Job Displacement Insurance and (the Lack of) Consumption-Smoothing

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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 their insurance value, which critically relies on workers' ability to dissave the lump-sum progressively to smooth consumption after layoff. Using de-identified high-frequency expenditure data and matched employee-employer data from Brazil, we find that displaced workers eligible for both UI and SP *increase* consumption at layoff by 35% *despite* experiencing a 17% consumption loss after they stop receiving any benefits. Moreover, this sensitivity of consumer spending to cash-on-hand is present across spending categories and sources of variation in UI benefits and SP amounts. We show that a simple structural model with present-biased workers can rationalize our findings, and we use it to illustrate their implications for the incentive-insurance trade-off between SP and UI. Specifically, the insurance value of SP programs – or of other policies that provide liquidity to workers at layoff – can be severely reduced when consumption is over-sensitive to the timing of benefit disbursement, undermining their advantage in terms of job-search incentives. Our findings highlight the importance of the difference between SP and UI in their disbursement policy, and shed new light on the need for job displacement insurance in a developing country context.

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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 139 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 mostly focuses on their different *contingency* policy (Baily, 1978). 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*). Because UI payments are contingent on non-employment, UI can provide insurance against both risks, but it may reduce job-search incentives through both an income effect and a substitution effect. Because the SP amount is paid lump-sum, SP may be better for incentives as it does not create any substitution effect, but it can only insure against the risk of displacement as it does not distinguish between workers who are re-employed immediately and those who remain non-employed.

While there is a vast literature on UI, 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 the difference in contingency policy, SP and UI also differ in when they make payments to workers. In particular, the insurance value of SP critically relies on workers' ability to dissave their lump-sum progressively to smooth consumption after layoff. Despite a growing body of evidence for the excess sensitivity of consumption to cash-on-hand in other policy contexts (e.g., Shapiro 2005, Olafsson & Pagel 2018, 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 would observe the same excess sensitivity when the cash-on-hand is triggered by a negative shock.

This paper investigates the insurance value of UI and SP following a common approach in the UI literature (since at least Gruber, 1997) that empirically evaluates the insurance value of various policies by studying workers' ability to smooth consumption after layoff. We take advantage of a rare combination of de-identified high-frequency expenditure data from VAT receipts originally linked to individual identifiers 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 spending categories before and after layoff in a context where displaced workers are eligible for UI and SP, with some variation in benefits across workers that we exploit.

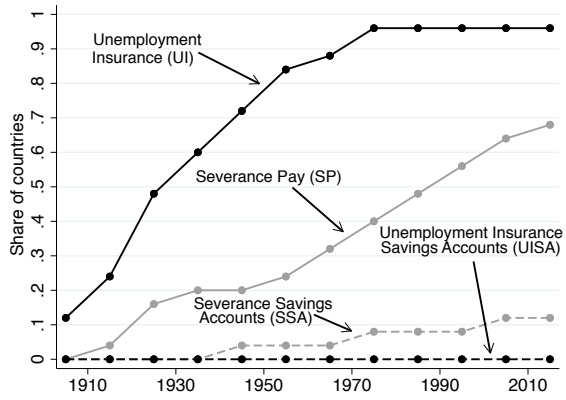
To the best of our knowledge, we provide the first evidence on the insurance value of SP programs. In addition, our analysis provides evidence on the need for job displacement insurance in a developing country context, where workers are often employed informally. Job displacement insurance can only cover *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 insurance value of any job displacement insurance program in such contexts. The need for insurance may be higher if traditional means of self-insurance (e.g., formal credit) are more limited (Chetty & Looney, 2007), but it may be lower 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* experienc-

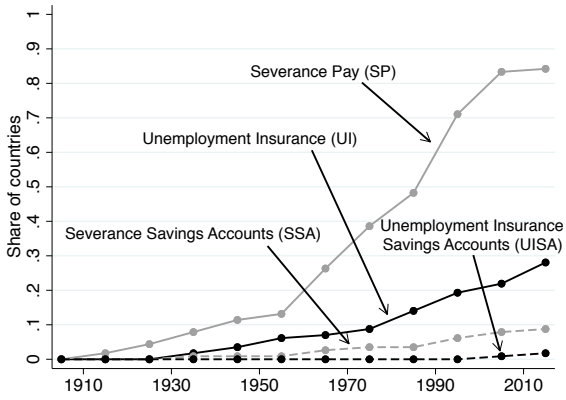
[1] This paper will focus on the *benefit payout schemes* – lump-sum vs. state-contingent. Job displacement insurance programs also differ in their *financing schemes* – insurance-based vs. savings-based (Parsons, 2016a). According to this typology, SP and UI programs are typically insurance-based. *Severance 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: GOVERNMENT-MANDATED JOB DISPLACEMENT INSURANCE AROUND THE WORLD

(a) Western Europe, USA, CAN, AUS, NZ (25 countries)



(b) Africa, Asia, Rest of Americas (114 countries)



Notes: The figure displays the share of countries with government-mandated job displacement insurance programs by decade. We collected data for 139 countries (see Appendix D for details) in Western Europe, USA, Canada, Australia, New Zealand (panel a), and Africa, Asia, 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, 2016a, 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).

ing a 17% long-term consumption loss 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 this setting, despite the higher labor market informality.

We provide other evidence of excess sensitivity of consumption to cash-on-hand using various sources of variation in benefits, which informs the mechanisms behind our results, and provides support for the quality of our expenditure data as we are able to replicate related findings from the literature. First, workers spend 20% more in the week that they receive their monthly UI paycheck. "Payday" effects on consumer spending have been documented in other contexts (e.g., Olafsson & Pagel, 2018), and have been associated to actual changes in consumption (Shapiro, 2005).[2] Second, workers who are eligible for one additional month of UI consume significantly more only in the period in which they draw the additional UI payment. Third, workers fail to smooth consumption in anticipation of the (expected) drop in income at UI exhaustion, which is associated with a drop in consumption of about 10%. Ganong & Noel (2018) recently documented a very similar finding using consumer spending data from the U.S. and showed that liquidity constraints cannot explain the excess sensitivity to cash-on-hand in that case.[3] We provide external validity for this finding, which is even more striking in the context that we study. Displaced workers tend to have limited financial resources at layoff in the U.S. (see, e.g., Chetty, 2008). In contrast, given the size of the SP amount that they receive at layoff, displaced workers in Brazil would have the liquidity to maintain their consumption levels well beyond UI exhaustion if they dissaved these financial resources more slowly.

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 provide insurance against the risk of remaining non-(formally-)employed,

[2]To the best of our knowledge this is the first paper to document "payday" effects for UI benefit payments. Olafsson & Pagel (2018) document payday effects around salary payment dates, which we also find in our data.

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

but also because workers' consumption is over-sensitive to the timing of transfers. The periodic nature of UI payments could play an important role in helping workers smooth consumption. To make this point more concretely, we show that a simple structural model with present-biased workers can rationalize both the consumption and the reemployment patterns in our data (the latter are similar to those in Gerard & Gonzaga, 2016). We then use the model to illustrate the role of disbursement policies for the incentive-insurance trade-off of job displacement insurance designs under excess sensitivity to cash-on-hand.

To motivate our empirical analysis, we begin the paper by laying out a dynamic model of job-search and consumption with forward-looking workers facing liquidity constraints after layoff, which is closely based on Card *et al.* (2007) and Chetty (2006, 2008). The consumption profiles under different job displacement insurance policies predicted by this standard model in the UI literature serve as a natural benchmark for our study. We also use the model to highlight the typical incentive-insurance trade-off in the design of these policies, and to discuss how this trade-off can be affected by labor market informality, which is relevant for developing countries such as Brazil.

Then, we describe the 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); it is a relatively rich and urban state, but its informal sector remains important, accounting for about 35% of private-sector employment. 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.

We also 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 (*ICMS*). Consumers are incentivized to give their ID number (*CPF*) to sellers at the time of purchase to be included on the receipt, and 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 of 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). Perhaps due to low participation costs (consumers only have to spell out their ID number at the moment of purchase), the take-up of this program was substantial: by 2015, more than 18 million individuals had opened 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, and we anonymously matched them with the Brazilian matched employee-employer dataset (*RAIS*), for about 400,000 de-identified users of a smartphone application that offers services to its users based on their account data. The resulting dataset covers the period 2010-2014; it is also matched with data from administrative UI records until 2012.[4] RAIS includes information on wages, hiring and separation months, and reason for separation, which allows us to observe all displacement events irrespective of benefit take-up and to calculate workers' statutory eligibility for SP and UI 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 expen-

[4]De-identified versions of the data were analyzed in a secure network-isolated data room; no data was shared with the developers of the application, and no original matched data was taken outside the secure data room.

diture data provide partial but meaningful coverage for purchases subject to the VAT (it excludes services and housing). Importantly, these 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. As 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 spending increases sharply after layoff for displaced workers, implying an estimate of +35%. Consumer spending 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 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).

These results can be directly linked 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. In addition, workers eligible for higher SP amounts have higher consumption levels in the first months after layoff; while workers eligible for higher UI replacement rates 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.

The consumption patterns around the time workers run out of UI benefits 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 non-durable 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%.[5] More importantly, non-durable expenditures are flat in the months before UI exhaustion, but then 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 between the two groups is only significant around the time workers eligible for an extra month of UI are more likely to receive UI payments.

The fact that workers fail to smooth consumption in anticipation of the (expected) drop in income at UI exhaustion is a useful result to shed light on the underlying models of behavior. Indeed, as shown in Ganong & Noel (2018), models with forward-looking but liquidity-constrained agents that have been used to explain instances of excess sensitivity of consumption to cash-on-hand for positive income shocks would predict that workers would save in anticipation of a negative shock to smooth consumption (Campbell &

[5]This specific result may not be a consumption response as 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.

Hercowitz, 2018; Kaplan & Violante, 2014; Kueng, 2018). Models in which workers have biased beliefs about their reemployment probabilities (Spinnewijn, 2015) or in which they underestimate the drop in income at UI exhaustion could predict a lack of savings in anticipation of UI exhaustion, but would not predict a sharp increase in consumption at layoff because of diminishing marginal utility of consumption.[6]

In contrast, present bias would generate both a high propensity to consume out of cash-on-hand and a low propensity to save in anticipation of a negative shock.[7] We show this by structurally estimating a version of the model laid out at the beginning of the paper in which we introduce present bias.[8] The estimated model can fit the key consumption and reemployment patterns in our data with a present bias parameter ($\widehat{\beta} = .71$) that is in line with the literature (Ericson & Laibson, 2019). Moreover, surveys that we conducted with UI applicants in São Paulo support the relevance of this mechanism: when asked, a majority 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. This mechanism has also the advantage of providing a policy rationale for the existence of forced savings account programs such as the one that exists in Brazil.

In a last step, we use the estimated model to illustrate the incentive-insurance trade-offs under counterfactual job displacement insurance designs and highlight the role of the disbursement policy. In particular, we show that the insurance value of SP can be severely reduced when consumption is over-sensitive to the timing of payments, undermining the advantage of SP in terms of incentive effects. Relatedly, our findings imply that the insurance value of providing liquidity to workers at layoff, a policy also advocated on the grounds that it mitigates incentive effects, may be limited unless it is done 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). In contrast, the literature on SP programs remains more limited despite their prevalence and the fact that Baily (1978) actually discussed both types of programs.[9] This paper also contributes to the growing literature at the intersection of public economics and behavioral economics (e.g., Chetty, 2015). In particular, the results bridges the literatures on UI (e.g., DellaVigna *et al.* , 2017) and household finance (e.g., Olafsson & Pagel, 2018), and highlight implications of present bias for the design of public policies (e.g., Lockwood, 2016), complementing the recent work by Ganong & Noel (2018).

The paper also contributes to a relatively small but growing literature on social insurance in the context of developing countries, which often have high labor market informality (e.g., Bosch & Campos-Vasquez, 2014; Gerard & Gonzaga, 2016). Developing countries spend much smaller shares of GDP on social insurance programs relative to developed countries (Chetty & Finkelstein 2013), but 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

[6] We also discuss in Section 5.1 why complementarities between consumption and leisure are unlikely to rationalize our findings.

[7] Savings constraints – e.g., due to the lack of a savings technology or to expenditure pressures from individuals' kin network (i.e., kinship taxation; Squires, 2018) – could generate these two predictions. However, surveys conducted with UI applicants in São Paulo suggest that this mechanism is not first-order in our context. Over 90% of these workers have bank accounts and kinship taxation does not seem to be a relevant threat. Arguably, expenditure pressures from one's kin network may be weaker after a salient negative shock.

[8] Conceptually, our model differs from those in DellaVigna *et al.* (2017) and Ganong & Noel (2018) by allowing workers to partially self-insure by engaging in (costly) income-generating activities while non-employed (e.g., added-worker effect, informal work activities). This innovation allows us to fit the key consumption and reemployment patterns in our data without assuming that income is exogenous when workers remain non-employed or that (some) workers experience more dramatic changes in consumption after layoff than observed empirically. This is also why the model can fit the data without introducing heterogeneity in time-discounting.

[9] 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.

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 of consumption 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 conceptual framework that guides our empirical analysis. Section 2 presents the empirical setting and our data. Sections 3 and 4 contain our main empirical results, investigating expenditure profiles around displacement events and UI payment events, respectively. Section 5 discusses the possible mechanisms behind our findings, shows that a simple structural model with present-biased workers is able to rationalize the key patterns in our data, and uses this model to illustrate the implications of our findings for the design of job displacement insurance programs. Section 6 concludes.

1 Conceptual framework and motivation

To motivate our empirical analysis, we begin by laying out a dynamic model of job-search and consumption with liquidity constraints closely based on Card *et al.* (2007) and Chetty (2006, 2008), which will later form the basis for our structural estimations in Section 5.[10] The consumption profiles of forward-looking workers under different job displacement insurance policies predicted by this standard model in the UI literature serve as a natural benchmark for our study. We also use the model to highlight the typical incentive-insurance trade-off with these programs. In the end of the section, we discuss how this trade-off is affected by informality in the labor market, which is particularly relevant for developing countries like Brazil.

We focus on the benefit payout schemes of SP and UI programs: the SP amount is paid lump-sum at layoff, while UI benefits are paid periodically to displaced workers who remain non-employed. Yet, their financing schemes may also differ: UI benefits are typically funded out of a payroll tax and the SP amount out of a firm's cash-flow. These financing schemes differ statutorily, but it is easy to show that they can be made equivalent in practice, motivating our focus. Specifically, 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 UI 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).[11] 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 who are laid off late transfer resources towards those who are laid off early.

[10] We follow the notation and presentation in DellaVigna *et al.* (2017) and Kolsrud *et al.* (2018).

[11] Real-world UI programs are rarely experienced-rated so that they also transfer resources between workers in firms with a low vs. a high expected layoff rate. These transfers do not provide any insurance (they are redistributive) and they distort firms' layoff decisions (e.g., Carvalho *et al.* , 2018). In terms of policy design, 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).

1.1 Setup

The model is in discrete time, starts in period $t = 0$ and ends in period $t = T$. A continuum of workers of mass 1 is initially employed at a firm that pays a wage w^e per period. With probability $1 - p$, a 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. It 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. It also specifies a per-period tax τ^b levied on workers still employed at the firm, which funds the expected UI outlay.[12] Assuming that the incidence falls on workers, the per-period take-home pay of these workers is reduced by τ^b. Finally, the policy 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 then reduced by the amount τ^f necessary 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 compensate her in case of layoff in exchange of a per-period premium when employed. It is also irrelevant whether the programs are explicitly managed by the government or implicitly by the firm.[13]

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 is normalized to correspond to her reemployment probability in period $t + 1$, with $h_{i,t} \in [0, 1]$. The utility cost of effort $\psi_i(h_{i,t})$ is assumed to be separable, increasing, and strictly convex. The worker's probability to be non-employed in period t equals the survival rate $S_{i,t} \equiv \Pi_{t'=0}^{t-1}(1 - h_{i,t'})$, with $S_{i,0} = 1$. In practice, we only 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 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:

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

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

[12] With this setup, the time it takes for workers to find a new job once they have exhausted their UI benefits has no effect on the cost of the UI program, which is the case when UI benefits are experienced-rated.

[13] Firms may be more likely to default on their obligations at layoff. This issue appears to be limited in the context that we study: in a survey of UI applicants in São Paulo, most respondents report that their firm paid all of what they were entitled to (see Section 2.3).

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 intertemporal budget constraints and budget balance conditions at the end of life:

$$c^e_{i,t} = a_{i,t} + y_{i,t} + (w^e - \tau^b - \tau^f) - \frac{a_{i,t+1}}{(1+r)} \text{ for } t = 0...T-1 \qquad c^e_{i,t} = a_{i,t} + y_{i,t} + w^e - \tau^b - \tau^f \text{ for } t = T$$

$$c^n_{i,t} = a_{i,t} + y_{i,t} + (f_t + b_t) - \frac{a_{i,t+1}}{(1+r)} \text{ for } t = 0...T-1 \qquad c^n_{i,t} = a_{i,t} + y_{i,t} \text{ for } t = T$$

$$c^r_{i,t} = a_{i,t} + y_{i,t} + w^r - \frac{a_{i,t+1}}{(1+r)} \text{ for } t = 1...T-1 \qquad c^r_{i,t} = a_{i,t} + y_{i,t} + w^r \text{ for } t = T$$

where $f_t = f \cdot \mathbb{1}\{t = 0\}$, $b_t = b \cdot \mathbb{1}\{t < P\}$, and $y_{i,t}$ captures any other disposable income received by the worker. In this model, the first order conditions for a worker who is non-employed in period t are simply:

$$\frac{\partial \psi_i(h_{i,t})}{\partial h_{i,t}} = \delta \cdot \left[V_{i,t+1}(a_{i,t+1}) - U_{i,t+1}(a_{i,t+1})\right] \tag{3}$$

$$\frac{\partial \nu_i(c^n_{i,t})}{\partial c^n_{i,t}} = (1+r) \cdot \delta \cdot \left[h_{i,t} \cdot \frac{\partial \nu_i(c^r_{i,t+1})}{\partial c^r_{i,t+1}} + (1 - h_{i,t}) \cdot \frac{\partial \nu_i(c^n_{i,t+1})}{\partial c^n_{i,t+1}}\right] \tag{4}$$

1.2 Benchmark predictions

This simple model can help illustrate the contrast between the effects of SP and UI on job search and consumption profiles. Below, we present the predictions of the model for the case of (relatively patient) forward-looking workers, which is a natural benchmark for the empirical analysis in the next sections.

Job search. SP and UI programs will have different impacts on workers' job search intensity (Baily, 1978). This is easily seen in the model. Given that job-search only affects reemployment in the next period,[14] an increase in the SP amount f or in the benefit level b_0 in period 0 will have no direct impact on job search. In contrast, an increase in the benefit level $b_{t'}$ in any period $t' > t$ will decrease job-search in period t according to the first-order condition (3), by increasing the value of non-employment in period $t+1$. This is the *substitution effect* in Chetty (2008). Increases in the SP amount f and in the UI benefit b_t in any period may also reduce job-search indirectly by increasing workers' (expected) liquidity after layoff (to the extent that it has a larger impact on the value function when non-employed than reemployed), the *liquidity effect* in Chetty (2008). This effect will be the same for increases of the same (expected) net present discounted value provided that equation (4) holds in period t, i.e., that workers' savings decision is not at a corner solution.

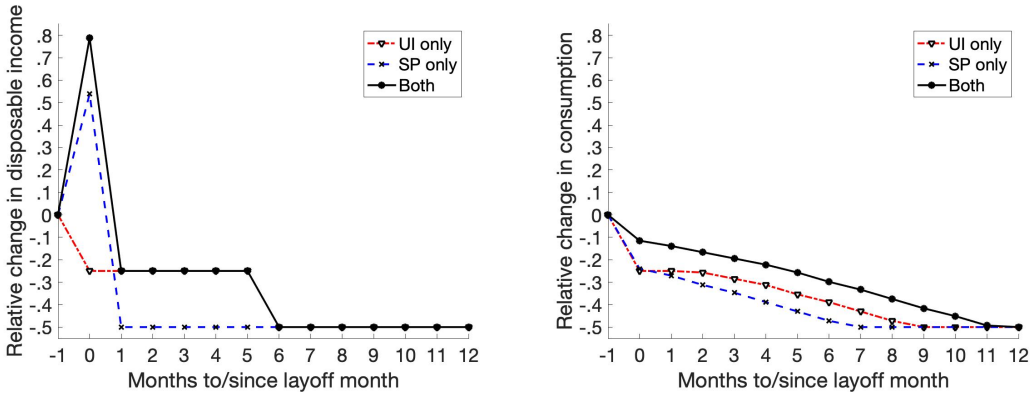
Consumption-smoothing. SP and UI programs will also have different impacts on consumption profiles after layoff. This is illustrated in Figure 2, in which we display simulations of our model under different job displacement insurance policies. To isolate the effects on consumption, we fix the monthly reemployment probabilities at $h_{i,t} = .15$ in all cases. We then consider three policies: a UI program with a 50% replacement rate ($b_t/w^e = .5$) and a six-month potential UI duration ($P = 5$); a SP program providing the same expected net present discounted value in a lump-sum fashion at layoff (corresponding to $f = 2.1 \cdot w^e$); and a policy combining both programs. For our simulations, we assume that workers have no assets at layoff ($a_{i,0} = 0$),

[14] Job-search affects reemployment in the same period in Card *et al.* (2007) and Chetty (2008), which explains the small difference in the exposition of substitution and liquidity effects in this paragraph compared to their exposition in these papers.

FIGURE 2: BENCHMARK CONSUMPTION PROFILES WITH SP AND UI PROGRAMS

(a) Income profile for workers who remain non-employed

(b) Consumption profile for workers who remain non-employed



Notes: The figure displays simulations of our motivating model under three job displacement insurance policies: a UI program with a 50% replacement rate and a six-month potential UI duration; a SP program providing the same expected net present discounted value in a lump-sum fashion at layoff; and a policy combining both programs. Panel (a) displays the changes in disposable income for workers who remain non-employed in each month after layoff under these policies (see text for details). Panel (b) displays the consumption profile of these workers as predicted by the model for a monthly discount rate of .995 and a log utility function. To isolate the effects on consumption, the monthly reemployment probabilities are fixed at .15 in all cases.

that they cannot borrow against their future income ($\overline{a}_i = 0$; we set the reemployment wage at $w^r = .85 \cdot w^e$), and that savings yield zero interest ($r = 0$). We also assume that workers live with another adult who earns the wage w^e in all periods and with whom they pool all financial resources, such that they have a minimum income of $w^e/2$. Figure 2a displays the resulting changes in disposable income for workers who remain non-employed in each month after layoff under the three policies. Figure 2b displays the predicted consumption profile of these workers for a monthly discount factor of $\delta = .995$ and a log utility function.

There are three main takeaways from Figure 2b.[15] First, in all cases, consumption drops immediately at layoff because workers are forward-looking and experience a drop in their expected liquidity after layoff. The drop is similar for the UI and SP programs because we calibrated the model such that they have the same expected net present discounted value; the drop is smaller when we combine the two programs because the associated drop in expected liquidity is smaller. Second, workers smooth consumption in the following months. With the SP program, workers dissave their SP amount progressively according to the first-order condition (4), thus taking into account the risk of remaining non-employed. With the UI program, workers follow the same first-order condition and save part of their UI benefits in anticipation of the drop in income at UI exhaustion. Third, workers who remain non-employed are able to maintain higher consumption levels with the UI program than with the SP program. The two programs have the same expected net present discounted value at layoff, and so provide similar insurance against the risk of layoff in expectations, but UI provides better insurance against the risk of remaining non-employed: a worker who remains non-employed receives more financial support with the UI program because UI benefits are state-contingent. This difference would increase with higher reemployment probabilities as UI benefits would have to increase for the program to keep the same expected net present discounted value at layoff. Put simply, it is cheaper to provide insurance when the risk of being in the "bad" state of the world is lower.

The typical incentive-insurance tradeoff between UI and SP programs in this standard model thus comes

[15] We reach similar conclusions if we relax the strong assumptions regarding workers' means of consumption smoothing in our simulations (e.g., no assets, no borrowing, exogenous income when non-employed), as long as they are not able to self-insure perfectly.

entirely from their different *contingency* policy. It is because UI benefits are state-contingent that UI both reduces job-search incentives and provides better insurance against the risk of remaining non-employed.

1.3 Informing the incentive-insurance tradeoff empirically

There is a vast empirical literature informing the incentive-insurance tradeoff of UI programs. On the incentive side, several papers use estimates of the impact of increases in the UI benefit level (db) or in the potential UI duration (dP) on reemployment rates to quantify the potential welfare loss of these reforms, namely the efficiency cost from behavioral responses to these reforms (see, e.g., Schmieder *et al.* , 2012).

On the insurance side, estimates of the consumption profile of employed vs. displaced workers have been used to quantify the potential welfare gains from these reforms, namely the insurance value from the consumption-smoothing gain of these reforms. Consider the case of increase in potential UI duration. The value of this additional insurance corresponds to the gains from transferring \$1 from the good state (i.e., remaining employed) to the bad state of the world (i.e., remaining non-employed after UI exhaustion), namely the gap in marginal utilities between the two states. Estimates of the consumption profile of employed vs. displaced workers can inform this quantity because it can be approximated by (Baily, 1978; Gruber, 1997):[16]

$$\frac{\mathbb{E}\left[\frac{\partial \nu_i(c^n_{i,P})}{\partial c^n_{i,t}}\right] - \mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right]}{\mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right]} \simeq \gamma \cdot \frac{\mathbb{E}\left[c^e_{i,t}\right] - \mathbb{E}\left[c^n_{i,P}\right]}{\mathbb{E}\left[c^e_{i,t}\right]} \tag{5}$$

where the two expectations are the average marginal utilities of consumption if remaining employed and if non-employed after UI exhaustion in absence of the reform (the target beneficiaries for an increase dP). The parameter γ is a coefficient of relative risk aversion and the last term in equation (15) is the relative difference in average consumption between the two states in absence of the reform. Based on this "consumption approach," several papers use evidence on the consumption profile of displaced workers to evaluate the insurance value of UI reforms for a range of values for the risk aversion parameter or to compare the insurance value of different reforms (see, e.g., Kolsrud *et al.* , 2018). A useful property of this approach is that it is "sufficient to obtain consumption-smoothing estimates for a 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 property rests on the assumption that, with multiple consumption categories, workers equalize their marginal utility across any two categories A and B within each time period. In that case, the insurance value could be approximated as in equation (15) using data on either category and an appropriate risk aversion parameter: $\gamma^A \cdot (\mathbb{E}\left[c^{e,A}_{i,t}\right] - \mathbb{E}\left[c^{n,A}_{i,P}\right]) / \mathbb{E}\left[c^{e,A}_{i,t}\right] = \gamma^B \cdot (\mathbb{E}\left[c^{e,B}_{i,t}\right] - \mathbb{E}\left[c^{n,B}_{i,P}\right]) / \mathbb{E}\left[c^{e,B}_{i,t}\right]$. For instance, the drop for food consumption will likely be smaller than that of entertainment expenditures because the risk aversion parameter for food is likely larger, i.e., food is a necessity. One can thus inform the insurance value even with partial consumption data (e.g., Gruber, 1997, used data on food only).

Similar approaches can be used to inform the incentive-insurance tradeoff of SP programs,[17] but the empirical literature on SP programs is more limited. We have some direct evidence on the impact of SP on job search. Card *et al.* (2007), for instance, showed that SP reduces reemployment rates, providing evidence

[16] The approximation assumes that the third-order derivative of the utility function is small (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 account for heterogeneity; Kolsrud *et al.* , 2018).

[17] In Appendix A, we derive the usual sufficient statistics formulas in the UI literature for this trade-off in the case of increases in the UI benefit level and in the potential UI duration. We then show how the formula compares for an increase in the SP amount at layoff.

of liquidity effects, but to a lesser extent than UI because of the additional substitution effect in that case. Such findings, interpreted through the lenses of models such as the one above, imply an important role for ensuring that workers have sufficient liquidity at layoff (e.g., Chetty, 2008; Shimer & Werning, 2008).

In practice, however, we still know very little about the consumption-smoothing effect of SP programs or about the way displaced workers would dissave sizable inflows of liquidity at layoff. This gap in the literature is critical because there is a growing body of evidence from various contexts that consumption is often excessively sensitive to cash-on-hand (e.g., Shapiro, 2005). In the extreme case of hand-to-mouth workers, for instance, the consumption profile would mirror the timing of disposable income in Figure 2a. The impacts of SP and UI on consumption-smoothing would be very different than with the forward-looking benchmark in Figure 2b. In fact, the gains from transferring \$1 from workers remaining employed to those who just got laid off, i.e., the insurance value of an increase in the SP amount, could even become negative in such an extreme case, as workers consume more at layoff. At the same time, it is unclear whether the existing evidence on excess sensitivity to cash-on-hand would necessarily apply in the displacement context given that the inflow of liquidity is caused by a salient negative shock, i.e, a layoff.

We contribute to filling this gap in the rest of the paper by exploiting a rare combination of longitudinal data on employment and consumption in a setting where displaced workers are eligible for both UI and SP at layoff. The core of our analysis consists in documenting novel findings regarding the consumption responses of displaced workers to these programs. We then build on the standard model above and investigate extensions of this model that can rationalize our findings, which we estimate structurally. Finally, we use our results to shed new light on the incentive-insurance tradeoff between UI and SP programs.

1.4 Developing country context

Informing this tradeoff is particularly relevant for middle-income and developing countries given that SP programs are predominant in these countries (see Figure 1). A distinctive feature of the labor markets in such contexts is the large share of *informal* workers, i.e., who are not registered with the government, 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. Yet, Gerard & Gonzaga (2016) show that the incentive-insurance tradeoff remains conceptually similar. One must only substitute "employed" with "formally employed" and "non-employed" with "non-formally-employed": UI and SP provide workers with insurance against the risk of displacement from a formal job; UI also provides them with insurance against the risk of remaining non-formally-employed; and the substitution effect of UI is to decrease incentives to be formally reemployed.

This does not imply, however, that the trade-offs of UI and SP programs are unchanged in such contexts.

Formal job search. The effects of UI and SP on formal reemployment may differ in labor markets with higher informality. 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 by working informally. On the other hand, the two main (complementary) views on the prevalence of informality in developing countries (Perry *et al.* , 2007) suggest that formal reemployment rates may be low even in the absence of incentive effects, 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). In practice, Gerard & Gonzaga (2016) show that the efficiency cost of an increase in potential UI duration is smaller in Brazil than in the US, and smaller in more informal labor markets within Brazil. This is because formal reemployment rates are indeed lower in labor markets with

higher informality absent any incentive effect, which limits the efficiency cost of UI. Britto (2018) also shows that a positive income shock at layoff reduces formal reemployment rates in the same setting but to a lesser extent than the increase in potential UI duration, mirroring the findings in Card *et al.* (2007).

Insurance value. The insurance value of UI and SP programs may also differ in labor markets with high informality. On the one hand, the need for insurance against the risk of displacement and the risk of remaining non-formally-employed can be large if it is hard to find a new formal job. Moreover, 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). On the other hand, the need for insurance against both risks can be small if informal jobs are easy to find and pay similar wages as before layoff. Even if wages in new jobs are lower than before layoff, such that there may be a need for insurance against the risk of displacement, the need for insurance against the risk of remaining non-formally-employed can be small if new informal jobs are close substitutes for new formal jobs. The need for any insurance may also 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 in that case, and workers may simply save the excess earnings while having a formal job.

The insurance value of programs that support displaced formal workers in these contexts is, therefore, an empirical question for which we have very little evidence. Our empirical analysis in the next sections contributes to addressing this limitation. In particular, the size of the consumption drop after layoff sheds light on the ability of displaced formal workers to self-insure by working informally.[18]

2 Institutional background and data

Before describing our empirical analysis, We present relevant institutional background information and discuss important details of the data sources that we use, and the sample of workers we focus on.

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 required by law, her hiring is reported to the government, and she becomes a formal employee. Hiring an employee formally is costly, however;[19] 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 developed 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

[18]The models that we estimate structurally in Section 5 will also allow displaced formal workers to self-insure by working informally.

[19]Payroll taxes include 20% for Social Security and 7.8% for funding various programs (e.g., training). Formal employees are also entitled to the minimum wage, a 13th monthly wage, a 50% overtime rate (above 44 hours/week), among other mandated benefits.

period.[20] 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 Appendix Figure A1). Finally, a peculiarity of the Brazilian UI program is that it is financed by a .65% tax on firms' sales 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.[21] 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.[22]

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).

For clarity, we refer to the workers' FGTS balance and SP separately in this section. However, as the focus of this paper is on the difference in *benefit payout schemes* – i.e., lump-sum vs. state-contingent – between job displacement insurance programs, we refer to both of these lump-sum programs under the term "SP" (or "SP amount") in our analysis in the next sections (starting in Section 3).

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

Figure 3a displays the average *statutory* job displacement insurance benefits in our *analysis sample*, and in a *benchmark sample* drawn randomly from the universe of displaced formal workers in São Paulo. We restrict attention to laid-off workers and we apply the same selection criteria in the two samples (Section 2.4 provides details on the construction of the two samples). The average FGTS balance, SP amount, and monthly UI benefit are 210%, 247%, and 53% of the average monthly wage at layoff in the analysis sample.[23] These figures are comparable in the benchmark sample, although monetary values are slightly lower (see the discussion on the representativity of the analysis sample below).

We depict a potential UI duration of five months because the average tenure at layoff is above 24 months in both samples. In sum, Figure 3a 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 in the months after layoff. There is a lot of heterogeneity across workers, however, a feature that we exploit in the next sections.

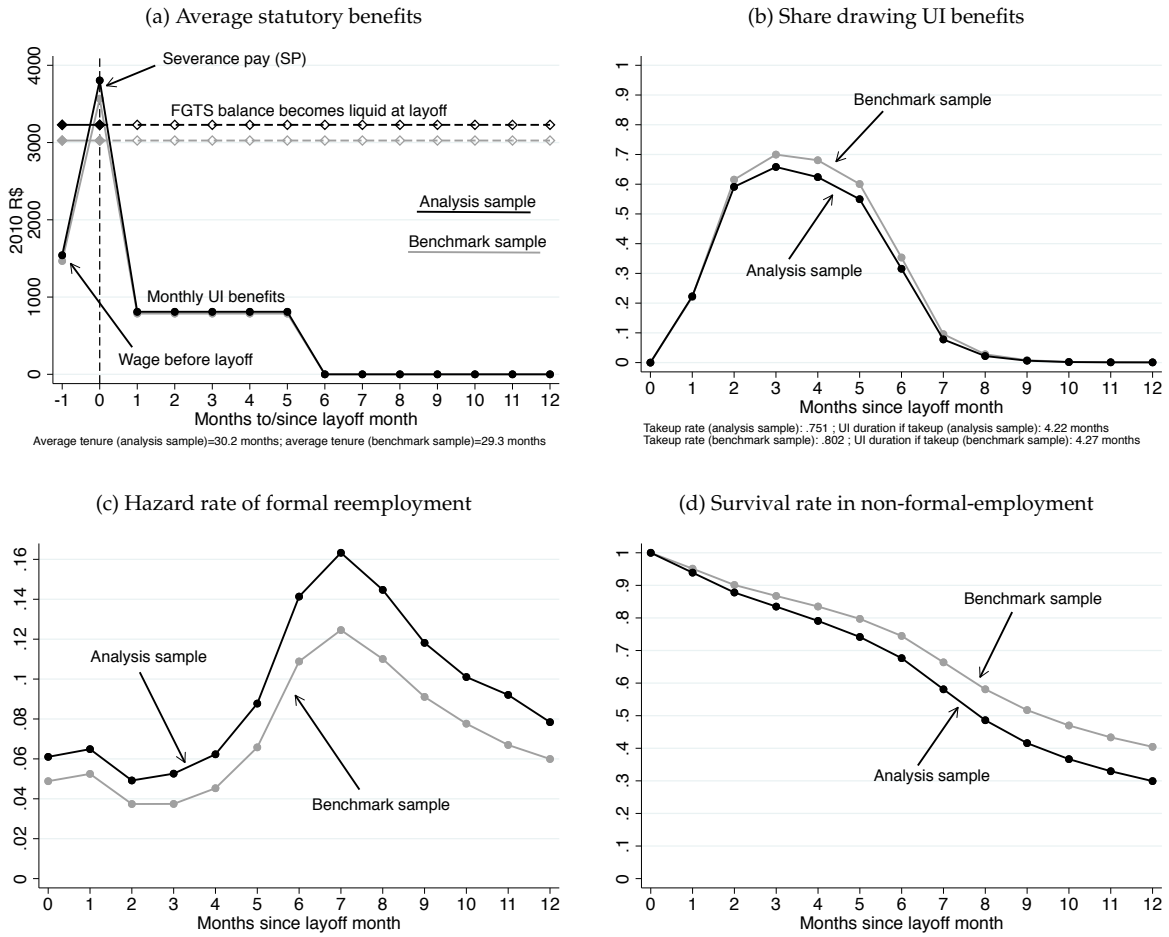
[20] There must also be at least 16 months between a worker's layoff date and the layoff date of her last successful application for UI. Note that the rules described in this paragraph were in place until February 2015, and thus for the whole period covered by our data.

[21] 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 account balance is pensionable, and bequested in case of death. In surveys conducted with UI applicants in São Paulo, only 7% reported having accessed their FGTS before layoff (see Appendix C and Table C2). 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 (but not the one corresponding to job A).

[22] 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.

[23] The averages of the replacement rates (UI benefit/wage, SP amount/wage, FGTS balance/wage) are slightly higher (see Table 1). Note that all monetary values in the paper are in reais (R$) of 2010. The exchange rate was around US$1=R$2 at the time.

FIGURE 3: BACKGROUND AND REPRESENTATIVITY OF DISPLACED FORMAL WORKERS IN THE ANALYSIS SAMPLE



Notes: The figure provides some background on displaced formal workers (involuntarily laid off) in the setting of our empirical analysis and compares our *analysis sample* to a *benchmark sample* drawn randomly 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 Appendix Figure A2 that the patterns presented in panels (a), (c), and (d) are very similar when we impose the same restriction.

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 registered firms must report all workers formally employed at any point during the previous year.[24] 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 136 UI applicants in São Paulo between July and August 2018 to get a sense of firms' compliance with their obligations to displaced workers (see Appendix C). Only 2% of respondents reported any issue in accessing their FGTS balance, 12% in receiving the 40% fine, and 16% in being given their advance notice. Moreover, conditional on firm's compliance, the average amount available lump-sum at layoff was about 486% of the monthly wage at layoff, which is in line with the statutory amounts that we calculate in our sample. Finally, conditional on having access to these resources, only 4% reported that they did not (intend to) withdraw the full FGTS balance (see Appendix Table C2).

Expenditures. The main novelty of this paper is that we can anonymously match the formal employment and the UI registry data with administrative expenditure data from de-identified receipts that originally specified 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 (*ICMS*) and to foster a culture of tax compliance. In a nutshell, the program introduced the possibility of identifying the consumer's ID number (their *CPF*) 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.[25] 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 NFP program improved compliance by firms. 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. This policy uses the fact that receipts already had a field to identify the buyer, which is relevant for tax purposes in the case of business-to-business transactions. With this policy, the cashier can simply insert the consumer's ID in that field for sales to final consumers. Moreover, no pre-registration is needed for consumers to start participating. Opening the online account is only necessary to claim the rewards –

[24] 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 employment.

[25] In Appendix Figure B1, we display a copy of a VAT receipt from São Paulo, which shows where the ID number is displayed.

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 enforcement of other tax bases. By 2015, more than 18 million people had opened an online account for the program.

Because receipts are linked to individual identifiers, these individual online accounts create a record of participants' reported expenditures for broad categories of consumer spending. We obtained access to such data, and were able to anonymously match them with data from RAIS and the UI registry, for 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.[26] Our matched sample includes about 400,000 de-identified 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 in this period for which these de-identified 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 (*CNAE*, the same as in RAIS), which allows us to classify expenditures in categories such as durables, non-durables, groceries, etc.[27]

By design, the expenditure data only include purchases for which workers in our sample chose to provide their ID number, but the data are not restricted by purchases' value or mode of payment. In particular, the data include cash transactions, which is key 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 data and in São Paulo as a whole are relatively comparable.

2.4 Descriptive statistics and representativity of the analysis sample

We present descriptive statistics for our main analysis sample to provide some background information about the empirical setting. We also evaluate its representativity by comparing it to a benchmark sample, drawn randomly from the universe of displaced formal workers in São Paulo.

Our main analysis sample 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 with expenditure data. 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 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 NFP participants throughout the period). We obtain a benchmark sample of 156,021 layoff events after selecting 5% of the overall sample (not restricted to those with expenditure data) at random.

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:[28] 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

[26] We accessed the de-identified matched data in a secure network-isolated data room; no data was shared with the developers of the application, and no original matched data was taken outside the secure data room.

[27] For instance, CNAE 4722-9/01 is Retail of meat and CNAE 4753-9/00 is Retail of household appliances, video and photo equipment. Appendix B describes in details how we mapped the CNAE codes of sellers into consumer spending categories.

[28] 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, 2019).

slightly larger in the analysis sample (97.4% vs. 97.1%). The shares of females (43.6% vs. 39.2%) and white workers (70.3% vs. 69.4%) were also larger in the analysis 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 analysis sample, e.g., the share of workers with a high school degree was 7.3pp higher (72.8% vs. 65.5%). Accordingly, workers in the analysis sample were "positively selected," i.e., they had more tenure (30.20 months vs. 29.26 months) and a higher monthly wage (R$1541 vs. R$1467) 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 between the two samples in Figure 3a (see Table 1 as well). One explanation for the relative comparability of workers in the two samples is that a large share of adults in São Paulo have smartphones (e.g., 91% in our survey of UI applicants; see Appendix Table C1).

Figures 3b-3d also show that the two samples are relatively comparable in terms of UI take up and formal reemployment outcomes. The patterns in these figures also provide useful background information on displaced formal workers in São Paulo (Gerard & Gonzaga, 2016, show the same patterns for the whole country). Figure 3b 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 analysis sample (80.2% in the benchmark 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 3b implies that most UI takers exhaust their potential UI duration. In fact, the average UI duration reaches 4.22 months among UI takers in the analysis sample (4.27 months in the benchmark sample).[29]

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 3c. The hazard rate of formal reemployment is slightly higher in the analysis sample, explaining the lower UI takeup rate and average UI duration in Figure 3b. However, the hazard rate is low in both samples: in the analysis sample, it is only around 6% in months 0 and 1, and it becomes even lower when more workers start drawing UI benefits. As a result, Figure 3d, which displays survival rates in non-formal-employment, shows that about 75% of displaced workers in the analysis sample (80% in the benchmark 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 UI benefits. Yet, the hazard rate remains relatively low, reaching at most 16% per month, and 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 analysis sample; 40% in the benchmark 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 (e.g., workers fired for cause). Therefore, although there are clear incentive effects, low formal reemployment rates are likely a feature of

[29]The samples in Figure 3b are restricted to workers laid off in 2011 such that we observe their full UI spell in the UI data. We show in Appendix Figure A2 that the patterns in Figures 3a, 3c, and 3d are similar when we impose that same restriction. We also show in Appendix Figure A2 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.

labor markets with high informality rather than a consequence of job displacement insurance policies.

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

	Analysis sample (Treatment)	Benchmark sample	Difference (1)-(2)		Analysis 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	

Notes: The table displays the mean of the variables in the left-hand side column at or before layoff for displaced formal workers in the analysis 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 benchmark sample drawn randomly from the universe of displaced formal workers in São Paulo (column 2). We also compare it to the "placebo" displaced workers from the analysis sample (column 4), which constitute the control group in Section 3. Monthly expenditures are averaged over the 12 months before layoff. 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

Before using our novel source of consumer spending data in the empirical analysis, we discuss their coverage and quality for the purpose of this paper.

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 consumers 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 consumers' 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 1.3, the consumption approach used in the UI literature to inform the insurance value of job displacement insurance policies does not require that one observes the total of all consumer spending 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 other panel dataset of expenditures in Brazil to which we could benchmark our data.

We thus provide two supporting pieces of evidence. We first show that the coverage of our data is relatively constant when we look at a cross-section of income levels. Figure 4a displays binscatters of the

correlation between log(expenditure) and log(wage) for formal workers in our data[30] 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.[31] The y-axis corresponds to the logarithm of the average monthly expenditure in the previous 12 months in the household (survey data) and as reported by the consumers in our sample (our data).

Figure 4a shows that our data capture a sizable share of households' total expenditures in the survey data. Specifically, for a given wage level, the mean expenditure in our data is equal to 17% of the mean expenditure in the survey data on average. The difference may come from three factors. Our data do not cover VAT-excluded items, such as services or rental costs, which account for about 35% of household expenditures in the survey data. Accordingly, when we restrict attention to categories that could be reported in our data, namely purchases of goods, the mean expenditure in our data increases to 26% of the mean expenditure in the survey data (see Appendix Figure B2). Additionally, the consumer may not provide her ID number for all her purchases and she may not be making all the households' purchases.[32] Importantly, the wage-expenditure gradient is similar in the two datasets (this is also true when using comparable categories in Appendix Figure B2), indicating that the coverage of our data is relatively constant across income levels.

Ideally, we would also show that the coverage remains constant over the panel dimension of our data, in particular across employment and income shocks. As mentioned above, there is no other panel dataset of expenditures in Brazil to benchmark our data in such a way. However, we can compare consumer spending responses to income changes that we observe in our data with evidence from other contexts. For instance, Figure 4b displays a binscatter of the correlation between $\Delta log(expenditure)$ and $\Delta log(wage)$ in our data;[33] the correlation of .269 is comparable to the correlation between changes in spending and labor income in Baker (2018), who uses data from a personal finance website in the U.S.

In the next sections, we will study how consumer spending changes following employment shocks, and compare our findings to evidence from other contexts. The evidence provided in Figure 4, together with the fact that we also replicate other relevant empirical patterns from the literature (based on different sources of data from different contexts) in the next sections, suggests that our data will likely capture actual changes in consumer spending after layoff rather than changes in data coverage. Moreover, we provide further evidence throughout our analysis that our findings are unlikely driven by coverage responses such as changes in the likelihood that workers provide their ID number when making purchases after layoff, changes in the proportion of households' purchases made by displaced workers after layoff, and increases in the share of workers' purchases made in informal stores after layoff.[34]

[30] To focus on consistent participants in the NFP program, we restrict the sample to the first year for which we know whether consumers in our sample were already participating in the previous year (i.e., 2011).

[31] We randomly assign a simulated interview date to formal workers in our data as described in Appendix B. This appendix also includes similar binscatters for other aggregations of expenditures.

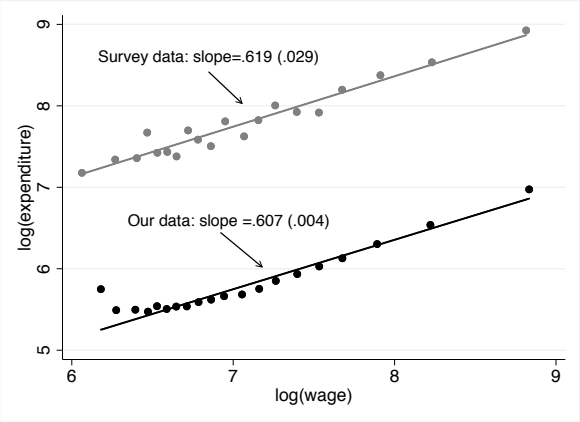
[32] The NFP program creates incentives for households to provide the same ID number when making a purchase, independently of the identity of the consumer, as it gives participants one lottery ticket for each R$50 in total reported purchases. In our survey of UI applicants, 62% of respondents who participate in the NFP program report using a unique ID number for all households' purchases.

[33] We exploit change 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 (we have better wage data for December in each year). 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.

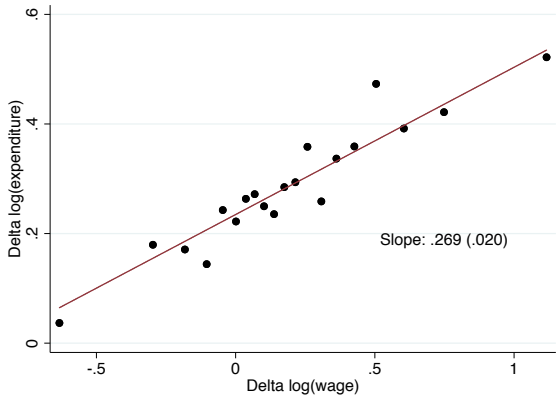
[34] In the first case, changes in time availability and other resources at layoff should not necessarily change the propensity to ask for receipts as participation costs are relatively low. Additionally, we find very different patterns in consumer spending after displacement for workers with different benefit eligibility but similar changes in time availability (i.e., laid off vs. fired); we find very similar patterns

FIGURE 4: BENCHMARKING OUR EXPENDITURE DATA

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



(b) ΔWage and ΔExpenditure in our data



Notes: The figure provides 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 data) and on December 31st (our data). The y-axis corresponds to the logarithm of the average monthly expenditure in the previous 12 months in the household (survey data) and as reported by the consumer (our data). Panel (b) 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, regressing $\Delta_{t-1}^{t+1} log(expenditure)$ on $\Delta_{t-1}^{t+1} log(wage)$ and year fixed effects, where t is the first year in the new job.

3 Expenditure profile around displacement

To measure changes in consumption after layoff, we conduct a number of event studies around the month of dismissal. Specifically, we present the results of a difference-in-differences analysis in which we estimate changes in consumer spending for displaced workers in the 12 months before and after layoff, compared to a control group of workers employed over the whole 25-month period. We begin by laying out our empirical strategy. We then present our main results, in which we consider all spending categories together, we investigate relevant differences across categories (e.g., durables vs. non-durables) and worker characteristics (e.g., females vs. males), and we link our results to the existing job displacement insurance policies. As a reminder, given the focus of this paper on the difference in *benefit payout schemes* – i.e., lump-sum vs. state-contingent – between job displacement insurance programs, we refer to all resources made available to workers in a lump-sum fashion at layoff under the term "SP" (or "SP amount") from here onwards.

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

at layoff for workers with similar benefit eligibility but different changes in time availability (i.e., workers reemployed immediately vs. remaining non-employed); and we find clear changes in consumer spending following income changes unrelated to time availability, i.e., after UI benefit payment dates and UI exhaustion. In the second case, we show that our results are very similar for men and women, which 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 in the Brazilian context. In the third case, we show that the long-term loss is even larger for a consumption spending category for which the informality margin is essentially irrelevant, i.e., pharmacy expenditures.

the whole 25-month period to net out overall trends in the data in a difference-in-differences specification:

$$y_{itk} = \alpha_i + \alpha_k + \alpha_t + \sum_{k=-12}^{k=12} \beta_k \cdot Treat_i + \epsilon_{itk}, \tag{6}$$

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 traces differential changes in consumer spending 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 with expenditure data 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).[35] These 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 her placebo statutory job displacement insurance benefits 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.

Columns (1), (4), and (5) in Table 1 show 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 5a shows that expenditure levels evolved similarly in the two groups prior to layoff, which supports the common-trend assumption underlying the specification in equation (6). Figure 5a 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.[36] 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).[37] 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 first decrease quickly, then appear to plateau, but decrease again faster 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 3b), but we 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 (see Figure 3d).

We use the specification in equation (6) 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

[35] 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.

[36] As we already netted out month fixed effects, the trend in the graph indicates the presence of possible tenure or age effects.

[37] In our survey of UI applicants, 63% of respondents report having learned about their upcoming layoff prior to the layoff month; Among those, the average worker learned about it 2.96 months prior to the layoff month (see Appendix Table C2).

(and thus UI benefit), total SP amount, and average expenditure prior to layoff (over $k = -12, ..., -6$).[38] 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.[39]

3.2 Main results

Figure 5b presents our first difference-in-differences results. The black line displays estimates using the whole treatment and controls groups as in Figure 5a (*unconditional* sample). The grey 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 prior to layoff (the black and grey 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 3d. This is the sample that could receive job displacement insurance benefits contingent on non-formal-employment.[40]

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, workers may be less likely to be asked or to choose to work overtime, or they may postpone some expenditures until they receive their SP amount.[41] We do not focus on this pattern because it is not always robust across samples and categories of expenditures (see below).

The most striking pattern is that expenditures increase by 31.4% and 37.6% in the month of layoff and in 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 3c), 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 12 months after layoff of -14.1% and -17.3% in the unconditional and survival samples, respectively.

Composition effects. Figure 5c shows that the patterns in Figure 5b 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).[42] 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

[38] We define cells by quintile of the wage, quintile of the total SP amount at layoff, 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.

[39] We avoid using specifications in logarithms because consumers do not report positive purchases in all months for all categories of spending we analyze. In the robustness checks, we show that our results hold for treatment effects at the median (see below).

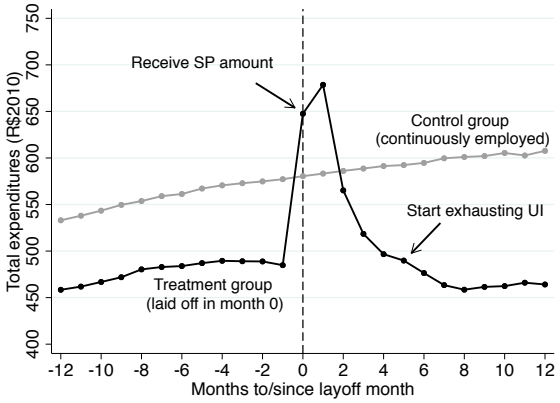
[40] Given that the survival sample changes over time, we divide the $\widehat{\beta_k}$'s coefficients by the mean in the reference month for workers who still belong to the sample in month k. We also re-weight the observations in the survival sample such that the 250 cells (wage $\times$ total SP amount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

[41] In our survey of UI applicants, 13% experienced a slower wage growth in the months before layoff (see Appendix Table C2).

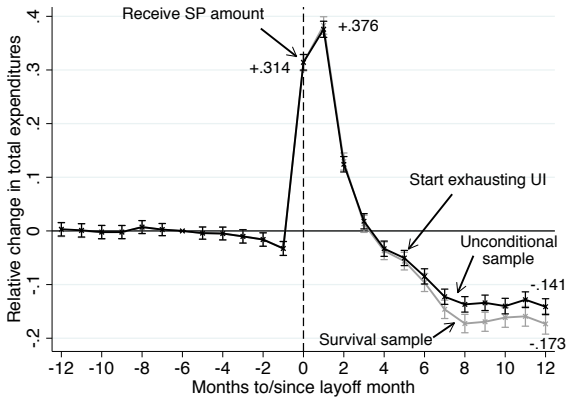
[42] We also re-weight observations in the conditional sample such that the 250 cells (wage $\times$ total SP amount $\times$ pre-layoff expenditure) have the same weight as in the unconditional sample (and the control group). Unless mentioned otherwise, we always follow the same reweighing strategy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

FIGURE 5: EXPENDITURE PROFILE AROUND DISPLACEMENT EVENT

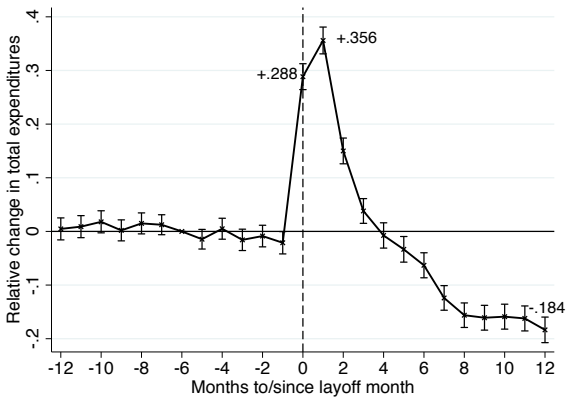
(a) Treatment vs. control (raw averages; unconditional sample)



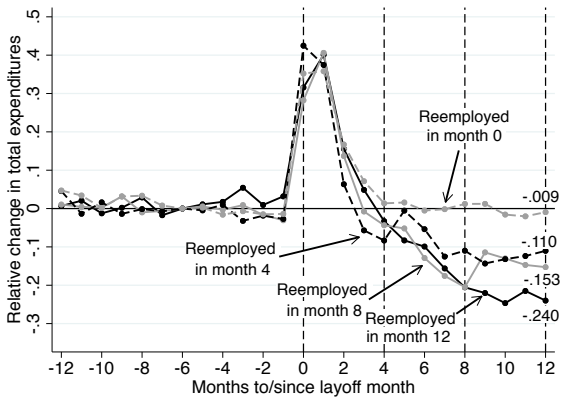
(b) DD estimates (unconditional and survival samples)



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



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



Notes: 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 (6); 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 grey 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. As a reminder, we refer to all resources made available to workers in a lump-sum fashion at layoff under the term "SP" (or "SP amount") in the analysis (and thus in this figure).

as for the survival sample afterward, with a long-run loss of -18.4%.[43]

Reemployment. Figure 5d 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 as comparison in each case). 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 5d. 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.[44] Second, workers reemployed immediately are the only group that does not experience any significant long-run loss. A likely reason for 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.[45] 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 Appendix Figure A3: 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 (and thus earning informal income).

Robustness checks. The patterns in Figure 5 are not due to outliers or to the fact that some consumers are not reporting positive purchases in all months. We show similar patterns in Appendix Figure A4 for the median or restricting attention to workers who report positive purchases in our data in all months prior to layoff.[46] Moreover, we obtain similar patterns 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. This addresses possible concerns of endogenous layoffs, e.g., of workers triggering their layoff to access their FGTS balance.

In sum, we find a large increase in expenditures at layoff despite a sizable long-run loss. The long-run loss that we estimate is comparable to existing estimates in the literature (see, e.g., Ganong & Noel, 2018, for evidence for the U.S.), despite the high informality in our context. In contrast, existing studies typically 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 26% of household expenditures on goods (see discussion of Figure 4a), we obtain a marginal propensity to spend on goods of .47 over the first three

[43] In month 12 after layoff, the survival and conditional samples coincide, but our estimated effect differs 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.

[44] 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.

[45] 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.

[46] The results for the median also address possible concerns that our findings might be due to displaced workers making purchases in order to invest in a small business at layoff (e.g., buying goods for resale). Only 4% of respondents in our survey of UI applicants report planning to use the lump-sum amounts they receive upon layoff to invest in a business (see Appendix Table C3). So any such behavior is likely concentrated in a small subset of displaced workers, and is thus less likely to affect the median results.

months after layoff. This figure is likely a lower bound as we use the statutory SP amounts and our survey of UI applicants indicates that firms' compliance with their obligations to displaced workers is not perfect.

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). This highlights the importance of using high-frequency data. With yearly data, we might have concluded that formal workers were relatively well insured against the consequences of job loss. However, Figure 5 shows that this average drop masks a very non-smooth consumption pattern across months, which matters given that the average marginal utility of consumption can differ substantially from the marginal utility of average consumption.

3.3 Heterogeneity by expenditure category

We interpret the findings in Figure 5 as capturing changes in consumption because these findings are robust across expenditure categories and, as we show here, are not driven by the purchase of durable goods.

Figure 6a displays the composition of expenditures before layoff (average expenditures from $k = -12$ to $k = -6$) in the treatment group (we show that it is very similar for the control group in Appendix Figure A5), the main categories being food (mostly groceries; about 39.5%), other non-durables (mostly personal goods; about 37%), and durables (about 10%).[47]

Figure 6b 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 Appendix Figure A5). 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 6c and 6d, which display results for non-durables and durables in relative terms for the unconditional and survival samples (as in Figure 5b). 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). We reinforce these points by displaying results for two subcategories of non-durables in Figures 6e and 6f, namely food and entertainment expenditures.[48] 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 (see discussion in Section 1.3). We show similar results for grocery expenditures separately in Appendix Figure A5. 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).

[47] 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 for renovations and construction. 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. See Appendix B for more details on our definition of consumer spending categories.

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

A remaining concern for interpreting our results as consumption responses is the possibility of consumers 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.[49] 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 Figure A5). Pharmacies in Brazil are comparable to those in the U.K.: they do not sell the same wide range of products as in the U.S., but they do not sell only drugs as in some continental European countries. The household survey used in Figure 4a 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 also less likely to be "bingeable," so we should expect a smaller increase at layoff. We still observe a significant increase of about 4%; importantly, 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. We replicate Figure 4a for non-durables in Appendix Figure B2. The coverage of our data is relatively constant across income levels for non-durables as well, and the mean non-durable expenditure in our data is equal to 35% of the mean non-durable expenditure in the survey data. Using this figure and comparing again the increase in non-durable expenditures to the increase in financial resources, we obtain a lower-bound for the marginal propensity to spend on non-durables of .22 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 7a 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 remains 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 of fired workers is relatively small (4,041 events), and estimates for that sample are less precise.

Estimates are close to 0 for both laid off and fired workers prior to displacement, but they clearly diverge upon displacement. Fired workers experience a *drop* in consumption that reaches -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 following months 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 (see Appendix Figure A6; this is consistent with incentive effects). The estimates converge at the end of the 25-month window; by then, survival rates in non-formal-employment are similar in the two samples.

Variation in UI replacement rates. Figure 7b compares difference-in-differences results for workers who were eligible for different UI replacement rates. We use variation in wages, which determine the UI benefit level. 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.[50] The average UI replacement rates in the top

[49] 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.

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

and bottom wage quartiles are 41% and 80%, respectively. Tenure levels are also generally higher in the top wage quartile, implying that workers' total SP 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 amount. In particular, we use the following specification:

$$\begin{aligned} y_{itk} =& \alpha_i + \alpha_t + \sum_{q=\{1,4\}} \sum_{k=-12}^{k=12} (\alpha_{kq} \cdot \mathbb{1}\{Wage_i \in Q_q\} + \beta_{kq} \cdot Treat_i \cdot \mathbb{1}\{Wage_i \in Q_q\}) \\ &+ \sum_{k=-12}^{k=12} (\delta_k \cdot f(Tenure_i) + \eta_k \cdot Treat_i \cdot f(Tenure_i)) + \epsilon_{itk}, \end{aligned} \tag{7}$$

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 difference-in-differences estimates for each quartile, separately. The coefficients δ_k's and η_k's control for tenure effects that are allowed to differ in the treatment and control groups.[51]

Figure 7b displays the estimated β_{kq}'s (divided by the mean in the reference month for treatment workers in the respective quartile) using the unconditional sample.[52] The increase at layoff is slightly larger for workers with higher UI replacement rates, but the most interesting difference appears in months 3 to 5 after layoff. While consumption continues to decrease in the top wage quartile, it remains relatively 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 7b 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 particularly striking; it will become clearer in Section 4, where we present the analysis of event studies centered around UI exhaustion.

Variation in total SP amount. We follow a similar approach to compare difference-in-differences results for workers who were eligible for different replacement rates from the total SP amount. We compare workers in the top and bottom quartiles of the tenure distribution in the treatment group, after restricting 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 (7), replacing $Tenure_i$ by $Wage_i$ and vice-versa. On average, the total SP amounts reach 428% and 670% of the pre-layoff wage in the bottom and top tenure quartiles, respectively.

Figure 7c displays the estimated $\widehat{\beta_{kq}}$'s (divided by the mean in the reference month for treatment workers in the respective quartile) using the unconditional sample.[53] The estimates in months 0 and 1 are much larger in the top quartile (+30.1% and +42.8% vs. +26% and +32.4%) and they converge by month 5, such that the increase in consumption is concentrated in the first months after layoff. This differential pattern in consumer spending around the time that lump-sum benefits are disbursed links more clearly the increase in consumption at layoff to these benefits. We obtain a lower bound for the marginal propensity to spend

[51] We use a third-order polynomial in tenure for the function $f(\cdot)$. We do not use weights when estimating the specification in equation (7) given that we exploit variation in wages and control for the other source of variation in total SP amount, directly.

[52] Results for the survival sample, as well as differences in survival rates, are presented in Appendix Figure A6.

[53] Results for the survival sample, and differences in survival rates, are presented in Appendix Figure A6.

on non-durables of .15 by following a similar approach as before but comparing the differential change in consumption and the difference in statutory SP amounts between the top and bottom quartiles. One would expect the marginal propensity to consume to decrease once workers already receive a large SP amount (as in the bottom quartile). Workers also likely increase spending categories that are not captured in our data. For instance, in our survey of UI applicants, 22% of respondents report planning to use the lump-sum amounts they receive upon layoff to repay debts (see Appendix Table C3).

3.5 Heterogeneity by worker characteristics

Finally, Figure 8 presents heterogeneity in our results by worker characteristics that are relevant to further describe the setting. Each line in each panel comes from a separate regression for a specific subsample of workers, using the specification in equation (6) for non-durable expenditures and re-weighting the respective subsample such that it compares better to the overall treatment group.

Panel (a) shows that the estimated consumption profiles 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 Appendix Figure A7).

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 may lose relatively more from remaining without a formal job.

4 Expenditure profile around UI payment dates and UI exhaustion

The evidence so far shows that workers increase consumption sharply at layoff despite experiencing a sizable long-term loss. Moreover, workers do not seem to smooth consumption in anticipation of the negative income shock at UI exhaustion. 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 also use the detailed information on UI payment dates in our data to investigate the patterns of consumer spending around UI payments within a month. We end by presenting the results of a Regression Discontinuity (RD) design, in which we compare the consumption and reemployment patterns of workers who were eligible for different potential UI durations. This analysis complements our findings (the sample size is small, so estimates are less precise) and provides out-of-sample moments to evaluate the predictive power of our

estimated models in Section 5. The sources of variation that we exploit in this section – a predictable drop in income at UI exhaustion, predictable disbursements of UI payments, and a predictably longer potential UI duration – provide further evidence on the sensitivity of displaced workers' consumption to the timing of benefit disbursement, and help shed light on the mechanisms behind our findings as discussed in Section 5.

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:

$$y_{itk} = \alpha_i + \alpha_t + \sum_{k=\underline{k}}^{k=\overline{k}} \beta_k + \epsilon_{itk}, \tag{8}$$

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 9a presents the results of an event analysis based on the specification in equation (8), 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 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, 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 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 (the month fixed effects are based on the month of the first day in each time period).

Figure 9a 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 Appendix Figure A8 that we 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, 2018). 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 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 9a 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 9a 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 9a could be affected by composition effects because the hazard rates of formal reemployment increase after UI exhaustion (see Figure 3c): 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 9b 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 9a and 9b 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 9c and 9d show the results from such an analysis, for a window of 90 days before and 150 days after the last UI payment date, for the same samples of UI exhaustees as in Figures 9a and 9b, respectively. 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 9a and 9b) and because payment dates can fall on any day within a month.

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 9c, 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 9d, there is a slight decrease in consumption starting in period 0 (although our estimates are only statistically significant from period 1 onward). The fact that the spike for the 7-day period after the last UI payment is as high as in previous months in Figure 9b indicates that the slight decrease in consumption in period 0 in Figure 9d comes from a decrease in consumption levels towards the end of the 30-day period. It is thus not the case that workers who remain without a formal job, and who experience a larger drop in consumption after UI exhaustion overall (12%), better smooth their consumption through the drop in income at UI exhaustion.

We show similar patterns in Appendix Figure A9 for the median and find again no evidence of consumption smoothing in anticipation of UI exhaustion, despite a sizable drop in consumption in later periods.

4.4 Regression discontinuity analysis

We end this section by presenting complementary results from a RD analysis, in which we compare workers eligible for four vs. five monthly UI payments. We exploit the same RD design as in Gerard & Gonzaga (2016) who provide more in-depth discussion and description of the institutional details. We follow their sample restriction and specification choices, but we consider a different outcome, i.e., non-durable expenditures. To the best of our knowledge, this is the first study of consumption responses to quasi-experimental variation in potential UI duration, although the sample size for our analysis is relatively small.

Sample. As described in Section 2, 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; Gerard & Gonzaga, 2016, show that the layoff density is not smooth around the other cutoffs).

Our main sample of analysis includes the subset of displaced workers in our data 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 $f\left(T_i\right)$:

$$y_i = \alpha + \beta \cdot 1\left(T_i \geq 0\right) + f\left(T_i\right) + \epsilon_i, \tag{9}$$

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 and cluster standard errors at the tenure level. We then address the ambiguous eligibility of workers with 22 to 24 months of tenure by excluding them from our regressions (we are left with a bandwidth of six months below 22 and above 24 months of tenure, including 7,613 observations; 2,028 observations have UI data). With this "donut hole" approach, which we illustrate in Figure 10 (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. We provide evidence supporting the validity of our RD design in Appendix Table A1 and Appendix Figure A10. 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 (9).

Results. First, we present results for UI outcomes, using the subsample with UI data. Figure 10a 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, 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.[54]

Figure 10b 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 (9) 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 10c 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 limited effect on formal reemployment in anticipation of UI exhaustion in our context. 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 10d displays RD estimates in each month since layoff *for the median*.[55] 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

[54] The increase in the share who drew five UI payments starts at lower tenure levels, and is less sharp, if we do not selected workers with a single formal job spell in the previous 36 months. We show in Appendix Figure A11 that we find no effect on UI takeup.

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

marginally significant (at the 10% level) and reaches 25.1% in months 5; it peaks at 40.2% and is significant at conventional levels in month 6. These results add further evidence of excess sensitivity of consumption to cash-on-hand. Workers know how much they are eligible for once their UI claim is approved, but the only detectable effect on consumption occurs precisely around the time the additional benefits are disbursed (we show in Appendix Figure A11 that estimates *for the mean* are not significantly different from 0). This result is thus consistent with the lack of consumption smoothing in anticipation of UI exhaustion in Figure 9.

5 Models of behavior and policy implications

Taken together, our results show that consumption can be very sensitive to cash-on-hand even in a context where inflows of liquidity are triggered by a salient negative shock. In this section, we shed light on the mechanisms behind our findings, which is critical for their policy implications. We begin by reviewing mechanisms that have been proposed in the literature to explain the excess sensitivity of consumption to cash-on-hand in various contexts, and we contrast their predictions to the results presented in the previous sections. This discussion guides a second step in which we follow a recent strand in the UI literature and investigate simple structural models of behavior that can fit our data. Specifically, we show that allowing workers to be present-biased in an extension of the standard model in Section 1 can, on its own, rationalize our findings. We end by using our estimated models to illustrate the incentive-insurance trade-offs under counterfactual job displacement insurance designs, highlighting the key role that differences in *disbursement* policy can play when consumption is highly sensitive to the timing of benefit payments.

5.1 Mechanisms of excess sensitivity to cash-on-hand

As discussed in Section 1, our key findings are at odds with the predictions of standard models of job-search and consumption with liquidity constraints and forward-looking agents in the UI literature (Card *et al.* , 2007; Chetty, 2008). Such models would fail to predict that workers increase consumption sharply when receiving their SP amount and fail to save in anticipation of the negative income shock at UI exhaustion.

We show below that a simple way to generate both a high propensity to consume out of cash-on-hand and a low propensity to save in anticipation of a negative shock is to allow for workers to be present-biased (Laibson, 1997). There is a large literature arguing that individuals are often present-biased when making inter-temporal choices, including job-search decisions (e.g., DellaVigna & Paserman, 2005). Moreover, present bias may be particularly strong after layoff as negative shocks and stress may increase time-discounting (e.g., Mullainathan & Shafir, 2013; Haushofer *et al.* , 2013; Haushofer & Fehr, 2014). This mechanism is also consistent with findings from our survey of UI applicants. A majority 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 mention the need "to control expenditures" or "to not spend it all at once" to explain their choice (see Appendix Table C4). Finally, this mechanism has the advantage of providing a policy rationale for the existence of forced savings accounts, such as the one that exists in Brazil.

There are a number of other mechanisms that have been proposed to explain the excess sensitivity of consumption to cash-on-hand in various settings. However, these mechanisms would not explain both the spike in consumption at layoff and the lack of savings in anticipation of UI exhaustion, at least not on their own. For instance, a class of models with forward-looking but liquidity-constrained agents can explain why expenditures may be very sensitive to cash-on-hand for transitory positive income shocks (Kaplan &

Violante, 2014). Near rationality could generate this prediction in situations where the loss from failing to smooth consumption is small (Kueng, 2018). This is unlikely the case in our setting as the inflow of liquidity is triggered by a salient negative shock and workers experience a sizable long-run loss in consumption. Workers may also discover special "lumpy" consumption needs over time that they cannot meet unless they have enough liquidity in hand (Campbell & Hercowitz, 2018). This could explain the larger response of durable expenditures at layoff, but our main results are not driven by durable expenditures. More importantly, this class of models cannot explain why agents would fail to save in anticipation of an expected negative income shock, such as the drop in income at UI exhaustion. This point, which was made recently by Ganong & Noel (2018) in the U.S. context, is even more striking in the context that we study. Displaced workers tend to have limited financial resources at layoff in the US. In contrast, given the size of the SP amount that they receive at layoff, displaced workers in Brazil would have the liquidity to maintain their consumption levels well beyond UI exhaustion if they dissaved these financial resources more slowly.

Models in which displaced workers have biased beliefs about their reemployment probabilities (Spinnewijn, 2015), or in which they are inattentive about or underestimate the drop in income at UI exhaustion, could predict a lack of savings in anticipation of UI exhaustion. However, these workers would not have a particularly high propensity to consume out of cash-on-hand. For instance, we show below that even workers with overly optimistic beliefs would not increase consumption sharply at layoff, because of diminishing marginal utility of consumption, and would therefore arrive at UI exhaustion with enough liquidity to perfectly smooth consumption given the size of the lump-sum amounts in our setting.

Complementarities between consumption and leisure are also unlikely to rationalize our findings. Models in which consumption spendings are substitutes with leisure (e.g., because of home production; Aguiar & Hurst, 2005) would neither explain the sharp increase in consumption at layoff (displaced workers likely have more time in hand) nor the drop in consumption at UI exhaustion (displaced workers do not have more time in hand after UI exhaustion). Models in which consumption is a complement to leisure would not explain why the sharp increase in consumption at layoff is similar for workers reemployed immediately in Figure 5d (these workers do not have more time in hand after layoff as they remain employed full time).

Finally, by construction, models with saving constraints would generate a high propensity to consume and a low propensity to save, and could thus be consistent with our findings. However, it is unlikely that agents face severe savings constraints in the population that we study, namely workers in urban São Paulo who are attached to the formal labor market. Technological constraints to save do not seem to be an issue as most workers in our sample likely have bank accounts (e.g, the share with bank accounts is 93% in our survey of UI applicants; see Appendix C). Expenditure pressures from individuals' kin network (i.e., kinship taxation; Squires, 2018), which have been shown to be important in some developing country contexts, could generate savings constraints. However, only 2% of UI applicants in our survey mention transfers to friends and family as an important category of spending for their lump-sum amount (see Appendix C). Arguably, expenditure pressures from one's kin network may be weaker after a salient negative shock. Nevertheless, we explore this mechanism in Appendix A and show that it would require unrealistically severe savings constraints to explain our findings, corresponding to a monthly interest rate of -16%.[56]

[56]This could also capture a high perceived inflation, but by Brazilian standards, inflation was low between 2010 and 2014, and workers could keep their income in an account at the same bank that holds the forced savings accounts to protect it against inflation.

5.2 Empirical moments for the structural models

For simplicity, the empirical moments that we use as "targets" to be matched by our structural 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 all workers take up UI and exhaust UI in the same months (month 1 and month 5, respectively), which allows us to align the months to/since the layoff event and the UI exhaustion event.[57] Figure 11a displays our first set of moments: the hazard rate of formal reemployment in each month since layoff. The hazard rate, which is increasing over the UI spell,[58] increases more sharply than in Figure 3c, peaking just after UI exhaustion (in month 6) and decreasing in subsequent months. Figure 11b displays our second set of moments: difference-in-differences estimates for non-durables expenditures using the *survival* subset of our estimation sample. The increase in consumption at layoff is sharper than in Figure 6c, peaking in month 0 at +48.9%, but the cumulative increase over the first 3 months is comparable. The pattern around UI exhaustion is also sharper than in Figure 6c, with the drop in consumption taking place just after UI exhaustion as in Figure 9d.

5.3 Structural model

We adapt the model in Section 1 and impose a few additional assumptions to set up a structural model that we fit to these moments. Many of our structural assumptions follow assumptions made in DellaVigna *et al.* (2017) for their "standard" model, which fit reemployment patterns in their data relatively well.[59]

Time and job search. We assume that time periods correspond to months, such that the variable $h_{i,t}$ corresponds to the probability of reemployment from one month to the next. The search cost function is assumed to take the form: $\psi_i(h_{i,t}) = \kappa_i \cdot h_{i,t}^{1+\theta}/(1+\theta)$, where the cost of search κ_i can vary across workers. The first-order condition (3) thus becomes: $\kappa_i \cdot h_{i,t}^{\theta} = \delta \cdot \left[V_{i,t+1}(a_{i,t+1}) - U_{i,t+1}(a_{i,t+1})\right]$. We also have:

$$h_t \equiv \int h_{i,t}\, di = \int \left(\frac{\delta \cdot \left[V_{i,t+1}(a_{i,t+1}) - U_{i,t+1}(a_{i,t+1})\right]}{\kappa_i} \right)^{1/\theta} di \tag{10}$$

The parameter θ is thus the inverse of the elasticity of search effort with respect to the net gain from reemployment in period $t+1$. Equation (10) 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 and the cardinal utility of consumption – to match the hazard rates in Figure 11a.

Utility of consumption. The per-period utility of consumption is assumed to be $u(c_{i,t}) = ln(c_{i,t})$.

Wage and benefits. We set the wage before layoff, the UI replacement rate, and the replacement rate from the total SP amount at their (statutory) mean in the 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 the wage growth for formally reemployed workers in the estimation sample is 14.4% lower than in the control group.[60]

Saving and borrowing. Workers are assumed to have zero assets at layoff, they cannot borrow against

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

[58] The decrease in the hazard rate in the first months after layoff in Figure 3c is driven by workers who do not takeup UI.

[59] 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 because, on its own, reference-dependence would not predict the most striking pattern in our data, namely 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.

[60] 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.

their future income, and any savings yield zero interest, $r = 0$. Workers often have limited liquid savings at layoff (see, e.g., Chetty, 2008, for the US). In our survey of UI applicants, for instance, more respondents had positive debts than positive savings (59% vs. 43%; see Appendix Table C1).

Other income and informality. We introduce two additional assumptions to account for 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 *exogenous* source of income to finance their consumption that is constant over time. In our survey of UI applicants, the average number of adults in their household was 1.99, and their household income before layoff was much higher than their own wage income (see Appendix C). Specifically, we 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 generate some income *endogenously* by engaging in informal work activities ($l_{i,t} \geq 0$) while not yet formally reemployed. This is a natural assumption in the Brazilian context, but a sizable share of workers engage in informal work activities to supplement their income during unemployment even in richer countries (see, e.g., Long & Polito, 2017). 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 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 in equation (1) thus becomes:[61]

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

Note that the function $\phi(l_{i,t})$ could capture other (costly) income-generating activities, such as added-worker effects, although it is natural to refer to informal work activities in our context. This innovation compared to DellaVigna *et al.* (2017) and Ganong & Noel (2018) allows us to match the consumption and reemployment patterns in our data without assuming exogenous income when not formally reemployed, without assuming that (some) workers experience more dramatic changes in consumption after UI exhaustion than observed empirically, and without introducing heterogeneity in time-discounting.

Imperfect compliance and alternative purposes for the lump-sum at layoff. We assume that the relative changes in non-durable expenditures in Figure 11b capture relative changes in consumption in the model. However, we have shown that displaced workers use part of their SP amount at layoff to buy durables. Moreover, 23% of UI applicants in our survey plan to use the resources they receive lump-sum at layoff to repay debts (see Appendix Table C3). There may thus be a demand for lump-sum in our setting that is separate from any job displacement insurance purpose. This is supported by findings from another small survey that we conducted with formal employees in São Paulo: 75% of respondents report that they would prefer a system granting them access to their forced savings account (FGTS) every 3 years, irrespective of layoff history (see Appendix Table C4; this figure is larger than the 47% who would prefer their monthly forced savings to be added to their wage instead). We thus allow the SP amount at layoff to be a free parameter bounded between zero and the full statutory amount, such that we allow part of the lump-sum to serve purposes that are not captured by our model (e.g., repaying debts). This also accounts for the possibility that firms do not comply perfectly with their obligations at layoff (see Section 2.3).

Estimation. In our estimations, we allow for two worker types in proportion s_1 and $(1 - s_1)$. We assume

[61] We assume that informal work does not crowd out search because time-on-hand does not seem to be a binding constraint for displaced workers (Krueger & Mueller, 2012) and because our empirical moments would not identify such crowd out in practice.

that they have different search costs, κ_1 and κ_2, but a same cost of informal work χ. The vector of free parameters ξ thus includes: (i) the search cost parameters for the two 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, θ; (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 for consumption purposes, ω. Depending on the model, we also include the monthly discount factor δ (assuming exponential δ-discounting) or the present bias parameter β (assuming quasi-hyperbolic $\beta\delta$-discounting; we assume "naiveté" following DellaVigna *et al.* , 2017) in the vector ξ.

For a given vector ξ, the model is solved by backwards induction. 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 moments in Figure 11 (starting in period 0), 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 hazard rates of reemployment after UI exhaustion (through dynamic selection), 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 θ is identified by the change in the hazard rates around UI exhaustion. 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 or the present bias parameter β are identified by the increase in consumption at layoff and the slope of the consumption profile afterward (as we show below, δ and β are not separately identified). We illustrate this clearly below by showing the consumption patterns predicted by a benchmark model in which we set $\delta = .995$ and $\beta = 1$. The cost of informal work, χ, and the inverse of the elasticity of informal work, λ, are identified by the level of consumption after UI exhaustion and the size of the drop in consumption at UI exhaustion, respectively. Finally, the share of the statutory lump-sum amount used for consumption purposes, ω, is identified by the height of the spike in consumption at layoff.

5.4 Estimation results and model fit

The estimation results in Table 2 are based on $N = 300$ random draws for each model. In column (1), we first present results from a model in which we assume exponential δ-discounting and estimate the monthly discount factor δ from the data. Figures 12a and 12b show that the moments predicted by this model fit the target empirical moments closely. However, the estimated monthly discount factor is $\widehat{\delta} = .82$, which is equivalent to a yearly discount factor of .09. Thus, an unrealistically high degree of impatience ("myopia") is necessary for a model with exponential discounting to rationalize the patterns in our data.[62] The model also requires a high degree of heterogeneity in search costs: the cost of search $\widehat{\kappa}$ is 3.2 times higher for the higher-cost type than for the lower-cost type, which account for about half our sample. Finally, we estimate that about 59% of the statutory lump-sum amount is used for consumption purposes.

The results in column (1) suggest that a standard model with forward-looking workers facing liquidity constraints, such as the one used for our benchmark predictions in Figure 2, cannot fit the data. To show

[62]The yearly discount factor that we estimate is even lower than the one estimated in DellaVigna *et al.* (2017) for their standard model (a yearly discount factor of .17), which they conclude is hard to reconcile with other estimates in the literature.

this, we re-estimated the same model setting $\delta = .995$; we also set ω at its estimated level in column (1) to make sure that we compare workers with similar financial resources at layoff. The estimated parameters are presented in column (2). Figures 12c and 12d show that this "benchmark model" can fit the reemployment patterns in our data.[63] However, it cannot fit the consumption patterns. The estimated model generates a smooth consumption profile, and thus fails to predict the spike at layoff and the drop at UI exhaustion.

The degree of impatience in column (1) is too high, but this may be because we assume exponential discounting. We thus re-estimated the model with quasi-hyperbolic $\beta\delta$-discounting, estimating β from the data, but setting $\delta = .995$ as the quality of fit in Figures 12a and 12b indicates that β and δ cannot be separately identified. This model can fit the data relatively well (see Figures 12e and 12f) and the estimated present bias parameter in column (3) is $\widehat{\beta} = .71$. Therefore, a model with present bias can rationalize the patterns in our data with values that are more in lines with the literature (Ericson & Laibson 2019).[64]

Finally, we show in Appendix A that introducing biased beliefs about reemployment probabilities into the benchmark model is not sufficient to fit the data. Specifically, we solved the model again, holding fixed the estimated parameters at their values in column (2), but introducing a *baseline bias* $\overline{h}$ (as in Spinnewijn, 2015) between workers' true reemployment probabilities $h_{i,t}$ and their perceived reemployment probabilities $\widetilde{h}_{i,t} = \min\{h_{i,t} + \overline{h}, 1\}$. Optimistic workers consume more and thus save less in the first months after layoff than in the benchmark model, but these changes remain relatively small even with widely optimistic beliefs ($\overline{h}$=.9) because of the consumption smoothing pressure from the diminishing marginal utility of consumption. As a result, the model does not predict a sharp increase in consumption at layoff and, given the size of the lump-sum amounts in our setting, workers are still able to perfectly smooth consumption at UI exhaustion (unsurprisingly, workers with such beliefs do not exert any search effort). Finally, we show in Appendix A that the model can rationalize the patterns in our data without myopia or present bias by introducing savings constraints, i.e., a monthly interest rate of $\widehat{r} = -16\%$. We are thus not able to formally separate savings constraints from excessive impatience without additional data. However, as discussed above, such severe savings constraints do not appear plausible in our setting.

5.5 Counterfactual policies

Our results have relevant implications for the design of job displacement insurance as models with present-biased vs. forward-looking workers would generate very different predictions for the same policy. To illustrate this, we solved again the estimated benchmark and present bias models under three counterfactual job displacement insurance designs. Figure 13 displays the predicted hazard rates of formal reemployment and relative changes in consumption for the two models under the current system and these three policies.

Shorter potential UI duration. We consider a policy that reduces the potential UI duration to four months, mimicking the variation in the RD analysis in Section 4.4. With the present bias model, the consumption profile remains identical in all months, except that the consumption drop now takes place in month 5 after layoff when workers eligible for 4 months of UI exhaust their UI benefits. The benchmark model does not predict such a sharp change in consumption. The present bias model also predicts a sizable increase in reemployment rates at the end of the new potential UI duration, and some limited increase in reemployment

[63] Everything else equal, patient workers would search harder for a new formal job early on to self-insure against the drop in income at UI exhaustion. 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 (2) requires both much higher search costs and a much higher search elasticity.

[64] The fit is slightly better for the model in column (1), e.g., the present-bias model predicts a sharper change in hazard rates at UI exhaustion. This suggests that a smoother functional form of hyperbolic discounting or a model with both present bias and some degree of myopia might better fit the data. For instance, we show in Appendix A that this is the case if we set $\delta = .95$ and $\beta = .8$.

rates prior to UI exhaustion. Its predictions are thus consistent with the findings of our RD analysis.[65]

SP only. We consider a policy in which displaced workers are not eligible for any UI benefits; they only receive their SP in a lump-sum fashion at layoff. The predicted consumption profile for the benchmark model under this SP-only policy mirrors the one in Figure 2, with a smooth consumption path after layoff. In contrast, consumption still spikes at layoff by about 30% with the present bias model. Thus, 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. The results also suggest that the benchmark model might underestimate the incentive-insurance trade-off associated to eliminating UI. Without UI incentive effects, formal reemployment rates would increase after layoff in both models, but consumption levels would drop much faster in the present bias model, with displaced workers having exhausted all their liquidity by month 2 after layoff.

SP only but paid in installments. Given our results, an interesting (yet untested) job displacement insurance policy would be to pay the SP amount in installments, *irrespective* of workers' reemployment status. Figure 13 shows that the predictions of the benchmark model are unchanged whether the SP amount is paid in a lump-sum fashion at layoff as above or in 4 monthly installments (for comparison purposes, we keep the same SP amount as above and assume again no UI). In contrast, paying the SP amount in 4 monthly installments would induce displaced workers to smooth consumption over that period in the present bias model. This highlights the key role that the disbursement policy of job displacement insurance programs may play, on top of their contingency policy, when consumption is very sensitive to the timing of payments.

5.6 Contingency vs. disbursement policy

We can illustrate the role of the contingency vs. disbursement policy of job displacement insurance programs more systematically by studying the incentive-insurance trade-off for marginal reforms around the current system, which we can evaluate with the type of sufficient statistics often used in the UI literature.

Specifically, we solved again the benchmark and present bias models under 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 SP amount paid lump-sum at layoff; and an unconditional transfer paying the same increase in 6 monthly installments (months 0-5), irrespective of workers' reemployment status.

Table 3 displays our results. Column (1) captures the insurance value of the reforms, i.e., the welfare effect from the consumption-smoothing gains, which is measured by a sufficient statistic similar to the one in equation 15.[66] Column (2) captures the incentive effect of the reforms, i.e., the welfare effect from distorting job-search incentives, which is measured by the fiscal externality of the reforms, i.e., the ratio of the behavioral effect – the increase in budgetary cost due to behavioral responses – to the mechanical effect – the increase in budgetary cost absent behavioral responses – of the reforms (Schmieder & von Wachter, 2017). 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 corresponds to a welfare loss. Importantly, in our calculations, we assume that normative preferences are free of present bias. This is both a usual

[65] The present bias model predicts some changes in anticipation of UI exhaustion for job-search but not savings decisions because the job-search decision is at an interior solution in all periods, while the savings decision is at a corner solution by month 2 already.

[66] We have to make one small adjustment in our case. In the benchmark model, the consumption-smoothing gains can be evaluated by comparing the marginal utilities of consumption in the periods when workers receive the extra benefit vs. pay for the extra benefit, as in equation 15. We need to know when benefits are paid but not when consumption actually increases because workers are indifferent between increases in consumption or savings in any period, i.e., the first-order condition (4) holds. However, we assume that normative preferences are free of present bias, so that increases in consumption or savings in any period do not have the same impact on welfare. Concretely, we must keep track of when consumption actually changes in that case; the sufficient statistics is then a weighted average of expressions such as the one in equation 15, one for each period t in which consumption changes, with the weights being $dc_t / \sum_t dc_t$.

assumption in the literature and an assumption that is supported by the fact that UI applicants mention the need "to control expenditures" or "to not spend it all at once" (see Section 5.1 and Appendix C).

Incentive-insurance trade-off with the benchmark model. The top panel in Table 3 displays results for the benchmark model. The insurance value is similar for the increases in the SP amount paid lump-sum and in the unconditional transfer paid in installments because differences in disbursement policy are inconsequential in this model. The insurance value is higher for the increase in the UI benefit level, and even more so for the increase in the potential UI duration, because of the difference in contingency policy (i.e., the UI reforms are better targeted). Yet, the insurance values are relatively similar across the four reforms because workers are able to smooth consumption after layoff in the benchmark model. In contrast, because of the difference in contingency policy, the incentive effect is very different across the reforms. As a result, the incentive-insurance trade-off appears substantially worse for the increases in UI benefits in the benchmark model, at least for the coefficient of relative risk aversion used in this exercise ($\gamma = 1$ with log utility).

Incentive-insurance trade-off with the present bias model. The incentive effect of the UI reforms in the present bias model remains in line with the results for the benchmark model. In contrast, the insurance value of these reforms differ in important ways in the present bias model. First, the insurance value decreases substantially and becomes negative for the increase in the SP amount at layoff. This is because consumption only increases in the first few periods after layoff in this case, when consumption is already much higher than before layoff. Second, the insurance value decreases to a lesser extent for the increase in the unconditional transfer because the changes in consumption are not as concentrated in the first few months after layoff. As a result, the figures in Table 3 suggest that a reform that would marginally reduce the SP amount paid lump-sum at layoff but marginally increase the unconditional transfer paid in installments would increase welfare. This illustrates again the role played by differences in disbursement policy when consumption is highly sensitive to the timing of payments. Third, the decreases in the insurance value are comparable for the increases in the UI benefit level and in the unconditional transfer because the additional resources are disbursed over the same period; the insurance value is still higher for the increase in the UI benefit level because of the different contingency policy. Fourth, the insurance value actually increases for the increase in the potential UI duration because the increase in consumption in that case is concentrated in a period when workers are consuming much less than before layoff in the present bias model. In sum, compared to the benchmark model, the insurance value and the incentive-insurance trade-off worsen substantially for an increase in the SP amount at layoff, including relative to other reforms.[67]

The results in Table 3 are useful to illustrate the policy implications of our findings, but we are careful to not draw definitive normative conclusions from this exercise. First, although their sign would be unchanged, the insurance values would be larger (in absolute values) if we were to use a utility function with a higher coefficient of relative risk aversion. This would widen the difference in insurance values across the reforms. Second, we note that the overall welfare effect of these reforms likely includes a third component when normative and positive preferences differ, namely the impact on possible *internalities* (Lockwood, 2016), as other choices of displaced workers may not be considered privately optimal from a normative point of view in that case. For instance, present-biased workers set their job-search efforts at privately suboptimal levels (i.e., too low) from a normative point of view (DellaVigna & Paserman, 2005). This component is difficult to quantify in practice but it might be particularly important for the welfare effect of increases in UI benefits, as their contingency policy implies larger effects on job search.

[67] In the present bias model, the insurance value and the incentive-insurance trade-off also worsen substantially for the increase in the UI benefit level compared to the increase in the potential UI duration, a point also made in Ganong & Noel (2018).

6 Conclusion

The results in this paper lead to three main conclusions. First, consumption is highly sensitive to cash-on-hand, even in a context where a large inflow of liquidity is triggered by a salient negative shock. 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; workers who are eligible for one additional month of UI consume significantly more only in the periods during which they draw additional UI payments; and workers fail to smooth consumption in anticipation of the (expected) drop in income at UI exhaustion, which is associated with a drop in consumption of about 10%. We use the evidence on consumer spending responses to various sources of variation in benefits to discuss mechanisms, and we show that a simple model with present-biased workers is able to rationalize the key consumption and reemployment patterns in our data, which is in line with the recent literature (e.g., DellaVigna *et al.* 2017, Ganong & Noel 2018). Taken together, our 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. Our results also suggest that disbursement policies could play an important role in other policy contexts. For instance, the private retirement savings literature (e.g., the 401k literature) has emphasized how present bias may affect savings decision (e.g., Laibson *et al.* , 1998), but it may severely affect dissaving decisions as well given that workers can currently access their 401k balances in a lump-sum fashion at retirement in the U.S.

Second, our results indicate that researchers using a consumption approach to evaluate the insurance value of job displacement insurance programs (e.g., Gruber, 1997) 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, the growing body of evidence that displaced workers may be present-biased highlights challenges for alternative revealed preference approaches used in the UI literature to evaluate the insurance value of these programs (e.g., Chetty, 2008).

Third, the long-term consumption loss in our setting is comparable to estimates from other studies and indicates that the need for job displacement insurance may be sizable despite the higher labor market informality. This is important because the need for job displacement insurance is not obvious ex-ante in this context: government-mandated schemes can only insure formal workers, and informal work activities can, in principle, be used for self-insurance. Some of 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.

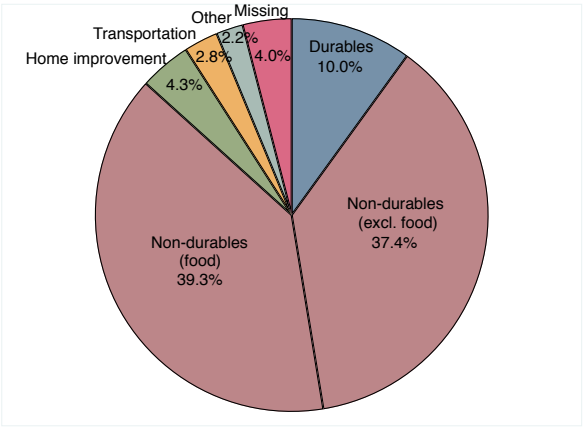
It is worth emphasizing that our results do not imply that the lump-sum disbursement of forced savings should generally be avoided. The same mechanisms that may prevent workers from dissaving their SP amount more slowly may justify the existence of forced savings to mobilize the resources necessary for lumpy investments that workers may not be able to make otherwise (e.g., Casaburi & Macchiavello, 2019). The key implication of our findings is that, if the goal of a policy is to provide insurance to displaced workers, a lump-sum disbursement could undermine this goal. Workers may still benefit from forced savings and occasional access to these financial resources in a lump-sum fashion for other purposes or at other times.

Finally, our findings shed light on three directions for future research. In terms of consumption responses, it would be interesting to understand the quantity vs. quality aspect of these responses if detailed

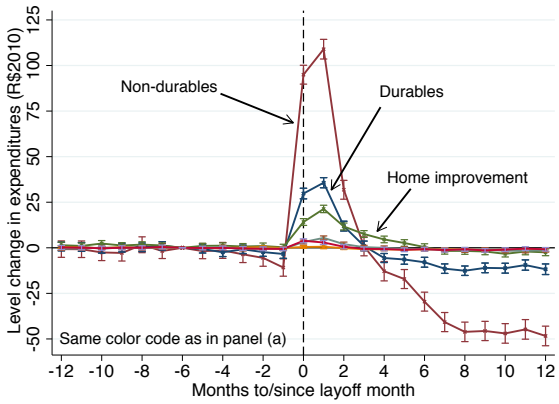
data on the specific products purchased by workers were available. In terms of job displacement insurance designs, it would be useful to build evidence on the impact of alternative disbursement schemes beyond UI and SP, such as the unconditional transfer paid in installments that we simulate in our models. In terms of developing country contexts, it would be particularly interesting to gather longitudinal data on formal and informal employment, together with longitudinal consumption data, in order to better understand the extent to which informal work activities are used as self-insurance mechanisms after the loss of a formal job.

FIGURE 6: HETEROGENEITY BY EXPENDITURE CATEGORY

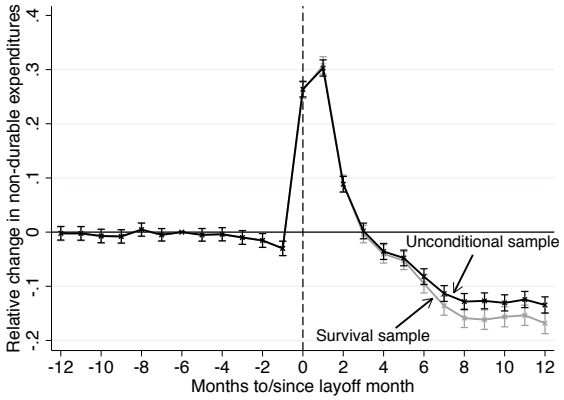
(a) Composition of expenditures (treatment)



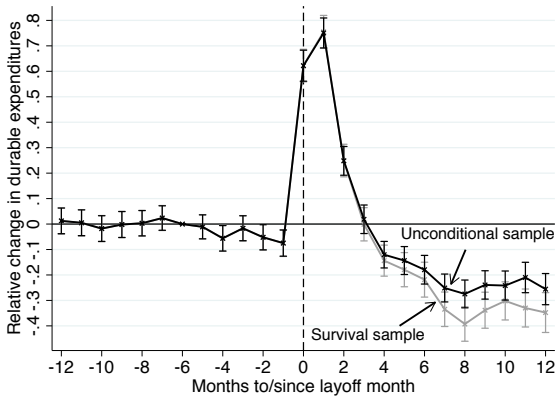
(b) DD estimates by category (unconditional sample)



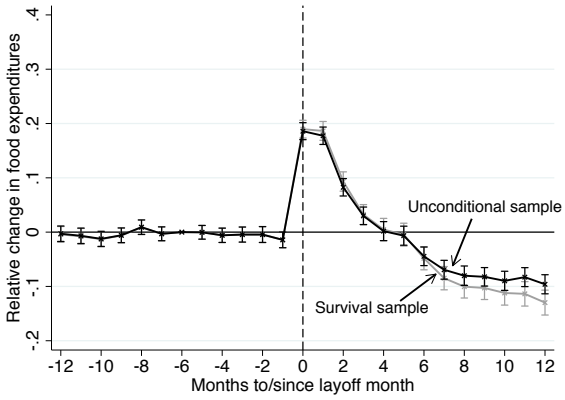
(c) DD estimates for non-durable expenditures



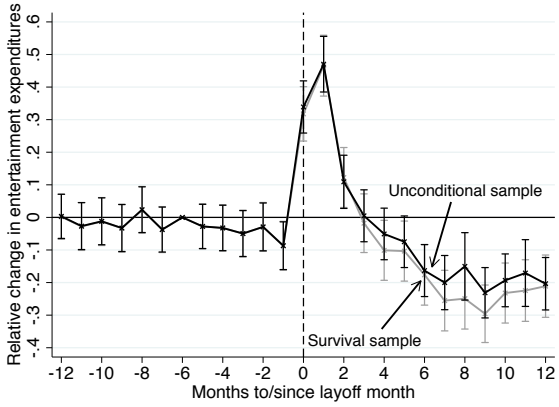
(d) DD estimates for durable expenditures



(e) DD estimates for food expenditures



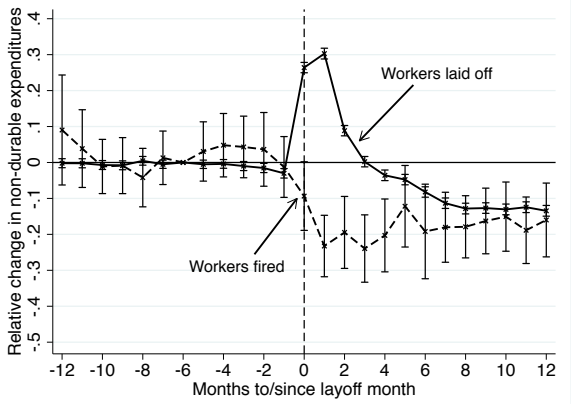
(f) DD estimates for entertainment expenditures



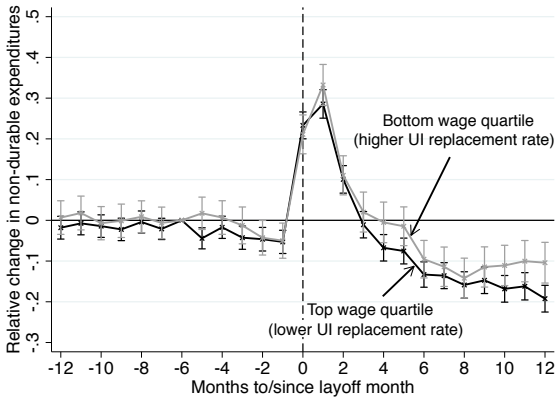
Notes: The figure presents results for different expenditure categories. Panel (a) displays the composition of expenditures before layoff (average over $k = -12$ to $k = -6$) in the treatment group. The main categories are food (mostly groceries; about 39%), other non-durables (mostly personal goods; about 37%), and durables (about 10%). The composition of expenditures is very similar in the control group (see Appendix Figure A5). Panel (b) presents difference-in-differences results in levels for the unconditional sample (point estimates and 95% confidence intervals) using the specification in equation (6) for each expenditure category, separately. 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 5a for non-durable (food and other non-durables together), durable, food, and entertainment expenditures, respectively.

FIGURE 7: LINKING OUR RESULTS TO VARIATION IN JOB DISPLACEMENT INSURANCE BENEFITS

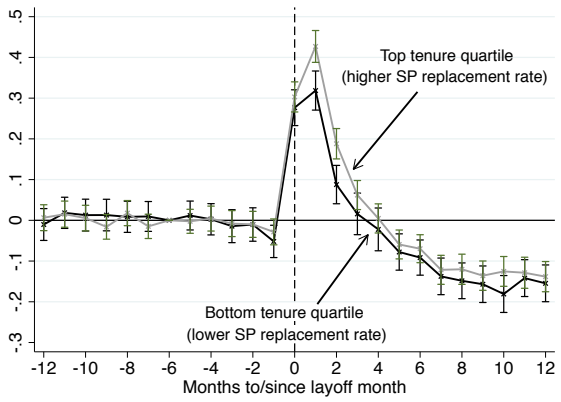
(a) Comparing DD estimates for laid-off and fired workers (unconditional sample)



(b) Comparing DD estimates for workers in bottom and top wage quartiles (top and bottom quartiles in UI replacement rates; unconditional sample)



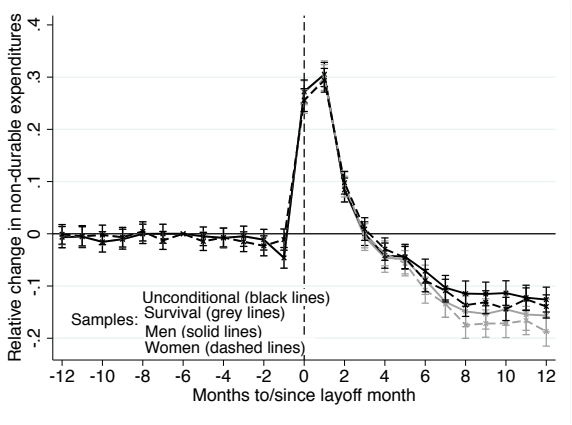
(c) Comparing DD estimates for workers in bottom and top tenure quartiles (bottom and top quartiles in replacement rates from total SP amount; unconditional sample)



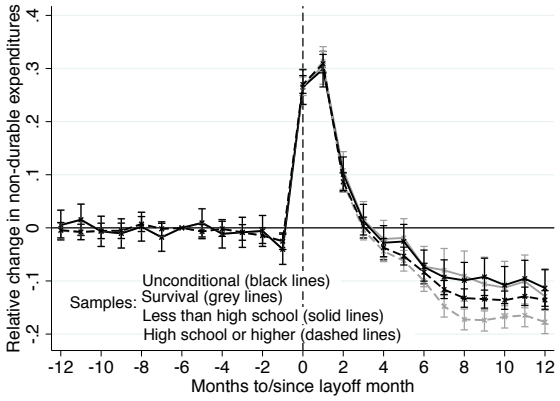
Notes: The figure present results for non-durable expenditures linking the patterns in previous figures to job displacement insurance benefits. Panel (a) displays difference-in-differences results (point estimates and 95% confidence intervals) using the specification in equation (6) for workers who were 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 the top quartile than in the bottom quartile (41% and 80% on average, respectively). We use the specification in equation (7), controlling flexibly for tenure levels at layoff to net out variations in the replacement rate from the total SP amounts. 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 amount is an increasing function of the tenure prior to layoff: on average, the total SP amount reaches 428% and 670% of the pre-layoff wage in the bottom and top tenure quartiles, respectively. We use a similar specification as in equation (7), 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). As a reminder, we refer to all resources made available to workers in a lump-sum fashion at layoff under the term "SP" (or "SP amount") in the analysis (and thus in this figure).

FIGURE 8: HETEROGENEITY BY WORKER CHARACTERISTICS

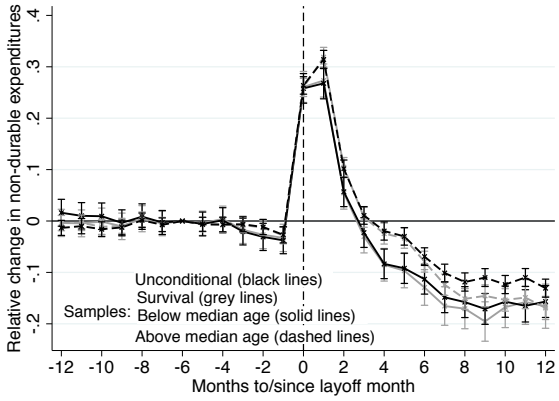
(a) Female vs. male workers



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



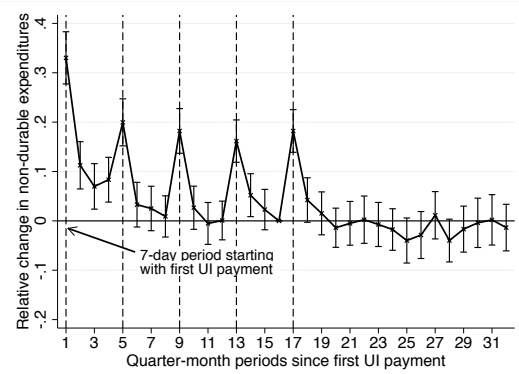
(c) Workers with more vs. less than the median age



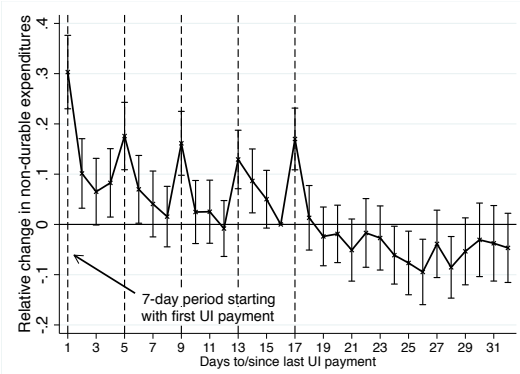
Notes: The figure presents some heterogeneity by worker characteristics. Each line in each panel displays difference-in-differences results (point estimates and 95% confidence intervals) based on a separate regression for each subsample of workers that uses the specification in equation (6) for non-durable expenditures, 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.

FIGURE 9: NON-DURABLE EXPENDITURE PROFILE AROUND UI PAYDAY AND UI EXHAUSTION EVENTS

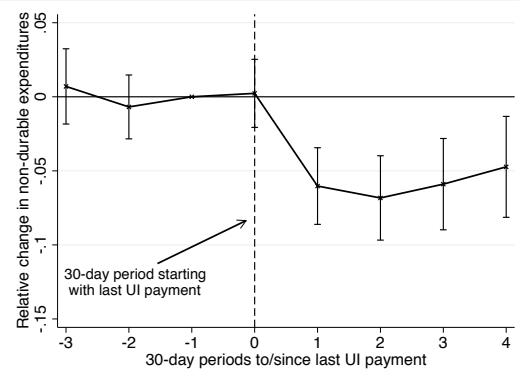
(a) Around UI payday within a month (UI exhaustees)



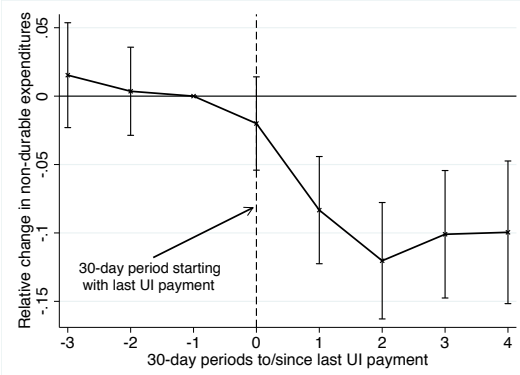
(b) Around UI payday within a month (UI exhaustees remaining without a formal job)



(c) Around UI exhaustion month (UI exhaustees)



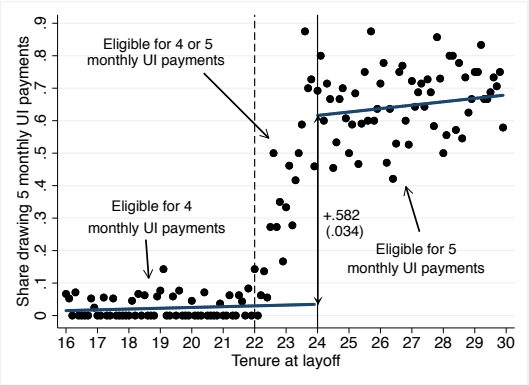
(d) Around UI exhaustion month (UI exhaustees remaining without a formal job)



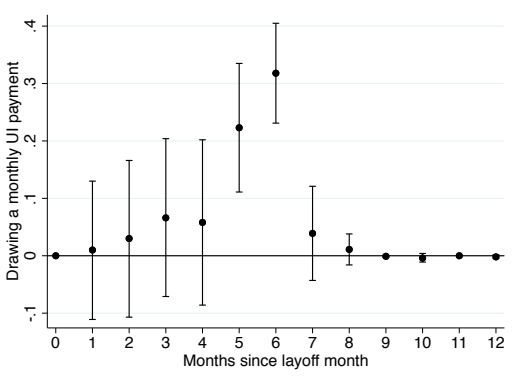
Notes: The figure presents the results (point estimates and 95% confidence intervals) of event analyses centered around UI payment dates and UI exhaustion in relative changes, based on the specification in equation (8). We use the subset of layoffs in our treatment group for which workers were eligible 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 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 (c) investigates how non-durable expenditures change around UI exhaustion by aggregating 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). Panels (b) and (d) presents the results of similar analyses for the subsample of 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 10: REGRESSION DISCONTINUITY RESULTS

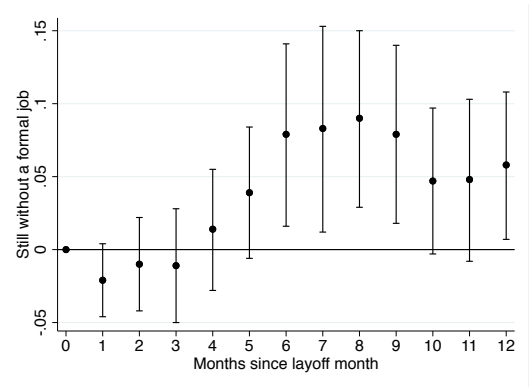
(a) Share drawing 5 monthly UI payments, raw data



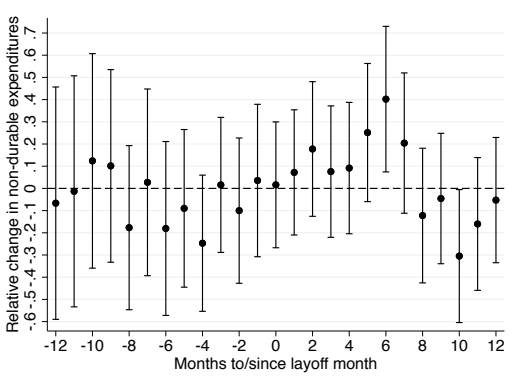
(b) Share drawing UI in each month, RD estimates



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



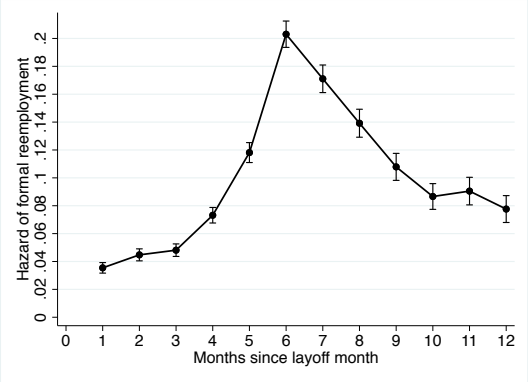
(d) Non-durable expenditures, RD estimates, median



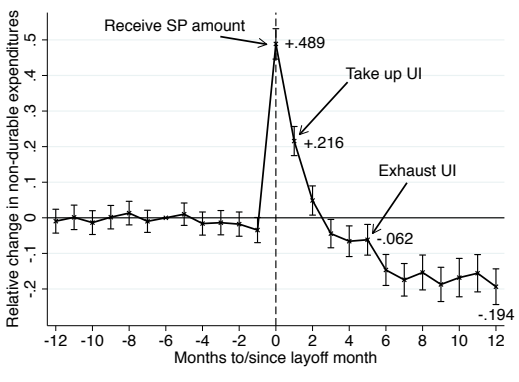
Notes: The figure presents results for the RD analysis around the 24-month cutoff. Workers with less than 22 months (resp. more than 24 months) of tenure at layoff were eligible for four (resp. five) monthly UI payments (workers with 22 to 24 months of tenure were eligible for four or five monthly UI 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 (9). 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 (9) in each month to/since layoff, separately, for various outcomes in that month: panel (b) uses a dummy for drawing any 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 11: TARGET EMPIRICAL MOMENTS FOR THE MODEL

(a) First set of target empirical moments: hazard rates of formal reemployment (workers taking up UI in month 1 after layoff)



(b) Second set of target empirical moments: DD estimates for non-durable expenditures (workers taking up UI in month 1 after layoff, survival sample)



Notes: The figure displays the target empirical moments used in our structural estimations. For simplicity, our estimation sample is restricted to displaced workers eligible for five monthly UI payments *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 (10,025 observations). Panel (a) displays our first set of moments: the hazard rates of formal reemployment in the months since layoff. The hazard rate of formal reemployment increases more sharply than in Figure 3c and peaks in month 6, just after all workers in this sample exhausted their UI benefits. Panel (b) displays our second set of moments: 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 6c, 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 6c, with the drop in consumption taking place just after UI exhaustion as in Figure 9d.

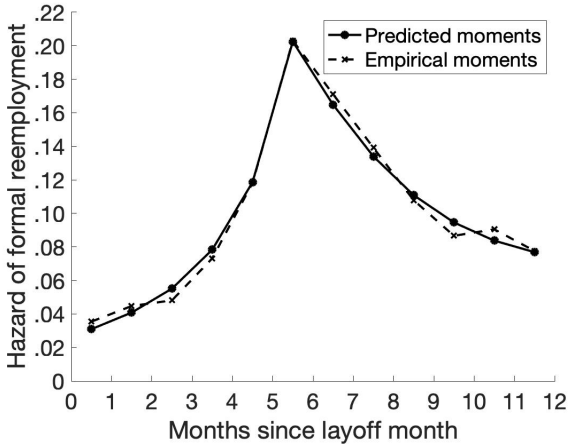
Table 2: Estimated parameters for the structural models

	δ-discounting with free δ (myopia)	δ-discounting with fixed δ (benchmark)	$\beta\delta$-discounting with free β (present bias)
	(1)	(2)	(3)
Parameters			
Discount factor (1 month): δ	0.820	0.995	0.995
	(0.112)	(fixed)	(fixed)
Present bias parameter: β	1	1	0.714
	(fixed)	(fixed)	(0.012)
Inverse of search elasticity: θ	0.416	0.014	0.237
	(0.226)	(0.001)	(0.017)
Search cost (higher-cost type): $\kappa 1$	6.091	85.077	10.541
	(1.007)	(3.263)	(2.494)
Search cost (lower-cost type): $\kappa 2$	1.914	38.435	1.303
	(0.843)	(1.537)	(0.129)
Share of higher-cost type: $s1$	0.482	0.462	0.540
	(0.049)	(0.029)	(0.064)
Share of lump-sum amount for consumption: ω	0.592	0.592	0.502
	(0.027)	(fixed)	(0.041)
Informal work cost: χ	0.536	0.574	0.544
	(0.009)	(0.049)	(0.011)
Inverse of informal work elasticity: λ	0.152	0.247	0.250
	(0.009)	(0.027)	(0.021)
Model fit			
Number of moments used	25	25	25
Number of estimated parameters	8	6	8
Goodness of fit	41.204	721.352	262.298

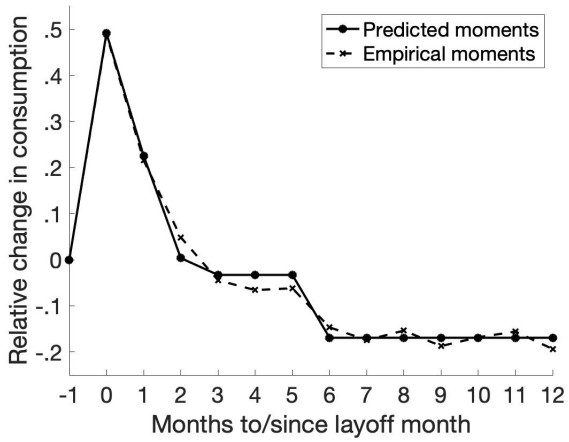
Notes: The table displays estimates for the three versions of the structural model described in Section 5.3. The parameters are estimated by minimizing the distance between the empirical moments displayed in Figure 11 – hazard rates of formal reemployment and relative changes in consumption (starting in month 0)– and the same moments as predicted by the models for a given vector of free parameters.

FIGURE 12: FIT OF THE ESTIMATED MODELS IN TABLE 2

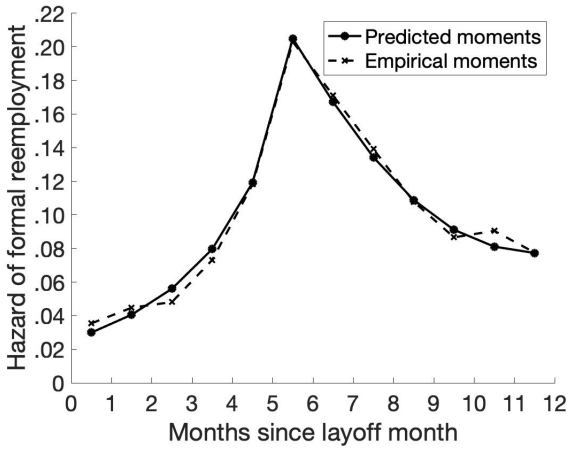
(a) Myopia model ($\widehat{\delta} = .82$, $\beta = 1$): formal reemployment



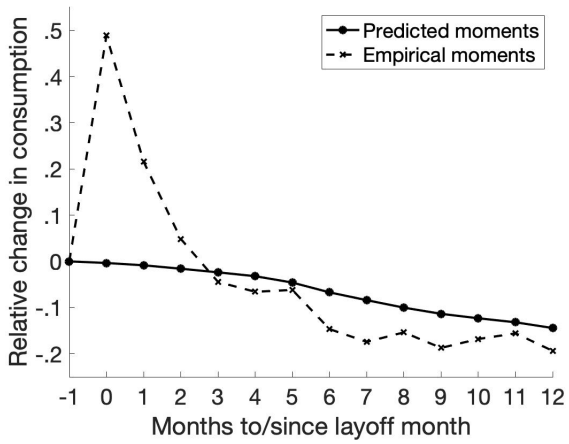
(b) Myopia model ($\widehat{\delta} = .82$, $\beta = 1$): consumption



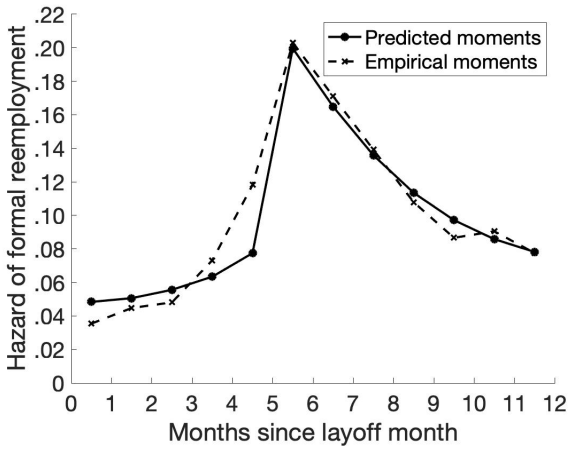
(c) Benchmark model ($\delta = .995$, $\beta = 1$): formal reemployment



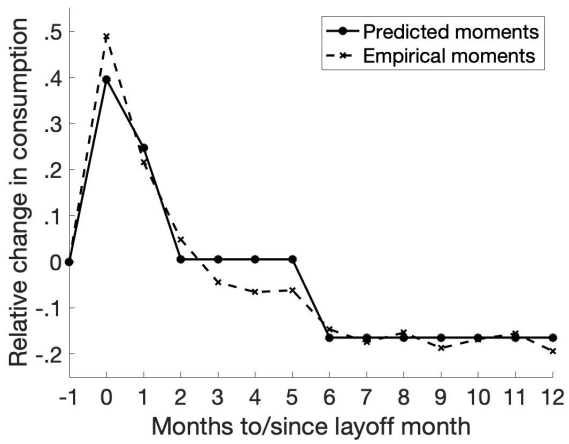
(d) Benchmark model ($\delta = .995$, $\beta = 1$): consumption



(e) Present bias model ($\delta = .995$, $\widehat{\beta} = .714$): formal reemployment



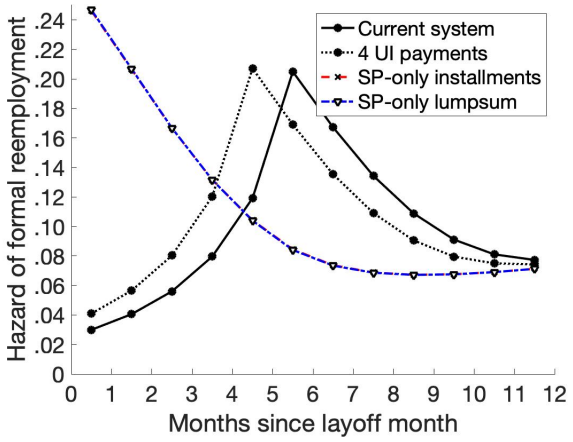
(f) Present bias model ($\delta = .995$, $\widehat{\beta} = .714$): consumption



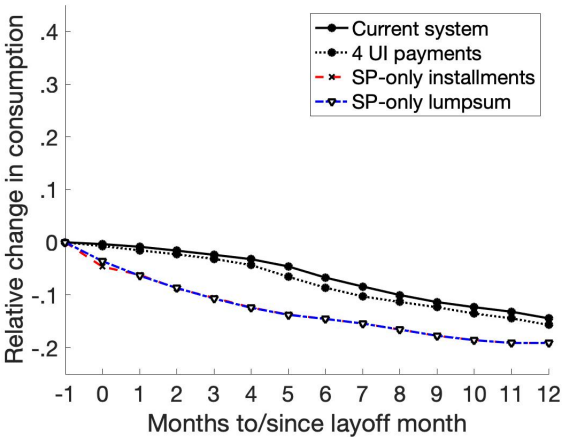
Notes: The figure compares the target *empirical* moments for our estimations to the same moments as *predicted* by the structural model for the parameter values in columns (1)-(3) in Table 2, respectively, in order to evaluate the fit of our estimated models.

FIGURE 13: COUNTERFACTUAL POLICIES

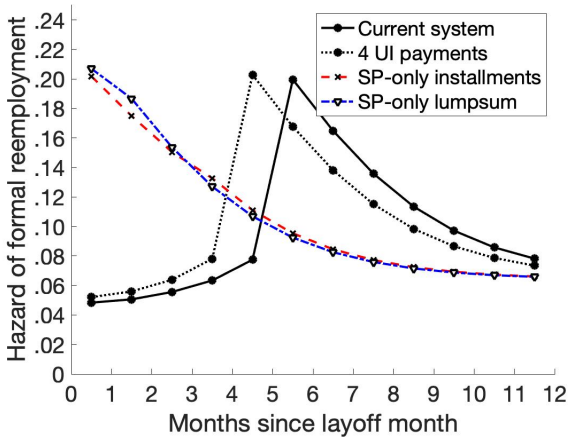
(a) Benchmark model ($\delta = .995$, $\beta = 1$): formal reemployment



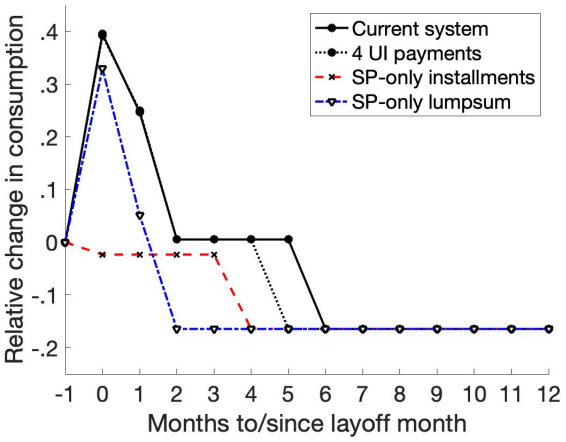
(b) Benchmark model ($\delta = .995$, $\beta = 1$): consumption



(c) Present bias model ($\delta = .995$, $\widehat{\beta} = .714$): formal reemployment



(d) Present bias model ($\delta = .995$, $\widehat{\beta} = .714$): consumption



Notes: The figure displays the hazard rates of formal reemployment and the relative changes in consumption based on simulations of our models under three counterfactual policies ("current system" replicates the solid lines in Figure 12c-12f): a policy that reduces the potential UI duration to four months ("4 UI payments"; it mimicks the variation in the RD analysis in Section 4.4); a policy in which displaced workers are not eligible for any UI benefits, i.e., they only receive the SP amount paid lump-sum at layoff ("SP-only lump-sum"); a policy that pays the same SP amount in 4 monthly installments, *irrespective* of workers' reemployment status ("SP-only installments"; we continue to assume no UI benefits in that case). The top and bottom panels compare the predictions of the benchmark and present bias models under these counterfactual policies. The lines corresponding to the predictions of the "SP-only lump-sum" and "SP-only installments" policies are on top of each other with the benchmark model.

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

	ΔWelfare Insurance Value	ΔWelfare Incentive Effect
	(1)	(2)
Benchmark model (δ=.995, β=1)		
Policy 1: marg. increase in potential UI duration	0.08	-0.49
Policy 2: marg. increase in UI benefit level	0.06	-0.21
Policy 3: marg. increase in lump-sum at layoff	0.04	-0.01
Policy 4: marg. increase in uncond. transfer paid in installments	0.04	-0.01
Present bias model (δ=.995, β=.714)		
Policy 1: marg. increase in potential UI duration	0.16	-0.31
Policy 2: marg. increase in UI benefit level	-0.09	-0.37
Policy 3: marg. increase in lump-sum at layoff	-0.21	0.00
Policy 4: marg. increase in uncond. transfer paid in installments	-0.12	0.01

Notes: The table displays results from evaluating the incentive-insurance trade-off of job displacement insurance reforms in the benchmark and present bias models. We consider 4 reforms that were constructed (separately for each model) 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 SP amount paid lump-sum at layoff (policy 3), and an unconditional transfer paying the same increase in the lump-sum amount but in 6 installments (months 0-5), irrespective of workers' reemployment status (policy 4). 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. All welfare effects are expressed in $1 per $1 spent on mechanical beneficiaries (those who would benefit from these reforms in absence of behavioral responses); a negative value implies a decrease in welfare.

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Online Appendix A

A.1. Additional figures and tables

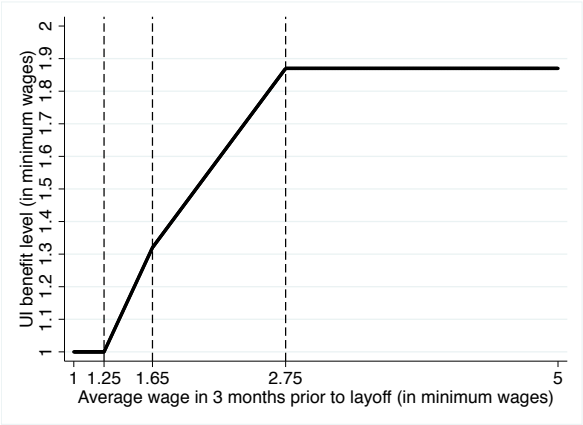
Full schedule of UI benefits in Brazil. The UI benefit level depends on a displaced formal worker's average wage in the three months prior to layoff and ranges from 100% to 187% of the minimum wage. Define w the displaced formal worker's average nominal wage in the three months prior to layoff expressed 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 $w \geq 2.75$

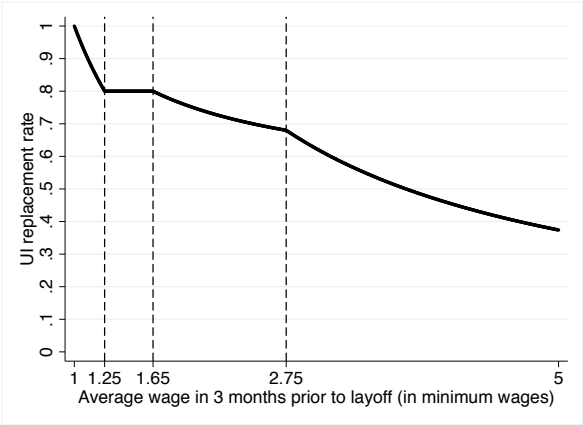
Figure A1 displays the relationship between w and b graphically, as well as the replacement rate (b/w).

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

(a) UI benefit level



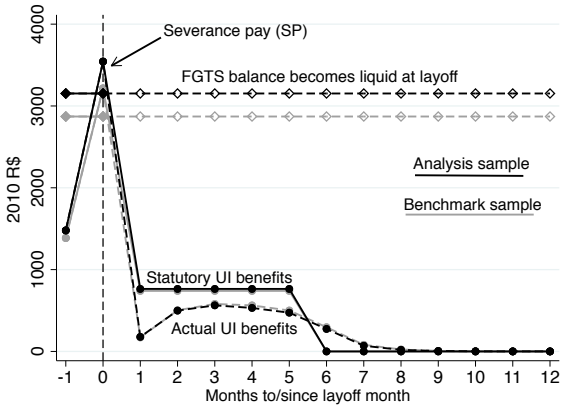
(b) UI replacement rate



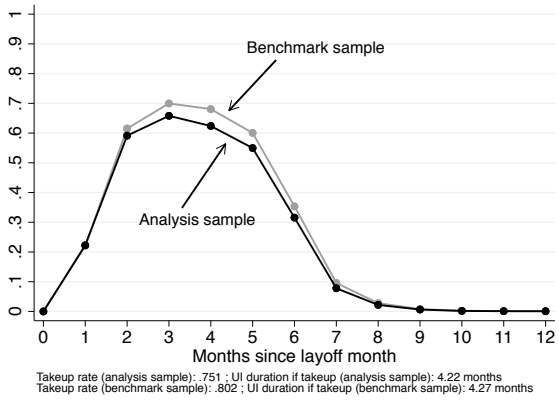
Notes: The panels display the UI benefit level and the UI replacement rate in the Brazilian UI program, which is a function of a displaced formal worker's average nominal wage in the three months prior to layoff, expressed in multiples of the prevailing minimum wage.

FIGURE A2: BACKGROUND AND REPRESENTATIVITY OF DISPLACED FORMAL WORKERS IN THE ANALYSIS SAMPLE, 2011

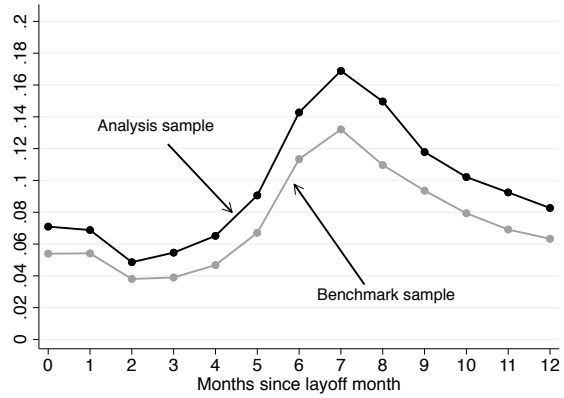
(a) Average statutory (and actual UI) benefits



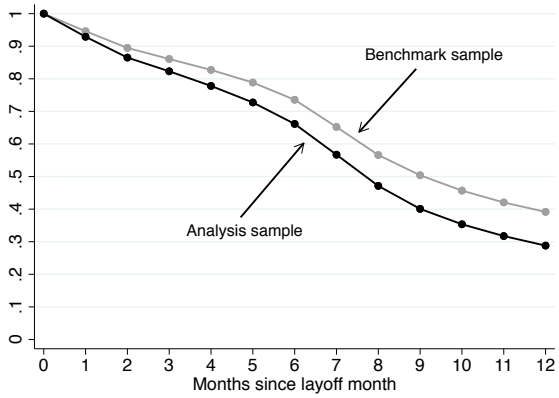
(b) Share drawing UI benefits



(c) Hazard rate of formal reemployment



(d) Survival rate in non-formal-employment



Notes: The figure replicates Figure 3 for workers in the analysis sample laid off in 2011, the sample used for Figure 3b 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 A3: EXPENDITURE PROFILE AROUND FORMAL REEMPLOYMENT EVENT

(a) DD estimates for total expenditures



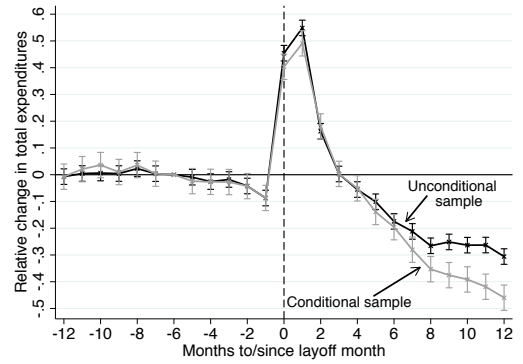
(b) DD estimates for non-durable expenditures



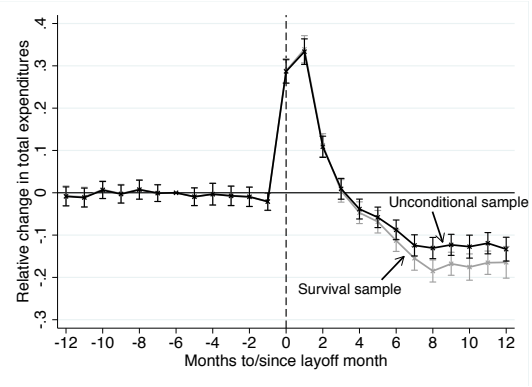
Notes: The figure presents difference-in-differences results for the change in expenditure levels around formal reemployment. We use laid off workers who were formally reemployed in months 0 to 10 after layoff as our treatment group. We use laid off workers who remained without a formal job 12 months 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 (6): 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 A4: EXPENDITURE PROFILE AROUND DISPLACEMENT EVENT, ROBUSTNESS CHECKS

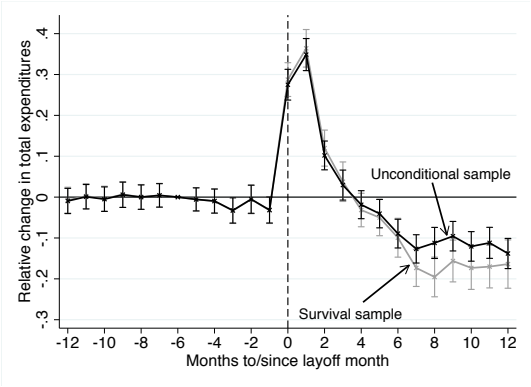
(a) DD estimates at the median



(b) DD estimates for consumers reporting positive purchases in all 12 months before layoff

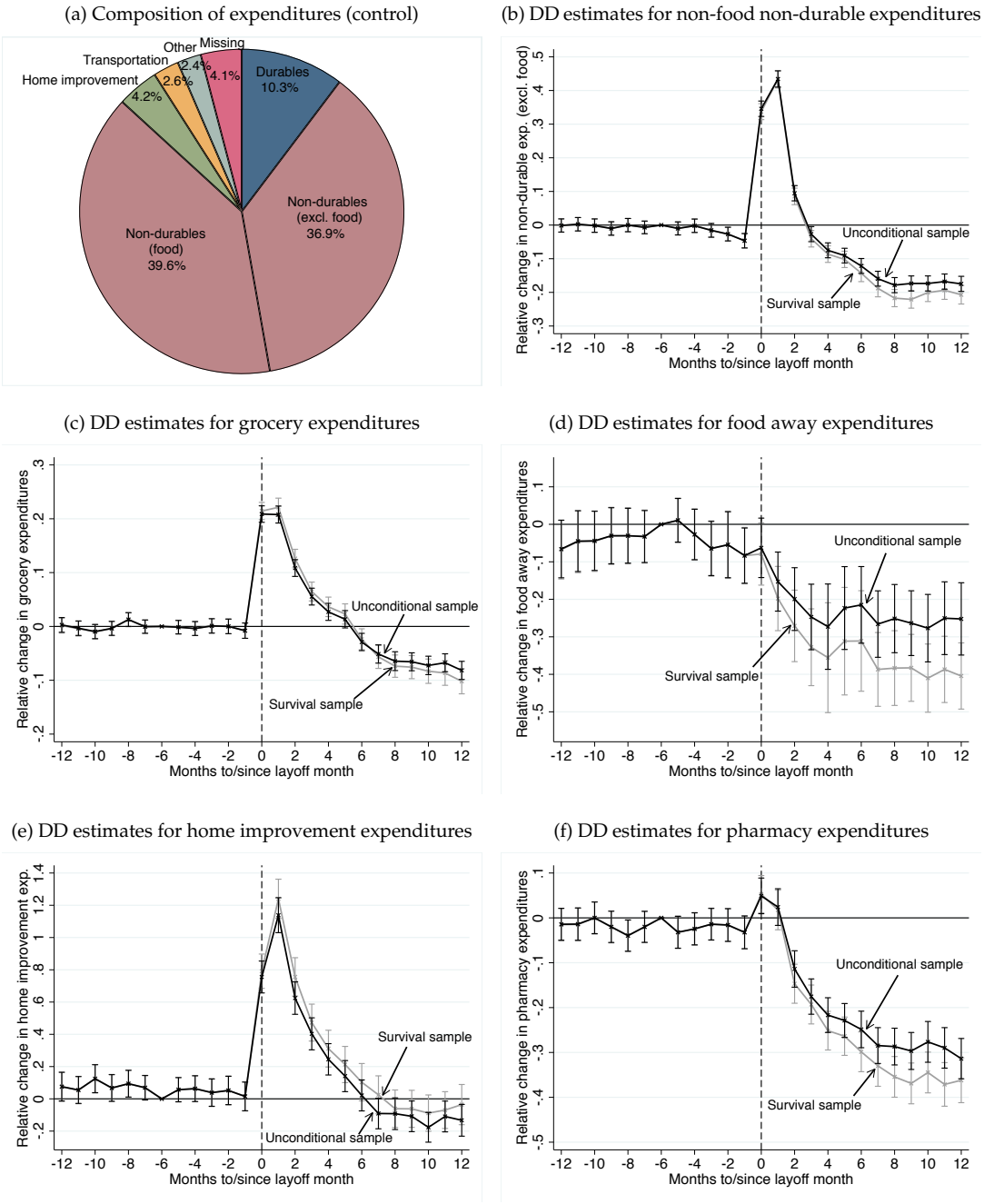


(c) DD estimates for workers laid off from a downsizing firm, which lost at least 30% of its workforce



Notes: The figure presents robustness checks for the results in Figure 5b. 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; we include instead results for the same conditional sample as in Figure 5c. Panel (b) presents similar results as in Figure 5b, but restricting our treatment and control groups to consumers 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 (c) presents similar results as in Figure 5b, but 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.

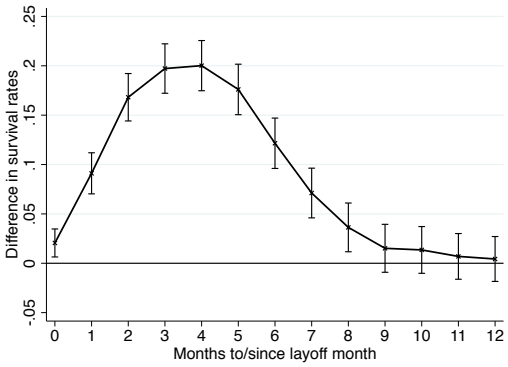
FIGURE A5: HETEROGENEITY BY EXPENDITURE CATEGORY (II)



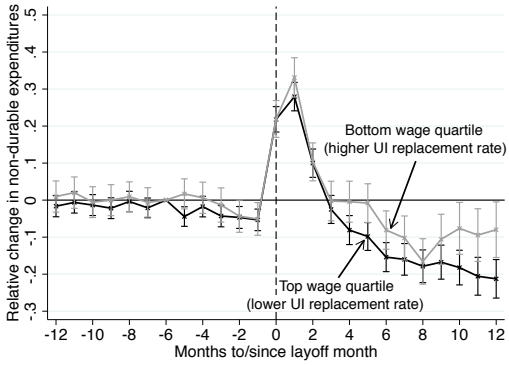
Notes: The figure presents results for different expenditure categories, as in Figure 6. Panel (a) displays the composition of expenditures before layoff (average over $k = -12$ to $k = -6$) in the control group. It is very similar as in the treatment group. Panels (c)-(f) present difference-in-differences results in relative changes as in Figure 5 (point estimates and 95% confidence intervals) using the specification in equation (6) 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 A6: LINKING OUR RESULTS TO VARIATION IN JOB DISPLACEMENT INSURANCE BENEFITS (II)

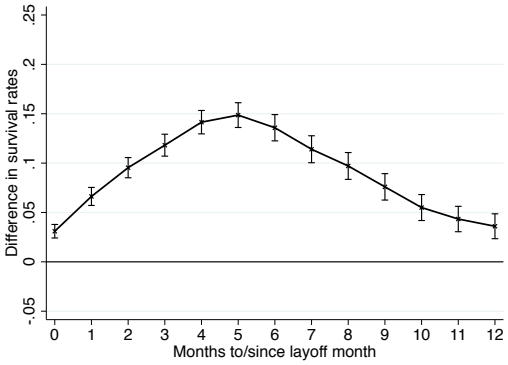
(a) Difference in survival rates in non-formal-employment for laid off vs. fired workers



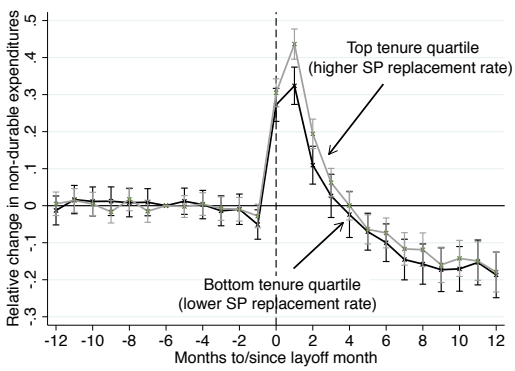
(b) Comparing DD estimates for workers in bottom and top wage quartiles (top and bottom quartiles in UI replacement rates; survival sample)



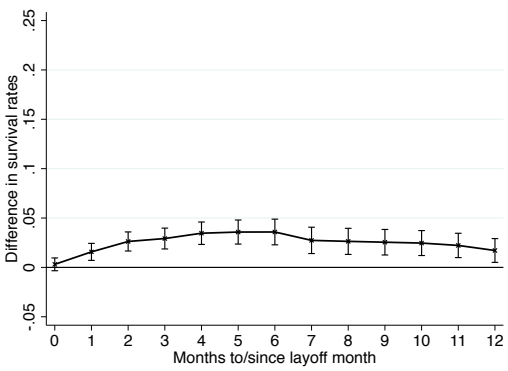
(c) Difference in survival rates in non-formal-employment for bottom vs. top wage quartile



(d) Comparing DD estimates for workers in bottom and top tenure quartiles (bottom and top quartiles in replacement rates from total SP amount; survival sample)



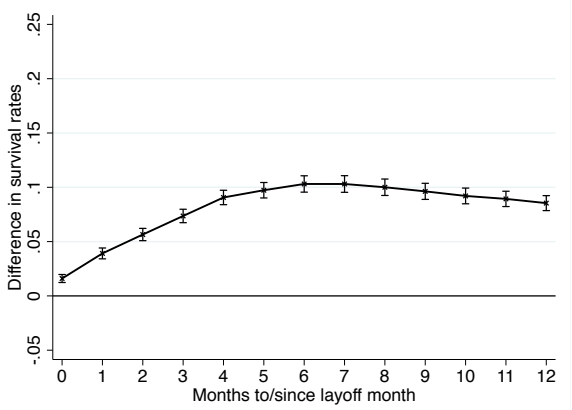
(e) Difference in survival rates in non-formal-employment for top vs. bottom tenure quartile



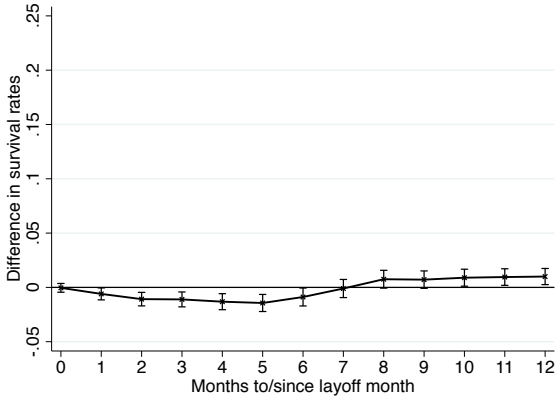
Notes: The figure presents results complementing the evidence in Figure 7. Panel (a) displays estimates of the difference in survival rates in non-formal-employment for laid off vs. fired workers. We combine the samples of laid off and fired workers (without the control group) for the months 0-12 after 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 7a). 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 7b and 7c, but for the survival 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 7b and 7c 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 A7: HETEROGENEITY BY WORKER CHARACTERISTICS, DIFFERENCE IN SURVIVAL RATES

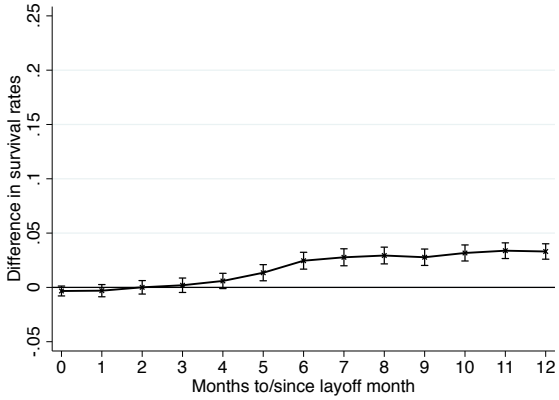
(a) Female vs. male workers



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



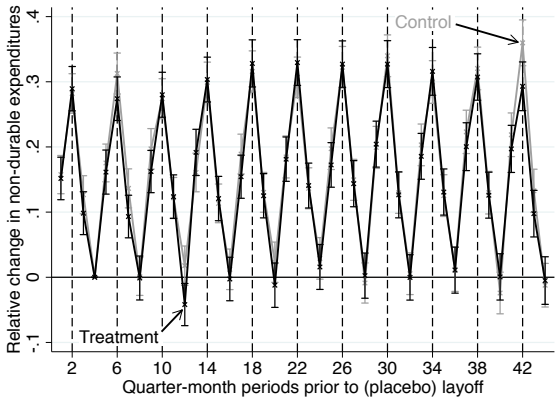
(c) Workers with more vs. less than the median age



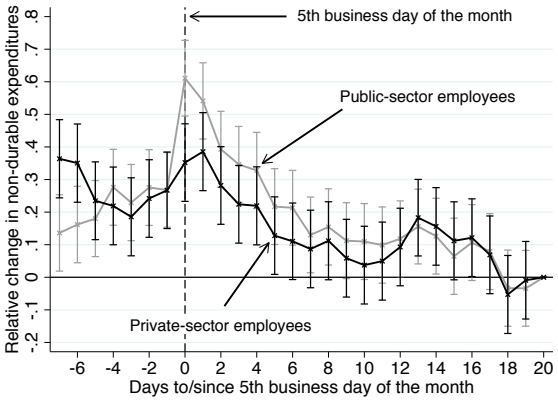
Notes: The figure displays estimates of the difference in survival rates in non-formal-employment for the categories of workers compared in Figure 8. We combine the samples compared in each panel for the months 0-12 after layoff, and regress a dummy variable for remaining without a formal job on month 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 8). 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 A6.

FIGURE A8: NON-DURABLE EXPENDITURE PROFILE AROUND PAYDAY EVENTS FOR FORMAL EMPLOYEES

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



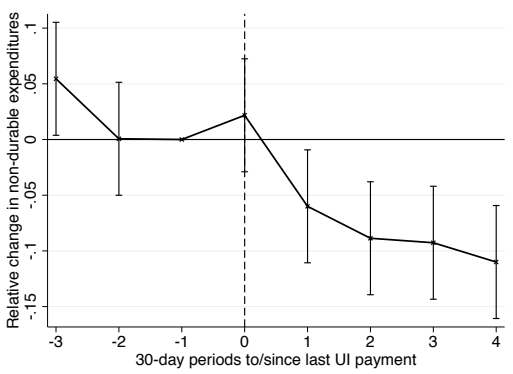
(b) Using all formal employees in our data



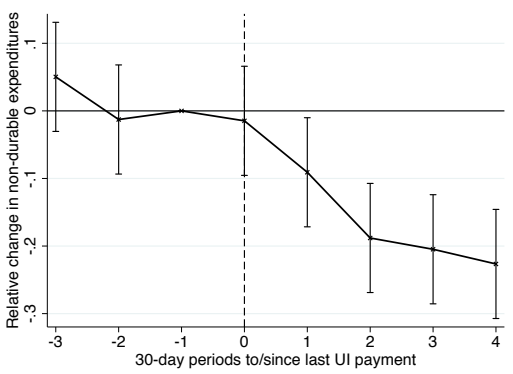
Notes: The figure complements the evidence in Figure 9a by showing that non-durable expenditure levels are also very sensitive to the timing of paydays for formal employees in our data. We use the fact that monthly salaries must be paid before the fifth business day of each month in Brazil. Panel (a) follows a similar approach as in Figures 9a 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 before the 5th business day of the month. Panel (b) uses a much larger dataset including all months in which a worker is observed formally employed in our 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 A9: NON-DURABLE EXPENDITURE PROFILE AROUND UI PAYDAY AND UI EXHAUSTION EVENTS, ROBUSTNESS CHECKS

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



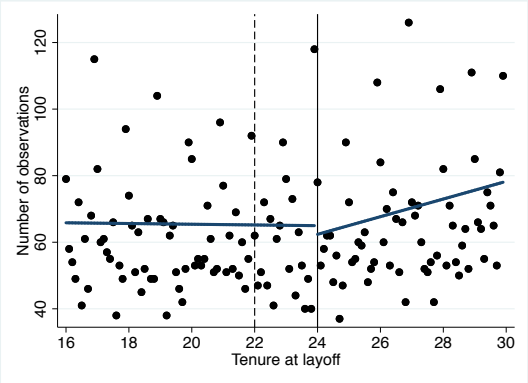
(b) Around UI exhaustion month (median; UI exhaustees remaining without a formal job)



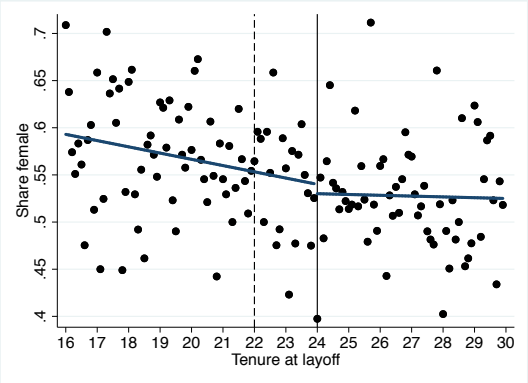
Notes: The figure displays robustness checks for the results in Figures 9c and 9d. It presents relative change in non-durable expenditures for the median (point estimates and 95% confidence intervals). The point estimates are larger than in Figures 9c and 9d, but the overall patterns remain identical: consumption levels are flat before UI exhaustion, but they drop rapidly after UI exhaustion.

FIGURE A10: REGRESSION DISCONTINUITY RESULTS, VALIDITY CHECKS

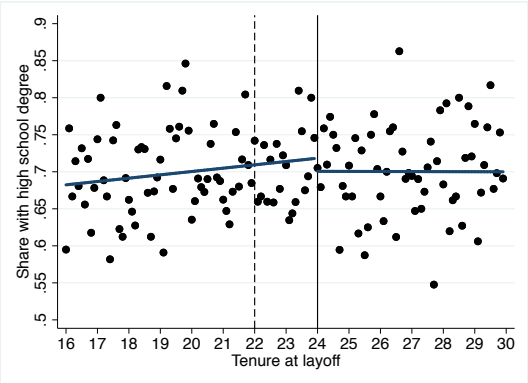
(a) Number of observations, raw data



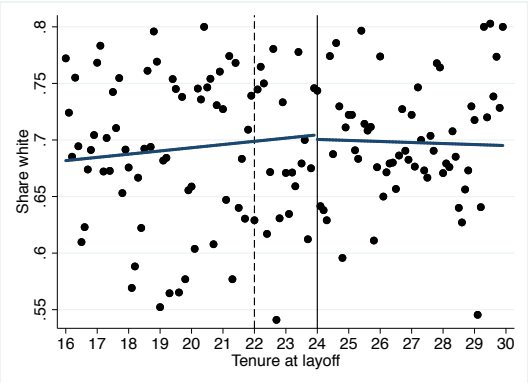
(b) Share female, raw data



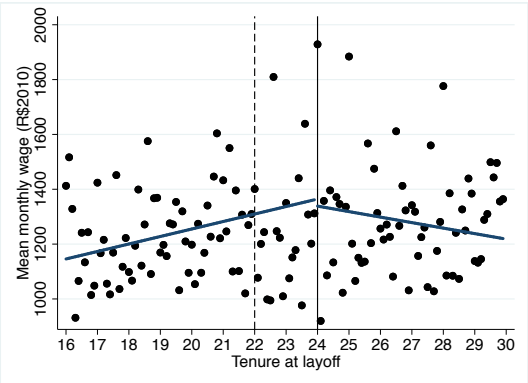
(c) Share with a high school degree, raw data



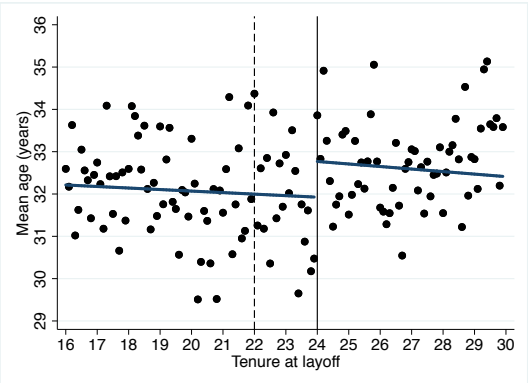
(d) Share white, raw data



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



(f) Age (years), raw data



Notes: 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. Panel (a) considers the distribution of the running variable (i.e., number of observations by tenure levels) and panels (b)-(f) the composition of the sample around the cutoff (the share or average of the variable in the title of the figure by tenure levels). The lines in the figure also illustrate our estimation strategy following equation (9).

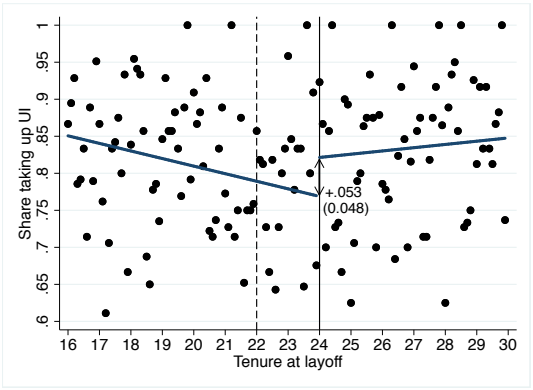
Table A1: Validity checks for the regression discontinuity (RD) design

	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

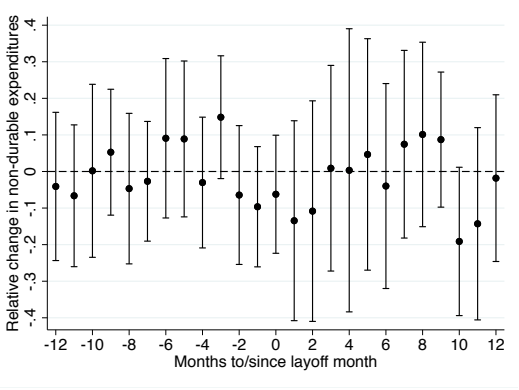
Notes: The table presents validity checks for the RD analysis around the 24-month cutoff for the full sample (panel A) and the subsample with UI data (panel B). It displays RD estimates (with their standard errors in parentheses) based on the specification in equation (9). Column (1) considers the distribution 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 A11: REGRESSION DISCONTINUITY RESULTS (II)

(a) Share taking up UI, raw data



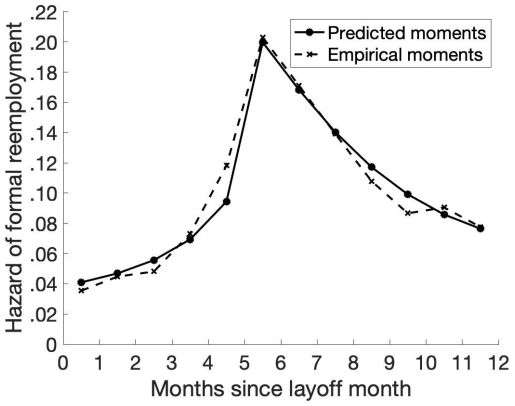
(b) Non-durable expenditures, RD estimates at the mean



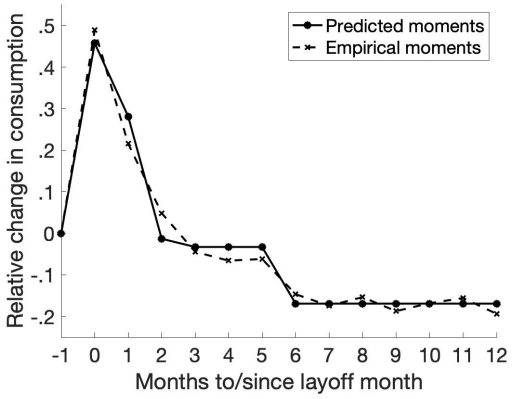
Notes: The figure presents results for the RD analysis around the 24-month cutoff for additional outcomes; workers with less than 22 months (resp. more than 24 months) of tenure at layoff were eligible for four (resp. five) monthly UI payments (workers with 22 to 24 months of tenure were eligible for 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 (9). Panel (b) displays 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) from using the specification in equation (9) in each month to/since layoff, separately, for non-durable expenditures in that month.

FIGURE A12: PREDICTED MOMENTS FOR ALTERNATIVE MODELS

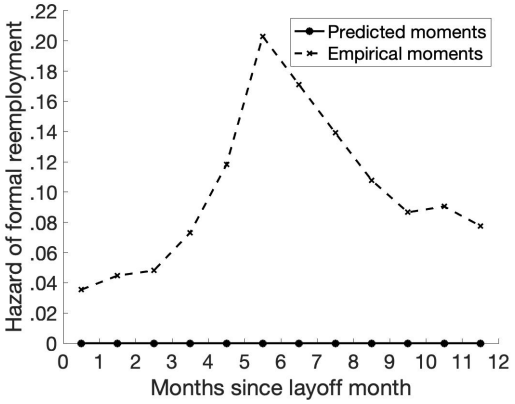
(a) Myopia and present-bias: formal reemployment



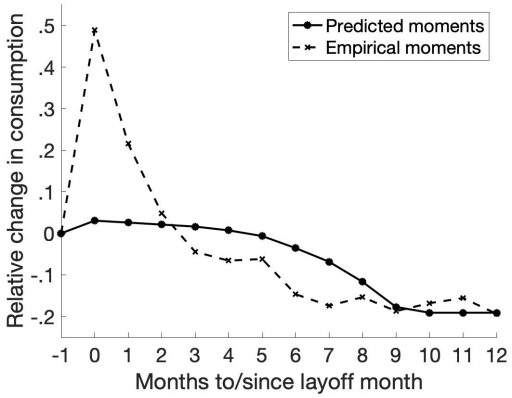
(b) Myopia and present-bias: consumption



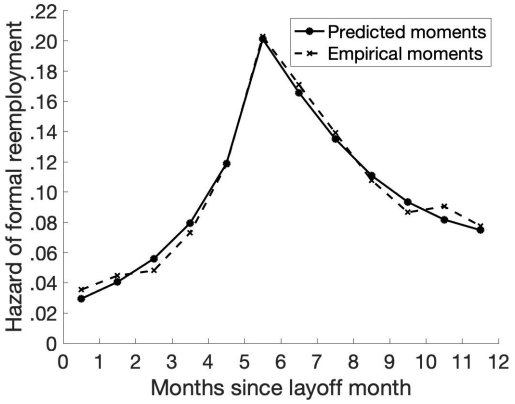
(c) Biased beliefs: formal reemployment



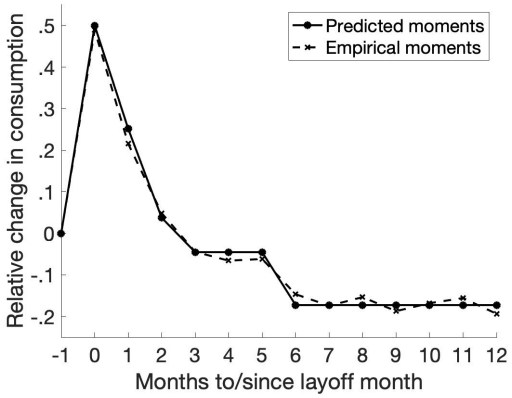
(d) Biased beliefs: consumption



(e) Savings constraints: formal reemployment



(f) Savings constraints: consumption



Notes: The figure compares empirical and predicted moments, as in Figure 12, for alternative versions of our structural model. Panels (a) and (b) presents simulations from a model in which we introduce both myopia and present-bias ($\delta = .95$, $\beta = .8$, $r = 0$), showing that such a model can fit the data even better than our present bias model in Figure 12. Panels (c) and (d) presents simulations from a version of the benchmark model ($\delta = .995$, $\beta = 1$) in which we introduce severely biased beliefs about reemployment probabilities, i.e., a baseline bias of $\overline{h}$=.9 (see text for details). Panels (e) and (f) presents the results from our "savings constraints" model. We re-estimated the model, setting ($\delta = .995$, $\beta = 1$), but including the interest rate r in our vector of free parameters ($\xi = \{\theta, \kappa_1, \kappa_2, s_1, \omega, \chi, \lambda, r\}$). The model is able to fit the data closely, but the estimated interest rate is unrealistically low, a monthly interest rate of $\widehat{r} = -.16$.

A2. Sufficient statistic formulas in the benchmark model

A common approach in the UI literature to evaluate the incentive-insurance trade-off with UI reforms is to express it in terms of sufficient statistics that can be estimated empirically. A similar approach could be used to assess SP reforms or to compare SP and UI reforms. We show this here for the model of Section 1.

Government's problem. With perfect experienced-rating, the balanced-budget equations for the UI and SP programs are:[68]

$$\tau^b = \frac{p}{1-p} \cdot b \cdot B$$
$$\tau^f = \frac{p}{1-p} \cdot f,$$

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., $1 + r = \delta = 1$). 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.

The government's problem is to maximize welfare:

$$W = \int \left[p \cdot U_{i,0} + (1-p) \cdot V_{i,0} \right] \, di. \tag{11}$$

We assume that the social welfare function is differentiable.

Sufficient statistics formulas. We now derive typical sufficient statistics formulas for the welfare effects of marginal changes in the SP amount (df), in the UI benefit level (db), and in the potential UI duration (dP), separately. We 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 ($d\widetilde{W}$):

$$df: \qquad d\widetilde{W} = \frac{\mathbb{E}\left[\frac{\partial \nu_i(c^n_{i,0})}{\partial c^n_{i,0}} \right] - \mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}} \right]}{\mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}} \right]} - \frac{d(b \cdot B)}{df} \tag{12}$$

Consider next a marginal change in the UI benefit level, b. Following the same steps as above, including

[68] With our setup, the time it takes for workers to find a new job once they have exhausted their UI benefits has no effect on the cost of the UI program, which is the case when UI benefits are experienced-rated.

the same normalization, we obtain the following welfare effect per \$1 spent on target beneficiaries:

$$db: \qquad d\widetilde{W} = \frac{\mathbb{E}^B\left[\frac{\partial \nu_i(c^n_{i,t})}{\partial c^n_{i,t}}\right] - \mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right]}{\mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right]} - \frac{1}{B}\cdot\frac{d(b\cdot B)}{db} \tag{13}$$

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:

$$dP: \qquad d\widetilde{W} = \frac{\mathbb{E}\left[\frac{\partial \nu_i(c^n_{i,P})}{\partial c^n_{i,P}}\right] - \mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right]}{\mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right]} - \frac{1}{b\cdot S_P}\cdot\frac{d(b\cdot B)}{dP} \tag{14}$$

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 (12)-(14) captures the potential welfare gain of the reforms, namely their insurance value. In equation (12), it depends on workers' gain from buying insurance against the risk of displacement, namely the consumption smoothing gain from transferring \$1 from the employed to the displaced state. In equations (13) and (14), it depends on workers' gain from buying insurance against the risk of remaining 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 consume. This is because workers are optimizing such that they are indifferent between increases in consumption or savings in all periods, i.e., the first-order condition (4) holds (the same formula would hold with hand-to-mouth workers as well as they would consume the extra resources when received).

The second term in equations (12)-(14) captures the potential welfare loss of the reforms, namely their efficiency cost, which is measured by the fiscal externalities caused by behavioral responses, e.g., workers remaining non-employed longer. It takes the form of a ratio between a behavioral effect – the increase in costs due to behavioral responses, i.e., $p\cdot\frac{d(b\cdot B)}{dx}$ for $x=\{f;b;P\}$ – and a 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. The only fiscal externalities are those on the UI budget in this model; 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. 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 (12)-(14) 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. Equations (12)-(14) 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, 2016b).

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 (13) and (14). 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 (13), for instance, can be written such that (Baily, 1978; Gruber, 1997):[69]

$$\frac{\mathbb{E}^B\left[\frac{\partial \nu_i(c^n_{i,t})}{c^n_{i,t}}\right] - \mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right]}{\mathbb{E}\left[\frac{\partial \nu_i(c^e_{i,t})}{\partial c^e_{i,t}}\right]} \simeq \gamma \frac{\mathbb{E}\left[c^e_{i,t}\right] - \mathbb{E}^B\left[c^n_{i,t}\right]}{\mathbb{E}\left[c^e_{i,t}\right]} \tag{15}$$

where γ is an average coefficient of relative risk aversion and the ratio in equation (15) 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 (12) and (14) 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.

A property of the expression in equation (15) 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 stronger assumptions, such as the absence of savings constraints (e.g., from kinship taxation; Jakiela & Ozier, 2016), biased beliefs (Spinnewijn, 2015), or present bias (DellaVigna & Paserman, 2005; Ganong & Noel, 2018). In the eventuality that these assumptions are violated, one would need to know when benefits are actually consumed (rather than when they are received) to assess the consumption-smoothing gain from job displacement insurance policies. The overall welfare effects also likely include a third component in the latter two cases: the impact of the reforms on *internalities*, as other workers' choices such as job-search effort may be privately suboptimal from a normative point of view. We come back to these points in Section 5.

[69] 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).

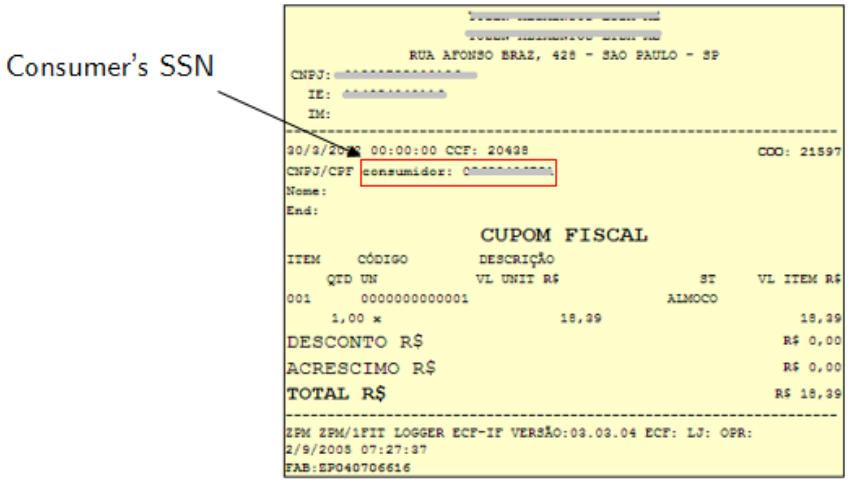
Online Appendix B - Spending Categorization

This appendix provides details on the spending categorization of the de-identified receipts data. It also describes the steps taken to compare the data in our paper with a Household Survey data from the Brazilian Census Bureau (IBGE), the Pesquisa de Orçamentos Familiares (POF) of 2008/2009.

B.1. Categorization of Receipts into Spending

The receipts data contains information about the total value of the receipt, the number of items in each receipt and a time stamp. The time recorded in the receipt allows us to create a weekly and monthly panel of spending used in the paper. Figure B1 shows an example of a receipt from a purchase in a São Paulo grocery shop. All receipts have a field to fill in the buyer's tax ID number (CNPJ or CPF as indicated in the figure) if provided. In business-to-business transactions, the tax ID is the CNPJ of the firm, and the receipt may be used as tax credit in the VAT system by the buying firm. In sales to final consumers, consumers' ID number (their CPF) can be indicated in this field as highlighted in Figure B1, in which case they are eligible for lottery tickets and tax rebates (see Naritomi 2018).

FIGURE B1: RECEIPT FROM SÃO PAULO



Notes: This picture shows the receipt typically issued by retail firms. All receipts have a field to fill in the buyer's tax ID number (CNPJ or CPF as indicated in the figure) if provided. In business-to-business transactions, the tax ID is the CNPJ, and the receipt may be used as tax credit in the VAT system by the buying firm. In sales to final consumers, consumers' ID number (their CPF) can be indicated in this field as highlighted in the picture, in which case they are eligible for lottery tickets and tax rebates (see Naritomi 2018). This specific receipt is a real receipt that was requested by the researchers at a retail establishment in São Paulo.

In order to categorize the data from receipts into different types of spending, we use the sector of activity of the establishment that issued the receipt as defined by the National Classification of Economic Activities - Classificação Nacional de Atividades Econômicas (CNAE) version 2.0. More precisely, we use the most dis-aggregated classification, which is a 7-digit sector definition that allows us to finely categorize the type of shop consumers are buying from. 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.

The two main categories that we would like to analyze separately are durables and non-durables. For this categorization, we followed the North American Industry Classification System (NAICS) from the U.S. Bureau of Labor Statistics (LBS). Two research assistants worked on this categorization separately to ensure consistency using the description of the CNAE codes. For instance, we classified as *durables* purchases 7-digit CNAE codes that have descriptions similar to the goods described in NAICS 423 ("Durable Goods"). It includes mostly personal electronics, home appliances, furniture, vehicles and vehicle parts.

We classified as *non-durables* purchases all CNAE codes that are associated with goods described in NAICS 424 ("Nondurable Goods"), but excludes transportation.[70] In our data, the main non-durable categories are food (e.g., groceries and food away from home), pharmaceuticals (e.g., drugstores), entertainment (e.g., books, games, theme parks), non-essential personal goods (e.g., apparel, cosmetics), temptation goods (liquor and tobacco stores, gambling, sugar-related foods such as ice-cream and candy stores), and other non-durables (e.g., office supply, pet stores).

We also created a separate category of spending from durables and non-durables: *home improvement*. It includes expenditures related to building materials, renovations and construction work. These are expenditures that could be considered complements to having more disposable time after layoff, and are related to construction work and purchases of tools that could be used in such activities.

Table B1 lists the CNAE codes that were classified into these three subcategories of consumer spending – durables, non-durables, home improvement – that we study in the paper. In total, we observe 988 CNAE codes in the data. However, the purchases are highly concentrated among a few CNAE codes. Table B1 lists the top CNAE codes for each category of consumer spending that together amount to 95% of the receipts in that category.

Within non-durable purchases, we discuss in the paper four subcategories in Figure 6 and Figure A5: food away from home, groceries, pharmaceutical and entertainment. *Food away from home* are CNAE codes with descriptions associated to food sold and typically prepared and consumed outside the home such as restaurants. *Groceries* are CNAE codes with descriptions associated to grocery shops or retail of food that is prepared at home. *Pharmaceutical* are CNAE codes with descriptions related to pharmacy and health-related expenditures. CNAE codes are categorized as *Entertainment* if their description was associated to consumer spending that is a complement to leisure time. Table B2 lists the top CNAE codes for each category of consumer spending that together amount to 95% of the receipts in that category.

B.2. Comparison between Spending data from Receipts vs. Survey

In order to check how our consumer spending measure compares with other data sources, we analyze the most recent household expenditure survey (POF) available at IBGE, which is 2008/2009. It collects data on income, household demographic characteristics and expenditures. The survey is representative at the state level, and we restrict attention to households in the state of São Paulo. Food expenditures are collected through diaries, and non-food expenditures are based on recall. Although there are potential concerns about data quality with this type of expenditure data - see, for instance, Lanjouw (2005) - it is a useful external data source to benchmark our data. Below, we describe several steps that were taken to harmonize the two datasets for this comparison.

[70] We created a separate *transportation* category that includes mostly gasoline or other fuel for auto-vehicles. There are also categories for "Other" (about 2.5%), which are CNAE codes that are not easily classified, and "missing" (about 4%), which are cases where the CNAE code is unknown.

Harmonizing POF and our consumer spending data

Our consumer spending sample. We pooled together the App de-identified users that were found in the matched employer-employee data (RAIS). In POF, surveys are conducted at different time periods between 2008 and 2009. To make our data compatible with the sampling structure of POF surveys, we drew a random simulated survey month for each worker in 2011, which is the earliest year in our data for which we can measure consumer spending for the previous 12 months (the data start in 2010). We restricted our sample only to individuals who were working in the selected month and who were between 20 and 50 years old. After selecting the hypothetical survey month, we used a window of 12 months of consumption from our data - the survey month and 11 months before - to aggregate the total expenditure. All values are monthly averages for 12 months.

POF survey sample. For the sake of comparison with our consumer spending data, we restrict attention to households with formal workers from São Paulo who were between 20 and 50 years old. Importantly, we need to identify formal labor income in POF as this is the only income source we can observe in RAIS. POF does not identify formal workers directly, but it is possible to define formality as private or public jobs with income subject to compulsory contribution to social security. All expenditure and income values are monthly averages for the 12-month period before (and including) the survey month.

Categorization of expenditures. The POF survey covers a broader set of expenditures compared to our consumer spending data: it includes housing and a larger range of services (e.g., transportation services such as public transport or taxis; these purchases are not taxed by the state VAT). In order to compare similar categories across the two datasets, we categorized the purchases from POF according to the "Descrição do Item" (the product description) in POF's catalog of 6-digit code products. In 2008/2009, the catalog had 13,785 items. Two research assistants categorized the whole catalog separately to ensure consistency. Similarly to the CNAE description, the text allows us to classify purchases between categories of interest such as: durables, non-durables, and the other sub-categories described above into which we categorized the CNAE of shops.

Another relevant difference between the two datasets is that total expenditures in POF are best measured at the household level, whereas our consumer spending data is reported by the individual. The POF survey has expenditure measures at the individual and household levels, but some relevant items are only measured at the household level, such as groceries or durables like home appliances. Thus, we restrict attention to household-level expenditure data in POF. Even though we cannot observe families in our data, it is possible that the expenditures we observe are not individual, but household expenditures. 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. Indeed, in the survey we conducted in São Paulo, more than 60% of workers indicated that all their household members participated in the NFP program with the same ID number (see Appendix Table C1).

The formal income vs. expenditure gradient in POF vs. in our consumer spending data

This section shows how the different consumption measures from the two sources of data behave relatively to formal income changes in a cross-section. Beyond potential data coverage differences, it is possible that, in our data, the consumer may not provide her ID number for all her purchases and the consumer may not be making all the households' purchases. Nonetheless, overall, the empirical regularities in the data show that the coverage of our data is relatively constant when we look at a cross-section of income levels.

We construct three consumer spending measures to compare POF and our data: *(i) Total expenditure*, which is the total household expenditure observed in POF and the total expenditure observed in our data, irrespective of their categories; *(ii) Comparable categories* excludes from the *Total expenditure* in POF the categories that cannot be measured in our data (e.g., housing); *(iii) Total non-durable expenditure* which is defined in both datasets as described in Table B1.

Figure B2 displays binscatters of the correlation between log(expenditure), for these three consumer spending measures, and log(wage) for formal workers in our data and in POF, as in Figure 4a in the paper. 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 data) or the simulated interview date (our data). The y-axis corresponds to the logarithm of the average monthly expenditure - according to one of the three measures described above – in the previous 12 months in the household (survey data) and as reported by the consumer (our data).

In all three cases, it is possible to observe two patterns. First, there is a similar income-spending gradient when comparing the slope in our data and the slope in the survey data. Second, there is a clear level difference between the two lines as the survey data, in levels, covers a larger share of expenditures. This is particularly true in panel (a) because we are comparing total expenditures irrespective of which categories they belong to. In panel (b), our data remain the same as in panel (a), but we exclude from the survey data the categories of consumer spending that are not included in our data (e.g., housing costs and services), which makes the slopes even more similar. Finally, panel (c) compares the income-spending gradient among non-durables only. The slopes are flatter for both samples, but they remain similar across the two samples.

TABLE B1: CROSS-WALK BETWEEN MAIN CATEGORIES OF SPENDING AND SECTOR OF ACTIVITY (CNAE)

Category	CNAE	Description
Durables	2621300	Fabricação de equipamentos de informática
	4511101	Comércio a varejo de automóveis, camionetas e utilitários novos
	4511102	Comércio a varejo de automóveis, camionetas e utilitários usados
	4530701	Comércio por atacado de peças e acessórios novos para veículos automotores
	4530703	Comércio a varejo de peças e acessórios novos para veículos automotores
	4530705	Comércio a varejo de pneumáticos e câmaras-de-ar
	4541203	Comércio a varejo de motocicletas e motonetas novas
	4541205	Comércio a varejo de peças e acessórios para motocicletas e motonetas
	4649401	Comércio atacadista de equipamentos elétricos de uso pessoal e doméstico
	4651601	Comércio atacadista de equipamentos de informática
	4752100	Comércio varejista especializado de equipamentos de telefonia e comunicação
	4753900	Comércio varejista especializado de eletrodomésticos e equipamentos de áudio e vídeo
	4754701	Comércio varejista de móveis
	4754702	Comércio varejista de artigos de colchoaria
	4754703	Comércio varejista de artigos de iluminação
	4755503	Comercio varejista de artigos de cama, mesa e banho
	4757100	Comércio varejista especializado de peças e acessórios para aparelhos eletroeletrônicos para uso doméstico, exceto informática e comunicação
	4759899	Comércio varejista de outros artigos de uso doméstico não especificados anteriormente
	6120501	Telefonia móvel celular
	9511800	Reparação e manutenção de computadores e de equipamentos periféricos
Non-durables	4639701	Comércio atacadista de produtos alimentícios em geral
	4646001	Comércio atacadista de cosméticos e produtos de perfumaria
	4691500	Comércio atacadista de mercadorias em geral, com predominância de produtos alimentícios
	4693100	Comércio atacadista de mercadorias em geral, sem predominância de alimentos ou de insumos agropecuários
	4711301	Comércio varejista de mercadorias em geral, com predominância de produtos alimentícios - hipermercados
	4711302	Comércio varejista de mercadorias em geral, com predominância de produtos alimentícios - supermercados
	4712100	Comércio varejista de mercadorias em geral, com predominância de produtos alimentícios - minimercados, mercearias e armazéns
	4713001	Lojas de departamentos ou magazines
	4721102	Padaria e confeitaria com predominância de revenda
	4721104	Comércio varejista de doces, balas, bombons e semelhantes
	4722901	Comércio varejista de carnes - açougues
	4724500	Comércio varejista de hortifrutigranjeiros
	4729699	Comércio varejista de produtos alimentícios em geral ou especializado em produtos alimentícios não especificados anteriormente
	4755502	Comercio varejista de artigos de armarinho
	4761001	Comércio varejista de livros
	4763601	Comércio varejista de brinquedos e artigos recreativos
	4763602	Comércio varejista de artigos esportivos
	4771701	Comércio varejista de produtos farmacêuticos, sem manipulação de fórmulas
	4771702	Comércio varejista de produtos farmacêuticos, com manipulação de fórmulas
	4772500	Comércio varejista de cosméticos, produtos de perfumaria e de higiene pessoal
	4781400	Comércio varejista de artigos do vestuário e acessórios
	4782201	Comércio varejista de calçados
	4789004	Comércio varejista de animais vivos e de artigos e alimentos para animais de estimação
	5611201	Restaurantes e similares
	5611203	Lanchonetes, casas de chá, de sucos e similares
Home improvement	4741500	Comércio varejista de tintas e materiais para pintura
	4742300	Comércio varejista de material elétrico
	4744001	Comércio varejista de ferragens e ferramentas
	4744005	Comércio varejista de materiais de construção não especificados anteriormente
	4744099	Comércio varejista de materiais de construção em geral

Notes: The table lists the cross-walk between the main spending categories used in the paper based on the sector of activity of the shops (CNAE) that issue the receipts. There is a total of 988 7-digit CNAE codes that were classified into different consumption categories based on the description of the sector. The list displayed in the table restricts attention to the top 7-digit CNAE codes in each category in terms of number of receipts that add up to 95% of all receipts in that category. *Durables*: 7-digit CNAE codes with descriptions similar to the goods described in NAICS 423 of U.S. LBS. improvements/works in a home. *Non-durables*: CNAE codes that can be classified following U.S. BLS NAICS 424 description (excluding transportation). *Home Improvement*: CNAE codes with descriptions related to construction materials or tools that can be used as a complement to labor in making

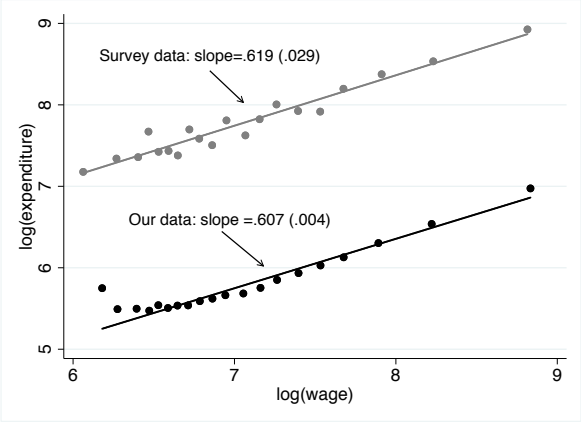
TABLE B2: CROSS-WALK BETWEEN SUBCATEGORIES OF NON-DURABLES USED IN THE PAPER AND SECTOR OF ACTIVITY (CNAE)

Category	CNAE	Description
Food away from home	5611201	Restaurantes e similares
	5611203	Lanchonetes, casas de chá, de sucos e similares
Groceries	4711301	Comércio varejista de mercadorias em geral, com predominância de produtos alimentícios - hipermercados
	4711302	Comércio varejista de mercadorias em geral, com predominância de produtos alimentícios - supermercados
	4712100	Comércio varejista de mercadorias em geral, com predominância de produtos alimentícios - minimercados, mercearias e armazéns
	4722901	Comércio varejista de carnes - açougues
Pharmaceutical	4771701	Comércio varejista de produtos farmacêuticos, sem manipulação de fórmulas
Entertainment	1811302	Impressão de livros, revistas e outras publicações periódicas
	4647802	Comércio atacadista de livros, jornais e outras publicações
	4756300	Comércio varejista especializado de instrumentos musicais e acessórios
	4761001	Comércio varejista de livros
	4763601	Comércio varejista de brinquedos e artigos recreativos
	4763602	Comércio varejista de artigos esportivos
	4763603	Comércio varejista de bicicletas e triciclos; peças e acessórios
	5822100	Edição integrada à impressão de jornais
	7722500	Aluguel de fitas de vídeo, DVDs e similares
	9321200	Parques de diversão e parques temáticos

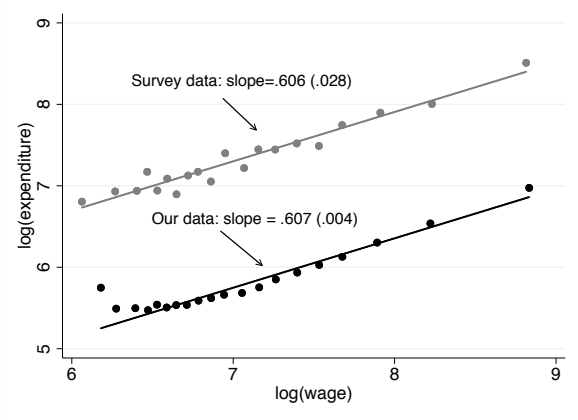
Notes: The table lists the cross-walk between subcategories of non-durable spending used in the paper and the sector of activity of the shops (CNAE) that issue the receipts. The list displayed in the table restricts attention to the top 7-digit CNAE codes in each category in terms of number of receipts that add up to 95% of all receipts in that category. *Food away from home*: CNAE codes with descriptions associated to food sold and typically prepared and consumed outside the home such as restaurants. *Groceries*: CNAE codes with descriptions associated to grocery shops or retail of food that is prepared at home. *Pharmaceutical*: CNAE codes with descriptions related to drugstores or health-care spending. *Entertainment*: CNAE codes with descriptions associated to consumer spending that is a complement to leisure time.

FIGURE B2: WAGE-EXPENDITURE GRADIENT IN OUR DATA VS. SURVEY DATA

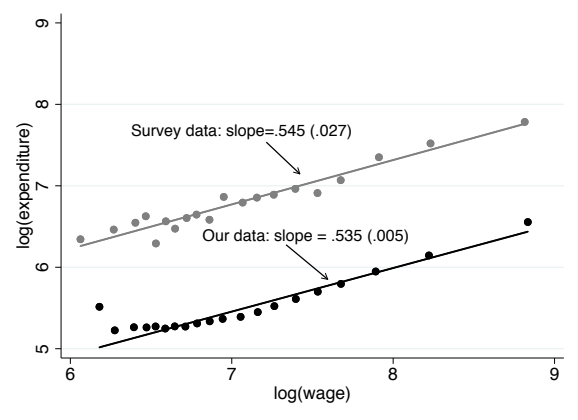
(a) Total expenditures



(b) Comparable categories



(c) Non-durables



Notes: The figure 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 data) or the simulated interview (our data). The y-axis corresponds to the logarithm of the average monthly expenditure in the previous 12 months in the household (survey data) and as reported by the user (our data). In panel (a), we compare a *Total expenditure* measure in the two datasets, which corresponds to the total household expenditure reported in POF and the total expenditure observed in our data, irrespective of their categories. In panel (b), we use a *Comparable categories* measure, which excludes from the *Total expenditure* in POF the categories that cannot be measured in our data (e.g., housing). In panel (c), we compare a *Total non-durable expenditure* measure, which is defined in both datasets as described in Table B1.

Online Appendix C - Survey conducted with workers in São Paulo

In this appendix we describe a survey that we conducted in the city of São Paulo, Brazil between July 23rd 2018 and August 3rd 2018. We focus on the key aspects of the survey that we discuss in the paper, and provide the relevant details of the data collection process.

Location. The interviews were conducted at the *Poupatempo* centers, which are popular one stop-shop citizen service centers used for a variety of services (e.g., driver's licence services, ID services, free internet through public computers *Acessa São Paulo*, etc.), of Sé and Taquera, and at the subway station *Bras*. These locations were chosen based on the fact that they have high foot traffic in general, and have an office where workers can claim UI benefits: a "Posto de Atendimento ao Trabalhador" (PAT).

Targeted population. The survey was targeted to two groups of workers: (i) *UI applicants*: workers who were laid off in the past 6 months from a formal job and applied for UI in the past 30 days (typically just before the interview as our interviews were conducted outside PATs); (ii) *Formal employees*: workers that, at the moment of the survey, were employed in the formal sector. We define "formal job" as a job in which the employer signed the worker's working card, which is mandatory and is the key paperwork to ensure that workers are protected by labor laws.

Analysis sample. For both samples, were restrict attention to workers with tenure lower than 72 months, which is one of the sample restrictions that we apply in our data in order to accurately calculate the statutory lump-sum benefits workers are eligible for (see Section 2).

Results. Table C1 displays descriptive statistics related to demographic characteristics, which we refer to in the paper. The first column restricts attention to the sample of *UI applicants*, and the second column shows the results for *Formal employees*. Table C5 shows the questionnaire's questions from which we created the variables described in Table C1.

Table C2 displays descriptive statistics related to the layoff experience. Thus, it restricts attention to the sample of *UI applicants*. Table C6 shows the questionnaire's questions from which we created the variables described in Table C2.

Table C.3. describes the top 3 categories of uses for the lump-sum benefits that workers have access to after layoff (*UI applicants* only). The idea is to capture what is "top of mind" when respondents are asked about "*How did you use, or how do you intend to use the amount received after layoff?*". The figure in the table for each category corresponds to the share of workers that mention that category among their top 3 uses.

Table C4 shows how much support some hypothetical reforms on the disbursement of benefits would have among the survey respondents. The survey questions that were used to construct the variables in Table C4 are listed in Table C7. The question about UI benefits was only asked to *UI applicants*, whose application had already been approved. The question about FGTS contributions was asked to both samples. The questions about unconditional access to the FGTS balance every three years was asked only to *formal employees*. The qualitative answers provided by respondents to justify their response were summarized by "key message" by the survey company directly, and then further aggregated by the research team for the categories mentioned in the table.

TABLE C1 : CHARACTERISTICS OF SURVEY RESPONDENTS

	UI applicants	Formal employees
(mean at/before layoff)	(1)	(2)
Share female	0.51	0.5
Age (years)	34.21	37.58
Share with high school degree	0.75	0.87
Share white	0.45	0.51
Household size	2.84	2.85
Number of adults in household	1.99	2.2
Share with smartphone	0.91	0.98
Share with bank account	0.93	0.99
Tenure (months)	29.42	115.19
Share with wage up to 2 minimum wages	0.56	0.45
Share with wage between 2 and 3 minimum wages	0.23	0.1
Share with wage between 3 and 5 minimum wages	0.10	0.26
Share with wage above 5 minimum wages	0.11	0.18
Share with household income up to 2 minimum wages	0.29	0.19
Share with household income between 2 and 3 minimum wages	0.25	0.16
Share with household income between 3 and 5 minimum wages	0.22	0.19
Share with household income above 5 minimum wages	0.24	0.46
Share with any savings	0.43	0.53
Total savings (in monthly wages)	2.05	3.71
Share with any debts	0.59	0.6
Total debts (in monthly wages)	7.17	15.53
Share participating in Nota Fiscal Paulista (NFP)	0.51	0.6
Share using unique CPF for the whole household for NFP	0.62	0.61
Number of observations	136	139

Notes: The table displays descriptive statistics from a survey conducted in the city of São Paulo, Brazil, between July 23rd 2018 and August 3rd 2018. The survey was targeted to two groups of workers: (i) *UI applicants*: workers that were laid off in the past 6 months from a formal job and applied for UI in the past 30 days; (ii) *Formal employees*: workers who, at the moment of the survey, were employed in the formal sector. Both samples were restricted to include workers with tenure lower than 72 months. *Wages*, *household income*, *savings*, and *debts* refer to the period before or at layoff for UI applicants. These variables were created based on the questionnaire' questions and answers displayed in Table C5. For more details on the Nota Fiscal Paulista (NFP) program see Section 2 or Naritomi (2018).

TABLE C2 : EXPERIENCE AT LAYOFF

	UI applicants
Share that learned about their layoff in advance	0.63
How many months in advance? (months before layoff)	2.96
Share that experienced a lower wage growth prior to layoff	0.13
Share that withdrew from FGTS account before layoff	0.07
Share that received 1-month advance notice	0.84
Share that obtained or is about to obtain access to FGTS account	0.98
Share that obtained or is about to obtain access to the layoff "fine"	0.88
Value of FGTS + fine + advance notice if received (in monthly wages)	4.86
Share that did not or does not intend to withdraw all FGTS and fine if received	0.04
Number of observations	136

Notes: This table describes the experience of workers at layoff in terms of the timing in which they learned about their layoff and their access to job displacement insurance benefits. It restricts attention to the sample of UI applicants. The survey questions used to construct the variables in this table are displayed in Table C6. FGTS is the forced savings accounts discussed in section 2.

TABLE C3: USE OF JOB DISPLACEMENT INSURANCE BENEFITS

	UI applicants
Share mentioning following category as (intended) use of lump-sum amount (FGTS and SP) (respondents could mention up to 3 categories)	
Non-durables (supermarket, clothing, food, etc.)	0.45
Durables (appliances, electronics, vehicles, furniture, etc.)	0.10
Special occasions (leisure, party, travel, shows, etc.)	0.06
Household bills (water, electricity, internet, schools, insurance, house cleaning, etc.)	0.46
Personal services (gym, beauty salon, etc.)	0.02
Home improvements	0.03
Debt repayment (excluding mortgages)	0.23
Mortgage payment	0.05
Investment in own business (goods for resale, materials, etc.)	0.04
Savings for long-term objectives (retirement, savings for university, etc.)	0.13
Savings for short-term objectives (emergencies, more expensive goods, travels, etc.)	0.27
Rent	0.01
Transfer to friends and family (gift, helping family or friend, etc.)	0.02
Other	0.07
Number of observations (only respondents who already received and withdrew FGTS and SP)	99

Notes: This table describes the answers to the question "*How did you use, or how do you intend to use the amount received after layoff? (Spontaneous answer: record the top 3 answers)*". The question refers to the lump-sum amount (FGTS + fine) described in section 2, and records the top 3 categories of use for these benefits that respondents spontaneously listed when prompted by the question. The second column shows the share of respondents that listed the category in the first column among their top 3 uses for the lump-sum amount. The idea is to capture what is "top of mind", i.e. most salient, when thinking about the uses for these benefits. The question was only asked to UI applicants who had already received and withdrew their FGTS and SP amount.

TABLE C4: SUPPORT FOR HYPOTHETICAL REFORM

	UI applicants	Formal employees
	(1)	(2)
Share preferring UI benefits paid in lump-sum fashion at layoff	0.4	
If no, reason for answer "To control expenditures"	0.39	
If no, reason for answer "To not spend it all at once"	0.36	
If no, reason for answer related to controlling expenditures	0.22	
If no, reason for answer unrelated to controlling expenditures ("It's not necessary at the moment")	0.03	
Number of observations (only respondents with UI application approved already)	121	
Share preferring monthly FGTS contributions as wage instead of forced savings deposit	0.47	0.43
If no, reason for answer "It is better to save"	0.21	0.5
If no, reason for answer "More money at layoff"	0.15	0.03
If no, reason for answer "I would/could spend it all/risk spending"	0.1	0.28
If no, reason for answer "It's safer in case of layoff / for insurance"	0.15	0.07
If no, reason for answer clearly related to controlling expenditures/saving constraints	0.19	0.08
If no, reason for answer possibly related to controlling expenditures/saving constraint	0.10	0.04
If no, reason for answer unrelated to controlling expenditures/saving constraints (e.g., "Greater value at the end", "The monthly value is small")	0.10	0.00
Number of observations	136	139
Share preferring unconditional access to FGTS account every 3 years		0.75
Number of observations		139

Notes: This table describes the answers to the questions on hypothetical reforms to the benefits workers currently have access to. The question about UI benefits was only asked to UI applicants, whose application had already been approved. The question about FGTS contributions was asked to both samples. The questions about unconditional access to the FGTS balance every three years was asked only to formal employees. The questions used in the survey to construct the variables in this table are displayed in Table C7. FGTS is the forced savings accounts discussed in section 2. The qualitative answers provided by respondents to justify their response were summarized by "key message" by the survey company directly, and then further aggregated by the research team for the categories mentioned in the table.

TABLE C5: SURVEY QUESTIONS FOR TABLE C1

Variable	Question	Answers
Share female	*Gender*	*(1) Male (2) Female*
Age (years)	*Age*	*(1) 16/24 (2) 25/34 (3) 35/44 (4) 45/59 (5) 60 OR +*
Share with high school degree	*What is the highest degree or level of schooling that you have completed? (Spontaneous answer)*	*(01) Illiterate (02) First primary incomplete (03) First primary complete (04) Second primary incomplete (05) Second primary complete (06) High school incomplete (07) High school complete (08) Undergrad incomplete (09) Undergrad (10) Master's (11) Phd*
Share white	*How would you describe yourself in terms of race? (Spontaneous answer)*	*(1) White (2) Indigenous (3) Black (4) Yellow (Asian) (5) Parda (6) Other*
Household size	*How many people depend on your household total income?*	*[insert text]*
Number of adults in household	*Number of adults, including you (at least 18 years old)*	*[insert text]*
Share with smartphone	*Do you own one of the following: smartphone*	*(1) yes (2) no*
Share with bank account	*Do you own one of the following: bank account*	*(1) yes (2) no*
Tenure (months)	*Before your layoff, for how long were you in this job? (At least 12 months)*	_____ *Year* _____ *Month*
Wage	*Among these income brackets (show the card with income brackets), where does the gross monthly salary that you had before layoff fit in? (CONSIDER additional salary, such as additional pay for night shift and hazard pay, and bonus. Do not consider benefits such as vale-refeição (food voucher), vale transporte (transportation voucher), and health insurance).*	*(1) up to 1/2 MW (up to R$468)* *(2) from 1/2 to 1 MW (R$468,1 to R$937)* *(3) from 1 to 2 MW (R$937,1 to R$1.874)* *(4) from 2 to 3 MW (R$1.874,1 to R$2.811)* *(5) from 3 to 5 MW (R$2.811,1 to R$4.685)* *(6) from 5 to 7 MW (R$4.685,1 to R$6.559)* *(7) from 7 to 10 MW (R$6.559,1 to R$9.370)* *(8) from 10 to 20 MW (R$9.370,1 to R$18.740)* *(9) more than 20 MW (more than R$18.740)*
Household income	*Among these income brackets (show the card with income brackets), where does your household total income that you had before layoff fit in? (Consider the income of everyone who contributes and all types of income sources: work, own business, rent, governmental transfers, etc.)*	*(1) up to 1/2 MW (up to R$468)* *(2) from 1/2 to 1 MW (R$468,1 to R$937)* *(3) from 1 to 2 MW (R$937,1 to R$1.874)* *(4) from 2 to 3 MW (R$1.874,1 to R$2.811)* *(5) from 3 to 5 MW (R$2.811,1 to R$4.685)* *(6) from 5 to 7 MW (R$4.685,1 to R$6.559)* *(7) from 7 to 10 MW (R$6.559,1 to R$9.370)* *(8) from 10 to 20 MW (R$9.370,1 to R$18.740)* *(9) more than 20 MW (more than R$18.740)*
Share with any savings	*Before layoff, were you able to monthly save part of your household income? For what purpose? (read the options and select which one that applies):*	*[1] Yes, for short-term purposes (emergencies, trips, expensive purchases, house improvement, etc.)* *[2] Yes, for long-term purposes (retirement, future expenditures to get children in college...)* *[3] No, I wasn't able to save*
Total savings (in monthly wages)	*At the moment of layoff, what was the total amount of savings that you had access to in terms of monthly wages before layoff?*	_____ *Months of wage*
Share with any debts	*Did you have debt?*	*(1) Yes (2) No*
Total debts (in monthly wages)	*What was the total value of debts that you had to pay in terms of monthly wage just before layoff?*	*The value was*_____ *months of wage.*
Share participating in Nota Fiscal Paulista (NFP)	*Are you a participant in the Nota Fiscal Paulista program?*	*(1) Yes (2) No*
Share using unique CPF for the whole household for NFP	*In your household, does everyone use the same CPF to request the Nota Fiscal Paulista? (read options)*	*(1) Yes, my CPF (2) Yes, the CPF of another person in the household (3) No*

Notes: The Variable column displays the variables reported in Table C1. The Question column contains the English translation of the questions that were used to create these variables. The Answers column shows the possible alternatives enumerators could choose from. The text in both columns were freely translated from the Portuguese original to English. MW is short for Minimum Wages, which was R$937 at the time of the interview.

TABLE C6: SURVEY QUESTIONS FOR TABLE C2

Variable	Question	Answers
Learned in advance about layoff	*Informally, did you know about your layoff before it happened?*	*(1) Yes, I knew more or less about it.* *(2) No, it was a surprise*
How many months in advance? (months before layoff)	*If the answer is yes, approximately for how much time before did you know about it?*	*How many months before layoff did you know about it? _____ Months before layoff*
Experienced a lower wage growth prior to layoff	*In the months before layoff, did you receive smaller wage readjustments or did your wage start to decrease (through working hours reduction, less additional hours, none wage readjustment, etc.) comparing with the period before?*	*(1) Yes (2) No*
Withdrew from FGTS account before layoff	*Did you withdraw part of your FGTS relative to this job before layoff?*	*(1) Yes (2) No*
Did not receive 1-month advance notice	*Did you receive an advance notice?*	*(1) Yes (2) No*
Got or is about to get access to FGTS account Got or is about to get access to severance pay Value of FGTS and severance pay if received (in monthly wages)	*Did you have access to your FGTS balance and to your FGTS fine (40%) after layoff? If the answer is yes, how many months of wage the entire amount was equivalent to? (Read the options and select the closest answer)*	*(1) Yes, but only FGTS, and how many months of wage it was? _____ Months of wage* *(2) Yes, but only the FGTS fine, and how many months of wage it was? _____ Months of wage* *(3) Yes, both FGTS and its fine, and how many months of wage it was? _____ Months of wage* *(4) No*
Did not or does not intend to withdraw all FGTS and severance pay if received	*Did you withdraw all these resources (FGTS balance and its fine (40%))? (Read the options and select the nearest one)*	*(1) Yes, I withdrew all of it (2) No, I withdrew only a part of it (3) No, I didn't withdraw any of it*

Notes: The Variable column displays the variables reported in Table C2. The Question column contains the English translation of the questions that were used to create these variables. The Answers column shows the possible alternatives enumerators could choose from. The text in both columns were freely translated from the Portuguese original to English. FGTS is the forced savings accounts discussed in section 2.

TABLE C7: SURVEY QUESTIONS FOR TABLE C4

Variable	Question	Answers
Prefers UI benefits paid in lump-sum fashion at layoff	*Would you prefer to receive all the amount of unemployment insurance in one installment (the same way it occurs with FGTS and its fine payment)?*	*(1) Yes (2) No. Why?* _____
Prefers monthly FGTS contributions as wage instead of forced savings deposits	*Would you prefer if, as a formal employee, the FGTS deposits were made directly into your account along with your wage, instead of being deposited into that account that your normally do not have access to as an employee? (the net amount would be exactly the same)*	*(1) Yes (2) No. Why?* _____
Prefers unconditional access to FGTS account every 3 years	*Would you prefer if, while formally employed, the deposits in FGTS were made available every three years irrespective of layoff, health issues or use of these resources?*	*(1) Yes (2) No. Why?* _____

Notes: The Variable column displays the variables reported in Table C4. The Question column contains the English translation of the questions that were used to create these variables. The Answers column shows the possible alternatives enumerators could choose from. The text in both columns were freely translated from the Portuguese original to English. FGTS is the forced savings accounts discussed in section 2.

Online Appendix D - Dataset on the prevalence of job displacement insurance programs across countries

This Appendix briefly describes the construction of the dataset documenting the prevalence of job displacement insurance (government-mandated) programs around the world since the start of the 20th century, which we use to construct Figure 1 in the paper.

Methodology

We worked with a team of Columbia undergraduates from many different parts of the world over a period of about six months, under the constant supervision of a PhD student in the Economics department, to create this dataset. In a first step, each undergraduate student was responsible for a set of countries (given their language proficiencies) and was instructed to find the necessary sources of information in order to document the existence and history of four sets of government-mandated programs (UI, SP, SSA, and UISA) since 1900 in each country. Effectively, given the many challenges involved, students often helped each other at this stage (they all met on a weekly basis). In a second step, another undergraduate student from the team verified the information for each country and coded it in a binary fashion, i.e., whether a significant version of each of the 4 programs existed in the country at some point for each decade since 1900. This step naturally involves some difficult decisions, so the students were instructed to justify all their decisions carefully. Using a binary classification also avoids the issue of comparing the generosity of programs along their many different policy parameters (it is much easier to go back in time to find information on the existence of a program than to find information on all its benefit schedules). In a third step, the PhD student reviewed all the data and the consistency of the coding across countries.

Output

The output of this work is twofold:

A. Data. A dataset documenting for 168 countries (according to 2018 borders) the existence of a significant version of each of the 4 programs in the country at some point for each decade since 1900. When some difficult coding decision was involved, a note explains the reason for our coding decision.

B. Documentation. A summary of the history of the 4 programs since 1900 for each of the 168 countries, including a list of the references used for each country.

The data and documentation will be made publicly available on our websites and we hope that it will be useful for other scholars across the social sciences. Any feedback to improve our coding decisions in specific instances will be much appreciated.

Note for Figure 1

The group of 25 countries labelled under "Western Europe, USA, CAN, AUS, NZ" in Figure 1 includes: Andorra, Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Liechtenstein, Luxembourg, Malta, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, the United States, and the United Kingdom.

The group of 114 countries labelled under "Africa, Asia, Rest of Americas" in Figure 1 includes: Afghanistan, Algeria, Angola, Argentina, Bahrain, Bangladesh, Benin, Bhutan, Bolivia, Botswana, Brazil, Brunei, Burkina

Faso, Burundi, Cambodia, Cameroon, Cape Verde, Central African Republic, Chad, Chile, China, Colombia, Republic of the Congo, Costa Rica, Cote d'Ivoire, Cuba, Cyprus, Democratic Republic of Congo, Djibouti, Dominican Republic, Ecuador, Egypt, El Salvador, Equatorial Guinea, Eritrea, Ethiopia, Gabon, Gambia, Ghana, Guatemala, Guinea, Guinea-Bissau, Guyana, Hong Kong, Haiti, Honduras, India, Indonesia, Iran, Iraq, Israel, Jamaica, Japan, Jordan, Kenya, Kuwait, Laos, Lebanon, Lesotho, Liberia, Libya, Madagascar, Malawi, Malaysia, Mali, Mauritania, Mauritius, Mexico, Mongolia, Morocco, Mozambique, Namibia, Nepal, Nicaragua, Niger, Nigeria, Oman, Pakistan, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Puerto Rico, Qatar, Rwanda, São Tome e Principe, Saudi Arabia, Senegal, Sierra Leone, Singapore, South Africa, South Korea, Sri Lanka, Sudan, Suriname, Swaziland, Syria, Taiwan, Tanzania, Thailand, Timor-Leste, Togo, Trinidad and Tobago, Tunisia, Turkey, UAE, Uganda, Uruguay, Venezuela, Vietnam, Yemen, Zambia, and Zimbabwe.

The data for 29 countries, mostly from Eastern Europe, were not used to generate the graphs in Figure 1: Albania, Armenia, Azerbaijan, Belarus, Bosnia and Herzegovina, Bulgaria, Croatia, Czech Republic, Estonia, Fiji, Georgia, Hungary, Kazakhstan, Kyrgyzstan, Latvia, Lithuania, Macedonia, Moldova, Montenegro, Poland, Romania, Russia, Serbia, Slovakia, Slovenia, Tajikistan, Turkmenistan, Ukraine, and Uzbekistan.