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INFORMAL LABOR AND THE EFFICIENCY COST OF SOCIAL PROGRAMS: EVIDENCE FROM THE BRAZILIAN UNEMPLOYMENT INSURANCE PROGRAM

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ABSTRACT

It is widely believed that the presence of a large informal sector increases the efficiency cost of social programs - transfer and social insurance programs - in developing countries. We evaluate such claims for policies that have been heavily studied in countries with low informality increases in unemployment insurance (UI) benefits. We introduce informal work opportunities into a canonical model of optimal UI that specifies the typical tradeoff between workers' need for insurance and the efficiency cost from distorting their incentives to return to a formal job. We then combine the model with evidence drawn from comprehensive administrative data to quantify the efficiency cost of increases in potential UI duration in Brazil. We find evidence of behavioral responses to UI incentives, including informality responses. However, because reemployment rates in the formal sector are low to begin with, most beneficiaries would draw the UI benefits absent behavioral responses, and only a fraction of the cost of (longer) UI benefits is due to perverse incentive effects. As a result, the efficiency cost is relatively low, and in fact lower than comparable estimates for the US. We reinforce this finding by showing that the efficiency cost is also lower in labor markets with higher informality within Brazil. This is because formal reemployment rates are even lower in those labor markets absent behavioral responses. In sum, the results go against the conventional wisdom, and indicate that efficiency concerns may even become more relevant as an economy formalizes.

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Gustavo Gonzaga Department of Economics Pontifical Catholic University of Rio de Janeiro (PUC-Rio) gonzaga@econ.puc-rio.br The informal sector – the part of an economy that escapes government monitoring – accounts for a larger share of employment in middle-income and developing countries. In a context of high informality, the conventional wisdom is that social programs – transfer and social insurance programs – impose high efficiency costs, particularly when they require beneficiaries to not be formally employed (Levy, 2008). The concern is that the ready availability of informal job opportunities exacerbates the usual disincentives to work in the formal sector created by such programs.¹

Despite this widespread view, the evidence behind it remains limited. First, due to data constraints, few papers credibly estimate the impact of social programs on employment choices in developing countries. Existing surveys often poorly measure eligibility and have sample sizes too small to exploit most sources of exogenous variation in program benefits. Administrative datasets are only slowly becoming available. Second, those studies finding that social programs induce some beneficiaries to not work in the formal sector typically lack a theoretical framework to interpret this evidence in terms of the relevant efficiency-equity or efficiency-insurance tradeoff. This is important because evidence of incentive effects in a context of high informality does not imply that the associated efficiency cost is relatively high, or higher than in a context of low informality.²

This paper addresses both limitations for the case of increases in Unemployment Insurance (UI) benefits. We first adapt a canonical framework in the UI literature to identify the measure of efficiency for the usual moral hazard problem – that increases in UI benefits distort incentives to return to a formal job – in our context. We then obtain new evidence on the size of the relevant effects using comprehensive administrative data on Brazilian UI beneficiaries, survey data, and quasi-experimental variation in UI benefits. This allows us to provide an estimate of efficiency costs that can be compared to estimates from countries with low informality. We also directly estimate how the efficiency cost varies across Brazilian labor markets with different degrees of informality. As a result, we show that the efficiency cost of a typical social program is not necessarily high(er) in a context of high(er) informality. Finally, we provide evidence for the insurance value of increases in UI benefits, the other side of the usual efficiency-insurance tradeoff with UI.

UI is an ideal program to study these issues. It requires the beneficiaries – displaced formal employees – to not be formally reemployed. It has been adopted or considered in a number of middle-income and developing countries.³ Moreover, international development agencies have

¹We define informal workers as those who escape government monitoring, which include informal employees and most self-employed workers in middle-income and developing countries. Informal workers are not eligible for UI; UI beneficiaries can work informally and still draw UI benefits. The set of (in)formal jobs – those held by (in)formal workers – constitute the (in)formal sector. We use "formal workers" and "formal employees" interchangeably.

²For instance, several papers investigate the impact of the Mexican *Seguro Popular* program, which has extended health care coverage to the non-formally employed (Azuara and Marinescu, 2013; Bosch and Campos-Vasquez, 2014). Yet, none uses a theoretical framework to interpret their findings in terms of the efficiency cost of the policy.

³Some form of UI exists in Algeria, Argentina, Barbados, Brazil, Chile, China, Ecuador, Egypt, Iran, Mexico, Turkey, Uruguay, Venezuela and Vietnam. The Philippines, Sri Lanka, and Thailand have been considering its introduction (Vodopivec, 2013; Velásquez, 2010).

emphatically pointed to the heightened moral hazard problem it supposedly creates in the presence of a large informal sector.⁴ Finally, compared to other programs, UI has been heavily studied in developed countries, which allows us to benchmark our results against estimates from contexts of low informality. Brazil also constitutes a uniquely well-suited empirical setting because it offers wide variation in the relative shares of formal and informal workers across labor markets. This allows us to further investigate how the efficiency cost varies with the degree of informality.

We begin by introducing informal job opportunities into the Baily-Chetty framework (Baily, 1978; Chetty, 2006, 2008). It is a partial-equilibrium framework that has been extensively used to evaluate the tradeoff with increases in UI benefits, between the need for insurance and the efficiency cost from the usual moral hazard problem, in developed countries. Our extension highlights the sufficient statistics that capture this tradeoff in our context. As often in economics, the efficiency cost is captured by the ratio of a behavioral effect to a mechanical effect ("leakage" ratio). The former measures the cost of the policy due to behavioral responses. Beneficiaries may delay formal reemployment by staying non-employed or working informally, thus drawing more UI benefits and contributing less to the UI system. The latter measures the cost that arises because some beneficiaries would draw the increased benefits absent behavioral responses. Welfare effects are positive if the ratio does not exceed the marginal value of insurance, the difference in the marginal utility of \$1 for mechanical beneficiaries – those targeted by the policy – and formal employees.

There are two main lessons from the theory. First, the behavioral effect only depends on the overall impact of behavioral responses on the paid UI duration and the time spent formally employed subsequently. One does not need to separate non-employment from informality responses (Feldstein, 1999). Our measure of efficiency costs is an upper bound, however, if some private costs of informality responses are not social costs (Chetty, 2009). Second, the efficiency cost is not necessarily high in a context of high informality. Ceteris paribus, the behavioral effect will be larger if workers have an extra margin of behavioral responses, i.e. working informally. However, the ceteris paribus condition may not hold. For instance, the two main views on the prevalence of informality suggest that displaced formal workers may return slower to a formal job in that context irrespective of UI. It may be harder for them to find a formal job ("exclusion" view) or they may choose to work informally for reasons unrelated to UI ("exit" view).⁵ The mechanical effect will be larger in that case. The potential (or "maximum") distortion will also be smaller: there is less room to delay formal reemployment when workers are already returning slower to a formal job.

⁴ "Because checking benefit eligibility imposes large informational and institutional demands, particularly under abundant and diverse employment opportunities in the unobservable informal sector, the resulting weak monitoring would make the incentive problem of the standard UI system much worse" (Robalino and Vodopivec, 2009). See also Acevedo, Patricio and Pagés (2006), and Vodopivec (2013). These policy papers cite evidence of moral hazard from Slovenia (van Ours and Vodopivec, 2006), a country with relatively low levels of informality.

⁵In the "exit" view, workers are voluntarily informal because they do not value the benefits of formality above its costs; in the "exclusion" view, workers are informal because formal jobs are more difficult to find (Perry et al., 2007).

As a result, the behavioral effect may not be larger, and the efficiency cost may even be smaller.

In light of the theory, we turn to the empirical analysis, which forms the core of the paper. We mainly rely on administrative data covering the universe of formal employment and UI spells over several years in Brazil. These data allow us to measure all the sufficient statistics capturing the efficiency cost. This would not be possible with survey data. Yet, we use survey data to investigate both the role of informal work opportunities in explaining our results and the value of insurance.

In a first step, we combine administrative and survey data to document key empirical patterns that differ from those observed in typical developed countries and motivate our analysis. First, most UI takers exhaust their benefits, e.g. more than 80% of those eligible for five months of UI do so.⁶ This is because the share who finds a new formal job each month is very low when workers are eligible for UI. This share is also lower than the share who finds an new formal job increases and peaks just after benefit exhaustion, which suggests clear behavioral responses. The pattern is not driven by workers leaving non-employment because the share who finds a new formal job also starts exceeding the share who finds any new job just after benefit exhaustion. This highlights the importance of informality responses. Third, the efficiency cost of increases in UI benefits may nevertheless be limited because the mechanical effect would be large. Indeed, displaced formal employees return slowly to a formal job even when they are no longer or not eligible for UI. This suggests that the decision of some of them to work informally is not only driven by UI incentives.

In a second step, we estimate the impacts and efficiency cost of an increase in potential UI duration. We do so by combining the administrative data with quasi-experimental variation from a tenure-based discontinuity. We estimate that a one-month increase in potential UI duration leads to a large increase in average paid UI duration (.86 month), but mostly because of a large mechanical effect. We show that the increase in formal reemployment rates after benefit exhaustion is driven by behavioral responses. Nevertheless, behavioral responses only account for 14.6% of the increase in paid UI duration. They also delay formal reemployment by .39 month and reduce the time spent formally employed subsequently by .24 month on average.⁷ Our results imply an upper bound for the efficiency cost of 20 cents per \$1 reaching mechanical beneficiaries. In comparison, results in Katz and Meyer (1990) and Landais (2015) imply an efficiency cost more than five times higher for the US, where the potential UI duration is comparable but informal work opportunities are limited.

In a third step, we replicate the second step in the 27 Brazilian states separately. We find that the efficiency cost is lower in states with higher informality. The mechanical effect is larger because displaced formal workers return slower to a formal job in those states even when they are

⁶About 35% of UI takers do so in the US where they are typically eligible for 24 weeks of UI (www.dol.gov).

⁷The literature often assumes these two effects to be symmetric. This is not the case in Brazil because, upon formal reemployment, workers only spend a share of their career formally employed. We find no impact on formal wages.

no longer or not eligible for UI benefits. Moreover, the impacts of behavioral responses on the paid UI duration, the duration out of formal employment, and the time spent formally employed are all smaller (in absolute values). This smaller behavioral effect is entirely due to the smaller potential distortion: workers who would have returned relatively rapidly to a formal job in absence of the policy appear equally responsive to the new incentives over our wide range of informality rates.

The above results suggest shifting the focus from efficiency concerns, as in the current policy debate, to workers' actual need for insurance in a context of high informality. An efficiency cost of 20 cents per \$1 implies that the welfare effect of an increase in potential UI duration would be positive if the average marginal utility of \$1 was only 20% larger for mechanical beneficiaries than for formal employees. Yet, a low efficiency cost could be consistent with a low value of insurance, e.g. if displaced formal workers can find informal jobs easily and those are good substitutes for formal jobs. In a final step, we then provide suggestive evidence using survey data. We find that mechanical beneficiaries experience lower levels of disposable income, despite the fact that many work informally. The marginal value of insurance may thus be sizable as other means of consumption smoothing may be more costly in developing countries (Chetty and Looney, 2006).

This paper contributes to the literature on optimal social insurance, which tends to focus on the context of richer countries.⁸ The closest paper to ours is perhaps Schmieder, von Wachter and Bender (2012*a*), which studies how the efficiency cost of increases in potential UI duration varies over the business cycle in Germany. Consistent with our findings, they find a lower efficiency cost during recessions when workers return slower to a formal job. Our paper differs in a key way: informality is low in Germany, but it is high in developing countries irrespective of business cycles.

This paper also contributes to a growing literature at the intersection of public and development economics.⁹ A theoretical literature argues that efficiency considerations may force governments to resort to alternative policies where enforcement is weak and informality is high. Yet, there is often little evidence on the efficiency cost of typical policies in the first place (Gordon and Li, 2009). We find that the efficiency cost of a typical social insurance policy is not necessarily higher in such a context and may even decrease with labor market informality. Moreover, because Brazil contains regions with such widely divergent levels of labor market informality, we are optimistic about the external validity of our study for other middle-income and developing countries.

Finally, this paper contributes to a growing literature on the impact of social policies in labor markets with high informality. First, it complements a literature that simulates the impact of policies in specific macro-search models (e.g. Meghir, Narita and Robin, 2015). A nice feature of our approach is that the set of sufficient statistics that capture our tradeoff of interest is not specific to

⁸See Chetty and Finkelstein (2013) for a review of this literature.

⁹See, for instance, Alatas et al. (2016), Best et al. (2015), Carillo, Pomeranz and Singhal (forthcoming), Gadenne (2014), or Naritomi (2015).

a particular model, but remains the same under a range of modelling assumptions (Chetty, 2006). This tradeoff is also not addressed in this literature, in which papers often assume away both the moral hazard problem and the insurance value by modelling UI as a lump-sum transfer upon layoff and workers as risk neutral. Furthermore, the many empirical moments that we document could help identify new models in this literature in the future. Second, this paper also complements another strand in the literature that uses quasi-experiments.¹⁰ Existing studies do not typically link their results to public economics theoretical frameworks, complicating interpretation. Last, this paper is the first to estimate how the impacts of a given policy vary with labor market informality.

The paper is structured as follows. Section 1 lays out our conceptual framework. Section 2 presents our empirical setting and our data. Section 3 documents empirical patterns that differ from those observed in richer countries and motivate our analysis. Section 4 estimates the average impacts and efficiency cost of an increase in potential UI duration and section 5 how these vary across labor markets with different degrees of informality. Section 6 provides suggestive evidence on the marginal value of insurance and discusses welfare effects. Section 7 concludes.

1 Conceptual framework

We begin by providing some background on labor markets with high informality. We then introduce informal job opportunities into the Baily-Chetty framework (Baily, 1978; Chetty, 2006, 2008). This allows us to identify sufficient statistics that capture the usual efficiency-insurance tradeoff with increases in UI benefits in our context. These statistics are sufficient to evaluate this tradeoff under a range of modelling assumptions (Chetty, 2006). We thus focus here on the intuition for our results. A specific model is presented in the Web Appendix.

1.1 Background

Labor markets in many middle-income and developing countries, including in all Latin American countries, feature both formal employees and a large share of informal workers. Formal employees typically work in jobs with regulated working conditions (e.g. overtime pay, firing costs), pay payroll and sometimes income taxes, and are entitled to a series of benefits (e.g. pensions). Informal workers, who pay no income or payroll taxes and are not eligible for these benefits, encompass unregistered employees in non-complying firms and most self-employed workers. A same firm may

¹⁰See, for instance, Bergolo and Cruces (2014), Camacho, Conover and Hoyos (2014), and Gasparini, Haimovich and Olivieri (2009), in addition to previously cited papers. Bergolo and Cruces (2014) estimate behavioral and mechanical effects of a policy on the government budget but do not study its efficiency cost. Two working papers, developed in parallel to our work, attempt to estimate the impact of UI on some labor market outcomes in Latin American countries (González-Rozada, Ronconi and Ruffo, 2011; Amarante, Arim and Dean, 2013). We discuss limitations of earlier working papers on UI in Brazil in the Web Appendix (Cunningham, 2000; Margolis, 2008; Hijzen, 2011).

hire both formal and informal employees.¹¹ Importantly for our purpose, informal workers cannot be covered by UI and the government cannot easily identify UI beneficiaries working informally.

There are two main views on the prevalence of informal workers in developing countries (Perry et al., 2007). In the "exit" view, workers are voluntarily informal because they do not value the benefits of formal employment above its costs. In the "exclusion" view, workers are informal because formal jobs are more difficult to find. Longitudinal survey data show that workers transit between formal and informal jobs in Latin American countries. This contradicts early versions of the exclusion view, which considered formal and informal sectors as segmented. However, it is fully consistent with formal jobs being more difficult to find (Meghir, Narita and Robin, 2015). Surveys also show that earnings levels are on average higher in the formal sector, although there is a lot of heterogeneity, and some workers may well be better off in the informal sector (for Brazil, see Botelho and Ponczek, 2011). Today, these two views are recognized as complementary.

1.2 Setup

Chetty (2008) considers a representative worker who lives for T periods, is laid-off at time zero, and is eligible for a UI benefit level b for a potential UI duration of P periods (period 1 is between time 0 and 1). When non-employed, she can search for a new job. She earns a net wage $w^F - \tau$ per period when reemployed, where τ is a tax financing the UI system. When all jobs are assumed to be formal, the moral hazard problem is that increases in UI benefits reduce job-search incentives.

We must make three changes to adapt this framework to our context. First, the worker must be laid off from a formal job. This is a condition for UI eligibility. Second, her job-search choices differ, as illustrated in Figure 1a. She can not only search for a new formal job, but also for an informal job. Relatedly, upon finding a job that would otherwise be formal, she can ask her employer to "hide" her on an informal payroll. In both cases, she might be willing to trade off utility gains from remaining eligible for UI against utility losses from informality costs, such as lower wages or hiding costs. The moral hazard problem thus now includes her increased incentives to work informally. Third, the UI tax can only be levied on formal workers. Besides these changes, we maintain other assumptions in Chetty (2008) for comparison purposes: the incidence of taxes and benefits falls on formal employees and beneficiaries, respectively; workers internalize all consequences of their choices except on the UI budget. We come back to these assumptions later in this section.

The worker's problem in this adapted Baily-Chetty framework is to choose optimal levels of search and hiding efforts in each period *t* after layoff and possibly of a series of other variables such as savings or reservation wages. The solution to this problem determines S_t the survival rate in non-formal employment (i.e. out of formal employment), $D^B \equiv \sum_{t=0}^{P-1} S_t$ the average paid UI

¹¹The 2002 World Bank's Investment Climate Survey in Brazilian manufacturing asked firms about the share of unregistered workers that a similar firm would likely employ. The median answer was 30% among small firms.

duration, $b D^B$ the average UI cost, $D^{NF} \equiv \sum_{t=0}^{\infty} S_t$ the average non-formal-employment duration, D^F the average time spent formally employed subsequently, and τD^F the average UI revenue.



Figure 1: Illustrations for our conceptual framework

Panel (a) illustrates the choice situation of the representative worker of the Baily-Chetty framework in a context of high informality. Upon layoff, she is eligible for a UI benefit level b_t for a potential UI duration of P periods ($b_t = b \mathbb{1}(t < P)$). While non-employed, she can search for a new formal job but also for an informal job, and she can keep searching for a formal job when working informally. Upon finding a job that would otherwise be formal, she can also ask her employer to "hide" her on an informal payroll. She only loses eligibility for UI when formally reemployed, so she might be willing to trade off utility gains from drawing UI benefits against utility losses from non-employment or informality, such as lower wages (w^i , w^h) or hiding costs. The moral hazard problem with increases in UI benefits thus now includes her increased incentives both to remain non-employed and to work informally. Panel (b) illustrates the mechanical and behavioral effects of an increase in UI benefits. It displays hypothetical survival rates in non-formal employment before the policy (solid line) and the associated average paid UI duration (light-gray area). First, increasing UI benefits increases UI costs through a mechanical effect. A higher benefit level (*db*) increases UI costs for a given average paid UI duration. A higher potential duration (*dP*) increases UI costs by increasing the average paid UI duration: workers who would have remained without a formal job after exhausting their benefits draw longer benefits mechanically (dark-gray area). Second, increasing UI benefits raises UI costs through a behavioral effect. Workers are likely to delay formal reemployment, shifting survival rates upward (dashed line) and increasing average paid UI duration by the difference between survival rates before and after the policy up to the potential duration. The average time spent formally employed subsequently will also decrease, reducing UI revenues.

1.3 Efficiency costs and welfare effects of changes in UI benefits

We can derive the welfare effect of increases in UI benefits in this relatively general framework through a perturbation argument. Increasing the benefit level db or the potential duration dP entails two types of effects.¹² This is illustrated in Figure 1b. First, there is a *mechanical effect*. UI beneficiaries would enjoy a higher benefit level without changing their behavior. Workers who would have remained without a formal job after exhausting their UI benefits would draw longer benefits mechanically. The mechanical effect increases UI costs by $\frac{d(b D^B)}{db}|_M = D^B$ and $\frac{d(b D^B)}{dP}|_M = b S_P$. These costs constitute a transfer from taxpayers to mechanical beneficiaries. The resulting welfare effect is: $(u'^b - u'^F) D^B$ and $(u'^P - u'^F) b S_P$, where u'^b and u'^P are the average marginal utilities of \$1 for mechanical beneficiaries with increases in the benefit level and the potential duration, respectively; u'^F is the average marginal utility when formally employed.

¹²We follow a main strand in the literature by considering changes in those policy parameters separately. Following Schmieder, von Wachter and Bender (2012*a*), a marginal change in potential UI duration (*dP*) corresponds to a marginal change in b_P , the benefit amount after the end of the preexisting potential UI duration, times *b*.

Second, there is a *behavioral effect*. Workers may change their behaviors in response to changes in UI benefits. Such behavioral responses do not generate any first-order gain in workers' utility (standard envelope argument), but some behavioral responses may affect the UI budget. In particular, increasing UI benefits reduces incentives to be formally reemployed, and increases incentives to stay non-employed or to work informally. Consequently, survival rates in non-formal employment may increase, increasing paid UI duration and thus UI costs by $b\frac{dD^B}{db}|_B = b\sum_{t=0}^{P-1} \frac{dS_t}{db}$ or $b\frac{dD^B}{dP}|_B = b\sum_{t=0}^{P} \frac{dS_t}{dP}$, depending on the policy change. The average time spent formally employed subsequently may also decrease, reducing UI revenues by $\tau \frac{dD^F}{dx}$ for $x \in \{b, P\}$. This behavioral effect must be paid for, so the resulting welfare effect is: $-u'^F \left[b\frac{dD^B}{dx}|_B - \tau \frac{dD^F}{dx} \right]$ for $x \in \{b, P\}$.

Putting everything together, we obtain the welfare effect of increases in UI benefits. It is common in public economics to measure welfare per unit impact on affected agents, so we normalize the welfare effect (dW) by the mechanical effect. We also divide by the average marginal utility of \$1 for formal workers to express welfare in a money metric (Chetty, 2006, 2008):

$$\frac{d\widetilde{W}}{db} = \frac{dW/db}{D^B u'^F} = \left(\frac{u'^b - u'^F}{u'^F}\right) - \left(\frac{b \frac{dD^B}{db}|_B}{D^B} - \frac{\tau \frac{dD^F}{db}}{D^B}\right) = \left(\frac{u'^b - u'^F}{u'^F}\right) - \left(\eta_{D^B,b} - \eta_{D^F,b}\right) \tag{1}$$

$$\frac{d\widetilde{W}}{dP} = \frac{dW/dP}{b S_P u'^F} = \left(\frac{u'^P - u'^F}{u'^F}\right) - \left(\frac{b \frac{dD^B}{dP}|_B}{b S_P} - \frac{\tau \frac{dD^F}{dP}}{b S_P}\right) = \left(\frac{u'^P - u'^F}{u'^F}\right) - \left(\frac{\sum_{t=0}^P dS_t/dP}{S_P} - \frac{D^B}{D^F} \frac{dD^F/dP}{S_P}\right)$$
(2)

where the simplification in equations (1) and (2) uses the UI budget constraint: $\tau D^F = b D^B$.

Equations (1) and (2) specify the usual trade-off with increases in UI benefits between insurance and efficiency. The welfare effect is positive if the *marginal value of insurance* exceeds the *efficiency cost*. The first parenthesis captures the marginal value of insurance, the relative welfare gain from transferring \$1 from formal workers to mechanical beneficiaries. It is likely positive when insurance markets are incomplete. The second parenthesis, the ratio of the behavioral to the mechanical effect, captures the efficiency cost, or the resources lost per \$1 reaching mechanical beneficiaries. It is common in public economics for the efficiency cost of a policy to be captured by such a "leakage" ratio, which often translates into behavioral elasticities (η) as in equation (1).

Equations (1) and (2) show that the efficiency cost can be evaluated by estimating a few sufficient statistics: the mechanical effect, and the impact of behavioral responses on the average paid UI duration and on the average time spent formally employed subsequently. We estimate such statistics and evaluate the efficiency cost for increases in potential UI duration in Sections 4 and 5. Doing so allows some welfare statements. Suppose we estimate an efficiency cost of 50 cents per \$1 reaching mechanical beneficiaries. The welfare effect would then be positive if the average marginal utility of \$1 was at least 50% higher for mechanical beneficiaries than for formal workers. Such a bound could be informative in some settings. However, one must also evaluate the marginal

value of insurance to pin down the welfare effect. This is challenging because marginal utilities are not easily measured. One approach is to use the following approximation as in Gruber (1997a):¹³

$$\frac{u'^{x} - u'^{F}}{u'^{F}} \simeq \gamma \frac{c^{F} - c^{x}}{c^{F}}, \text{ with } x \in \{b, P\}$$
(3)

The marginal value of insurance becomes the product of the relative difference in consumption levels (*c*) between formal employees and mechanical beneficiaries, and the average coefficient of relative risk aversion (γ). The applicability of this decomposition is often limited by the lack of suitable consumption data and by the fact that the relevant value of γ is typically unknown.¹⁴ In section 6, we provide suggestive evidence by estimating the difference in disposable income levels between formal employees and mechanical beneficiaries, using it to approximate the difference in consumption levels, and then calibrating the marginal value of insurance for different values of γ .

Finally, the marginal value of insurance and the efficiency cost of increases in UI benefits likely vary with the preexisting benefit level and potential duration. However, the sign of the welfare effect for a given starting point indicates whether the optimal benefit level b^* or potential duration P^* is above or below the existing value of the policy instrument under a convexity assumption.¹⁵

1.4 The role of informal job opportunities

Equations (1) and (2) show that it is not necessary to estimate the effect of increases in UI benefits on all margins of behaviors to estimate the efficiency cost. In particular, it is not necessary to know whether workers remain non-employed or work informally when they delay formal reemployment. A corollary is that equations (1) and (2) apply even in the absence of informal job opportunities.¹⁶ This result is the UI version of Feldstein (1999), who argues that it is not necessary to know whether changes in tax rates affect taxable income through changes in labor supply or tax evasion to estimate their efficiency cost. Both types of responses affect the tax base (resp. the UI benefit and UI tax bases) similarly; both involve private utility costs, such as lower income or evasion costs (resp. lower wages or hiding costs), and so generate no first-order utility gains for individuals.

Why does informality matter then? First, the size of the sufficient statistics may differ in labor

¹³The approximation assumes that third derivatives of utility functions are small (Chetty, 2006).

¹⁴Chetty (2008) argues that the marginal value of insurance can also be decomposed into estimable sufficient statistics under additional assumptions: the impacts of changes in incentives vs. liquidity on rates of (formal) reemployment. The applicability of this decomposition is limited in our context because, if rates of (formal) reemployment are affected by several choice variables (e.g. search efforts for formal and informal jobs, reservations wages), one must estimate those impacts holding constant the level of all choice variables but one (e.g. search efforts for formal jobs).

¹⁵Simulations in Chetty (2008) suggest that this assumption holds in a comparable framework. It holds, e.g., if the marginal value of insurance (resp. the efficiency cost) is decreasing (resp. increasing) in the policy parameter.

¹⁶In fact, equations (1) and (2) translate to expressions in Chetty (2008) and Schmieder, von Wachter and Bender (2012*a*) when all jobs are formal, if one further assumes that new formal jobs are never lost ($D^F = T - D^{NF}$).

markets with high informality. Ceteris paribus, the behavioral effect will be larger if workers have an additional margin of behavioral responses, i.e. working informally. However, the ceteris paribus assumption may not hold. There are many differences in those labor markets besides workers' ability to respond to UI incentives. As a result, the efficiency cost will not necessarily be higher. For instance, the two main views on the prevalence of informality suggest that displaced formal workers may return slower to a formal job in those labor markets, irrespective of (increases in) UI benefits. It may be more costly to find formal jobs, and informal jobs may be attractive for reasons unrelated to UI. The mechanical effect will be larger in that case, as survival rates in non-formal employment will be higher to begin. Moreover, the behavioral effect may not be larger because the potential (or "maximum") distortion will be smaller: there is less room to increase survival rates in non-formal employment when workers are already returning slower to a formal job. Employment decisions may also be less responsive if it is costly to find formal jobs or if workers are working informally for reasons unrelated to UI.¹⁷ Ultimately, how the efficiency cost compares in contexts of high informality is thus an empirical question. Similarly, how the marginal value of insurance compares is also an empirical question. Ceteris paribus, it will be lower, as informal jobs provide an additional means of consumption smoothing. Yet, it may be higher if other means of consumption smoothing are more costly in developing countries (Chetty and Looney, 2006).

Second, our measure of efficiency costs may be an upper bound. We assume that workers internalize all consequences of their choices except on the UI budget, which is equivalent to assuming that private and social costs of behavioral responses are equal. This is why it is not necessary to estimate all behavioral responses in our framework or in Feldstein (1999). However, Chetty (2009) argues that Feldstein (1999) overestimates the efficiency cost by assuming that private and social costs of tax evasion responses are equal. Tax evasion responses always entail private costs but sometimes also generate positive externalities such that their social costs can be lower than their private costs. The same argument applies in our case. For instance, the fine when a worker is found working informally is a private cost of informality responses, but it has a positive externality on the government budget. Yet, many other private costs of informality responses do not generate such externalities (e.g. lower wages, hiding costs), so our measure of efficiency costs is likely informative. In fact, Best et al. (2015) argue that considering all private costs of tax evasion responses (or informality in our case) as social costs is a natural starting point for developing countries.

1.5 Informality and the assumptions of our conceptual framework

Before turning to the empirics, we review important assumptions of the Baily-Chetty framework that we maintained and discuss their relevance in a context of high informality.

¹⁷Formally, comparative statics depend on the sign of third derivatives of search cost functions. Schmieder, von Wachter and Bender (2012a) make a comparable point about the size of the behavioral effect over the business cycle.

We abstract from the possible impact of UI on layoff decisions. This is because this distortion can be tackled by the experience-rating of UI benefits. In practice, UI benefits are never perfectly experienced-rated and this has been shown to increase layoffs in developed countries. The same problem may be important in our context. For instance, a firm may report laying off a worker eligible for UI, but keep her on an informal payroll. We show in the Web Appendix that UI seems to distort reported layoff decisions at low tenure levels in Brazil. However, existing institutions such as high firing costs appear sufficient to prevent such responses for other groups of workers.

We assume that the incidence of taxes and benefits falls on formal employees and beneficiaries, respectively. These are reasonable assumptions at least in the long run. There is also some empirical evidence that the incidence of taxes on formal workers falls on those workers in Latin American countries, at least for those earning more than the minimum wage.¹⁸ We verify that results are not driven by workers earning close to the minimum wage in our empirical application.

We consider the UI program in isolation. In reality, decreases in formal employment due to increases in UI benefits may create fiscal externalities on other tax or benefit bases. This is certainly the case in developed countries where formal labor income is an important tax base. Developing countries rely less on labor income taxes,¹⁹ and it is not straightforward how decreases in formal employment would affect tax bases that they rely more on (e.g. consumption). Moreover, an increase in UI benefits may in fact increase formal employment in a general equilibrium model if risk-averse workers value it above the tax that they pay for it (Acemoglu and Shimer, 1999). Fiscal externalities may also arise from impacts of increases in UI benefits on formal reemployment wages, even when considering the UI program in isolation, if we assumed that the UI tax was proportional to wages. However, we find no evidence of such an impact in our empirical application.

There may be other relevant general equilibrium effects with ambiguous implications for efficiency. For instance, search externalities would mitigate the efficiency cost (Landais, Michaillat and Saez, 2010), and may be important if informality is partly explained by the exclusion view.

Finally, the optimal UI literature typically assumes that new jobs are never lost (e.g., Chetty, 2008) or $D^F = T - D^{NF}$, for some fixed horizon T. One can then estimate impacts on non-formalemployment duration instead of impacts on the time spent formally employed subsequently: $\left|\frac{dD^F}{dx}\right| = \left|\frac{dD^{NF}}{dx}\right|$. We do not follow this assumption, which would bias estimates of the efficiency cost in our context because new formal jobs are often lost. Upon formal reemployment, suppose that a worker would spend a share $q \in [0, 1]$ of her remaining career formally employed: $D^F = q (T - D^{NF})$. We

¹⁸See for instance, Cruces, Galiani and Kidyba (2010), Gruber (1997*b*), and Kugler and Kugler (2009). Table 6 in Cruces, Galiani and Kidyba (2010) shows that their less-than-full incidence result is driven by a tax change for which they can only noisily estimate very short-term effects. The incidence of tax changes for which they can estimate longer-term effects and for which the minimum wage is not binding falls on formal workers.

¹⁹Most formal workers do not pay income taxes in Brazil. Payroll taxes are high but they are mostly benefit taxes, so changes in formal employment affect both costs and revenue with ambiguous net effects on the government budget.

then have: $\left|\frac{dD^{NF}}{dx}\right| \ge \left|\frac{dD^F}{dx}\right| = q \left|\frac{dD^{NF}}{dx}\right|$. Moreover, increases in UI benefits give workers more time and resources to look for better and more stable jobs, so we could also have: $\frac{dq}{dx}(T - D^{NF}) \ge 0.^{20}$

In sum, our assumptions constitute a natural starting point and a useful benchmark. Moreover, they do not appear to bias our comparison of estimates of the efficiency cost in a context of high informality with existing estimates from more developed countries in a predictable direction.

2 Empirical setting: motivation, background, and data

In this section, we present our empirical setting, relevant institutional details, and our data.

2.1 Why Brazil?

Brazil is an appealling empirical setting for several reasons. First, UI has existed in Brazil for many years, so workers are aware of its associated incentives. Second, the longest potential UI duration is five months. We thus investigate the efficiency cost of increasing potential duration from a relatively low level, which is the relevant starting case for developing countries. Moreover, the potential UI duration is comparable in the US, which allows us to benchmark our results against US estimates. Third, Brazil is not an outlier in terms of informality: its average informality rate is close to the average across Latin American countries (Perry et al., 2007). Figure 2a displays the average composition of the labor force in Brazil over our main period of analysis (2005-2009). Every worker has a working card in Brazil. When an employer signs her working card, which is mandatory, her hiring is reported to the government, and she becomes a formal employee. Yet, hiring an employee formally is costly and informality is high even in non-farm employment.²¹ In fact, Figure 2a shows that the share of non-farm private formal employees is about equal to the share of non-farm informal workers (informal employees and self-employed workers). Fourth, there is a lot of heterogeneity in informality rates across labor markets in Brazil. Figure 2b shows that the shares of non-farm private formal employees and non-farm informal workers vary greatly across states. These shares also strongly correlate with levels of income per capita, as they do across countries (Perry et al., 2007). This heterogeneity allows us to investigate how the efficiency cost varies with the degree of informality.²² Finally, we have access to comprehensive administrative data, which,

 $^{^{20}}$ A similar point is made in Schmieder, von Wachter and Bender (2012*b*).

²¹Payroll taxes include 20% for Social Security, 8% deposited in workers' seniority account (FGTS), and 7.8% for funding an array of programs (e.g. training, education). Formal employees are also entitled to the minimum wage, a 13th monthly wage, 30 days of paid leave per year, an overtime rate of 50% for hours exceeding 44 hours a week, etc. We do not consider farm workers in the paper because a negligible share of them draw UI.

²²We consider the heterogeneity across Brazilian states, and thus do not consider alternative definitions of labor markets, because it is not possible to construct measures of informality rates at lower levels of aggregation over our main period of analysis (2005-2009) in Brazil. This can only be done using data from the decennial censuses.



Figure 2: Labor market composition in Brazil (2005-2009)

Source: PNAD. In panel (a), "informal workers" consists of informal employees (12.3%) and self-employed workers (14.6%), and "other non-farm" of employers (3.5%), public employees (6.8%), domestic employees (7.1%), and unpaid workers (1.3%). In panel (b), each dot is a state average; lines are unweighted linear fits. The average monthly income per adult is calculated for adults 19 to 54 years old.

combined with quasi-experimental variation in UI benefits, allow us to estimate all the sufficient statistics in Section 1. It wouldn't be possible with the survey data available in Brazil or in other developing countries. Yet, existing surveys also offer some advantages in Brazil (see below).

2.2 The Brazilian UI program

The Brazilian UI program, which current rules have applied to since 1994, works as follows. A worker who is laid off from a private formal job and who has at least six months of job tenure at layoff is eligible for UI benefits after a 30-day waiting period. There must also be at least 16 months between a worker's layoff date and the layoff date of her last successful application. She must apply in person for UI within 120 days of her layoff date. If she is deemed eligible, her UI benefits are automatically deposited every 30 days at a state bank, Caixa, as long as her name does not appear in a database where employers report new hirings monthly. The potential UI duration depends on her accumulated tenure across all formal jobs in the 36 months prior to layoff. She is eligible for up to three, four, or five monthly UI payments if she has more than 6, 12, or 24 months of accumulated 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 (see the full schedule in the Web Appendix). The replacement rate is thus very high at the bottom of the wage distribution (100% for workers who earned the minimum wage). Importantly, however, all our results are robust to excluding beneficiaries with very high or very low replacement rates.

A departure from our framework is that UI is financed by a .65% tax on firms' sales in Brazil.

We considered instead the case of a tax on formal workers, which is the main source of funding for UI in other countries, including developing countries (Velásquez, 2010). A tax on formal workers is the interesting case conceptually. They are the beneficiaries of the program and UI aims at providing insurance, not at redistribution. The incidence of a tax on formal workers is also likely to fall on those workers, and is certainly more likely to do so than a sales tax. We thus use Brazil as an empirical setting to estimate and illustrate the efficiency cost of increases in UI benefits in a context of high informality as derived in a benchmark framework. The odd financing of the Brazilian program is unlikely to invalidate this objective. A 2.5% payroll tax would be sufficient to fund UI (UI expenditures/total eligible payroll $\simeq .025$) and it is unlikely that the composition of the formal labor force would be very different substituting such a tax for the existing one.²³

Finally, as in other countries with UI, displaced formal employees are entitled to some layoff benefits. In particular, employers must give them a one-month advance notice and a severance payment of 40% of the amount deposited in their seniority account (8% of their wage each month) during their tenure at the firm. Formal employees are only allowed to withdraw from this account upon layoff or retirement. Finally, firms face financial penalties for not complying with labor laws, including hiring workers informally, but the risk of detection is low (Almeida and Carneiro, 2012).

2.3 Data

We provide a brief description of our data below and more details in the Web Appendix.

Our analysis relies on two administrative datasets. RAIS is a matched employee-employer dataset covering by law the universe of formal employees, including public employees. Every year, all tax-registered firms must report all workers formally employed at any point during the previous year.²⁴ RAIS has data on tenure, age, gender, education, sector, establishment size and location, reason for separation, and since 2002, hiring and separation dates for every job 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. We are the first researchers to use the UI registry. It includes the application date, the amount, the month, and since 2005, the date of all UI payments. We merge workers in both datasets through a unique ID number. We then use the data to measure the paid UI duration, the non-formal-employment duration, and the time spent formally employed subsequently for displaced formal employees (from now on we omit the term "private"), as well

²³With a sales tax, u'^F in equations (1) and (2) would become the marginal utility of those bearing the incidence of the tax. The tax may also increase the efficiency cost if its incidence does not fall on those benefiting from UI.

²⁴Compliance 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 to formal employees. There are thus incentives for truthful reporting. RAIS is also used by some ministries to monitor formal job takeup. RAIS has a slightly better coverage of formal employment than the data used by the UI agency (MTE, 2008). Accordingly, a few workers reported as formally reemployed in RAIS are still drawing benefits in the UI data. This slightly biases our estimates of the efficiency cost upward.

as to estimate the impact of changes in UI benefits on these variables. Our main analyses use data from 2005 to 2010, such that we have exact separation, hiring, and payment dates. We also confirm all our results using data from 2002 (imputing payment dates). To provide some perspective, there were about 40,300,000 formal employees and 625,650 new UI beneficiaries in each month in 2009.

We also use the microdata of two surveys conducted by the Brazilian Institute of Geography and Statistics (IBGE). Both surveys ask for the labor market status of every household member above ten years old, including information on wage (and other income), tenure, and the signing of the working card. To guarantee confidentiality, survey respondents cannot be matched to other datasets. We measure informality rates in the 27 Brazilian states as in Figure 2, using data from yearly household surveys (PNAD), which are representative at the state level. In contrast, monthly labor force surveys (PME) are only representative for the six largest metropolitan areas of Brazil, but they have a panel structure. Households are surveyed for two periods of four consecutive months, eight months apart from each other. We use PME to assess the importance of informal work opportunities for displaced formal employees and to investigate the need for insurance.

3 Key empirical patterns motivating our analysis

We begin our empirical analysis by documenting key empircal patterns for displaced formal workers, which differ from those observed in typical developed countries. We construct patterns of UI benefit collection and formal reemployment from the administrative data. Specifically, we measure survival rates in non-formal employment, and the shares finding a new formal job, taking up UI, drawing UI, and exhausting UI in each month since layoff. In the administrative data, we have no information on the labor status of a displaced formal worker who is not yet formally reemployed. We thus use PME to estimate survival rates in non-employment and the share finding any new job (formal or informal) in each month since layoff. We then assess the importance of informal work opportunities by contrasting patterns of formal reemployment and overall reemployment for comparable samples of workers. We present three main lessons from the data, after summarizing the data construction. More details and robustness checks are provided in the Web Appendix.

3.1 Data construction

We measure the survival rate in non-formal employment at the start of each month since layoff from the layoff and hiring dates in the administrative data. The share finding a new formal job in each month is simply the difference between survival rates at the start and end of the month. The hazard rate of formal reemployment is the ratio of that share to the survival rate at the start of the month. We measure the shares taking up UI, drawing UI, and exhausting UI in each month since layoff from the layoff, hiring, UI application, and UI payment dates in the administrative data. Specifically, we consider a worker as taking up UI (resp. drawing UI, exhausting UI) in a month if she applies within 120 days of layoff and draws her first UI payment (resp. any UI payment, her last UI payment given her potential UI duration) in that month before being formally reemployed.

We estimate displaced formal workers' hazard rate of overall reemployment in each month since layoff by maximum likelihood using the panel structure in PME. It is then straightforward to compute survival rates in non-employment and the share finding any new job in each month. We can identify non-employed displaced formal employees in PME because non-employed individuals are asked about characteristics of their previous job (working card signed, reason for separation, tenure at layoff). We then estimate a piece-wise constant hazard function using information about the length of their non-employment spell (in months) and whether they are still non-employed in the next month. Specifically, we allow for different hazard rates in months 0, 1-2, 3-4, 5-6, 7-8, and 9-10 since layoff. We restrict the time horizon after layoff and we group months by pair because of the limited sample size. We allow for a different hazard rate in month 0 because of the 30-day waiting period. Finally, we use sampling weights and cluster standard errors by individual.²⁵

We use comparable samples in the administrative data and in PME. We only know UI eligibility and potential duration for non-employed workers who had more than 24 months of tenure at layoff in PME. This is because we don't have any information about UI benefits or about tenure in other formal jobs than the last one in the 36 months prior to layoff. We thus restrict our samples to those workers; their tenure at layoff is sufficient to know that they are eligible for five months of UI. They accounted for 30% of displaced formal employees and 37% of UI takers between 2005 and 2009. We further select individuals 18-54 years old who were laid off from a full-time private formal job between 2005 and 2009 in the six metropolitan areas covered by PME. Our PME sample includes 19,904 observations from 12,327 individuals contributing to the likelihood function. Our administrative sample includes 3,393,055 individuals. We do not consider workers laid off in 2010 to avoid right-censoring issues. Concerns related to attrition are limited with PME; our PME sample would be only 5.4% higher in the absence of attrition. Moreover, we show in the Web Appendix that our results are similar if we reweight the PME sample such that it compares

²⁵The likelihood function corrects for a stock sampling issue within month. Define λ_m , the daily hazard rate constant over month *m* since layoff. Furthermore, define k(b) with $b \in [0, 30]$, the distribution of survey interviews over days within a month. To be recorded as non-employed, an individual must survive *b* days without a job given that she survived *m* months. Finally, define $d_{i,m} = 1$ if individual *i*, non-employed since month *m*, is recorded as employed in the following month. The likelihood for a given observation is then: $L_{i,m} = d_{i,m} \int_0^{30} [1 - exp(-(30-b)\lambda_m - b\lambda_{m+1})] \frac{k(b)exp[-b\lambda_m]}{\int_0^{30} k(s)exp[-s\lambda_m]ds} db + (1 - d_{i,m}) \int_0^{30} [exp(-(30-b)\lambda_m - b\lambda_{m+1})] \frac{k(b)exp[-b\lambda_m]}{\int_0^{30} k(s)exp[-s\lambda_m]ds} db$. PME interviews are evenly spread over a month, so we assume k(b) = 1/30. Standard errors for the shares reemployed and the survival rates in non-employment are obtained by the delta method. We do not study the formality of new jobs in PME because the reemployment of workers with the working card signed, as reported in PME, may not be reported to the government immediately.

Figure 3: UI benefit collection, formal reemployment, and overall reemployment patterns after layoff



(a) UI benefit collection in each month

(b) Share finding a new formal job vs. find- (c) Survival rate ing any new job in each month

in non(-formal)employment at the start of each month

Displaced formal employees eligible for five months of UI after layoff (after a 30-day waiting period). UI benefit collection and formal reemployment patterns are constructed using administrative data. Overall reemployment patterns are estimated using survey data (PME). Panel (a) displays the share taking up UI, drawing UI, and exhausting UI in each month since layoff. Panels (b) and (c) display the share finding a new formal job in each month and the survival rate in non-formal employment at the start of each month, for all workers, for UI takers, and for those who took up UI in their first month of eligibility. These statistics are population means, so we don't include confidence intervals. Panels (b) and (c) also display the estimated share finding any new job in each month since layoff and the estimated survival rate in non-employment at the start of each month for all workers (we don't observe UI benefit collection in PME) with their 95% confidence intervals.

better to the administrative sample based on observables.²⁶

Finally, note that considering UI for workers with some attachment to their formal job, e.g. 24 months of tenure, may in fact be the most interesting starting case for developing countries. Moreover, we show in the Web Appendix that formal reemployment rates are very similar for workers in our sample and for a random sample of all displaced formal workers in Brazil. Note also that we cannot estimate survival rates in non-formal employment in PME or in similar surveys in other countries. The panel is too short and working individuals are not asked about previous jobs or non-employment spells. For instance, we don't know when a worker who is non-employed in her first interview and informally reemployed in her second interview is first formally reemployed, if she is not formally reemployed by the fourth interview. We thus need to combine survey and administrative data to assess the importance of informal work opportunities.

3.2 **Three main lessons**

We draw three main lessons from the data, which are based on the empirical patterns presented in Figure 3. Panel (a) displays the share taking up UI, drawing UI, and exhausting UI in each month since layoff. Panels (b) and (c) display the share finding a new formal job in each month and the survival rate in non-formal employment at the start of each month. For expositional purpose, we present these statistics separately for all workers, for UI takers, and for those who took up UI in their first month of eligiblity. Panels (b) and (c) also display the share finding any new job in each month and the survival rate in non-employment at the start of each month estimated in PME for all

²⁶We also provide descriptive statistics for the two samples in the Web Appendix.

workers (we don't observe UI benefit collection in PME). The corresponding hazard rates are only displayed in the Web Appendix because they don't provide any additional information.

Lesson 1. Most UI takers exhaust their benefits, and average paid UI duration is high, because the share finding a new formal job in each month is low while workers are eligible for UI. The share finding any new job is much higher, so many UI beneficiaries must be working informally.

Most UI takers exhaust their benefits. For instance, panel (a) shows that 45.5% of displaced formal employees take up UI in their first month of eligibility and that 41.1% exhaust their benefits exactly in their fifth month of eligibility. In total, 80.6% take up UI and 83% of UI takers exhaust their benefits, for an average paid UI duration of 4.63 months among UI takers. Exhaustion rates were only around 35.6% over the same period in the US, where potential UI duration is only slightly higher (23.8 weeks on average).²⁷ The difference is due to the lower rates of formal reemployment while workers are eligible for UI. For instance, panel (c) shows that 86.5% of the workers who took up UI in their first month of eligibility remain without formal employment five months after layoff. The figure is lower for all workers because many UI non-takers find a formal job during the 30-day waiting period. Importantly, we show in the Web Appendix that those patterns are not specific to workers with high replacement rates.

Panel (b) shows that the share of all displaced formal employees finding any new job exceeds the share finding a new formal job in the first months after layoff. As a result, 35.8% of them remain non-employed five months after layoff while 77.6% remain without formal employment (panel c). Many must thus be working informally, including many UI beneficiaries because 68.8% of all displaced formal employees are still drawing UI benefits in the following month (panel a).²⁸

Lesson 2. The share finding a new formal job in each month increases, peaks, and starts exceeding the share finding any new job exactly after benefit exhaustion. This suggests a clear behavioral response to UI and the importance of informal work opportunities to explain it.

Panel (b) shows that the share of displaced formal employees finding a new formal job in each month increases and peaks exactly after most of them exhaust their UI benefits. For instance, the share more than doubles in the month of UI exhaustion for those who took up UI in their first month of eligibility, and peaks in the following month. This suggests a clear behavioral response to UI incentives. Such a pattern is not observed in most developed countries (Card, Chetty and Weber, 2007*b*).²⁹ It is similar but attenuated in the other samples because of UI non-takers and because those who took up UI later also exhaust their benefits later. We show in the Web Appendix that this pattern is not simply due to workers returning to the same employer.

²⁷Excluding years with extended benefits (www.dol.gov). High exhaustion rates have also been documented for the cases of Argentina and China (González-Rozada, Ronconi and Ruffo, 2011; Vodopivec and Tong, 2008).

²⁸Among those who report being reemployed informally in the first five months after layoff in our PME sample, 32.7% and 61% report being reemployed as self-employed workers and informal employees, respectively.

²⁹van Ours and Vodopivec (2006) find a sizeable spike in Slovenia.

Panel (b) also shows that the share of all displaced formal employees finding any new job in each month since layoff does not increase after UI exhaustion. In fact, the share finding a new formal job in each month becomes larger than the share finding any new job, exactly after most UI takers exhaust their benefits. Therefore, behavioral responses to UI incentives cannot be simply explained by workers remaining non-employed. In particular, the increase in the share formally reemployed after benefit exhaustion must be due to a decrease in the share finding an informal job among those finding a new job, an increase in the share finding a new formal job among those working informally, or an increase in the share moving to the formal payroll within a firm.

Lesson 3. The mechanical effect of increases in UI benefits would be relatively large. Average paid UI duration is already high (for increases in the benefit level) and rates of formal reemployment remain low even after benefit exhaustion (for increases in the potential duration).

The average paid UI duration is already high, therefore the mechanical effect of an increase in the UI benefit level would be large. The mechanical effect of an increase in the potential UI duration corresponds to the survival rate in non-formal employment after benefit exhaustion (the equivalent of S_P if takeup took place at time 0). It would also be large because rates of formal reemployment remain low even after benefit exhaustion in Brazil. For instance, panel (c) shows that 77% of displaced formal employees who took up UI in their first month of eligibility would draw an extra month of UI absent any behavioral response. In fact, 48.4% of them remain without a formal job ten months after layoff. As shown in equations (1) and (2), relatively large mechanical effects will limit the efficiency cost from behavioral responses to increases in UI benefits.

The fact that survival rates in non-formal employment remain high after benefit exhaustion, even though survival rates in non-employment are much lower, indicates that the decision to work informally is not only due to UI incentives in Brazil. A related statistic is that most formal employees spend a significant amount of time out of formal employment over their career. For instance, we show in the Web Appendix that a random sample of workers formally employed in 2000 spent on average only 62% of their career formally employed between 2000 and 2010 (81.3 months), were laid off 2.1 times from a formal job, and drew a total of 7.6 months of UI.

3.3 Generalizability of the empirical patterns

The samples used for Figure 3 are constrained by features of PME surveys. However, in addition to the robustness checks mentioned above, we show in the Web Appendix that all the patterns based on the administrative data generalize to samples from earlier years, from other parts of Brazil, with different tenure levels, and with different levels of potential UI duration. Average paid UI duration is always very high relative to potential UI duration. Rates of formal reemployment always increase sharply after benefit exhaustion, but remain low. Importantly, the correspondingly

large mechanical effect is unlikely due to some long-term effect of UI eligibility in earlier months because rates of formal reemployment are also low for workers who are not eligible for UI.

4 The efficiency cost of increases in potential UI duration

In Section 3, we documented evidence of behavioral responses to UI incentives, which appeared to be driven by informality responses. We also documented that displaced formal employees return slowly to a formal job even when they are no longer (or not) eligible for UI. Consequently, the efficiency cost of (increases in) UI benefits may be limited, despite evidence of incentive effects.

In this section, we directly estimate the impacts and efficiency cost of increases in UI benefits. We exploit variation in potential UI duration at a tenure-based cutoff through a regression discontinuity (RD) design.³⁰ We then compare our estimates with estimates from contexts of low(er) informality. We also confirm our results using another empirical strategy in the Web Appendix. We present the RD design in the paper because it allows us to study how the impacts and efficiency cost vary across Brazilian labor markets with different informality rates in the next section.

4.1 A tenure-based eligibility cutoff

Displaced formal employees are eligible for three, four, or five months of UI if they have more than 6, 12, or 24 months of accumulated tenure in the 36 months prior to layoff. We exploit the 24-month cutoff for workers who had a single formal job in the previous 36 months using the administrative data. We cannot apply the RD design in PME because we only know UI eligibility and potential UI duration for workers with more than 24 months of tenure at layoff in PME.

Our focus on workers who had a single formal job in the previous 36 months is due to 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. First, there is a mandatory one-month advance notice of layoff in Brazil. Firms either lay off workers immediately, paying an extra monthly wage, or keep workers employed during the period. We cannot separately identify these cases in RAIS and the advance notice period counts for UI eligibility. Second, a partial month of tenure can count as a full month for UI eligibility. We thus measure the running variable, and consequently UI eligibility, with less

³⁰In theory, one could also exploit variation in UI benefit levels from kinks in the benefit schedule through a regression kink (RK) design. We show in the Web Appendix that there is no evidence of behavioral responses around those kinks, despite large changes in slope. The conclusion of this section – that the efficiency cost is relatively low – would thus carry over to increases in the UI benefit level. However, this could be because the kinks are located at high levels of the replacement rate in Brazil (80% and 68%). Beneficiaries may be less responsive to changes in benefit levels at high replacement rates, especially when some beneficiaries are also working informally. This is why we focus on variation in potential UI duration, for which we have variation at all levels of the replacement rate, for which we find clear evidence of behavioral responses, and for which we can then study heterogeneous impacts across labor markets.

noise for workers who had a single formal job spell in the previous 36 months, which is useful for our empirical strategy. These workers constitute 50.2% of workers, and 51.6% of UI takers, with tenure levels around the 24-month cutoff. Importantly, this selection is unlikely to drive our results. We obtain similar results with the other empirical strategy, which does not impose such a selection. Moreover, rates of formal reemployment are comparable in our RD sample (see below), for all workers with more than 24 months of tenure at layoff (see Figure 3c), and for a random sample of all displaced formal workers over the same period (see Web Appendix).

We exploit 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). This should not be surprising. There is no other policy varying discontinuously at this cutoff. Moreover, the decision to lay off workers with relatively high tenure at a firm (or to report them as laid off) may not be very responsive to variation in their UI benefits upon layoff, even in the absence of experience-rating of UI benefits. They may have accumulated job-specific human capital and firing costs are sizable for these workers. In particular, termination of an employment contract for workers with more than 12 months of tenure must be overseen by a union or a Labor Ministry representative. This increases firing costs because of the administrative burden it imposes and because of firms' often imperfect compliance with workers' dues. In contrast to the 24-month cutoff, we show in the Web Appendix that the layoff density is not smooth around the other eligibility cutoffs. It increases discontinuously at six months of tenure. The decision to lay off workers recently hired at a firm may be more responsive to UI eligibility.³¹ It decreases discontinuously at 12 months of tenure, which is when firing costs also increase.

Our main sample of analysis includes all individuals in the administrative data, 18-54 years old, laid off between 2005 and 2009, and 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 months (resp. five months) of UI. Workers with 22 to 24 months of tenure were eligible for either four or five months of UI 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 duration precisely. The final sample includes 2,283,765 observations. Its composition is discussed below and more details on its construction are provided in the Web Appendix.

4.2 Graphical evidence

We begin by providing some graphical evidence in Figure 4. Panels (a) and (b) display survival rates in non-formal employment and hazard rates of formal reemployment, constructed as in sec-

³¹In fact, the layoff density also increases discontinuously at another eligibility cutoff for such workers, when they reach 16 months between their layoff date and the layoff date of their last successful application to UI.

tion 3, for UI takers eligible for four vs. five months of UI in our sample. Hazard rates are very low while workers are eligible for UI. They start increasing sharply after four months for workers eligible for four months of UI, but only after five months for those eligible for five months of UI. Survival rates then remain higher for a few months for workers eligible for five months of UI. Yet, the behavioral effect suggested by these patterns is small compared to the mechanical effect because survival rates remain relatively high even for workers eligible for four months of UI.

Panels (c)-(f) display a series of RD graphs, which plot outcome variables aggregated by tenure levels around the cutoff. Panel (c) shows that there is no visible change in the distribution of our sample around the cutoff. In the Web Appendix, we also show that there is no visible change in the composition of our sample around the cutoff. Those graphs support the validity of our RD design.

Panel (d) disentangles the behavioral and mechanical effects of the increase in potential UI duration on the average paid UI duration of UI takers (takeup is constant at around 86%; see graph in Web Appendix). For this purpose, it displays two variables. First, it displays the average paid UI duration, which is redefined as follows because UI takeup is imperfect and does not take place at time 0: $D_i^B \equiv \sum_{j=1}^P \mathbb{1}(DrawingUI_{i,j} = 1)$, where $P = \{4,5\}$ is the potential UI duration and where $DrawingUI_{i,j} = 1$ indicates that worker *i* drew a *j*th month of UI.³² The change in the average of this variable at the cutoff captures the total effect on average paid UI duration. To separate the behavioral and mechanical effects, we must measure the average paid UI duration that would prevail absent behavioral responses to the increase in potential UI duration. We do so by displaying the average of the following *counterfactual paid UI duration* (for the lack of a better term):

$$\widetilde{D_{i}^{B}} \equiv \sum_{j=1}^{4} \mathbb{1}(DrawingUI_{i,j} = 1) + \mathbb{1}(DrawingUI_{i,4} = 1) \ \mathbb{1}(NotFormallyReemployed_{i,4,1} = 1),$$

where *NotFormallyReemployed*_{*i*,4,1} indicates whether worker *i* remains without formal employment one month after drawing her fourth month of UI. With perfect assignment and compliance, actual and counterfactual paid UI duration would be equal for workers eligible for five months of UI. The counterfactual paid UI duration would capture the sum of the paid UI duration and the mechanical effect of the increase in potential UI duration for workers eligible for four months of UI. The change in the average of this variable at the cutoff would thus capture the behavioral effect.

The average paid UI duration is around 3.9 months for workers with less than 22 months of tenure, who were all eligible for four months of UI. It then increases between 22 months and 24 months of tenure, as an increasing share of workers were eligible for a fifth month of UI. This increase would start at lower tenure levels and would be much less sharp if we had not selected workers with a single formal job spell in the previous 36 months. The average paid UI duration

 $^{3^{32}}$ As in Section 3, we consider a worker as taking up UI (resp. drawing a j^{th} month of UI) if she applied within 120 days of layoff and drew her first UI payment (resp. her j^{th} month of UI) before being formally reemployed.

reaches around 4.75 months for workers with more than 24 months of tenure, who were all eligible for five months of UI. The total effect is thus large (.85 month). However, it is mostly due to a mechanical effect. The average counterfactual paid UI duration is around 4.65 months for workers eligible for four months of UI, indicating that most of them would draw a fifth month of UI mechanically. It starts increasing at exactly 22 months of tenure and reaches 4.75 months (the average paid UI duration) above the cutoff, indicating that the behavioral effect on paid UI duration only amounts to about .10 month, and that assignment and compliance are not problematic in our case.

Finally, panels (e) and (f) provide evidence of behavioral responses for longer-term outcomes. UI takers eligible for a fifth month of UI had longer non-formal-employment durations (panel e) and spent fewer months formally employed (panel f) in the three years after layoff. The change around the cutoff is less evident because of the scale of the variable relative to the effect (it is clearer for non-formal-employment duration with a shorter horizon; see graph in Web Appendix). We consider a three-year horizon because we find no difference in the share remaining without formal employment by then (see below).

4.3 Empirical strategy

We turn now to the econometric analysis to obtain precise estimates of all those effects. Our empirical strategy is as follows. Let T_i be the normalized tenure at layoff for worker *i*, such that $T_i = 0$ at the 24-month cutoff. In the absence of noise in the eligibility of workers with 22 to 24 months of tenure, we would simply regress an outcome y_i on a constant, an indicator for tenure levels above the cutoff $\mathbb{1}(T_i \ge 0)$, and a control function in tenure $f(T_i)$:

$$y_i = \alpha + \beta \ \mathbb{1}(T_i \ge 0) + f(T_i) + \varepsilon_i, \tag{4}$$

The average treatment effect at the cutoff is captured by β under a continuity assumption for the control function at $T_i = 0$. As common in the RD literature, we approximate the control function with local linear functions over a bandwidth *h* on each side of the cutoff using a kernel function *K*. 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 1,969,137 observations). The value of the outcome at the cutoff from below is then estimated by a local linear function fitted only to observations below 22 months. This approach is comparable to other "donut hole" approaches in the RD literature. It is illustrated in panels (c)-(f) in Figure 4. The line below the cutoff (resp. above the cutoff) is estimated using observations in a bandwidth of six months below 22 months of tenure (resp. above 24 months of tenure). The estimated impact is the difference between the two lines at the cutoff.³³

³³We show in the Web Appendix that using polynomials of degrees higher than one does not provide a great fit given our "donut hole" approach. However, we show that our conclusions are robust to using quadratic functions.



(a) Survival rate in non-formal employ- (b) Hazard rate of formal reemployment (c) Share of observations by tenure at layment (UI takers) off (density of the running variable) (UI takers)



paid UI duration (UI takers)

layoff censored at 3 years (UI takers)

(d) Behavioral and mechanical effects on (e) Non-formal-employment duration after (f) Months formally employed in the 3 years after layoff (UI takers)



Workers with tenure levels below 22 months (resp. between 22 and 24 months, above 24 months) were eligible for 4 months of UI (resp. either 4 or 5 months of UI, 5 months of UI). Panels (a) and (b) display survival rates in non-formal employment and hazard rates of formal reemployment for workers with tenure levels between 16 and 22 months and between 24 and 30 months. Panels (c)-(f) displays averages by tenure levels (.1 month bins) around the cutoff. Panel (c) displays the density of the running variable. Panel (d) displays the paid UI duration (black dots) and the "counterfactual" paid UI duration (gray dots), which allows to decompose the total effect on paid UI duration into behavioral and mechanical effects. Panels (e) and (f) display the non-formal-employment duration and the number of months spent formally employed in the three years after layoff. Below the cutoff (resp. above the cutoff), the line is estimated using an edge kernel and observations in a bandwidth of six months below 22 months of tenure (resp. above 24 months of tenure).

In our main specification, we use the theoretically optimal edge kernel (Cheng, Fan and Marron, 1997) and a bandwidth of six months below 22 and above 24 months of tenure. A visual inspection of the data in Figure 4 suggests that a six-month bandwidth fits the data well. In contrast, a smaller bandwidth (e.g. less than two months) appears to miss overall trends for some outcome variables (e.g. Figure 4f; we present results with smaller bandwidths as robustness checks). This is important because we need to predict the trend in the outcomes between 22 and 24 months using only observations below 22 months. This is also why we cannot use optimal bandwidths from the RD literature. Finally, we cluster standard errors at the level of the running variable.

4.4 Main results

Table 1 displays the estimated impact of the one-month increase in potential UI duration ($\hat{\beta}$) and the estimated level at the cutoff from below ($\hat{\alpha}$) for several outcome variables.

Results in panel A support the validity of our RD design. We find no evidence of a discontinuity in the distribution or the composition (age, gender, education, replacement rate) of our sample at the cutoff. Estimates are neither statistically nor economically significant, and results are similar if we consider other worker characteristics or use smaller bandwidths (see Web Appendix). The average worker at the cutoff is 29.9 years old, is more likely to be male (58.2%), has 9.16 years of education, and is offered a replacement rate of 79.1% by the UI program.

Next, we turn to UI outcomes (panel B). Since we do not find any impact on UI takeup in column 1, we focus on UI takers in the other columns. We find a small increase in average paid UI duration up to the fourth month of UI, providing evidence of behavioral responses in anticipation of the fifth month of UI. We estimate a total effect on average paid UI duration of .861 month. Increasing potential UI duration is thus costly in Brazil. However, it is mostly due to a mechanical effect. We estimate an increase in the average counterfactual paid UI duration, which captures the behavioral effect, of only .126 month. The behavioral effect on average paid UI duration thus leads to a loss of $\frac{.126}{.861-.126} = 17.1$ cents per \$1 reaching mechanical beneficiaries.³⁴

Finally, we consider longer-term outcomes for UI takers (last column in panel B, and panel C). We estimate increases of .29 month and .389 month in non-formal-employment duration one year and three years after layoff, respectively. The three-year impact measures the total change in non-formal-employment duration because we find no impact on the share remaining without formal employment three years after layoff.³⁵ We estimate a decrease in the time spent formally employed in the three years after layoff of .243 month. Therefore, estimating impacts on non-formal-employment duration and assuming that they translate into symmetric impacts on subsequent formal employment, as often done in the UI literature, can be misleading. We show in the Web Appendix that the difference is due to the fact that, upon formal reemployment, workers spend only a share q = .746 of their remaining career in the three years after layoff formally employed (we don't find a significant impact on this share). Finally, we find no effect on the probability to be formally employed three years after layoff or on the wage among those formally employed then.³⁶

³⁴To confirm that the average counterfactual paid UI duration closely approximates the average paid UI duration for workers eligible for five months of UI, we ran a similar regression as in column (3) on the following variable: $\sum_{j=1}^{4} \mathbb{1}(DrawingUI_{ij} = 1) + \mathbb{1}(DrawingUI_{i4} = 1) \mathbb{1}(T_i \ge 0) \mathbb{1}(NotFormallyReemployed_{i1} = 1)$, which captures the paid UI duration below the cutoff and the counterfactual paid UI duration above the cutoff. We find an impact of .87 months, which is very close to the estimate in column 3.

³⁵We present impacts on hazard rates and survival rates in each month after layoff in the Web Appendix.

³⁶Moreover, we find no impact on the withdrawal of additional UI benefits in the three years after layoff (insignificant impact of .003 month for a constant of .917 month).

4.5 Efficiency cost

We now evaluate the efficiency cost of the one-month increase in potential UI duration. The only statistic that we have not yet estimated is the coefficient $\frac{D^B}{D^F}$ in equation (2), which scales down the behavioral effect on the time spent formally employed subsequently. It corresponds to the average number of periods of UI benefit collection per period of formal employment financing the UI system. Chetty (2008) and Schmieder, von Wachter and Bender (2012*a*) approximate it by the unemployment rate. This would be misleading in our context because of the large share of informal workers. We approximate it by the number of UI beneficiaries per private formal employee between 2005 and 2009 (as in Landais, 2015). We obtain an average ratio of .086, implying that there were about 11.5 private formal employees per UI beneficiary. This is consistent with descriptive statistics discussed earlier: a random sample of workers spent on average 82.4 months formally employed and drew a total of 7.7 months of UI between 2000 and 2010, for a ratio of .093. The resulting efficiency cost amounts to $\frac{.126+.086\times.243}{.861-.126} = .20$ (with standard errors of .023 obtained by the delta method) or 20 cents per \$1 reaching mechanical beneficiaries.

4.6 Comparison with estimates from contexts of low(er) informality

Our estimate of efficiency costs, which may still constitute an upper bound, is comparatively low despite the high degree of informality in Brazil. For instance, Katz and Meyer (1990) and Landais (2015) estimate a behavioral effect on average paid UI duration that is larger than the mechanical effect using data from the US. This implies an efficiency cost above \$1 per \$1 reaching mechanical beneficiaries, not even accounting for changes in the time spent formally employed subsequently. The comparison with Landais (2015) is particularly interesting. He also considers marginal changes in potential UI duration across workers within a labor market. The average potential UI duration in his sample (20-27 weeks) is comparable to the potential UI duration on the right of our cutoff (five months or 22 weeks). Yet, informal work opportunities were limited in the US in the late 1970s - early 1980s, the period covered by his sample. We estimate a larger impact on average paid UI duration following a marginal change in potential UI duration (.86 vs. .2-.4). However, the efficiency cost is larger in his case because the mechanical effect is smaller: only 11%-18% of UI beneficiaries exhaust their UI benefits. Landais (2015) concludes that one half to two-thirds of the increase in average paid UI duration is due to behavioral responses in his case.

The UI program has not changed much in the US since the early 1980s, but exhaustion rates have increased. For instance, Card and Levine (2000) document exhaustion rates around 35% for the mid-1990s in the US. Yet, findings in their paper also suggest a higher efficiency cost for the US. Their estimates imply that a one-month increase in potential UI duration leads to .077-month in average paid UI duration *up to the pre-existing potential UI duration* (as in column B2

in Table 1). This effect alone, which can only be due to behavioral responses and is likely to largely underestimate the behavioral effect on average paid UI duration up to the new potential UI duration, already implies an efficiency cost higher than ours. Assuming a mechanical effect of .35 month, they would obtain .077/.35=.22 or 22 cents per \$1 reaching mechanical beneficiaries.

Comparisons with estimates from other countries is complicated because the potential UI duration is typically much longer.³⁷ Yet, estimates for Austrian and Slovenian UI beneficiaries eligible for 20 to 30 weeks and three to six months of UI, respectively, imply an efficiency cost at least as large as ours, despite the lower degree of informality in these countries (Card, Chetty and Weber, 2007*a*; van Ours and Vodopivec, 2006, details for all comparisons are in the Web Appendix).

4.7 Robustness checks

We present robustness checks for our impacts of interest in the Web Appendix. First, we consider smaller bandwidths around the cutoff and quadratic functions in tenure. Results are quantitatively similar with two exceptions. The impacts on non-formal-employment duration and on the time spent formally employed after layoff are smaller (in absolute values). If anything, we may thus overestimate the efficiency cost in our main specification. Our results are also unchanged if we include a rich set of individual controls, if we only include workers laid off between 2005 and 2007 as in panel C in Table 1 (such that we estimate results for short- and longer-term outcomes on the same sample), if we include workers laid off since 2002, or if we exclude workers with very high and very low replacement rates. The efficiency cost resulting from those estimates ranges from 14.8 to 20.1 cents per \$1. Finally, we further confirm all our results by using the other source of quasi-experimental variation in potential UI duration in Brazil – temporary extensions of UI benefits. We exploit a policy that extended UI benefits by two months at the end of March 2009, for workers laid off in December 2008 from a list of 42 sector-state pairs. We use a difference-indifference strategy comparing workers laid off in December vs. November 2008, from eligible vs. ineligible sector-state pairs. We obtain an efficiency cost of 15.5 cents per \$1. The slightly lower estimate is likely due to the fact that workers did not learn about the policy directly upon layoff.

5 Efficiency cost and informality rates

We showed in Section 4 that the average efficiency cost of increases in potential UI duration was not necessarily high(er) in a context of high informality, compared to estimates from richer countries. In this section, we directly estimate how the efficiency cost of increases in potential UI duration

³⁷Similarly, recent estimates from the US that use variation during the Great Recession are capturing the effect of an increase in potential UI duration for workers whose potential UI duration is already much longer than 26 weeks.

compares across Brazilian labor markets with different informality rates. Our findings allow us to reinforce and shed further light on the conclusions of the cross-country comparison in Section 4.

The empirical exercise in this section compares to a literature that correlates the effects of changes in UI benefits with unemployment rates (e.g. Schmieder, von Wachter and Bender, 2012*a*). Such correlations do not imply causal relationships as unemployment or informality rates are not policy parameters that can be modified ceteris paribus. High unemployment rates may simply indicate labor markets where the cost of finding a job is high or the return from doing so is low. Our point is precisely that labor markets with high informality are not only labor markets where it is easier for displaced formal workers to respond to UI incentives. It may also be harder for them to find a formal job in these labor markets or they may choose to work informally for reasons unrelated to UI. As a result, the efficiency cost may not be higher in contexts of higher informality.

5.1 Main empirical strategy and results

Our main empirical strategy consists in estimating the same RD specifications as in Table 1 for the 27 Brazilian states separately, and then regressing our estimates on state-level informality rates. Figure 5 displays our results. State-level estimates for different outcomes are plotted against the average state-level informality rate in the different panels. We assign to each observation the informality rate prevailing in the state and year of layoff, which is measured using PNAD and is defined as the share of informal workers in the non-farm labor force, and we then average informality rates within state.³⁸ Lines display the result of a WLS regression of our RD estimates on informality rates, weighting estimates by the inverse of their standard error squared.³⁹

Panel (a) displays estimates of the impact of the one-month increase in potential UI duration on the paid UI duration. It is large in every state and slightly larger in states with higher informality, although the slope is not significant. Panels (b) and (c) then decompose this total effect into mechanical and behavioral effects. The mechanical effect is the estimated discontinuity at the cutoff for the difference between actual and counterfactual paid UI durations (see Figure 4d). It is large in every state and *increasing* in informality rates. The slope is significant and implies that the mechanical effect increases from .71 month to .79 month over our range of informality rates. In contrast, the behavioral effect, which is the impact on the counterfactial paid UI duration, is relatively small in every state and is *decreasing* in informality rates. The slope is also significant and implies a decrease of 35.9% over our range of informality rates, from .145 month to .093 month.

The mechanical effect is increasing in informality rates because workers return slower to a

³⁸We define again informal workers as self-employed workers and informal employees because 93.7% of displaced formal workers who report their first new job as informal report one of these two categories (see footnote 28).

³⁹All estimates are based on samples restricted to UI takers because we show in the Web Appendix that there is no systematic correlations between UI takeup and informality rates.

formal job in states with higher informality rates, absent increases in potential UI duration. This is shown in panel (d), which displays the average non-formal-employment duration at the cutoff from below. It is increasing in informality rates. Importantly, we show in the Web Appendix that these patterns generalize beyond our RD sample. The mechanical effect of increases in potential UI duration and the average non-formal-employment duration are increasing in informality rates for any level of potential UI duration. Moreover, this is not due to different responses to the existing potential UI duration because we find the same patterns for workers who are not eligible for UI.

The behavioral effect on average paid UI duration is decreasing in informality rates because the increase in potential UI duration leads to smaller delays in formal reemployment in states with higher informality rates. This is shown in panels (e) and (f), which display the estimated impacts on non-formal-employment duration one year and three years after layoff. They are smaller in states with higher informality, although the slope is only marginally significant for the longer horizon (it is significant at conventional levels if we include workers laid off since 2002; see Web Appendix).

To understand why the behavioral effect on average paid UI duration is decreasing in informality rates, it is useful to decompose it as follows: $dD^B|_B = dD^B_{max}|_B \times \frac{dD^B|_B}{dD^B_{max}|_B}$. The maximum behavioral effect, $dD^B_{max}|_B$, captures the "potential" (or maximum) distortion. It is the difference between the longer potential UI duration (five months) and the average paid UI duration at the cutoff from below (4.62 months in Table 1). It is necessarily limited when most UI takers would draw a fifth month of UI mechanically. The ratio, $\frac{dD^B|_B}{dD^B_{max}|_B}$, captures how large the behavioral effect is relative to this upper bound. We show in the Web Appendix that the maximum behavioral effect is decreasing in informality rates for the same reason as the mechanical effect is increasing: displaced formal workers return slower to a formal job in states with higher informality rates. In contrast, the ratio is not correlated with informality rates. Therefore, the behavioral effect decreases with informality rates because the potential distortion decreases, and not because workers who would not draw a fifth month of UI mechanically are less responsive to UI incentives. This also explains why the impact on non-formal-employment duration is decreasing in informality rates.

Next, we consider the number of months formally employed in the three years after layoff. Panel (g) displays the average at the cutoff from below. It is lower in states with higher informality. This is mostly because displaced formal employees take longer to find a new formal job (panel d). It is also partly because, upon formal reemployment, workers spend a smaller share q of their remaining career formally employed (see Web Appendix). Panel (h) displays the estimated impact of the increase in potential UI duration on the number of months formally employed after layoff. The impact is less negative in states with higher informality rates. This is because the impact on non-formal-employment duration is smaller (panel f); we find no differential impact on the share q (see Web Appendix). In sum, these results confirm that the behavioral effect is decreasing in informality rates because the potential distortion is decreasing. Finally, we combine our estimates to evaluate the efficiency cost in each state (panel i). Standard errors are obtained by the delta method. We use the average number of UI beneficiaries per private formal employee in each state between 2005 and 2009 to approximate the scaling coefficient $\frac{D^B}{D^F}$ in equation (2).⁴⁰ As expected given the above results, the efficiency cost is lower in states with higher informality rates. The slope is significant and implies that the efficiency cost decreases by over 50% over our range of informality rates, from 24.5 cents to 10 cents per \$1.

In sum, these findings reinforce that the efficiency cost of increases in UI benefits is not necessarily higher in labor markets with higher informality. Displaced formal workers return slower to a formal job absent any change in potential UI duration, which leads to a larger mechanical effect, and limits the consequences from behavioral responses, including informality responses.

5.2 Robustness checks

We show in the Web Appendix that the patterns in Figure 5 hold for the same robustness checks as in section 4. The efficiency cost is always decreasing in informality rates. Estimates become in fact more precise and patterns clearer when we increase sample sizes by including workers laid off since 2002. We also confirm our results using another empirical approach. We estimate correlations with informality rates directly by using a variant of equation (4), in which all the righthand-side variables are interacted with informality rates. This allows us to estimate correlations with informality rates holding (un)employment rates constant (also interacting all the right-handside variables with (un)employment rates). Informality and unemployment rates are correlated in middle-income countries. The above results could thus in theory come from underlying correlations with unemployment rates, which have already been studied in contexts of lower informality. This approach also allows us to use more disaggregated measures of the informality rate prevailing in a given labor market, namely state-level informality rates that vary along two important dimensions: year and gender. Labor markets became more formal over time and women remain less likely to work in the formal sector in Brazil (see Web Appendix). Finally, this approach allows us to show that our correlations are not simply due to fixed differences across states or to the fact that workers in different states have different observables: results are similar including year fixed effects, state fixed effects, and a rich set of individual controls.

6 Marginal value of insurance and welfare effect

We estimated an average efficiency cost of 20 cents per \$1 for an increase in potential UI duration in Brazil. The welfare effect would then be positive if the average marginal utility of \$1 was at

⁴⁰These ratios are slightly increasing in informality rates, but not significantly so (see Web Appendix).



Figure 5: Heterogeneity of regression discontinuity estimates with labor market informality

the cutoff from below (after 3 years)

(d) Non-formal-employment duration at (e) Impact on non-formal-employment du- (f) Impact on non-formal-employment duration at the cutoff (after 1 year) ration at the cutoff (after 3 years)



(g) Number of months formally employed (h) Impact on number of months formally in the next 3 years at the cutoff from below employed in the next 3 years at the cutoff

(i) Efficiency cost



The figure displays results from estimating specifications as in Table 1 in each Brazilian state separately. State-level estimates are plotted against average state-level informality rates. Lines display the result of a WLS regression of these estimates on informality rates, weighting estimates by the inverse of their standard error squared, with their 95% confidence intervals (dashed lines). The estimated slope is provided in each panel with its standard error in parenthesis. The efficiency cost is decreasing in informality rates because the mechanical effect on paid UI duration is increasing, the behavioral effect on paid UI duration is decreasing, and the behavioral effect on the time spent formally employed after layoff is becoming less negative.

least 20% larger for mechanical beneficiaries than for formal employees. In comparison, this bound would be at least five times larger in the US given results in Katz and Meyer (1990) and Landais (2015). Moreover, the measure of efficiency costs may still be an upper bound in our context, so the welfare effect may be positive for even lower levels of the marginal value of insurance. To provide some benchmark, Chetty (2008) argues that the average marginal utility of \$1 is 150% larger for UI beneficiaries than for formal employees in the US.⁴¹ A relatively low efficiency cost, however, does not imply that the welfare effect is positive. The "exit" and "exclusion" views on the prevalence of informal workers are consistent with a relatively low efficiency cost, but have polar implications for the marginal value of insurance. The marginal value of insurance may be high if it is relatively hard to find a new formal job and informal jobs are imperfect means of self-insurance. The marginal value of insurance may be low if it is relatively easy to find informal jobs and those are close substitutes for formal jobs. The sign of the welfare effect thus remains ambiguous.

The focus of the paper is on the efficiency side, but we provide suggestive evidence for the value of insurance in this section. We first find that mechanical beneficiaries experience lower levels of disposable income, despite the fact that many work informally. This is both because a sizeable share appears to remain non-employed, even after exhausting their UI benefits (see Figure 3c), and because the informally reemployed have lower earnings. We then explore implications by calibrating the marginal value of insurance, approximating consumption gaps in equation (3) by disposable income gaps (there is no data on consumption or savings for displaced formal employees in Brazil) and using different values for the relative risk aversion parameter, γ . Our approximation of consumption gaps may be informative because other means of consumption smoothing, besides informal employment, may be costly in developing countries (Chetty and Looney, 2006).

6.1 Disposable income gaps

We investigate disposable income gaps using PME. Ideally, we would measure disposable income for the same individuals when they are formally employed, when they draw UI benefits, and when they exhaust their UI benefits to approximate c^F , c^B , and c^P in equation (3). However, PME has no information on UI benefits and its panel is too short to follow many workers through those three states. PME has also no information about earnings prior to layoff for non-employed workers. We thus proceed as follows given the data limitations. We start with the same sample of nonemployed displaced formal workers eligible for five months of UI as in Section 3, for the same reasons discussed in that section. We then restrict attention to those who were also surveyed in the previous or the following month. As a result, we observe workers non-employed, formally employed in the month before layoff, or employed in their first month of reemployment (formal or informal). We then use the data as a repeated cross-section in the following specification:

$$DisposableIncome_{i} = \alpha + \sum_{k} \sum_{t} \beta_{kt} \ \mathbb{1}(Status_{i} = k \& Period_{i} = t) + \varepsilon_{i},$$
(5)

⁴¹There is a typo in footnote 34 in Chetty (2008): the marginal value of insurance is the ratio of the liquidity to the moral hazard effect $(\partial s_0/\partial w)$ and not to the total effect $(\partial s_0/\partial b)$; therefore, it is equal to .6/.4 = 1.5, and not to .6.

where $k = \{FormReemp, InfReemp, Nonemp\}$ indicates workers formally reemployed, informally reemployed, or non-employed, and $t = \{before, around, after\}$ workers observed before, around, and after UI exhaustion (months 0–4, 5–7, and 8–10 after layoff, respectively). Workers formally employed in the month before layoff constitute the omitted group. The estimated $\hat{\beta}_{kt}$'s for $k = \{InfReemp, Nonemp\}$ allow us to shed light on the relative disposable income of UI beneficiaries (for $t = \{before, around\}$) and UI exhaustees (for $t = \{around, after\}$).⁴² They are displayed in Table 2, in percentage of the average outcome prior to layoff (omitted group). Standard errors are clustered by individual and obtained by the delta method; regressions use sampling weights.

Outcomes in columns (1)-(2) and (3)-(4) are workers' real net earnings and disposable income, respectively. We account for taxes on formal earnings to construct net outcomes (see Web Appendix). Disposable income is defined as a family's net monthly income per capita using an equivalence scale of one half for children. The definition accounts for income smoothing across family members. Columns (2) and (4) evaluate possible concerns of selection bias with a specification such as equation (5). In particular, we include a rich set of individual controls, which captures most of the selection effect for differences in earnings and disposable income between formal employees and informal or non-employed workers in the overall population (see Web Appendix).

Estimates are relatively similar before, around, and after UI exhaustion. Displaced formal workers do not appear to accept jobs with differentially higher (for formal jobs) or lower (for informal jobs) wages when still eligible for UI. Informally reemployed workers have average earnings and disposable income levels about 45% and 32% lower than formally employed workers before layoff. Non-employed workers have average disposable income levels about 50% lower. The differences are smaller for formally reemployed workers. Our estimates are similar when we include a rich set of individual controls. They are also unchanged if we use the same controls to re-weight the sample so that it compares better to the subsample of workers formally employed before layoff or to a comparable sample of workers in the administrative data (see Web Appendix). We would expect our controls to have much more influence on our estimates if these estimates were largely driven by selection effects. The estimated disposable income gaps are in fact even larger when we include worker fixed effects (see Web Appendix). Finally, comparing averages may understate the need for insurance as 30% of the non-employed have no disposable income.⁴³

Columns (5)-(6) investigate the potential effect of UI benefits, which are not accounted for in

⁴²This interpretation assumes that their first month of reemployment remains informative for the disposable income in later months of those who are reemployed informally. Note that abstracting from the facts that UI takeup is imperfect and not observed in PME is unlikely to severely bias our results: takeup is above 80%, and 50% of UI non-takers are formally reemployed 30 days after layoff (see Section 3).

⁴³Ideally, we would also study how our estimates correlate with informality rates, but PME only covers six metropolitan areas. We display estimates for the two areas with the lowest informality rates and the other four areas separately in the Web Appendix. This is tentative, but we find no evidence that results are systematically different. Note that results in Table 2 are also comparable if we use a sample of workers laid off since 2003 (see Web Appendix).

columns (3)-(4), on our estimates. Specifically, we add the average UI benefit level from a comparable sample of workers in the administrative data when constructing the disposable income of the non-employed and informally reemployed workers. In so doing, we close the disposable income gap for informally reemployed workers and reduce it to 11%-15% for non-employed workers.

6.2 Marginal value of insurance

Results in columns (3)-(4) (resp. columns 5-6) suggest that the marginal value of insurance may be sizable (resp. small) for increases in the potential UI duration (resp. in the UI benefit level), for which mechanical beneficiaries are not already receiving UI benefits (resp. are already receiving UI benefits), even for those working informally (resp. particularly for those working informally). To clarify these points, we calibrate the marginal value of insurance as follows:

$$\gamma \frac{c^{F} - c^{x}}{c^{F}} \simeq \gamma \left[share_{non}^{x} \frac{DispInc^{F} - DispInc_{non}^{x}}{DispInc^{F}} + (1 - share_{non}^{x}) \frac{DispInc^{F} - DispInc_{inf}^{x}}{DispInc^{F}} \right]$$

with $x = \{b, P\}$, and where *non* and *inf* stand for *non-employed* and *working informally*, respectively. We use the survival rates in non-employment and non-formal employment in Figure 3 to compute the share of non-employed UI beneficiaries and UI exhaustees: $share_{non}^{b} = \sum_{t=1}^{5} S_{t,non} / \sum_{t=1}^{5} S_t = .643$ and $share_{non}^{P} = \sum_{t=6}^{8} S_{t,non} / \sum_{t=6}^{8} S_t = .378$, where S_t and $S_{t,non}$ are the survival rates in non-formal employment and in non-employment, respectively. We use results in columns (3) and (5) of Table 2 for the disposable income gaps of non-employed and informally employed UI exhaustees (*around*) and UI beneficiaries (*be fore*), respectively.⁴⁴

We obtain a marginal value of insurance of $.07\gamma$ and $.38\gamma$ for increases in the UI benefit level and in the potential UI duration, respectively. The welfare effect of an increase in potential UI duration may therefore be positive for relatively small values of γ , given our estimate of the efficiency cost ($\gamma \ge .53$).⁴⁵ In fact, the welfare effect would still be positive for $\gamma = 1.58$ if disposable income gaps overestimated consumption gaps by a factor of three. Chetty and Szeidl (2007) argue that a moderate drop in consumption may imply a high marginal value of insurance because of a high coefficient of local risk aversion for unemployment shocks due to consumption commitments.

⁴⁴Our choice may underestimate disposable income gaps as earnings have already often decreased in the months just prior to layoff and some workers informally reemployed may quickly lose their informal job. It may overestimate these gaps if some workers report being formally reemployed but are kept on an informal payroll temporarily.

⁴⁵This lower bound would become $\gamma >= 1.05$ with the extreme assumption that the marginal value of insurance was nil for UI exhaustees working informally. The welfare effect of an increase in the UI benefit level could be positive if γ was large of if the efficiency cost was very low, which could be consistent with the absence of clear behavioral responses around the kinks in the UI benefit schedule (see Web Appendix).

7 Conclusion

This paper studied the efficiency cost of increases in UI benefits in a context of high informality, by combining an optimal UI framework and empirical evidence for Brazilian UI beneficiaries. Its findings run counter to widespread claims in policy circles that heightened concerns about the usual moral hazard problem – that UI distorts incentives to return to a formal job – preclude the existence or expansion of UI in this context. We argued that the associated efficiency cost was not necessarily higher. We then found that it was rather low in Brazil compared to countries with low informality, and that is was lower in labor markets with higher informality within Brazil. These results are based on a benchmark measure of efficiency that constitutes a natural starting point. We discussed other mechanisms that could affect the size of the efficiency costs; more research is needed to evaluate their empirical relevance for countries with high informality.

Our results suggest shifting the focus from efficiency concerns, as in the current policy debate, to workers' need for insurance in a context of high informality. Our evidence in that respect remains suggestive. The fact that displaced formal workers return slowly to a formal job irrespective of (increases in) UI benefits, which drives our efficiency results, implies that UI is relatively costly. For instance, a flat payroll tax that would fund the UI system would have to be about three times higher in Brazil than in the US (2.5% vs. 0.8%). Moreover, the same fact implies that formal employment may not be the *normal* state of the world for many formal workers in this context. Their demand for UI may then be relatively low given the cost, especially in the first months after hiring. Yet, other means of consumption smoothing, besides informal employment, are often more costly in developing countries. There is also evidence that Brazilian workers are willing to trade off lower formal wages for mandated benefits, including benefits related to job loss (Almeida and Carneiro, 2012). How workers' need for insurance compares thus remains an open question.

Our findings have implications for other policies that aim at helping displaced formal workers in developing countries. "UI Savings Accounts" are sometimes presented as an alternative to UI in these countries because of the heightened efficiency concerns. We show that those concerns may not be founded, at least for modest potential UI durations. Yet, given that the average paid UI duration is close to the potential UI duration, a program giving UI takers the net present value of the average UI benefits per UI spell at takeup may be attractive. It would provide most workers with a comparable degree of insurance but would eliminate the (limited) moral hazard problem.⁴⁶

The findings of this paper have also broader implications for our understanding of social policies in developing countries. First, many social programs generate incentives for people to carry out their economic activities informally in these countries. For the same reasons as for UI, they are viewed as imposing high efficiency costs. Our results cast doubt on whether these concerns are

⁴⁶We thank Johannes Schmieder for this observation.

necessarily founded in these cases too. Consider for instance a welfare program for households with formal income levels below a threshold. The moral hazard problem may be more severe when households can become eligible not only by reducing their labor supply but also by working informally. Yet, many households likely have low formal income levels absent program incentives (i.e. a large mechanical effect). Whether the efficiency cost will be high is thus an empirical question. Second, our results indicate that efficiency concerns may sometimes become more relevant when a country's economy formalizes. The potential distortion of social policies will likely increase (i.e. a larger share of workers will be formally employed absent policies' incentives), and unless agents' ability to respond to policies' incentives decreases (e.g. it becomes harder to work informally), their efficiency costs will increase as well. This appears to be the case for UI in Brazil: the potential distortion is larger in labor markets with lower informality, but there is no evidence that agents' ability to respond to UI incentives is lower. Of course, agents' ability to respond to UI incentives may start decreasing at lower levels of informality than those observed across Brazilian states.

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A. VALIDITY CHEC	KS (VARIABLES AT	LAYOFF)			
	Share of observations	Age	Male	Years of	Replacement
	by tenure level	()	(dummy)	education	rate
E	(1)		(0)		
lenure∠24 months	-0003 (.0019)	.0816 .0769)	.0001 (.0072)	0301 (.0236)	003/ (.0046)
Constant	0073***)00 03***	5810***	0 163***	7908***
	(.0017)	(.0617)	(.0065)	(.02)	(.0042)
Observations	1969137	1969137	1969137	1969137	1969137
B. IMPACTS OF LOI	NGER UI BENEFITS	(FIRST YEAR AFTER	LAYOFF)		
		Paid UI d	luration	Counterfactual	Duration without
	UI takeup	Anticipated	Total	paid UI	a formal job
	(dimmer)	(P=4)	$(P = \{4, 5\})$	duration	(censored at 1 year)
	(duiiiii) (1)	(111 111011115) (2)	(111 1110) (3)	(III III0IIIIS) (4)	(111 111011115)
Tenure>24 months	900	0426***	8605***	1259***	2004***
	(.0037)	(.0055)	(.0084)	(.0093)	(.0664)
Constant	$.8626^{***}$	3.821^{***}	3.877^{***}	4.624***	9.087***
	(.0032)	(.0047)	(.007)	(800.)	(.0585)
Observations	1969137	1704333	1704333	1704333	1704333
C. IMPACTS OF LO	NGER UI BENEFITS	(THREE YEARS AFTE	(R LAYOFF)		
	Not yet formally	Duration without	Months formally	Decembe	er 3 years later
	reemployed 3 years	a formal job	employed in	Formally	Real monthly wage
	after layoff	(censored at $\tilde{3}$ years)	3 years after layoff	employed	if formally employed
	(dummy)	(in months) (7)	(in months) (3)	(dummy)	(R, in logs)
Ē	(1)	(7)	(C) **0000	(+)	(C) 0100
lenure 24 months	0004 (.0054)	.3894** (.1813)	2432** (.0976)	(0036)	0018
Constant	.2351***	16.87^{***}	14.89^{***}	.4969***	6.804^{***}
	(.0048)	(.16)	(.0814)	(.0032)	(.0064)
Observations	917344	917344	917344	917344	448800
Standard errors clustered	l by tenure level in pare	ntheses. Significance level	ls: * 10%, ** 5%, ***1	%. The table disp	lays RD estimates for the
one-month increase in pc cutoff) on each side of th	otential UI duration at the se cutoff and use an edge	24-month tenure cutoff.	All specifications includes supportive evidence for	e linear controls in the validity of the	RD design by testing for
a discontinuity in the de-	nsity of the running varia	able (column 1; the outcor	me is the share of the sa	ample by .1-month	tenure bin) or in average
worker chararacteristics :	t the cutoff. Panels B and	1 C present treatment effec	ts in the short and longe	r-term. The sample	in Panel C is restricted to
workers laid off between	2005 and 2007, such tha	t we can follow the sample	e for three years after lay	/off. Columns (4) a	and (5) consider outcomes
in December because we	only measure monthly v	vages precisely in Decemb	oer in RAIS. In all cases	, the constant captu	tres the estimated average
outcome at the cutoff from	m below.				

	Earning vs. prior (1)		Disposable vs. priot (3)	income $(\Delta\%)$ to layoff (4)	Disposable in vs. pric (5)	come + UI $(\Delta\%)$ or to layoff (6)
Formally reemployed before UI exhaustion	1599***	1733***	0848**	0904**	0848**	0903**
Formally reemployed around UI exhaustion	(.0400) 234***	(.0200) 2583***	(.0429) 114**	(2000.) 135***	114**	(.000.) 1341***
Formally reemployed after UI exhaustion	(.0488)269***	(.0418) 2909***	(.0512) 0962	(.0457) 1128*	(.0512) 0962	(.0456) 113*
Informally reemployed before UI exhaustion	(.057)4814**	(.0539) 4617***	(.0667) 33***	(.0637) 2947***	(.0667) .0477	(.0636). $0859**$
Informally reemployed around UI exhaustion	(.0276) 4293***	(.0248) 3833***	(.0348) 3092***	(.0305) 25***	(.043) .0653	$(.038)$. 126^{***}
Informally reemployed after UI exhaustion	(.0453) 5071***	(.038) 4963***	(.0419) 3283***	(.036)2902***	(.0499). 0141	(.0437) .0511
Non-employed before UI exhaustion ^a	(.0347)	(.0339)	(.0451) 5092***	(.0403) 5273***	(.0535) 1283***	(.048) 147**
Non-employed around UI exhaustion b			(.0191) 4958***	(.016)5241***	(.0288) 1105***	(.024)1412***
Non-employed after UI exhaustion c			(.0229) 4943*** (.0241)	(.0195) 5273*** (.0216)	(.0322) 1106*** (.033)	(.0269) 1473*** (.0283)
Observations Including controls	5,059 No	5,059 Yes	34,608 No	34,608 Yes	34,608 No	34,608 Yes
Standard errors clustered by individual and obtained	d by the delta	method in pa	rentheses. Sig	nificance levels:	: * 10%, ** 5%,	***1%. The ta

outcome in columns (5)-(6) is constructed by adding the average UI benefit level for a comparable sample of displaced formal workers in the

administrative data when constructing the disposable income of displaced formal workers who are reemployed informally or remain non-employed.

before, around, or after UI exhaustion. We express our estimates in percentage of the average levels for formal employees prior to layoff. The

Controls in columns (2), (4), and (6) include dummies for separation years, calendar separation months, metropolitan areas, gender, education levels (9), and sector of activity in the current (for formal employees prior to layoff) or last formal job (otherwise; 24 sectors), as well as second-order polynomials in age and tenure in the current (for formal employees prior to layoff) or last formal job (otherwise). Estimations are performed using

sampling weights. a 31% have no disposable income. b 29% have no disposable income. c 28% have no disposable income.

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