Informal Labor and the Efficiency Cost of Social Programs: Evidence from Unemployment Insurance in Brazil

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It is widely believed that the presence of a large informal sector increases the efficiency cost of social programs in developing countries. We evaluate such claims for the case of Unemployment Insurance (UI) by combining an optimal UI framework with comprehensive data from Brazil. Using quasi-experimental variation in potential UI duration, we find clear evidence for the usual moral hazard problem that UI reduces incentives to return to a formal job. Yet, the associated efficiency cost is lower than in the U.S., and is lower in labor markets with higher informality within Brazil. This is because formal reemployment rates are lower to begin with where informality is higher, so that a larger share of workers would draw UI benefits absent any moral hazard. In sum, efficiency concerns may actually become more relevant as an economy formalizes.

The informal sector accounts for a larger share of employment in developing countries.[1] 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 jobs exacerbates the disincentives to work in the formal sector that are created by such programs. Despite this widespread view, however, the evidence behind it remains limited. Still relatively few papers credibly estimate the impact of social programs on formal employment in developing countries.[2] Moreover, 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

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[1] Informal workers – those who escape government monitoring – include informal employees and most self-employed workers in developing countries. 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] See, for instance, Gasparini et al. (2009); Azuara and Marinescu (2013); Bergolo and Cruces (2014, 2018); Bosch and Campos-Vasquez (2014); Camacho et al. (2014); Garganta and Gasparini (2015).

efficiency-equity or efficiency-insurance tradeoff. This is important because evidence of (dis-)incentive effects does not imply that the associated efficiency cost is relatively high, or that it is higher than in a context of lower informality.[3]

This paper addresses these limitations for the case of Unemployment Insurance (UI). We first adapt a canonical framework in the UI literature to a context of high informality, and use it to highlight the relevant measure of efficiency for the usual moral hazard problem with increases in UI benefit, i.e., that they distort incentives to return to a formal job. We then exploit rich administrative data on Brazilian UI beneficiaries and a regression discontinuity (RD) design to estimate the efficiency cost of an increase in potential UI duration. We also estimate how the efficiency cost varies across Brazilian labor markets with different degrees of informality. In so doing, we show that the efficiency cost of a typical social policy can actually be lower in labor markets with higher informality.

UI is an ideal program to study these issues. It requires the beneficiaries – displaced formal employees – to not be formally reemployed. It has been adopted or considered in a number of developing countries (Velásquez, 2010; Holzmann et al., 2012; Vodopivec, 2013). Moreover, international development agencies have emphatically pointed to the heightened moral hazard problem it supposedly creates in the presence of a large informal sector.[4] Finally, in contrast to other programs common in developing countries (e.g., conditional cash transfers), comparable UI programs exist and have been studied in developed countries, allowing us to benchmark our results against estimates from contexts of low informality.

We begin by adapting the partial-equilibrium framework in Chetty (2006, 2008), which has been used extensively in the context of developed countries to evaluate the tradeoff with increases in UI benefits between displaced workers' need for insurance and the efficiency cost from the usual moral hazard problem. Our extension highlights the sufficient statistics that capture this tradeoff when displaced workers have the possibility to work informally. The efficiency cost is captured by the ratio of a *behavioral effect* to a *mechanical effect*. The former measures the cost of increasing UI benefits 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 (i.e., "mechanically"). Welfare effects are positive if the ratio does not exceed the *value of insurance*, the difference in the marginal utility of $1 between mechanical beneficiaries and formal employees.

There are two main lessons from the theory. First, the behavioral effect only

[3]The above-cited papers do not use a theoretical framework to interpret their findings in terms of the efficiency cost of the policy that they study. Bergolo and Cruces (2018) is a recent exception, in which the authors study the efficiency cost of a conditional cash transfer program in Uruguay.

[4] *"Because checking benefit eligibility imposes large informational and institutional demands, particularly under abundant and diverse employment opportunities in the unobservable informal sector, the resulting weak monitoring would make the incentive problem of the standard UI system much worse"* (Robalino et al., 2009, page 2). See also, for instance, Acevedo et al. (2006) and Vodopivec (2013).

depends on the impact of behavioral responses on the paid UI duration and on the duration without a formal job. So, one does not need to separate non-employment and informality responses. Second, the efficiency cost is not necessarily higher in a context of high informality. The two main views on the prevalence of informality suggest that displaced 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 prefer to work informally for reasons unrelated to UI ("exit" view).[5] As a result, the mechanical effect will be larger because more workers will draw the increased UI benefits absent behavioral responses. The behavioral effect may also be smaller, as there will be "less room" to delay formal reemployment.

Next, we turn to the empirical analysis. In a first step, we estimate the efficiency cost of an increase in one dimension of UI benefits, the potential UI duration. We combine administrative data on the universe of formal employment and UI spells over several years in Brazil with an RD design around a tenure-based cutoff at which the potential UI duration increases from four to five months of UI.

We document clear evidence of behavioral responses to UI incentives. The hazard rates of formal reemployment increase sharply by more than 100% after month 4 since layoff for workers eligible for four months of UI, but only after month 5 for those eligible for five months of UI. However, the hazard rates are very low to begin with, and they remain relatively low even when workers are no longer eligible for UI benefits, e.g., about 50% of workers remain without a formal job 12 months after layoff. As a result, these behavioral responses do not imply a high efficiency cost. Specifically, we estimate that the one-month increase in potential UI duration at the RD cutoff leads to a large increase in the average paid UI duration (.861 month), but mostly because of a large mechanical effect. Behavioral responses only account for 14.6% of the overall increase in the average paid UI duration (.126 month). Behavioral responses also increase the average duration without a formal job by .389 month. This corresponds to an increase of 2.3% given that the duration is relatively high to begin with. All together, these results imply an efficiency cost of $.217 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 U.S., where the potential UI duration is comparable (slightly higher) but the informal sector is much smaller. The driving force behind this difference is that displaced formal workers return much slower to a formal job in Brazil, such that providing UI is more costly (i.e., more workers draw their full potential UI duration) but not necessarily in terms of efficiency (i.e., many of them would do so even absent UI incentives). Relatedly, if the marginal effect on the duration without a formal job (.389 month) is rather large in our setting, the implied elasticity (.092) is smaller than existing estimates from European countries and the U.S. (Schmieder and von Wachter, 2017).[6]

[5]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. Nowadays, these two views are considered complementary (Perry et al., 2007).

[6]Moreover, if we apply the same constant-hazard approximation used in Schmieder and von Wachter

In a second step, we show that the efficiency cost is also lower in labor markets with higher informality within Brazil by replicating the RD analysis in each of the 27 Brazilian states, separately. We find that the one-month increase in potential UI duration leads to a larger increase in the average paid UI duration in states with higher informality, and that this is entirely driven by a larger mechanical effect. Displaced workers return slower to a formal job in states with higher informality, even when they are no longer (or not) eligible for UI.[7] Moreover, the impacts of behavioral responses on the paid UI duration and on the duration without a formal job are in fact decreasing in informality rates.[8] These results are robust to only using variation in informality rates across states over time and to controlling for a rich set of worker characteristics. In sum, our findings indicate that efficiency concerns may become more relevant as an economy formalizes.

Our findings suggest shifting the policy debate from efficiency concerns to displaced workers' need for insurance in a context of high informality. Indeed, while the "exit" and "exclusion" views on informality are both consistent with a relatively low efficiency cost, they have opposite implications for the value of insurance. The 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). The administrative data do not allow us to study the extent to which workers actually work in informal jobs after layoff, or the extent to which informal jobs allow them to mitigate their income loss at layoff. In a last step, we use labor force surveys to shed some light on these questions.

The surveys allow us to estimate hazard rates of overall reemployment (including informal jobs) for a group of workers eligible for five months of UI,[9] which we can compare to hazard rates of formal reemployment for the same group in the administrative data. We estimate that 35.8% of workers remain non-employed by month 5 after layoff (this figure is comparable in the U.S.; Chetty, 2008), while 77.6% remain without a formal job. Therefore, many but not all UI beneficiaries likely work informally. Our estimates also imply that the sharp increase in formal reemployment after workers exhaust their UI benefits is likely driven by informality responses rather than non-employment responses: after month 5 since layoff, the share of workers leaving non-employment each month does not increase and it becomes smaller than the share finding a new formal job. Finally, the surveys allow us to estimate that household earnings fall by 45%-50% of their pre-layoff levels for workers who remain non-employed and that working informally only closes about half of that gap. All together, these findings suggest that the value of insurance may be sizable, particularly if other means of consumption-smoothing are more costly in developing countries (Chetty and Looney, 2006).

(2017) to compute the efficiency cost, our estimate of efficiency cost is also smaller than the estimates implied by the studies from European countries and the U.S. reviewed in this paper.

[7] We find the same pattern for workers who were not eligible for UI because they were fired for cause.

[8] The elasticity of the duration without a formal job is thus also decreasing in informality rates.

[9] Due to data limitations, it is not possible to replicate the RD analysis using the surveys (see Section V.A) or to study systematic differences across labor markets (the surveys only cover six large cities).

This paper contributes to the social insurance literature, which focuses on the context of richer countries (Chetty and Finkelstein, 2013). Schmieder et al. (2012) and Kroft and Notowidigdo (2016) study the efficiency cost of increases in UI benefits over the business cycle in Germany and the U.S. Consistent with our findings, the efficiency cost is lower in downturns when workers return slower to a formal job. Our paper differs in a key way: informality is low in these countries, but it is high in developing countries irrespective of the state of the business cycle.

This paper also contributes to a literature at the intersection of public and development economics, which focuses on tax policies.[10] A theoretical literature argues that efficiency concerns 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 high, and may even be lower in labor markets with high informality. Moreover, because Brazil contains regions with very different levels of 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 policies in equilibrium-search models (e.g., Meghir et al., 2015). A feature of our approach is that the sufficient statistics that capture our tradeoff of interest are not specific to a particular model, but remain the same under a range of modelling assumptions (Chetty, 2006). 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.[11] Third, this paper estimates how the impacts of a social insurance program vary with labor market informality.

The paper is structured as follows. Section I lays out the conceptual framework for our analysis. Section II presents our empirical setting. Section III estimates the average impacts and efficiency cost of an increase in potential UI duration using administrative data. Section IV studies how these vary across labor markets with different degrees of informality. Section V sheds additional light on our results using complementary evidence from survey data. Section VI concludes.

I. Conceptual framework: UI in labor markets with high informality

In this section, we first provide some background on labor markets with high informality. We then highlight the sufficient statistics formulas that capture the

[10] See, for instance, Best et al. (2015), Carillo et al. (2017), or Naritomi (2019).

[11] In addition to previously cited studies, we are aware of three relevant UI papers. González-Rozada et al. (2011) and Amarante et al. (2013) estimate the impact of UI policies on some labor market outcomes in Latin American countries without studying their efficiency cost. Gonzalez-Rozada and Ruffo (2016) study the impact of a UI policy in Argentina and its efficiency cost. However, they do not compare the correct measure of efficiency cost in their context and in contexts of lower informality (see the discussion in Appendix A, where we also discuss earlier working papers on UI in Brazil).

usual efficiency-insurance tradeoff with UI and how the presence of a large informal sector affects these formulas, motivating the empirical analysis in the next sections. We provide a more detailed presentation of the theory in Appendix B.

A. Labor markets with high informality

Labor markets in developing countries, including all Latin American countries, feature a large share of both formal employees and informal workers. Formal employees 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 informal (i.e., unregistered) employees in non-complying firms (a firm may hire both formal and informal employees) and most self-employed workers.

For instance, Figure 1 documents the prevalence of informal workers in Brazil over our main period of analysis (2005-2009). Every worker has a working card in Brazil. When an employer signs her working card, which is mandatory, her hiring is reported to the government, and she becomes a formal employee. Yet, hiring an employee formally is costly,[12] and although firms face penalties for not complying with labor laws (including for hiring workers informally), the risk of detection is relatively low. As a result, informality is high even in non-farm employment.[13] Figure 1a shows that the share of private-sector formal employees is about the same as the share of informal employees and self-employed workers combined. Brazil's average informality rate is close to the average among Latin American countries. However, there is a lot of heterogeneity across labor markets in Brazil. Figure 1b shows that the shares of private-sector formal employees and informal workers vary greatly across Brazilian states, and strongly correlate with income per capita, as they do across countries (Perry et al., 2007).

There are two main views on such prevalence of informal workers in developing countries (Perry et al., 2007). In the "exit" view, workers are 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 often transit between formal and informal jobs in Latin American countries (Bosch and Maloney, 2010). This contradicts early versions of the exclusion view, which considered formal and informal sectors as segmented, but it remains consistent with formal jobs being more difficult to find (Meghir et al., 2015). Surveys also show that earnings are on average higher in the formal sector, although there is a lot of heterogeneity (Botelho and Ponczek, 2011). Today, these two views are recognized as complementary.

[12] Payroll taxes include 20% for Social Security and 7.8% (on average) for funding an array of programs. Formal employees are also entitled to the minimum wage, a 13^{th} monthly wage, 30 days of paid leave per year, a seniority account (*FGTS*) in which employers must deposit 8% of their wage each month, etc.

[13] We exclude farm workers because a negligible share of them is formal and draws UI.

B. Baily-Chetty formulas in a context of high informality

Baily (1978), Chetty (2006), and a series of subsequent papers have derived sufficient statistics formulas to evaluate the usual efficiency-insurance tradeoff with UI in the context of richer countries, where informality is less prevalent.

Consider a typical framework in this literature, in which a representative worker lives for T periods, is laid off at the start of period 0, and is eligible for UI benefit level b for a maximum of P periods (Chetty, 2008). When non-employed, she can search for a new job; search efforts are costly but they determine her reemployment probability. When reemployed, she earns a net wage of $w - \tau$ in each period until T, where τ is a tax financing the UI system. In this framework, the worker must choose optimal levels of search effort – and possibly of other variables (e.g., savings, reservation wages) – in each period t after layoff. The moral hazard problem is that increases in UI benefits reduce job-search incentives.

We make two changes to adapt this framework to a context of high informality. First, informal workers are not covered by UI and do not pay payroll taxes. So, the representative worker must be laid off from a formal job and the UI tax can only be levied once the worker is reemployed in a formal job. Second, the government cannot easily identify UI beneficiaries working informally, so workers can draw UI benefits as long as they are not reemployed formally. This alters the nature of the moral hazard problem. Increasing UI benefits may now also increase incentives to work informally (potentially reducing search efforts for a formal job) or to hide a job that would otherwise be formal (i.e., by staying on an informal payroll). In both cases, workers might be willing to trade off utility gains from remaining eligible for UI against utility losses from informality (e.g., lower wages or hiding costs). We keep all the 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.[14] We come back to these assumptions below.

As with other sufficient statistics formulas in the literature, we can derive the welfare effect of changes in UI benefits in this framework from the mechanical and behavioral effects of the policy. Consider, for instance, a policy that increases the potential UI duration (dP), i.e., the policy variation that we study in the empirical analysis.[15] First, there is a *mechanical* effect. Workers who would have remained without a formal job after exhausting their UI benefits would draw benefits for an additional period without changing their behavior (they are *mechanical* beneficiaries). The mechanical effect increases UI costs by $b \times dD^B/dP|_M = b \times S_P$, where S_t is the survival rate without a formal job and $D^B \equiv \sum_{t=0}^{P-1} S_t$ is the average paid UI duration. This is illustrated in Figure 2a, which displays survival rates without a formal job for workers eligible for four

[14]So, the worker also takes into account, for instance, any relevant non-wage benefits of formal jobs.

[15]As in Schmieder et al. (2012), a marginal change in potential UI duration corresponds to a marginal change in b_P, the benefit amount after the end of the pre-existing potential UI duration, times b.

months of UI. These survival rates come from simulations of a dynamic model of job-search à la Chetty (2008), in which workers who are not yet formally reemployed can search for a formal job at a convex search cost (as in Chetty, 2008), but also earn additional income informally at a convex effort cost.[16]

Second, there is a *behavioral effect*. Workers may change their behaviors in response to the policy. 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 the potential UI duration reduces incentives to be formally reemployed. Survival rates without a formal job may then increase, increasing the paid UI duration and UI costs by $b \times dD^B/dP|_B = b \times \sum_{t=0}^{P} dS_t/dP$. This is illustrated in Figure 2a, which also displays survival rates without a formal job from the same model for workers eligible for five month of UI. If workers take longer to return to a formal job, the time spent formally employed subsequently (paying the UI tax) may also decrease, reducing UI revenues by $\tau \times dD^{NF}/dP$, with $D^{NF} \equiv \sum_{t=0}^{T} S_t$ the average duration without a formal job.

The mechanical effect constitutes a transfer from taxpayers to mechanical beneficiaries. So, its effect on welfare is: $(u'^P - u'^F) \times b \times S_P$, where u'^P and u'^F are the average marginal utilities of \$1 for mechanical beneficiaries and when formally employed, respectively. By contrast, the behavioral effect only constitutes a cost that must be paid for, so its welfare effect is: $-u'^F \times \left[b \times dD^B/dP|_B + \tau \times dD^{NF}/dP\right]$. Putting everything together, we obtain a formula for the overall welfare effect:

$$(1) \qquad \frac{dW/dP}{b \times S_P \times u'^F} = \left(\frac{u'^P - u'^F}{u'^F}\right) - \left(\frac{dD^B/dP|_B}{S_P} + \frac{\tau}{b} \times \frac{dD^{NF}/dP}{S_P}\right)$$

where we divide by the mechanical effect to measure welfare per unit impact on targeted beneficiaries, and by the average marginal utility of \$1 when formally employed to express welfare in a money metric.

The first term in equation (1) is the *value of insurance*, the welfare gain from transferring \$1 from formal employees to mechanical beneficiaries. The second term is the *efficiency cost*, the resources lost per \$1 reaching mechanical beneficiaries. That the efficiency cost is captured by the ratio of the behavioral effect to the mechanical effect is a general result that applies to changes in other policy instruments, e.g., to changes in the UI benefit level (see Appendix B2).[17]

Equation (1) shows that the efficiency cost can be evaluated by estimating a few sufficient statistics: the mechanical effect, the impact of behavioral responses on the average paid UI duration, and the impact of behavioral responses on the average duration without a formal job. We estimate these statistics and evaluate

[16] As in Chetty (2008), workers also decide how much to save in each period given their employment status (they cannot borrow against their future income). The model is presented in Appendix B, together with details on the simulations used in this section.

[17] Schmieder et al. (2012) derive their formula for a similar policy in a different way. However, more recently, the same authors recognized the generality of the ratio of the behavioral effect to the mechanical effect as a measure of the efficiency cost of changes in UI benefits (Schmieder and von Wachter, 2017).

the efficiency cost of an increase in potential UI duration using administrative data in Sections III and IV. Suppose that we estimate an efficiency cost of $.5 per $1 reaching mechanical beneficiaries. The welfare effect would be positive if the average marginal utility of $1 were at least 50% higher for mechanical beneficiaries than when formally employed. The focus of the paper is on the efficiency cost, but we also shed some light on the value of insurance using survey data in Section V (estimating it is always more challenging; e.g., Landais and Spinnewijn, 2019).

C. *The role of informal work opportunities*

Equation (1) shows that it is not necessary to estimate the effect of increases in UI benefits on all margins of behaviors to evaluate the efficiency cost. Specifically, it is not necessary to know whether workers work informally or remain non-employed when they delay formal reemployment.[18] This is an application of the result in Chetty (2006). Workers internalize all consequences of their choices except on the UI budget, so behavioral responses – including informality responses – only have first-order welfare effects through their impact on the UI budget. A corollary is that the formula in equation (1) applies even without informality: one would simply substitute "formally employed" by "employed" and "without a formal job" by "in non-employment" in the framework of Chetty (2008).

Why does informality matter then? First, the magnitude of the sufficient statistics capturing the efficiency cost may differ in labor markets with high informality.

On the one hand, the two main views on the prevalence of informality suggest that displaced workers may return slower to a formal job in those labor markets, irrespective of UI benefits. This is illustrated in Figure 2b. It first displays survival rates without a formal job *in absence of UI benefits* using the same "baseline" model as for Figure 2a. Then, it shows that survival rates decrease slower if we increase the cost of finding a formal job ($\simeq$ "exclusion" view) or decrease the cost of working informally ($\simeq$ "exit" view) in this model.[19] A key implication of workers returning slower to a formal job is that the mechanical effect of increases in UI benefits will be larger. This is illustrated in Figure 2c and Figure 2d, which replicate Figure 2a with the higher cost of finding a formal job and the lower cost of working informally used in Figure 2b, respectively. As shown in equation (1), a larger mechanical effect will decrease the efficiency cost (everything else equal).

On the other hand, it may be easier to respond to UI incentives and to delay formal reemployment when UI beneficiaries have an additional margin of behavioral responses, i.e., working informally. This is a typical concern with social programs that require beneficiaries to not be formally employed (Levy, 2008).

[18] The measure of efficiency in equation (1) is very much in the spirit of the literature arguing that workers choosing to work informally instead of formally may impose large efficiency losses (Levy, 2008): it is equivalent to assuming that the efficiency losses are as large as if workers had chosen to remain idle.

[19] Survival rates decrease faster in Figure 2b than in Figure 2a using the baseline model as there are no UI benefits anymore. We calibrated the change in model parameters such that survival rates are similar with the higher cost of finding a formal job and the lower cost of working informally in Figure 2b.

Formal reemployment rates may thus be severely reduced once workers become eligible for longer UI benefits. At the same time, if workers are already returning slower to a formal job in a context of high informality, there is "less room" for survival rates to increase in response to UI incentives, as shown in Figures 2c and 2d.[20] Therefore, the behavioral effect may not be larger in such a context. The behavioral effect on the average paid UI duration is in fact smaller in Figures 2c and 2d than in Figure 2a, which contributes to reducing the efficiency cost.

The model used in Figure 2 is rather simple,[21] and different modeling assumption may lead to different predictions regarding how the behavioral effect varies with informality. Yet, it illustrates that the efficiency cost is not necessarily higher in contexts of higher informality, motivating our empirical analysis. Moreover, different modeling assumptions would lead to the same sufficient statistics as long as workers internalize all consequences of their choices except on the UI budget.

Second, the value of insurance may also differ in labor markets with higher informality. The "exit" and "exclusion" views on informality have similar predictions regarding the efficiency cost in Figure 2, but opposite implications for the value of insurance. It will be lower if working informally is easier and informal jobs are close substitutes for formal jobs. It will be higher if finding a formal job is more difficult and informal jobs provide poor means of self-insurance.[22] Thus, the size of the value of insurance in contexts of high informality is an empirical question too. We make a first step in addressing this question in Section V.

D. Informality and the assumptions of the Baily-Chetty framework

Before turning to the empirics, we review important assumptions of the Baily-Chetty framework that we maintain in our analysis for comparison purposes.

First, we abstract from any impact of UI on layoff decisions. This is because this distortion can be tackled by the experience-rating of UI benefits. In practice, UI is never perfectly experience-rated and this has been shown to increase layoffs in developed countries. The same problem may be important in a context of high informality, as firms may report laying off workers eligible for UI but keep them on an informal payroll. We show in Appendix D1 that UI seems to distort layoff decisions at low tenure levels in Brazil. However, existing institutions (e.g., high firing costs) appear to prevent such responses for workers with higher tenure.[23]

Second, we assume that the incidence of taxes and benefits falls on formal employees and beneficiaries, respectively. These are reasonable assumptions at

[20] Given the survival rates when workers are eligible for four months of UI, the behavioral effect on the paid UI duration can be at most $5 - 3.35 - .45 = 1.2$ months in Figure 2a. This upper bound is much smaller in Figure 2c and Figure 2d ($5 - 3.80 - .83 = .37$ month and $5 - 3.73 - .78 = .49$ month).

[21] For instance, it makes the strong assumption that working informally does not affect search costs, while working informally could, e.g., increase the cost of finding a formal job if there is a binding time constraint or decrease it if finding a formal job requires liquidity.

[22] The value of insurance may also be higher if other means of consumption smoothing, such as formal credit or savings, are more costly in developing countries (Chetty and Looney, 2006).

[23] Relatedly, we abstract from any impact of increases in UI benefits on the takeup of UI benefits (Kroft, 2008) because we do not find any impact on UI takeup in the empirical analysis.

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 when the minimum wage is not binding.[24] We verify that our results are not driven by workers earning close to the minimum wage in the empirical analysis.

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 (Lawson, 2017). Developing countries rely less on such taxes,[25] and it is not straightforward how decreases in formal employment would affect tax bases that they rely more on (e.g., consumption). Relatedly, a strand in the taxation literature argues that the efficiency cost of a tax increase is lower if behavioral responses are mostly coming from evasion responses rather than real responses (Chetty, 2009). The point is that evasion responses might have positive fiscal externalities on other components of the government budget (e.g., other tax bases, revenue from tax audits). The same argument implies that the efficiency cost of increases in UI benefits would be lower if behavioral responses are mostly coming from informality responses rather than non-employment responses.[26] Finally, even when considering the UI program in isolation, fiscal externalities may arise if increases in UI benefits affect formal reemployment wages and the UI tax is proportional to wages (Nekoei and Weber, 2017). We do not find any effect on formal wages in the empirical analysis.

Fourth, we abstract from general equilibrium effects. 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). Moreover, estimates of changes in formal employment based on variation in UI benefits across workers within a labor market may overestimate general-equilibrium estimates in the presence of search frictions (Landais et al., 2018). The challenge with estimating labor-market wide effects is that it requires variation in UI benefits across labor markets, which we do not have in Brazil. Finally, in general equilibrium, the efficiency cost may be different if decreases in formal employment come from increases in informality rather than from increases in non-employment. Increases in informality might lead to misallocation costs, i.e., making it harder for formal firms to compete (Ulyssea, 2018). By contrast, increases in non-employment might lead to worse demand effects, i.e., non-employed workers likely spending less in the economy than workers earning income informally.

In sum, the assumptions of the Baily-Chetty framework that we maintain in our analysis constitute a natural starting point and a useful benchmark. In particular, they do not appear to bias the comparison of our estimates of the efficiency cost in a context of higher informality with estimates from the literature in a predictable

[24] See for instance, Gruber (1997), Kugler and Kugler (2009), and Cruces et al. (2010).

[25] Most formal workers do not pay income taxes in Brazil. The effect of changes in formal employment on the government budget through payroll taxes is not straightforward as they are partly benefit taxes.

[26] This would be the case, e.g., if informal earnings are spent on taxed commodities or if an increase in informality increases the revenue from labor inspections (e.g., penalties from hiring employees informally).

way. Relaxing these assumptions will be an important avenue for future research.

II. Empirical setting: institutional details and data

We present here key institutional details for the empirical analysis and our data.

A. The Brazilian UI program

The Brazilian UI program worked as follows between 1994 and 2015, which includes our period of analysis. A worker laid off from a private-sector formal job with at least six months of tenure at layoff was eligible for UI benefits after a 30-day waiting period. The worker also needed at least 16 months between her layoff date and the layoff date of her last successful UI application. She had to apply in person for UI within 120 days of her layoff date. If she was deemed eligible, her UI benefits were automatically deposited every 30 days at a state bank (*Caixa*) as long as her name did not appear in a database (*CAGED*) where employers report new hirings monthly. The potential UI duration was a function of her accumulated tenure across all formal jobs in the 36 months prior to layoff. She was eligible for up to three, four, or five monthly UI payments if she had more than 6, 12, or 24 months of accumulated tenure, respectively. The benefit level depended on her average wage in the three months prior to layoff and ranged from 100% to 187% of the minimum wage (see the full schedule in Appendix C1). The replacement rate was thus very high at the bottom of the wage distribution (100% for workers who earned the minimum wage). Importantly, all the empirical results are robust to excluding beneficiaries with very high and very low replacement rates.

That the longest potential UI duration is five months in Brazil implies that we investigate the efficiency cost of increasing the potential UI duration from a relatively low starting point. This is the relevant case to consider for developing countries. Moreover, the potential UI duration is comparable in the U.S. (23.9 weeks on average),[27] allowing us to benchmark our results against U.S. estimates.

A departure from the conceptual 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 more interesting case conceptually. They are the beneficiaries of the program and UI aims at providing insurance, not redistribution. The incidence of a tax on formal workers is also more likely to fall on those workers 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 UI program is unlikely to invalidate this objective. A 2.8% payroll tax would be sufficient to fund it (UI costs/total

[27] Own calculations using data from www.dol.gov., excluding years with extended benefits.

eligible payroll $\simeq .028$) and it is unlikely that the composition of the formal labor force would be markedly different substituting such a tax for the existing one.[28]

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

B. Data

We provide a description of our data here; see Appendix C3 for more details.

The empirical analysis relies on two administrative datasets. First, we use a matched employee-employer dataset covering the universe of formal employees in Brazil (*RAIS*). Every year, all tax-registered firms must report all workers formally employed at any point during the previous year.[29] RAIS has data on tenure, age, gender, education, sector, establishment size and location, reason for separation, and between 2002 and 2010, hiring and separation dates for every job spell within a year. Additionally, it includes the average monthly wage over the spell and the December wage for spells that survive until the end of the year. Second, we use the *UI registry.* It has data on the application date, the amount, the month, and since 2005, the date for all UI payments. We can match workers between these two datasets through a unique ID number, allowing us to measure the paid UI duration and the duration without a formal job for all displaced formal employees. To provide some perspective, there were about 40,300,000 formal employees and 625,650 new UI beneficiaries per 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 1. 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 study the extent to which displaced formal employees are working informally while they remain without a formal job and the extent to which informal jobs allow them to mitigate their earnings loss at layoff.

[28]With a sales tax, u'^F in equation (1) becomes the marginal utility of those bearing its incidence. We discuss in Appendix C2 why the efficiency cost in the case of a .65% sales tax would unlikely be larger than the efficiency cost as calculated in the paper (assuming a payroll tax instead).

[29]Compliance is high because of large penalties when the data are late or incomplete. 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 in the next sections upward.

III. Main results: the efficiency cost of an increase in potential UI duration

In this section, we estimate the impacts and efficiency cost of increasing UI benefits by exploiting variation in potential UI duration at a tenure-based cutoff through a regression discontinuity (RD) design. We then compare our results with existing estimates from contexts of low informality. We also confirm our results using another empirical strategy in Appendix D8. We present the RD design in the paper because it allows us to study how our estimates vary across Brazilian labor markets with different degrees of informality in the next section.

A. Empirical strategy

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. In this section, we exploit the 24-month cutoff for workers who had a single formal job in the previous 36 months using the administrative data.

We focus on the 24-month cutoff because the usual conditions supporting the validity of an 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 Appendix D1 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.[30] It decreases discontinuously at 12 months of tenure, which is when firing costs also increase.

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

[30] This has been confirmed in a recent paper by Carvalho et al. (2018). The layoff density also increases discontinuously at another eligibility cutoff for such workers, when they reach 16 months between their layoff date and the layoff date of their last successful UI application (Gerard et al., 2020).

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. Moreover, rates of formal reemployment are comparable in the RD sample (see below), for all workers with more than 24 months of tenure at layoff, and for a random sample of all displaced formal workers over the same period (see Appendix D3).

Our main analysis sample includes all displaced workers in the administrative data, 18-54 years old, who had a single full-time private-sector formal job in the 36 months before layoff. We focus on layoffs that took place between 2005 and 2009 such that we have data on the exact separation, hiring, and UI payment dates, and we can follow all workers for at least one year after layoff. Those with less than 22 months (resp. more than 24 months) of tenure at layoff were eligible for four months (resp. five months) of UI. Those 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 layoffs (we discuss its composition below).

Our econometric specification 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 at layoff, 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 $f(T_i)$:

$$y_i = \alpha + \beta \times \mathbb{1}(T_i \geq 0) + f(T_i) + \epsilon_i, \tag{2}$$

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 using a "donut hole" approach, i.e., by excluding these workers from our regressions (we are left with 1,969,137 layoffs). 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.

In practice, we use the theoretically optimal edge kernel (Cheng et al., 1997) and a bandwidth of six months below 22 and above 24 months of tenure. As we show graphically below, a six-month bandwidth fits the data well. This is helpful because the empirical strategy relies on predicting the trend in the outcomes between 22 and 24 months using 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.

B. Validity checks

Figure 3 provides evidence supporting the validity of the RD design. Each panel plots the average of some selected variable by tenure level (.1-month bin) around the cutoff and reports the result from estimating the RD specification in equation (2) for this variable. 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 β, the difference between the two lines at the cutoff, is reported with its standard error (in parenthesis).

Figure 3a displays the share of observations in the sample by tenure level. There is no visible change in the distribution of the running variable around the cutoff.[31] The estimated β is small and insignificant. So, workers do not appear more likely to be laid off once they become eligible for a fifth month of UI. Figures 3b-3e also show that there is no change in the composition of the sample around the cutoff in terms of age, gender, education, and statutory UI replacement rates.[32] Point estimates are small and insignificant. Around the cutoff, men constitute about 58% of the sample; the average worker is about 30 years old, has about 9.2 years of education, and would receive a UI benefit level replacing about 79% of her wage if she took up UI after layoff. Finally, Figure 3f shows that the share of workers taking up UI is relatively high in our setting, around 86%, and is stable around the cutoff.[33] The estimated β is again small and insignificant. This result is not a validity check per se, but it indicates that any impact of the increase in potential UI duration will not be driven by a change in UI takeup, which is a margin of behavior that is absent from the conceptual framework in Chetty (2008).[34]

C. Key patterns behind our results

Before turning to the RD results, Figure 4 presents key patterns of UI collection and formal reemployment in the RD sample, which helps connect the RD results to the conceptual framework in Section I. Since we do not find any impact on UI takeup in Figure 3, we focus on UI takers in the rest of the empirical analysis.

Figure 4a first displays the share drawing UI in each month since layoff for UI takers eligible for four vs. five months of UI in the RD sample (1,704,333 layoffs). Nobody draws UI in month 0 after layoff, because of the 30-day waiting period. The share drawing UI increases to 65%-68% in month 1 after layoff and reaches 92%-95% in months 2-4 after layoff. So, it is not the case that all UI takers

[31]The systematic pattern across tenure levels within a month comes from the fact that workers are more likely to be hired or laid off at specific points within a month (e.g., the end or the start of a month).

[32]There is also no change in wage, sector of activity, or firm size around the cutoff (see Appendix D2).

[33]Using the layoff, hiring, UI application, and UI payment dates, we define a worker as taking up UI (resp. drawing UI) in a given month if she applies within 120 days of layoff and draws her first UI payment (resp. any UI payment) in that month before being formally reemployed.

[34]Allowing for endogenous UI takeup in this framework is straightforward (see, e.g., Kroft, 2008).

take up UI in their first month of eligibility. The share drawing UI is similar for workers eligible for four and five months of UI in months 1-4, but it evolves very differently once workers start exhausting their UI benefits. It drops sharply after month 4 since layoff for those eligible for four months of UI, but only after month 5 for those eligible for five months of UI (given the delayed UI takeup, the share drawing UI continues to decrease over the next 2-3 months). The pattern of UI collection in Figure 4a implies that most UI takers exhaust their benefits: exhaustion rates are 90.3% and 87.2% for workers eligible for four and five months of UI, respectively. In comparison, exhaustion rates were 35.6% over the same period in the U.S., where the potential UI duration was only slightly higher.[35]

Figure 4a also displays the hazard rate of formal reemployment in each month since layoff for the same workers.[36] Exhaustion rates are very high in our setting because the hazard rate is very low in the first few months after layoff (it is nil in the first month after layoff by construction for UI takers). The hazard rate increases sharply, by more than 100%, after workers start exhausting their UI benefits (i.e., in months 4 and 5 since layoff for those eligible for four and five months of UI, respectively), peaking in the following month. This pattern suggests clear behavioral responses to UI incentives. It is also particularly striking in our setting: the relative increase in hazard rates after UI exhaustion is not as severe in developed countries (Card et al., 2007).[37] We show in Appendix D3 that the increase in hazard rates after month 5 for workers eligible for five months of UI is even sharper in our setting if we focus on workers taking up UI in month 1 after layoff, who actually exhaust their UI benefits in month 5.[38]

As discussed in Section I.C, however, the fact that formal reemployment rates are severely reduced when workers are eligible for longer UI benefits does not imply that the efficiency cost of increasing the potential UI duration will be high. This is best seen in Figure 4b, which displays the survival rate without a formal job in each month since layoff for the same workers. The very low hazard rates in the first few months after layoff imply that 94%-95% of UI takers are still without a formal job in month 4 after layoff. Survival rates decrease faster after month 4 for workers eligible for four months of UI, but only after month 5 for those eligible for five months of UI. As a result, survival rates remain higher for a few months in the latter group. Yet, the behavioral effect on the paid UI duration implied by this pattern would be small, especially compared to the mechanical effect. This is because formal reemployment rates remain low even after UI exhaustion, such that most workers would draw additional months of UI mechanically. In fact, 48%-51% of workers are still without a formal job 12 months after layoff.

As we show in Appendix D3, the finding that workers return slowly to a formal

[35]Own calculations using data from www.dol.gov (excluding years with extended benefits).

[36]We construct the survival rate at the start of each month since layoff using the layoff and hiring dates. The hazard rate is the difference between survival rates at the start of that month and at the start of the following month divided by the survival rate at the start of the month.

[37]We note that van Ours and Vodopivec (2006) find a sizeable spike at UI exhaustion in Slovenia.

[38]We show in Appendix D4 that this pattern is not driven by workers returning to the same employer.

job even after UI exhaustion is not specific to the RD sample. It is also unlikely due to some long-term effect of UI eligibility in earlier months. The survival rate without a formal job 12 months after separation is similar for workers who were not eligible for UI because they were fired for cause (see Appendix D3).[39]

D. Main results

We now turn to the main RD results, which are presented in Figure 5. It displays similar RD graphs as in Figure 3 for a series of key outcome variables.

We begin by decomposing the overall effect on the average paid UI duration into behavioral and mechanical effects. For this purpose, Figure 5a displays the average of two variables by tenure level around the cutoff. The first one is the actual paid UI duration: $D_i^B \equiv \sum_{j=1}^5 \mathbb{1}(DrawingUI_{i,j} = 1)$, where $DrawingUI_{i,j} = 1$ indicates that worker i drew a j^{th} month of UI before being formally reemployed. The change in the average of this variable at the cutoff captures the overall effect on the average paid UI duration. To separate behavioral and mechanical effects, however, we must also measure the paid UI duration that would prevail absent behavioral responses to the increase in potential UI duration. This is captured by the second variable, which we label the "counterfactual" paid UI duration:

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

where $NotFormal_{i,4,1} = 1$ indicates that worker i remains without a formal job one month after drawing her fourth month of UI. With perfect assignment and compliance, actual and counterfactual paid UI durations are equal for workers eligible for *five* months of UI ($\widetilde{D_i^B} = D_i^B$).[40] The averages of these two variables are similar above the cutoff in Figure 5a, indicating that assignment and compliance issues are minimal. In that case, the difference between the averages of the actual and counterfactual paid UI durations for workers eligible for *four* months of UI captures the mechanical effect (i.e., the share who would draw a fifth month of UI without changing their behavior). Moreover, the change in the average counterfactual paid UI duration at the cutoff captures the behavioral effect.[41]

Figure 5a shows that the average paid UI duration is around 3.88 months for workers with less than 22 months of tenure at layoff. It increases between 22 and 24 months of tenure, as an increasing share of workers were eligible for a fifth

[39] Hazard rates of formal reemployment decrease monotonically in the months after layoff for these workers, which is consistent with the fact that they were not eligible for UI.

[40] All those remaining without a formal job after drawing their fourth month of UI would draw a fifth month of UI, so we have: $\mathbb{1}(DrawingUI_{i,4} = 1) \times \mathbb{1}(NotFormal_{i,4,1} = 1) = \mathbb{1}(DrawingUI_{i,5} = 1)$.

[41] The overall effect is $E[D_i^B|P = 5] - E[D_i^B|P = 4]$. The mechanical effect is $E[\mathbb{1}(DrawingUI_{i,4} = 1) \times \mathbb{1}(NotFormal_{i,4,1} = 1)|P = 4] = E[\widetilde{D_i^B}|P = 4] - E[D_i^B|P = 4]$. So their difference, which captures the behavioral effect, is $E[D_i^B|P = 5] - E[\widetilde{D_i^B}|P = 4] = E[\widetilde{D_i^B}|P = 5] - E[\widetilde{D_i^B}|P = 4]$.

month of UI,[42] and reaches around 4.74 months above the cutoff. The estimated overall effect on the average paid UI duration is thus large, at $\beta = .861$ month, and it is precisely estimated. However, it is mostly due to a mechanical effect. The average counterfactual paid UI duration is around 4.62 months for workers eligible for four months of UI, indicating that most of them would draw a fifth month of UI mechanically. The average counterfactual paid UI duration starts increasing at 22 months of tenure and becomes indistinguishable from the average paid UI duration above the cutoff. The estimated behavioral effect on the paid UI duration is $\beta = .126$ month and it is also precisely estimated. Together, these two estimates imply a mechanical effect of $.861 - .126 = .735$ month.

Next, we estimate the behavioral effect on the duration without a formal job. Figure 5b displays the average duration censored at one year after layoff, which we can construct for all workers in the RD sample. It decreases with tenure at layoff, holding fixed workers' potential UI duration, i.e., below 22 months and above 24 months of tenure. However, it increases between 22 and 24 months of tenure, as an increasing share of workers were eligible for a fifth month of UI. We obtain a precise estimate for the implied delay in formal reemployment of $\beta = .29$ month. Yet, this is an underestimate because the survival rate without a formal job is still significantly higher above the cutoff one year after layoff (see Appendix D5). By contrast, Figure 5c shows that there is no difference in survival rates at the cutoff anymore three years after layoff (the sample excludes layoffs occurring after 2007). The impact on the average duration without a formal job censored at three years after layoff, which we display in Figure 5d, thus captures the behavioral effect in full. We obtain a statistically significant estimate of $\beta = .389$ month. This corresponds to an increase of 2.3% given the average duration of 16.87 months at the cutoff if eligible for four months of UI. The elasticity of the duration without a formal job with respect to the potential UI duration is thus $.023/(1/4) = .092$.

We can now evaluate the efficiency cost of the increase in potential UI duration. The only statistic that we have not yet estimated is the ratio τ/b in equation (1), which scales down the behavioral effect on the average duration without a formal job. Using the UI budget constraint: $\tau \times (T - D^{NF}) = b \times D^{B}$, we obtain $\tau/b = D^{B}/(T - D^{NF})$, which corresponds to the average paid UI duration per period of formal employment financing the UI system. Chetty (2008) and Schmieder et al. (2012) 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 formal employee between 2005 and 2009 following Landais (2015). We obtain an average ratio of .086 (or 8.6 UI beneficiaries per 100 formal employees).[43] The resulting efficiency cost amounts to $(.126 + .086 \times .389)/.735 = .217$ (with a standard error of .034

[42] 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.

[43] For a flat payroll tax rate and a fixed UI replacement rate, τ/b is the ratio of the payroll tax rate to the UI replacement rate (Schmieder and von Wachter, 2017). Using the average UI replacement rate in the RD sample (79%), a value of $\tau/b = .086$ is thus equivalent to assuming a payroll tax rate of 6.8%.

obtained by the delta method) or \$.217 per \$1 reaching mechanical beneficiaries.

Finally, Figures 5e and 5f use the same sample as in Figures 5c and 5d to display the share of workers formally employed and their average monthly wage in December three years after layoff (December wages are better measured in the data). They show that increasing the potential UI duration does not appear to improve outcomes once reemployed in a formal job: workers are no more likely to be employed and do not earn higher wages (estimates are small and insignificant).

E. Robustness checks

We present robustness checks in Table 1. We first reproduce the estimates in Figure 5 that are needed to evaluate the efficiency cost, i.e., the overall effect and the behavioral effect on the average paid UI duration as well as the behavioral effect on the average duration without a formal job. We then show that these results are similar if we include layoffs since 2002 (predicting UI payment dates within a month prior to 2005; see Appendix D6), if we use a smaller (4-month) bandwidth around the cutoff, if we exclude layoffs after 2007 (as for the duration without a formal job), if we include a rich set of individual controls,[44] or if we exclude workers with very high (above 80%) and very low (below 20%) statutory UI replacement rates.[45] The estimates of efficiency cost from these robustness checks range from \$.166 to \$.208 per \$1 reaching mechanical beneficiaries.

We further confirm our results by using the other source of quasi-experimental variation in potential UI duration in Brazil, namely temporary extensions of UI benefits (see Appendix D8). We exploit a policy 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-differences strategy comparing workers laid off in December vs. November 2008 from eligible vs. ineligible sector-state pairs. The efficiency cost is \$.168 per \$1 with this alternative empirical strategy.

F. Comparison with estimates from contexts of low informality

Our estimate of the efficiency cost 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 the average paid UI duration that is larger than the mechanical effect using data from the U.S. This implies an efficiency cost above \$1 per \$1 reaching mechanical beneficiaries, not even accounting for changes in

[44] Specifically, we control for year fixed effects, calendar month fixed effects for the month of hiring and the month of separation, state fixed effects, fourth-order polynomials in log wage and age (demeaned), five education categories (incomplete primary school, completed primary school, completed middle school, completed high school, completed college), four sectors of activity in the lost job (construction, industry, commerce, services), gender, ten firm size categories (as coded in RAIS), as well as the share of formal employees, the share of informal workers, the share of unemployed workers, and the share of workers in any other employment status in the state and year of layoff (calculated using PNAD).

[45] We use layoffs since 2002 for all these robustness checks to maximize sample size. The only relevant difference is that the impact on the duration without a formal job is smaller with the 4-month bandwidth. Estimates of behavioral effects are even smaller with smaller bandwidths (see Appendix D7).

the duration without a formal job. 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 above our cutoff (five months). Yet, informality was limited in the U.S. in the late 1970s - early 1980s, the period covered by his sample. We estimate a larger marginal effect on the average paid UI duration (.86 vs. .2-.4). However, the efficiency cost is higher in his case because the mechanical effect is smaller: only 11%-18% of UI takers exhaust their UI benefits. Landais (2015) concludes that one half to two-thirds of the increase in the average paid UI duration is due to behavioral responses in his setting. This figure is only .126/.861 = 14.6% in our case.

Making comparisons with other studies from countries with low informality is more challenging because existing studies rarely provide all the estimates necessary to compute the efficiency cost (and the potential UI duration is often longer). Nevertheless, the literature review in Schmieder and von Wachter (2017) is useful for this purpose. The authors compare estimates from Europe and the U.S. of the marginal effect of increasing the potential UI duration on the non-employment duration, and of the associated elasticity. The non-employment duration is equivalent to the duration without a formal job when all jobs are assumed to be formal, as typically done in these studies. The authors also compute an approximation of the efficiency cost for each of these studies under the assumption that the hazard rate of (formal) reemployment is constant after layoff. This assumption is rather strong: the hazard rate is not constant in our setting and is rarely constant in richer countries neither. However, we can compute the efficiency cost under the same assumption for comparison purposes. In so doing, we obtain an efficiency cost of .065, which is less than a third of our actual estimate (.217). Yet, this comparison reinforces our conclusion that the efficiency cost is relatively low in our setting. The mean efficiency cost in Schmieder and von Wachter (2017) is .58; the median is .52; and the 25th and 75th percentiles are .14 and .94, respectively.[46] Relatedly, if the marginal effect on the duration without a formal job is rather large in our setting (.389 month),[47] the implied elasticity is relatively small (.092). The mean elasticity in Schmieder and von Wachter (2017) is .33; the median is .32, and the 25th and 75th percentiles are .13 and .43.

IV. Heterogeneity: efficiency cost and the size of the informal sector

We showed that the average efficiency cost of an increase in potential UI duration is not necessarily higher in Brazil than in richer countries, despite pervasive informality. We provide here a complementary analysis by estimating how the efficiency cost varies across Brazilian labor markets with different informality rates.

[46] Moreover, the estimates in Schmieder and von Wachter (2017) assume a 3% payroll tax rate, while we are assuming a payroll tax rate more than twice as high (6.8%; see footnote 43).

[47] The mean marginal effect is .22; the median is .2, and the 25th and 75th percentiles are .11 and .31.

This analysis relates to a literature that correlates the effects of changes in UI benefits with unemployment rates in richer countries. Such correlations do not imply causal relationships as unemployment rates are not policy parameters that can be modified *ceteris paribus*. However, a higher unemployment rate may indicate that the cost of finding a job in a given labor market is higher. Accordingly, Schmieder et al. (2012) and Kroft and Notowidigdo (2016) find that the efficiency cost of increasing UI benefits decreases with unemployment rates. Similarly, as we discussed in Section I.C, higher informality rates may not only indicate labor markets in which it may be easier for displaced workers to respond to UI incentives, but also labor markets in which it may be harder for them to find a formal job or in which more of them may choose to work informally irrespective of UI benefits. As a result, the efficiency cost may decrease with informality rates.

A. Empirical strategy and results

Our main empirical strategy consists in estimating the same RD specification as in Figure 5 for each of the 27 Brazilian states separately, and then regressing these estimates on state-level informality rates, weighting them by the inverse of their standard error squared.[48] We restrict attention again to UI takers, as there is no correlation between UI takeup and informality rates (see Appendix E2).

First, we consider the RD specification for the paid UI duration. Figure 6a displays the average paid UI duration at the cutoff for workers eligible for four months of UI, i.e., the estimated constant (α) in our specification. It is large in every state, between 3.81 and 3.96 months, and it is increasing in informality rates. Providing UI is thus more costly in labor markets with higher informality, which is a finding that is not specific to the RD sample (see Appendix E1). Accordingly, Figure 6b shows that the overall effect on the average paid UI duration (i.e., the estimated β) is large in every state and is increasing in informality rates (the slope is significant if we include layoffs since 2002; see Appendix E3).

Second, we consider the RD specification for the counterfactual paid UI duration. Although the overall effect is increasing in informality rates, Figure 6c shows that the behavioral effect on the average paid UI duration (i.e., the estimated β) is in fact *decreasing* in informality rates. Therefore, it must be that the mechanical effect is *increasing* in informality rates. This is shown in Figure 6d.[49] The estimated slopes imply that the behavioral effect decreases from .144 month to .093 month over our range of informality rates, while the mechanical effect increases from .71 month to .79 month. These findings are thus consistent with the predictions from increasing the cost of finding a formal job or decreasing the cost of working informally in the illustrative model used in Section I.C.

Finally, we consider the RD specification for the duration without a formal

[48]We assign to each observation in the RD sample the informality rate (share of informal workers in non-farm labor force) in the state and year of layoff, and then average informality rates within state.

[49]The estimated mechanical effect corresponds to the difference between the estimated constants in the RD specification for the counterfactual paid UI duration and for the actual paid UI duration.

job censored at three years after layoff. Figure 6e displays the average duration without a formal job at the cutoff for workers eligible for four months of UI (i.e.. the estimated α). Consistent with the pattern for the mechanical effect, it shows that displaced workers return slower to a formal job in labor markets with higher informality, even in absence of longer UI benefits. The estimated slope implies that the average duration increases from about 16 months to about 20 months over our range of informality rates. This pattern is again unlikely due to some long-term effect of UI eligibility in earlier months. We show in Appendix E1 that workers who were fired for cause and were not eligible for UI also return slower to a formal job in labor markets with higher informality. Moreover, Figure 6f shows that the behavioral effect on the average duration without a formal job is in fact decreasing in informality rates. The estimated slope is not significant in Figure 6f (estimates are noisy for small states), but it is significant if we censor the duration at one year after layoff (so we can use the whole RD sample; see Appendix E2) or if we extend the sample to include layoffs since 2002 (see Appendix E3).

Together, the patterns in Figures 6e and 6f imply that the elasticity of the duration without a formal job with respect to the potential UI duration is decreasing in informality rates. Moreover, the patterns in Figures 6c, 6d, and 6f imply that the efficiency cost is decreasing in informality rates, which is shown in Figure 7a. The efficiency cost is estimated to decrease by about 50% over our range of informality rates, from \$.25 to \$.13 per \$1 reaching mechanical beneficiaries.[50]

B. Robustness checks

Figures 7b-7f show that this conclusion holds for the same robustness checks as in Table 1. The slope is estimated more precisely if we include layoffs since 2002, and it remains negative if we use a 4-month bandwidth around the cutoff, if we exclude layoffs after 2007, if we include a rich set of individual controls,[51] or if we exclude workers with very high and very low statutory UI replacement rates.[52]

Additionally, we confirm our results through another regression analysis in Appendix E4. We estimate correlations with informality rates directly by using a variant of equation (2), in which all the right-hand-side variables are interacted with informality rates. This approach allows us to estimate correlations with informality rates holding unemployment rates constant (also interacting all the right-hand-side variables with unemployment rates). Informality and unemployment rates are often 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 low informality. This approach also allows us to use more disaggregated measures of the relevant informality rate,

[50]We use the average number of UI beneficiaries per formal employee (as in Section III.D) in each state between 2005 and 2009 (shown in Appendix E2) to approximate the ratio τ/b in equation (1).

[51]Given that we run separate regressions for each state, we do not control for state fixed effects or state-level shares of workers in different employment status (see footnote 44).

[52]We replicate Figure 6 for each of these robustness checks in Appendix E3.

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. Finally, it allows us to show that the correlations with informality rates in Figure 6 are not simply due to fixed differences across states. Results are qualitatively similar including both year and state fixed effects.

In sum, the results in this section reinforce that the efficiency cost of increases in UI benefits is not necessarily high or higher in labor markets with higher informality. The driving force behind this finding is that displaced formal workers return slower to a formal job, absent UI incentives, which leads to a larger mechanical effect and limits the implications of behavioral responses.

V. Complementary evidence using survey data

A relatively low efficiency cost, however, does not imply that the welfare effect is relatively high in Brazil. Indeed, as discussed in Section I.C, a low efficiency cost is consistent with a low marginal value of insurance if it is easy to find informal jobs and if 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 if informal jobs are imperfect means of self-insurance.

A limitation of the administrative data used so far is that we have no information on the labor status of a worker who is not formally employed. As a result, we are unable to show the extent to which displaced workers actually work in informal jobs after layoff or the extent to which these jobs allow them to mitigate their earnings loss at layoff. In this section, we then combine the administrative data with data from labor force surveys (*PME*) to study these two questions.[53]

A. Displaced formal workers' propensity to work informally after layoff

Methodology.– PME surveys have some limitations for studying the extent to which UI beneficiaries resort to informal jobs after layoff. The surveys only record information about the current job for employed workers and about the last job for non-employed workers, and do not record any information about UI.

First, it implies that we can only assess UI eligibility and potential UI duration for formal employees and non-employed workers with more than 24 months of tenure in the current job and at layoff, respectively.[54] These workers are eligible for five months of UI upon layoff. Second, it creates challenges to study displaced workers' reemployment outcomes in the months after layoff. If workers remain non-employed, we can identify them directly and measure both their UI eligibility and their non-employment duration in any monthly survey. PME surveys record

[53]PME only covers six metropolitan areas, so we cannot study heterogeneity with informality rates.

[54]Given the details of the UI program described in Section II.A, we are not able to determine the UI eligibility or the potential UI duration unambiguously for workers with lower tenure levels. For that reason, we cannot conduct the same RD analysis as in the previous sections using PME surveys.

the nature of their last job (i.e., formal employee), the nature of their job separation (i.e., layoff), their tenure at separation, and how many months they have been non-employed since separation. By contrast, if workers are reemployed, PME surveys do not record any information about previous jobs or non-employment spells. Moreover, workers are only surveyed for four consecutive months, eight months apart from each other. Thus, we are unable to identify many displaced formal employees who are reemployed in the first few months after layoff, or measure the time it took them to find a new job (formal or informal).[55]

These data limitations require us to combine survey and administrative data. Using the administrative data, we can measure formal reemployment rates – from being without a formal job to being formally employed the following month – in each month since layoff. Using PME, we can estimate overall reemployment rates – from being non-employed to being employed (formally or informally) the following month – in each month since layoff. By contrasting patterns of formal reemployment and overall reemployment for comparable samples of displaced workers, we can then infer the extent to which they are working informally after layoff.

We show the results from such an exercise in Figure 8. The dashed lines in Figure 8a display estimates of the hazard rate of overall reemployment in each month since layoff. We select all individuals recorded as non-employed in PME surveys, who were 18-54 years old, who had more than 24 months of tenure at layoff, and who were laid off from a full-time private-sector formal job between 2005 and 2009. We then restrict attention to those who were also interviewed in the next month and we estimate a piece-wise constant hazard function by maximum likelihood using information about the length of their non-employment spell and whether they are still recorded as non-employed in the next month.[56] 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 (the sample includes 19,904 observations from 12,327 workers contributing to the likelihood function). 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 at the worker level. Once we have estimated the hazard rates, it is straightforward to compute the survival rates in

[55]For instance, the 4-month panel structure implies that we can follow a worker formally employed in a given monthly survey (and laid off afterward) for a maximum of three consecutive months after layoff.

[56]Define λ_m, the daily hazard rate constant over month m since layoff, and $k(b)$ with $b \in [0, 30]$, the distribution of survey interviews over days within a month (in practice, they are evenly spread over a month, so we assume $k(b) = 1/30$). To be recorded as non-employed, an individual must survive b days without a job given that she survived m months. Define $d_{i,m}$ such that $d_{i,m} = 1$ if individual i non-employed since month m is recorded as employed in the next month, and $d_{i,m} = 0$ otherwise. The likelihood for a given observation is then:

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

non-employment and the shares finding a new job (i.e., survival rate × hazard rate) in each month since layoff, which we display in Figures 8b and 8c.

By contrast, the solid lines in Figures 8a-8c display the hazard rates of formal reemployment (as in Figure 4a), the survival rates without a formal job (as in Figure 4b), and the shares finding a new formal job using the administrative data. The sample is restricted to workers 18-54 years old who where laid off from a full-time private-sector formal job between 2005 and 2009 from the six metropolitan areas covered by PME and who had more than 24 months of tenure at layoff (the sample includes 3,393,055 layoffs).[57] We present formal reemployment patterns both using all workers and using the subsample of UI takers for the sake of comparability with the survey-based estimates (we cannot restrict attention to UI takers in PME surveys) and with the patterns in Figure 4, respectively.

Results.– Figure 8a shows that the hazard rates of formal reemployment are similar in this sample and in the RD sample for UI takers eligible for five months of UI. Hazard rates are initially very low, start increasing faster in month 5 after layoff, peak in the following month, but remain relatively low even after UI exhaustion (as in Figure 4a). As a result, Figure 8b shows that more than 50% of UI takers are still without a formal job 10 months after layoff (as in Figure 4b). Figure 8a also shows that the hazard rates of formal reemployment are initially higher in the overall sample, which includes workers who do not take up UI. In fact, 12% of the overall sample find a new formal job in the first two months after layoff, when hazard rates are essentially nil among UI takers (the UI takeup rate is 80.6% in the overall sample). In the following months, hazard rates of formal reemployment are very similar whether we consider all workers or only UI takers.

Most importantly, Figure 8a shows that the estimated hazard rates of overall reemployment are much higher than the hazard rates of formal reemployment, particularly in the first few months after layoff. In fact, Figure 8c shows that the estimated share of workers finding a new job exceeds the share of workers finding a new formal job in each of the first 5 months after layoff. As a result, while 77.6% of the sample remains without a formal job at the start of month 5 after layoff, Figure 8b shows that only 35.8% remains non-employed (this figure is comparable in the U.S.; see Chetty, 2008). Many displaced formal workers thus work informally after layoff, including many UI beneficiaries (e.g., 68.8% of the overall sample is still drawing UI in month 5 after layoff). Nevertheless, it is not the case that all those who remain without a formal job are working informally.

Finally, Figure 8c suggests that informality responses are key drivers of the behavioral responses to UI incentives. The estimated share of displaced formal workers finding a new job does not increase after UI exhaustion. In fact, the share of the sample finding a new formal job in each month becomes larger than the share finding any new job exactly around the time that workers exhaust

[57]The metropolitan areas covered by PME are Belo Horizonte, Porto Alegre, Rio de Janeiro, Recife, Salvador, and São Paulo. Workers with more than 24 months of tenure at layoff accounted for 30% of displaced formal workers and 37% of UI takers between 2005 and 2009 in the administrative data.

their UI benefits. Therefore, the increase in formal reemployment rates after UI exhaustion, which our RD analysis showed was driven by UI incentives, does not come from an increase in the number of workers leaving non-employment. In that case, it must be due to a shift from informal to formal jobs, i.e., to a relative increase in the number of workers finding a formal job vs. an informal job among those finding a new job, or to an increase in the number of workers finding a formal job among those who had already found an informal job after layoff.

Robustness checks.– A possible concern for the comparability of the two samples used in Figures 8a-8c is that PME may not be representative of the population 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 5.7% 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 the two samples (e.g., in gender composition) in Appendix F1. Nevertheless, we show in Appendix F2 that all our results are similar if we reweight the PME sample such that it compares better to the administrative sample based on a rich set of worker characteristics.[58]

B. *Displaced formal workers' earnings loss after layoff*

Methodology.– The data limitations of PME surveys highlighted above have also implications for studying displaced formal workers' earnings loss after layoff, and the extent to which they can mitigate this loss by working informally. Ideally, we would follow individuals over time prior to layoff and in the months after layoff, including after UI exhaustion. Unfortunately, the panel of consecutive monthly interviews is too short to follow workers from layoff to UI exhaustion, and PME surveys do not record any information about earnings in prior jobs.

We proceed as follows given these limitations. We start with the PME sample used above but add non-employed workers who report having just been laid off from a formal job and were recorded as formally employed in the previous month. The resulting sample thus includes 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. We use that sample as a repeated cross-section and estimate the following specification for the net household earnings y of worker i:

$$y_{ikt} = \alpha + \sum_k \sum_t \beta_{kt} \times \mathbb{1}(status_k = k \ \& \ month_t = t) + \epsilon_{ikt}, \tag{3}$$

where k indicates the employment status (formally reemployed, informally reemployed, non-employed) and t indicates the month since layoff. As above, we

[58]We use fixed effects for year of layoff, month of layoff, education levels, gender, sector of activity in the lost job, and state, as well as second-order polynomials in age and tenure at layoff.

estimate different β_{kt} coefficients for months 0, 1-2, 3-4, 5-6, 7-8, and 9-10, we use sampling weights, and we cluster standard errors at the worker level. Workers formally employed in the month before layoff are the omitted group. We account for taxes to construct net earnings, and we use household earnings to account for income sharing and any labor supply response from household members.

Figure 8d displays the estimated β_{kt} divided by the average net household earnings before layoff (α). It presents results both omitting and including the rich set of worker characteristics mentioned above. This is to address possible concerns of selection bias with the specification in equation (3). We would, for instance, underestimate the relative earnings of workers reemployed informally if their earnings were below the average before layoff. The controls that we include can explain most of the cross-sectional difference in earnings between formal employees and informal workers in the overall population (see Appendix F3). Thus, we would expect them to have a strong influence on our results in Figure 8d if these results were severely affected by selection bias.

Results.– Figure 8d shows that household earnings drop by about 55% at layoff. Household earnings stay around 50%-55% of their pre-layoff levels in following months for workers who remain non-employed. Displaced formal workers are able to increase their household earnings to around 70%-75% (resp. 90%-95%) of pre-layoff levels upon finding an informal job (resp. a formal job). Our estimates are relatively stable before, around, and after UI exhaustion. So, 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. Importantly, our estimates barely change when we include individual controls (estimates slightly increase for workers reemployed informally). It is therefore unlikely that they are severely affected by selection bias. In sum, working informally does not appear to allow displaced formal workers to fully mitigate their earnings loss after layoff.[59]

C. *Value of insurance and welfare effects*

The estimates in this section allow us to shed some light on the value of insurance from increasing the potential UI duration. First, we compute the share of non-employed workers among mechanical beneficiaries, using the survival rates in Figure 8b: $share^P_{non} = \sum_{t=6}^{8} S_{t,non} / \sum_{t=6}^{8} S_t = .378$, where S_t and $S_{t,non}$ are the survival rates without a formal job and in non-employment (we assume that workers exhaust UI in months 6 to 8 after layoff), respectively. Second, using this share and the estimates of earnings losses for non-employed and informally reemployed workers in Figure 8d (averaging estimates in months 6 to 8 after layoff), we compute the average loss in household earnings for mechanical beneficiaries

[59]Adding the average UI benefit level from a comparable sample of workers in the administrative data brings workers who remain non-employed to 85%-90% of household earnings before layoff, and to 105%-110% for those reemployed informally (see Appendix F4). Therefore, without estimating its efficiency cost, it appears unlikely that an increase in the UI benefit level would generate large welfare gains in Brazil (non-employed workers may be able to self-insure against 10%-15% income losses).

compared to before layoff: $.378 \times (1 - .536) + (1 - .378) \times (1 - .753) = 32.9\%$.[60]

The magnitude of the value of insurance in equation (1) then depends on UI exhaustees' ability to find other ways to self-insure against such a loss and on the curvature of their utility function. In particular, we can approximate the value of insurance as: $\gamma \times \rho \times .329$, where the average earnings loss is multiplied by the sensitivity of consumption to the earnings loss (ρ) and by the coefficient of relative risk aversion (γ).[61] This decomposition shows that the value of insurance would exceed the efficiency cost (.217), and the welfare effect would be positive, for a benchmark coefficient of risk aversion of $\gamma = 2$ if the consumption loss was 33% as large as the earnings loss ($\rho \geq .33$). Chetty and Szeidl (2007) argue that the coefficient of risk aversion may be higher for displaced workers due to consumption commitments. Moreover, recent work from richer countries estimate that the loss in household consumption spending is about one third of the loss in household income (earnings and government transfers) for displaced workers (e.g., Ganong and Noel, 2019). However, more evidence is needed on the sensitivity of consumption to earnings losses for displaced formal workers in contexts of high informality in order to provide a complete evaluation of the welfare effects.

VI. Conclusion

This paper studied the efficiency cost of extending UI benefits in a context of high informality, by combining an optimal UI framework and empirical evidence from the Brazilian UI program. 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 first argued that the associated efficiency cost was not necessarily higher. We then found that it was rather low in Brazil compared to countries with low informality, and that is was lower in labor markets with higher informality within Brazil. These results are based on a benchmark measure of efficiency that constitutes a natural starting point. We discussed other mechanisms that could affect the magnitude of the efficiency cost; more research is needed to evaluate their empirical relevance for countries with high informality.

Our findings suggest shifting the focus from efficiency concerns, as in the current policy debate, to workers' need for insurance in a context of high informality. Our evidence in that respect remains suggestive. That displaced formal workers

[60] This calculation assumes that the average household earnings of workers reemployed informally in months 6 to 8 after layoff is also a good approximation for the average household earnings of displaced workers who found an informal job in earlier months and remain without a formal job after UI exhaustion. We may thus underestimate the average loss in household earnings if some workers reemployed informally quickly lost their informal job. By contrast, we may overestimate this loss if informal earnings grew quickly with tenure in the job (or if some workers reported being reemployed in a formal job but were actually kept on an informal payroll). Finally, abstracting from the facts that UI takeup is imperfect and not observed in PME is unlikely to matter much for this calculation: UI takeup is above 80% and most of the workers who do not take up UI are formally reemployed in the first two months after layoff.

[61] We use the approximation: $(u'^P - u'^F)/u'^F = \gamma \times (\Delta c/c)$, which assumes that third derivatives of utility functions are small (Chetty, 2006) and we decompose the consumption loss as $\Delta c/c = \rho \times \Delta y/y$.

return slowly to a formal job irrespective of UI benefits, which is the driving force behind 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 four times higher in Brazil than in the U.S. (2.8% vs. 0.7%). Moreover, it implies that formal employment may not be the "normal" state of the world for many formal workers, so their demand for UI may be relatively low given the cost, especially in the first months on the job. Yet, other means of consumption smoothing (e.g., access to credit) are often more costly in developing countries. There is also evidence that Brazilian workers are willing to trade off lower 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 job displacement insurance policies. UI Savings Accounts are sometimes presented as an alternative to UI in developing countries because of the heightened efficiency concerns.[62] We show that, once brought to the test of the data, those concerns may not be founded, at least for modest potential UI durations. Yet, as the average paid UI duration is close to the potential UI duration in Brazil, granting UI takers their full UI benefits irrespective of when they find a new formal job could provide most workers with a comparable degree of insurance while eliminating any moral hazard concern.

Finally, the findings of this paper have broader implications for our understanding of social policies in developing countries. First, similarly to UI programs, other social programs are viewed as imposing high efficiency costs in developing countries because they generate incentives to work informally. Our results cast doubt on whether such concerns are founded in these cases too. Consider a welfare program for households with formal income levels below a cutoff. The usual moral hazard problem is that households may reduce their labor supply to become eligible. In a context of high informality, they may also work informally. Yet, many households likely work informally and have low formal income levels absent the program's incentives (i.e., the mechanical effect is likely large). Whether its efficiency cost will be relatively high in a context of high informality is thus an empirical question. Second, our results indicate that efficiency concerns may in fact become more relevant when a country's economy formalizes. The share of workers who would be formally employed absent policies' incentives increases. So, unless workers become less responsive to these incentives (e.g., it becomes harder to work informally), the efficiency cost of these policies may increase as well.

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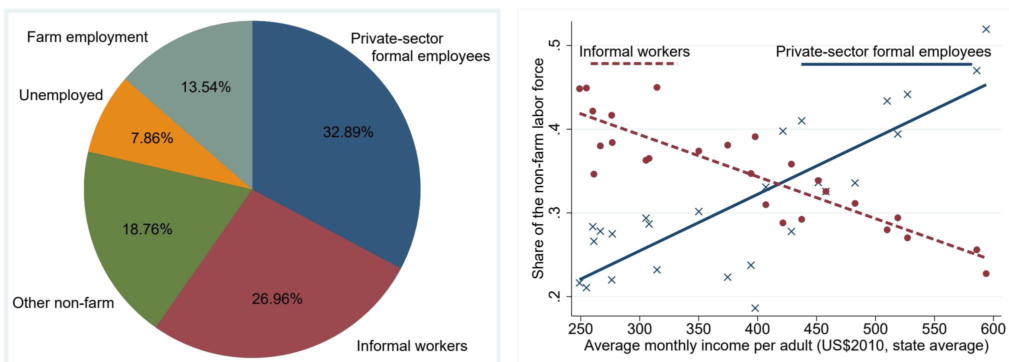
Figure 1. : Prevalence of informal workers in Brazil (2005-2009)

(a) Average composition of the labor force

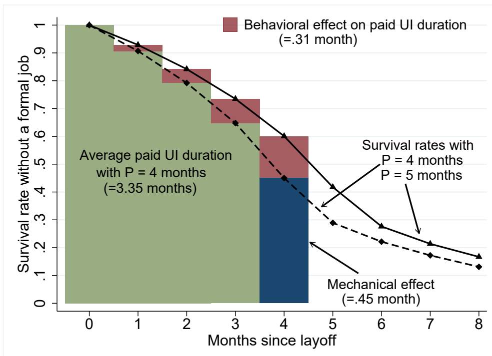
(b) Private-sector formal employees and informal workers by state (out of the non-farm labor force)



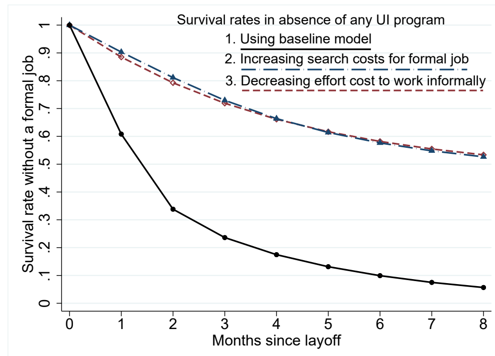
Notes: The figure documents the prevalence of private-sector formal employees and informal workers in Brazil over our main period of analysis (2005-2009; see data section). In panel (a), "informal workers" consists of non-farm informal employees (12.3%) and self-employed workers (14.6%), and "other non-farm" 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. Source: PNAD surveys (see data section).

Figure 2. : Illustrating the conceptual framework

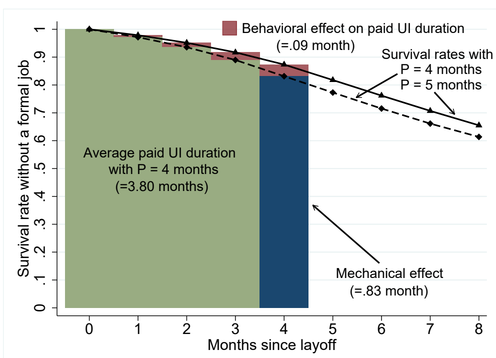
(a) Behavioral and mechanical effects of an increase in potential UI duration (dP) in baseline model



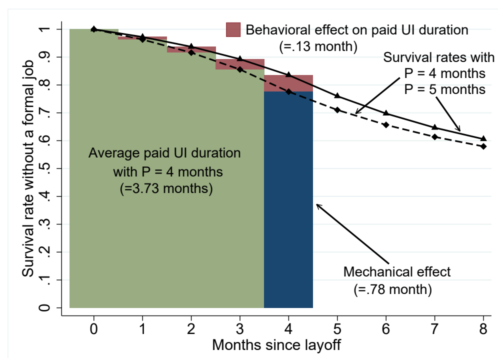
(b) Survival rates in absence of UI (1) with baseline model, (2) with higher cost of finding formal job, and (3) with lower cost of working informally



(c) Behavioral and mechanical effects using baseline model with higher cost of finding a formal job



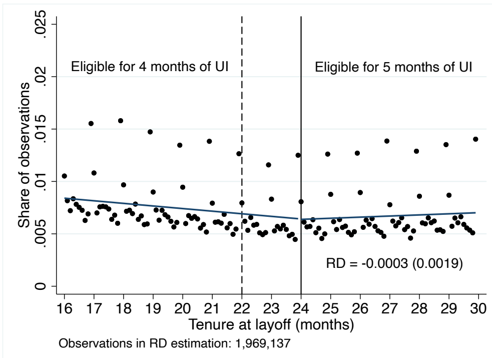
(d) Behavioral and mechanical effects using baseline model with lower cost of working informally



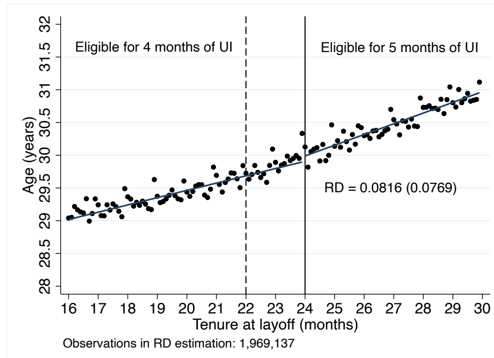
Notes: The figure displays survival rates without a formal job from simulations of a dynamic model à la Chetty (2008), in which workers who are not yet formally reemployed can search for a formal job at some convex search cost (as in Chetty, 2008), but also earn income informally at some convex effort cost. Panel (a) uses a "baseline" model to illustrate the mechanical effect of an increase in potential UI duration (dP) from 4 months to 5 months, as well as the behavioral effect on the paid UI duration. Panel (b) then illustrates that workers may return slower to a formal job in a context of high informality, even in absence of UI benefits: compared to the baseline model, survival rates without a formal job in absence of any UI program remain higher if we double the cost of finding a formal job ($\simeq$ "exclusion" view) or if we decrease the cost of working informally by 80% ($\simeq$ "exit" view; we calibrated the change in model parameters such that survival rates are similar between the two cases). Finally, panels (c) and (d) illustrate the implications of the higher cost of finding a formal job and of the lower cost of working informally for the efficiency cost of the same increase in potential UI duration as in panel (a). In both cases, because workers return slower to a formal job, the mechanical effect of the policy increases. In this simple model, the behavioral effect on the paid UI duration also decreases in both panels (c) and (d).

Figure 3. : Evidence supporting the validity of the RD design

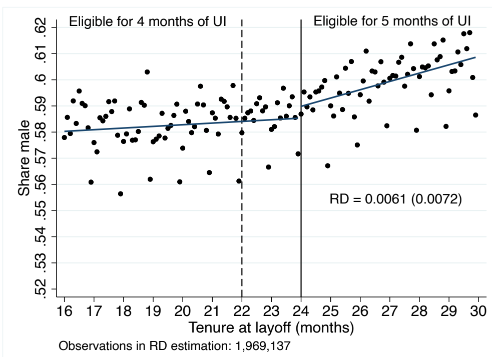
(a) Share of observations



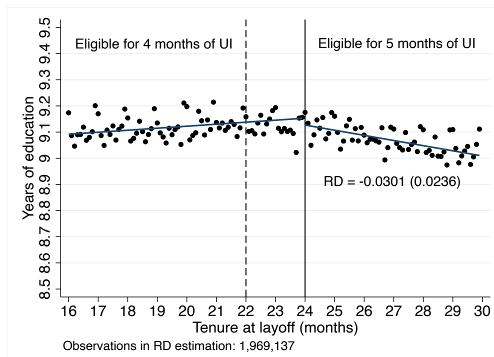
(b) Average age



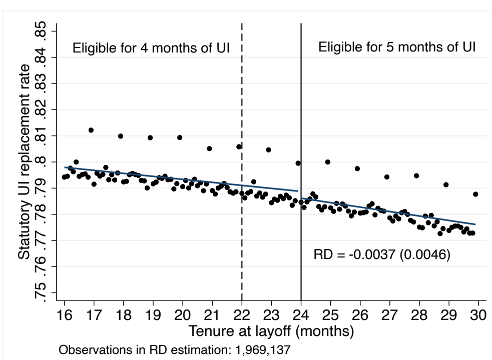
(c) Share of male workers



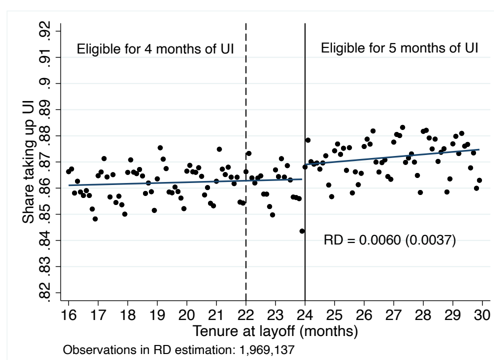
(d) Average education level



(e) Average statutory UI replacement rate



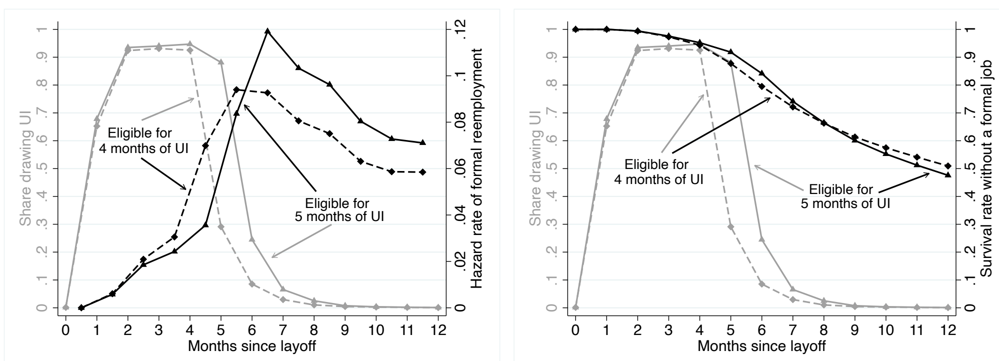
(f) Share of UI takers



Notes: The figure provides evidence supporting the validity of the RD design. Panels (a)-(f) display averages of a series of variables by tenure levels (.1 month bins). The line below the cutoff (resp. above the cutoff) 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). The estimated β, the difference between the two lines at the cutoff, is displayed in each panel with its standard error (in parenthesis). Panel (a) displays the share of observations by tenure level. It shows that there is no visible change in the distribution of the running variable around the cutoff. Panels (b)-(e) display the average of a series of workers' characteristics by tenure level. It shows that there is no visible change in the composition of the sample around the cutoff. Panel (f) displays the share of workers taking up UI after layoff by tenure level. It shows that any impact of the increase in potential UI duration is not driven by a change in UI takeup.

Figure 4. : UI and formal reemployment in the RD sample

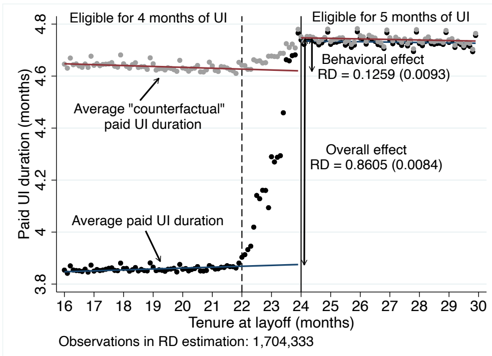
(a) UI and hazard rates of formal reemployment (b) UI and survival rates without a formal job



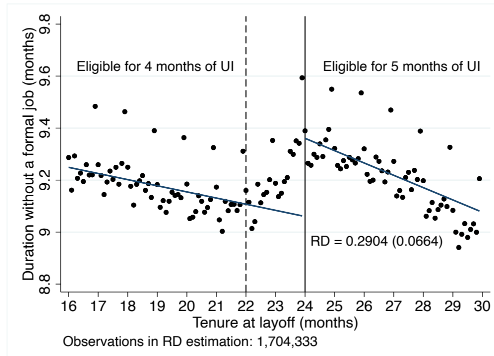
Notes: The figure presents key patterns of UI collection and formal reemployment in the RD sample for UI takers eligible for 4 months of UI (tenure levels between 16 and 22 months) and 5 months of UI (tenure levels between 24 and 30 months), separately (1,704,333 layoffs). Panel (a) compares the share drawing UI and the hazard rate of formal reemployment in each month since layoff. Panel (b) compares the share drawing UI and the survival rate without a formal job in each month since layoff.

Figure 5. : Main RD results

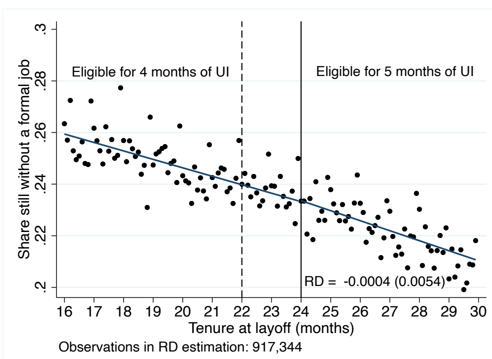
(a) Impact on the average paid UI duration: overall effect and behavioral effect



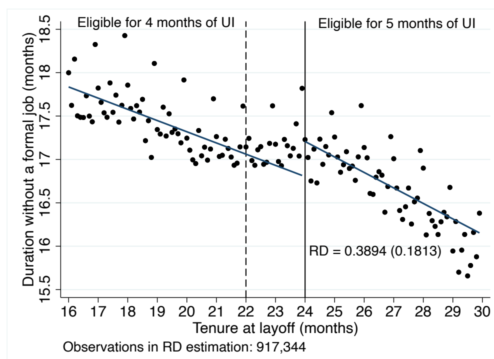
(b) Impact on the average duration without a formal job censored at 1 year after layoff



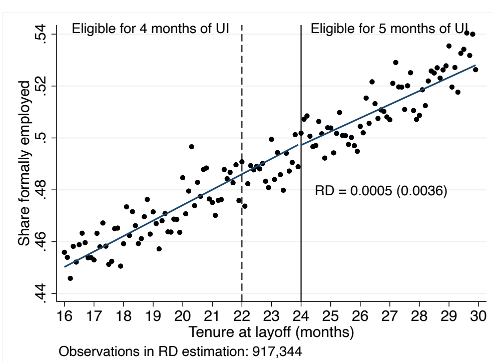
(c) Impact on the survival rate without a formal job 3 years after layoff



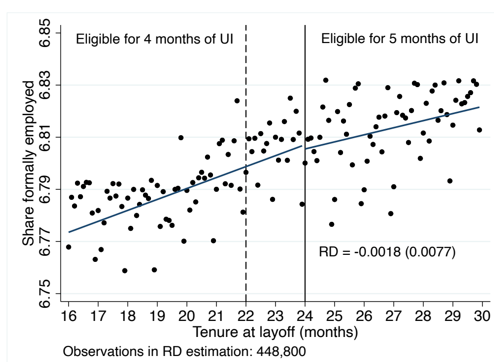
(d) Impact on the average duration without a formal job censored at 3 years after layoff



(e) Impact on the share of workers formally employed in December 3 years after layoff



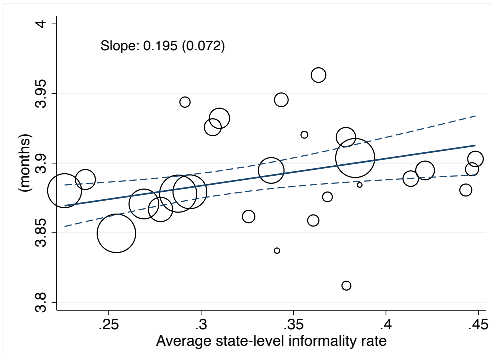
(f) Impact on the average real monthly wage if formally employed in December 3 years after layoff



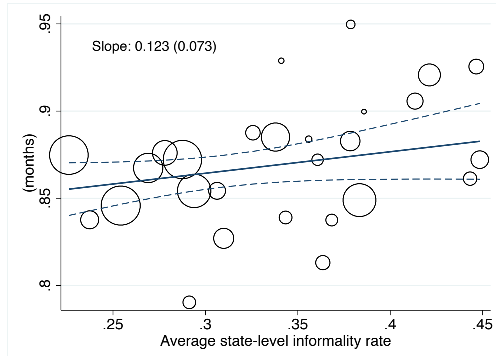
Notes: The figure presents the main RD results. Panels (a)-(f) display averages of a series of outcome variables by tenure levels (.1 month bins). The line below the cutoff (resp. above the cutoff) is estimated using an edge kernel and observations in a bandwidth of six months below 22 months of tenure (resp. above 24 months of tenure). The estimated β, the difference between the two lines at the cutoff, is displayed in each panel with its standard error (in parenthesis). Panel (a) displays the paid UI duration and the counterfactual paid UI duration, which allow us to decompose the overall effect on the paid UI duration into behavioral and mechanical effects. Panel (b) displays the duration without a formal job censored at 1 year after layoff. Panels (c)-(f) restrict attention to workers laid off between 2005 and 2007, which we can follow for 3 years after layoff. They consider the likelihood of remaining without a formal job 3 years after layoff, the duration without a formal job censored at 3 years after layoff, the likelihood of being formally employed in December 3 years after layoff, and the logarithm of the real monthly wage among those workers formally employed in December 3 years after layoff.

Figure 6. : Heterogeneity of RD results with informality rates

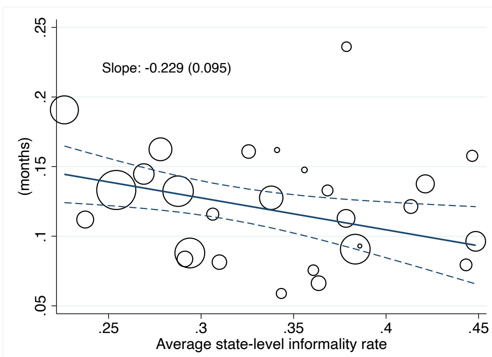
(a) Average paid UI duration if eligible for 4 months of UI



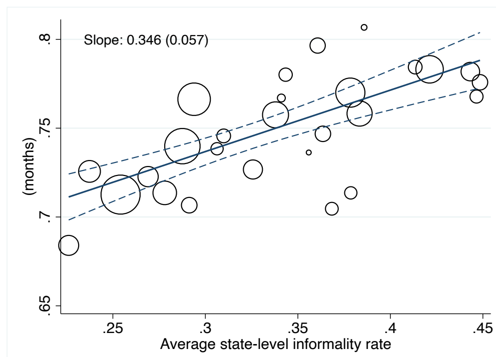
(b) Overall effect on average paid UI duration



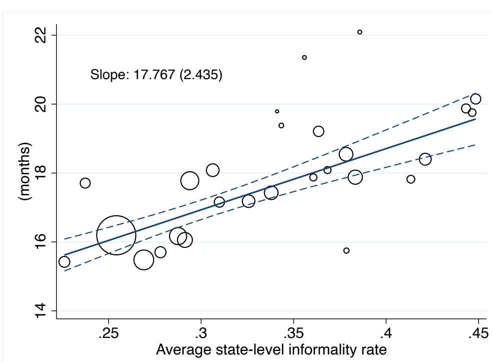
(c) Behavioral effect on average paid UI duration



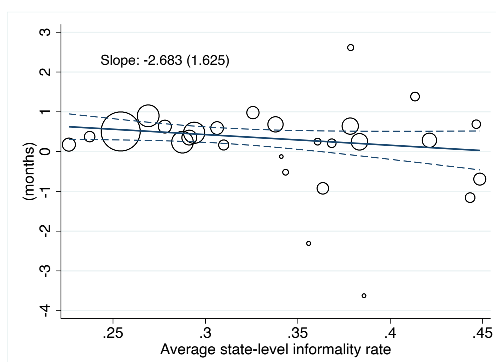
(d) Mechanical effect on average paid UI duration



(e) Average duration without a formal job (censored at 3 years) if eligible for 4 months of UI

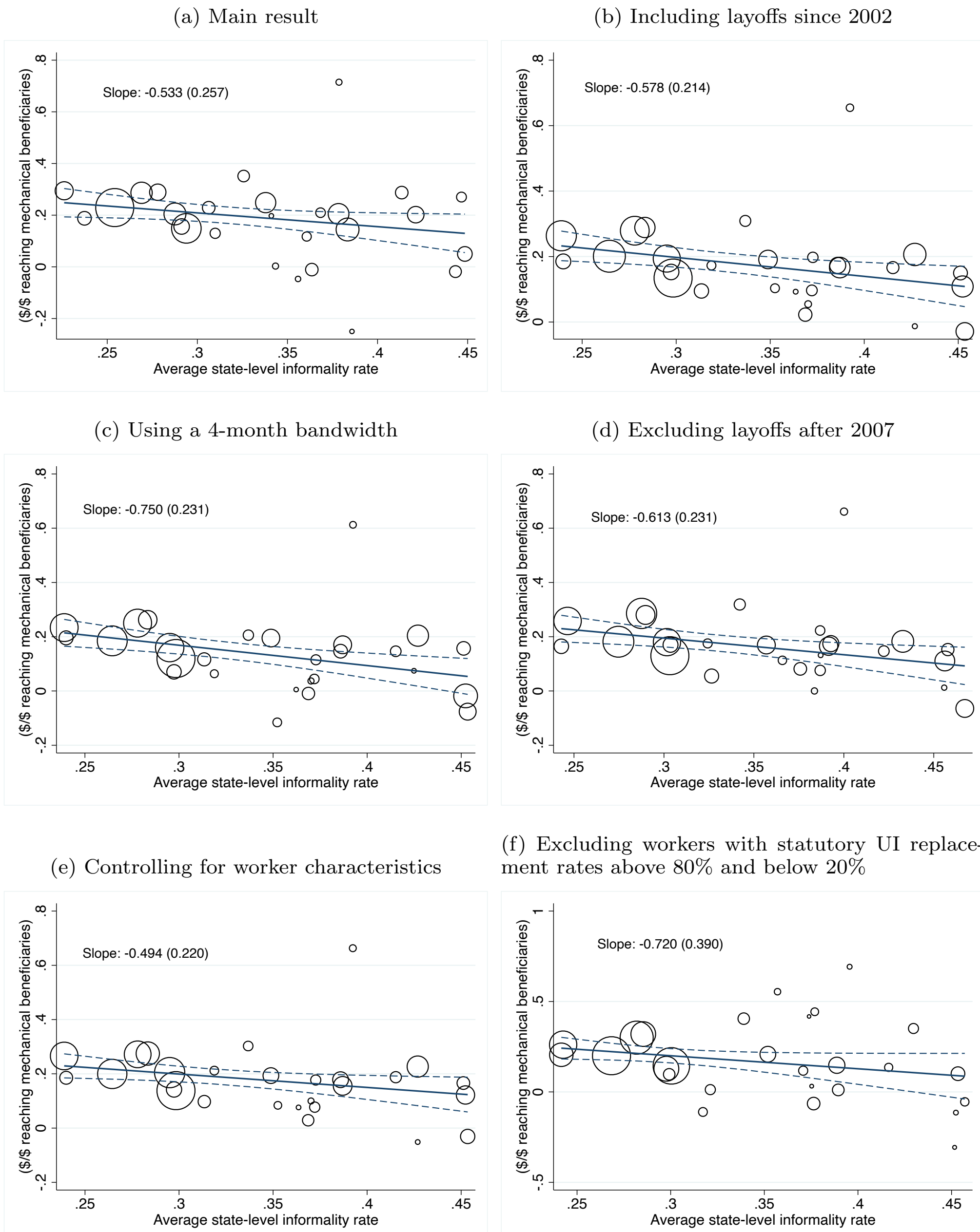


(f) Behavioral effect on average duration without a formal job (censored at 3 years)



Notes: The figure displays results from estimating our main RD specification for each of the 27 Brazilian states, 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 them by the inverse of their standard error squared (with 95% confidence intervals). The estimated slope is displayed with its standard error (in parenthesis). Panels (a) and (b) consider the specification for the paid UI duration, displaying the estimated constant α and the estimated β (i.e., the overall effect on the average paid UI duration), respectively. Panel (c) considers the specification for the counterfactual paid UI duration, displaying the estimated β, i.e., the behavioral effect on the average paid UI duration. Panel (d) displays the mechanical effect, which is equal to the difference between the estimated constants α in the specifications for the counterfactual and actual paid UI durations. Panels (e) and (f) consider the specification for the duration without a formal job, displaying the estimated constant α and the estimated β (i.e., the behavioral effect on the average duration without a formal job), respectively.

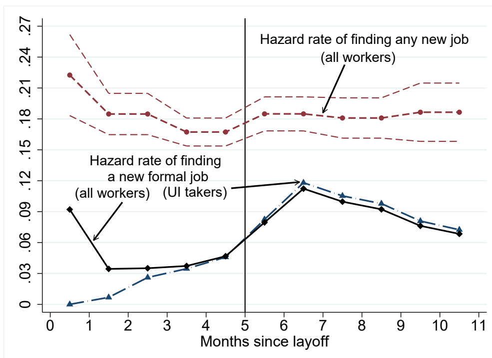
Figure 7. : Efficiency cost and informality (main result and robustness checks)



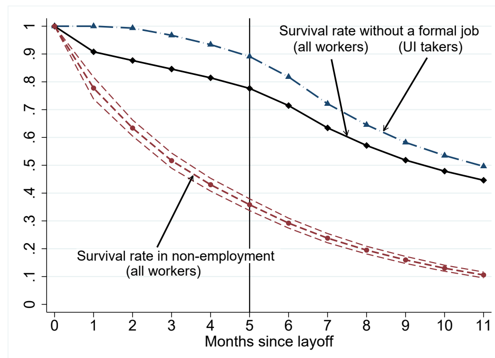
Notes: The figure displays estimates of the efficiency cost computed as in Section III.D for each of the 27 Brazilian states, separately. State-level estimates are plotted against average state-level informality rates. Lines display the result of a WLS regression of these estimates on informality rates, weighting them by the inverse of their standard error squared (with 95% confidence intervals). The estimated slope is displayed with its standard error (in parenthesis). Panel (a) presents estimates based on the results in Figure 6. Panels (b)-(f) then present estimates based on the same robustness checks as in Table 1.

Figure 8. : Complementary evidence using survey data

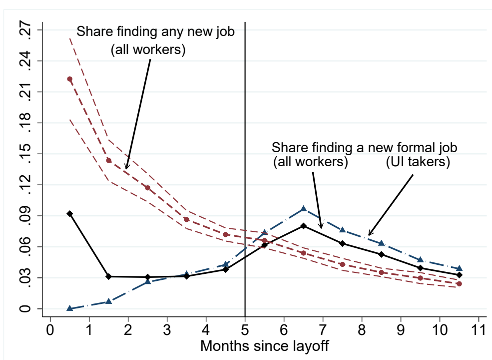
(a) Hazard rate of finding a new formal job vs. hazard rate of finding any new job



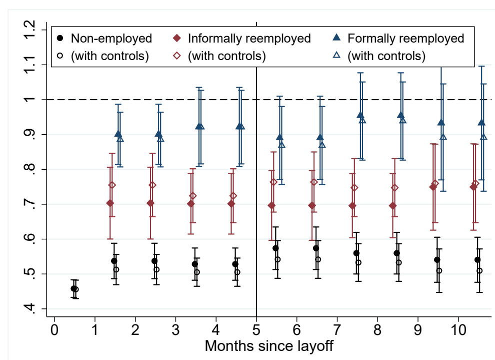
(b) Survival rate without a formal job vs. survival rate in non-employment



(c) Share finding a new formal job vs. share finding any new job



(d) Relative household earnings after layoff compared to before layoff



Notes: The figure provides complementary evidence using data from PME surveys. All displaced formal workers in this analysis are eligible for five months of UI after layoff (after a 30-day waiting period). Panels (a)-(c) display the hazard rate of finding any new job (overall reemployment) in each month since layoff estimated using PME surveys (with 95% confidence intervals), the associated survival rate in non-employment, and the associated share finding any new job (i.e., survival rate × hazard rate), respectively. Panels (a)-(c) also display the hazard rate of finding a new formal job (formal reemployment) computed in the administrative data for a comparable sample of workers, the associated survival rate without a formal job, and the associated share finding a new formal job, respectively. We present formal reemployment patterns both using all workers and using the subsample of UI takers for the sake of comparability with the survey-based estimates (we cannot restrict attention to UI takers in PME surveys) and with the patterns in Figure 4, respectively. Panel (d) displays estimates of relative household earnings in each month since layoff compared to pre-layoff levels using PME surveys (with 95% confidence intervals), for workers who remain non-employed and for those employed in their first month of reemployment in a formal or an informal job. It presents results both omitting and including a rich set of worker characteristics to address possible concerns of selection bias (see text for details).

Table 1: Robustness checks for the RD analysis

	A. Main specification			**B. Including layoffs since 2002**		
	Actual paid UI duration (months)	Counterfactual paid UI duration (months)	Duration without a formal job censored at 3 years (months)	Actual paid UI duration (months)	Counterfactual paid UI duration (months)	Duration without a formal job censored at 3 years (months)
	[1]	[2]	[3]	[1]	[2]	[3]
Tenure ≥ 24 months	0.8605	0.1259	0.3894	0.8519	0.1150	0.3720
	(0.0084)	(0.0093)	(0.1813)	(0.0080)	(0.0062)	(0.1453)
# Observations	1,704,333	1,704,333	917,344	2,667,223	2,667,223	1,880,941
	C. Using a 4-month bandwidth			**D. Excluding layoffs after 2007**		
	Actual paid UI duration (months)	Counterfactual paid UI duration (months)	Duration without a formal job censored at 3 years (months)	Actual paid UI duration (months)	Counterfactual paid UI duration (months)	Duration without a formal job censored at 3 years (months)
	[1]	[2]	[3]	[1]	[2]	[3]
Tenure ≥ 24 months	0.8382	0.1060	0.1856	0.8477	0.1089	0.3720
	(0.0114)	(0.0078)	(0.1839)	(0.0085)	(0.0070)	(0.1453)
# Observations	1,725,025	1,725,025	1,216,133	1,880,941	1,880,941	1,880,941
	E. Controlling for worker characteristics			**F. Excluding workers with statutory UI replacement rates above 80% and below 20%**		
	Actual paid UI duration (months)	Counterfactual paid UI duration (months)	Duration without a formal job censored at 3 years (months)	Actual paid UI duration (months)	Counterfactual paid UI duration (months)	Duration without a formal job censored at 3 years (months)
	[1]	[2]	[3]	[1]	[2]	[3]
Tenure ≥ 24 months	0.8508	0.1142	0.3972	0.8273	0.1129	0.4135
	(0.0069)	(0.0035)	(0.0619)	(0.0091)	(0.0078)	(0.1394)
# Observations	2,667,223	2,667,223	1,880,941	1,224,723	1,224,723	962,394

Notes: The table presents robustness checks for the RD analysis. Panel A first reproduces the estimates in Figure 5 that are needed to evaluate the efficiency cost, i.e., the effects on the actual paid UI duration (i.e., overall effect on the average paid UI duration), on the counterfactual paid UI duration (i.e., behavioral effect on the average paid UI duration), and on the duration without a formal job (i.e., behavioral effect on the average duration without a formal job). Panels B-F then present results for the same outcomes including layoffs since 2002, using a 4-month bandwidth around the cutoff, excluding layoffs after 2007, including a rich set of individual controls (see text for details), and excluding workers with statutory UI replacement rates above 80% and below 20%. We use layoffs since 2002 for all these robustness checks to maximize sample size.