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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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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, partly due to data constraints, few papers credibly estimate the impact of social programs on employment choices in developing countries.[2] 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.[3]

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 a high-informality context. We then obtain new evidence on the size of the relevant effects using 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, as our findings suggest shifting the policy debate from efficiency concerns to workers' actual need for insurance, we end by shedding some light on the insurance value of increases in UI benefits, the other side of the usual efficiency-insurance tradeoff.

UI is an ideal program to study these issues. It requires the beneficiaries – displaced formal

[1] We 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.

[2] The list has been growing in recent years (e.g., Azuara and Marinescu, 2013; Banerjee et al., forthcoming; Bergolo and Cruces, 2014, 2016; Bosch and Campos-Vasquez, 2014; Camacho, Conover and Hoyos, 2014; Garganta and Gasparini, 2015; Gasparini, Haimovich and Olivieri, 2009; Imbert and Papp, 2015).

[3] The above-cited papers investigate the impact of different social programs on employment outcomes, but none of them attempts to use a theoretical framework to interpret their findings in terms of the efficiency cost of the policy. Bergolo and Cruces (2016) is a recent exception, but it is unclear whether they successfully do so. Bergolo and Cruces (2014) estimates behavioral and mechanical effects of a policy, but do not study its efficiency cost.

employees – to not be formally reemployed. It has been adopted or considered in a number of middle-income and developing countries.[4] Moreover, international development agencies have emphatically pointed to the heightened moral hazard problem it supposedly creates in the presence of a large informal sector.[5] Finally, in contrast to other programs prevalent in middle-income and developing countries (e.g., Conditional Cash Transfers), comparable UI programs exist and have been studied in developed countries, which allows us to benchmark our results against estimates from contexts of low informality.[6] 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 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 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). Yet, our measure of efficiency costs is an upper bound 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

[4]Some 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).

[5]*"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.

[6]For instance, the CCT program studied in Bergolo and Cruces (2016), and the elasticity they estimate, does not directly compare with existing programs and estimates from developed countries.

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).[7] The mechanical effect will be larger in that case. The potential distortion or "maximum behavioral effect" will also be smaller: there is less room to delay formal reemployment when workers are already returning slower to a formal job. As a result, the actual 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 explore the role of informal work opportunities in explaining our results and to discuss the value of insurance.

In a first step, we combine administrative and survey data to document key empirical patterns that motivate our analysis and drive our subsequent results. First, the average paid UI duration is high compared to the potential UI duration because most UI takers exhaust their benefits, e.g., more than 80% of those eligible for five months of UI do so. This figure is only about 35% in the US where UI takers are typically eligible for 24 weeks of UI. The difference comes from the fact that the share of workers who finds *a new formal job* each month is very low when they are eligible for UI in Brazil. This share is also lower than the share who finds *any new job*, so some UI beneficiaries likely work informally. Second, the share who finds a new formal job increases and peaks just after UI benefit exhaustion, which suggests clear behavioral responses to UI incentives. The pattern does not appear to be driven by workers leaving non-employment, however. The share who finds any new job does not increase around benefit exhaustion, and in fact the share who finds a new formal job starts exceeding it exactly after most UI takers exhaust their UI benefits. This highlights the importance of informality responses. Third, despite the existence of behavioral responses, the efficiency cost of increases in UI benefits may not be relatively high. This is because displaced formal employees return slowly to a formal job even when they are no longer or not eligible for UI. Consequently, the mechanical effect of increases in UI benefits would be large and the maximum behavioral effect would be limited. Moreover, the average paid UI duration may thus be high, and displaced workers may choose to work informally, for reasons mostly unrelated to UI incentives. Finally, and importantly, we show that the differences between Brazil and typical developed countries hold when we compare labor markets with different degrees of informality within Brazil. The average paid UI duration is higher in labor markets with higher informality, and displaced formal workers return slower to a formal job even when they are no longer or not eligible for UI. As a result, the mechanical effect of increases in UI benefits is larger, the maximum

[7] In 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).

behavioral effect is smaller, and the efficiency cost may not be higher in those labor markets.

In a second step, we estimate the impacts and efficiency cost of an increase in one dimension of UI benefits, the 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 documented in the first step 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.[8] All together, our results imply an upper bound for the efficiency cost of \$.2 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 much less prevalent.

In a third step, we replicate the second step in each of the 27 Brazilian states, separately, and we find that the efficiency cost is *lower* in states with higher informality. As documented in the first step, the mechanical effect is larger. 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). We show that this smaller behavioral effect is due to the smaller maximum behavioral effect: workers who would have returned relatively rapidly to a formal job in absence of the increase in UI benefits appear equally responsive to the new incentives over our wide range of informality rates. Furthermore, our results hold if we only exploit variation in informality rates across states over time and if we control for a rich set of worker characteristics. In sum, our findings indicate that efficiency concerns may become more relevant as an economy formalizes.

In a last step, we turn to the marginal value of insurance given that our findings suggest shifting the policy debate to workers actual need for insurance. In that respect, an efficiency cost of \$.2 per \$1 implies that the welfare effect of an increase in potential UI duration would be positive if the average marginal utility of \$1 were at least 20% larger for mechanical beneficiaries than for formal employees. Yet, the "exit" and "exclusion" views on informality, which are both consistent with a relatively low efficiency cost, have in fact polar implications for the marginal value of insurance. The marginal value of insurance may be low (resp. high) if it is relatively easy (resp. hard) to find informal jobs and those are close substitutes for formal jobs (resp. poor means of self-insurance). There is no publicly available data to evaluate the marginal value of insurance directly in our context, for instance through a "consumption approach" (Gruber, 1997*a*).[9] Nevertheless, we use

[8] The 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.

[9] The approach in Chetty (2008) does not apply, at least in our context, because it requires estimated changes in rates of formal reemployment following changes in incentives or liquidity to correspond to partial derivatives of search

survey data to explore the extent to which informal work opportunities allow workers to mitigate disposable income drops following layoff. This is an imperfect but important first step in order to evaluate their need for insurance. We find that 30% of displaced formal workers eligible for five months of UI remain without any job six months after layoff, which is comparable to US figures (Chetty, 2008). Average earnings levels are also much lower than before layoff for those reemployed informally. As a result, displaced formal workers would experience large drops in disposable income without UI. However, the existing benefit level appears sufficient to eliminate almost any drop in disposable income while drawing UI benefits. These findings imply that welfare effects are likely very small for increases in the benefit level, without the need to estimate the associated efficiency costs. In contrast, the marginal value of insurance, and thus welfare effects, may be sizable for increases in the potential UI duration, particularly if other means of consumption smoothing are more costly in developing countries (Chetty and Looney, 2006).

This paper contributes to the literature on optimal social insurance, which focuses on the context of richer countries (Chetty and Finkelstein, 2013). Schmieder, von Wachter and Bender (2012*a*), for instance, 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 in 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, which has so far focused on tax policies.[10] 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. For instance, UI Savings Accounts are sometimes presented as an alternative to UI in developing countries because of the heightened efficiency concerns.[11] We show that, once brought to the test of the data, those concerns may not be founded, at least for modest potential UI durations. 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 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 poli-

efforts. This is unlikely to be the case in practice as changes in incentives or liquidity would modify several choice variables determining observed rates of formal reemployment (e.g., search efforts for formal and informal jobs).

[10]See, for instance, Bachas and Soto (2016), Best et al. (2015), Carillo, Pomeranz and Singhal (2017), Gadenne (2014), Jensen (2016) or Naritomi (2015).

[11]For instance, the program implemented in Jordan with the support of the World Bank in recent years is a forced savings scheme to which workers contribute when formally employed. "UI benefits" drawn by a worker in excess of what she contributed over her lifetime must be paid back at retirement (http://www.social–protection.org).

cies in specific equilibrium-search models (e.g., Meghir, Narita and Robin, 2015). A feature of our approach is that the set of sufficient statistics that capture our tradeoff of interest is not specific to 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, such as the correlation between formal reemployment rates and informality rates, could help identifying new models in this literature in the future. Second, this paper also complements another strand in the literature that uses quasi-experiments. Existing studies do not typically link their results to public economics theoretical frameworks, complicating interpretation.[12] Last but not least, 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 key empirical patterns that motivate our analysis and drive subsequent results. 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 sheds some first light 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 canonical Baily-Chetty framework (Baily, 1978; Chetty, 2006, 2008). This extension allows us to highlight the 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 on the intuition for our results, and leave the presentation of a specific model for the Web Appendix. For completeness, we consider both the cases of increases in the UI benefit level and of increases in the potential UI duration in this theoretical section. However, the policy variation that we exploit in our empirical analysis comes from an increase in the potential UI duration.

[12] In addition to previously cited papers, we are aware of three relevant UI papers developed in parallel to our work. González-Rozada, Ronconi and Ruffo (2011) and Amarante, Arim and Dean (2013) estimate the impact of UI on some labor market outcomes in Latin American countries but without studying its efficiency cost. Gonzalez-Rozada and Ruffo (2016) study the impact of UI in Argentina and its efficiency cost using an optimal UI framework. However, they do not compare the correct measure of efficiency cost in their context and in richer countries, or across labor markets, and thus do not evaluate the main question in this paper, which is whether the efficiency cost is actually larger in contexts of higher informality. We provide more information in the Web Appendix. We also discuss limitations of earlier working papers on UI in Brazil in the Web Appendix (Cunningham, 2000; Margolis, 2008; Hijzen, 2011).

1.1 Background

Labor markets in many middle-income and developing countries, including 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, 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 hire both formal and informal employees (e.g., Ulyssea, 2017).[13] 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 (e.g., Bosch and Maloney, 2010). This contradicts early versions of the exclusion view, which considered formal and informal sectors as segmented (e.g., Fields, 1975). However, it is fully consistent with formal jobs being more difficult to find (e.g., 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 generally 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 search not only for a new formal job, but also for an informal job, and she can keep searching for a formal job when working informally. Relatedly, upon finding a job that would otherwise be formal, she can ask her employer to "hide" her on an informal payroll. She only loses eligibility for UI when formally reemployed, so the moral

[13] The 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.

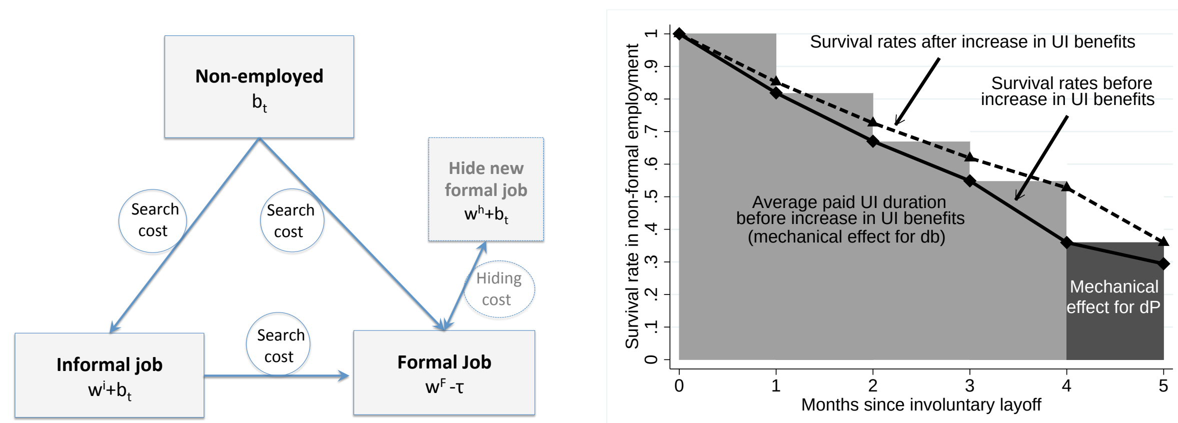
hazard problem with increases in UI benefits now includes her increased incentives to remain non-employed and to work informally. In both cases, she might be willing to trade off utility gains from remaining eligible for UI against utility losses from informality costs, such as lower wages or hiding costs. 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, and workers internalize all consequences of their choices except on the UI budget. We come back to these assumptions below.

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 (e.g., savings, 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 duration, $b \times D^B$ 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 $\tau \times D^F$ the average UI revenue.

Figure 1: Illustrations for our conceptual framework

(a) Choice situation in a context of high informality

(b) Mechanical and behavioral effects



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 ($b_t = b \times \mathbb{1}(t < P)$). 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 mechanically: 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 UI 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.[14] 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 thus increases UI costs by $\frac{d(b\times D^B)}{db}|_M = D^B$ and $\frac{d(b\times D^B)}{dP}|_M = b\times S_P$. These costs constitute a transfer from taxpayers to mechanical beneficiaries. The resulting welfare effect is: $(u'^b - u'^F)\times D^B$ and $(u'^P - u'^F)\times b\times S_P$, 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.

Second, there is a *behavioral effect*. Workers may change their behaviors in response to changes in UI benefits. Behavioral responses do not generate first-order gains in workers' utility (standard envelope argument), but some of these 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 $b\times\frac{dD^B}{db}|_B = b\times\sum_{t=0}^{P-1}\frac{dS_t}{db}$ or $b\times\frac{dD^B}{dP}|_B = b\times\sum_{t=0}^{P}\frac{dS_t}{dP}$, depending on the policy. The average time spent formally employed subsequently may also decrease, reducing UI revenues by $\tau\times\frac{dD^F}{dx}$ for $x\in\{b,P\}$. This behavioral effect must be paid for, so the resulting welfare effect is: $-u'^F\times\left[b\times\frac{dD^B}{dx}|_B - \tau\times\frac{dD^F}{dx}\right]$ 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):

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

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

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

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

[14] We 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.

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 these statistics and evaluate the efficiency cost for increases in potential UI duration in Sections 4 and 5. Note that the first component of the efficiency cost is typically much larger than the second one because the tax τ necessary to fund the UI program is much smaller than the benefit level b. This is why we spend more time discussing impacts on the paid UI duration in the empirical analysis. Note also that one assumption that is often made in the UI literature, but which we do not follow, is that new jobs are assumed to never be lost, or $D^F = T - D^{NF}$. One can then estimate impacts on non-formal-employment duration instead of impacts on the time spent formally employed subsequently, as $|\frac{dD^F}{dx}| = |\frac{dD^{NF}}{dx}|$. This assumption 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 then have: $|\frac{dD^{NF}}{dx}| \geq |\frac{dD^F}{dx}| = q\,|\frac{dD^{NF}}{dx}|$. Moreover, increases in UI benefits may allow workers to look for better and more stable jobs, so we could also have: $\frac{dq}{dx}(T - D^{NF}) \geq 0$.[15]

Suppose that we estimate an efficiency cost of \$.5 per \$1 reaching mechanical beneficiaries. The welfare effect would then be positive if the average marginal utility of \$1 were at least 50% higher for mechanical beneficiaries than for formal workers. Such a bound could be informative in some settings. Yet, one must also evaluate the marginal value of insurance to pin down the welfare effect. Therefore, although the focus of this paper is on the efficiency cost of increases in UI benefits, we also provide some evidence to shed light on the marginal value of insurance in Section 6. This is challenging because marginal utilities are not easily measured. One approach, referred to as the "consumption approach", is to use the following approximation (Gruber, 1997*a*):[16]

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

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

[15] A comparable point is made in Schmieder, von Wachter and Bender (2012*b*).

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

of relative risk aversion (γ). The applicability of this decomposition is often limited, however, by the lack of suitable consumption data and by the fact that the relevant value of γ is typically unknown.[17] For instance, there is no publicly available data in Brazil to measure consumption differences between formal employees and mechanical beneficiaries as in e.g., Kolsrud et al. (2015). Nevertheless, available survey data allow us to measure differences in income levels. We can thus assess the extent to which informal work opportunities allow displaced formal workers to mitigate income drops after layoff. This is an important first step to evaluate their need for insurance.

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.[18]

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.[19] 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 in order to estimate their efficiency cost. Both types of responses affect the tax base (resp. the UI benefit and tax bases), and thus the government budget, similarly. Moreover, both types of responses involve private utility costs, such as lower income or evasion costs (resp. lower wages or hiding costs); they thus generate no first-order utility gains and constitute real social costs.

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

[17] Chetty (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. However, the applicability of this decomposition is limited, at least in our context. Indeed, it requires the estimated changes in rates of formal reemployment to correspond to partial derivatives of search efforts. This is unlikely in practice as changes in incentives or liquidity will modify several choice variables determining observed rates of formal reemployment (e.g., search efforts for formal and informal jobs). This point is actually clearly made in Chetty (2008).

[18] Simulations in Chetty (2008) suggest that this assumption holds in a comparable framework. It holds, for instance, if the marginal value of insurance (resp. the efficiency cost) is decreasing (resp. increasing) in the policy parameter.

[19] In 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 assumes that new formal jobs are never lost ($D^F = T - D^{NF}$). More recently, Schmieder and von Wachter (2017) made the same point about the generality of our formulas in equations (1) and (2).

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, or informal jobs may be attractive for reasons unrelated to UI. The mechanical effect will be larger in both cases, as survival rates in non-formal employment will be higher to begin with. Moreover, the behavioral effect may not be larger because the "potential" distortion will be smaller. To understand this, let's consider the behavioral effect on the average paid UI duration and 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 distortion. It is the difference between the average paid UI duration that would prevail in absence of behavioral responses to the increase in UI benefits and the potential UI duration, which is an upper bound for the average paid UI duration. In Figure 1, it corresponds to the area until the potential UI duration and above the survival rates prevailing before the increase in UI benefits (solid line). It is necessarily limited when displaced formal workers return slower to a formal job. The ratio, $\frac{dD^B|_B}{dD^B_{max}|_B}$, captures how large the actual behavioral effect is relative to this upper bound. The concern that informal work opportunities may exacerbate the efficiency cost must then rest on the presumption that this ratio is much higher in a context of high informality. However, there is no clear prediction as to how this ratio varies with informality (formally, comparative statics depend on the unknown sign of third derivatives of search cost functions).[20] Intuitively, employment decisions may not necessarily be very responsive if it is costly to find formal jobs or if workers are working informally for reasons unrelated to UI. Therefore, how the efficiency cost compares in contexts of high vs. low informality is ultimately an empirical question.

Second, the marginal value of insurance may also differ in labor markets with high informality. The two main views on informality, which are consistent with a relatively low efficiency cost, have in fact polar implications for the marginal value of insurance. The marginal value of insurance may be low (resp. high) if it is relatively easy (resp. hard) to find informal jobs and those are close substitutes for formal jobs (resp. imperfect means of self-insurance). Moreover, the marginal value of insurance may also be higher if other standard means of consumption smoothing are more costly in developing countries, such as formal credit or savings (Chetty and Looney, 2006). Therefore, how the efficiency cost varies with the degree of informality is also an empirical question.

Third, 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)

[20] Schmieder, von Wachter and Bender (2012*a*) make a comparable point about the size of the behavioral effect over the business cycle.

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. Best et al. (2015) argue in fact 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. Importantly, this is not in conflict with the large literature highlighting the potential negative effects of informality responses to government policies (Levy, 2008). This literature argues that workers choosing to work informally instead of formally may impose large efficiency losses. Our measure of efficiency is very much in this spirit; it goes all the way to assuming that the efficiency losses are as large as if workers had chosen to remain idle instead of engaging in some economic activity.[21]

1.5 Informality and the assumptions of our conceptual framework

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

First, 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.[22] However, existing institutions, e.g., high firing costs, appear to prevent such responses for workers with higher tenure.

Second, 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.[23] We verify that results are not driven by workers earning close to the minimum wage in our empirical application.

[21]Other arguments that workers choosing to work informally generate negative externalities through, e.g., reduced economic growth, would likely apply similarly – or even more severely – if these workers remained idle instead.

[22]This has been confirmed in a recent paper by Carvalho, Corbi and Narita (2017).

[23]See 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.

Third, 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,[24] 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 general equilibrium if workers value it above the tax that they pay for it (Acemoglu and Shimer, 1999; Meghir, Narita and Robin, 2015). The challenge with estimating labor-market wide effects of changes in UI benefits, however, is that it requires exogenous variation in UI benefits across labor markets. For instance, estimates based on variation across workers within a labor market overestimate general-equilibrium estimates in the presence of search frictions (Landais, Michaillat and Saez, forthcoming), which may be important if informality is partly explained by the exclusion view. Finally, 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 were proportional to wages (Nekoei and Weber, 2017). However, we find no evidence of such an impact in our application.

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. Relaxing some of these assumptions, as it was done for the context of more developed countries in a series of papers in recent years, will be an important next step for future research.

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 in Brazil. 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 (23.8 weeks on average; excluding years with extended benefits),[25] which allows us to benchmark our results against US estimates.

[24] Most 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.

[25] Own calculations using data from the US Department of Labor (www.dol.gov).

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; see data section below). 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,[26] and although firms face financial penalties for not complying with labor laws, including hiring workers informally, the risk of detection is low (Almeida and Carneiro, 2012). As a result, informality is high even in non-farm employment.[27] 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 directly investigate how the efficiency cost varies with the degree of informality across labor markets.[28]

Finally, we have access to comprehensive administrative data, which, combined with quasi-experimental variation in UI benefits, allow us to estimate all the sufficient statistics entering in the measure of efficiency cost. This 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

[26] Payroll 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 13$^{\text{th}}$ monthly wage, 30 days of paid leave per year, an overtime rate of 50% for hours exceeding 44 hours a week, etc.

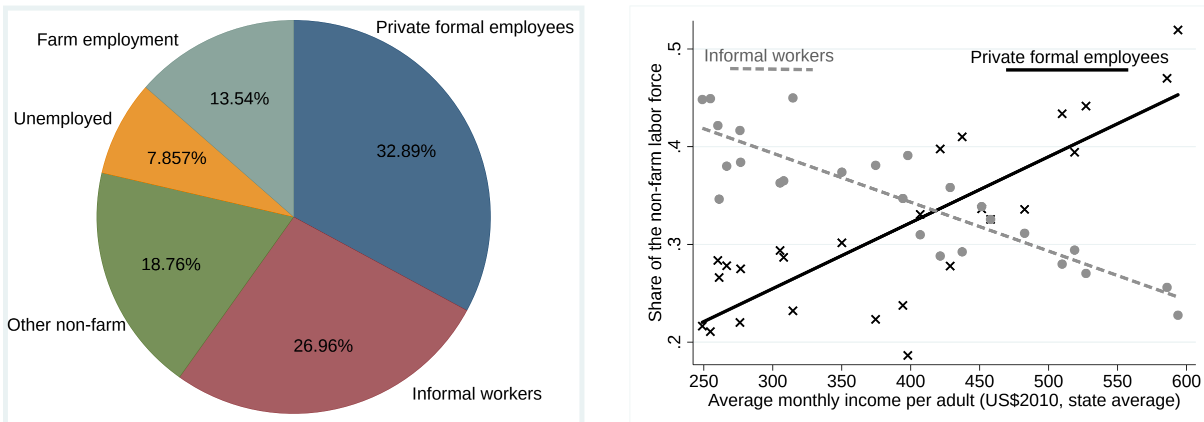
[27] We do not consider farm workers in the paper because a negligible share of them is formal and draw UI.

[28] We consider the heterogeneity across Brazilian states, and 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)

(a) Average composition of the labor force

(b) Share of private formal employees and informal workers by state (out of the non-farm labor force)



Source: PNAD 2005-2009 (the survey was not conducted in 2010). 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 18 to 54 years old.

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 markedly different substituting such a tax for the existing one.[29]

Finally, as in many other countries with UI, displaced formal employees are entitled to some layoff benefits. In particular, employers must provide 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).

[29] With 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.

Formal employees are only allowed to withdraw from this account upon layoff or retirement.

2.3 Data

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

Our analysis relies primarily 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.[30] 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 after layoff for displaced formal employees (from now on we omit the term "private"), as well as to estimate the impact of changes in UI benefits on these variables. Our main analysis uses data from 2005 to 2010, such that we have exact separation, hiring, and payment dates,[31] but 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 use yearly household surveys (PNAD), which are representative at the state level, to measure informality rates in the 27 Brazilian states as in Figure 2. We also exploit monthly labor force surveys (PME), which are only representative for the six largest metropolitan areas of Brazil, but have a useful 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 explore their need for insurance.

[30] Compliance is high because of large penalties when the data are late or incomplete. The main purpose of RAIS is to administer a federal wage supplement 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.

[31] The RAIS dataset does not include separation dates in more recent years.

3 Key empirical patterns

Before estimating the impacts and efficiency cost of an increase in UI benefits, we begin our empirical analysis by documenting key empirical patterns for displaced formal workers in Brazil, which motivate our analysis and drive our results in the next sections. Using the administrative data, we document patterns of UI benefit collection and formal reemployment, which differ from those observed in typical developed countries. We also document how these patterns compare across labor markets with different informality rates within Brazil. A limitation of the administrative data, however, is that we have no information on the labor status of a displaced formal worker who is not yet formally reemployed. We then complement the administrative data by using PME to estimate patterns of overall reemployment (formal or informal). This allows us to shed light on the importance of informal work opportunities by contrasting patterns of formal reemployment and overall reemployment. We present four 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

Using the administrative data, we construct survival rates in non-formal employment, hazard rates of formal reemployment, and the shares finding a new formal job, taking up UI, drawing UI, and exhausting UI in each month after layoff. We construct the survival rate in non-formal employment at the start of each month from the layoff and hiring dates. The share finding a new formal job in each month is the difference between survival rates at the start of that month and at the start of the following 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 from the layoff, hiring, UI application, and UI payment dates. 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.

Using the panel structure of PME, we estimate displaced formal workers' hazard rate of overall reemployment in each month after layoff by maximum likelihood. 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 because non-employed individuals are asked about characteristics of their previous job (working card signed, reason for separation, tenure at layoff). We estimate a piece-wise constant hazard function using information about the length of their non-employment spell (in months) and whether they are still recorded as 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.[32]

Note that we cannot directly estimate hazard rates of formal reemployment or the share working informally in each month after layoff in PME. This is because informal workers are not asked about previous jobs or non-employment spells and because the panel is too short. For instance, if we knew which workers remained without a formal job t months after layoff, we would be able to estimate the hazard rate of formal reemployment in month t after layoff from two consecutive interviews. However, although we have that information for those who remained non-employed, we don't have it for those working informally – henceforth, we also cannot estimate the share working informally t months after layoff. This wouldn't be a problem if we could follow workers every month for an extended period after layoff. However, we can follow a worker who is observed non-employed in her first month after layoff for at most three subsequent months. Therefore, we cannot estimate hazard rates of formal reemployment beyond three months after layoff. These limitations, which apply to labor force surveys in other Latin American countries as well, require us to combine survey and administrative data to assess the importance of informal work opportunities.

3.2 Sample selection

The main takeaways in this section are based on estimates from the administrative data, which are population estimates. Yet, to shed light on the role of informal work opportunities, we must contrast those estimates to our maximum likelihood estimates based on survey data. Therefore, we construct samples as comparable as possible in the two datasets, given constraints in PME surveys.

We can only assess UI eligibility and potential UI duration for non-employed workers who had more than 24 months of tenure at layoff in PME. This is because the surveys do not record information about UI benefits or tenure in formal jobs prior to the last one in the 36 months before 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. Note that considering UI for workers with some

[32] The 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: $L_{i,m} = d_{i,m} \int_0^{30} \left[1 - exp\left(-\left(30-b\right)\lambda_m - b\lambda_{m+1}\right)\right] \frac{k(b)exp[-b\lambda_m]}{\int_0^{30} k(s)exp[-s\lambda_m]ds} db + (1-d_{i,m}) \int_0^{30} \left[exp\left(-\left(30-b\right)\lambda_m - b\lambda_{m+1}\right)\right] \frac{k(b)exp[-b\lambda_m]}{\int_0^{30} k(s)exp[-s\lambda_m]ds} db$. 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.

attachment to a formal job is a natural starting case for developing countries. Moreover, we show in the Web Appendix that the patterns based on the administrative data are similar if we consider workers with lower tenure levels at layoff. We further select individuals 18-54 years old who were laid off from a full-time private formal job between 2005 and 2009. We do not consider workers laid off in 2010 to avoid right-censoring issues. We also restrict the sample to the six metropolitan areas covered by PME. We show below and in the Web Appendix that the patterns based on the administrative data are similar if we consider displaced formal workers from all Brazilian regions. Ultimately, the PME sample includes 19,904 observations from 12,327 individuals contributing to the likelihood function. The administrative sample includes 3,393,055 layoffs.

A remaining concern for the comparability of these two samples is that PME may not be representative of the population recorded in the administrative data. Such a concern may arise because of attrition across survey rounds. Fortunately, attrition is limited in PME: our sample would only be 5.4% larger in the absence of attrition. Yet, even in the absence of attrition, representativeness may be an issue because the sampling structure of PME surveys does not directly aim at being representative of our population of interest. For instance, we document some differences between our two samples (e.g., in gender composition) when providing descriptive statistics in the Web Appendix. Nevertheless, we show in the Web Appendix that all our results are similar if we reweigh the PME sample such that it compares better to the administrative sample based on observables.

Finally, we construct three additional samples to document how the empirical patterns based on the administrative data compare in labor markets with different levels of informality. We cannot credibly investigate such heterogeneity for our survey estimates because PME only covers six metropolitan areas. The first sample is a random sample of layoffs (10% or 845,591 layoffs) constructed similarly as the administrative sample described above, but including layoffs from the whole country. The second and third samples also include displaced formal employees from the whole country, but only workers who were not eligible for any UI benefits. The second sample is a random sample of layoffs (20%; 192,166 layoffs) involving workers who didn't have enough tenure at layoff and had no other formal job in the 36 months prior to layoff. The third sample is a random sample of separations (25%; 27,814 separations) involving workers with more than 24 months of tenure at separation, but who were fired for cause, and thus not eligible for UI.

3.3 Four main lessons

We present the main takeaways in four lessons based on the empirical patterns in Figures 3 and 4.

Figure 3 displays average patterns of UI collection, formal reemployment, and overall reemployment using the comparable samples from the administrative and survey data. Panel (a) displays the share taking up UI, drawing UI, and exhausting UI in each month since layoff. The other panels display the survival rate in *non-formal employment* at the start of each month (panel b), the hazard rate of *formal reemployment* in each month (panel c), and the share finding *a new formal job* in each month (panel d). For exposition purposes, these patterns are presented for all workers, for UI takers, and for those who took up UI in their first month of eligibility, separately. Panels (b)-(d) also display our maximum likelihood estimates for the survival rates in *non-employment*, the hazard rates of *overall reemployment*, and the shares finding *any new job*, respectively. These patterns are presented for all workers only because we don't observe UI benefit collection in PME.

Figure 4 compares the empirical patterns based on the administrative data in Figure 3 across labor markets with different levels of informality. The panels plot average state-level estimates for UI and formal reemployment outcomes 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. We assign to each observation the informality rate in the state and year of each layoff, which is measured in PNAD and is defined as the share of informal workers in the non-farm labor force, and we then average informality rates within state.[33]

Lesson 1. *Most UI takers exhaust their benefits, and average paid UI duration is high. This is because the share finding a new formal job is low when workers are eligible for UI. The estimated share finding any new job is higher, so many (but not all) UI beneficiaries likely work informally.*

Most UI takers exhaust their benefits. Figure 3a shows that 45.5% of the sample take up UI in their first month of eligibility and that 41.1% exhaust their benefits four months later. In total, 80.6% of the sample 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 35.6% over the same period in the US, where potential UI duration is only slightly higher.[34] The difference is due to the lower rates of formal reemployment while workers are eligible for UI. For instance, Figure 3b 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. Figures 4a and 4b show that those patterns generalize to the rest of Brazil: the takeup rate and the average paid UI duration among UI takers are high in all Brazilian states. Moreover, we show in the Web Appendix that those patterns are not specific to workers with very high replacement rates.

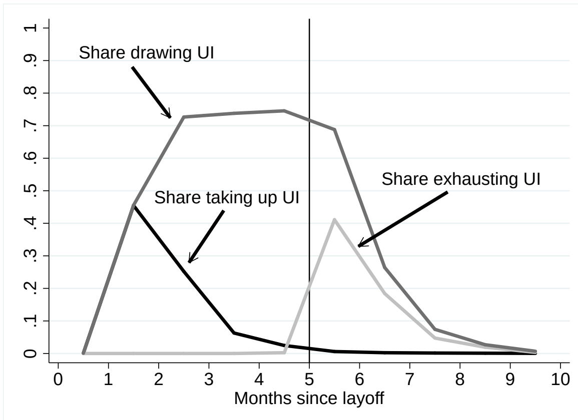
Figure 3 also shows that many but not all UI beneficiaries likely work informally. For instance, Figure 3d shows that the estimated share of all displaced formal employees finding any new job exceeds the share finding a new formal job in the first few months after layoff. As a result, if 35.8%

[33] We define 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 34).

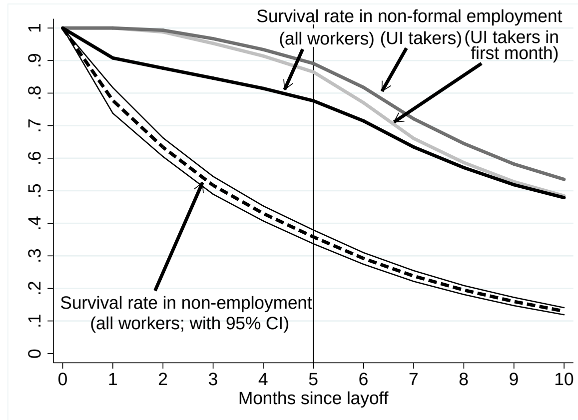
[34] Own calculations using data from www.dol.gov (excluding years with extended benefits). High exhaustion rates are also found in Argentina and China (González-Rozada, Ronconi and Ruffo, 2011; Vodopivec and Tong, 2008).

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

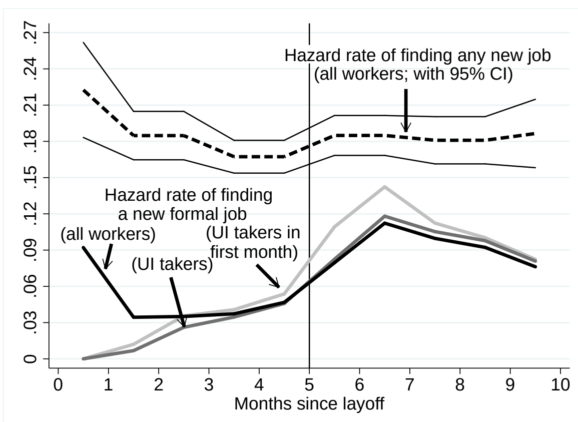
(a) UI benefit collection in each month



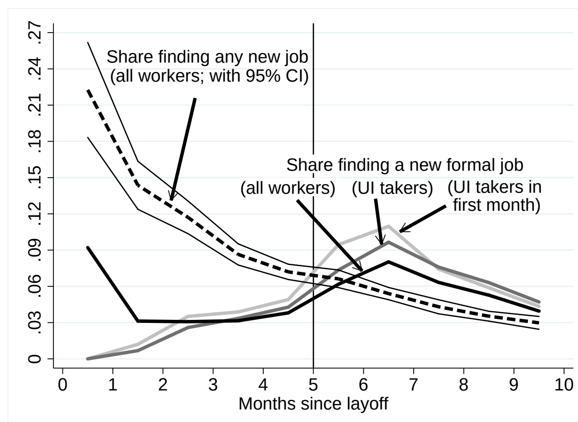
(b) Survival rate in non(-formal)-employment at the start of each month



(c) Hazard rate of finding a new formal job vs. finding any new job in each month



(d) Share finding a new formal job vs. finding any new job in 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. The following panels display the survival rate in *non-formal employment* at the start of each month (panel b), the hazard rate of *formal reemployment* in each month (panel c), and the share finding *a new formal job* in each month (panel d). These patterns are presented for all workers, for UI takers, and for those who took up UI in their first month of eligibility, separately. All these statistics are population means, so we don't include confidence intervals. Panels (b)-(d) also display the estimated survival rate in *non-employment*, the hazard rate of *overall reemployment*, and the share finding *any new job*, respectively, with their 95% confidence intervals. These estimates are presented for all workers because we don't observe UI benefit collection in PME.

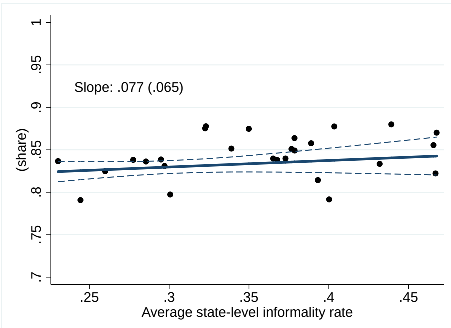
of them remain non-employed five months after layoff, which is comparable to US figures (Chetty, 2008), 77.6% remain without formal employment (Figure 3b). Many displaced formal employees are thus likely working informally, including many UI beneficiaries because 68.8% of all displaced formal employees are still drawing UI benefits in the following month (Figure 3a).[35]

The average paid UI duration can be high because of large behavioral responses to UI incentives or because displaced formal employees would not return rapidly to a formal job even in absence of

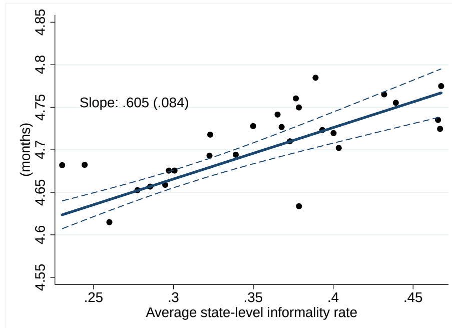
[35]Among those who report being reemployed informally in the first five months after layoff in our PME sample, 32.7% and 61% report being reemployed as self-employed workers and informal employees, respectively.

Figure 4: UI benefit collection and formal reemployment across Brazilian states

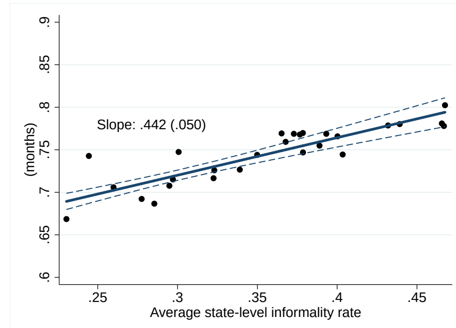
(a) **UI takeup rate** for workers eligible for 5 months of UI



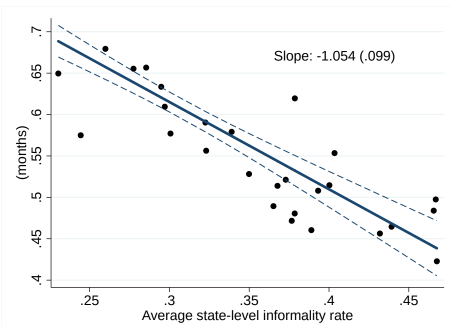
(b) **Average paid UI duration** for workers eligible for 5 months of UI



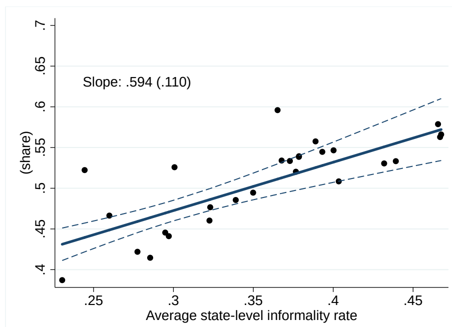
(c) **Mechanical effect of one-month increase in potential UI duration** for workers eligible for 5 months of UI



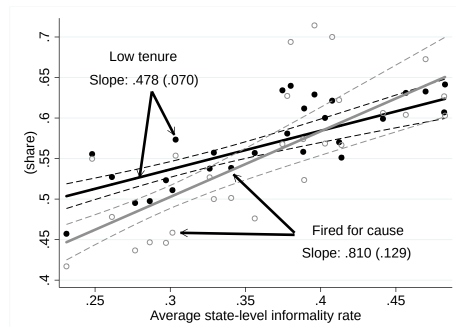
(d) **Maximum behavioral effect of one-month increase in potential UI duration** for workers eligible for 5 months of UI



(e) **Survival rate without a formal job 10 months after layoff** for workers eligible for 5 months of UI



(f) **Survival rate without a formal job 10 months after layoff** for workers **not** eligible for UI



The figure displays state-level estimates 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. Panels (a)-(e) are based on a random sample of layoffs that is constructed similarly as the sample used in Figure 3 (displaced formal workers with more than 24 months of tenure at layoff), but including layoffs from the whole country. These workers are eligible for five months of UI. Panel (f) is based on two random samples that are constructed similarly as the sample used in Figure 3, but that only include workers not eligible for UI: the "low tenure" sample includes workers who didn't have enough tenure at layoff to be eligible for UI (and had no other job in the previous 36 months); the "fired for cause" sample includes workers with more than 24 months of tenure at separation, but who were fired for cause and thus not eligible for UI.

UI incentives. We discuss suggestive evidence of behavioral responses in the next lesson.

Lesson 2. *The share finding a new formal job in each month increases and peaks exactly after benefit exhaustion, suggesting clear behavioral responses to UI incentives. The share finding a new formal job also starts exceeding the share finding any new job exactly after benefit exhaustion, suggesting that informality responses partly drive behavioral responses to UI incentives.*

Patterns in Figures 3c and 3d suggest clear behavioral responses to UI incentives. The hazard rate of formal reemployment and the share of displaced formal employees finding a new formal job in each month increase and peak 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. Such a pattern is not as severe in most developed countries (Card, Chetty and Weber, 2007*b*).[36] It is similar but attenuated in the other samples in Figures 3c and 3d, 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 generalizes to the rest of Brazil, and that it is not driven by workers returning to the same employer.

Figure 3d also suggests that informality responses drive part of the behavioral responses to UI incentives. The estimated share of all displaced formal employees finding any new job 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.[37] Therefore, the increase and peak in the share finding a new formal job after UI exhaustion, which is indicative of behavioral responses to UI incentives, is unlikely due to an increase in the number of workers leaving non-employment. In that case, it can only be due to a shift from informal to formal jobs: a decrease in the number finding an informal job among those finding a new job, or an increase in the number finding a formal job among those working informally.

The existence of behavioral responses implies that (increases in) UI benefits (would) generate some distortion. Yet, it does not imply that the efficiency cost is necessarily high. For instance, the efficiency cost may be limited if displaced formal employees would not return rapidly to a formal job even in absence of UI incentives. We discuss related evidence in the next lesson.

Lesson 3. *Rates of formal reemployment are low even when workers are no longer or not eligible for UI. Consequently, the mechanical effect of increases in UI benefits would be relatively large, the maximum behavioral effect would be limited, and the efficiency cost may not be relatively high.*

Figure 3 shows that, despite the existence of behavioral responses, rates of formal reemployment remain low even when workers are no longer eligible for UI. For instance, Figure 3b shows that 77% of displaced formal employees who took up UI in their first month of eligibility remain without a formal job one month after benefit exhaustion, and would draw an extra month of UI mechanically following an increase in potential UI duration (the mechanical effect of an increase in the UI benefit level would also be large as the average paid UI duration is high). In fact, 48.4% of them remain without a formal job ten months after layoff. The figure is higher for the sample of all UI takers. This is much higher than the comparable figure in the US, given that it is even higher than the UI exhaustion rate over the same period (excluding again years with extended benefits). Figure 4 shows that those patterns generalize to the rest of Brazil. Figure 4c shows that the average mechanical effect of a hypothetical one-month increase in potential UI duration is high in all states, ranging from .7 to .8 month.[38] Figure 4e shows that the share of UI takers remaining without a

[36] van Ours and Vodopivec (2006) find a sizeable spike in Slovenia.

[37] The hazard rates of formal and overall reemployment cannot be compared because the shares that remain without any job and without any formal job (the denominators of the hazard rates) are very different around UI exhaustion.

[38] The mechanical effect is equal to the share of UI takers exhausting their UI benefits and remaining without a formal job one month after benefit exhaustion.

formal job ten months after layoff ranges from about 40% to 60% across Brazilian states.

Importantly, the low rates of formal reemployment even after benefit exhaustion are unlikely due to some long-term effect of UI eligibility in earlier months. Figure 4f shows that the share remaining without a formal job ten months after layoff is even higher for displaced formal employees who are not eligible for any UI benefits. The average paid UI duration would thus be mechanically high if we were to extend UI eligibility to these workers (about 70% of them remain without a formal job five months after layoff; see survival rates in the Web Appendix).

Figure 3 also shows that the estimated survival rates in non-employment remain much lower than the survival rates in non-formal employment after benefit exhaustion. This suggests that the decision of displaced formal employees to work informally is not only due to UI incentives. Relatedly, most formal employees spend a significant amount of time out of formal employment over their career. 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 in the next 10 years (81.3 months), were laid off 2.1 times from a formal job, and drew a total of 7.6 months of UI.

The low rates of formal reemployment in Brazil imply not only that the mechanical effect of increases in UI benefits is large, but also that the potential distortion is limited. For instance, Figure 4d shows that the maximum behavioral effect on average paid UI duration of a hypothetical one-month increase in potential UI duration ranges from .4 month to .7 month.[39] As a result, the efficiency cost of increases in UI benefits may not be relatively high in Brazil. For instance, the ratio of the maximum behavioral effect in Figure 4d to the mechanical effect in Figure 4c provides an upper bound for the first component of the efficiency cost of an increase in potential UI duration (see equation 2). It ranges from .5 to 1, implying that at most \$.5 to \$1 would be lost due to a behavioral effect on average paid UI duration per \$1 reaching mechanical beneficiaries. In comparison, Katz and Meyer (1990) and Landais (2015), who study increases in potential UI duration in the US, estimate a behavioral effect on average paid UI duration that is larger than the mechanical effect, implying that more than \$1 would be lost per \$1 reaching mechanical beneficiaries in their cases. Therefore, the efficiency cost may not be relatively high in Brazil, despite the prevalence of informal work opportunities. In the next section, we directly estimate the efficiency cost of an increase in potential UI duration, and how the actual and maximum behavioral effects compare. However, we first document that differences across labor markets within Brazil are consistent with the differences between Brazil and countries with lower informality as discussed above.

Lesson 4. *The average paid UI duration is higher in labor markets with higher informality*

[39] The maximum behavioral effect of an increase in potential UI duration on average paid UI duration is equal to the extended potential UI duration (6 months) minus the sum of the existing average paid UI duration in Figure 4b and the average mechanical effect in Figure 4c. Note that the maximum behavioral effect of an increase in the UI benefit level would also be limited. It is equal to the difference between the existing potential UI duration (5 months) and the existing average paid UI duration in Figure 4b, which ranges from .2 month to .35 month.

and displaced formal workers return more slowly to a formal job even when they are no longer or not eligible for UI. As a result, the mechanical effect (resp. the maximum behavioral effect) of increases in UI benefits is larger (resp. smaller), and the efficiency cost may not be higher.

Figure 4b shows that the average paid UI duration is higher in states with higher informality. Again, this could be because behavioral responses to UI incentives are larger or because displaced formal employees would return more slowly to a formal job even in absence of UI incentives. We estimate how the behavioral effect of an increase in potential UI duration differs across states in Section 5. However, Figures 4c, 4e, and 4f already document that rates of formal reemployment are lower in states with higher informality, even when displaced formal employees are no longer or not eligible for UI. Figures 4c and 4e show that the mechanical effect of a hypothetical one-month increase in potential UI duration, and the share of displaced formal employees who remain without a formal job ten months after layoff, are strongly increasing in informality rates. Moreover, these patterns are unlikely due to some long-term effect of UI eligibility in earlier months. Figure 4f shows that the share remaining without a formal job ten months after layoff is also strongly increasing in informality rates for workers who were not eligible for any UI benefits.

The lower rates of formal reemployment imply not only that the mechanical effect of increases in UI benefits will be larger, but also that the potential distortion will be smaller in states with higher informality. For instance, Figure 4d shows that the maximum behavioral effect on average paid UI duration of a hypothetical increase in potential UI duration is strongly decreasing in informality rates. Therefore, the efficiency cost of an increase in potential UI duration would be decreasing in informality rates, unless the ratio of the actual to the maximum behavioral effects is much larger in states with higher informality. We directly estimate how the efficiency cost of an increase in potential UI duration differ across states with different levels of informality in Section 5.

Finally, we confirm our findings through a regression analysis in the Web Appendix. We estimate correlations between informality rates and individual-level UI and formal reemployment outcomes, using the same samples as in Figure 4. This allows us to estimate correlations with informality rates holding (un)employment rates constant. 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 been studied in contexts of lower informality. This approach also allows us to use more disaggregated measures of the relevant informality rate, namely state-level informality rates disaggregated by year and gender. Labor markets became more formal over time in Brazil and women remain less likely to work in the formal sector (see Web Appendix). Furthermore, this approach allows us to show that the correlations in Figure 4 are not simply due to fixed differences across states or to difference in workers' observables: results are similar including year fixed effects, state fixed effects, and a rich set of individual controls.

In sum, the potential distortion appears to increase as an economy formalizes. The concern that informal work opportunities exacerbate the efficiency cost thus rests on the assumption that the actual behavioral effect is much larger compared to the maximum behavioral effect in a context of high informality, which we evaluate in the next sections.

3.4 Generalizability of the empirical patterns

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 at layoff and different levels of potential UI duration. The average paid UI duration is always very high compared to the potential UI duration. Rates of formal reemployment always increase sharply after benefit exhaustion, but remain relatively low.

4 The efficiency cost of increases in potential UI duration

In Section 3, we documented evidence of behavioral responses to UI incentives, which are likely due in part to 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. We then compare our estimates with existing 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 labor markets with different informality rates in the next section.

Note that, in theory, one could also exploit variation in the UI benefit level 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 relatively 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 of them are 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, and for which we find clear evidence of behavioral responses.

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 in this section using the administrative data. We cannot exploit this cutoff in PME because we only know UI eligibility and potential UI duration for workers with more than 24 months of tenure at layoff.

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, more precisely 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 all 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 Figures 3 and 4), and for a random sample of all displaced formal workers over the same period (see Web Appendix).

We focus on the 24-month cutoff because the usual conditions supporting the validity of a RD design appear to hold at this cutoff: the layoff density is smooth and there is no difference in sample composition at the cutoff (see below). 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.[40]

[40]This has been again confirmed in a recent paper by Carvalho, Corbi and Narita (2017). 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 UI spell (Gerard, Rokkanen and Rothe, 2016).

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 UI 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

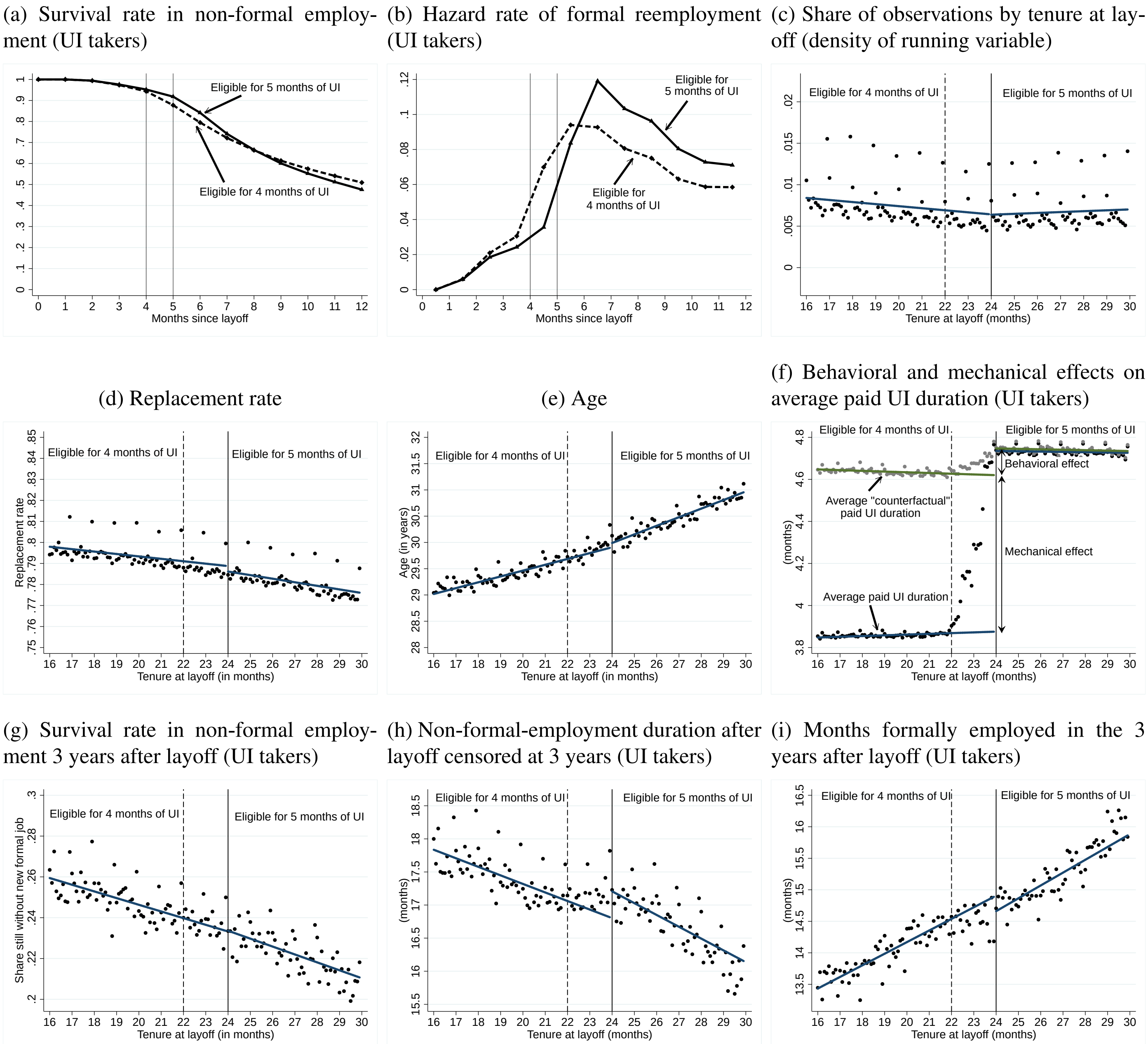
We begin by providing some graphical evidence in Figure 5. Panels (a) and (b) display survival rates in non-formal employment and hazard rates of formal reemployment, constructed as in section 3, for UI takers eligible for four vs. five months of UI in our sample. Hazard rates are very low while workers 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. Note also that the survival and hazard rates are very similar in Figure 5 and in Figure 3 for UI takers eligible for five months of UI, despite the differences between the samples used in the two figures.

Panels (c)-(i) display a series of RD graphs, which plot the average of some selected variables by tenure levels around the cutoff. First, panels (c)-(e) show that there is no visible change in the distribution and composition of our sample around the cutoff (we show that there is no visible change in the average of other worker characteristics around the cutoff in the Web Appendix). This graphical evidence supports the validity of our RD design.

Second, panel (f) disentangles the behavioral and mechanical effects of the increase in potential UI duration on the average paid UI duration of UI takers (UI takeup is constant at around 86%; see Web Appendix). For this purpose, it displays two variables. The first variable is the actual change in the average paid UI duration at the cutoff, which is defined as follows: $D_i^B \equiv \sum_{j=1}^{P} \mathbb{1}(DrawingUI_{i,j} = 1)$, 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. The change in this variable at the cutoff captures the total effect on average paid UI duration.[41] However, to separate the behavioral and mechanical

[41] 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

Figure 5: Graphical evidence for the regression discontinuity (RD) design



(a) Survival rate in non-formal employment (UI takers)

(b) Hazard rate of formal reemployment (UI takers)

(c) Share of observations by tenure at layoff (density of running variable)

(d) Replacement rate

(e) Age

(f) Behavioral and mechanical effects on average paid UI duration (UI takers)

(g) Survival rate in non-formal employment 3 years after layoff (UI takers)

(h) Non-formal-employment duration after layoff censored at 3 years (UI takers)

(i) 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)-(i) display averages by tenure levels (.1 month bins) around the cutoff. Panel (c) displays the density of the running variable, and panels (d) and (e) display worker characteristics (replacement rate and age). Panel (f) displays the paid UI duration (black dots) and the "counterfactual" paid UI duration (gray dots), which allows us to decompose the total effect on paid UI duration into behavioral and mechanical effects. Finally, panels (g)-(i) then focus on longer-term outcomes, restricting attention to workers laid off between 2005 and 2007 such that we can follow the sample for three years after layoff. Panel (g) displays the share still without a formal job three years after layoff, and panels (h) and (i) 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).

days of layoff and drew her first UI payment (resp. her j^{th} month of UI) before being formally reemployed.

effects, we must also 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 second variable, 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 difference between the extended potential UI duration (five months) and the counterfactual paid UI duration also captures the maximum behavioral effect on average paid UI duration.

The average paid UI duration is around 3.9 months for workers with less than 22 months of tenure. It then increases between 22 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 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, and that the maximum behavioral effect on paid UI duration is around .35 month. The average counterfactual paid UI duration starts increasing at exactly 22 months of tenure and reaches 4.75 months above the cutoff, indicating that the behavioral effect on paid UI duration only amounts to about .10 month or about a third of its maximum value. Note that the averages of the actual and counterfactual paid UI durations are similar above the cutoff indicating that assignment and compliance are not an issue in our case.

Third, panels (g)-(i) consider behavioral responses for longer-term outcomes. We use an horizon of three years after layoff because panel (g) shows that there is no change at the cutoff in the share remaining without formal employment by then. Panels (h) and (i) show that UI takers eligible for a fifth month of UI had a longer non-formal-employment duration and spent fewer months formally employed in those three years. In contrast, we show in the Web Appendix (and in regression results below) that there is no change around the cutoff in the probability to be formally employed and in the wage conditional on formal employment three years after layoff.

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 $\mathbb{1}(T_i \geq 0)$, and a control function in tenure $f(T_i)$:

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

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 thus 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)-(i) in Figure 5. 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.[42]

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 5 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 5h; 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 theoretical 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 ($\widehat{\beta}$) and the estimated level at the cutoff from below ($\widehat{\alpha}$) for several variables.

Results in panel A support the validity of our RD design. We find again no evidence of a discontinuity in the distribution or the composition (age, gender, education, replacement rate) of our

[42]We 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, our conclusions are robust to using quadratic functions.

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 in 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 from a level of 4.624 months at the cutoff from below.[43] The behavioral effect on average paid UI duration thus leads to a loss of $\frac{.126}{.861-.126}$ =\$.171 per \$1 reaching mechanical beneficiaries. This first component of the efficiency cost is relatively low because the mechanical effect is large and the potential distortion is limited, and not because workers who would otherwise have found a formal job rapidly are not responsive to the change in UI incentives. Indeed, the ratio of the actual to the maximum behavioral effects is relatively high, at $\frac{.126}{5-.4.624}$ =33.5%, but it is not high enough for the behavioral effect to be relatively large.

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 (we present impacts on hazard rates and survival rates in each month after layoff in the Web Appendix). 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.[44]

[43] To confirm that the average counterfactual paid UI duration closely approximates the average paid UI duration for workers eligible for five months of UI, we also ran a similar regression as in column (3) on the following variable: $\sum_{j=1}^{4} \mathbb{1}(DrawingUI_{ij} = 1) + \mathbb{1}(DrawingUI_{i4} = 1)\, \mathbb{1}(T_i \geq 0)\, \mathbb{1}(NotFormallyReemployed_{i1} = 1)$, 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.

[44] Moreover, 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).

Table 1: Regression discontinuity estimates of the impact of the one-month increase in potential UI duration

A. VALIDITY CHECKS (VARIABLES AT LAYOFF)	Share of observations by tenure level (1)	Age (years) (2)	Male (dummy) (3)	Years of education (4)	Replacement rate (5)
Tenure≥24 months	-.0003	.0816	.0061	-.0301	-.0037
	(.0019)	(.0769)	(.0072)	(.0236)	(.0046)
Constant	.0073***	29.93***	.5819***	9.163***	.7908***
	(.0017)	(.0617)	(.0065)	(.02)	(.0042)
Observations	1,969,137	1,969,137	1,969,137	1,969,137	1,969,137

B. IMPACTS (FIRST YEAR AFTER LAYOFF)		Paid UI duration			
	UI takeup (dummy) (1)	Anticipated ($P=4$) (months) (2)	Total ($P=\{4,5\}$) (months) (3)	Counterfactual paid UI duration (months) (4)	Duration without a formal job (censored at 1 year) (months) (5)
Tenure≥24 months	.006	.0426***	.8605***	.1259***	.2904***
	(.0037)	(.0055)	(.0084)	(.0093)	(.0664)
Constant	.8626***	3.821***	3.877***	4.624***	9.087***
	(.0032)	(.0047)	(.007)	(.008)	(.0585)
Observations	1,969,137	1,704,333	1,704,333	1,704,333	1,704,333

C. IMPACTS (THREE YEARS AFTER LAYOFF)				December 3 years later	
	Not yet formally reemployed 3 years after layoff (dummy) (1)	Duration without a formal job (censored at 3 years) (months) (2)	Months formally employed in 3 years after layoff (months) (3)	Formally employed (dummy) (4)	Real monthly wage if formally employed (R$, logs) (5)
Tenure≥24 months	-.0004	.3894**	-.2432**	.0005	-.0018
	(.0054)	(.1813)	(.0976)	(.0036)	(.0077)
Constant	.2351***	16.87***	14.89***	.4969***	6.804***
	(.0048)	(.16)	(.0814)	(.0032)	(.0064)
Observations	917,344	917,344	917,344	917,344	448,800

Standard errors clustered by tenure level in parentheses. Significance levels: * 10%, ** 5%, ***1%. The table displays RD estimates for the one-month increase in potential UI duration at the 24-month tenure cutoff. All specifications include linear controls in tenure (normalized to the cutoff) on each side of the cutoff and use an edge kernel. Panel A presents supportive evidence for the validity of the RD design by testing for a discontinuity in the density of the running variable (column 1; the outcome is the share of the sample by .1-month tenure bin) or in average worker chararacteristics at the cutoff. Panels B and C present treatment effects in the short and longer-term. The sample in Panel C is restricted to workers laid off between 2005 and 2007, such that we can follow the sample for three years after layoff. Columns (4) and (5) consider outcomes in December because we only measure monthly wages precisely in December in RAIS. In all cases, the constant captures the estimated average outcome at the cutoff from below.

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 instead 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 a standard error of .023 obtained by the delta method) or \$.2 per \$1 reaching mechanical beneficiaries.

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

Our estimate of the efficiency cost, which may constitute an upper bound in our context, 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. Interestingly, the efficiency cost is larger in his case, despite the fact that the behavioral effect is smaller as a share of its maximum value. The efficiency cost is thus smaller in our context because the potential distortion is smaller, and not because workers who would find a formal job rapidly in absence of the increase in potential UI duration are less responsive to UI incentives.

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 per \$1 reaching mechanical beneficiaries.

Comparisons with estimates from other countries with lower informality is complicated because the potential UI duration is often longer.[45] 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).[46] Finally, comparisons with other Latin American countries is complicated because the few papers that study the impact of UI on some labor market outcomes do not report the estimates necessary for the efficiency cost.[47]

4.7 Robustness checks

We present robustness checks for our impacts of interest in the Web Appendix. 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, 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 \$.148 to \$.201 per \$1 reaching mechanical beneficiaries.

We further confirm all our results by using the other source of quasi-experimental variation in potential UI duration in Brazil, namely temporary extensions of UI benefits. We exploit a policy (fully described in the Web Appendix) that extended UI benefits by two months in March 2009 for workers laid off in December 2008 from a list of 42 sector-state pairs. We use a difference-in-difference strategy comparing workers laid off in December vs. November 2008, from eligible

[45] Similarly, 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.

[46] Details for all these comparisons with existing estimates are provided in the Web Appendix.

[47] For instance, estimates from proportional hazard models, as in Gonzalez-Rozada and Ruffo (2016), do not map easily into the correct measure of efficiency cost.

vs. ineligible sector-state pairs. We obtain an efficiency cost of $.155 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 an increase in potential UI duration was not necessarily high in a context of high informality, compared to estimates from richer countries. In this section, we directly estimate how the efficiency cost of the increase in potential UI duration varies 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 relates 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. For instance, we showed in Section 3 that displaced formal workers return slower to a formal job in states with higher informality in Brazil even when they are no longer or not eligible for UI. It may 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.

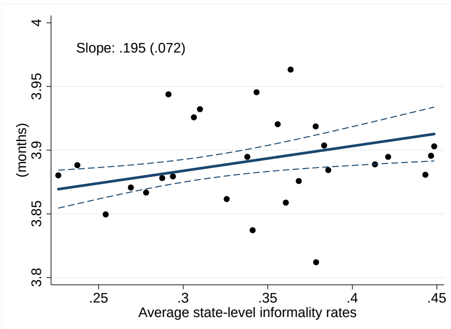
5.1 Main empirical strategy and results

Our main empirical strategy consists in estimating the same RD specifications as in Table 1 for each of the 27 Brazilian states separately, and then regressing our estimates on state-level informality rates (constructed as in Figure 4 for the RD sample), weighting estimates by the inverse of their standard error squared. Figures 6 and 7 display our results. All 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.

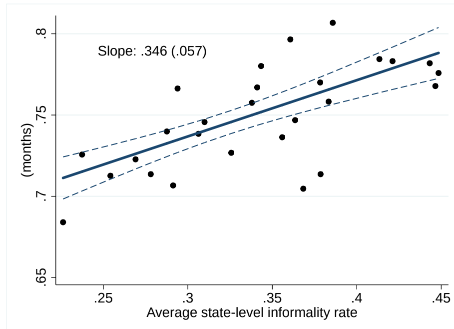
Figure 6 considers UI collection outcomes. Panels (a), (b), and (c) first replicate descriptive patterns shown in Figure 4 for our RD sample (without the increase in potential UI duration). Panel (a) displays the average paid UI duration at the cutoff from below. It is large in every state compared to the potential UI duration of four months, ranging from 3.8 months to 3.95 months, and it is increasing in informality rates. Panel (b) displays the mechanical effect of the one-month increase in potential UI duration, which is the estimated discontinuity at the cutoff for the difference

Figure 6: Heterogeneity of regression discontinuity estimates with labor market informality (I)

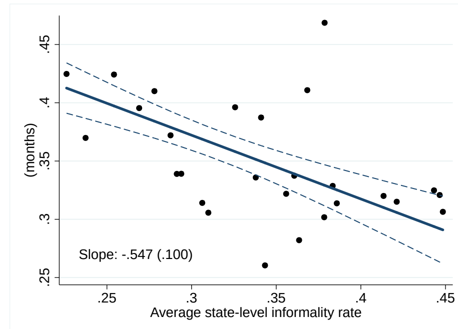
(a) Average paid UI duration at the cutoff from below



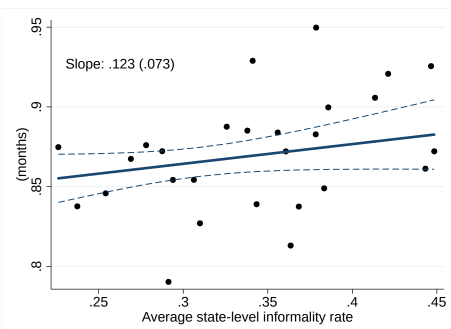
(b) Mechanical effect on average paid UI duration



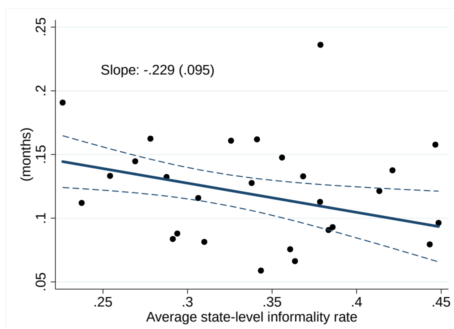
(c) Maximum behavioral effect on average paid UI duration



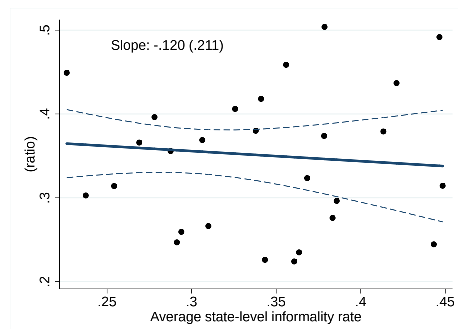
(d) Total effect on average paid UI duration



(e) Behavioral effect on average paid UI duration



(f) Ratio of actual to maximum behavioral effects on average paid UI duration



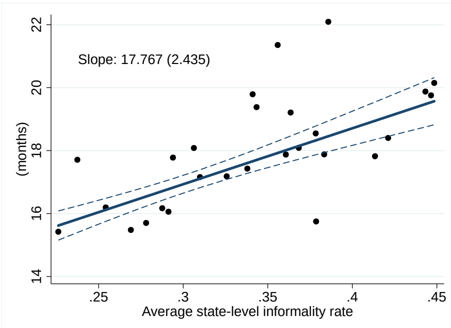
The figure displays results from estimating specifications as in Table 1 in each Brazilian state separately. State-level estimates are plotted against average state-level informality rates. Lines display the result of a WLS regression of these estimates on informality rates, weighting estimates by the 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.

between actual and counterfactual paid UI durations (see Figure 5f). It is again large in every state and strongly increasing in informality rates. The slope implies that the mechanical effect increases from .71 month to .79 month over our range of informality rates. The patterns in panels (a) and (b) imply that the maximum behavioral effect on average paid UI duration ($5-4.624 = .376$ month in Table 1) is limited in every state and is decreasing in informality rates. This is shown in panel (c). The slope implies a decrease from .41 month to .29 month over our range of informality rates. This also implies that at most \$.58 to \$.37 would be lost due to a behavioral effect on average paid UI duration per \$1 reaching mechanical beneficiaries in our RD sample.

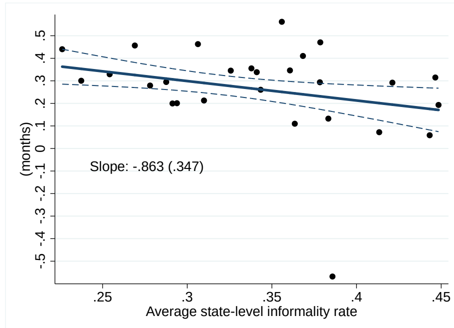
Panels (d), (e), and (f) then present results for the impact of the one-month increase in potential UI duration at the cutoff. Panel (d) shows that the total effect on the average paid UI duration is large in every state. It is slightly larger in states with higher informality, but the slope is not significant. Given that the mechanical effect is increasing in panel (b), the relatively flat slope in panel (d) implies that the behavioral effect on average paid UI duration must be decreasing in informality rates. This is shown in panel (e). The slope is significant and implies a decrease

Figure 7: Heterogeneity of regression discontinuity estimates with labor market informality (II)

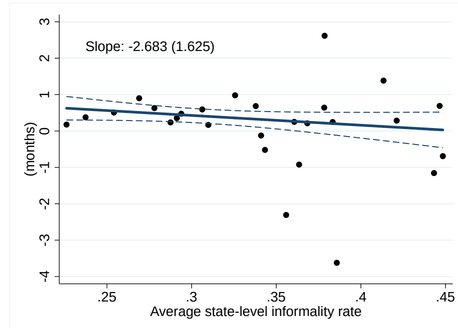
(a) Non-formal-employment duration at the cutoff from below (after 3 years)



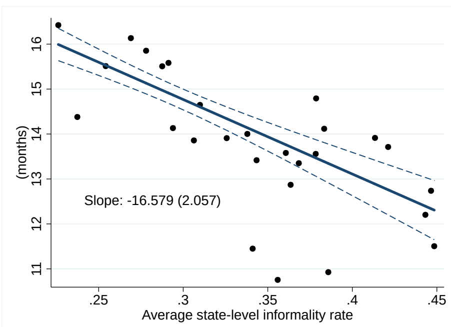
(b) Impact on non-formal-employment duration at the cutoff (after 1 year)



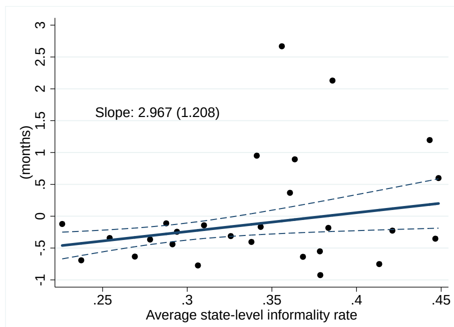
(c) Impact on non-formal-employment duration at the cutoff (after 3 years)



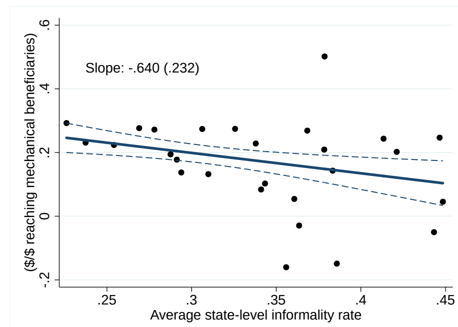
(d) Number of months formally employed in the next 3 years at the cutoff from below



(e) Impact on number of months formally employed in the next 3 years at the cutoff



(f) 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 average paid UI duration is increasing (see Figure 6), the behavioral effect on average paid UI duration is decreasing (see Figure 6), and the behavioral effect on the average number of months spent formally employed after layoff is becoming less negative.

of 35.9% over our range of informality rates, from .145 month to .093 month. Finally, panel (f) displays the ratio of the actual to the maximum behavioral effects on average paid UI duration (the ratio of the estimates in panels (e) and (c); standard errors are obtained by the delta method). It is relatively high, ranging from 20% to 50%, but there is no evidence that workers' ability to respond to UI incentives increases with informality rates. Therefore, the behavioral effect on average paid UI duration is smaller in states with higher informality, because the potential distortion is smaller, and not because workers who would not draw a fifth month of UI mechanically are less responsive to UI incentives. Of course, workers' ability to respond to UI incentives may start decreasing at lower levels of informality than those observed in Brazil and other Latin American countries.

Figure 7 considers longer-term outcomes. Panel (a) displays the average non-formal-employment duration (censored after three years) at the cutoff from below. It is large and strongly increasing in informality rates; the difference reaches four months over our range of informality rates. This confirms patterns shown in Figure 4, namely that displaced formal workers return slower to a formal job in labor markets with higher informality, irrespective of increases in potential UI duration. There is thus less room to begin with for UI to distort their formal reemployment decisions. Panels (b) and (c) then display the impact of the one-month increase in potential UI duration at the cutoff on the same variable. It is decreasing in informality rates. We present results censoring the duration at both one year (panel b) and three years (panel c) after layoff, because state-specific estimates becomes noisier for smaller states with the longer horizon.

Next, panels (d) and (e) present results for the number of months formally employed in the three years after layoff. Panel (d) displays the average at the cutoff from below. It is lower in states with higher informality, implying that there is again less room to begin with for UI to distort this outcome. This is mostly because displaced formal employees take longer to find a new formal job, as shown in panel (a). It is also partly because, upon formal reemployment, workers spend a smaller share q of their remaining career formally employed. We show in the Web Appendix that this share decreases significantly from about 75% to 73% over our range of informality rates. Panel (e) then displays the impact of the one-month increase in potential UI duration at the cutoff. The impact is negative in most states (estimates are noisy in smaller states), but less negative in states with higher informality rates. This is because the impact on non-formal-employment duration is smaller, as shown in panels (b) and (c). Indeed, we find no differential impact on the share q of workers' remaining career formally employed (see Web Appendix).

Finally, in panel (f), we combine our estimates to evaluate the efficiency cost (standard errors are obtained by the delta method). As in Section 4, we use the average number of UI beneficiaries per private formal employee in each state between 2005 and 2009 to approximate the scaling coefficient $\frac{D^B}{D^F}$.[48] The efficiency cost is lower in states with higher informality rates. The slope is significant and implies that the efficiency cost decreases by over 60% over our range of informality rates, from \$.245 to \$.1 per \$1 reaching mechanical beneficiaries.

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 of behavioral responses, including informality responses.

5.2 Robustness checks

We show in the Web Appendix that the patterns in Figures 6 and 7 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 through another regression analysis in the Web

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

Appendix. We estimate correlations with informality rates directly by using a variant of equation (4), in which all the right-hand-side variables are interacted with informality rates. This approach has the same advantages as those highlighted for a similar approach in Section 3. It allows us to estimate correlations with informality rates holding (un)employment rates constant (also interacting all the right-hand-side variables with (un)employment rates) and to use more disaggregated measures of the informality rate prevailing in a given labor market (state-level informality rates disaggregated by year and gender). It also allows us to show that our correlations are not simply due to fixed differences across states or to differences in workers' observables: results are similar including year fixed effects, state fixed effects, and a rich set of individual controls.

In sum, the efficiency cost increases as labor market formalizes in Brazil, because the potential distortion increases and workers's actual ability to respond to UI incentives does not decrease.

6 Marginal value of insurance and welfare effect

We estimated an average efficiency cost of \$.2 per \$1 reaching mechanical beneficiaries 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 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 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.[49]

A relatively low efficiency cost, however, does not imply that the welfare effect is positive in Brazil. A low efficiency cost is in fact consistent with a low marginal value of insurance if it is relatively easy to find informal jobs and those are close substitutes for formal jobs. Yet, a low efficiency cost is also consistent with a high marginal value of insurance if it is relatively hard to find a new formal job and informal jobs are imperfect means of self-insurance.

Although the focus of this paper is on the efficiency side, we end by shedding some light on the marginal value of insurance. There is no publicly available data in Brazil to evaluate the marginal value of insurance directly, for instance through a "consumption approach" (Gruber, 1997*a*; Kolsrud et al., 2015). Nevertheless, we use PME to explore the extent to which informal work opportunities (and other household members' labor supply) allow displaced formal workers to mitigate disposable income drops following layoff. This is an important first step in order to evaluate their need for insurance, particularly if other standard means of consumption smoothing

[49] There 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.

(e.g., formal credit) are more costly in developing countries (Chetty and Looney, 2006).[50] Using PME, we already showed in Figure 3 that not all displaced formal workers who remain without a formal job find an informal job rapidly in Brazil. In fact, the share of workers eligible for five months of UI that remain non-employed six months after layoff (about 30%) is comparable to US figures (Chetty, 2008). We complement this finding in the present section by investigating changes in earnings and disposable income following layoff in PME.

Unfortunately, we cannot systematically estimate how our results vary with informality rates in this section because PME only covers six metropolitan areas. Yet, we show in the Web Appendix that results are similar in the two areas with the lowest informality rates and in the other four areas.

6.1 Earnings and disposable income drops following layoff

We would ideally compare earnings and disposable income for the same individuals when they are formally employed and after they exhaust their UI benefits (resp. when they draw UI benefits) in order to measure disposable income drops for mechanical beneficiaries of increases in the potential UI duration (resp. in the UI benefit level). Unfortunately, PME has no information on UI benefit collection and its panel is too short to follow workers from layoff to UI exhaustion. PME has also no information on earnings in prior jobs. We thus proceed as follows given the data limitations.

We start with the same sample of non-employed 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 in a formal or an informal job. Finally, we use the data as a repeated cross-section in the following specification for the outcome y of worker i:

$$y_{ikt} = \alpha + \sum_{k}\sum_{t}\beta_{kt}\,\mathbb{1}(Status_i = k \;\&\; Period_i = t) + \varepsilon_i, \tag{5}$$

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 $\widehat{\beta_{kt}}$'s for $k = \{InfReemp, Nonemp\}$ allow us to shed light on the relative earnings and disposable income of UI beneficiaries (for $t = \{before, around\}$) and UI exhaustees (for $t = \{around, after\}$) who are working informally and remain non-employed, respectively.[51] They are displayed in Table 2, in

[50] Brazil had the highest real interest rates in the emerging world between 2000 and 2009 (Segura-Ubiergo, 2012).

[51] This interpretation assumes that their first month of reemployment is informative for the disposable income in later months of displaced formal workers reemployed informally. Our approach may underestimate disposable income gaps

percentage of the average outcome prior to layoff (omitted group). Standard errors are clustered by individual and obtained by the delta method. All regressions use sampling weights.

Outcomes in columns (1)-(2) and (3)-(4) are workers' real net earnings and real 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, as well as for possible labor supply responses from other household members.

There is a concern of selection bias with a specification such as equation (5). For instance, we would underestimate (resp. overestimate) earnings and disposable income drops for displaced formal workers reemployed informally, if these workers are positively selected (resp. negatively selected), i.e., if they had a disposable income above average (resp. below average) prior to layoff. We assess the severity of this issue by comparing estimates from specifications that omit (col. 1 and 3) or include (col. 2 and 4) a rich set of individual controls (gender, age, education, tenure, sector). We show in the Web Appendix that these controls capture 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. We would thus expect these controls to have a strong influence on our coefficient estimates if these estimates were largely driven by selection effects.

Table 2 shows that our 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 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 and 30% of them have no disposable income at all. The differences are smaller for formally reemployed workers. Importantly, our estimates are essentially unchanged when we include the individual controls. They are also similar if we use the same controls to reweight 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). Moreover, the disposable income gaps are even larger when we include worker fixed effects (see Web Appendix).[52] It is therefore unlikely that our estimates are largely driven by selection effects.

Results in columns (1)-(4) indicate that displaced formal workers who remain without a formal job experience much lower disposable income levels, even when reemployed informally. We can

because earnings have often already decreased in the month just prior to layoff and because some workers informally reemployed may quickly lose their informal job. We may instead overestimate these gaps if some workers report being formally reemployed but are kept on an informal payroll temporarily. Finally, note that abstracting from the facts that UI takeup is imperfect and not observed in PME is unlikely to severely bias our results: UI takeup is above 80%, and 50% of UI non-takers are formally reemployed 30 days after layoff (see Section 3).

[52] Results are also similar if we use a sample of workers laid off since 2003 (see Web Appendix).

compute overall estimates for UI beneficiaries and UI exhaustees, combining results in Table 2 and estimates of the shares of non-employed and informally reemployed workers among them. We calculate these shares from the survival rates in non-employment and non-formal employment in Figure 3: $share^b_{non} = \sum_{t=1}^{5} S_{t,non} / \sum_{t=1}^{5} S_t = .643$ and $share^P_{non} = \sum_{t=6}^{8} S_{t,non} / \sum_{t=6}^{8} S_t = .378$, where S_t and $S_{t,non}$ are the survival rates in non-formal employment and in non-employment. We then obtain a drop in disposable income of 43.7% for UI beneficiaries and of 38.5% for UI exhaustees.[53]

A caveat with these figures is that they do not account for the UI benefits that workers can receive while eligible for UI. We thus gauge the role of UI benefits in columns (5)-(6). 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, the drop in disposable income disappears for the informally reemployed workers and it is reduced to 11%-15% for non-employed workers. Taking into account UI benefits then reduces our overall estimate of the drop in disposable income for UI beneficiaries to 5.7%.[54] The figure of course doesn't change for UI exhaustees as they do not draw UI benefits anymore.

6.2 Marginal value of insurance and welfare effects

The above estimates allow us to shed some light on the marginal value of insurance for mechanical beneficiaries of increases in UI benefits. A drop in disposable income of only 5.7% suggests that the marginal value of insurance is limited for increases in the UI benefit level. Workers may be able to find additional ways to self-insure against such small income shocks. Therefore, without estimating its efficiency cost, it appears that an increase in the UI benefit level is unlikely to generate large welfare gains in Brazil. In contrast, a drop in disposable income of 38.5% indicates that informal work opportunities do not allow UI exhaustees to fully mitigate the income shock following layoff without UI. The size of the marginal value of insurance for an increase in the potential UI duration will then depend on workers' ability to find other ways to self-insure against such income shocks and on the curvature of their utility function. This is illustrated in Table 3. The marginal value of insurance is calculated as: $\gamma \times \rho \times \frac{DispInc^F - DispInc^P}{DispInc^F}$, where the drop in disposable income is multiplied by a coefficient of relative risk aversion (γ) and a parameter capturing the sensitivity of consumption to disposable income shocks (ρ). The marginal value of insurance would exceed our estimate of efficiency cost (.2), and welfare effects would thus be positive, for a benchmark coefficient of risk aversion of 2, as long as disposable income gaps are less than four times as large as consumption gaps. Chetty and Szeidl (2007) argue that the coefficient of risk aversion may

[53] The drop for UI beneficiaries is calculated as: $(1 - .643) \times \frac{-.33 - .3092}{2} + .643 \times \frac{-.5092 - .4958}{2} = -.437$. The drop for UI exhaustees is calculated as: $(1 - .378) \times \frac{-.3092 - .3283}{2} + .378 \times \frac{-.4958 - .4943}{2} = -.385$.

[54] The drop for UI beneficiaries is calculated as: $(1 - .643) \times \frac{.0477 + .0653}{2} + .643 \times \frac{-.1283 - .1105}{2} = -.057$.

Table 2: Estimates of earnings and disposable income gaps by reemployment status vs. prior to layoff

	Earnings (Δ%) vs. prior to layoff		Disposable income (Δ%) vs. prior to layoff		Disposable income + UI (Δ%) vs. prior to layoff	
	(1)	(2)	(3)	(4)	(5)	(6)
Formally reemployed before UI exhaustion	-.1599***	-.1733***	-.0848**	-.0904**	-.0848**	-.0903**
	(.0408)	(.0328)	(.0429)	(.0389)	(.0429)	(.0387)
Formally reemployed around UI exhaustion	-.234***	-.2583***	-.114**	-.135***	-.114**	-.1341***
	(.0488)	(.0418)	(.0512)	(.0457)	(.0512)	(.0456)
Formally reemployed after UI exhaustion	-.269***	-.2909***	-.0962	-.1128*	-.0962	-.113*
	(.057)	(.0539)	(.0667)	(.0637)	(.0667)	(.0636)
Informally reemployed before UI exhaustion	-.4814***	-.4617***	-.33***	-.2947***	.0477	.0859**
	(.0276)	(.0248)	(.0348)	(.0305)	(.043)	(.038)
Informally reemployed around UI exhaustion	-.4293***	-.3833***	-.3092***	-.25***	.0653	.126***
	(.0453)	(.038)	(.0419)	(.036)	(.0499)	(.0437)
Informally reemployed after UI exhaustion	-.5071***	-.4963***	-.3283***	-.2902***	.0141	.0511
	(.0347)	(.0339)	(.0451)	(.0403)	(.0535)	(.048)
Non-employed before UI exhaustion ^{a}			-.5092***	-.5273***	-.1283***	-.147***
			(.0191)	(.016)	(.0288)	(.024)
Non-employed around UI exhaustion ^{b}			-.4958***	-.5241***	-.1105***	-.1412***
			(.0229)	(.0195)	(.0322)	(.0269)
Non-employed after UI exhaustion ^{c}			-.4943***	-.5273***	-.1106***	-.1473***
			(.0241)	(.0216)	(.033)	(.0283)
Observations	5,059	5,059	34,608	34,608	34,608	34,608
Including controls	No	Yes	No	Yes	No	Yes

Standard errors clustered by individual and obtained by the delta method in parentheses. Significance levels: * 10%, ** 5%, ***1%. The table displays results from estimating variants of equation (5). In particular, it displays estimates of the average earnings (columns 1-2) and disposable income (columns 3-4) of displaced formal workers who are reemployed formally, who are reemployed informally, or who 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 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. 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.

Table 3: Calibrations for the marginal value of insurance of an increase in potential UI duration

Coefficient of relative risk aversion (γ)	Sensitivity of consumption to disposable income shocks .25	.5	.75	1
.5	.049	.097	.157	.193
1	.096	.193	.289	.385
2	.193	.385	.578	.77
4	.385	.77	1.154	1.54

The table displays calibrations for the marginal value of insurance of increases in potential UI duration. The marginal value of insurance is calculated as: $\gamma \times \rho \times \frac{DispInc^F - DispInc^P}{DispInc^F}$, where γ is a coefficient of relative risk aversion, and ρ captures the sensitivity of consumption to disposable income shocks. Calibrations use an estimate of the disposable income drop of 38.5% (see text).

be even higher for layoff shocks due to consumption commitments. However, more evidence is needed on the sensitivity of consumption to income shocks for displaced formal workers in order to provide a complete evaluation of the welfare effects of an increase in potential UI duration.

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 is 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' actual 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, it implies that formal employment may not be the *normal* state of the world for many formal workers in our 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' demand for insurance varies with informality 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 lump-sum at takeup may be attractive. It would provide most workers with a comparable degree of insurance but eliminate the (limited) moral hazard problem.[55]

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 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.[56] Of course, agents' ability to respond to UI incentives may start decreasing at lower levels of informality than those observed in Brazil.

[55] We thank Johannes Schmieder for this observation.

[56] This could also explain why Bergolo and Cruces (2016) found a sizable drop in formal employment at an eligibility cutoff for a CCT program, at a time where formal employment rates were increasing even among CCT beneficiaries.

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