is marginally significant (at the 10% level) and reaches 25.1% in months 5, and it peaks at 40.2% and is

[48] The increase in the share of workers who drew five monthly UI payments would start at lower tenure levels, and would be much less sharp, if we had not selected workers with a single formal job spell in the previous 36 months.

[49] We also show in the Appendix that we find no effect on UI takeup.

[50] We use the outcome in levels in the regression and then divide the estimated coefficients by the median on the left of the cutoff.

significant at conventional levels in month 6. We show in the Appendix that RD estimates *for the mean* are too noisy (the sample size is relatively small), but results are consistent with those in Figure 9d for non-linear transformations of the outcome.

These results add further evidence of excess sensitivity of consumption to cash-on-hand as workers know exactly how much they are eligible for once their UI claim is approved. There is no detectable difference in consumption in the beginning of the unemployment spell, but a significant difference only around the time the additional benefits are disbursed. The results in Figure 9d are thus consistent with the apparent lack of consumption smoothing in anticipation of UI exhaustion in Figure 8. They also are a contribution on their own as it is rarely the case that one can study consumption responses to extensions of UI benefits.

5 Models of behavior rationalizing our findings

In this section, we follow a recent strand in the literature and explore how simple models of behavior can fit the consumption and formal reemployment patterns in our data. In particular, we consider variants of the "standard" model in DellaVigna *et al.* (2017), which they show can fit reemployment patterns in their data relatively well.[51] This model corresponds to the model laid out in Section 1. We then use the estimated models to illustrate the incentive-insurance trade-offs under counterfactual job displacement insurance designs, highlighting the key role played by the disbursement policy.

5.1 Target empirical patterns for the estimated models

For simplicity, the empirical moments that we use as "targets" to be matched by our models are based on the subset of displaced workers eligible for five monthly UI benefits who took up UI in their first month of eligibility. The main advantage of selecting this specific estimation sample is that it allows us to align the months since the layoff event and the UI exhaustion event.[52] This is shown in Figure 10. Panel (a) displays the share drawing UI benefits and the hazard rate of formal reemployment in each month since layoff. UI takeup and UI exhaustion take place in month 1 and month 5, respectively, for all workers in this sample. The hazard rate of formal reemployment, which is increasing over the UI spell,[53] increases more sharply than in Figure 2c, peaking in month 6 at around .2 and decreasing in subsequent months. Panel (b) displays difference-in-differences estimates for non-durables expenditures, in which the treatment group is restricted to the *survival* subset of our estimation sample. The increase in consumption at layoff is sharper than in Figure 5c, peaking in month 0 at +48.9% and decreasing in the following month already, but the cumulative increase over the first 3 months is comparable. The pattern around UI exhaustion is sharper than in Figure 5c, with the drop in consumption taking place just after UI exhaustion as in Figure 8d.

5.2 Structural model

We adapt the discrete-time model in Section 1, and impose a few additional assumptions, in order to set up a simple structural model that is able to fit the consumption and formal reemployment patterns in our data.

[51] We thank the authors for sharing their codes. The key point of DellaVigna *et al.* (2017) is that a reference-dependence model can perform even better without any heterogeneity. We do not use their reference-dependence model as our benchmark because, on its own, reference-dependence would not predict the most striking pattern in our data, i.e., the spike in consumption at layoff (it is also more demanding computationally). This does not imply, however, that reference-dependence is inconsistent with our findings.

[52] In so doing, we also follow DellaVigna *et al.* (2017) and Ganong & Noel (2018) by taking the timing of UI takeup as given.

[53] The decrease in the hazard rate in the first months after layoff in Figure 2c is driven by workers who do not takeup up UI.

Time and job search. We assume that time periods correspond to months, such that the variable $h_{i,t}$ in the model corresponds to the probability of reemployment from one month to the next. The search cost function is assumed to take the form: $\psi_i(h_{i,t}) = \kappa_i \cdot h_{i,t}^{1+\gamma}/(1+\gamma)$, where the cost of search κ_i can vary across workers. Given this assumption and equation (1), the first-order condition for $h_{i,t}$ is given by: $\kappa_i \cdot h_{i,t}^{\gamma} = \delta \cdot \left[V_{i,t+1}(a_{i,t+1}) - U_{i,t+1}(a_{i,t+1})\right]$, where the expression on the right-hand side is the net gain from reemployment in period $t+1$. We also have:

$$h_t \equiv \int h_{i,t}\, di = \int \left(\frac{\delta \cdot \left[V_{i,t+1}(a_{i,t+1}) - U_{i,t+1}(a_{i,t+1})\right]}{\kappa_i} \right)^{1/\gamma} di \tag{12}$$

The parameter γ is thus the inverse of the elasticity of search effort with respect to the net gain from reemployment. Equation (12) also shows that, for a given elasticity, the scale of the costs of search k_i must be adjusted to the scale of the net gain from reemployment – e.g., it is a function of the discount factor δ and the cardinal utility of consumption – to match our first set of moments, the hazard rates in Figure 10a.

Utility of consumption. The per-period utility of consumption is assumed to be logarithmic: $\ln(c_{i,t})$. We are currently replicating our results with different utility functions, as the coefficient of relative risk aversion is equal to 1 with this utility function, which is relatively low, implying a relatively low insurance value.

Wage and benefits. We set the wage prior to layoff, the UI replacement rate, and the replacement rate from the total SP and FGTS amount at the mean in our estimation sample: $w^e = \text{R\$}1,439$, $b/w^e = .69$, and $f/w^e = 4.73$. We also set the reemployment wage $w^r = .856 \cdot w^e$ because we find that the wage growth for formally reemployed workers in the estimation sample is 14.4% lower than in the control group.[54]

Saving and borrowing. As in DellaVigna *et al.* (2017), workers are assumed to have zero assets at layoff and they cannot borrow against their future income, $\overline{a}_i = 0$, and any savings yield zero interest, $r = 0$. Displaced workers often have limited liquid savings at layoff (see, e.g., Chetty, 2008, for the US). In our survey of UI applicants, more respondents had positive debts than positive savings (59% vs. 46%) and only one third reported "drawing down my savings" as one of the ways they would be able to find the equivalent of one monthly wage in case of an emergency (they could report multiple options). In one of our models below, we relax the $r = 0$ assumption and introduce savings constraints by allowing r to be negative.

Other income and informality. We introduce two additional assumptions to rationalize the fact that consumption does not drop to zero after UI exhaustion (or after layoff for workers who are not eligible for any job displacement insurance). First, we assume that workers have another source of income to finance their consumption that is constant over time. In our survey of UI applicants, the median respondent lived in a two-adult household, and her household income before layoff was between 3 and 5 times the minimum wage, while her own wage was only between 2 and 3 times the minimum wage. Specifically, we thus assume that workers live with another adult who earns the same wage w^e and that the two of them pool their financial resources, such that workers have an income of at least $w^e/2$ in all periods. Together with the "zero assets at layoff" assumption, this implies $c = w^e$ prior to layoff. Second, to rationalize the fact that consumption never drops by as much as 50% after layoff, we allow workers to engage in some informal work activities while not yet formally reemployed. This is a natural assumption in the Brazilian context. Specifically, we assume that displaced workers who are not formally reemployed can earn additional income $l_{i,t} \cdot w^l$ at a utility cost: $\phi(l_{i,t}) = \chi \cdot l_{i,t}^{1+\lambda}/(1+\lambda)$. Without information on informal wages for our

[54] We use the specification: $DeltaLogWage_{it} = \alpha_t + \beta \cdot Treat_i + \epsilon_{it}$, where $DeltaLogWage_{it}$ is the difference between the log(wage) in the new job vs. before displacement (after vs. before placebo layoff in the control group). The regression sample includes one observation per worker in the control group and per worker formally reemployed by month 12 in the estimation sample.

sample, we set $w^l = w^r$ but impose the restriction $l \in [0,1]$, such that informal earnings cannot exceed formal earnings. The value function $U_{i,t}(a_{i,t})$ thus becomes:

$$U_{i,t}(a_{i,t}) = \max_{l_{i,t}, h_{i,t}, a_{i,t+1}} \nu_i(c_{i,t}) - \phi(l_{i,t}) - \psi_i(h_{i,t}) + \delta \cdot \left[h_{i,t} \cdot V_{i,t+1}(a_{i,t+1}) + (1 - h_{i,t}) \cdot U_{i,t+1}(a_{i,t+1}) \right]$$

The function $\phi(l_{i,t})$ could also capture other (costly) income-generating activities, such as added-worker effects, although it is natural to refer to informal work activities in our context. Note that this innovation compared to DellaVigna *et al.* (2017) and Ganong & Noel (2018) allows us to match the long-term consumption loss without the need to introduce heterogeneity in the sensitivity of consumption to cash-on-hand.

Other purposes for the lump-sum at layoff. We assume that our second set of moments in Figure 10b – the relative changes in non-durable expenditures – capture relative changes in consumption in our model. However, we have shown that displaced workers use part of their lump-sum amount at layoff to buy durables. Moreover, in focus groups conducted with displaced workers in São Paulo, participants mentioned using part of their lump-sum to repay debts. There may thus be a demand for lump-sum transfers in our context that is separate from any job displacement insurance purpose. This is also supported by findings from surveys that we conducted with formal employees in São Paulo. About 75% of respondents reported that they would prefer a system granting them access to their forced savings account every 3 years, irrespective of layoff history. This figure is larger than the share of respondents who would prefer having access to their forced savings account immediately (48%) or who would prefer their monthly forced savings to be added to their wage instead (43%). In practice, we thus allow the lump-sum amount to be a free parameter bounded between zero and the full statutory amount (473% of the monthly wage prior to layoff), such that we allow part of the lump-sum to serve purposes that are not captured by our model (e.g., repaying debts).

Estimation. We present the results from estimating three models below. In each case, we allow for two worker types in proportion s_1 and $(1 - s_1)$, respectively. We assume that they have different search costs, κ_1 and κ_2, but that the cost of informal work χ is the same for all workers. The vector of free parameters ξ thus includes: (i) the levels of search cost for the two worker types, κ_1 and κ_2, the share of the first type in our sample, s_1, and the inverse of the elasticity of search effort, γ; (ii) the cost of informal work, χ, and the inverse of the elasticity of informal work, λ; and (iii) the share of the statutory lump-sum amount used to finance consumption, ω. The first model allows for myopia by including the (monthly) discount factor δ in the vector of free parameters ξ, assuming away savings constraints or present bias by setting the interest rate at $r = 0$ and the present bias parameter (from beta-delta discounting) at $\beta = 1$. The second model allows for present bias by including β in the vector ξ, and setting the discount factor at $\delta = .995$ and the interest rate at $r = 0$. The third model considers instead the possible role of savings constraints by including r in the vector ξ, and setting the discount factor at $\delta = .995$ and the present bias parameter at $\beta = 1$.

For a given vector ξ, the model is solved by backwards induction (see DellaVigna *et al.* , 2017, for details). We then use indirect inference to choose the vector $\widehat{\xi}$ that minimizes the distance: $(m(\xi) - \widehat{m})' V (m(\xi) - \widehat{m})$, where the vector $\widehat{m}$ includes the empirical moments in Figure 10, the vector $m(\xi)$ includes these same moments as predicted by the model for a given vector ξ, and V is a weighing matrix (we use a diagonal matrix with the inverse of the variance of the empirical moments). The algorithm selects $\widehat{\xi}$ by randomly drawing a set of N vectors of initial values for the free parameters, by finding the best estimator $\widehat{\xi}_n$ for each vector (i.e., a local minimum), and by selecting the best estimator among those N local minima.

Identification. The parameters are estimated jointly but the features of our empirical moments that likely identify each of our parameters seem rather intuitive. The heterogeneity in search cost is necessary to

rationalize the downward sloping curve in the hazard rates of reemployment after UI exhaustion, and the hazard rates towards the end of the period identify the search cost of the higher cost type, κ_1. The inverse of the elasticity of search effort, γ, is identified by the change in the hazard rates around UI exhaustion. The level of the hazard rates in the first few months and the shape of the hazard rates when they decrease after UI exhaustion identify the share of higher cost types, s_1, and the search cost of the lower cost type κ_2.

The discount factor δ, the present bias parameter β, or the (negative) interest rate r, are identified by the increase in consumption at layoff (i.e., a patient worker with no savings constraint would save to insure herself against the risk of remaining non-formally-employed in later months) and the slope of the consumption profile in the following months. We illustrate this clearly below by showing the consumption patterns predicted by a "benchmark model" in which we assume away myopia, present bias, and savings constraints ($\delta = .995$, $\beta = 1$, and $r = 0$). The cost of informal work, χ, and the inverse of the elasticity of informal work, λ, are identified by the level of consumption after all job displacement insurance benefits have been exhausted and by the size of the drop in consumption at UI exhaustion. Finally, the share of the lump-sum amount used to finance consumption, ω, is identified by the height of the spike in consumption at layoff.

5.3 Main results

The results in Table 2 are based on 300 random draws for each model. Columns (1)-(3) display results for the myopia model ($\delta \in \xi$), the present bias model ($\beta \in \xi$), and the savings constraint model ($r \in \xi$), respectively. Figure 11 shows that the moments predicted by these models given the estimated parameters in Table 2 fit the empirical moments relatively well, especially for the myopia and savings constraints models.

In column (1), the estimated monthly discount factor is quite low, at $\widehat{\delta} = .82$, which is equivalent to a yearly discount factor of $.09$. Thus, a very high degree of impatience is necessary for a model with exponential discounting to rationalize the patterns in our data. Interestingly, the degree of impatience is not too far from the one estimated by DellaVigna *et al.* (2017) for their standard model, which is equivalent to a yearly discount factor of .17. Our estimate of the inverse of the elasticity of job search ($\widehat{\gamma} = .42$) is also quite similar to theirs ($\widehat{\gamma} = .35$). The model requires a high degree of heterogeneity in the other job search parameters to fit our data: the cost of search is 3.2 times higher for the higher-cost type than for the lower-cost type. Finally, we estimate that about 59% of the statutory lump-sum amount is used to finance consumption. A large share of the lump-sum is thus used for other purposes.

Column (2) shows that a model with beta-delta discounting can also rationalize the patterns in our data, and with values that are more in lines with the literature: the estimated present bias parameter is $\widehat{\beta} = .71$ (and we set $\delta = .995$). The goodness-of-fit is not as good as in column (1), however: for instance, the model predicts a sharper change in hazard rates at UI benefit exhaustion.

Column (3) shows that the model requires a severe savings constraint, with an interest rate of about $\widehat{r} = -16\%$, to rationalize the patterns in our data without myopia or present bias. As the model assumes that this negative interest rate is real (rather than perceived), it also requires that a larger share of the statutory lump-sum amount is devoted to consumption (i.e., enters our model) rather than used for other purposes.

The other interesting point to make for the results in column (3) relates to the job search parameters. Everything else equal, patient workers facing constraints to carry savings from one period to the next would search harder for a new formal job early on – compared to the impatient workers in the models of columns (1) and (2) – in order to self-insure against low consumption levels in the future. Therefore, to rationalize the same hazard rates of reemployment in the first months after layoff, and their increase around UI exhaustion, the model in column (3) requires both much higher search costs and a much higher search elasticity. In other

words, the workers value the gains from job-search to a greater extent than those in the models of columns (1) and (2) as they do not discount the future as heavily, but searching for a job is much more costly for them.

To make this point clearer, we show in the Appendix the predicted hazard rates of formal reemployment and the relative changes in consumption based on simulations of the *myopia* model in columns (1) of Table 2, but replacing the discount factor to $\delta = .995$. We thus force the workers to be more patient but we do not change the other estimated parameters, e.g., we keep search costs identical across workers. In so doing, we dramatically over-estimate hazard rates of formal reemployment. In that model, displaced workers thus believe that they will be reemployed much faster. Yet, even with such beliefs, the model does not predict that their consumption would spike at layoff. This indicates that having high beliefs regarding reemployment probabilities does not easily generate the consumption patterns in our data.

5.4 Counterfactual policies

We can use the models estimated above to illustrate the possible impact of alternative job displacement insurance designs. Figure 12 displays the hazard rates of formal reemployment (left panels) and the relative changes in consumption (right panels) based on simulations of the models in columns (1)-(3) of Table 2 under three alternative job displacement insurance policies (the solid black lines replicate the solid black lines in Figure 11, and thus correspond to the current system). Patterns are comparable across models.

First, we consider a policy in which displaced workers are not eligible for any UI benefits (dotted black lines in the graphs); they only receive the lump-sum amount at layoff. We find that consumption would still spike by about 30% at layoff. This indicates that an increase in consumption at layoff when workers are eligible for SP is not necessarily limited to settings where workers are also eligible for UI benefits.

Second, we consider a policy that reduces the potential UI duration to four months (blue lines in the graphs), mimicking the variation in the RD analysis in Section 4.4. We find that consumption would be identical in all months except in month 5 after layoff, when workers eligible for 5 months of UI would draw their additional UI payment, but workers eligible for a shorter potential UI duration would increase search efforts even prior to UI exhaustion. These results indicate that our estimated model is consistent with the findings of our RD analysis and with the evidence of anticipation effects in job-search found in other studies.

Third, we consider a budget-neutral policy in which workers receive the UI benefits drawn by the average worker in the sample (i.e., with the average UI duration) in a lump-sum fashion at layoff, on top of the SP amount of the current system (red lines in the graphs). The left panels show that search efforts would be higher than in the current system in the first few months given the absence of substitution effects (they would be initially lower than with the "no UI benefits" policy because of income effects). However, the right panels highlight the trade-off with such a policy when consumption is highly sensitive to the timing of payments: consumption would be higher in the first few months, when workers are already consuming more than before layoff in the current system, but would be much lower in months 3 to 5 after layoff. This highlights the important role of the disbursement policy of UI programs when consumption is highly sensitive to the timing of payments. It also illustrates why a majority of respondents in our survey of UI applicants may say that they would not want to receive all their UI benefits in a lump-sum fashion at layoff despite the clear financial advantage, and mention the need "to control expenditures" or "to not spend it all at once" to explain their choice. This also suggests that the degree of impatience that we estimate may not represent workers' "optimal" preferences, i.e., the "normative" preferences to use for a welfare analysis.

The tradeoff between SP and UI would be very different in a model where consumption is not highly sensitive to the timing of payments. To illustrate this point, we estimated a "benchmark model" in which

we assume away myopia, present bias, and savings constraints ($\delta = .995$, $\beta = 1$, and $r = 0$). To maintain the same financial resources, we also set the share of the statutory lump-sum amount used for consumption at its level in column (1) in Table 2 (we are thus left with six free parameters). The moments predicted by this model under the same counterfactual policies are displayed in Figure 13. Under the current system, the estimated model is able to match the shape of the hazard rates of formal reemployment.[55] However, it fails to match the consumption profile: displaced workers are predicted to smooth their consumption after layoff. The tradeoff between SP and UI, which is only due to their different contingent policy in this model, would differ substantially. For the counterfactual policy that pays all job displacement insurance benefits in a lump-sum fashion at layoff, for instance, the changes in consumption patterns now appear much less dramatic than the changes in reemployment rates compared to the simulation results in Figure 12.

Given the findings of this paper, another interesting counterfactual policy to consider is an unconditional transfer paid in installments, i.e., *irrespective* of displaced workers' reemployment status. Figure 14 displays the predicted moments based on simulations of the models in columns (1)-(3) of Table 2 under a budget-neutral policy in which workers receive the same lump-sum amount and UI benefits as the average worker in the sample in 9 monthly installments unconditionally (the patterns are again comparable across models). In our context, such a scheme would both reduce incentive effects and improve consumption smoothing compared to the current system, to the extent that the higher consumption levels in the first few months after layoff do not represent workers "optimal" consumption choices. Note that such a policy would not provide as much income support for workers who remain without a formal job in a setting where hazard rates of formal reemployment are higher: if workers were reemployed faster, the UI benefits drawn by the average worker would be relatively lower, and the monthly installments would be relatively smaller.

5.5 Marginal reforms and normative implications

Finally, we follow a main strand in the literature and consider marginal reforms around the current system. This allows us to evaluate the incentive-insurance tradeoffs using the type of sufficient statistics formulas derived in Section 1.2, and thus to illustrate normative implications of our findings.

Specifically, we consider four reforms that were designed such that their mechanical effect (i.e., their budgetary cost in absence of behavioral responses) would be identical: a one-month increase in the potential UI duration; an increase in the UI benefit level; an increase in the lump-sum amount at layoff; and an unconditional transfer paying the same increase in 6 monthly installments (months 0-5), irrespective of workers' reemployment status. We simulate these reforms for the benchmark model, which assumes away myopia, present bias, and savings constraints, and for the models in columns (1)-(3) of Table 2.

Table 3 presents our results. Column (1) captures the insurance value of the reforms, i.e., the welfare effect from the consumption-smoothing gains. Column (2) captures the incentive effect of the reforms, i.e., the welfare effect from distorting job-search incentives. Column (3) displays the sum of columns (1) and (2). All the values are in \$1 per \$1 spent on mechanical beneficiaries (those who would benefit from these reforms in absence of behavioral responses); a negative value means a decrease in welfare. The incentive effect is calculated in the same way as in the formulas in Section 1.2, by the ratio of the behavioral effect – the increase in budgetary cost due to behavioral responses – to the mechanical effect of the reforms. In contrast, we must slightly deviate from these formulas when evaluating the consumption-smoothing gains of the reforms. This is because we assume that normative preferences are free of myopia or present bias, and so we cannot assume that increases in consumption or savings in any period have the same impact on welfare.

[55] As discussed above, the model requires much higher search costs and a much higher search elasticity to do so.

In particular, we must take into account *when* displaced workers actually change their consumption.[56] Note that the insurance value is systematically low in Table 3, in parts because we use a relatively low coefficient of relative risk aversion (it is equal to 1 with log utility); it will increase (in absolute values) once we replicate our results with utility functions that feature higher coefficients of relative risk aversion.

The top panel in Table 3 displays results for the benchmark model. This allows us to illustrate the incentive-insurance tradeoff when the disbursement policy is essentially irrelevant as consumption is not very sensitive to the timing of payments in this model. Accordingly, the insurance value is similar for the increases in the lump-sum and in the unconditional transfer. It is higher for the increases in UI benefits, because of the difference in contingency policy, i.e., UI benefits are better targeted. However, the insurance values are comparable across reforms because workers are able to smooth consumption after layoff in this model (see the solid black line in Figure 13b). The incentive effects are worse for the increases in UI benefits, again because of the difference in contingency policy. As a result, the welfare effects are lower for the increases in UI benefits, and even negative for the coefficient of relative risk aversion used in this exercise.

There are four key differences to highlight for the results based on the models in columns (1)-(3) in Table 2 (they are qualitatively similar across the three models). First, the insurance value decreases substantially and become negative for the increase in the lump-sum amount of layoff. This is because consumption only increases in the first few periods after layoff in this case, when consumption was already much higher than before layoff. Second, the insurance value decreases to a lesser extent for the increase in the unconditional transfer. This is because the changes in consumption are not as concentrated in the first few months after layoff with this reform, illustrating the key role of the disbursement policy when consumption is very sensitive to the timing of payment. Third, the decrease in the insurance value is comparable for the increases in the UI benefit level and in the unconditional transfer because the additional resources are disbursed over the same period, but the insurance value is still higher with the former reform because of the difference in contingency policy. Fourth, the insurance value in contrast increases for the increase in the potential UI duration because the increase in consumption in that case is concentrated in a period when workers were consuming much less than before layoff (i.e., the period following UI exhaustion).

The incentive effect of the reforms remains in line with results for the benchmark model (although with some variation across models). As a result, the changes in the welfare effect of the reforms in column (3), compared to the benchmark model, follow from these four points. For instance, the welfare effect decreases substantially for the increase in the lump-sum and becomes systematically lower than for the increase in the unconditional transfer, illustrating again the key role of the disbursement policy. In one model, the welfare effect of the increase in the lump-sum becomes even smaller than the welfare effect of the increase in potential UI duration, despite the sizable incentive effect in the latter case.

6 Conclusion

Our results lead to three main conclusions. First, consumption is highly sensitive to cash-on-hand, even in a context where a windfall of cash is accompanied by a large negative shock, such as the loss of a formal job. We find that displaced workers increase consumption at layoff by about 35% despite experiencing a long-term consumption loss of about 17% when they stop receiving any benefits; they spend 20% more in the week they receive their monthly UI paycheck; and workers who are eligible for one additional month of UI

[56] We note that, when assuming that normative and positive preferences differ, the overall welfare effect of these reforms likely includes a third component that is difficult to evaluate: the impact on possible *internalities*, as other workers' choices may not be privately optimal from a normative point of view.

consume significantly more only in the period during which they draw the additional UI payment. These results imply that the disbursement policy of job displacement insurance schemes may affect their insurance value to workers. In particular, the trenched disbursement of UI benefits may be particularly important in helping displaced workers better smooth consumption compared to a SP program. The importance of this difference in disbursement policy has been largely overlooked in the debate between UI and SP, which focuses on their different contingency policy and its implications for moral hazard and targeting.

Importantly, our results are not driven by liquidity constraints as we find that workers fail to smooth consumption in anticipation of the (expected) drop in income at UI exhaustion, which is associated with a 10% drop in consumption. These results on their own contribute to the literature by complementing the findings in Ganong & Noel (2018) who also document a sharp drop in spending at UI exhaustion in the US. Overall, a high degree of impatience appears necessary to rationalize the patterns in our data, which is in line with the recent literature (e.g., DellaVigna *et al.* 2017, Ganong & Noel 2018).

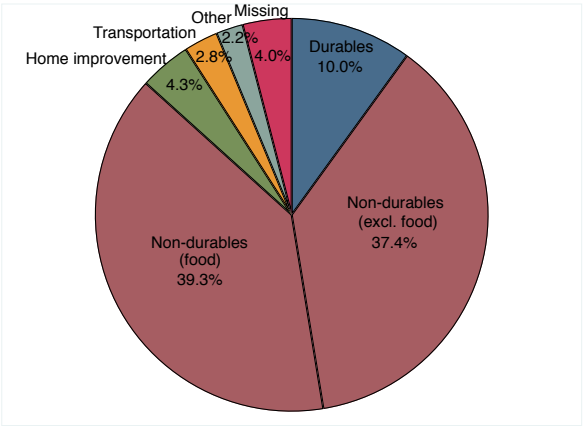
Second, our results indicate that researchers using a consumption approach to evaluate the insurance value of job displacement insurance schemes should carefully consider their choice of time aggregation, as yearly consumption flows can mask substantial non-smooth patterns at a monthly (or weekly) level. Moreover, if such behavior can be interpreted as excessive impatience from a normative perspective, it presents challenges to alternative revealed preference approaches used in the literature (Chetty, 2008).

Third, the long-term consumption loss in our setting is comparable to estimates from other studies and indicates that there is a need for job displacement insurance despite the higher labor market informality. This is important because the need for job displacement insurance is not obvious ex-ante in our context: government-mandated schemes can only insure formal workers, and the informality margin can, in principle, be used for self-insurance. Heterogeneous results indicate that informal jobs may be better substitutes for formal jobs for younger and lower educated workers, suggesting that the contingency policy of UI may be less relevant for these workers, although its disbursement policy may still help consumption smoothing.

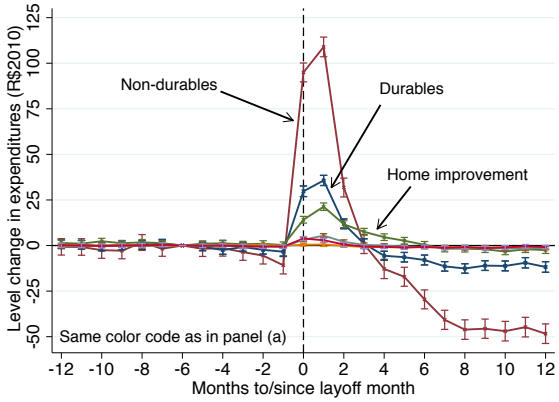
Finally, our findings shed light on three directions for future research. In terms of consumption patterns, if detailed data on the specific products purchased by workers could be used, it would be interesting to understand the quantity vs. quality gradient of such consumption changes. In terms of job displacement insurance design, it would be useful to build evidence on the impact of alternative disbursement schemes beyond UI and SP, such as the unconditional transfer paid in installments that we simulate in our models. Finally, in a developing country context, it would be interesting to gather panel data on total employment, including informal jobs, and consumption to better understand how informal job opportunities are used as self-insurance mechanisms after the loss of a formal job.

FIGURE 5: HETEROGENEITY BY EXPENDITURE CATEGORY

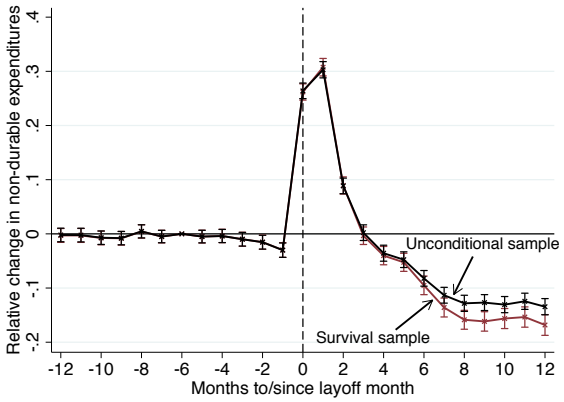
(a) Composition of expenditures (treatment)



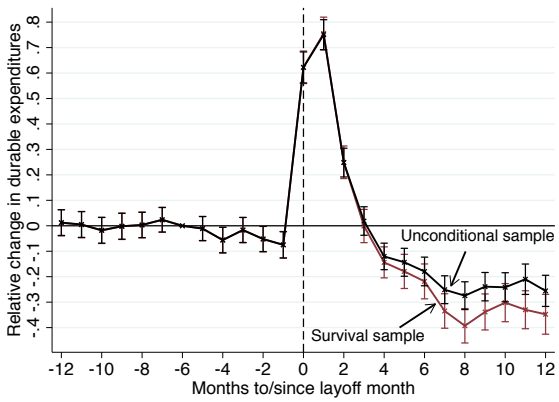
(b) DD estimates by category (level changes; unconditional sample)



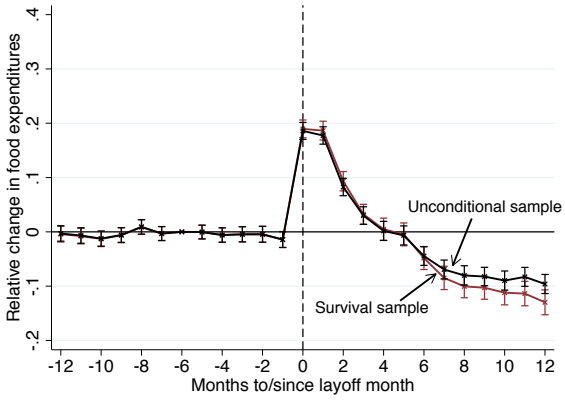
(c) DD estimates for non-durable expenditures



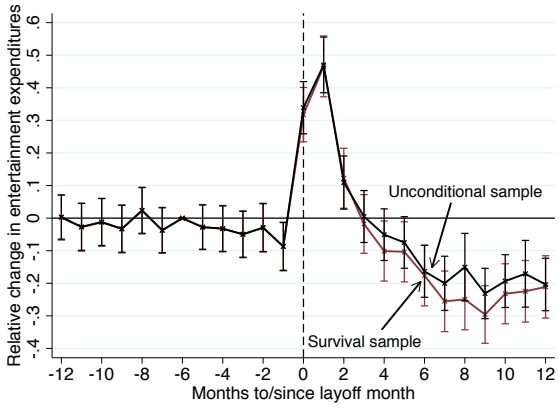
(d) DD estimates for durable expenditures



(e) DD estimates for food expenditures



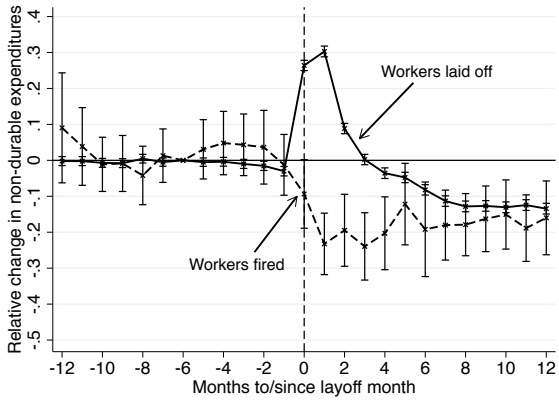
(f) DD estimates for entertainment expenditures



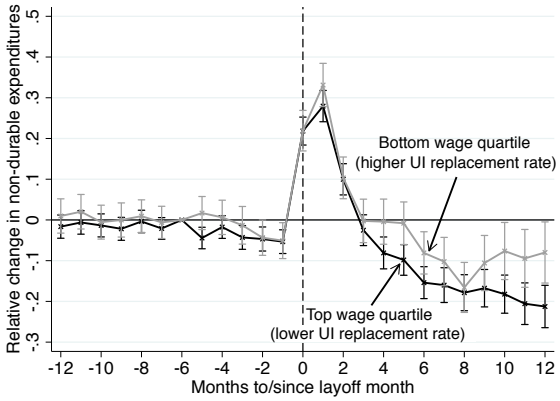
The figure presents results for different expenditure categories. Panel (a) displays the composition of expenditures before layoff (average over $k = -12$ to $k = -6$) in the treatment group. The main categories are food (mostly groceries; about 39%), other non-durables (mostly personal goods; about 37%), and durables (about 10%). The composition of expenditures is very similar in the control group (see Appendix). Panel (b) presents difference-in-differences results in levels (point estimates and 95% confidence intervals) using the specification in equation (8) for each expenditure category, separately, and the unconditional sample. It shows that non-durables are driving most of the increase in expenditures at layoff. Panels (c)-(f) present difference-in-differences results in relative changes as in Figure 4 for non-durable (food and other non-durable expenditures together), durable, food, and entertainment expenditures, separately.

FIGURE 6: LINKING OUR RESULTS TO VARIATION IN JOB DISPLACEMENT INSURANCE BENEFITS

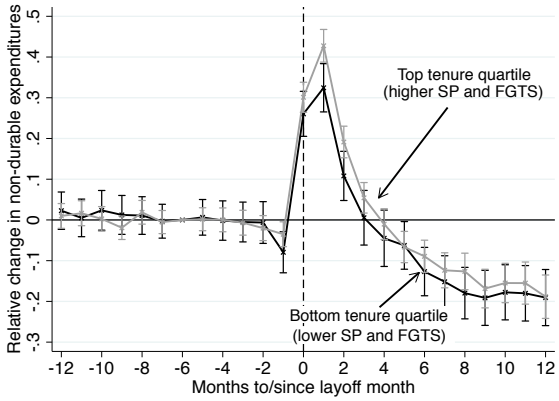
(a) Comparing DD estimates for laid-off and fired workers (unconditional sample)



(b) Using variation in UI replacement rates from variation in wages (controlling for tenure; survival sample)



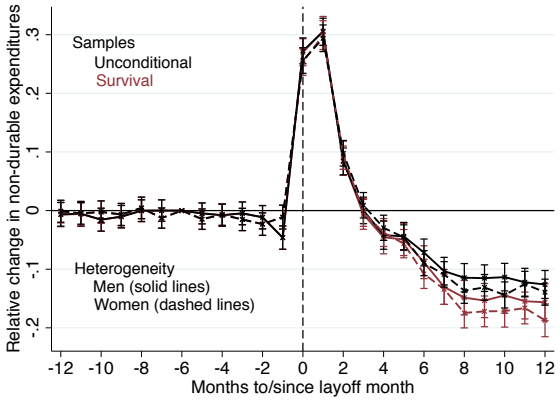
(c) Using variation in SP and FGTS from variation in tenure (controlling for wages; survival sample)



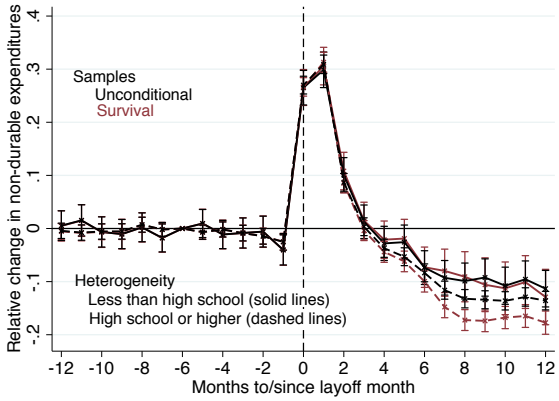
The figure present results for non-durable expenditures linking the patterns in previous figures to job displacement insurance benefits. Panel (a) displays difference-in-differences results (point estimates and 95% confidence intervals) using the specification in equation (8) for workers who were laid off and workers who were fired for cause, separately. The latter sample was not eligible for any job displacement insurance benefits at separation. Panel (b) displays difference-in-differences results comparing workers in the top and bottom quartiles of the wage distribution at layoff. The UI replacement rate is a decreasing function of the wage prior to layoff and was thus much lower in top than in the bottom quartile (41% and 80% on average, respectively). We use the specification in equation (9), controlling flexibly for tenure levels at layoff to net out variations in the replacement rate from the total SP and FGTS amount. Panel (c) displays difference-in-differences results comparing workers in the top and bottom quartiles of the tenure distribution at layoff. The replacement rate from the total SP and FGTS amount is an increasing function of the tenure prior to layoff: on average, the total SP and FGTS amount reached 428% and 670% of the pre-layoff wage in the bottom and top tenure quartiles, respectively. We use a similar specification as in equation (9), defining quartiles based on the tenure distribution and controlling flexibly for wages to net out variations in UI replacement rates. In both panels (b) and (c), the samples are restricted to workers who had more than 24 months of tenure at layoff, such that they were all eligible for 5 months of potential UI duration (see text for more details).

FIGURE 7: HETEROGENEITY BY WORKER CHARACTERISTICS

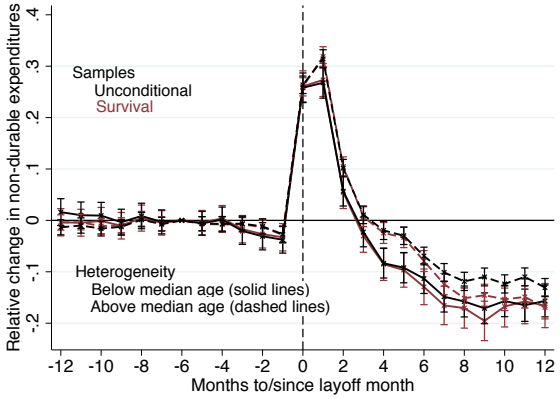
(a) Female vs. male workers



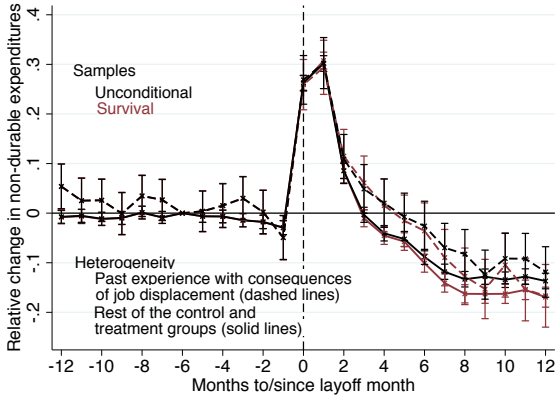
(b) Workers with at least vs. less than a high school degree



(c) Workers with more vs. less than the median age



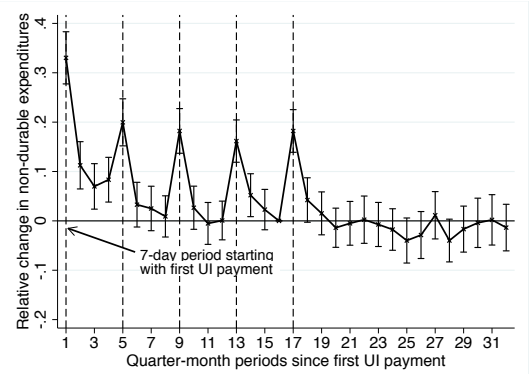
(d) Workers with past experience with the consequences of job displacement vs. workers in the rest of the treatment group



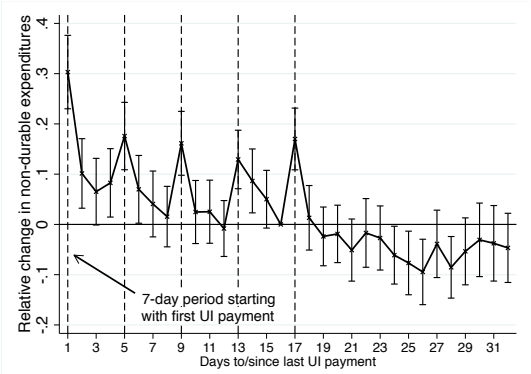
The figure presents some heterogeneity by worker characteristics. Each line in each panel displays difference-in-differences results (point estimates and 95% confidence intervals) for non-durable expenditures based on a separate regression that uses the specification in equation (8), and re-weight the respective subsample such that it compares better to the overall treatment group following the same strategy as in previous figures. Panel (a) compares female and male workers; panel (b) and (c) compare workers with less than vs. at least a high school degree and workers with less vs. more than the median age in the treatment group, respectively. Panel (d) displays results for workers who experienced another layoff from a formal job in the previous 5 years, had at least 12 months of tenure at that time, and remained at least 9 months without a formal job then (but less than 12 months to avoid including workers who transitioned into the informal sector) vs. for workers who constitute the rest of the treatment group.

FIGURE 8: NON-DURABLE EXPENDITURE PROFILE AROUND UI PAYDAY AND UI EXHAUSTION EVENTS

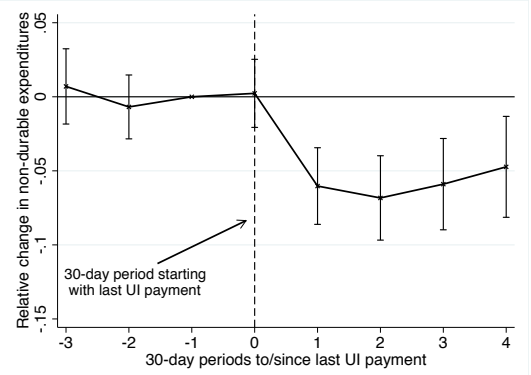
(a) Around UI payday within a month (UI exhaustees)



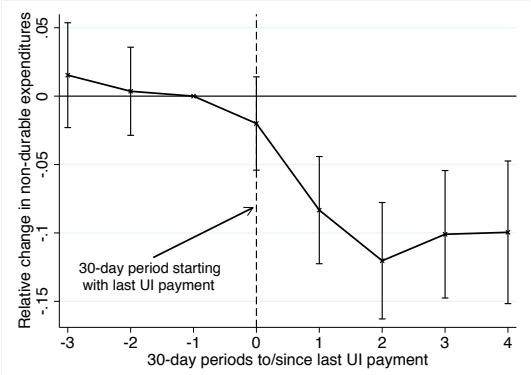
(b) Around UI payday within a month (UI exhaustees remaining without a formal job)



(c) Around UI exhaustion month (UI exhaustees)



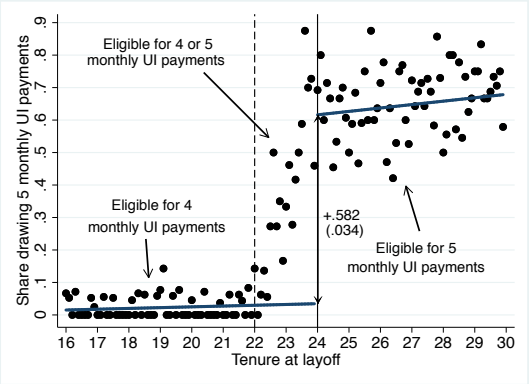
(d) Around UI exhaustion month (UI exhaustees remaining without a formal job)



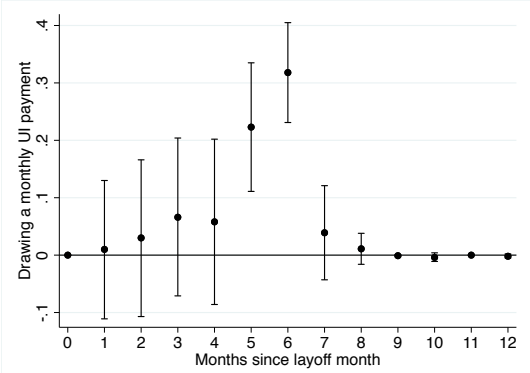
The figure presents the results (point estimates and 95% confidence intervals) of event analyses centered around UI payment or UI exhaustion dates in relative changes, based on the specification in equation (10). We use the subset of layoffs in our treatment group for which workers were eligible for 5 months of UI and were observed drawing 5 monthly UI payment. We use again our control group to net out overall trends in the data (see text for details). Panel (a) investigates how non-durable expenditures change around UI payment dates within a month. The window of analysis starts at the first UI payment date. We then divide the time between two payment dates into four quarter-month periods: three 7-day periods spanning the first 21 days since a UI payment (the first period includes the payment date) and a fourth period including the remaining days until the next UI payment (vertical lines indicate 7-day periods starting with a UI payment date). To investigate how expenditure levels evolve after UI exhaustion in the same analysis, we construct comparable time periods after the last UI payment (see text for details). Panel (b) uses the same sample but aggregate the data by 30-day periods centered around UI exhaustion (the vertical line indicates the 30-day period starting with the last UI payment date). Panel (c) presents the results of a similar analysis in which the sample is restricted to workers who remain without a formal job for at least 150 days after the last UI payment date (the end of our analysis window).

FIGURE 9: REGRESSION DISCONTINUITY RESULTS

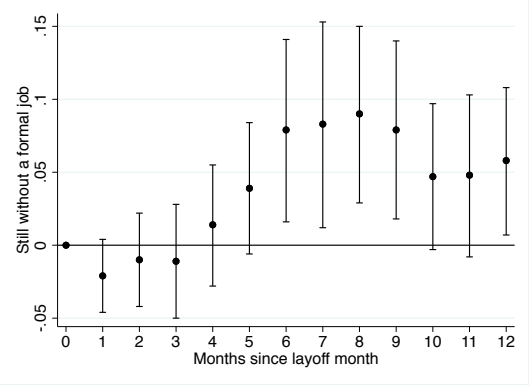
(a) Share drawing 5 monthly UI payments, raw data



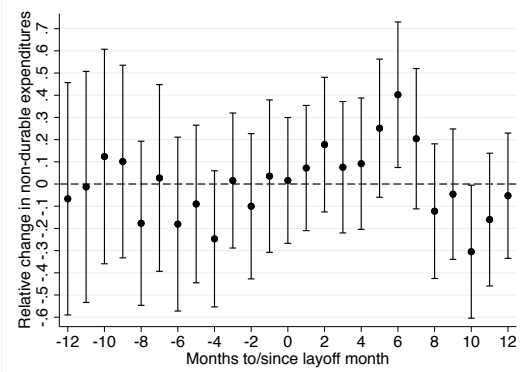
(b) Share drawing UI in each month, RD estimates



(c) Survival in non-formal-employment, RD estimates



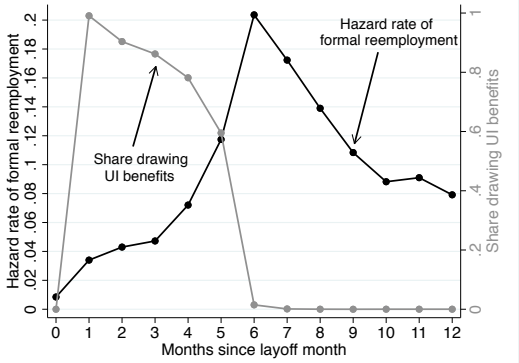
(d) Non-durable expenditures, RD estimates at the median



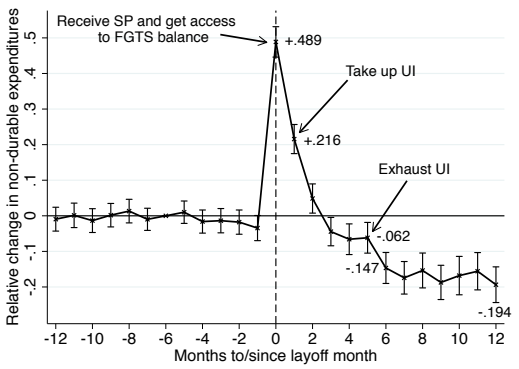
The figure presents results for the RD analysis around the 24-month cutoff; workers with less than 22 months (resp. more than 24 months) of tenure at layoff were eligible for four (resp. five) monthly UI payments (workers with 22 to 24 months of tenure were eligible for four or five monthly UI payments). Panel (a) plots the share of workers who drew five monthly UI payments by tenure levels. The lines in the figure illustrate our empirical strategy based on the specification in equation (11). The line on the left of the cutoff (resp. right of the cutoff) is estimated using observations in a bandwidth of six months below 22 months (resp. above 24 months) of tenure. The treatment effect (reported with its standard error) is the difference between the two lines at the cutoff. Panels (b)-(d) display RD estimates (with their 95% confidence intervals) from using the specification in equation (11) in each month to/since layoff, separately, for various outcomes in that month: panel (b) uses a dummy for drawing UI benefits, panel (c) uses a dummy for remaining without a formal job, and panel (d) shows RD estimates for non-durable expenditures for the median.

FIGURE 10: TARGET EMPIRICAL MOMENTS AND MODEL FIT

(a) Target empirical moment: hazard of formal reemployment (workers taking up UI in month 1 after layoff; also show share drawing UI)



(b) Target empirical moment: DD estimates for non-durable expenditures (workers taking up UI in month 1 after layoff, survival sample)



The figure displays the moments used in our structural estimations. For simplicity, our estimation sample is restricted to displaced workers eligible for five monthly UI payments, and who took up UI in their first month of eligibility, such that we align the months to/since both the layoff event and the UI exhaustion event. Panel (a) displays the share drawing UI benefits and the hazard rate of formal reemployment in each month since layoff. The layoff still takes place in month 0, but UI takeup and UI exhaustion now take place in month 1 and month 5, respectively, for all workers in this sample. The hazard rate of formal reemployment now increases more sharply and peaks in month 6, after all workers exhausted their UI benefits. Panel (b) displays difference-in-differences estimates for non-durables expenditures, in which the treatment group is restricted to the survival subset of our estimation sample. The increase in consumption at layoff is sharper than in Figure 5c, but the cumulative increase over the first 3 months (months 0-2) is comparable. The pattern around UI exhaustion is also sharper than in Figure 5c, capturing the patterns in Figure 8d.

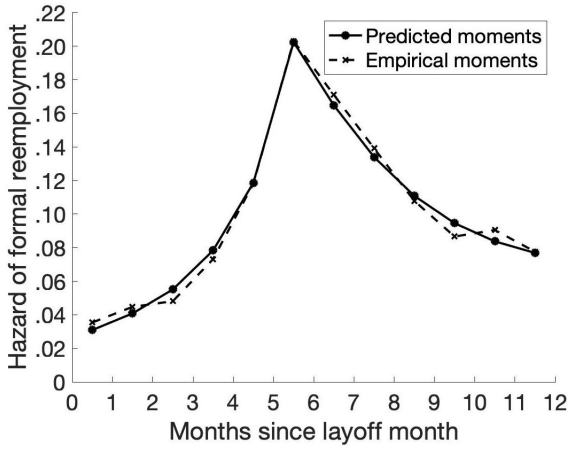
Table 2: Estimated parameters

	Free discount factor δ (β and r fixed)	Free present bias parameter β (δ and r fixed)	Free interest rate r (δ and β fixed)
	(1)	(2)	(3)
Parameters			
Discount factor (1 month): δ	0.820	0.995 (fixed)	0.995 (fixed)
Present bias: β	1 (fixed)	0.714	1 (fixed)
Inverse of search elasticity: γ	0.416	0.237	0.013
Search cost (higher-cost type): $\kappa 1$	6.091	10.541	96.104
Search cost (lower-cost type): $\kappa 2$	1.914	1.303	46.645
Share of higher-cost type: $s1$	0.482	0.540	0.414
Share of lump-sum amount for consumption: ω	0.592	0.502	0.701
Informal work cost: χ	0.536	0.544	0.536
Inverse of informal work elasticity: λ	0.152	0.250	0.131
Real interest rate: r	0 (fixed)	0 (fixed)	-0.163
Model fit			
Number of moments used	25	25	25
Number of estimated parameters	8	8	8
Goodness of fit	41.204	262.298	46.135

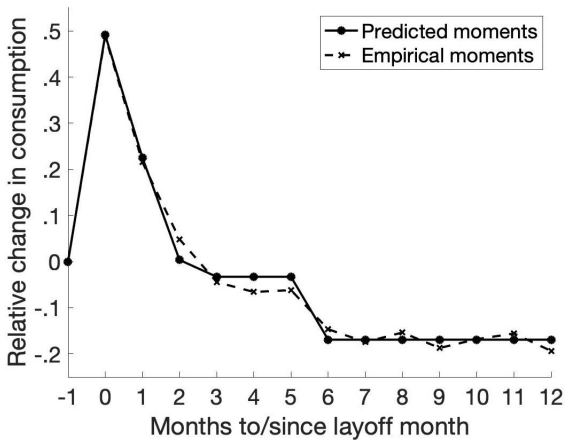
The table displays estimates for the three versions of the structural model described in Section 5.2. The parameters are estimated by minimizing the distance between the empirical moments displayed in Figure 10 – hazard rates of formal reemployment and relative changes in consumption – and the same moments as predicted by the models for a given vector of free parameters (standard errors will soon be added under each estimate in the table). Columns (1)-(3) display estimates for models that allow for the possibility of myopia (low discount factor), present bias (β-δ discounting) and savings constraints (negative interest rate), respectively.

FIGURE 11: PREDICTED MOMENTS BASED ON OUR ESTIMATED MODELS VS. EMPIRICAL MOMENTS

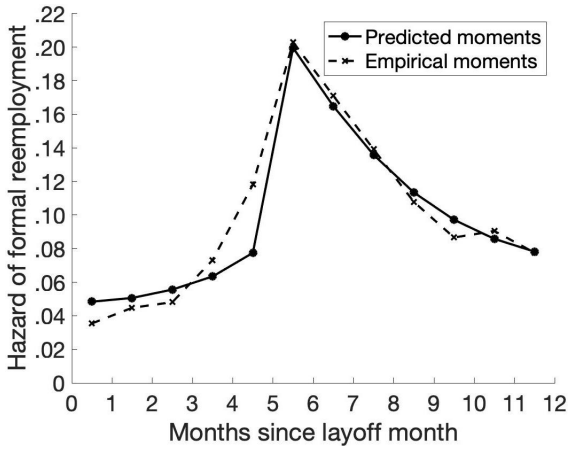
(a) Model 1 (myopia): hazard of formal reemployment



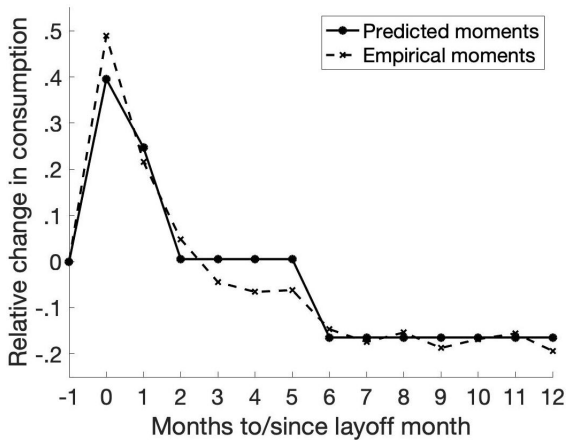
(b) Model 1 (myopia): relative changes in consumption



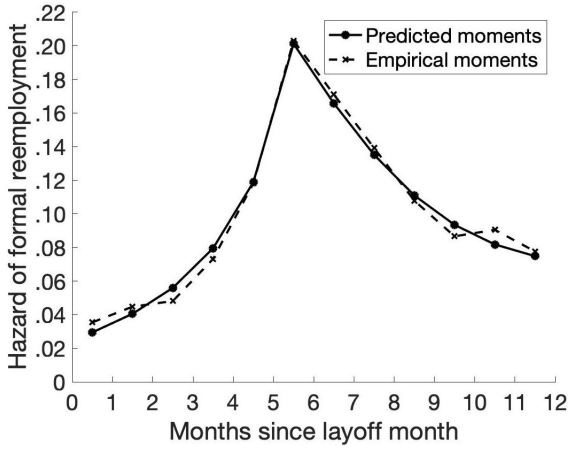
(c) Model 2 (present bias): hazard of formal reemployment



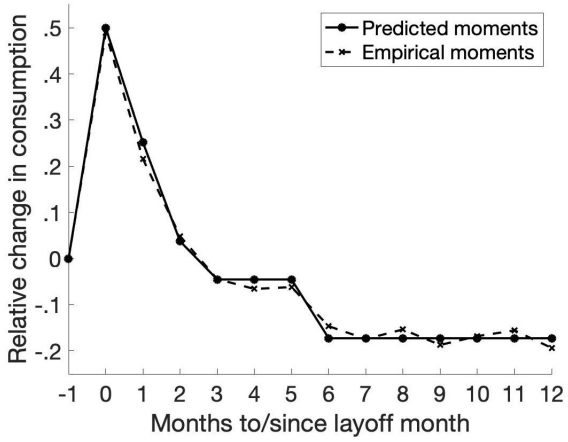
(d) Model 2 (present bias): relative changes in consumption



(e) Model 3 (sav. constr.): hazard of formal reemployment



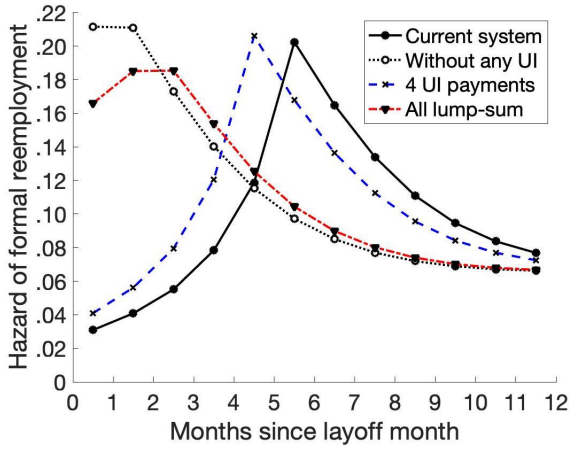
(f) Model 3 (sav. constr.): relative changes in consumption



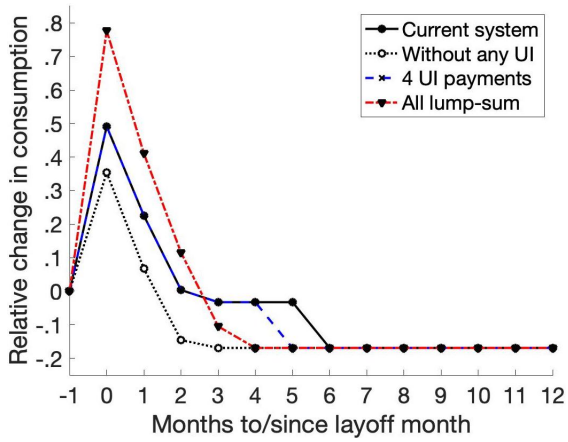
The figure compares the hazard rates of formal reemployment and the relative changes in consumption displayed in Figure 10 (i.e., empirical moments) to the same moments as predicted by the structural model, for our estimates of the vector of free parameters in Table 2 (i.e., predicted moments). The predicted moments for the models 1-3 are based on simulations of the model using the parameter values in columns (1)-(3) in Table 2, respectively. The figure shows that the predicted moments fit the empirical moments relatively well, particularly for the myopia and saving constraints (models 1 and 3).

FIGURE 12: COUNTERFACTUAL POLICIES

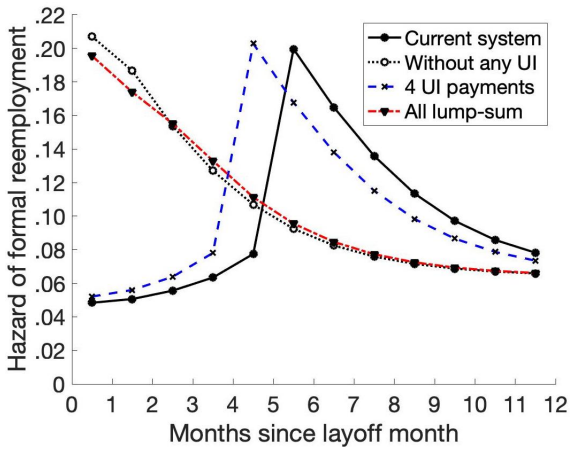
(a) Model 1 (myopia): hazard of formal reemployment



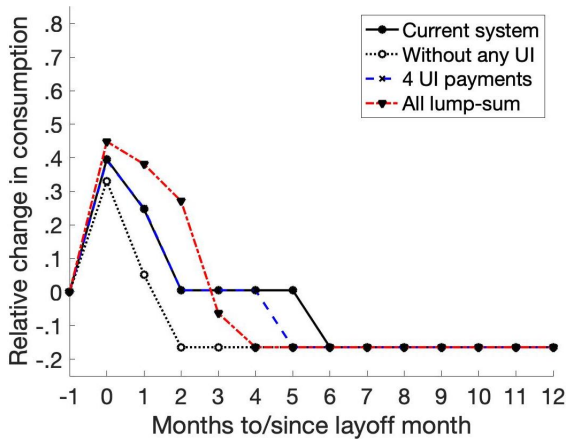
(b) Model 1 (myopia): relative changes in consumption



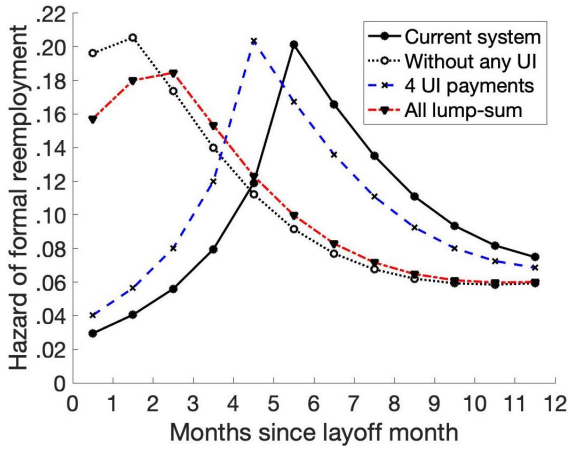
(c) Model 2 (present bias): hazard of formal reemployment



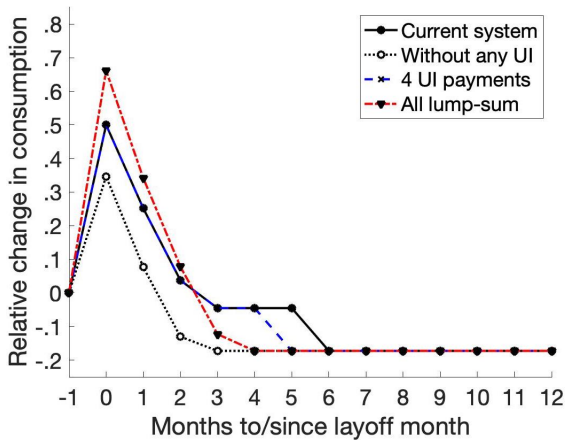
(d) Model 2 (present bias): relative changes in consumption



(e) Model 3 (sav. constr.): hazard of formal reemployment



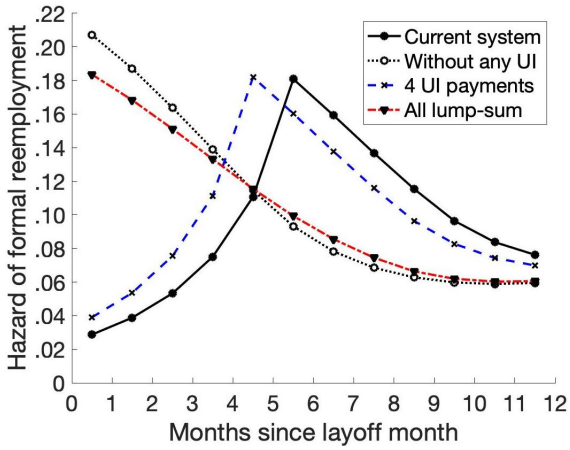
(f) Model 3 (sav. constr.): relative changes in consumption



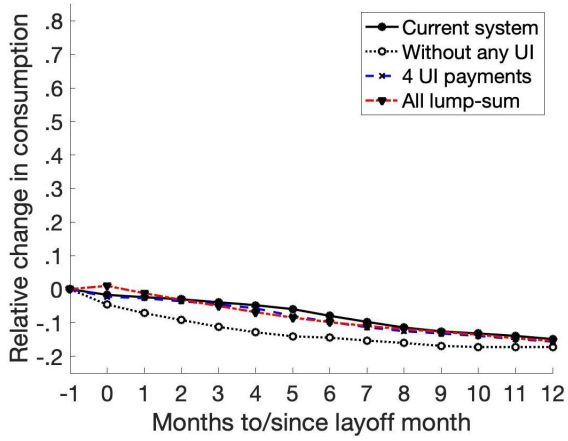
The figure displays the hazard rates of formal reemployment (panel a) and the relative changes in consumption (panel b) based on simulations of the models in column (1)-(3) of Table 2 under three alternative job displacement insurance policies (the solid black lines replicate the solid lines in Figure 11, and thus correspond to the current system). First, we consider a policy in which displaced workers are not eligible for any UI benefits (dotted black lines); they only receive the lump-sum amount at layoff. Second, we consider a policy that reduces the potential UI duration to four months (blue lines), mimicking the variation in the RD analysis in Section 4.4. Third, we consider a budget-neutral policy in which workers receive the UI benefits drawn by the average worker in the sample (i.e., with the average UI duration) in a lump-sum fashion at layoff, on top of the SP amount of the current system (red lines).

FIGURE 13: PREDICTIONS WITH BENCHMARK MODEL (NO MYOPIA, PRESENT BIAS, OR SAVINGS CONSTRAINT)

(a) Hazard of formal reemployment



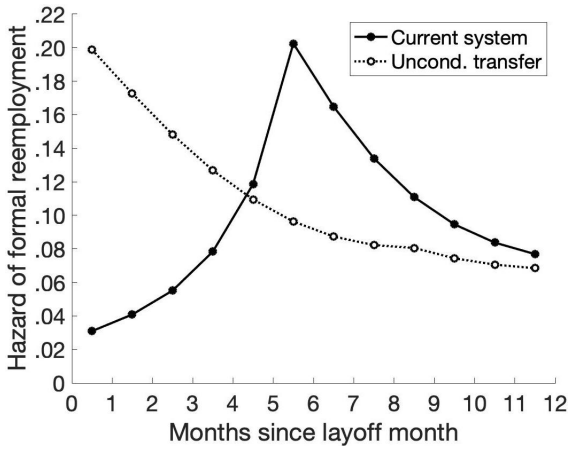
(b) Relative changes in consumption



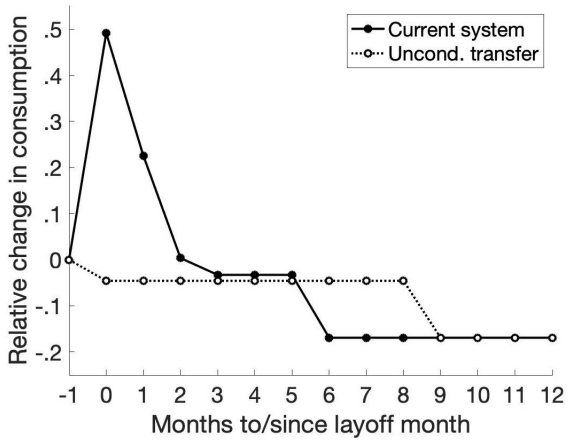
The figure displays the hazard rates of formal reemployment (panel a) and the relative changes in consumption (panel b) based on a model that assumes away myopia, present bias, and savings constraint. Specifically, we re-estimated the structural model setting $\delta = .995$, $\beta = 1$, and $r = 0$. To maintain the same financial resources, we also fixed the share of the statutory lump-sum amount used for consumption at its level in column (1) in Table 2 (we are thus left with six free parameters). The panels display the moments predicted by this model under the current system (black lines), which shows that such a model can match the reemployment patterns in our data, but not the consumption patterns. The panels also display the moments predicted by this model under the same three counterfactual policies as in Figure 12).

FIGURE 14: PREDICTED MOMENTS UNDER AN UNCONDITIONAL TRANSFER PAID IN INSTALLMENTS

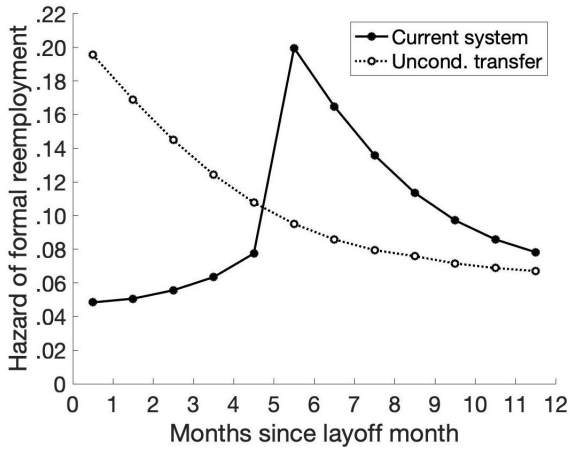
(a) Model 1 (myopia): hazard of formal reemployment



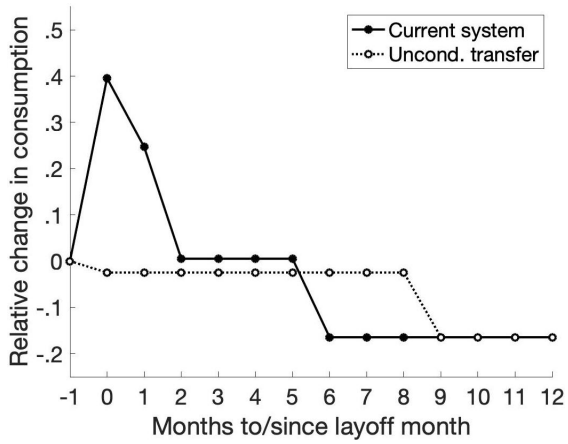
(b) Model 1 (myopia): relative changes in consumption



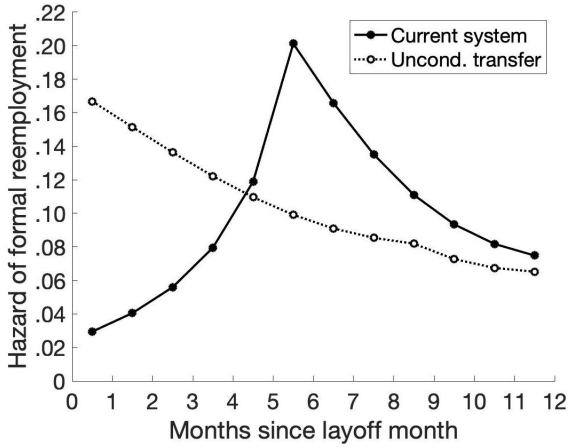
(c) Model 2 (present bias): hazard of formal reemployment



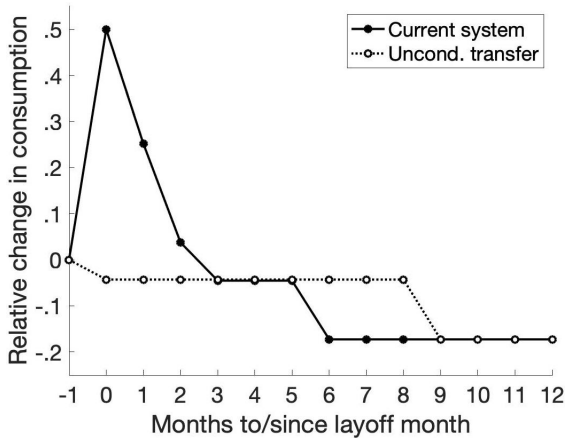
(d) Model 2 (present bias): relative changes in consumption



(e) Model 3 (sav. constr.): hazard of formal reemployment



(f) Model 3 (sav. constr.): relative changes in consumption



The figure compares the hazard rates of formal reemployment and the relative changes in consumption in our estimation sample (i.e., empirical moments) and as obtained from simulations of the models in columns (1)-(3) of Table 2 under an unconditional transfer paid in monthly installments. Specifically, we consider a budget-neutral policy in which workers receive the same lump-sum amount and UI benefits as the average worker in the sample in 9 monthly installments, *irrespective* of their reemployment status.

Table 3: Evaluating the incentive-insurance tradeoff of job displacement insurance reforms

	Insurance value	Incentive effect	Resulting effect on welfare
	(1)	(2)	(3)
Benchmark model: no myopia, present bias, or savings constraint			
Policy 1: marg. increase in potential UI duration	0.08	-0.49	-0.41
Policy 2: marg. increase in UI benefit level	0.06	-0.21	-0.15
Policy 3: marg. increase in lump-sum at layoff	0.04	-0.01	0.03
Policy 4: marg. increase in uncond. transfer paid in installments	0.04	-0.01	0.03
Model 1: myopia			
Policy 1: marg. increase in potential UI duration	0.17	-0.54	-0.37
Policy 2: marg. increase in UI benefit level	-0.14	-0.32	-0.46
Policy 3: marg. increase in lump-sum at layoff	-0.28	0.00	-0.28
Policy 4: marg. increase in uncond. transfer paid in installments	-0.16	0.00	-0.16
Model 2: present bias			
Policy 1: marg. increase in potential UI duration	0.16	-0.31	-0.15
Policy 2: marg. increase in UI benefit level	-0.09	-0.37	-0.46
Policy 3: marg. increase in lump-sum at layoff	-0.21	0.00	-0.21
Policy 4: marg. increase in uncond. transfer paid in installments	-0.12	0.01	-0.11
Model 3: savings constraint			
Policy 1: marg. increase in potential UI duration	0.18	-0.56	-0.38
Policy 2: marg. increase in UI benefit level	-0.15	-0.21	-0.36
Policy 3: marg. increase in lump-sum at layoff	-0.30	0.00	-0.30
Policy 4: marg. increase in uncond. transfer paid in installments	-0.17	0.00	-0.16

The table displays results from evaluating the incentive-insurance trade-off of job displacement insurance reforms in 4 versions of our model: a benchmark case that assumes away any myopia, present bias, or savings constraints as in Figure 13; and the models in columns (1)-(3) of Table 2. We consider 4 reforms that were designed such that their *mechanical effect* (their budgetary cost in absence of behavioral responses) would be identical: a one-month increase in the potential UI duration (policy 1), an increase in the UI benefit level (policy 2), an increase in the lump-sum amount at layoff (policy 3), and an unconditional transfer paying the same increase in the lump-sum amount in 6 installments (months 0-5), irrespective of reemployment status (policy 4). Column (1) captures the insurance value of the reforms, i.e., the welfare effect from the consumption-smoothing gains. It is relatively low because we are using a low coefficient of relative risk aversion for now (it is 1 with log utility). Column (2) captures the standard incentive effect of the reforms, i.e., the welfare effect from distorting job-search incentives (this is the usual *fiscal externality* measured by the ratio of the behavioral effect – the increase in budgetary cost due to behavioral responses – to the mechanical effect of the reforms). Column (3) displays the sum of columns (1) and (2). All the values are in $1 per $1 spent on mechanical beneficiaries (those who would benefit from these reforms in absence of behavioral responses); a negative value means a decrease in welfare.

References

ALMEIDA, RITA, & CARNEIRO, PEDRO. 2012. Enforcement of Labor Regulation and Informality. *American Economic Journal: Applied Economics*, **4(3)**, 64–89.

ATTANASIO, ORAZIO, & PISTAFERRI, LUIGI. 2016. Consumption Inequality. *Journal of Economic Perspectives*, **30(2)**, 3–28.

BAILY, MARTIN. 1978. Some Aspects of Optimal Unemployment Insurance. *Journal of Public Economics*, **10**, 379–402.

BAKER, SCOTT. forthcoming. Debt and the Response to Household Income Shocks: Validation and Application of Linked Financial Account Data. *Journal of Political Economy*.

BLANCHARD, OLIVIER, & TIROLE, JEAN. 2006. The Joint Design of Unemployment Insurance and Employment Protection: A First Pass. *Journal of the European Economic Association*, **6(1)**, 45–77.

CARD, DAVID, & LEVINE, PHILLIP. 2000. Extended Benefits and the Duration of UI Spells: Evidence from the New Jersey Extended Benefit Program. *Journal of Public Economics*, **78**, 107–138.

CARD, DAVID, CHETTY, RAJ, & WEBER, ANDREA. 2007. Cash–on–Hand and Competing Models of Intertemporal Behavior: New Evidence from the Labor Market. *Quarterly Journal of Economics*, **122(4)**, 1511–1560.

CARVALHO, CRISTIANO, CORBI, RAPHAEL, & NARITA, RENATA. 2018. Unintended consequences of unemployment insurance: Evidence from stricter eligibility criteria in Brazil. *Economics Letters*, **162(C)**, 157–161.

CASABURI, LORENZO, & MACCHIAVELLO, ROCCO. forthcoming. Demand and Supply of Infrequent Payments as a Commitment Device: Evidence from Kenya. *American Economic Review*.

CHETTY, RAJ. 2006. A General Formula for the Optimal Level of Social Insurance. *Journal of Public Economics*, **90**, 1879–1901.

CHETTY, RAJ. 2008. Moral Hazard versus Liquidity and Optimal Unemployment Insurance. *Journal of Political Economy*, **116(2)**, 173–234.

CHETTY, RAJ, & FINKELSTEIN, AMY. 2013. Social Insurance: Connecting Theory to Data. *Handbook of Public Economics*, **5**, 111–193.

CHETTY, RAJ, & LOONEY, ADAM. 2007. Income Risk and the Benefits of Social Insurance: Evidence from Indonesia and the United States. *Fiscal Policy and Management in East Asia. NBER East Asia Seminar on Economics 16 (eds. T. Ito and A. Rose): Chicago, University of Chicago Press.*

DELLAVIGNA, STEFANO, & PASERMAN, DANIELE. 2005. Job Search and Impatience. *Journal of Labor Economics*, **23(3)**, 527–588.

DELLAVIGNA, STEFANO, LINDNER, ATTILA, REIZER, BALAZS, & SCHMIEDER, JOHANNES. 2017. Reference-Dependent Job Search: Evidence from Hungary. *Quarterly Journal of Economics*, **132(4)**, 1969–2018.

GANONG, PETER, & NOEL, PASCAL. 2018. Consumer Spending During Unemployment: Positive and Normative Implications. *forthcoming American Economic Review*.

GERARD, FRANÇOIS, & GONZAGA, GUSTAVO. 2016. Informal Labor and the Efficiency Cost of Social Programs: Evidence from the Brazilian Unemployment Insurance Program. *NBER Working Paper 22608*.

GORDON, ROGER, & LI, WEI. 2009. Tax Structure in Developing Countries: Many Puzzles and a Possible Explanation. *Journal of Public Economics*, **93 (7-8)**, 855–866.

GRUBER, JON. 1997. The Consumption Smoothing Benefits of Unemployment Insurance. *American Economic Review*, **87 (1)**, 192–205.

HASTINGS, JUSTINE, & WASHINGTON, EBONYA. 2010. The First of the Month Effect: Consumer Behavior and Store Responses. *American Economic Journal: Economic Policy*, **2(2)**, 142–162.

HENDREN, NATHANIEL. 2017. Knowledge of Future Job Loss and Implications for Unemployment Insurance. *American Economic Review*, **107 (7)**, 1778–1823.

HOLZMANN, ROBERT, POUGET, YANN, VODOPIVEC, MILAN, & WEBER, MICHAEL. 2012. *Severance Pay Programs around the World: History, Rationale, Status, and Reforms*. Washington DC: The International Bank for Reconstruction and Development, The World Bank. Chap. 2, pages 17–120.

JAKIELA, PAMELA, & OZIER, OWEN. 2016. Does Africa Need a Rotten Kin Theorem? Experimental Evidence from Village Economies. *Review of Economic Studies*, **83(1)**, 231–268.

KOLSRUD, JONAS, LANDAIS, CAMILLE, NILSSON, PETER, & SPINNEWIJN, JOHANNES. 2018. The Optimal Timing of Unemployment Benefits: Theory and Evidence from Sweden. *American Economic Review, forthcoming*.

KUENG, LORENZ. 2018. Excess Sensitivity of High-Income Consumers. *Quarterly Journal of Economics, forthcoming*.

NARITOMI, JOANA. 2018. Consumers as Tax Auditors. *Mimeo, LSE*.

OLAFSSON, ARNA, & PAGEL, MICHAELA. 2016. The Liquid Hand-to-Mouth: Evidence from Personal Finance Management Software.

PARSONS, DONALD. 2016. Job Displacement Insurance: A Policy Typology. *IZA Working Paper*, **9865**.

PARSONS, DONALD. forthcoming. The Simple Analytics of Job Displacement Insurance. *The Journal of Risk and Insurance*.

PERRY, GUILLERMO, MALONEY, WILLAM, ARIAS, OMAR, FAJNZYLBER, PABLO, MASON, ANDREW, & SAAVEDRA-CHANDUVI, JAIME. 2007. Informality: Exit and Exclusion. *The World Bank, Washington DC*.

SCHMIEDER, JOHANNES, VON WACHTER, TILL, & BENDER, STEFAN. 2012. The Effects of Extended Unemployment Insurance Over the Business Cycle: Evidence from Regression Discontinuity Estimates over Twenty Years. *Quarterly Journal of Economics*, **127(2)**, 701–752.

SHAPIRO, JESSE. 2005. Is there a Daily Discount Rate? Evidence from the Food Stamp Nutrition Cycle. *Journal of Public Economics*, **89 (2-3)**, 303–325.

SPINNEWIJN, JOHANNES. 2015. Unemployed but optimistic: Optimal insurance design with biased beliefs. *Journal of the European Economic Association*, **13**(1), 130–167.

SQUIRES, MUNIR. 2018. Kinship Taxation as an Impediment to Growth: Experimental Evidence from Kenyan Microenterprises.

STEPHENS, MELVIN. 2001. The long-run consumption effects of earnings shocks. *Review of Economics and Statistics*, **83 (1)**, 28–36.

Appendix A

Full schedule of UI benefits in Brazil

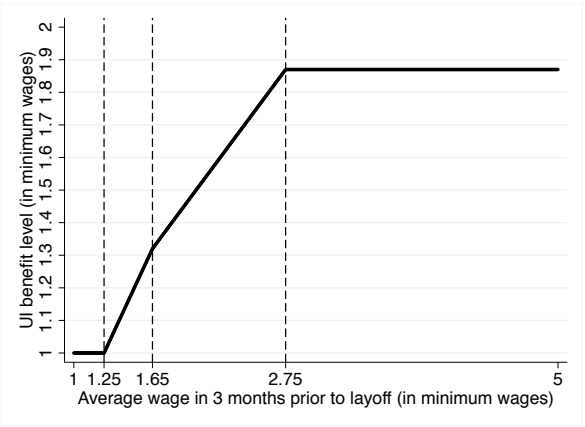
The UI benefit level depends on a displaced formal worker's average wage in the three months prior to layoff and ranges from 100% to 187% of the minimum wage. Define w the displaced formal worker's average nominal wage in the three months prior to layoff expressed in multiples of the prevailing minimum wage (mw). Her UI benefit level (b) is then calculated as follows:

- $b = mw$ if $w < 1.25$
- $b = .8\ w$ if $1.25 \leq w < 1.65$
- $b = 1.32\ mw + .5\ w$ if $1.65 \leq w < 2.75$
- $b = 1.87\ mw$ if $w \geq 2.75$

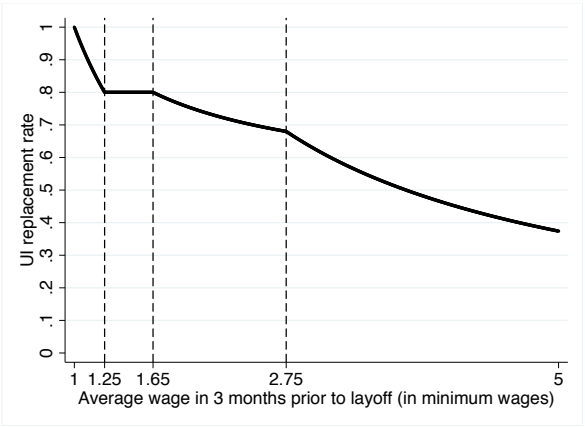
Figure A1 displays the relationship between w and b graphically, as well as the replacement rate (b/w).

FIGURE A1: UI BENEFIT LEVEL AND REPLACEMENT RATE SCHEDULE IN BRAZIL

(a) UI benefit level



(b) UI replacement rate



The panels display the UI benefit level and the UI replacement rate in the Brazilian UI program, which is a function of a displaced formal worker's average nominal wage in the three months prior to layoff, expressed in multiples of the prevailing minimum wage.

VAT receipt

We reproduce here a VAT receipt from São Paulo, in which the consumer provided her ID number.

FIGURE A2: VAT RECEIPT FROM SÃO PAULO

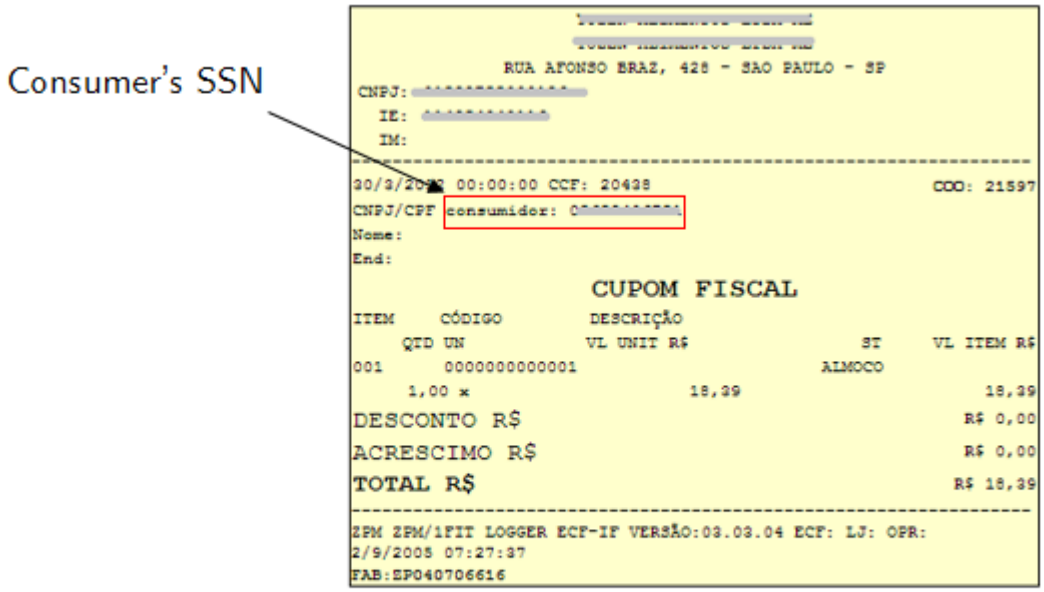
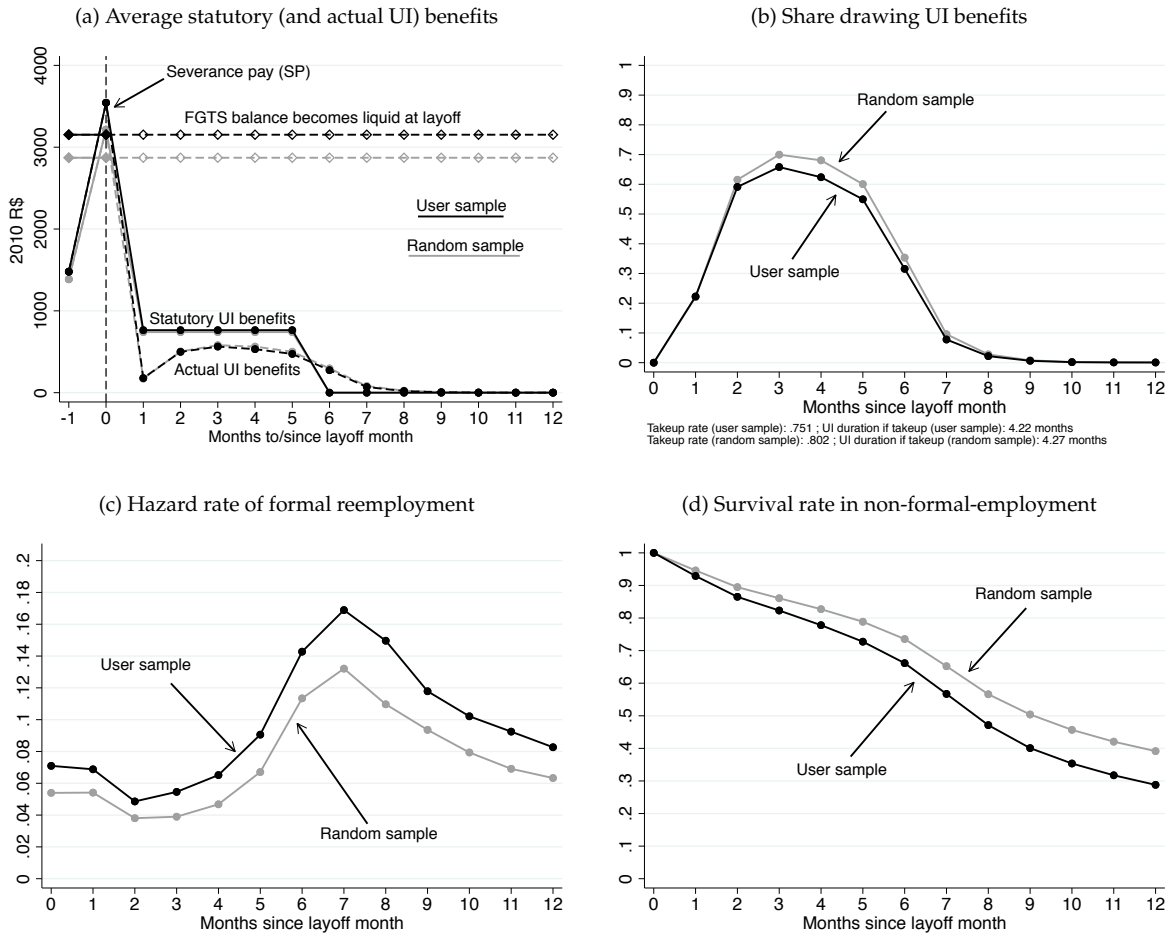


FIGURE A3: BACKGROUND AND REPRESENTATIVITY OF DISPLACED FORMAL WORKERS IN THE USER SAMPLE, 2011



The figure replicates Figure 2 for workers in the user sample laid off in 2011, the sample used for Figure 2b in the paper. Panel (a) also shows the difference between the average statutory UI benefits and the average UI benefits actually drawn by these workers. The difference comes from the facts that workers do not take up UI as soon as they become eligible and that UI takeup is imperfect.

FIGURE A4: EXPENDITURE PROFILE AROUND FORMAL REEMPLOYMENT EVENT

(a) DD estimates for total expenditures



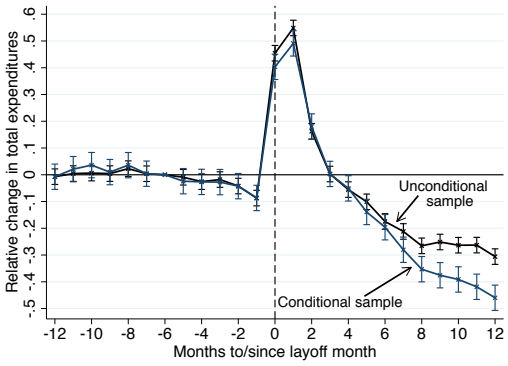
(b) DD estimates for non-durable expenditures



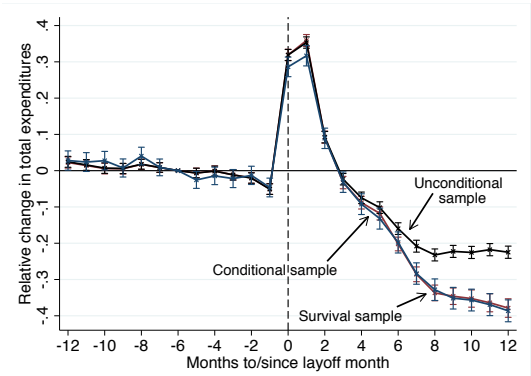
The figure presents difference-in-differences results for the change in expenditure levels around formal reemployment. We use laid off workers who were formally reemployed in months 0 to 10 after layoff as our treatment group. We use laid off workers who remained without a formal job 12 months after layoff as our control group. We randomly assign a "placebo" reemployment event in the months 0 to 10 after layoff to these workers, respecting the distribution of reemployment events across the months 0 to 10 in the treatment group. We then keep the observations in the 5-month window centered around the (placebo) formal reemployment event. The control group allows us to net out overall changes in expenditures – unrelated to formal reemployment – in the months after layoff. We then follow a similar specification as in equation (8): we regress monthly expenditures on worker fixed effects, event time fixed effects (here we have $k = -2, ..., 2$ and we use $k = -1$ as reference month), month fixed effects, and event time fixed effects interacted with a dummy variable for being in the treatment group. We plot the estimated coefficients on these interactions (point estimates and 95% confidence intervals) in relative changes, dividing our estimates by the mean in the treatment group in the reference month. All samples are also re-weighted such that they compare better to the overall sample of laid off workers (as in other empirical analyses; see Section 3.1 for more details), and we cluster standard errors by worker. Panels (a) and (b) display results for total expenditures and non-durable expenditures, respectively. In each panel, we present results from a regression in which we include all workers, and from a regression in which we only include the subset of workers formally reemployed in months 8-10 after layoff (when all job displacement insurance benefits have been exhausted), separately.

FIGURE A5: EXPENDITURE PROFILE AROUND DISPLACEMENT EVENT, ROBUSTNESS CHECKS

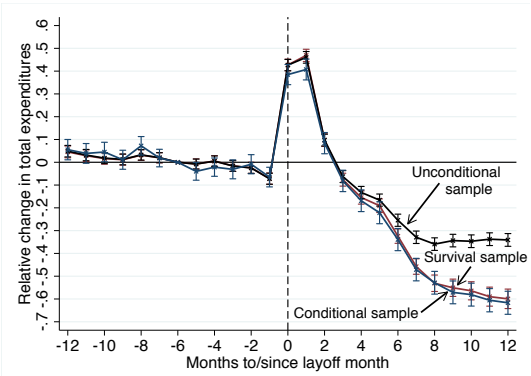
(a) DD estimates at the median



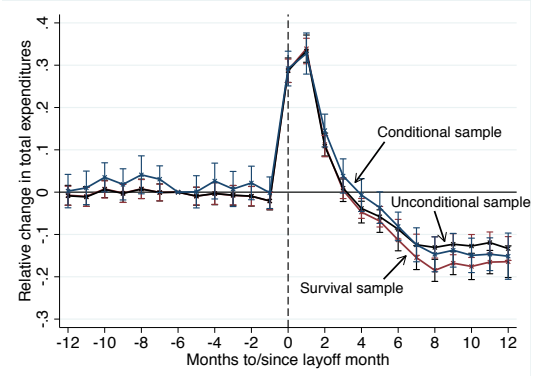
(b) DD estimates using log(total expenditures +10)



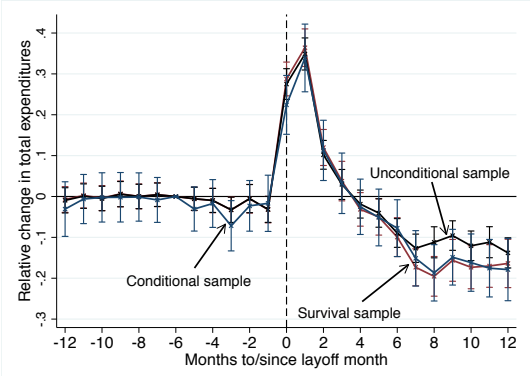
(c) DD estimates using inverse hyperbolic sine transformation



(d) DD estimates for users reporting positive purchases in all 12 months before layoff

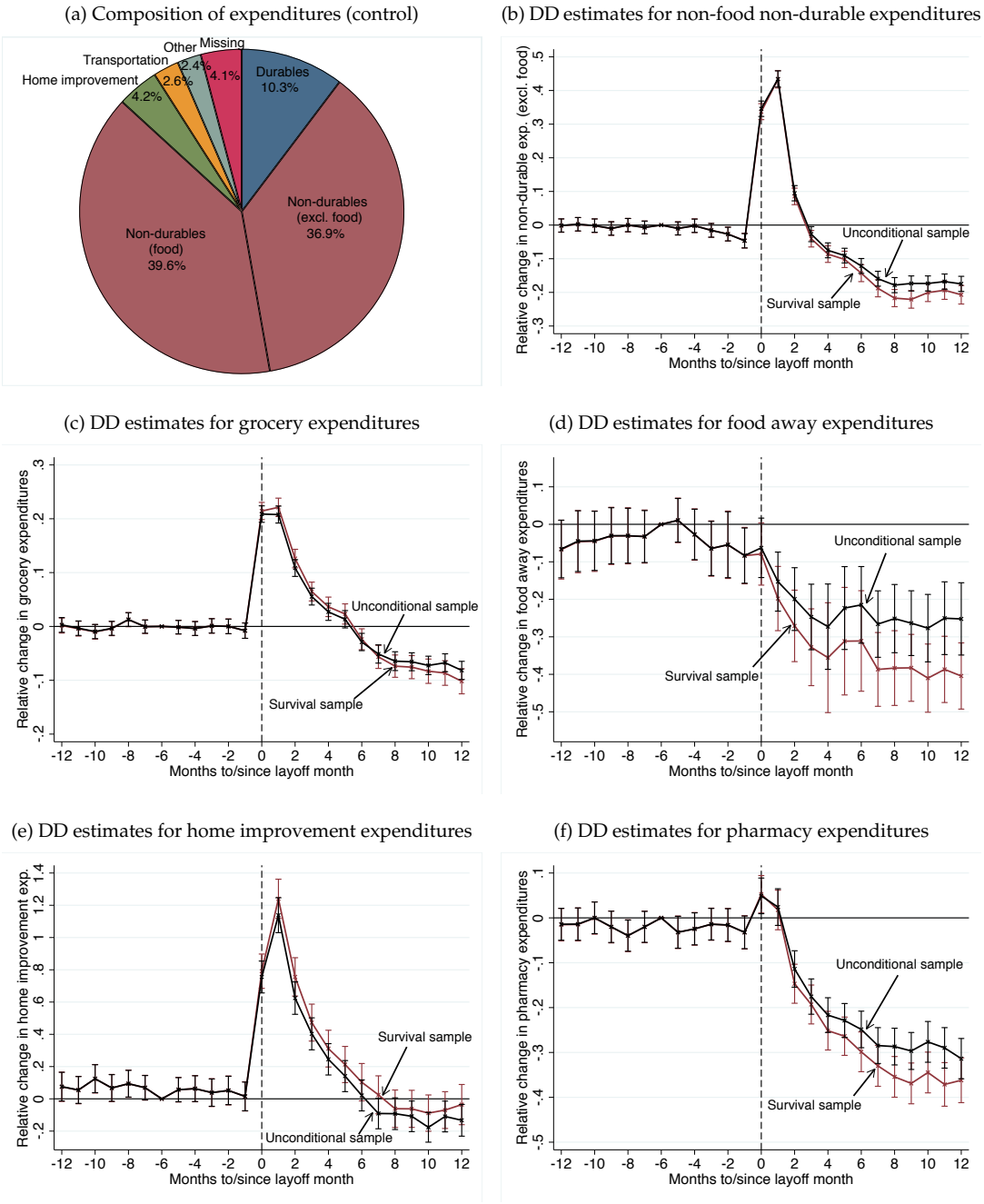


(e) DD estimates for workers laid off from a downsizing firm, which lost at least 30% of its workforce



The figure presents robustness checks for the results in Figures 4b and 4c. Panel (a) presents difference-in-differences results for the median (point estimates and 95% confidence intervals). We cannot control for month and worker fixed effects in this case, so we do not present results for the survival sample. Panels (b) and (c) present results using the logarithm of the outcome plus 10 and using an inverse hyperbolic sine transformation of the outcome, respectively. The long-term loss becomes larger, particularly in panel (c), but it might be overestimated in those cases given the slight downward pre-trend in the outcome prior to layoff. We thus prefer to be conservative and choose the specification used in Figure 4 as our preferred specification. Panel (d) presents results restricting our treatment and control groups to users reporting positive purchases in all 12 months before (placebo) layoff and having an average expenditure to wage ratio between 10% and 150% prior to layoff. Panel (e) presents results restricting our treatment group to workers laid off from a firm that lost at least 30% of its workforce in the year of the layoff. We restrict attention to firms (i) with at least 10 employees 12 months before layoff, (ii) that had not been downsizing in the year prior to the year of layoff, and (iii) that remained smaller in the year following layoff.

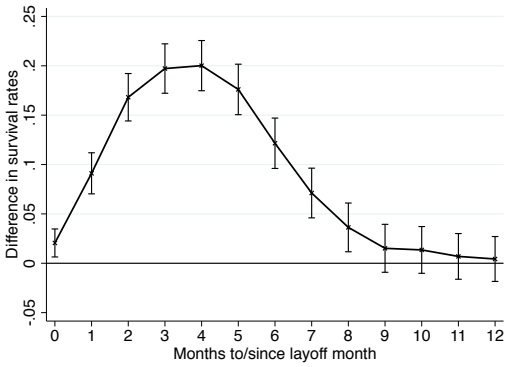
FIGURE A6: HETEROGENEITY BY EXPENDITURE CATEGORY (II)



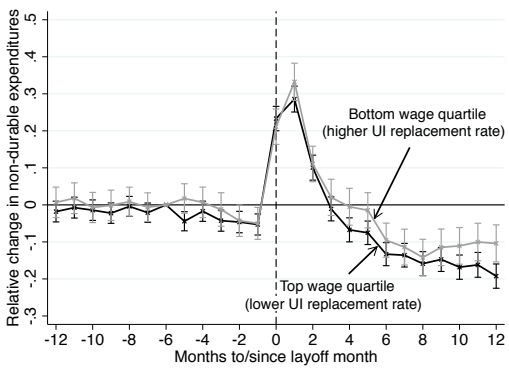
The figure presents results for different expenditure categories, as in Figure 5. Panel (a) displays the composition of expenditures before layoff (average over $k = -12$ to $k = -6$) in the control group. It is very similar as in the treatment group. Panels (c)-(f) present difference-in-differences results in relative changes as in Figure 4 (point estimates and 95% confidence intervals) using the specification in equation (8) for non-durable (excluding food), grocery, food away, home improvement, and pharmacy expenditures, separately. Interestingly, home improvement expenditures, a category for which complementarities with leisure may be important, is the only category for which estimates for the survival sample are always higher than estimates for the unconditional sample.

FIGURE A7: LINKING OUR RESULTS TO VARIATION IN JOB DISPLACEMENT INSURANCE BENEFITS (II)

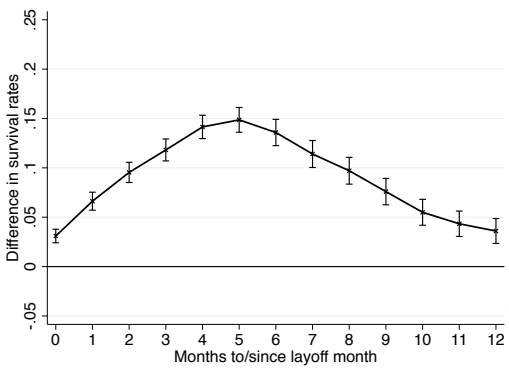
(a) Difference in survival rates in non-formal-employment for laid off vs. fired workers



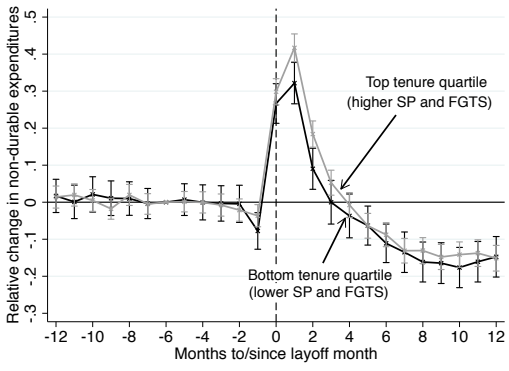
(b) Using variation in UI replacement rates from variation in wages (controlling for tenure; uncond. sample)



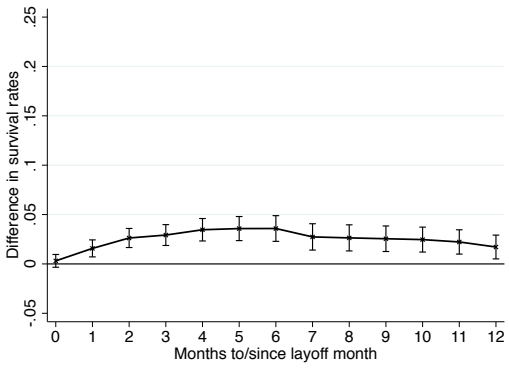
(c) Difference in survival rates in non-formal-employment for bottom vs. top wage quartile



(d) Using variation in SP and FGTS from variation in tenure (controlling for wages; unconditional sample)



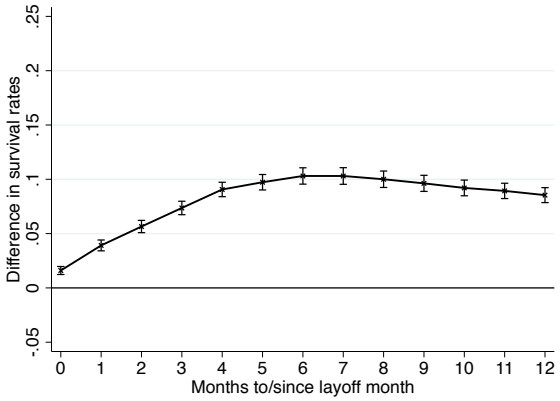
(e) Difference in survival rates in non-formal-employment for top vs. bottom tenure quartile



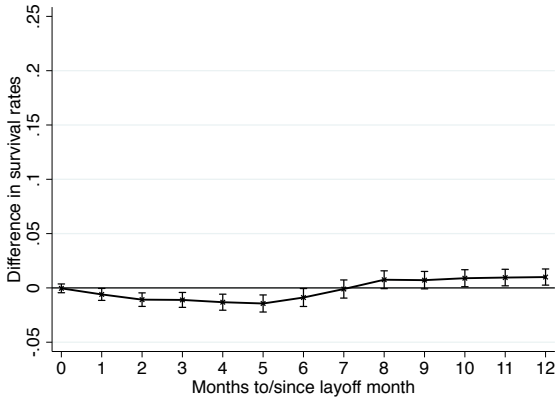
The figure presents results complementing the evidence in Figure 6. Panel (a) displays estimates of the difference in survival rates in non-formal-employment for laid off vs. fired workers. We combine the samples of laid off and fired workers (without the control group) for the months 0-12 after separation, and regress a dummy variable for remaining without a formal job on month fixed effects, event time fixed effects, and event time fixed effects interacted with a dummy variable for having been laid off (re-weighting the fired sample as in Figure 6a). We plot the estimated coefficients for this interaction. The survival rates are higher for laid off workers while they are eligible for UI, but they converge quickly after UI exhaustion, which is consistent with incentive effects. Panels (b) and (d) present similar results as in Figures 6b and 6c, but for the unconditional samples. Panels (c) and (e) displays estimates of the difference in survival rates in non-formal-employment for the bottom vs. top wage quartile and for the top vs. bottom tenure quartile, respectively. We proceed in a similar way as for panel (a), except that we also interact the same third-order polynomials as for Figures 6b and 6c with the event time fixed effects. The survival rates are much higher for workers with higher UI replacement rates while they are eligible for UI, but they converge quickly after UI exhaustion, which is again consistent with incentive effects.

FIGURE A8: HETEROGENEITY BY WORKER CHARACTERISTICS, DIFFERENCE IN SURVIVAL RATES

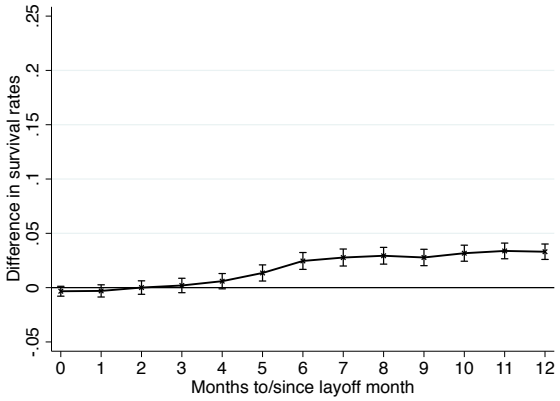
(a) Female vs. male workers



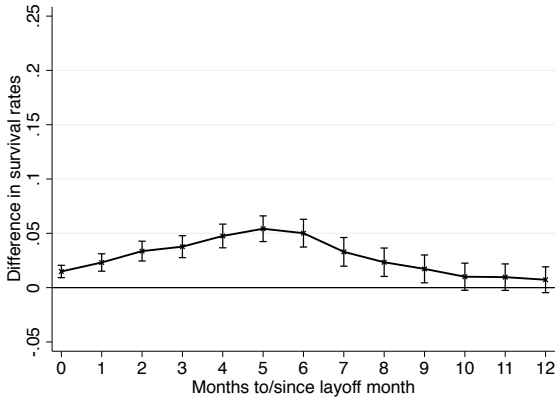
(b) Workers with at least vs. less than a high school degree



(c) Workers with more vs. less than the median age



(d) Workers with past experience with the consequences of job displacement vs. workers in the rest of the treatment group

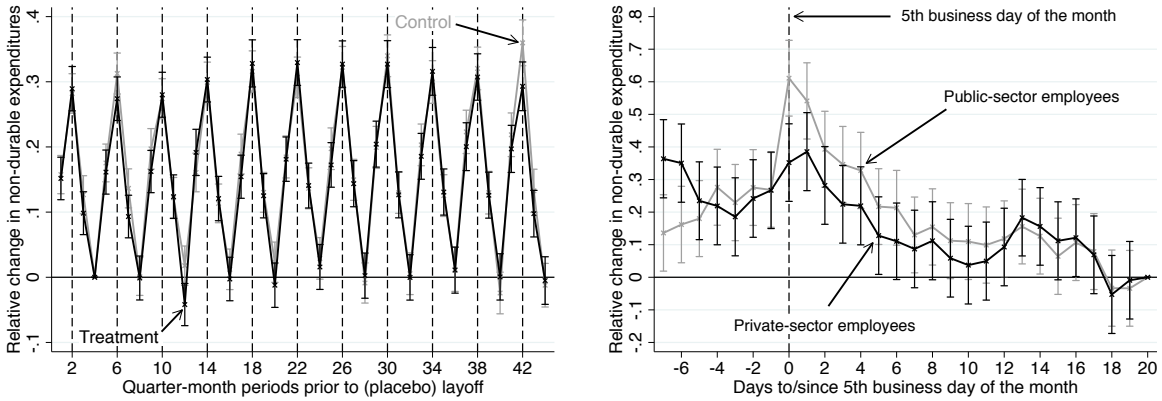


The figure displays estimates of the difference in survival rates in non-formal-employment for the categories of workers compared in Figure 7. We combine the samples compared in each panel for the months 0-12 after layoff, and regress a dummy variable for remaining without a formal job on month fixed effects, event time fixed effects, and event time fixed effects interacted with a dummy variable for being in the first category referenced to in the title of each panel, e.g., for being a female worker in the case of panel (a) (we reweight all sub-samples as in Figure 7). We plot the estimated coefficients for this interaction. For comparison purposes, we use the same scale for the y-axis as for differences in survival rates presented in Figure A7.

FIGURE A9: NON-DURABLE EXPENDITURE PROFILE AROUND PAYDAY EVENTS FOR FORMAL EMPLOYEES

(a) Using our treatment and control groups in the months prior to layoff

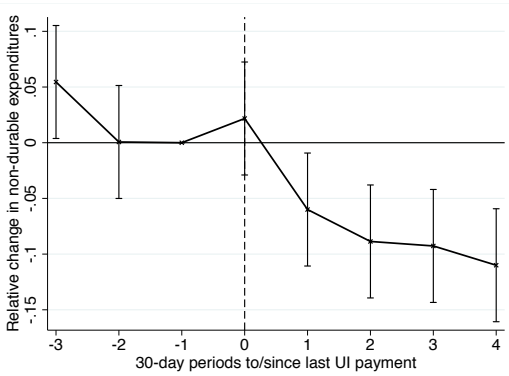
(b) Using all formal employees in the user data



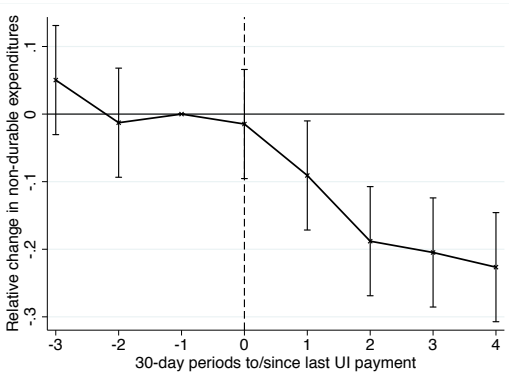
The figure complements the evidence in Figure 8a by showing that non-durable expenditure levels are also very sensitive to the timing of paydays for formal employees in our data. We use the fact that monthly salaries must be paid before the fifth business day of each month in Brazil. Panel (a) follows a similar approach as in Figures 8a using our treatment and control groups in the months prior to (placebo) layoff (month -12 to -2). We divide each month into 4 quarter-month periods: a first time period including the days before the fifth business day of the month; two 7-day periods (the first one identified by a vertical line starts on the fifth business day); and a fourth period including all the remaining days of the month. We then average expenditures at the worker-by-period level and we present the results (point estimates and 95% confidence intervals) of separate event analyses for the treatment and control groups (period 3 is the reference period; outcome variables are not de-trended but the specification includes month fixed effects). Non-durable expenditure levels are about 30% higher in the 7 days following the fifth business day of the month compared to the days at the end of the month. Expenditure levels are already higher in the days prior to the fifth business day because many firms pay workers earlier in the month. Panel (b) uses a much larger dataset including all months in which a worker is observed formally employed in the user data, in order to present results at the daily level. We include all months for which we observe 7 days before and 21 days after (and including) the fifth business day of the month (day 0). We present the results of separate event analyses for public-sector and private-sector employees because many public administrations pay their employees exactly on the fifth business day of the month. Accordingly, the decrease in expenditure levels in the 21 days following the fifth business day of the month (the last day is the reference period) is steeper for public employees: a reduction of 60% in non-durable expenditure levels compared to 40% for private-sector employees. The increase on the fifth business day compared to earlier days is also steeper for public-sector employees.

FIGURE A10: NON-DURABLE EXPENDITURE PROFILE AROUND UI PAYDAY AND UI EXHAUSTION EVENTS, ROBUSTNESS CHECKS

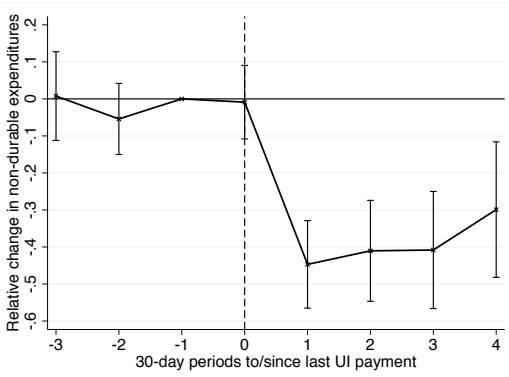
(a) Around UI exhaustion month (median; UI exhaustees)



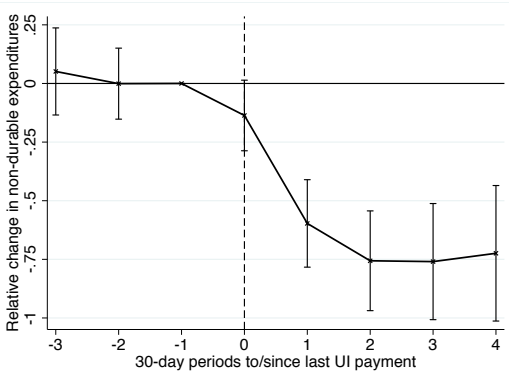
(b) Around UI exhaustion month (median; UI exhaustees remaining without a formal job)



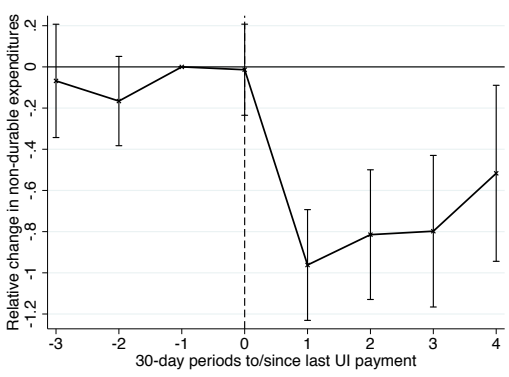
(c) Around UI exhaustion month (log+10; UI exhaustees)



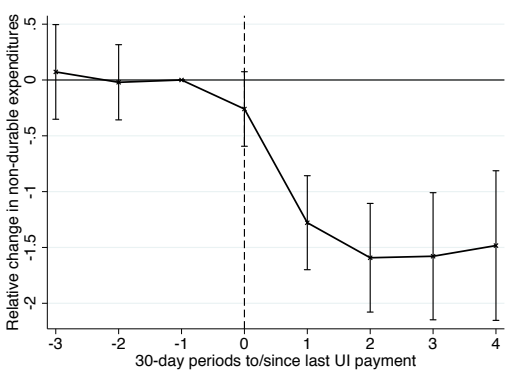
(d) Around UI exhaustion month (log+10; UI exhaustees remaining without a formal job)



(e) Around UI exhaustion month (inverse hyperbolic sine; UI exhaustees)



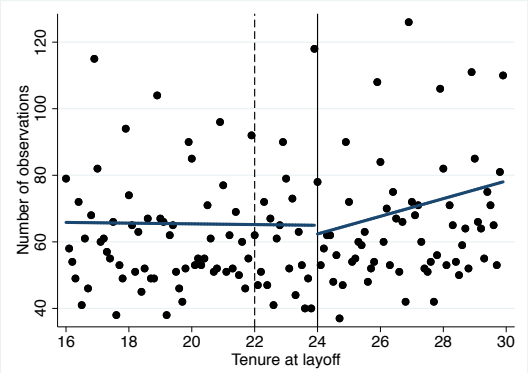
(f) Around UI exhaustion month (inverse hyperbolic sine; UI exhaustees remaining without a formal job)



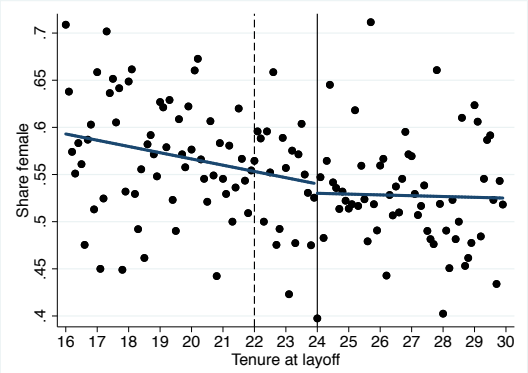
The figure displays robustness checks for the results in Figures 8c and 8. Panels (a) and (b) present relative change in non-durable expenditures for the median (point estimates and 95% confidence intervals). Panels (c)-(d) and (e)-(f) present relative changes in non-durable expenditures from specifications using the logarithm of the outcome plus 10 and using an inverse hyperbolic sine transformation of the outcome, respectively. The point estimates are larger than in Figures 8b and 8c, and they vary in the different rows, but the overall patterns remain identical: consumption levels are flat before UI exhaustion, but they drop rapidly after UI exhaustion.

FIGURE A11: REGRESSION DISCONTINUITY RESULTS, VALIDITY CHECKS

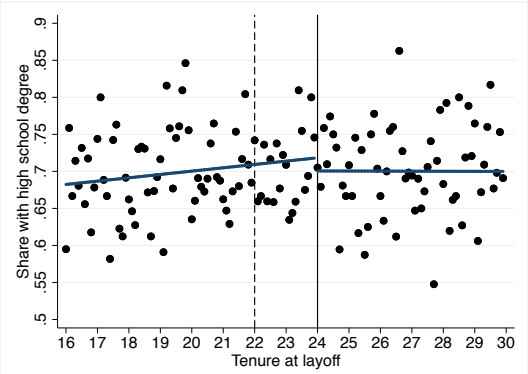
(a) Number of observations, raw data



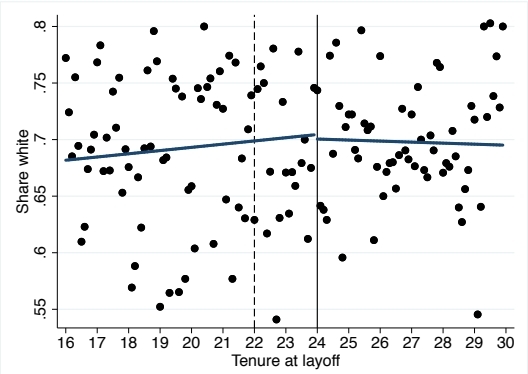
(b) Share female, raw data



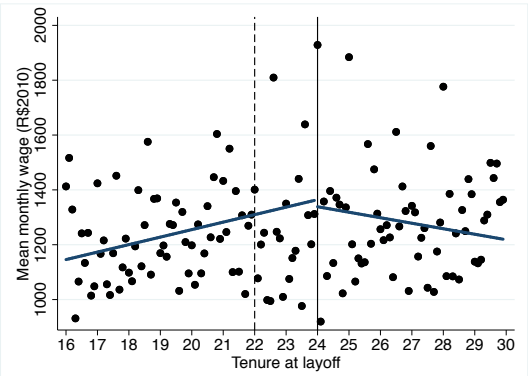
(c) Share with a high school degree, raw data



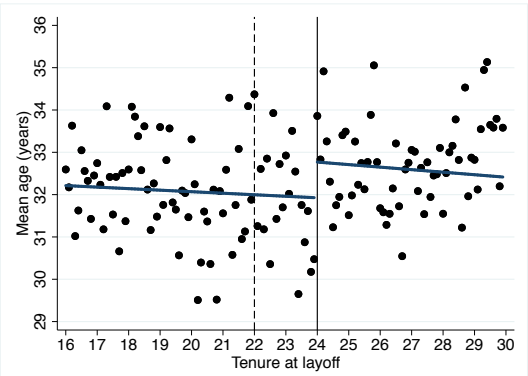
(d) Share white, raw data



(e) Monthly wage at layoff (R$2010)



(f) Age (years), raw data



The figure presents validity checks for the RD analysis around the 24-month cutoff; workers with less than 22 months (resp. more than 24 months) of tenure at layoff were eligible for four (resp. five) monthly UI payments (workers with 22 to 24 months of tenure were eligible for four or five monthly UI payments). Panels (a)-(f) plot various outcomes by tenure levels and report RD estimates (with their standard errors; these estimates are also reported in Table A1) based on the specification in equation (11). Panel (a) considers the distribution of the running variable (i.e., number of observations) and panels (b)-(f) the composition of the sample around the cutoff.

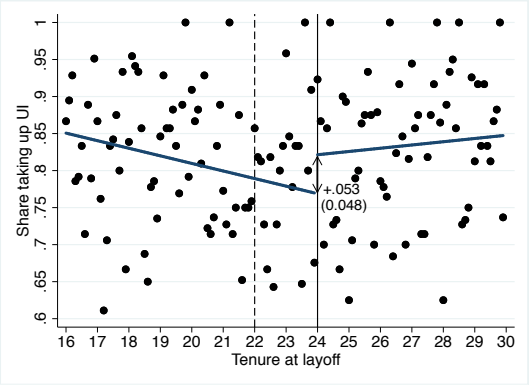
Table A1: Validity checks for the regression discontinuity (RD) design

	Number of observations	Share female	Share with high school degree	Share white	Monthly wage at layoff	Mean age
	(1)	(2)	(3)	(4)	(5)	(6)
A. Full sample						
Tenure>=24	-2.602	-0.00994	-0.0178	-0.00400	-25.75	0.844
	(9.436)	(0.0270)	(0.0227)	(0.0308)	(110.8)	(0.669)
Constant	64.98***	0.540***	0.718***	0.704***	1,364***	31.93***
	(8.441)	(0.0173)	(0.0189)	(0.0267)	(58.99)	(0.601)
Number of observations	7,613	7,613	7,613	7,613	7,613	7,613
B. UI sample						
Tenure>=24	-2.758	0.0474	-0.0832*	-0.104	-44.38	1.242
	(3.784)	(0.0561)	(0.0454)	(0.0707)	(173.5)	(1.356)
Constant	18.88***	0.453***	0.749***	0.781***	1,276***	32.01***
	(3.328)	(0.0441)	(0.0393)	(0.0571)	(130.8)	(1.119)
Number of observations	2,028	2,028	2,028	2,028	2,028	2,028

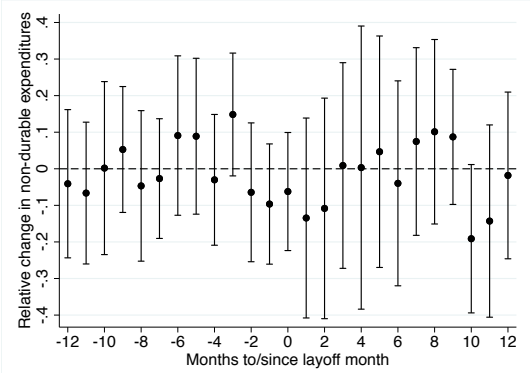
The table presents validity checks for the RD analysis around the 24-month cutoff for the full sample (panel A) and the subsample with UI data (panel B). It displays RD estimates (with their standard errors in parentheses) based on the specification in equation (11). Column (1) considers the distribution of the running variable (i.e., the number of observations) and columns (2)-(6) the composition of the sample around the cutoff. Significance levels: 1%*** 5%**, 10%*.

FIGURE A12: REGRESSION DISCONTINUITY RESULTS, OTHER OUTCOMES

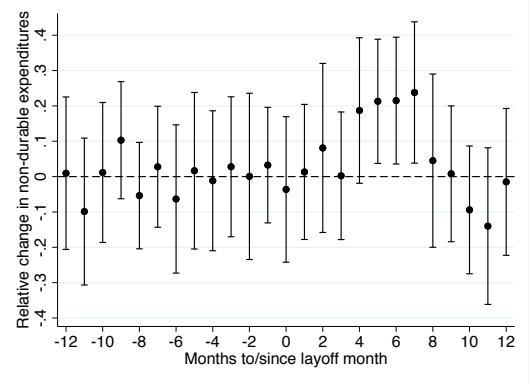
(a) Share taking up UI, raw data



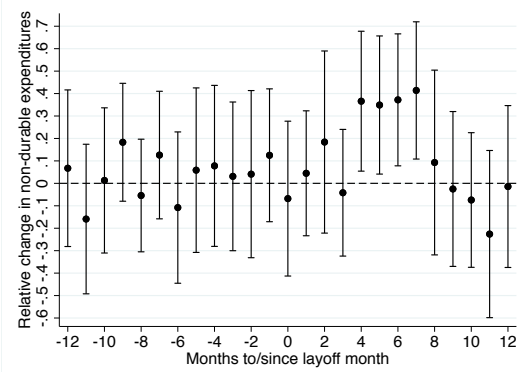
(b) Non-durable expenditures, RD estimates at the mean



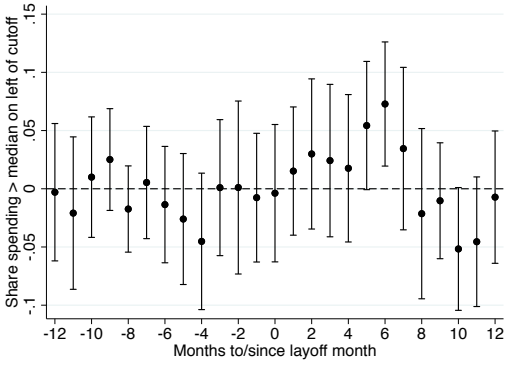
(c) Non-durable expenditures, RD estimates using log(non-durable expenditures +10)



(d) Non-durable expenditures, RD estimates using inverse hyperbolic sine transformation



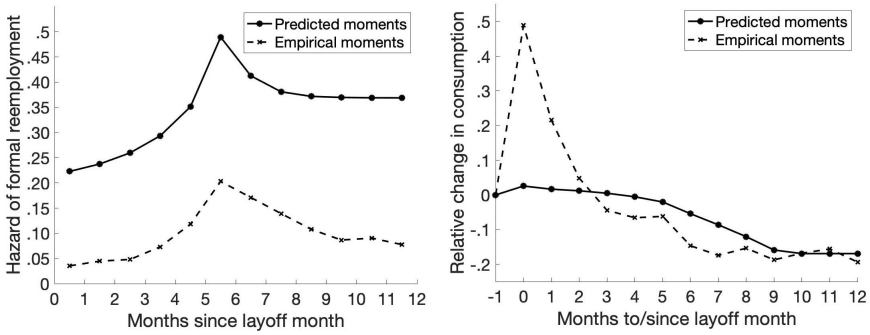
(e) Non-durable expenditures, RD estimates using a dummy for spending > median on the left of the cutoff



The figure presents results for the RD analysis around the 24-month cutoff for other outcomes; workers with less than 22 months (resp. more than 24 months) of tenure at layoff were eligible for four (resp. five) monthly UI payments (workers with 22 to 24 months of tenure were eligible for four or five monthly UI payments). Panel (a) plots the share of workers taking up UI by tenure levels and report RD estimates (with their standard errors) based on the specification in equation (11). Panels (b)-(d) display RD estimates (with their 95% confidence intervals) from using the specification in equation (11) in each month to/since layoff, separately, for non-durable expenditure outcomes in that month. Panel (b) show RD estimates in relative changes for the mean (we use the outcome in levels in the regression and then divide the estimated coefficients by the mean on the left of the cutoff). Panels (c)-(e) show RD estimates using the logarithm of the outcome plus 10, an inverse hyperbolic sine transformation of the outcome, and a dummy for consuming more than the median on the left of the cutoff, respectively.

FIGURE A13: SIMULATIONS OF THE MYOPIA MODEL BUT REPLACING THE DISCOUNT FACTOR TO $\delta = .995$

(a) Hazard of formal reemployment (b) Relative changes in consumption



The figure displays the predicted hazard rates of formal reemployment and the relative changes in consumption based on simulations of the myopia model, using the same parameter values as in columns (1) in Table 2, but replacing the discount factor to $\delta = .995$. We thus force the workers to be more patient but we do not change the other estimated parameters, e.g., we keep search costs identical. As expected given the discussion of equation (12), the model now dramatically over-estimates hazard rates of formal reemployment. In that model, displaced workers thus believe that they will be reemployed quickly when making their consumption decision in the first few months after layoff. Yet, even with such beliefs, the model does not predict that their consumption would spike at layoff. This indicates that *having high beliefs regarding reemployment probabilities* does not easily generate the consumption patterns in our data.