Informal Labor and the Efficiency Cost of Social Programs: Evidence from 15 Years of Unemployment Insurance in Brazil

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[Note: the paper is currently being revised; some aspects of the presentation will thus differ from the text]

Abstract

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). We introduce informal work opportunities into a canonical model of optimal UI that specifies the typical tradeoff between workers' need for insurance and the efficiency cost from distorting their incentives to return to a formal job. We then combine the model with evidence drawn from 15 years of comprehensive administrative data to quantify the efficiency cost of the UI program in Brazil. We first show that beneficiaries respond to UI incentives. Exogenous increases in UI maximum benefit duration led to falls in formal–sector reemployment rates due to offsetting rises in informal employment. However, because reemployment rates in the formal sector are low, most beneficiaries would draw the UI benefits absent behavioral responses. Consequently, only a fraction of the cost of (longer) UI benefits is due to perverse incentive effects and the efficiency cost is thus relatively small. Using variation in the share of formal and informal workers across regions and over time, we then show that the efficiency cost of longer UI benefits in fact increases with labor market formality. In sum, the results go against the conventional wisdom, and indicate that efficiency considerations may even become more relevant as the formal sector expands. Finally, we provide evidence that the insurance value of longer UI benefits may be sizeable in Brazil. (*JEL* H00, J46, J65)

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The enforcement of tax compliance and social program eligibility is a major challenge in developing countries, where the informal sector accounts for a large share of GDP and employment.[1] In a context of high informality, the conventional wisdom dictates that taxes and social spending impose high efficiency costs (Gordon and Li, 2009). This is thought to be particularly the case for social programs that require beneficiaries to not be formally employed (Levy, 2008). The concern is that informal job opportunities exacerbate programs' disincentives to work in the formal sector.

Despite this widespread view, the evidence behind it remains limited. First, due to data constraints, very few papers credibly estimate the impact of social programs on employment choices. Existing surveys often poorly measure eligibility and have sample sizes too small to exploit most sources of exogenous variation in program benefits. Large administrative datasets are only slowly becoming available in developing countries. Second, those studies finding that social programs induce some beneficiaries to not work in the formal sector lack a theoretical framework to interpret this evidence and quantify the relevant tradeoff between efficiency and equity or insurance.[2]

This paper addresses both limitations for the case of Unemployment Insurance (UI) in Brazil. We introduce informal work opportunities into a canonical partial–equilibrium framework of optimal UI to guide our empirical analysis. We then provide new evidence on the size of the relevant effects using 15 years of restricted access administrative data, longitudinal survey data, and quasi–exogenous variation in UI incentives. As a result, we quantify the tradeoff between formal job–search incentives and insurance, and we provide the first estimates of efficiency costs for a typical social program in a setting where informal labor is prevalent.

UI is an ideal program to study these issues. It requires the beneficiaries — displaced formal employees — to not be formally reemployed. It has been adopted or considered in a number of middle–income and developing countries, including Mexico recently.[3] 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] Compared to other programs, UI has

[1]For instance, the informal sector accounts for 40% of GDP and 55% of the labor force on average in both Brazil and Latin America (Schneider, Buehn and Montenegro, 2010; Perry et al., 2007).

[2]For instance, several papers investigate the impact of the Mexican *Seguro Popular* program, which extended health care coverage to the informally employed (Azuara and Marinescu, 2013; Campos-Vazquez and Knox, 2008; Bosch and Campos-Vasquez, 2010; Aterido, Hallward-Driemeier and Pagés, 2011). However, none of them makes use of a theoretical framework to interpret the estimated impacts in terms of the efficiency cost of the policy.

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

[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, Vodopivec and Bodor, 2009). The authors are the current and former Labor Team leaders at the Social Protection anchor of the World Bank. See also Acevedo, Eskenazi and Pagés (2006), and Vodopivec (2013). These policy papers cite evidence of moral hazard from Slovenia (van Ours and Vodopivec, 2006), a country with relatively high levels of formality. The proposed alternative is a system of Unemployment Insurance Savings Accounts (UISA, Feldstein and Altman, 2006), such as

been studied extensively in developed countries, allowing us to benchmark our results against existing estimates, and eligibility conditions are relatively straightforward. Brazil also constitutes a uniquely well–suited empirical setting because it offers wide variation across space and time in the share of formal and informal workers, which include self–employed workers and informal employees.[5] This allows us to explore how efficiency costs vary with labor market formality.

We begin by adapting the Baily framework of optimal UI (Baily, 1978; Chetty, 2006). We introduce informal work opportunities and we consider changes in UI maximum duration instead of changes in benefit levels (Schmieder, von Wachter and Bender, 2012). The framework allows us to identify sufficient statistics that capture the tradeoff between workers' need for insurance and the efficiency cost from distorting their incentives to return to a formal job. The efficiency cost of longer UI benefits is captured by the ratio of a *behavioral* cost to a *mechanical* cost. The former measures the cost of the policy due to behavioral responses. Beneficiaries may delay formal reemployment to draw additional benefits. The latter measures the cost absent behavioral responses. Beneficiaries who would not be formally reemployed after UI exhaustion in the absence of the policy draw additional benefits mechanically. Intuitively, the ratio measures the share of social spending lost through behavioral responses. Welfare effects are positive if the ratio does not exceed the *insurance value*, the social value of the income transfer to target (mechanical) beneficiaries.[6]

We then exploit a unique dataset matching the universe of formal employment spells in Brazil to the universe of UI payments from 1995 to 2010. We observe how rapidly each worker returns to a formal job after layoff. This allows us to estimate the mechanical cost of longer UI benefits for every displaced formal employee. We estimate the behavioral cost using two empirical strategies, a temporary UI extension (difference–in–difference, DD) and a tenure–based eligibility cutoff (regression discontinuity, RD). The second empirical strategy is based on variation in maximum benefit duration across workers within every state and year. We can thus estimate how efficiency costs vary with the labor market (state) share of formal and informal workers. Finally, we use longitudinal survey data to estimate overall (formal and informal) reemployment rates and investigate what share of UI beneficiaries remains unemployed vs. works informally. The same surveys provide suggestive evidence for the insurance value of longer UI benefits.

This paper has five main findings. First, UI is very costly in Brazil. Almost all UI takers, who are eligible for three to five months of UI in Brazil, exhaust their benefits. Increasing maximum

the new Jordanian program designed in consultation with the World Bank (http://www.social–protection.org). The new Mexican program will also rely almost entirely on giving workers access to their own savings.

[5]The variation in formal employment rates across states over our 15 years of data covers the existing variation across Latin American countries today. The same is true for the (strongly correlated) variation in income per capita.

[6]This is a standard result in public economics (e.g. Hendren, 2013). Our measure of efficiency applies to a large class of models (Chetty, 2006). Importantly, one does not need to identify distortions along other margins of behaviors to estimate efficiency costs, even with informality. This result relates to Feldstein (1999). Our ratio provides an upper bound on efficiency costs if all private costs of "evasion" are not true social costs (Chetty, 2009).

benefit duration by one month (RD) or two months (DD) increases average benefit duration by .9 month or 1.86 months. UI may be costly for two reasons. On the one hand, it may be mechanically costly if workers are unable or unwilling to return rapidly to a formal job, absent behavioral responses. On the other hand, behavioral responses to UI incentives may be large where informal work opportunities are prevalent. Concerns that social programs may be particularly distorting in a context of high informality focus on the second explanation. However, the first explanation is consistent with the two main views on labor informality in the literature (Perry et al., 2007): workers may not return rapidly to a formal job because formal jobs are more difficult to find (*exclusion* view) or because informal jobs may be relatively attractive for reasons unrelated to UI (*exit* view).

Second, workers are very responsive to UI incentives. Formal reemployment rates are low while workers are eligible for UI; they spike at UI exhaustion; and the spike shifts with exogenous changes in UI maximum benefit duration. Moreover, the response likely comes from beneficiaries working informally. We find no such spike in overall reemployment rates and there are more workers finding a first new formal job than workers finding a first new job around UI exhaustion.

Yet, our third finding is that the efficiency cost of (longer) UI benefits is limited. This is because, despite workers being responsive to UI incentives, the potential distortion is small. Formal reemployment rates are low even after UI exhaustion and even for ineligible workers. So most displaced formal employees would not return rapidly to a formal job in the absence of UI incentives and would draw UI benefits mechanically. Consequently, any behavioral cost is small compared to the mechanical cost, and the efficiency cost is thus limited. We estimate an average efficiency cost of longer UI benefits of 10 cents (RD) to 15 cents (DD) per $1 reaching mechanical beneficiaries. Furthermore, the efficiency cost of offering five months of UI to currently ineligible workers would be at most 10 cents per $1. In comparison, estimates for the US imply an efficiency cost of longer UI benefits above $1 per $1 reaching mechanical beneficiaries (Katz and Meyer, 1990).

Fourth, the efficiency cost of longer UI benefits in fact increases with labor market formality. On the one hand, in a more formal labor market, workers may return more rapidly to a formal job in the absence of UI incentives. It may be easier to find a formal job. The potential distortion would then increase as more workers could respond to UI incentives. On the other hand, it may become harder for such a worker to actually respond to UI incentives. We find that workers do return more rapidly to a formal job in the absence of UI incentives in labor markets where the share of formal employees increases and the share of informal workers decreases. The efficiency cost increases because this effect dominates any decrease in workers' ability to respond to UI incentives.

Finally, despite the fact that many of them appear to work informally, mechanical beneficiaries have relatively low levels of disposable income and a significant share of them remain unemployed. This suggests that the insurance value of longer UI benefits may be sizable in Brazil.

The issues studied in this paper are not specific to UI and our findings may apply broadly.

Consider an income transfer program targeting households with observed (formal) income levels below a threshold. The usual incentive problem is that non–eligible households may reduce their earnings in order to qualify for the program. In a context of high informality, they may also choose to work informally because informal earnings are easier to hide. At the same time, many households would likely have low formal income levels even absent program incentives. In a more formal labor market, households may be more likely to have formal income levels above the threshold in the absence of the program. However, it may be more difficult for such a household to respond to program incentives. Whether the efficiency cost will be high and whether it will decrease with labor market formality will again depend on which of these two effects dominates.

This paper extends a large theoretical and empirical literature on social insurance in developed countries.[7] The closest paper to ours is perhaps Schmieder, von Wachter and Bender (2012), which investigates how the impact of UI extensions varies over the business cycle in Germany. Consistent with our findings, they estimate smaller efficiency costs during recessions when base reemployment rates are low. Our paper differs in a key way. Informality is limited in Germany. Moreover, booms and busts occur periodically, but formal employment is persistently low in developing countries and is expected to rise with economic development. We also contribute to a growing literature at the intersection of public economics and development.[8] A theoretical literature argues that efficiency considerations may force governments to resort to alternative, second–best policies where enforcement is weak and informality is high. However, there is little empirical evidence on the relevant impact of typical policies in such countries (Gordon and Li, 2009). We find that the efficiency cost of a common social program is low in Brazil even though informality is prevalent.

Finally, our approach and findings contribute to the nascent empirical literature on the impact of social programs in countries with high informality.[9] Existing studies do not typically link their results to standard public finance theoretical frameworks, complicating interpretation. We use such a framework to guide our empirical analysis; we provide new empirical evidence that allows us to

[7] Chetty and Finkelstein (2012) review the literature. Katz and Meyer (1990), Card and Levine (2000), and Landais (2012) empirically investigate the impact of UI extensions on benefit collection and formal reemployment rates in the US. As in most of the literature, we find no effect of UI extensions on subsequent match quality in the formal sector.

[8] See, for example, Best et al. (2013), Carillo, Pomeranz and Singhal (2014), Kleven and Waseem (2013), Niehaus and Sukhtankar (2013), Olken and Singhal (2011), Pomeranz (2012), or Sivadasan and Slemrod (2008).

[9] In addition to previously cited papers, Bérgolo and Cruces (2010), Camacho, Conover and Hoyos (2009), and Gasparini, Haimovich and Olivieri (2009) also focus on impacts at the formal–informal employment margin. We are aware of two working papers, developed in parallel to our work, attempting to estimate the impact of UI on some labor market outcomes in Latin American countries (González-Rozada, Ronconi and Ruffo, 2011; Amarante, Arim and Dean, 2013). We are aware of three working papers on UI in Brazil that are mostly descriptive (Cunningham, 2000; Margolis, 2008; Hijzen, 2011). A complementary literature investigates the impact of UI in macro-labor models with an informal sector (Zenou, 2008; Ulyssea, 2010; Robalino, Zylberstajn and Robalino, 2011; Meghir, Narita and Robin, 2012). In practice, there is no need for insurance in these models as they assume risk neutral workers. Moreover, they cannot study moral hazard because they typically model UI as a lump-sum transfer that formal employees are entitled to upon layoff. Finally, on the benefit side, Chetty and Looney (2006, 2007) highlight the likely high value of social insurance in developing countries given households' difficulty at smoothing consumption after employment shocks.

directly estimate the efficiency cost from distorting incentives to return to a formal job; and we evaluate the resulting welfare effects. We are also the first paper to estimate how behavioral responses to a social program vary with labor market formality. In so doing, our results go against the conventional wisdom that social programs must be particularly distorting in a context of high informality. Efficiency considerations may even become more relevant as the formal sector expands. Of course, this may be a local relationship and, at some point, the efficiency cost of (longer) UI benefits may start decreasing with labor market formality.[10] Yet, because Brazil contains regions with such widely divergent levels of labor market formality, we are optimistic about the external validity of our study for the context of developing countries.

The remainder of this paper is structured as follows. Section 1 provides some background and describes our data. Section 2 presents the conceptual framework that guides our analysis. Section 3 proposes a first look at the data, highlighting important differences in UI outcomes between Brazil and more developed countries. Section 4 estimates the efficiency cost of longer UI benefits using administrative data. Section 5 provides complementary evidence using survey data. Section 6 incorporates the results in our framework to evaluate welfare effects. Section 7 discusses some limitations of our framework and possible extensions. Section 8 concludes.

1 Background and Data

1.1 Labor markets in Latin America and Brazil

Labor markets in many developing countries, including Latin American countries, are characterized by the coexistence of formal employees and informal workers.[11] Formal employees typically work in jobs with strictly regulated working conditions (e.g., overtime pay, firing costs) and relatively high payroll taxes. In exchange, they are entitled to a series of benefits (e.g., pensions, disability). Informal workers, who pay no income or payroll taxes and are not eligible for these benefits, encompass employees in non–complying firms (mostly smaller firms) and most self–employed workers (mostly unskilled). A same firm may hire both formal and informal employees.[12]

There are two main views on the prevalence of informal workers in developing countries (Perry et al., 2007). In the *exit* view, workers are voluntarily informal because they do not value the benefits of formal employment above its costs (Maloney, 1999). In the *exclusion* view, workers

[10] Once displaced formal employees would return rapidly to the formal sector in the absence of UI incentives, we would expect an increase in labor market formality to be only associated with a decrease in workers' ability to respond to UI incentives.

[11] We define a job as informal if it escapes monitoring by the government. This is the relevant definition in our context. Informal jobs cannot be offered UI and UI agencies cannot identify beneficiaries working informally.

[12] The 2002 World Bank's Investment Climate Survey in Brazilian manufacturing asks participating firms about the share of unregistered workers that a similar firm likely employs. The median answer is 30% for small firms.

would prefer formal jobs but formal jobs are more difficult to find. Longitudinal survey data show that workers transit between formal and informal jobs in Latin American countries (Bosch and Maloney, 2010). This evidence contradicts early versions of the *exclusion* view, which considered formal and informal sectors as segmented (Fields, 1975). However, it is fully consistent with formal jobs being more difficult to find (Meghir, Narita and Robin, 2012). Surveys also show that earnings levels are higher in the formal sector, although there is a lot of heterogeneity, and some informal workers may be better off than in their alternative options in the formal sector (for Brazil, see Botelho and Ponczek, 2011). Today, these two main views are recognized as complementary.

In contrast to other countries, formal employment is well–defined in Brazil. Every worker has a working card. When an employer signs her working card, which is mandatory, an employee becomes formal and her hiring is reported to the government. Hiring an employee formally is costly, however, and informal labor is prevalent.[13] Figure Ia displays the average distribution of the working–age population by labor status over our sample years (1995–2009). The share of non–farm private formal employees (23.9%) is about equal to the share of non–farm informal workers (21.6%).[14] Brazil is an extremely diverse country, however. Figure Ib shows that the shares of formal employees and informal workers vary greatly across state–year pairs (27 states), and that these shares are strongly correlated with income per capita. In the paper, we use this variation to explore how the efficiency cost of longer UI benefits changes with labor market formality.

1.2 The Brazilian Unemployment Insurance program

Unemployment insurance is a sizable program in Brazil. UI expenditures amount to 2.5% of the total eligible payroll, more than three times the corresponding figure for the US (www.dol.gov). The first UI program was introduced in 1986, but with a very small scope. A more complete UI program was established in the 1988 Constitution. The current rules have applied since 1994.

A worker who is involuntarily laid off from a private formal job and who has at least six months of job tenure at layoff is eligible for UI benefits after a 30–day waiting period. Additionally, there must be at least 16 months between a worker's layoff date and the layoff date of her last successful application. The maximum benefit duration depends on workers' accumulated tenure across all formal jobs in the 36 months prior to layoff. Specifically, workers are eligible for three, four, or five months of UI if they have more than 6, 12, or 24 months of accumulated tenure, respectively. In the paper, we pay particular attention to workers with more than 24 months of job tenure at layoff (about 40% of UI beneficiaries). This is because their eligibility can be assessed in both

[13] For instance, payroll taxes are over 35%. The Web Appendix provides more information on labor legislation.

[14] Self–employed workers (12.1%) and informal employees (9.4%). These shares are also equal within gender, but labor force participation is higher for males (Appendix Figure A.1). Farm employment is mostly informal. The figures use data from PNAD surveys described below. We display formal employment shares on maps in the Web Appendix.

administrative and survey data. However, the patterns driving our main results apply broadly.

The benefit level depends on workers' average wage in the three months prior to layoff. The replacement rate starts at 100% at the bottom of the wage distribution but is already down to 60% for workers who earned three times the minimum wage. We show the full schedule in the appendix. Importantly, all our results are robust to excluding beneficiaries with very high replacement rates.

In theory, UI beneficiaries must actively search for a new job. However, there was no monitoring of search efforts until at least 2011. A worker applied in person for UI benefits in the first month only. Payments were then automatically deposited at Caixa Economica, an official bank, every 30 days as long as the worker's name did not appear in a database where employers report new hirings monthly. In a companion paper, we argue that our results may also rationalize this complete absence of monitoring (Gerard and Gonzaga, 2013*b*). Finally, UI is financed by firms' payments of a .65% tax on total sales, instead of a payroll tax, as is common in other countries.

1.3 Data

In this paper, we mainly exploit two very large restricted–access administrative datasets. RAIS (Relação Anual de Informações Sociais) is a longitudinal matched employee–employer dataset covering by law the universe of formal employees, including public employees. Every year, tax-registered firms must report every worker formally employed during the previous calendar year.[15] RAIS includes information on wage, tenure, age, gender, education, sector of activity, establishment size and location, hiring and separation dates, and reason for separation. We are the first researchers to be granted access to the second administrative dataset, the registry of all UI payments (month and amount). Individuals in both datasets are identified through the same social security number (PIS). We currently have consistent data from 1995 to 2010. In 2009, there were about 40,300,000 formal employees and 625,650 new UI beneficiaries in each month.

Combining the two datasets, we know whether any displaced formal employee is eligible for UI, how many UI payments she draws, and when she is formally reemployed. However, the UI data we received from the Labor Ministry has one limitation. Until 2006, the data do not report UI payments from the second year if the UI spell of a worker spans two consecutive years. We therefore restrict attention to workers laid off in the first five months of the year to avoid right–truncated UI spells. We show in the paper that this restriction is unlikely to drive our results.[16]

[15]The main purpose of RAIS is to administer a federal wage supplement (Abono Salarial) to formal employees. There are thus incentives for truthful reporting. RAIS is also used by ministries managing other social programs in order to monitor formal job take-up. RAIS has a somewhat better coverage of formal employment than the data used by the UI agency (MTE, 2008). Accordingly, a few workers who are reported as formally reemployed in RAIS are observed drawing benefits in the UI data. As a result, we slightly overestimate efficiency costs in the paper.

[16]Formal reemployment patterns based on RAIS are similar for workers laid off throughout the year. This is also true for UI benefit collection patterns after 2007, when our UI data no longer suffer from the same limitation.

In the administrative data, we observe the formal reemployment of displaced formal employees. We estimate their overall (formal and informal) reemployment using the longitudinal structure of monthly urban labor force surveys (PME, Pesquisa Mensal de Emprego, 2003–2010), which cover the six largest urban areas of Brazil[17] and are used to compute official employment statistics. Using PME, we can also gauge workers' need for insurance by comparing disposable income levels across (re)employment status. Finally, we use nationally–representative yearly household surveys (PNAD, Pesquisa Nacional por Amostra de Domicílios, 1992–2009) to construct yearly measures of labor market composition in each of the 27 Brazilian states. Both surveys are conducted by the Instituto Brasileiro de Geografia e Estatística (IBGE). They ask for the labor market status of every household member above ten years old, including information on wage, tenure, and the signing of the working card. The unemployed are asked similar information about their last job (except the wage), the reason for separation, and the length of their unemployment spell. There is no information on UI benefits. Moreover, we can only assess UI eligibility for workers with more than 24 months of job tenure at layoff because the surveys do not ask about other previous jobs.[18]

2 Conceptual framework

This section presents the conceptual framework that guides our empirical analysis. We adapt a canonical partial–equilibrium framework of optimal UI with endogenous search efforts and liquidity constraints (Baily, 1978; Chetty, 2006). First, we introduce informal work opportunities. Second, we consider changes in maximum benefit duration instead of changes in benefit levels because of our empirical application (Schmieder, von Wachter and Bender, 2012). Changes in maximum benefit duration also capture the introduction of a UI program (from zero to a positive value). The framework allows us to identify sufficient statistics that specify the typical tradeoff between workers' need for insurance and the efficiency cost from distorting their reemployment decisions. Importantly, estimating the efficiency cost only requires information on formal reemployment, which is available in administrative data. We conclude by discussing how the magnitude of the sufficient statistics may vary with labor market composition and (in)formality.

As discussed in Chetty (2006), the set of sufficient statistics capturing our tradeoff is robust to many assumptions regarding the underlying model of job search. Therefore, we focus on the intuition for our results. A specific model is presented in the Appendix. We discuss mechanisms

[17]Belo Horizonte, Porto Alegre, Recife, Rio de Janeiro, Salvador, and São Paulo.

[18]PME has a similar structure as the Current Population Surveys (CPS) in the US. Households enter the sample for two periods of four consecutive months, eight months apart from each other. There exist PME surveys prior to 2003, but they cannot be used for our main purpose because UI eligibility can only be assessed for those who are actively searching for a new job. Using such a sample would overestimate reemployment rates of displaced formal employees. PNAD surveys were not conducted in 1994 and in census years (2000 and 2010). Finally, to guarantee confidentiality, it is not possible to link respondents in PME and PNAD to other sources of information, even for the government.

beyond the scope of our framework (e.g., general equilibrium effects, externalities) in section 7.

Workers' problem. We consider the optimal behavior of a representative worker who cycles in and out of formal employment. In the formal sector, she earns a fixed wage w^f in each period and pays a tax τw^f, which is used exclusively to finance UI.[19] She faces a fixed layoff probability q such that formal employment spells last on average $D^f = \frac{1}{q}$ periods. Upon layoff, she is eligible for a fixed level of UI benefits $b \equiv rw^f$, with replacement rate r, for a maximum duration of P periods. The incidence of taxes and benefits is assumed to fall on workers. When all jobs are formal, as in the standard Baily–Chetty framework, the worker's problem is to choose how much to search for a new job in each period by trading off utility gains from reemployment and convex search costs. The incentive problem is that UI may induce lower search efforts and prolong unemployment spells.

Figure IIa illustrates how the choice situation of a displaced formal worker in our context differs from the standard Baily–Chetty framework. An unemployed worker can search for a formal job, but also for an informal job. In fact, UI increases her return from finding an informal job because she can draw UI benefits while working informally. Relatedly, upon finding a job that would otherwise be formal, she can ask her employer to not report her hiring immediately.[20] In both cases, she would be willing to trade off the gains from drawing UI benefits against some resource costs such as lower wages or hiding costs. These additional margins of behavioral responses have raised concerns that the UI incentive problem may be more severe in developing countries.

The worker's problem is to choose optimal levels of search efforts for formal and informal jobs (and hiding efforts) in each period until she returns to a formal job and a new cycle begins. Even though her choices may be unobservable, the solution to this problem determines some observable outcomes. In particular, it determines survival rates out of formal employment in each period t after layoff (S_t), the average duration between formal jobs ($D^o \equiv \sum_{t=1}^{\infty} S_t$), the average benefit duration ($B \equiv \sum_{t=1}^{P} S_t$), the average cost per UI spell (bB), and the steady–state share of formal employees ($\frac{D^f}{D^f+D^o}$). We now examine the impact of a change in maximum benefit duration on these outcomes.

Mechanical and behavioral costs of longer UI benefits. Increasing maximum benefit duration (dP) increases UI costs through two channels. This is illustrated in Figure IIb.[21] First, there is

[19]We discuss fiscal externalities in section 7. UI programs are typically funded by a tax on formal wages. This is not the case in Brazil. We focus here on the more typical financing of UI for our framework to apply beyond the Brazilian case. If the incidence of sales and labor taxes are similar, the fact that UI is financed through a sales tax in Brazil does not alter the analysis. If the incidence of a sales tax falls on buyers (resp. sellers) instead of workers, g^E below becomes the social value of $1 for the average buyer (resp. seller) of formal goods and services.

[20]Here is a quote found on *Yahoo Answers!* (March 3rd, 2011; our translation). Question: I am starting in a new firm (...) and I have five days left before receiving another month of UI. If I am registered before I get my next payment, do I lose the UI this month? (...) Response: In general, when receiving UI benefits, employees talk to the employer, asking him to sign the working card after the employee has received all UI payments as a *favor*. Most firms agree without any problem. Some do not, fearing that the Labor Ministry will find out (...). Talk to your boss and say: "Look, I would like to negotiate with you about registering my working card. The thing is that I still have the last month of my UI to receive on day X and I would not like to miss it. Would you mind registering my card after that date?" (...)

[21]It is straightforward to construct a similar figure for the case of the introduction of an UI program. Note that fol-

a *mechanical cost*. Some workers would have remained without a formal job after exhausting their UI benefits even in absence of the reform. Whether unemployed or informally employed, they would draw the extra benefits without changing their behavior, increasing average benefit duration by S_{P+1} and UI costs by bS_{P+1}. Second, there is a *behavioral cost*, which is the cost due to behavioral responses. Increasing maximum benefit duration reduces incentives to be formally reemployed. As a result, survival rates out of formal employment may shift upward, increasing average benefit duration by $\sum_{t=1}^{P+1} \frac{dS_t}{dP}$ and UI costs by $b\sum_{t=1}^{P+1} \frac{dS_t}{dP}$. The behavioral cost may be due to workers choosing to stay unemployed or to work informally. Finally, the average duration between formal jobs will change by $\sum_{t=1}^{\infty} \frac{dS_t}{dP}$, affecting the steady–state share of formal employees.

From impacts to welfare effects. We can now derive the effects of a change in maximum benefit duration on welfare W, assuming that workers behave optimally and that the UI budget must be balanced in expectations. We focus on the steady state (Landais, Michaillat and Saez, 2010).

In each period, a share $\frac{D^f}{D^f+D^o}$ is formally employed, a share $q\frac{D^f}{D^f+D^o}$ becomes eligible for UI, and a share $q\frac{D^f}{D^f+D^o}B$ draws UI benefits. The balanced–budget constraint requires:

$$\frac{D^f}{D^f+D^o}\tau w^f = q\frac{D^f}{D^f+D^o}Bb \quad \Rightarrow \quad \tau = qrB \tag{1}$$

Equation (1) implies that an increase in maximum benefit duration increases the UI tax through its impact on average benefit duration: $\frac{d\tau}{dP} = qr\frac{dB}{dP}$.[22] Such a tax increase will entail a welfare loss for formal employees. This welfare loss will be compensated by a welfare gain for *mechanical beneficiaries*, who would draw the additional benefits without changing their behavior. In contrast, there are no first order welfare gains from behavioral responses, such as changes in search efforts. This is a standard envelope argument. Behavioral responses only matter through their effect on the UI budget (Chetty, 2006). The welfare effects of increasing maximum benefit duration are thus:

$$\frac{dW}{dP} = q\frac{D^f}{D^f+D^o}rw^f\left[S_{P+1}\left(\frac{U_{P+1}}{S_{P+1}}g^{U_{P+1}} + \frac{S_{P+1}-U_{P+1}}{S_{P+1}}g^{I_{P+1}}\right) - \left(S_{P+1} + \sum_{t=0}^{P+1}\frac{dS_t}{dP}\right)g^E\right] \tag{2}$$

lowing Schmieder, von Wachter and Bender (2012), we assume that the maximum benefit duration P can be increased by a fraction of one, such that a marginal change (dP) can be analyzed. A marginal change in P then corresponds to a marginal change in b_{P+1}, the benefit amount after regular UI exhaustion, times b.

[22]The change in the duration between formal jobs dD^o has no effect on the UI tax rate. If D^o increases, the number of individuals paying UI taxes decreases, but the number of future beneficiaries also decreases. The two effects on the UI budget cancel out in the steady–state. In Chetty (2008) and Schmieder, von Wachter and Bender (2012), the change in the duration between formal jobs increases the UI tax rate because new jobs are never lost (only the first effect matters). We adopt a steady–state approach (infinite horizon) because a significant share of workers who are formally reemployed experience a new layoff in the following months in Brazil. We show empirically that longer UI benefits reduces the number of months of formal employment after layoff but also the share experiencing a new layoff from the formal sector (Appendix Table A.11). To focus on the main concern of moral hazard, we also follow the literature by assuming a fixed layoff probability q. This assumes sufficient experience–rating of benefits such that changes in UI have no effect at the layoff margin. We discuss the empirical relevance of this assumption in section 3.1.

The first of the two terms in brackets captures the welfare gain for mechanical beneficiaries. The ratio $\frac{U_{P+1}}{S_{P+1}}$ is the share of *unemployed* mechanical beneficiaries, with U_t the survival rates out of any job. $g^{U_{P+1}}$ and $g^{I_{P+1}}$ are the average social value of \$1 for unemployed and informally employed mechanical beneficiaries, respectively. The second term captures the welfare loss for formal employees who pay for both the mechanical and the behavioral cost. g^E is the average social value of \$1 for formal employees. Without welfare weights, social values correspond to average marginal utilities. The expression in brackets is multiplied by a scale factor, the share of workers becoming eligible for UI each period times the benefit level. Reorganizing, we express welfare effects in a money metric, the welfare effects of a percentage change in the formal payroll (Chetty, 2008):

$$\frac{d\widetilde{W}}{dP} = \frac{1}{\frac{D^f}{D^f+D^o}} \frac{dW/dP}{g^E w^f} = qr\, S_{P+1} \left[\left(\frac{U_{P+1}}{S_{P+1}} \frac{g^{U_{P+1}} - g^E}{g^E} + \frac{S_{P+1} - U_{P+1}}{S_{P+1}} \frac{g^{I_{P+1}} - g^E}{g^E} \right) - \widetilde{\eta} \right] \tag{3}$$

where $\widetilde{\eta} \equiv \sum_{t=1}^{P+1} \frac{dS_t/dP}{S_{P+1}}$ is the ratio of the behavioral cost to the mechanical cost.

Equation (3) highlights the trade–off between insurance and efficiency. The first of the two terms in brackets captures the *social value of insurance*, or the social value of transferring \$1 from the average formal employee to the average mechanical beneficiary. The social value of such a transfer arises from market incompleteness. Intuitively, it will be decreasing in the share of beneficiaries working informally and in the degree of insurance provided by informal wages. The second term, the ratio of the behavioral to the mechanical cost (the resources lost for each \$1 reaching target beneficiaries), captures the *efficiency cost*. This is a standard result in public economics (e.g. Hendren, 2013). Importantly, one does not need to separately estimate impacts on unemployment and informal employment (or other margins of behavior) to estimate the efficiency cost. The overall impact on average benefit duration works as a sufficient statistic. This result is related to Feldstein (1999), who shows that the taxable income elasticity is a sufficient statistic for the efficiency cost of taxation, even in the presence of tax evasion (or informality).[23]

Neither the social value of insurance nor the efficiency cost is a structural parameter. Evaluating equation (3) around an existing UI program, however, provides a local welfare test. Moreover, under some assumptions, the sign of equation (3) tells us whether the optimal maximum benefit duration is above or below the existing one.[24] In the paper, we estimate the efficiency cost using

[23] The argument assumes that the private and social costs of evasion (or informality) are the same (Chetty, 2009). In our case, the costs include lower informal wages and other losses incurred in hiding some economic activities. If some behavioral responses do not imply any resource cost (e.g., costless reporting behaviors), they do not generate any efficiency cost, and our measure of efficiency is an upper bound. As argued by Best et al. (2013), considering evasion costs as social costs is a natural starting point for developing countries. In fact, concerns that UI may encourage informal work would be irrelevant if such behavioral responses were socially costless.

[24] This holds if the social value of insurance (resp. the efficiency cost) is monotonically decreasing (resp. increasing) in maximum benefit duration. It may be the case in our setting. For instance, we show that the share of UI beneficiaries working informally increases in the months after layoff, potentially reducing the need for insurance. Moreover, we

administrative data on formal reemployment and quasi–exogenous variation in maximum benefit duration. We then use longitudinal survey data to investigate what share of UI beneficiaries remain unemployed. The same survey data provide suggestive evidence on the need for insurance.

Efficiency and (in)formality. The conventional wisdom is that the efficiency cost will be larger in the presence of informal work opportunities, which are more prevalent in developing countries. Consider the illustration in Figure IIb. In the worst case scenario, all workers will delay formal reemployment in response to longer UI benefits. The area above pre–reform survival rates up to the new maximum UI duration thus constitutes a *maximum behavioral cost*. The ratio of that area to the mechanical cost measures a *maximum efficiency cost* or a potential distortion. Evidence from Europe and the US shows moderate behavioral responses to UI incentives, such that the efficiency cost is much smaller than this upper bound.[25] Yet, it may be easier to respond to UI incentives when it is easier to find informal work opportunities. The efficiency cost of longer UI benefits may turn out to be relatively large compared to the maximum efficiency cost in developing countries.

This does not necessarily imply that the efficiency cost is larger where informal jobs are prevalent. Displaced formal employees may not return rapidly to a formal job in such labor markets, and may work informally, even in the absence of (longer) UI benefits. On the one hand, it may be costly to find formal jobs (*exclusion* view). On the other hand, informal jobs may be attractive for reasons unrelated to UI (*exit* view). Survival rates out of formal employment may then decrease slowly absent behavioral responses. As a result, the maximum efficiency cost may be limited and the efficiency cost may be small even if it is large compared to the maximum efficiency cost.

How the efficiency cost of longer UI benefits varies with labor market formality (i.e. the share of formal employees and informal workers) is thus an empirical question. It depends on the correlation between labor market formality and workers' costs and benefits of finding formal and informal jobs. For instance, it may be easier to find a formal job in more formal labor markets, which would increase the maximum efficiency cost. In contrast, it may be harder to find an informal job, which would limit workers' ability to respond to UI incentives. The sign of the correlation between labor market formality and the efficiency cost will depend on which of these two effects dominates.

In the paper, we use variation across Brazilian states and over time to explore the empirical relationship between labor market formality and the efficiency cost of longer UI benefits. The results help rationalize our main finding, which contradicts the view that the efficiency cost ought to be larger in a context of high informality. The social value of insurance may also vary with labor market formality.[26] We provide some evidence on workers' need for insurance, but we are unable

show that an upper bound on the efficiency cost increases with maximum benefit duration.

[25] Even if not formulated as such, it is straightforward from, e.g., Card and Levine (2000), Card, Chetty and Weber (2007*a*), Chetty (2008), Katz and Meyer (1990), Landais (2012), or Schmieder, von Wachter and Bender (2012). For instance, the maximum behavioral cost of increasing maximum UI duration from 26 to 39 weeks is 26.7 weeks in Table 4 of Katz and Meyer (1990). The behavioral cost is 1.2 weeks and the mechanical cost .9 weeks.

[26] The social value of insurance may be lower if liquidity constraints are less binding (markets more complete). It

to examine this relationship and therefore to draw a clear conclusion regarding the relationship between welfare effects and (in)formality. Yet, our results imply a shift in the policy debate from a concern about efficiency to a question of workers' actual need for insurance.

3 A first look at the data

In this section, we highlight the main differences in UI outcomes between Brazil and typical developed countries. First, almost all UI beneficiaries exhaust their benefits in Brazil. Second, displaced workers' very low rates of formal reemployment, which drive the high exhaustion rates, increase after benefit exhaustion. This suggests a clear behavioral response to UI incentives. However, formal reemployment rates are relatively low even after benefit exhaustion or even for displaced workers not eligible for UI. As a result, most of the cost of (longer) UI benefits is driven by a large mechanical cost. The maximum behavioral cost is comparatively small and the maximum efficiency cost is thus limited. Third, formal reemployment rates after benefit exhaustion, and thus the maximum efficiency cost, are larger in labor markets with a higher share of formal employees and a lower share of informal workers. We investigate whether the actual efficiency cost increases with labor market formality in section 4. We investigate whether UI beneficiaries work informally and whether behavioral responses are due to workers' choosing to work informally in section 5.

3.1 Average benefit duration: illustration of the main results

Figure III illustrates our main results. Figure IIIa displays average benefit duration by job tenure at layoff for a random sample of UI takers, 18–54 years old, laid off between 1995 and 2009 from a full–time private–sector job. Take–up rates are around 80% (Appendix Figure A.3a). For the sake of clarity, the sample is restricted to workers who had a single formal job in the previous 36 months.[27] Their UI eligibility only depends on job tenure at layoff. They are eligible for three, four, or five months of UI if they have more than 6, 12, or 24 months of job tenure, respectively. Note that we imperfectly assess eligibility for tenure levels just below the eligibility cutoffs for two reasons. 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

may be higher if it becomes more difficult for workers to use informal work opportunities as a self–insurance device.

[27] They account for 62% of UI beneficiaries in the sample. Samples in the paper always exclude workers laid off in 2000 and 2010. PNAD surveys used to create measures of labor market composition were not carried out in these census years and data from the censuses are not directly comparable. Samples also exclude brief periods of temporary UI extensions, such as the one described in section 4.1. Finally, samples only include workers displaced in the first five months of every year because of the limitation in the UI data discussed in section 1.3. We show in the Web Appendix that the patterns displayed in Figure III are unlikely to be driven by this sample restriction. We reproduce Figure III for workers displaced in the first five months and in the next seven months of the year after 2007, when our UI data no longer suffer from the same limitation. The two panels are almost identical.

period. We cannot separately identify these cases in the data and the advance notice period counts for UI eligibility. Second, 15 days of tenure count as one month for UI eligibility.

Strikingly, average benefit duration is almost exactly equal to maximum benefit duration in Brazil. In other words, almost all UI takers exhaust their benefits, even those eligible for five months of UI. Moreover, we obtain similar patterns when excluding workers at the bottom of the wage distribution, who have very high replacement rates (see Web Appendix). Exhaustion rates are much lower in developed countries. For instance, exhaustion rates were around 35% over the same period in the US, where the average beneficiary was eligible for about 24 weekly UI payments.[28]

Average benefit duration may be very high in Brazil for two reasons. On the one hand, behavioral responses to UI incentives may be particularly large where informal work opportunities are prevalent. On the other hand, average benefit duration may be mechanically high if workers are unable or unwilling to return to a formal job rapidly, absent behavioral responses. Both cases are consistent with the data, but they have polar implications in terms of efficiency.

Figure IIIb provides some first evidence that the latter case best applies in Brazil. We use data on the timing of formal reemployment for the same workers to predict benefit duration had they been eligible for three to six months of UI. We assume that they would draw a n^{th} month of UI if they were not formally reemployed n months after layoff. Such counterfactuals capture a behavioral and a mechanical cost for workers' true maximum benefit duration, but only a mechanical cost for hypothetically longer UI benefits. We construct similar counterfactuals for workers with less than six months of job tenure at layoff, who are not eligible for UI. In this case, our sample includes workers who would not take up UI if eligible and our counterfactuals are likely lower bounds.[29]

Our counterfactuals, which successfully match average benefit duration for workers' true maximum benefit duration, imply that workers would *mechanically* draw most UI payments following an increase in maximum benefit duration. Currently ineligible workers would draw 2.85, 3.72, or 4.55 UI payments absent behavioral responses if they were eligible for three, four, or five months of UI, respectively. The associated efficiency cost would be at most 5 cents, 7.5 cents or 10 cents per $1. Moreover, a one–month increase in maximum benefit duration would mechanically increase average benefit duration by .91, .88, or .84 month for workers eligible for three, four, or five months of UI, respectively. As a result, the potential distortion is limited. Any behavioral cost of longer UI benefits will be small compared to the mechanical cost. The efficiency cost of (longer) UI benefits will thus be limited, however large it is compared to the maximum efficiency cost.

We show similar patterns for workers who had several formal jobs in the previous 36 months

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

[29] Some workers who had just below six months of job tenure at layoff are eligible for UI because of the same two rules explained previously (Appendix Figure A.3a).

in Appendix Figure A.4. Their UI eligibility depends on the accumulated tenure across all formal jobs in the 36 months prior to layoff. We focus on workers with a single formal job in the previous 36 months because our assessment of UI eligibility gets noisier with each previous formal job.

Another concern with UI is that it creates incentives for firms and workers to increase layoff rates (Feldstein, 1976). Such a concern is partly founded in the Brazilian case (Appendix Figure A.5). Layoff rates increase discontinuously at six months of job tenure when formal employees become eligible for UI, and decrease discontinuously at 12 months of job tenure when firing costs increase.[30] Layoff rates at other cutoffs for UI eligibility are smooth. To focus on the main concern of moral hazard, we follow the recent literature in Public Economics (Chetty, 2006) and abstract from the impact of UI on layoffs in this paper.[31] For our purpose, note that the mechanical cost of longer UI benefits is large, and the efficiency cost thus limited, for workers with tenure levels between 6 and 12 months, even though some of them may have been strategically laid off.

3.2 Formal reemployment patterns

The high average benefit duration and the large mechanical cost in Figure III are driven by displaced workers' low rates of formal reemployment, even after benefit exhaustion or even for displaced workers not eligible for UI. This is illustrated in Figure IV. The black lines display hazard rates of formal reemployment and survival rates out of formal employment for UI takers from our random sample who have more than 24 months of job tenure at layoff (44% of the sample). We pay particular attention to these workers in the paper because their eligibility, five months of UI, can be assessed in both administrative and survey data. We also restrict attention to a period of sustained economic growth (2002–2009) to strenghten our conclusions.

UI exhaustion rates are high because hazard rates of formal reemployment are very low, below 4% a month, when workers are eligible for UI. Hazard rates then spike to 12% a month, but they stay relatively low. As a result, 50% of the sample remain without a formal job 12 months after layoff. The spike after benefit exhaustion suggests a clear behavioral response to UI incentives.[32] In section 4, we show that the spike shifts following quasi–exogenous increases in maximum benefit duration. Nevertheless, it is clear from Figure IVb that any behavioral cost of longer UI benefits

[30] 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 (unpaid wages, overtime compensation, etc.).

[31] This is because the optimal policy to tackle such distortions is well–known and consists in introducing experience–rating of UI benefits, or a layoff tax (Blanchard and Tirole, 2006). The decrease in layoff rates at 12 months of job tenure suggests that such a policy would be effective. The Brazilian government has in fact considered its introduction (*O Globo*, February 11, 2014). Finally, note that layoff rates increase at another UI eligilibility cutoff, when workers have just 16 months between their layoff date and the layoff date of their last successful application (not shown).

[32] Such a spike is not observed in most developed countries (Card, Chetty and Weber, 2007*b*). van Ours and Vodopivec (2006) find a sizeable spike in Slovenia.

will be small compared to the mechanical cost. The efficiency cost will thus be limited.

In the Web Appendix, we show similar patterns for workers eligible for three, four, or five months of UI, and who had a single or several formal job(s) in the previous 36 months.[33] Finally, note that we are the first to document those patterns because one cannot estimate hazard rates of formal reemployment using survey data. This is because one cannot observe later transitions to formal jobs once workers are reemployed informally. Existing panels are too short and questions about past employment and unemployment spells are not asked to (informally) employed individuals. The same limitations apply to other middle–income and developing countries (e.g. Mexico).

3.3 Labor market heterogeneity

So far, we have considered displaced formal employees from all Brazilian states in all sample years. However, Brazil is an extremely diverse country, where labor market formality (e.g. the share of formal employees and informal workers) varies greatly across states and over time. We show here that the patterns described above apply broadly, although with some interesting heterogeneity.

This is first illustrated by the grey lines in Figure IV. They compare Pernambuco, a poorer state in the North–East with a less formal labor market, and Rio Grande do Sul, a richer state in the South with a more formal labor market. The efficiency cost of longer UI benefits would be limited in both states. Hazard rates of formal reemployment are very low while workers are eligible for UI; they increase after benefit exhaustion; but they remain low. Yet, hazard rates increase more in Rio Grande do Sul. Consequently, the maximum efficiency cost of longer UI benefits is larger there because of a smaller mechanical cost and a larger maximum behavioral cost.

We generalize these results in Appendix Figure A.6 using the whole random sample of UI takers. The panels plot the average of individual outcomes by state and year against the respective formal employment rate, the share of non–farm private formal employees in the working–age population from PNAD surveys. We measure the difference between actual and maximum benefit duration in the UI data. We capture the mechanical cost and the maximum behavioral cost of a hypothetical two–month increase in maximum benefit duration as follows. We assume that workers would draw one extra month of UI (resp. two extra months of UI) if they are not formally reemployed within one month (resp. two months) of exhausting their regular UI benefits.[34] It is then

[33] Most results in the paper are based on samples of workers displaced in the first five months of every year because of limitation in our UI data discussed in section 1.3. We show in the Web Appendix that the patterns displayed in Figure IV are unlikely to be driven by this sample restriction. First, we reproduce the main pattern in Figure IVb without restricting the sample to UI takers. This allows us to compare survival rates out of formal employment for workers displaced in the first five months and in the next seven months of the year. They are almost identical. Second, we reproduce Figure IVb, comparing workers displaced in the first five months and in the next seven months of the year after 2007, when our UI data no longer suffer from the same limitation. The patterns are almost identical.

[34] We follow the same assumption throughout the rest of the paper to construct counterfactual benefit durations of longer UI benefits. Using this assumption, we successfully match the average benefit duration as measured using UI

straightforward to estimate the mechanical cost, the maximum behavioral cost (not shown), and the maximum efficiency cost (the ratio of the mean of the first two outcomes). The average benefit duration is close to the maximum benefit duration in every state and year. A two–month increase in maximum benefit duration would have been mechanically costly everywhere, increasing average benefit duration from 1.2 to 1.8 months. As a result, the maximum efficiency cost would have been limited, from 20 cents to 80 cents per $1 reaching mechanical beneficiaries. The mechanical cost is decreasing and the maximum efficiency cost is increasing in the share of formal employees. This is because of the larger increase in hazard rates of formal reemployment after benefit exhaustion.

Table I further investigates the relationship between labor market composition and UI outcomes using the whole random sample of UI takers. We first assess the robustness of the above correlations using the following specification for outcome y of individual i (top panel):

$$y_{i,s,t} = \alpha_s + \beta_t + \gamma\, ShareFormalEmployees_{s,t} + X_{i,s,t} + \varepsilon_{i,s,t}, \tag{4}$$

where $ShareFormalEmployees$ is the demeaned share of non–farm private formal employees in state s and year t. We present OLS estimates $\widehat{\gamma}$ from specifications with and without state fixed effects (α_s), year fixed effects (β_t) and a rich set of individual controls ($X_{i,s,t}$). Standard errors $\varepsilon_{i,s,t}$ are clustered by state.[35] We consider five outcomes. Takeup rates and benefit duration come from the UI data. The mechanical cost and the maximum behavioral cost of a hypothetical two–month increase in maximum benefit duration are measured as above. The maximum efficiency cost is not an average of individual outcomes. The estimated $\widehat{\gamma}$'s then result from nonlinear combinations of coefficients from separate regressions for the mechanical and the maximum behavioral cost.[36]

The correlations between formal employment rates and UI outcomes are robust relationships in the data. We first use the whole variation across states and over time, as in Appendix Figure A.6 (col. 1). Our results are not due to national trends or fixed differences across states. They hold when we include year and state fixed effects (col. 2 and 3). Moreover, they are not due to simple composition effects or to a differential selection into UI takeup. Our results are robust to adding a rich set of individual controls and we find no evidence of a correlation between UI takeup and formal employment in this case (col. 4).[37] The results are also robust to focusing on a period of

data of workers who were actually eligible for longer UI benefits (Appendix Table A.12).

[35]Significance levels are similar if we bootstrap t–statistics (not shown). In an earlier version of the paper, we showed some correlations using more disaggregated labor markets (Gerard and Gonzaga, 2013*a*). However, labor market shares of formal and informal workers are only available at this level of disaggregation in census years. Finally, we always use different labor market shares for men and women given the differences highlighted in Figure A.1.

[36]We reproduce marginal effects for the maximum efficiency cost using a one percentage point increase in formal employment. Because this is a nonlinear combination, the effect would be proportionally larger for larger increases in formal employment. Standard errors are obtained by the delta method.

[37]Because of the 30–day waiting period, we would expect a negative correlation if hazard rates of formal reemployment are correlated with local formal employment rates.

sustained economic growth, to excluding workers with very high or very low replacement rates, or to excluding the least and most formal Brazilian states (Appendix Table A.1).

A correlation between formal employment rates and UI outcomes is an equilibrium relationship between two endogenous variables, not a causal relationship. The same is true for the correlation between (un–)employment rates and UI outcomes studied in developed countries (e.g. Schmieder, von Wachter and Bender, 2012). However, the underlying forces behind our correlations are likely to be very different whether variations in formal employment are due to variations in, e.g., informal employment vs. unemployment. For instance, workers' ability to respond to UI incentives may decrease with a decrease in the share of informal workers. To study changes in labor market formality, rather than overall changes in formal employment, we use the following specification:

$$y_{i,s,t} = \alpha_s + \beta_t + \sum_l \gamma_l \; LaborMarketShare_{l,s,t} + X_{i,s,t} + \varepsilon_{i,s,t}. \quad (5)$$

where $LaborMarketShare_{l,s,t}$ is the demeaned share of labor status l in state s and year t. We consider the six categories of labor status in Figure Ia. An increase in a share l is necessarily associated with a decrease in a share $l' \neq l$. In specification (4), an increase in formal employment comes at the expense of all other categories of labor status combined. In specification (5), a linear combination $\gamma_l - \gamma_{l'}$ allows us to consider a change in the share of formal employees associated with a change in the share of a specific labor status such as the share of informal workers, thus a change in labor market formality. We display some combinations of interest in Table I (bottom panel) using our preferred specification with state and year fixed effects, and individual controls.

The mechanical cost of a two–month increase in maximum benefit duration decreases with labor market formality (col. 2). As a result, the maximum behavioral cost and the maximum efficiency cost increase. Our estimates imply that the maximum efficiency cost increases by 8.2% following a 10% increase in the share of formal employees (2.9 pp), when associated with a decrease in the share of informal workers. We obtain similar estimates if we split informal workers between informal employees (7.1%, col. 3) and self–employed workers (10%, col.4) in specification (5). To provide some benchmark, the magnitudes are about half as large as for an increase in formal employment associated with a decrease in unemployment (col. 1). Once again, our results are not due to simple composition effects or to a differential selection into UI takeup. They are robust to not including individual controls or to considering the same robustness checks as for the results in the top panel (Appendix Table A.2). Importantly, we find no systematic relationship between the estimated coefficients for UI takeup and other UI outcomes among the many specifications.

Displaced formal employees return more rapidly to a formal job after exhausting their UI benefits in more formal labor markets.[38] As a result, the maximum efficiency cost of longer UI benefits

[38] Displaced formal employees who are not eligible for UI also return more rapidly to a formal job (not shown). In

is larger. We investigate in the next section whether this translates into a larger efficiency cost.

4 The behavioral cost and efficiency cost of longer UI benefits

In this section, we use quasi–exogenous variation in maximum benefit duration to estimate the behavioral cost and efficiency cost of longer UI benefits. First, we exploit a temporary UI extension in some Brazilian cities. We show that the spike in hazard rates of formal reemployment after benefit exhaustion (Figure IVa) is due to behavioral responses. However, the associated behavioral cost is small compared to the mechanical cost. As a result, the efficiency cost is limited, even though it is large compared to the maximum efficiency cost. Second, we use a regression discontinuity design around the 24–month tenure cutoff, displayed in Figure III. The eligibility cutoff provides variation in maximum benefit duration across workers in every state and year. This allows us to estimate how the efficiency cost varies with labor market formality. In Table I, we showed that the potential distortion (maximum efficiency cost) increases with labor market formality. Yet, it may also become harder for a given worker to actually respond to UI incentives. We find that the efficiency cost increases with labor market formality because the first effect dominates.

4.1 A temporary UI extension in some Brazilian cities

In 1996, beneficiaries who exhausted their regular benefits between September and November became eligible for two additional months of UI in some Brazilian cities. The extension was politically motivated and the selection of cities was mostly unrelated to local labor market conditions.

A UI extension limited to the city of São Paulo was first proposed to the President by José Serra, a politician from the same political party (PSDB) who was failing in his run for mayor of São Paulo. Serra justified his proposal by the rising unemployment in the city. In response, unions defended an extension in all cities, "not only where the PSDB candidate is doing badly" (Folha de São Paulo, 08/22/1996). Since 1994, the committee managing the UI fund, which includes representatives of the government, the unions and the employers, is allowed to extend UI for selected groups of workers for up to two months. The committee can extend UI without prior Congressional approval as long as the extenstion does not cost more than 10% of the UI fund's liquidity reserves. In 1996, the proposition of the unions was rejected because it would have cost more than this threshold and would have required Congressional approval, a slow process. As a compromise, the extension was implemented in Brazil's nine historical metropolitan areas and the Federal District.[39]

the Web Appendix, we present some individual heterogeneity in UI outcomes. Importantly, the maximum efficiency cost of longer UI benefits increases with beneficiaries' current maximum benefit duration because the mechanical cost decreases and the maximum behavioral cost increases.

[39]They were the only metropolitan areas at the time: Bélem, Belo Horizonte, Curitiba, Fortaleza, Porto Alegre,

Figure Va shows that unemployment was trending similarly in selected cities, São Paulo aside, and in all other Brazilian cities. In São Paulo, unemployment rose sharply between 1996 and 1997, suggesting particularly poor labor market conditions at the time. Figure Vb summarizes the timeline of the UI extension. The extension was first proposed on August 14. It was adopted a week later, to start on September 1, 33 days before the first round of local elections. The extension was strictly temporary; no additional benefit would be paid after December 31. With our administrative data starting in 1995, we can only assess UI eligibility for workers with more than 24 months of job tenure at layoff. These workers could draw two extra months of UI, after exhausting their five months of regular benefits, if they were laid off in April or May 1996. The timing of the UI extension guarantees that they were not strategically laid off at the time.

4.1.1 Empirical strategy and sample selection

We adopt a difference–in–difference strategy. Our sample includes full–time private formal employees, 18–54 years old, laid off in April or May. We have nine treatment areas. We exclude São Paulo because of the differential trend in Figure Va. We use 1995 and 1997 as control years, and the 20 cities that were granted the status of metropolitan area since 1996 as control areas. Appendix Tables A.3 and A.4 present the distribution and composition of our sample. We have about 230,000 workers. There are a few differences between workers from control and treatment areas, but these differences appear every year.[40] Moreover, labor market conditions at the state level were similar around 1996. We cannot compare labor market conditions at the city level because the nine historical metropolitan areas and the Federal District are the only urban areas separately identified in PNAD surveys. Figure Va shows that unemployment was trending similarly in treatment areas compared to all other Brazilian cities, which include control areas. Worse labor market conditions in treatment areas in 1996 would in fact bias our estimates against finding small efficiency costs.

4.1.2 Graphical results

Our results can be seen graphically. Figure VI displays hazard rates of formal reemployment and survival rates out of formal employment for UI takers in control and treatment areas. Lines trace each other very closely in control areas and control years. In contrast, the spike in formal

Recife, Rio de Janeiro, Salvador, and São Paulo. Note that *"the choice of the first nine metropolitan regions* (in the 1970s) *was more related to the objective of developing an urban system according to the needs of a particular economic development strategy than to contemplating cities with actual characteristics of metropolitan regions. The proof of this claim was that Santos, Goiania and Campinas did not become metropolitan regions at that time, despite meeting some of the most important criteria to be considered a metropolitan area."* (Guimarães, 2004, our translation).

[40] Workers in treatment areas are more likely to be older and to come from the service sector. Treatment areas are relatively larger, constituting 68% of the sample (22% of the sample is composed of workers from Rio de Janeiro). Control and treatment areas are displayed on a map in the Web Appendix. They are similarly spread across the country and they spanned a similar range of formal employment rates in the 2000 census.

reemployment after benefit exhaustion shifts by two months in treatment areas in 1996. As a result, an additional 15% of UI takers were still out of formal employment seven months after layoff.[41] Survival rates in control years and treatment areas suggest a mechanical cost of 1.6 months and thus a maximum efficiency cost of 25 cents per \$1 reaching mechanical beneficiaries. The difference in survival rates between treatment and control years suggests a behavioral cost of .25 month and thus an efficiency cost of 15 cents per \$1, or 60% of the maximum efficiency cost. The efficiency cost is limited not because of small distortions in the behavior of those who otherwise would find a new formal job rapidly, but because most displaced formal employees would not find a new formal job rapidly. In Appendix Figure A.7, we present similar results by treatment area. The efficiency cost is relatively small in every city, from 11 cents to 23 cents per \$1 reaching mechanical beneficiaries. The maximum efficiency cost is larger in cities with more formal employment and this translates into a larger efficiency cost. However, we cannot make any statistical inference with a cross–section of nine treatment cities. Our second empirical strategy will address this limitation.

4.1.3 Regression results

We estimate the following specification for worker i from area m in year t:

$$y_{i,m,t} = \alpha \; TreatArea_m + \beta \; Year1996_t + \gamma \; [TreatArea_m \times Year1996_t] + \varepsilon_{i,m,t}, \tag{6}$$

where $TreatArea$ and $Year1996$ identify a treatment area and a treatment year, respectively. γ is a difference–in–difference estimator for the impact of the UI extension on outcome y under a common–trend assumption. ε is an error term clustered by area.[42]

We report estimates of $\widehat{\gamma}$ in Table II for four different outcomes. UI takeup, regular benefit duration, and extended benefit duration come from the UI data (col. 1, 2, and 3). The outcome in column (4) is a counterfactual benefit duration had every worker in our sample been eligible for seven months of UI. As in section 3.3, we assume that workers would draw one extra month of UI (resp. two extra months of UI) if they are not formally reemployed within one month (resp. two months) of exhausting their regular UI benefits.[43] The mean in control years and areas captures the mechanical cost (after subtracting the regular benefit duration); the difference–in–difference captures the behavioral cost. It is then straightforward to estimate the (maximum) efficiency cost.

We expect and find no effect on UI take–up or regular benefit duration. At the time, beneficiaries drew on average 4.97 months out of their five months of regular UI. The extension increased

[41] Survival rates out of formal employment for UI non–takers present no differential trend, supporting our identifying assumption of a common–trend absent the UI extension (see Web Appendix).

[42] Significance levels are similar if we bootstrap t–statistics by resampling our 29 clusters (not shown).

[43] Using this assumption, we successfully match the average benefit duration as measured using UI data of workers who were actually eligible for the extension (Appendix Table A.12).

benefit duration by 1.86 months. However, only 13% of that increase (.25 month) is due to behavioral responses. Indeed, beneficiaries in control years would have mechanically drawn 1.59 additional months of UI (6.56-4.97), had they been eligible for the extension. Using our estimates, we obtain an efficiency cost of 15.5 cents per \$1 reaching mechanical beneficiaries. In comparison, estimates in Katz and Meyer (1990) imply an efficiency cost of \$1.33 per \$1 reaching mechanical beneficiaries for a 13–week UI extension in the US.[44] The efficiency cost is relatively small in our case, but it is large with respect to the maximum efficiency cost (27 cents per \$1).

Results are robust to including a rich set of individual controls, to excluding workers with very high and very low replacement rates, to excluding observations from Rio de Janeiro, to using either one of the control years, and to performing a placebo analysis around the next local election in 2000 (Appendix Table A.5).[45] Finally, we also consider longer–term outcomes (Appendix Table A.11). We find no effect on subsequent wages in the formal sector. The UI extension decreased the number of months of formal employment in the two years after layoff, but also the share of workers experiencing a new layoff from the formal sector. These last results motivate the choice of a steady state budget constraint in section 2 (infinite horizon).

The efficiency cost of longer UI benefits is limited because most displaced formal employees would not find a new formal job rapidly. Yet, those who would find a new formal job rapidly respond strongly to UI incentives. In section 5, we use survey data to provide evidence that such behavioral responses are likely due to workers' choosing to work informally.

4.2 A tenure–based discontinuity in UI eligibility

In section 3, we showed that displaced formal employees return more rapidly to a formal job after benefit exhaustion in more formal labor markets. As a result, the *maximum* efficiency costs of longer UI benefits (potential distortion) is larger. We investigate here whether the actual efficiency cost increases with labor market formality. The eligibility cutoffs at 6 months, 12 months, and 24 months of job tenure (Figure III) provide variation in maximum benefit duration across workers in every state and year, and thus constitute natural empirical strategies to address our question. We focus on the variation provided by the 24–month cutoff because the layoff density is not smooth through the 6–month and 12–month cutoffs, as explained in section 3.1 (Appendix Figure A.5).

[44] The behavioral cost is 1.2 weeks and the mechanical cost is .9 week (Katz and Meyer, 1990).

[45] The Web Appendix presents some individual heterogeneity.

4.2.1 Sample selection

Our sample includes full–time private formal employees, 18–54 years old, laid off between 1997 and 2009, who had a single formal job in the previous 36 months.[46] Workers with less than 22 months and more than 24 months of job tenure at layoff are eligible for four months and five months of UI, respectively. Workers who had between 22 months and 24 months of job tenure in our data are eligible for four or five months of UI because of two rules discussed in section 3.1.

4.2.2 Graphical results

Figure VIIa displays UI benefit duration averaged by week of tenure. Average benefit duration is constant and close to four months of UI for workers who had less than 22 months of job tenure.[47] It increases to 4.9 months of UI for workers who had more than 24 months of job tenure. As expected, benefit duration is between four and five months of UI for workers who had between 22 and 24 months of job tenure. We estimate how much of the increase in average benefit duration is due to behavioral responses in Figure VIIb. We construct a counterfactual benefit duration had all workers been eligible for five months of UI following the same approach as in sections 3.3 and 4.1. We assume that a worker would draw one additional month of UI if she is not formally reemployed one month after exhausting her fourth month of UI.[48] We plot these counterfactual benefit durations averaged by week of tenure. The mean on the left of the cutoff captures the mechanical cost (after substracting the first four months of benefit duration); the discontinuity at the cutoff captures the behavioral cost. Beneficiaries would mechanically draw 4.8 UI payments if they were eligible for five months of UI. The behavioral cost amounts to only .08 month or 9% of the increase in benefit duration. These patterns are similar to the ones presented in Figure III.

4.2.3 Empirical strategy

We use a regression discontinuity (RD) design around the 24–month cutoff. Let T_i be the normalized job tenure at layoff for worker i such that $T_i = 0$ at the cutoff. Following Card and Lee (2008), we regress an outcome (y_i) on a constant, an indicator for tenure levels above the cutoff ($\mathbb{1}(T_i \geq 0)$),

[46] The sample starts in 1997 because we must observe workers' formal employment in the previous 36 months to assess UI eligibility. We focus on workers with a single formal job in the previous 36 months because our assessment of UI eligibility gets noisier with each previous formal job. Results are qualitatively similar using discontinuities at 12 months and 24 months of accumulated job tenure for workers who had several formal jobs in the previous 36 months.

[47] A few beneficiaries (4.5%) supposedly eligible for four months of UI drew five months of UI.

[48] Using this assumption, we successfully match the average benefit duration as measured using UI data of workers who were actually eligible for five months of UI (Appendix Table A.12).

and a control function in job tenure ($f(T_i)$):

$$y_i = \alpha + \beta\ \mathbb{1}(T_i \geq 0) + f(T_i) + \varepsilon_i, \tag{7}$$

The treatment impact is captured by β, i.e., the change in y at the tenure cutoff, under the assumption that the control function is continuous at $T_i = 0$. In practice, we face two problems. The first problem is standard with RD designs. The control function is unknown. The second problem is specific to our case. We cannot assess the eligibility of workers who had between 22 and 24 months of tenure at layoff. We therefore approximate $f(T_i)$ using linear functions on each side of the cutoff and we exclude workers who had between 22 and 24 months of tenure from the regressions:

$$y_i = \alpha + \beta\ \mathbb{1}(T_i \geq 0) + \gamma\, T_i + \delta\ \mathbb{1}(T_i \geq 0) \times T_i + \varepsilon_i, \quad \text{for } T_i \notin (-2, 0], \tag{8}$$

where the error term ε_i is clustered by week of tenure. The fitted lines on both sides of the cutoff in Figure VII correspond to our identifying assumptions. On the left, the line is fitted to the observations below 22 months of tenure and then extrapolated for tenure levels between 22 and 24 months. The discontinuity at the cutoff captures our estimated impact of the one–month increase in maximum benefit duration. We consider again four outcomes. UI takeup, benefit duration up to four months of UI, and benefit duration up to five months of UI come from the UI data (col. 1, 2, and 3). The outcome in column (4) is the counterfactual benefit duration displayed in Figure VIIb, which captures the mechanical cost (after subtracting the first four months of UI duration) and the behavioral cost.

We investigate how the costs of longer UI benefits vary with labor market formality by augmenting specification (8) as follows:

$$\begin{aligned} y_{i,s,t} = {} & \alpha_s + \omega_t + \beta\ \mathbb{1}(T_{i,s,t} \geq 0) + \gamma\, T_{i,s,t} + \delta\ \mathbb{1}(T_{i,s,t} \geq 0) \times T_{i,s,t} + \sum_l \zeta_l\ \mathit{LaborMarketShare}_{l,s,t} \\ & + \sum_l \kappa_l\ \mathit{LaborMarketShare}_{l,s,t} \times \mathbb{1}(T_{i,s,t} \geq 0) + \sum_l \psi_l\ \mathit{LaborMarketShare}_{l,s,t} \times T_{i,s,t} \\ & + \sum_l \xi_l\ \mathit{LaborMarketShare}_{l,s,t} \times \mathbb{1}(T_{i,s,t} \geq 0) \times T_{i,s,t} + \varepsilon_{i,s,t} \end{aligned} \tag{9}$$

where α and ω are state and year fixed effects. *LaborMarketShare* is the demeaned share of the population, 18–54 years old, in labor status l in state s and year t. We first consider a specification that only includes the share of non–farm formal employees, as in Table I (top panel). When the outcome is the counterfactual benefit duration displayed in Figure VIIb, ζ and κ capture respectively how the mechanical cost and the behavioral cost vary with formal employment. It is then straightforward to estimate how the (maximum) efficiency cost varies with formal employment.

However, as discussed earlier, the underlying forces behind our correlations are likely to be very different depending on whether variations in formal employment are due to variations in, e.g., informal employment vs. unemployment. To study changes in labor market formality, rather than overall changes in formal employment, we consider a specification that includes the labor market share of the same categories of labor status as in Table I (bottom panel). Linear combinations $\zeta_l - \zeta_{l'}$ (resp. linear combinations $\kappa_l - \kappa_{l'}$) allow us to estimate, e.g., how the mechanical cost (resp. the behavioral cost) varies with an increase in the share of formal employees and a decrease in the share of informal workers, thus a change in labor market formality.

4.2.4 Validity checks

Before turning to our results, we check whether the layoff density and the means of observable characteristics trend smoothly with job tenure through the 24-month cutoff (Lee, 2008).

We find no evidence of an increase in the density of layoffs at the 24–month cutoff (Appendix Figure A.8a).[49] Workers do not appear to be selectively laid off when they become eligible for longer UI benefits. This is not surprising given that 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. We test for a discontinuity using the following specification:

$$N_{bin} = \alpha + \beta\ \mathbb{1}(T_{bin} \geq 0) + \gamma\, T_{bin} + \delta\ \mathbb{1}(T_{bin} \geq 0) \times T_{bin} + \varepsilon_{bin}, \tag{10}$$

where N_{bin} is the number of observations by week of tenure. The first row in Appendix Table A.6 presents OLS estimates of $\widehat{\beta}$. The coefficient is small and not statistically significant with tenure windows of either eight months or six months around the cutoff (col. 1 and 2, respectively).

The rest of Appendix Table A.6 checks for differences in sample composition at the cutoff. We examine how observable characteristics vary with job tenure using the following specification:

$$x_i = \alpha + \beta\ \mathbb{1}(T_i \geq 0) + \gamma\, T_i + \delta\ \mathbb{1}(T_i \geq 0) \times T_i + \varepsilon_i, \tag{11}$$

where x is a characteristic of worker i. Estimates of $\widehat{\beta}$ are neither economically nor statistically significant for log wages, replacement rates, gender, years of education, firm size, the share of private formal employees, and whether a worker comes from the construction or the service sector. Estimates are statistically significant, but only with the wider tenure window, for whether a worker comes from the trade or the industrial sector. Estimates are statistically significant for age. We plot workers' age by tenure bin in Appendix Figure A.8b. There is no visible discontinuity; any change is economically small (.2 year), and is only statistically significant because of our sample size.

[49] Layoff rates vary across weeks within month, motivating our clustering of standard errors by week of tenure.

The degree of potential bias from the small amount of selection can be assessed by estimating the effect of these covariates on our main outcome of interest, the counterfactual benefit duration displayed in Figure VIIb. Following Card, Chetty and Weber (2007*a*), we estimate the effect of covariates on our outcome, $y = X\phi$, where X denotes a rich set of observables, including demographics, wages, sector of activity, and state and year fixed effects. Some of these, such as wages, are endogenous outcomes and are likely to also capture unobserved characteristics. We then predict the outcome for each observation i using the estimated $\widehat{\phi}$ vector. Finally, we plot the predicted outcome in Appendix Figure A.8c, averaged by week of tenure, and regress it by WLS using specification (11). The predicted outcome is smooth through the 24–month cutoff and the estimated $\widehat{\beta}$'s are close to zero (last row of Appendix Table A.6). We conclude that any discontinuity in UI outcomes at the cutoff can be attributed to the causal effect of maximum benefit duration. Our estimates below are in fact identical whether or not we control for individual characteristics.

4.2.5 Regression results

Table III displays our overall RD estimates $\widehat{\beta}$ from specification (8) using a tenure window of eight months around the cutoff. We find no impact on UI take–up (col. 1). Average benefit duration was close to maximum benefit duration for workers eligible for four months of UI (col. 2). We estimate an increase of .89 month at the eligibility cutoff (col. 3), but the behavioral cost only amounts to .08 month (col. 4). The overall efficiency cost is thus small, around 10 cents per \$1 reaching mechanical beneficiaries, but it is again relatively large with respect to the maximum efficiency cost (25 cents per \$1). These results confirm our findings in section 4.1. They are robust to controlling for individual characteristics, to using a smaller tenure window, to focusing on a period of sustained economic growth, to excluding workers with very high or very low replacement rates, and to excluding the least and most formal Brazilian states (Appendix Table A.1).[50]

Table IV displays estimates from specification (9). Regressions include year and state fixed effects, and a rich set of (demeaned) individual controls. We first consider a specification that only includes the share of non–farm private formal employees (top panel). As in Table I, the mechanical cost decreases and the maximum efficiency cost increases with formal employment. We find that the behavioral cost also increases, and thus the efficiency cost increases.[51]

[50]In Appendix Table A.11, we also consider longer–term outcomes. We find no effect on subsequent wages in the formal sector. The number of months of formal employment in the two years after layoff and the share of workers experiencing a new layoff from the formal sector decreases at the cutoff. These results confirm our findings in section 4.1. The Web Appendix also presents some individual heterogeneity. Finally, most results in the paper, including those in Table III, are based on samples of workers displaced in the first five months of every year because of the limitation in our UI data discussed in section 1.3. We show in the Web Appendix that the results in Table III are unlikely to be driven by this sample restriction. RD estimates are similar for workers displaced in the first five months of the year and in the next seven months of the year after 2007, when our UI data no longer suffer from the same limitation.

[51]Appendix Figure A.9 presents some graphical evidence. We grouped each state–year pair by formal employ-

We next consider a specification that includes the labor market share of the same categories of labor status as in Table I (bottom panel) to study changes in labor market formality rather than in formal employment. We display linear combinations of interest in the bottom panel of Table IV.

The efficiency cost of longer UI benefits increases with labor market formality (second row). In Table I, we showed that the potential distortion (maximum efficiency cost) increases with labor market formality. Yet, it may also become harder for a given worker to actually respond to UI incentives. We find that the efficiency cost increases because the first effect dominates. As in Table I, the mechanical cost decreases and the maximum efficiency cost increases. Any change in workers' ability to respond to UI incentives appears unable to mitigate this effect and the behavioral cost increases as well. Our estimates imply that the efficiency cost increases by 10.7% following a 10% increase in the share of formal employees (2.9 pp), when associated with a decrease in the share of informal workers. To provide some benchmark, the magnitude is about half as large as for an increase in formal employment associated with a decrease in unemployment (first row).

In the last two rows of Table IV, we split informal workers between informal employees (third row) and self–employed workers (fourth row) in specification (9). Interestingly, the efficiency cost does not increase following an increase in the share of formal employees and a decrease in the share of informal employees. The potential distortion increases, but workers' ability to respond to UI incentives must effectively decrease, and the two effects cancel out.

Our results are not due to simple composition effects. They are robust to not controlling for individual characteristics, to using a smaller tenure window, to focusing on a period of sustained economic growth, to excluding workers with very high or very low replacement rates, to excluding the least and most formal Brazilian states, and to allowing estimates to vary with individuals' characteristics (Appendix Tables A.8, A.9, and A.10). It is unlikely that our results are due to a differential selection into UI takeup. We find no effect of formal employment on takeup, either directly or at the cutoff in the top panel in Table IV. Some linear combinations are statistically significant in the bottom panel of Table IV, but they imply no more than a 0.5% change in UI takeup following a 10% change in formal employment. Moreover, our estimates of interest do not vary systematically with the size and significance of the estimates for UI takeup across the results of our many robustness checks. Note that the efficiency cost does not decrease, either economically or statistically, following an increase in the share of formal employees and a decrease in the share of informal workers, self–employed workers, or informal employees in any of our robustness checks.

ment level and estimated specification (8) separately for each group. Using our results, we obtain an estimate of the mechanical cost, maximum efficiency cost, behavioral cost, and efficiency cost by group with their standard errors. The fitted line and confidence intervals then come from WLS regressions of our estimates on our measure of formal employment. The maximum efficiency cost is relatively small because the mechanical cost is large at every level of formal employment (not shown). Importantly, the maximum efficiency cost increases with formal employment. This increase is in part translated into an increase in the actual efficiency cost. The behavioral cost (resp. the efficiency cost) is relatively small but it doubles (resp. triples) from the lowest to the highest levels of formal employment.

Our findings go against the widespread view that the efficiency cost of social (insurance) programs must be larger in the presence of informal work opportunities. The efficiency cost of longer UI benefits is limited and it increases (weakly) with labor market formality. This is true even though the next section provides evidence that a significant share of displaced formal employees who remain without a new formal job are working informally.

5 Unemployed vs. informal, and the need for insurance

We showed that displaced formal employees do not return rapidly to a formal job in Brazil. In this section, we provide evidence that a significant share of them is working informally. We also provide evidence that the spike in hazard rates of formal reemployment after benefit exhaustion, which we showed is due to behavioral responses, is offset by a decrease in informal employment rather than unemployment. To do so, we compare patterns of formal reemployment in our administrative data to patterns of overall (formal and informal) reemployment estimated using longitudinal monthly labor force surveys (PME, 2003–2010). We use the same surveys to shed some light on the insurance value of UI benefits. We can only assess UI eligibility for workers who had more than 24 months of job tenure at layoff in PME, so we focus on these workers in the analysis. Moreover, PME only covers six metropolitan areas, so we do not investigate labor market heterogeneity.

5.1 Unemployed vs. working informally?

We investigate here whether the many displaced formal employees who do not return rapidly to a formal job in Brazil remain unemployed vs. work informally. To do so, we estimate hazard rates of overall (formal and informal) reemployment by maximum likelihood using information from consecutive interviews for the same individual in PME surveys (as in e.g. Rothstein, 2011).[52] We then compare our estimates to hazard rates of formal reemployment from the administrative data.

5.1.1 Sample selection

PME has a similar structure as the Current Population Surveys (CPS) in the US. Households enter the sample for two periods of four consecutive months, eight months apart from each other. In each survey, information on labor status is recorded for every household member above ten years old. We have information on wage, tenure, and the signing of the working card. For the unemployed,

[52] We cannot estimate hazard rates of formal reemployment with PME because we don't observe later transitions to formal jobs once workers are reemployed informally. The panel is too short and questions about past employment and unemployment spells are not asked of employed individuals. We do not estimate hazard rates of overall and informal reemployment separately because some workers who report finding a new formal job in PME may not be reported to the government as formally employed until they exhaust their benefits (see footnote 19 in section 2).

we have similar information about the last job, except the wage, as well as the reason for separation and the length of the unemployment spell (in months). The surveys do not ask for any information about other previous jobs, so we can only assess UI eligibility for workers who had more than 24 months of job tenure at layoff. They are eligible for five months of UI. Our sample of analysis includes full–time private formal employees, 18–54 years old, laid off between 2003 and 2009 in the six metropolitan areas covered by PME, and is limited to those who are observed in two consecutive interviews. As in the CPS, there is no information on UI takeup, so our sample is not restricted to UI takers. Takeup is around 75% in a comparable sample in the administrative data.

5.1.2 Estimation strategy

We want a likelihood function that is robustly estimated but also flexible enough to capture a possible spike in overall reemployment. We choose a piece–wise constant hazard function allowing for different hazard rates in months 0, 1–2, 3–4, 5–6, 7–8, and 9–10 after layoff.[53] Our likelihood function must also correct for a stock sampling issue within a month. Define λ_m, the daily hazard rate constant over month m since layoff. Assume information is recorded on day $b \in [0,30]$ within month m. To be recorded as unemployed, an individual must have survived b days without a job, given that she already survived m months. Define $k(b)$, the distribution of interviews over days within a month. Finally, define $d_{i,m} = 1$ if individual i, unemployed since month m, is reemployed in the next interview. The likelihood for a given observation is then:

$$
\begin{aligned}
L_{i,m} = d_{i,m} \int_0^{30} \left[1 - exp\left(-\left(30-b\right)\lambda_m - b\lambda_{m+1}\right)\right] \frac{k\left(b\right) exp\left[-b\lambda_m\right]}{\int_0^{30} k\left(s\right) exp\left[-s\lambda_m\right] ds} db \\
+ \left(1-d_{i,m}\right) \int_0^{30} \left[exp\left(-\left(30-b\right)\lambda_m - b\lambda_{m+1}\right)\right] \frac{k\left(b\right) exp\left[-b\lambda_m\right]}{\int_0^{30} k\left(s\right) exp\left[-s\lambda_m\right] ds} db \qquad (12)
\end{aligned}
$$

Interviews are evenly spread over a month (IBGE, 2007), so we assume a uniform distribution, $k(b) = 1/30$. In the estimation, we use sampling weights and cluster standard errors by individual.

We compare the estimated overall reemployment rates with formal reemployment rates. The sample used to construct formal reemployment rates applies the same selection filters in our administrative data. However, our estimates may not be perfectly comparable. First, there is some attrition in PME. We have 26,395 observations from 16,271 individuals contributing to the likelihood function. These numbers would be 5.6% and 6.4% higher without attrition. Attrition levels are in fact higher in the CPS (Rothstein, 2011). Second, although PME is representative of the

[53] This choice is informed by formal reemployment patterns in the administrative data. In particular, it is important to allow for a different hazard rate in month 0 after layoff. Formal reemployment rates are higher in the first month, when the sample is not restricted to UI takers, because of the 30–day waiting period for UI benefits. We group other months by pair because estimates for individual months tend to bounce up and down in consecutive months.

labor force in six metropolitan areas, it may not be representative for displaced formal employees who had more than 24 months of tenure at layoff in these areas. This is a reason why we estimate overall reemployment rates from transitions between months rather than from the distribution of spells of unemployment duration. There is no simple solution to these issues. In Appendix Figure A.10, we show that our results are similar if we reweight the PME sample such that it compares better to the administrative sample on observables (DiNardo, Fortin and Lemieux, 1996).

5.1.3 Results

Figure VIII presents our results. For the sake of clarity, we display the share of workers reemployed in each month after layoff (panel a) and the survival rate out of any job (panel b) resulting from the estimated hazard rates. The 95% confidence intervals are obtained by the delta method. We include the corresponding patterns of formal reemployment from the administrative data. We also show the share drawing UI and the share exhausting UI in each month after layoff in this sample. The former is almost nil in the first month after layoff because of the 30-day waiting period.[54]

There are five lessons from Figure VIII. First, many displaced formal employees must be reemployed informally. The share of workers reemployed is higher than the share of workers formally reemployed in the first five months after layoff. As a result, 35% remain unemployed five months after layoff, but 80% are still without a formal job.[55] Second, many UI beneficiaries must be reemployed informally. While 35% remain unemployed five months after layoff, 75% are drawing UI in that month. Third, a significant share of UI beneficiaries and exhaustees must be actually unemployed. More than 70% exhaust their UI benefits, but the difference between the share unemployed and formally reemployed never exceeds 45%. Fourth, many workers must remain in the informal sector even after exhausting UI benefits. The difference between survival rates out of any job and out of any formal jobs persists after most beneficiaries exhaust their benefits.

Last, the spike in hazard rates of formal reemployment after benefit exhaustion, which we showed is due to behavioral responses, must be offset by a decrease in informal employment rather than a decrease in unemployment. The share of workers formally reemployed increases five months after layoff because of the spike in hazard rates. In contrast, the share of workers reemployed de-

[54]Patterns of formal reemployment and UI collection are based on workers displaced in the first five months of every year because of the limitation in our UI data discussed in section 1.3. Patterns of overall reemployment are instead obtained for workers displaced throughout the year. Appendix Figure A.10 shows that patterns of overall reemployment are similar if we only use workers displaced in the first five months of every year. Patterns of overall reemployment, formal reemployment, and UI collection are also similar if we use workers laid off throughout the year after 2007, when our UI data no longer suffers from the same limitation.

[55]Among the workers who report being reemployed informally in the first five months after layoff, 31.2% become self–employed, 59.9% become informal employees, 7.3% become domestic employees (mostly women), and 1.6% become unpaid workers. Among the workers who report being reemployed formally, 94.4% become formal employees in a private firm, 3.3% become public employees, and 2.3% become employers.

creases monotonically. Therefore, the spike cannot be due to a change in the proportion of workers leaving unemployment. Instead, more workers (resp. less workers) must be transiting between informal and formal employment (resp. between unemployment and informal employment). In fact, beyond five months after layoff, the share of workers who find a formal job in each month exceeds the share of workers who find a first new job. All together, the evidence suggests that informal work opportunities are an important driver of the behavioral cost of longer UI benefits in Brazil.

5.2 Need for insurance: earnings and disposable income

The above analysis suggests that many UI beneficiares may self–insure through the informal sector, but that a significant share remains unemployed. Labor status, however, does not directly provide information on the need for income support. Workers reemployed in the informal sector may earn a relatively low wage. The unemployed may have other family members with a high income. Here, we provide some evidence on earnings and disposable income levels in the same PME sample.

5.2.1 Earnings

Columns (1)–(2) in Table V investigate earnings levels for informally reemployed workers. Ideally, we would like to compare earnings levels upon reemployment and prior to layoff for a given individual. PME does not have information about the wage in the lost job and the panel is too short to follow most workers from layoff to reemployment. Instead, we proceed as follows. First, we restrict attention to workers who are observed either formally employed in the month prior to layoff or reemployed in the ten months after layoff. Second, we construct net monthly earnings from gross monthly earnings reported in PME. Finally, we use the data as a repeated cross–section:

$$\begin{aligned} Earnings_i = \alpha + \beta_{before}\ [FormReemp_i \times BeforeExhaust_i] \\ + \gamma_{before}\ [InfReemp_i \times BeforeExhaust_i] + \beta_{around}\ [FormReemp_i \times AroundExhaust_i] \\ + \gamma_{around}\ [InfReemp_i \times AroundExhaust_i] + \beta_{after}\ [FormReemp_i \times AfterExhaust_i] \\ + \gamma_{after}\ [InfReemp_i \times AfterExhaust_i] + \varepsilon_i \end{aligned} \tag{13}$$

where $FormReemp$ and $InfReemp$ indicate workers reemployed formally and informally, respectively. We separate those reemployed before UI exhaustion (months 1–4 after layoff), around UI exhaustion (months 5–7), and after UI exhaustion (months 8–10). Estimations are performed using sampling weights and clustering standard errors by individual. In Table V, we normalize estimates with respect to the average net monthly earnings prior to layoff (omitted group). Workers who are reemployed informally in the first few months after layoff have earning levels 47% lower than workers prior to layoff (col. 1). Estimates are similar for workers who are reemployed informally

around or after UI exhaustion (47% and 50%). The earnings gap is lower for workers reemployed in the formal sector before, around, or after UI exhaustion (14%, 26%, and 23%, respectively).

In the absence of heterogeneity, these estimates capture average differences between net monthly earnings upon reemployment and prior to layoff. In practice, there may be some selection into reemployment and into formal vs. informal reemployment. Yet, our estimates are mostly unchanged if we control for workers' characteristics, including gender, education, age and tenure (col. 2), or if we re–weight the sample so that it compares better to the sample of workers who are observed formally employed in the month prior to layoff or to the sample of workers from the administrative data (see Web Appendix). It is also unclear in which direction the bias would go. Some unobserved heterogeneity may still be unrelated to the heterogeneity that we are controlling for. However, the fact that the estimated earnings gap is of a similar magnitude for workers reemployed before, around, and after UI exhaustion goes against the most obvious selection stories. The only notable difference is the lower earnings gap for workers who are reemployed in the formal sector in the first few months after layoff. This is likely driven by workers who do not take up UI.

5.2.2 Disposable income

Households may be able to smooth employment shocks across household members. Columns (3)–(4) in Table V thus investigates disposable income rather than earnings levels. Disposable income is defined as household net monthly income per capita, with an equivalence scale of one half for children. We adopt a similar specification as above, but now include the unemployed:

$$\begin{aligned} DispInc_i = \alpha &+ \beta_{before}\ [FormReemp_i \times BeforeExhaust_i] + \gamma_{before}\ [InfReemp_i \times BeforeExhaust_i] \\ &+ \delta_{before}\ [Unemp_i \times BeforeExhaust_i] + \beta_{around}\ [FormReemp_i \times AroundExhaust_i] \\ &+ \gamma_{around}\ [InfReemp_i \times AroundExhaust_i] + \delta_{around}\ [Unemp_i \times AroundExhaust_i] \\ &+ \beta_{after}\ [FormReemp_i \times AfterExhaust_i] + \gamma_{after}\ [InfReemp_i \times AfterExhaust_i] \\ &+ \delta_{after}\ [Unemp_i \times AfterExhaust_i] + \varepsilon_i \end{aligned} \tag{14}$$

We normalize estimates with respect to the average net disposable income prior to layoff (omitted group). Workers who remain unemployed and those reemployed informally have about 50% and 30% less disposable income, respectively, than workers prior to layoff (col. 3). Estimates are similar for workers observed before, around, or after UI exhaustion. They are mostly unchanged if we control for workers' characteristics (col. 4), or if we re–weight the sample so that it compares better to the sample of workers who are observed formally employed in the month prior to layoff or to the administrative sample (see Web Appendix).[56] To provide some benchmark, the maximum

[56] In the Web Appendix, we further investigate selection and the ability of our individual controls to mitigate this issue. We use the fact that we observe disposable income for unemployed and reemployed workers in the previous

UI benefit level (1.87 minimum wages) would close the whole gap for the informally reemployed and more than half of the gap for the unemployed (not shown). Note that comparing averages may understate the need for insurance, as a third of the unemployed have no household income at all.

6 Welfare calibrations

We showed that the efficiency cost of longer UI benefits is relatively low in Brazil. The limited evidence from PME surveys suggests that displaced formal employees are at best partially self–insured through the informal sector, and that the need for insurance may thus be sizable. In this section, we incorporate our results into our conceptual framework to evaluate welfare effects.

In section 2, we derived the following expression for the welfare effects of longer UI benefits:

$$\frac{d\widetilde{W}}{dP} = qr\, S_{P+1}\left[\left(\frac{U_{P+1}}{S_{P+1}}\frac{g^{U_{P+1}}-g^E}{g^E}+\frac{S_{P+1}-U_{P+1}}{S_{P+1}}\frac{g^{I_{P+1}}-g^E}{g^E}\right)-\widetilde{\eta}\right] \tag{15}$$

where welfare effects are measured in terms of an equivalent percentage change in the total payroll of eligible formal employees. The first of the two terms in brackets captures the social value of insurance. They correspond to the social value of transferring \$1 from the average taxpayer to the average unemployed or informally employed mechanical beneficiary (UI exhaustee), respectively. The second term, the ratio of the behavioral to the mechanical cost, captures the efficiency cost.

We estimated the efficiency cost of longer UI benefits in Brazil. Using expression (15), this is sufficient to derive a lower–bound on the social value of insurance such that welfare effects are positive. For instance, the average efficiency cost was around 15.5 cents per \$1 (resp. 10 cents per \$1) in our first empirical strategy (resp. second empirical strategy). As a result, welfare effects would have been positive as long as the social value of \$1 was 15.5% larger (resp. 10% larger) for the average mechanical beneficiary than for the average taxpayer.

We can put more structure on the social value of insurance and use our survey evidence to provide a better sense of the implied magnitudes. Following Baily (1978) and Chetty (2006), the

month while they are all unemployed. We adopt a specification similar to (14) and compare estimates that use the level vs. the change in disposable income in the left–hand side. Our results are consistent with a simple model of liquidity constraints (Chetty, 2008). Workers who are reemployed informally in any period, and workers who are reemployed formally around UI exhaustion, had lower levels of disposable income in the previous month than workers who remain unemployed. For instance, the specification in levels underestimates individual gains from finding an informal job by about 12.5% (R\$30). Our individual controls capture about two thirds of that difference.

social value of insurance can be decomposed as follows:[57]

$$\frac{g^{H_{P+1}} - g^E}{g^E} \simeq \gamma \frac{c^E - c^{H_{P+1}}}{c^E}, \quad \text{with H=I,U.} \tag{16}$$

where $\frac{c^E - c^{H_{P+1}}}{c^E}$ corresponds to the mean consumption gap between taxpayers and mechanical beneficiaries of type H. γ captures an average coefficient of relative risk aversion. A high value of γ and large consumption gaps increase the social value of insurance. There is no data on consumption or savings for UI beneficiaries in Brazil. We thus propose to approximate consumption gaps by the disposable income gaps estimated in section 5.2, equating taxpayers to formal employees. We then calibrate the social value of insurance for different coefficients of relative risk aversion.[58]

Specifically, we assume that unemployed and informally employed mechanical beneficiaries have about 51.5% and 27.1% less disposable income, respectively. In section 5.1, we estimated that about 29% of our sample of displaced formal employees were still unemployed six months after layoff, but that 72% were still without a formal job. We thus assume that 40% of mechanical beneficiaries are unemployed and 60% are informally employed. Using the administrative sample, we estimate that the maximum efficiency cost of a one–month increase in maximum UI duration is 53 cents per \$1 for these workers. We assume that the efficiency cost would amount to 40% of the maximum efficiency cost, as in Table III, or 21 cents per \$1 reaching mechanical beneficiaries.[59]

The results of our calibrations are presented in Table VI. For a given coefficient of relative risk aversion (col. 1), the table displays the social value of insurance in our calibrations (col. 2) and the resulting welfare effects (col. 3). Instead of relying on our calibrations, one can skip column (1) and read the welfare effects for a given social value of insurance. In the last two columns, we also evaluate welfare effects under the assumption that the social value of transferring \$1 to the informally employed is nil. This assumption may seem extreme given the evidence in section 5.2, but it provides a useful comparison. We display the social value of insurance for the unemployed mechanical beneficiaries in column (4), and the resulting welfare effects in column (5).

Welfare effects are positive unless the social value of insurance is relatively low because the efficiency cost is relatively low. In particular, welfare effects are positive as long as the social value of \$1 is 21% larger for mechanical beneficiaries than for taxpayers ($\gamma = .55$). Assuming that

[57] Without welfare weights, social values correspond to average marginal utilities. The decomposition assumes that third derivatives of utility functions are small.

[58] If displaced workers have significant savings, lower values of γ should be considered (Chetty and Szeidl, 2007).

[59] We also use estimates of layoff rates, takeup rates, and replacement rates for these workers in the administrative data. In this paper, we showed that the efficiency cost of longer UI benefits varies with labor market formality. As discussed in section 5, PME only covers six metropolitan areas and thus does not allow us to study how the need for income support may also vary with labor market formality. Nevertheless, we show in the Web Appendix that our estimated disposable income gaps are relatively similar in the two metropolitan areas in the North–East of Brazil, which are poorer and have a lower share of formal employees, and in the four other areas.

the social value of transferring \$1 to informally employed mechanical beneficiaries is nil, welfare effects are positive as long as the social value of \$1 is about 50% larger for *unemployed* mechanical beneficiaries than for taxpayers ($\gamma = 1$). A similar bound would be above 100% in the US, using estimates of the efficiency cost from Katz and Meyer (1990). To provide some benchmark, Chetty (2008) estimates that the social value of \$1 in the US is 150% larger for UI beneficiaries at the start of their UI spell than for employed individuals. Welfare effects in our calibrations would be equivalent to raising formal wages by .43% (col. 3) or .14% (col. 5) for such a social value.

7 Discussion

We established that the efficiency cost of (longer) UI benefits from distorting incentives to return to a formal job, as derived in a standard public finance theoretical framework, is relatively low in Brazil. Here, we discuss some limitations of our framework.

First, our measure of efficiency entails both an income and a substitution effect. Separating them provides information on the social value of insurance (Chetty, 2008). Yet, conditional on a social value of insurance, the ratio of the behavioral to the mechanical cost constitutes the relevant policy elasticity (Hendren, 2013) and the welfare effects depend solely on this measure in a large class of models, as long as an envelope condition applies to the agents' problem (Chetty, 2006).

UI may also distort layoff rates. We abstracted from this margin because the optimal policy to tackle this distortion, experience–rating of UI benefits or a layoff tax, is well–known. Policies that increase firing costs are common in developing countries. Experience rating may thus be feasible. The Brazilian government is in fact considering its introduction (*O Globo*, February 11, 2014).[60]

There may be relevant general equilibrium effects. The welfare effects of UI would increase with search externalities, but decrease with wage bargaining (Landais, Michaillat and Saez, 2010). We followed the literature and considered UI in isolation but, in reality, behavioral responses to UI incentives may create fiscal externalities. In this case, the impact of (longer) UI benefits on the size of the formal sector, e.g., through changes in the overall duration out of formal employment (D^o), becomes relevant.[61] UI could also increase (resp. decrease) the size of the formal sector if workers value UI above (resp. below) its private cost (Hamermesh, 1979). Evidence in Almeida and Carneiro (2012) suggests that Brazilian workers are willing to trade off lower formal wages

[60] Patterns in Appendix Figure A.5 suggest that UI may affect layoffs at low tenure levels in Brazil. Existing institutions, including firing costs, appear sufficient to prevent such responses at higher tenure levels.

[61] With fiscal externalities, our budget constraint would be: $\frac{D^f}{D^f+D^o}\tau w^f = q\frac{D^f}{D^f+D^o}Bb + R$, where R is the monthly average "other" public spending per individual financed through labor income tax. Then, we would have:

$$\frac{d\widetilde{W}}{dP} = qr\, S_{P+1}\left[\left(\frac{U_{P+1}}{S_{P+1}}\frac{g^{U_{P+1}}-g^E}{g^E} + \frac{S_{P+1}-U_{P+1}}{S_{P+1}}\frac{g^{I_{P+1}}-g^E}{g^E}\right) - \widetilde{\eta} - \frac{R}{b}\frac{dD^o/dP}{S_{P+1}}\right]$$

for mandated benefits, including benefits related to job–loss risk. Finally, the impact of UI on the size of the informal sector may generate real externalities. An estimate of this impact multiplied by the social cost or social value of the externality would then enter the welfare formula. There is no consensus, however, on the magnitude or sign of such externalities.[62]

Last, a welfarist perspective may not be an accurate positive theory of governments. If governments consider their budget as fixed, our results would be reversed. UI is costly and it becomes relatively cheaper per UI spell, even if more distorting, with labor market formality.

8 Conclusion

This paper estimates the efficiency cost of (longer) UI benefits in a context of high informality by combining a model of optimal UI and an unusually rich dataset on Brazilian UI beneficiaries over 15 years. Its findings run counter to widespread claims in policy circles that heightened concerns of moral hazard preclude the existence or expansion of UI in developing countries. The efficiency cost is rather small and it even increases with labor market formality.[63]

We discussed some mechanisms beyond the scope of our framework (e.g., externalities) that could affect the efficiency cost of UI benefits. More research is needed to evaluate their empirical relevance. Moreover, the evidence regarding workers' need for insurance remains suggestive. Further evidence from income or liquidity shocks could shed more light on this general issue in the literature (Card, Chetty and Weber, 2007*a*). For instance, UI saving accounts adopted in several developing countries may offer quasi–exogenous variation in workers' access to their own savings.

The findings of this paper have broader implications for our understanding of social policies in developing countries. First, many social programs and taxes generate incentives for people to carry out their economic activities informally. For the same reasons as for UI, they are viewed as imposing large efficiency costs in a context of high informality. By going against the conventional wisdom, our results cast doubt on whether efficiency considerations actually limit the expansion of social policies in these cases too. Recent work by Kleven and Waseem (2013) points in the same direction: intensive–margin taxable income elasticities are small in Pakistan even though evasion is widespread. Better understanding the relationship between efficiency and formality in other settings appears as a promising avenue for future research.

Second, governments' lack of strict enforcement strategies is often seen as an issue in developing countries, although little is known about their costs. The absence of job–search monitoring for

[62]One could argue that a behavioral cost due to workers choosing to work informally is less detrimental to social welfare than a behavioral cost due to workers choosing to remain unemployed.

[63]This is because most UI beneficiaries would exhaust their benefits absent incentive effects in less formal labor markets. An interesting implication is that paying the net present value of the benefits that UI beneficiaries are eligible for in the first month would have little impact on UI costs but would eliminate any remaining incentive effect.

UI beneficiaries in Brazil may in fact be rational. If most beneficiaries are eligible mechanically, stronger monitoring may imply large costs but small benefits (Gerard and Gonzaga, 2013*b*).

Finally, our results suggest that weak governmental institutions, which may be partly responsible for the persistence of informal work opportunities, may become even more policy relevant when a country's economy develops and its formal sector expands. The potential distortion of fiscal and social policies is likely to increase, and unless agents' ability to respond to policies' incentives effectively decreases, their efficiency costs may increase as well.

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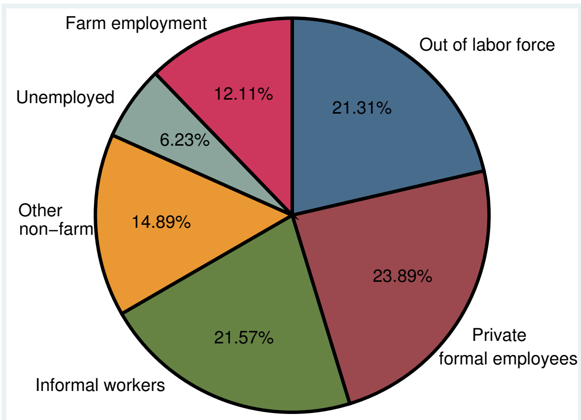
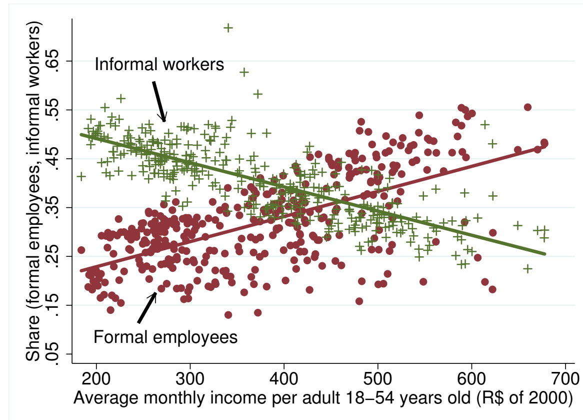
Figure I: Description of Brazilian labor markets

(a) Average distribution by labor status



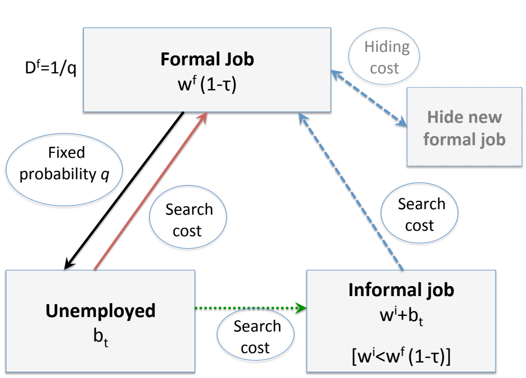
(b) State–year variation in the share of formal employees and informal workers (out of non–farm labor)



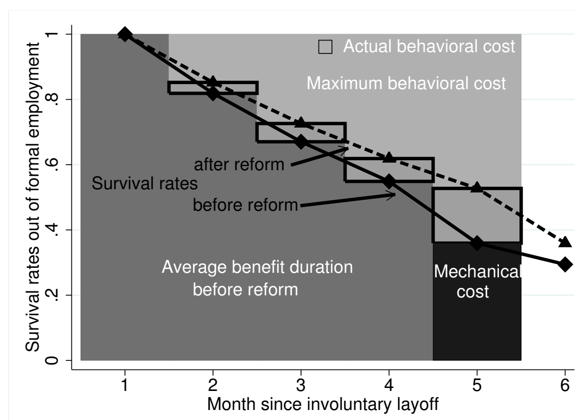
Own calculations using PNAD surveys and survey weights. Panel (a) displays the distribution of the Brazilian population, 18–54 years old, by labor status (1995–2009). The share of non–farm private formal employees is about equal to the share of non–farm informal workers, which includes self–employed workers (12.13%) and informal employees (9.44%). These shares are also roughly equal within gender, even though labor force participation is much higher for males (Appendix Figure A.1). *Other non–farm* includes public employees (5.45%), domestic workers (5.54%), employers (2.72%), and unpaid workers (1.18%). We pool *other non–farm* together because only 6.9% of the displaced formal employees who report being reemployed in the first ten months after layoff in PME surveys report finding a new job falling into one of these work categories. Panel (b) displays the share of formal employees and informal workers out of non–farm employment by state–year pair (1995–2009, 27 states). These shares vary greatly across state–year pairs, and these shares are strongly correlated with income per capita (R\$1.9≃US\$1 in 2000).

Figure II: Connecting theory to the data

(a) Choice situation with informal work opportunities



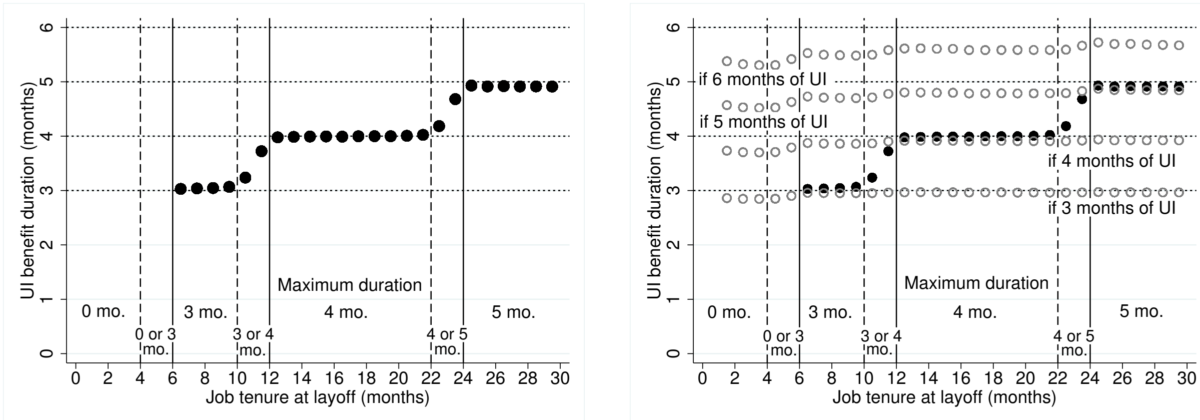
(b) The costs of increasing UI maximum duration



Panel (a) illustrates the choice situation of a displaced formal employee in our context. While unemployed, she can search for a formal job but also for an informal job. In fact, UI increases her return from finding an informal job because she can draw UI benefits while working informally. Relatedly, upon finding a job that would otherwise be formal, she can ask her employer to not report her hiring immediately. In both cases, she would be willing to trade off the gains from drawing UI benefits against some resource costs such as lower wages or hiding costs. These additional margins of behavioral responses have raised concerns that the UI incentive problem may be more severe in a context of high informality. Panel (b) displays hypothetical survival rates out of formal employment for UI beneficiaries before and after an increase in maximum benefit duration. It illustrates how increasing UI maximum duration increases average benefit duration (the area below survival rates up to the maximum benefit duration), and thus UI costs, through two channels. First, there is a *mechanical cost*. Some workers would have remained without a formal job after exhausting their UI benefits even in absence of the reform. Whether unemployed or informally employed, they would draw the extra benefits without changing their behavior. Second, there is a *behavioral cost*, the cost due to behavioral responses. Increasing maximum benefit duration reduces incentives to be formally reemployed. As a result, survival rates out of formal employment may shift upward, increasing average benefit duration. The behavioral cost may be due to workers choosing to stay unemployed or to work informally. In Panel (b), we assume that the behavioral cost amounts to only a small share of the maximum behavioral cost (light grey area). The ratio of the (maximum) behavioral cost to the mechanical cost captures the (maximum) efficiency cost, or the (maximum) resources lost for each \$1 transferred to target beneficiaries.

Figure III: Average benefit duration for UI takers

(a) Actual benefit duration (b) Actual vs. counterfactual duration

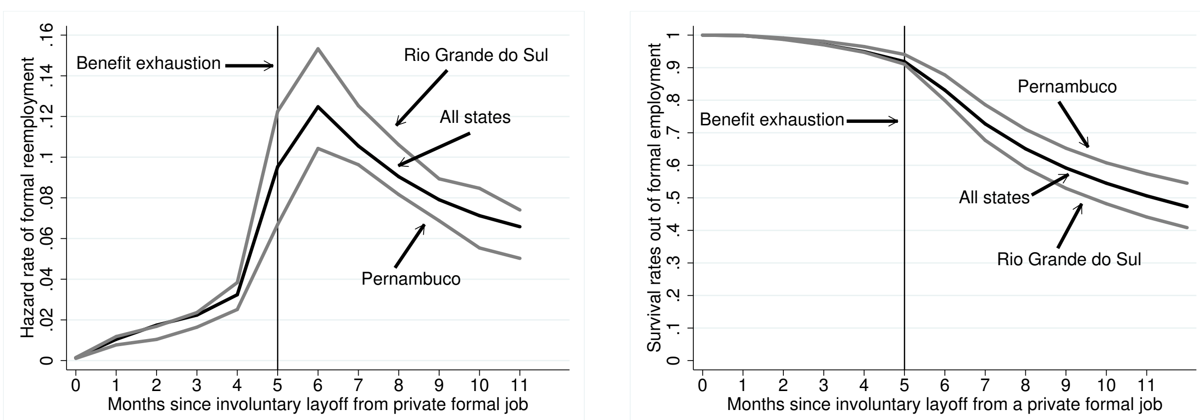


(workers with a single formal job in the previous 36 months).

Panel (a) displays average benefit duration for a random sample of UI takers, 18–54 years old, laid off between 1995 and 2009, who had a single formal job in the previous 36 months. Their maximum UI duration only depends on job tenure at layoff. We restrict attention to workers with more than six months of job tenure at layoff who we know are eligible for UI. There is some ambiguity in UI eligibility at some tenure levels because of two rules explained in the text. Strikingly, average benefit duration is almost exactly equal to maximum benefit duration in Brazil. Panel (b) provides some first evidence that this is mainly *not* due to behavioral responses. We use data on the actual timing of formal reemployment for the same workers to predict benefit duration had they been eligible for three to six months of UI (see text). Such counterfactuals would capture both a behavioral and a mechanical cost for workers' true maximum benefit duration, but only a mechanical cost for longer UI benefits. We construct similar counterfactuals for workers with less than six months of job tenure at layoff, who are not eligible for UI. In this case, our sample includes workers who would not take up UI if eligible and our counterfactuals are likely lower bounds. Our counterfactuals, which successfully match average benefit duration for workers' true maximum benefit duration, imply that workers would *mechanically* draw most additional UI payments following an increase in maximum benefit duration. Bootstrapped 95% confidence intervals are too tight to be presented in the figure.

Figure IV: Formal reemployment patterns for UI takers

(a) Hazard of formal reemployment (b) Survival out of formal employment



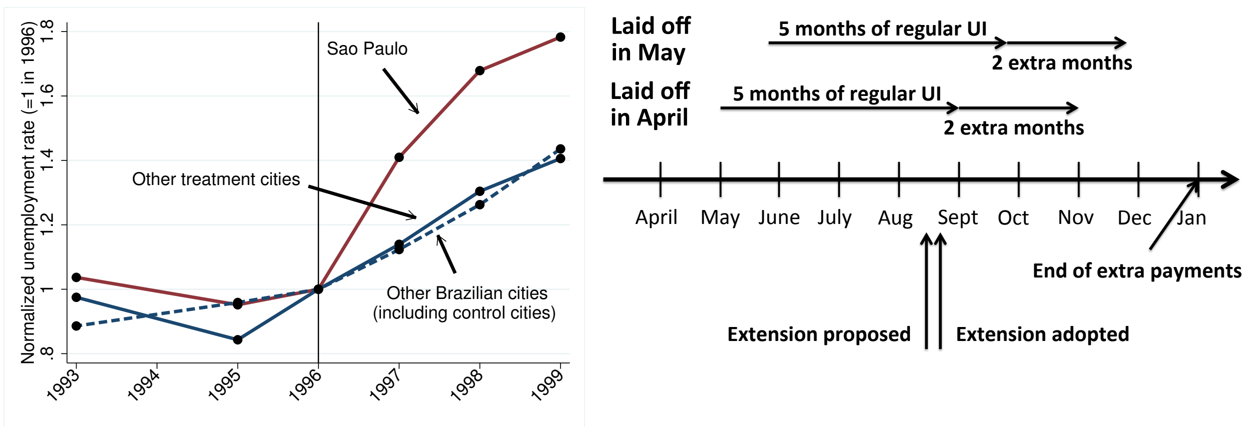
(workers with more than 24 months of tenure at layoff, eligible for five months of UI)

The panels display hazard rates of formal reemployment (a) and survival rates out of formal employment (b) for a random sample of UI takers, 18–54 years old, laid off between 2002 and 2009 (period of sustained economic growth), who had more than 24 months of job tenure at layoff. The black lines consider workers from all Brazilian states. The grey lines compare workers from Pernambuco, a poorer state in the North–East with a lower share of formal employees, and workers from Rio Grande do Sul, a richer state in the South with a higher share of formal employees (share non–farm private formal employees: 15.7% in Pernambuco, 29.7% in Rio Grande do Sul; mean income per adult: R$256 in Pernambuco, R$495 in Rio Grande do Sul). Average benefit duration is high because hazard rates of formal reemployment are very low, below 4% a month, when workers are eligible for UI. Hazard rates then spike to 12% a month, but they stay relatively low. As a result, 50% of workers remain without a formal job 12 months after layoff. The spike after benefit exhaustion suggests a clear behavioral response to UI incentives. Nevertheless, it is clear from Panel (b) that any behavioral cost of longer UI benefits will be small compared to the mechanical cost. The efficiency cost will thus be limited. Hazard rates increase more after benefit exhaustion in Rio Grande do Sul. Consequently, the maximum efficiency cost of longer UI benefits is larger there because of a smaller mechanical cost and a larger maximum behavioral cost.

Figure V: Background information for the 1996 temporary UI extension

(a) Unemployment trends

(b) Timeline of the UI extension

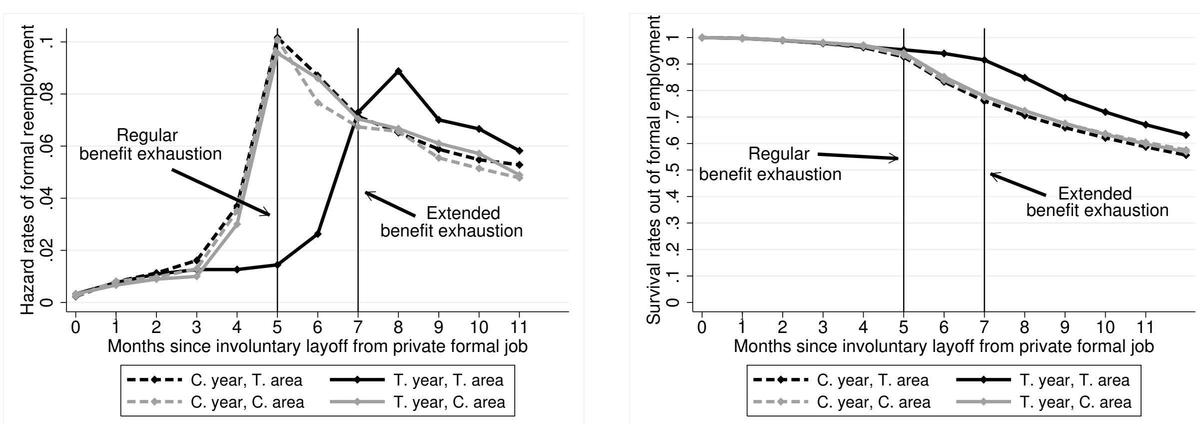


UI beneficiaries who exhausted their regular benefits between September and November 1996 became eligible for two extra months of UI. The extension was limited to Brazil's nine historical metropolitan areas and the Federal District. Panel (a) displays normalized unemployment rates for adults 18–54 years old using PNAD surveys. It separately considers São Paulo (used to first justify the UI extension), the other treatment cities, and all other Brazilian cities, a group that includes control cities (the nine historical metropolitan areas and the Federal District are the only urban areas separately identified in PNAD surveys). It shows that unemployment was trending similarly in treatment cities, São Paulo aside, and in all other Brazilian cities, strengthening our common–trend assumption. In São Paulo, unemployment rose sharply between 1996 and 1997, suggesting particularly poor labor market conditions at the time (we drop São Paulo from the sample of analysis). Panel (b) summarizes the timeline of the UI extension. The extension was first proposed on August 14. It was adopted a week later to start on September 1. The extension was strictly temporary; no additional benefit would be paid after December 31. In the analysis, we focus on workers who had more than 24 months of job tenure at layoff. These workers could draw two extra months of UI, after exhausting their five months of regular benefits, if they were laid off in April or May 1996.

Figure VI: Graphical evidence for the impact of 1996 temporary UI extension

(a) Hazard of formal reemployment

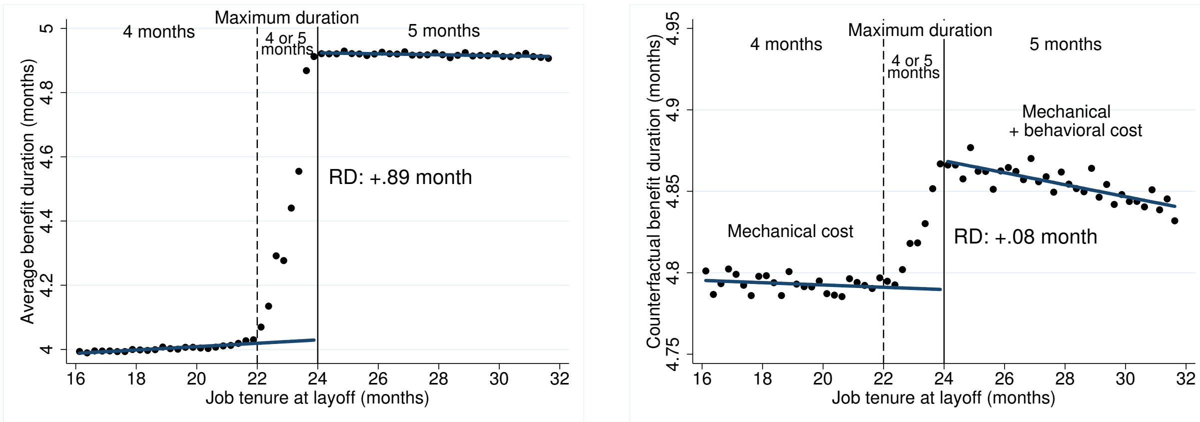
(b) Survival out of formal employment



Panels (a) and (b) display hazard rates of formal reemployment (a) and survival rates out of formal employment (b) in each month after layoff for UI takers 18–54 years old, who had more than 24 months of job tenure at layoff, in control and treatment cities and in control and treatment years. In 1996, in treatment areas, these workers were eligible for seven instead of five months of UI. Lines trace each other very closely in control areas or control years. In contrast, the spike in formal reemployment after benefit exhaustion shifts by exactly two months in treatment areas in 1996. As a result, an additional 15% of UI takers were still out of formal employment seven months after layoff. Survival rates in control years and treatment areas suggest a mechanical cost of 1.6 months and thus a maximum efficiency cost of about 25 cents per $1 reaching mechanical beneficiaries. The difference in survival rates between treatment and control years suggests a behavioral cost of .25 month and thus an efficiency cost of about 15 cents per $1, or 60% of the maximum efficiency cost.

Figure VII: Overall impact of the regression discontinuity design around the 24–month tenure cutoff

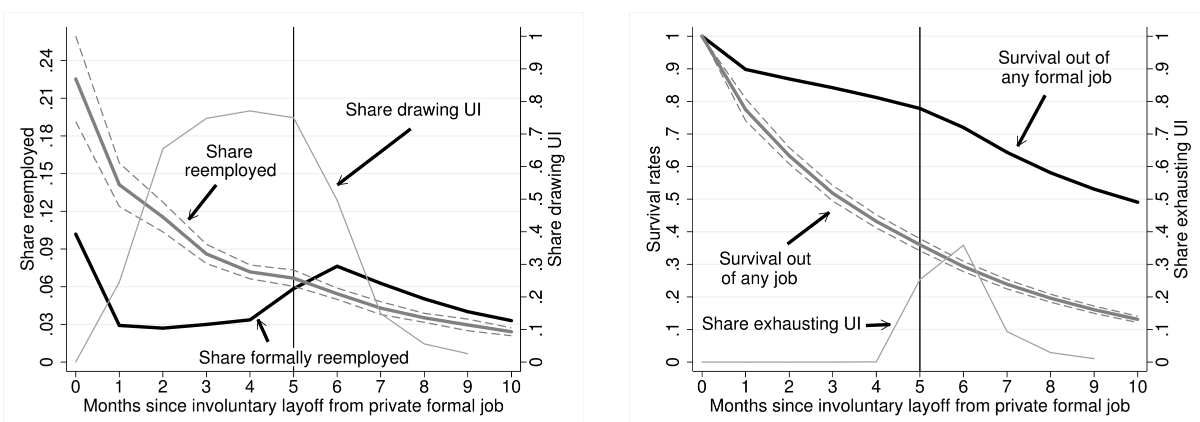
(a) Actual benefit duration (b) Counterfactual duration if 5 months of UI



Panel (a) displays UI benefit duration averaged by week of tenure for UI takers 18–54 years old, laid off between 1997 and 2009, who had a single formal job in the previous 36 months. Workers with less than 22 months and more than 24 months of job tenure at layoff were eligible for four months and five months of UI, respectively. Workers who had between 22 months and 24 months of job tenure at layoff in our data were eligible for four or five months of UI (see text). The fitted lines on both sides of the cutoff in Figure VII correspond to our identifying assumptions. On the left, the line is fitted to the observations below 22 months of tenure and then extrapolated for tenure levels between 22 and 24 months. The discontinuity at the cutoff then captures our estimated impact of the one–month increase in maximum benefit duration. Average benefit duration is constant and close to four months of UI for workers who had less than 22 months of job tenure. It increases to 4.9 months of UI for workers who had more than 24 months of job tenure. In panel (b), we estimate how much of the increase in average benefit duration is due to behavioral responses. We construct a counterfactual benefit duration had all workers been eligible for five months of UI by assuming that a worker would draw one extra month of UI if she is not formally reemployed one month after exhausting her fourth month of UI (see text). We plot the counterfactual benefit durations averaged by week of tenure. The mean on the left of the cutoff captures the mechanical cost (after subtracting the first four months of benefit duration); the discontinuity at the cutoff captures the behavioral cost. Beneficiaries would mechanically draw 4.8 UI payments if they were eligible for five months of UI. The behavioral cost amounts to only .08 month or 9% of the increase in benefit duration.

Figure VIII: Overall (formal+informal) vs. formal reemployment for displaced formal employees

(a) Share reemployed (hazard × survival) (b) Survival rates



Panels (a) and (b) compare *formal* reemployment rates using administrative data (RAIS) and *overall* (formal and informal) reemployment rates estimated by maximum likelihood using information from consecutive interviews in PME surveys. The PME sample includes private formal employees, 18–54 years old, laid off between 2003 and 2009 in the six metropolitan areas covered by PME surveys, who had more than 24 months of job tenure at layoff, and who are observed in two consecutive interviews. These workers were eligible for five months of UI. There is no information on UI takeup in PME, so the sample is not restricted to UI takers. For the sake of clarity, we display the share of workers reemployed in each month after layoff (a), and the survival rate out of any job (b), resulting from our estimated hazard rates of overall reemployment (95% confidence intervals are obtained by the delta method). Patterns of formal reemployment are based on a random sample from our administrative data (applying the same selection filters). We also show the share drawing UI and the share exhausting UI in each month after layoff in the administrative sample. In the text, we use these graphs to argue (i) that many displaced formal employees must be reemployed informally, (ii) that many UI beneficiaries must be reemployed informally, (iii) that a significant share of UI beneficiaries and UI exhaustees must be actually unemployed, (iv) that many workers must remain in the informal sector even after exhausting UI benefits, and (v) that the spike in hazard rates of formal reemployment after benefit exhaustion must be offset by a decrease in informal employment rather than unemployment.

Table I: Formal employment and UI costs

Outcomes	Mean	Only including the share of non–farm private formal employees $\widehat{\gamma}_{form.employee}$ (1)	(2)	(3)	(4)
UI takeup rate	.7474	-.1422***	-.1816***	-.262***	-.0398
		(.0329)	(.0418)	(.0225)	(.0369)
		N=1,461,760			
Average benefit duration (months)	4.351	-.2998***	-.2169***	-.2768***	-.3075***
		(.0309)	(.0236)	(.0135)	(.0643)
Mechanical cost of a 2–month increase in max. UI duration (months)	1.413	-.849***	-.6304***	-.6893***	-.6935***
		(.0589)	(.0612)	(.0327)	(.0647)
Max. behavioral cost of a 2–month increase in max. UI duration (months)	.7154	1.149***	.8472***	.9661***	1.001***
		(.0667)	(.0565)	(.0432)	(.1201)
Max. efficiency cost of a 2–month increase in max. UI duration ($/$)	.5064	1.371***	.8293***	.9354***	.9617***
		(.083)	(.0614)	(.0425)	(.1079)
		N=1,092,590			
Year fixed effects		No	Yes	Yes	Yes
State fixed effects		No	No	Yes	Yes
Other controls		No	No	No	Yes

Outcomes	Mean	Including more detailed labor market composition (specifications with year and state fixed effects, and other controls) $\widehat{\gamma}_{form.employee} - \widehat{\gamma}_{unemployed}$ (1)	$\widehat{\gamma}_{form.employee} - \widehat{\gamma}_{inf.workers}$ (2)	$\widehat{\gamma}_{form.employee} - \widehat{\gamma}_{inf.employee}$ (3)	$\widehat{\gamma}_{form.employee} - \widehat{\gamma}_{self-employed}$ (4)
UI takeup rate	.7474	.3274*	.0253	-.04	.1224
		(.1679)	(.0801)	(.101)	(.099)
		N=1,461,760			
Average benefit duration (months)	4.351	-.9287***	-.4795***	-.424***	-.5625***
		(.2349)	(.1084)	(.1332)	(.1454)
Mechanical cost of a 2–month increase in max. UI duration (months)	1.413	-2.309***	-1.018***	-.8682***	-1.243***
		(.3394)	(.1065)	(.1528)	(.1729)
Max. behavioral cost of a 2–month increase in max. UI duration (months)	.7154	3.238***	1.498***	1.292***	1.806***
		(.5575)	(.1883)	(.2664)	(.2813)
Max. efficiency cost of a 2–month increase in max. UI duration ($/$)	.5064	3.171***	1.436***	1.233***	1.739***
		(.5305)	(.1698)	(.2435)	(.2608)
		N=1,092,590			

s.e. clustered by state (27) in parentheses (significance levels: * 10%, ** 5%, ***1%). Random sample of full–time private formal employees, 18–54 years old, laid off between 1995 and 2009, and eligible for three to five months of UI. Outcomes, other than takeup, are conditional on takeup. Every regression includes a *type* fixed effect (six types: one vs. several formal jobs in the previous 36 months; eligible for three, four, or five months of UI). Other controls include calendar separation month, sector of activity (21), education (4), gender, and firm size (10) fixed effects, as well as 4th order polynomials in age, tenure and log real wage before layoff. The table displays correlations between five UI outcomes (described in the text) and labor market composition (state level). The top panel considers correlations with formal employment as in equation (4). UI beneficiaries return more rapidly to a formal job when the share of non–farm private formal employees is higher. As a result, the mechanical cost (resp. the maximum behavioral cost and the maximum efficiency cost) of a hypothetical 2–month increase in maximum benefit duration systematically decreases (resp. increase) with formal employment. The second panel considers a broader description of labor market composition as in equation (5). We display combinations of coefficients that allow us to study changes in labor market formality, rather than overall changes in formal employment. UI beneficiaries return more rapidly to a formal job following an increase in the share of non–farm private formal employees and a decrease in the share of informal workers (col. 2). As a result, the mechanical cost (resp. the maximum behavioral cost and the maximum efficiency cost) decreases (resp. increase) with labor market formality. The magnitudes are about half as large as for a decrease in unemployment (col. 1). We obtain similar estimates if we split informal workers between informal employees (col. 3) and self–employed workers (col.4) in specification (5).

Table II: Difference–in–difference results for the 1996 temporary UI extension

	Outcomes			
	UI takeup rate	Regular UI duration (months)	Extended UI duration (months)	Extended UI duration vs. counterfactual (months)
	(1)	(2)	(3)	(4)
TreatArea $\times$ Year1996	-.0148	.0003	1.859***	.246***
	(.0156)	(.0032)	(.0126)	(.0205)
Mean (T. areas, C. years)	.73	4.97	4.97	6.56
	N=233,997		N=171,790	
	Associated UI costs			
	Mechanical cost (months)	Max. efficiency cost ($/$)	Behavioral cost (months)	Efficiency cost ($/$)
	(1)	(2)	(3)	(4)
	1.593***	.2696***	.246***	.1545***
	(.0172)	(.015)	(.0205)	(.0141)

s.e. clustered by metropolitan area (29) in parentheses (significance levels: * 10%, ** 5%, ***1%). Full–time private formal employees, 18–54 years old, laid off in control and treatment cities and in control and treatment years, and who had more than 24 months of job tenure at layoff. In 1996, in treatment areas, these workers were eligible for seven instead of five months of UI benefits. Outcomes, other than takeup, are conditional on takeup. The top panel displays the estimated impacts of the 1996 temporary UI extension on four outcomes (described in the text) using a difference–in–difference as in equation (6). The bottom panel displays the implied impacts on UI costs (s.e. for non–linear functions of coefficients are obtained by the delta method). We expect and find no effect on UI takeup or regular benefit duration. The extension increased benefit duration by 1.86 months. However, only 13% of that increase (.25 month) is due to behavioral responses. Indeed, beneficiaries in control years would have mechanically drawn 1.59 additional months of UI, had they been eligible for the extension. Using our estimates, we obtain an efficiency cost of 15.5 cents per $1 reaching mechanical beneficiaries. The efficiency cost is relatively small, but it is large with respect to the maximum efficiency cost (27 cents per $1). The efficiency cost is limited not because of small distortions in the behavior of those who would find a new formal job rapidly, but because most displaced formal employees would not find a new formal job rapidly.

Table III: Overall regression discontinuity results for the 24–month tenure cutoff

	UI takeup rate (1)	UI duration up to 4 months (months) (2)	UI duration up to 5 months (months) (3)	UI duration up to 5 months vs. counterfactual (months) (4)
Job tenure $\geq$ 24 months	.0067	.0047***	.8949***	.0812***
	(.0096)	(.0013)	(.0043)	(.0037)
Mean	.83	3.96	4.01	4.79
($20 \leq Tenure < 22$)	N=1,964,426		N=1,644,695	
		Associated UI costs		
	Mechanical cost (months) (1)	Max. efficiency cost ($/$) (2)	Behavioral cost (months) (3)	Efficiency cost ($/$) (4)
	.8297***	.2558***	.0806***	.0971***
	(.0022)	(.0043)	(.0037)	(.0046)

s.e. clustered by week of tenure in parentheses (significance levels: * 10%, ** 5%, ***1%). Full–time private formal employees, 18–54 years old, laid off between 1997 and 2009, who had a single formal job in the previous 36 months. The sample is restricted to workers with tenure at layoff between 16 and 32 months. Those with more than 24 months of job tenure at layoff were eligible for five instead of four months of UI. Outcomes, other than takeup, are conditional on takeup. The top panel displays the estimated impacts of increasing maximum benefit duration from four to five months on four outcomes (described in the text) using a regression discontinuity design as in equation (8). The bottom panel displays the implied impacts on UI costs (s.e. for non–linear functions of coefficients are obtained by the delta method). Increasing maximum benefit duration by one month increased benefit duration by .895 month. However, only 9% of that increase (.08 month) is due to behavioral responses. The overall efficiency costs are thus small, around 10 cents per $1.

Table IV: Formal employment and regression discontinuity results

	UI takeup rate [mean] (1)	UI takeup rate [RD] (2)	Mechanical cost (months) (3)	Max. efficiency cost ($/$) (4)	Behavioral cost (months) (5)	Efficiency cost ($/$) (6)
	Only including the share of non–farm private formal employees					
At the mean	.8296***	.0086	.8301***	.255***	.0819***	.0987***
	(.0083)	(.0098)	(.0019)	(.0035)	(.0026)	(.0033)
Δ form. employee	-.0422	.0006	-.2632***	.5281***	.0866***	.136***
	(.0276)	(.0211)	(.0177)	(.0301)	(.0213)	(.0268)
	N=1,996,050			N=1,670,505		
	Including more detailed labor market composition					
Δ form. employee	.1636	.1237	-1.058***	2.1***	.5581***	.8084***
−Δ unemployed	(.1144)	(.1265)	(.1065)	(.2073)	(.0026)	(.2151)
Δ form. employee	.1155**	-.0251	-.4738***	1.007***	.2619***	.374***
−Δ inf. worker	(.0562)	(.0631)	(.0334)	(.0743)	(.0497)	(.062)
Δ form. employee	.0873	-.134*	-.2807***	.7448***	-.0505	-.0275
−Δ inf. employee	(.0621)	(.0802)	(.0592)	(.1258)	(.1056)	(.1322)
Δ form. employee	.1435*	.0827	-.6816***	1.299***	.5718***	.7764***
−Δ self–employed	(.0738)	(.0839)	(.083)	(.1386)	(.0752)	(.0991)
	N=1,996,050			N=1,670,505		

s.e. clustered by week of tenure in parentheses (significance levels: * 10%, ** 5%, ***1%, s.e. for nonlinear functions of coefficients are obtained by the delta method). Sample as in Table III. Outcomes, other than takeup, are conditional on takeup. Regressions include year, state, calendar separation month, sector of activity (21), education (4), gender, and firm size (10) fixed effects, as well as 4th order polynomials in (demeaned) age and log real wage before layoff. The table displays the correlations between six UI outcomes (described in the text) and labor market composition as in equation (9). The top panel considers a specification that only includes the share of non–farm private formal employees. The second panel considers a specification that includes the labor market share of the same categories of labor status as in Table I (bottom panel) and Figure Ia. We display combinations of coefficients that allow us to study how the costs of longer UI benefits vary with changes in labor market formality, rather than overall changes in formal employment. The efficiency cost increases with formal employment (top panel). As in Table I, the mechanical cost decreases and the maximum efficiency cost increases. We find that the behavioral cost also increases, and thus the efficiency cost increases. The efficiency cost increases with labor market formality (bottom panel, second row). In Table I, we showed that the share of workers who could potentially respond to UI incentives (maximum efficiency cost) increases with labor market formality. In contrast, it may become harder for such a worker to actually respond to UI incentives. We find here that the efficiency cost increases, when the share of formal employees increases and the share of informal workers decreases, because the first effect dominates. Magnitudes are about half as large as for an increase in formal employment associated with a decrease in unemployment (first row). In the last two rows, we split informal workers between informal employees (third row) and self–employed workers (fourth row) in specification (9). Interestingly, the efficiency cost does not increase following an increase in the share of formal employees and a decrease in the share of informal employees. The share of workers who could potentially respond to UI incentives (maximum efficiency cost) increases, but workers' ability to respond to UI incentives must effectively decrease, and the two effects cancel out.

Table V: Earnings and disposable income levels by reemployment status (vs. prior to layoff)

	Average net earnings vs. prior to layoff (R$531.9) (1)	(2)	Average net disposable income vs. prior to layoff (R$342.4) (3)	(4)
Formally reemployed before UI exhaustion	-.1351***	-.1646***	-.0558	-.0739*
(upon reemployment, Δ%)	(.0389)	(.0313)	(.0429)	(.038)
Formally reemployed around UI exhaustion	-.2569***	-.2637***	-.1465***	-.1477***
(upon reemployment, Δ%)	(.0417)	(.0361)	(.0447)	(.04)
Formally reemployed after UI exhaustion	-.2291***	-.2541***	-.0958	-.1065*
(upon reemployment, Δ%)	(.0568)	(.0508)	(.0599)	(.0564)
Informally reemployed before UI exhaustion	-.4725***	-.4554***	-.3364***	-.2875***
(upon reemployment, Δ%)	(.0312)	(.0302)	(.0317)	(.0289)
Informally reemployed around UI exhaustion	-.4726***	-.4194***	-.3287***	-.2711***
(upon reemployment, Δ%)	(.0357)	(.032)	(.035)	(.0311)
Informally reemployed after UI exhaustion	-.5025***	-.4773***	-.315***	-.2729***
(upon reemployment, Δ%)	(.0403)	(.0371)	(.0432)	(.0388)
Unemployed before UI exhaustion [a]			-.5211***	-.5377***
(Δ%)			(.0186)	(.0161)
Unemployed around UI exhaustion [b]			-.4939***	-.5149***
(Δ%)			(.0222)	(.0199)
Unemployed after UI exhaustion [c]			-.5131***	-.5355***
(Δ%)			(.0211)	(.0197)
	N=6,470		N=27,578	
Including controls	No	Yes	No	Yes

s.e. clustered by individual and obtained by the delta method in parentheses (significance levels: * 10%, ** 5%, ***1%). Data from monthly urban labor force surveys covering the six largest metropolitan areas of Brazil (PME, 2003–2010). Full–time private formal employees, 18–54 years old, laid off between 2003 and 2009, who had more than 24 months of job tenure at layoff, who are observed either formally employed in the month prior to layoff or unemployed in the months after layoff, and who are surveyed in two consecutive interviews. These workers are eligible for five months of UI. The table displays average net earnings and average net disposable income levels of displaced formal workers who are reemployed formally, who are reemployed informally, or who remain unemployed, before UI exhaustion (months 1–4 after layoff), around UI exhaustion (months 5–7), and after UI exhaustion (months 8–10). We normalize estimates with respect to the average levels prior to layoff (repeated cross–section). Controls include year, metropolitan area, calendar separation month, gender, education (9), sector of activity of the lost job (21), and race (5) fixed effects, as well as second–order polynomials in age and tenure. Estimations are performed using sampling weights. Workers who are reemployed informally in the first few months after layoff have earning levels 47% lower than workers prior to layoff (col. 1). Estimates are similar for workers who are reemployed informally around or after UI exhaustion (47% and 50%). The earnings gap is lower for workers reemployed in the formal sector before, around, or after UI exhaustion (14%, 26%, and 23%, respectively). Workers who remain unemployed and those reemployed informally have about 50% and 30% less disposable income, respectively, than workers prior to layoff (col. 3). Estimates are similar for workers observed before, around, or after UI exhaustion. Estimates are mostly unchanged when we control for observables characteristics (col. 2 and 4). Exchange rate: R$1.9≃US$1 (in R$ of 2000). [a] 33% have no disposable income. [b] 30% have no disposable income. [c] 29% have no disposable income.

Table VI: Calibrated welfare effects of a marginal increase in UI maximum duration

			Insurance value =0 if informally reemployed	
Coeff. of rel. risk aversion	Insurance value	Welfare effects	Insurance value (unemployed only)	Welfare effects
(1)	(2)	(3)	(4)	(5)
.25	.09	**-.04**	.13	**-.05**
.55	.21	**0**	.28	**-.03**
1	.38	**.05**	.51	**0**
2	.76	**.18**	1.03	**.07**
3	1.14	**.31**	1.54	**.14**
4	1.52	**.43**	2.06	**.2**

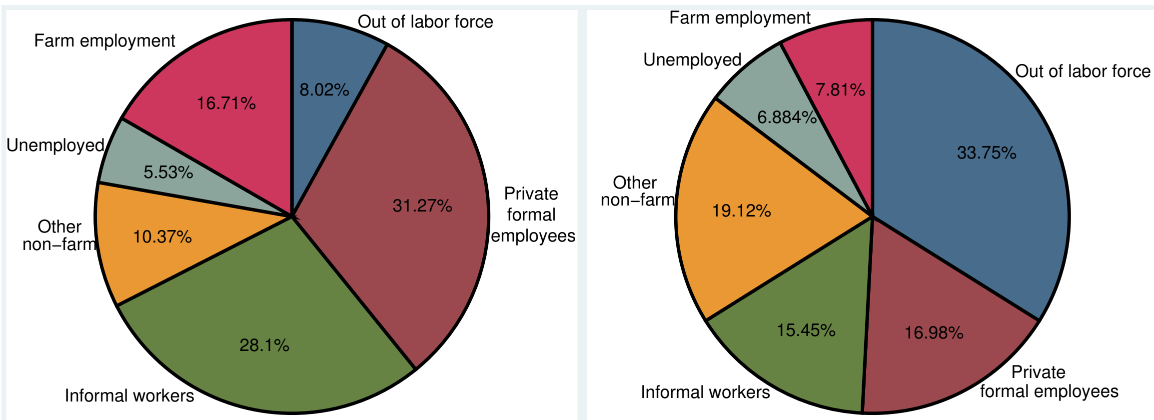
Welfare effects are measured in terms of an equivalent percentage change in the total payroll of eligible formal employees. The table displays the results of our calibrations for the social value of insurance and the welfare effects of increasing maximum benefit duration by one month for our sample of workers in PME surveys (Table V and Figure VIII). See text for a discussion of our main assumptions. For a given coefficient of relative risk aversion (col. 1), the table displays the social value of insurance in our calibrations (col. 2) and the resulting welfare effects (col. 3). Instead of relying on our calibrations, one can skip column (1) and read the welfare effects for a given social value of insurance. In the last two columns, we also evaluate welfare effects under the assumption that the social value of transferring $1 to the informally employed is nil. This assumption may seem extreme given the evidence in section 5.2, but it provides a useful comparison. We display the social value of insurance for the *unemployed* mechanical beneficiaries in column (4), and the resulting welfare effects in column (5). Welfare effects are positive unless the social value of insurance is relatively low because the efficiency cost is relatively low. In particular, welfare effects are positive as long as the social value of $1 is 21% larger for mechanical beneficiaries than for taxpayers. In our calibrations this corresponds to $\gamma = .55$. Assuming that the social value of transferring $1 to informally employed mechanical beneficiaries is nil, welfare effects are positive as long as the social value of $1 is about 50% larger for *unemployed* mechanical beneficiaries than for taxpayers ($\gamma = 1$). A similar bound would be above 100% in the US, using estimates of the efficiency cost from Katz and Meyer (1990). To provide some benchmark, Chetty (2008) estimates that the social value of $1 in the US is 150% larger for UI beneficiaries at the start of their UI spell than for employed individuals. Welfare effects in our calibrations would be equivalent to raising formal wages by .43% (col. 3) or .14% (col. 5) for such a social value.

A Appendix

A.1 Figures and Tables

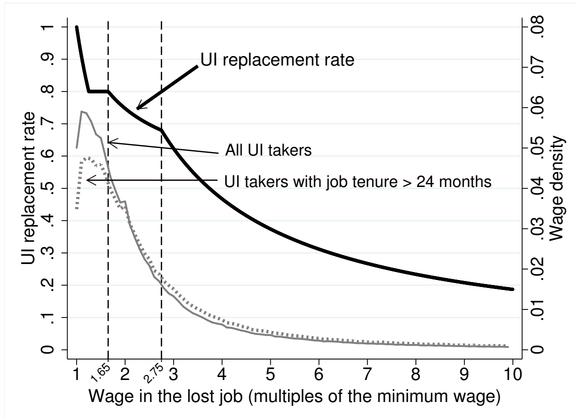
Figure A.1: Average distribution by labor status (males and females, separately)

(a) Males   (b) Females



Own calculations using PNAD surveys and survey weights. Panels (a) and (b) display the distribution of the Brazilian population, 18–54 years old, by labor status (1995–2009) for males and females, respectively. The share of non–farm private formal employees is about equal to the share of non–farm informal workers, which include self–employed workers (15.89% for males, 8.61% for females) and informal employees (12.21% for males, 6.84% for females), even though labor force participation is much higher for males. *Other non–farm* includes public employees (4.94% for males, 5.92% for females), domestic workers (.69% for males, 10.08% for females), employers (4.04% for males, 1.48% for females), and unpaid workers (.7% for males, 1.64% for females). We pool *other non–farm* together because only 3.8% (males) and 12.7% (females, mostly domestic workers) of the displaced formal employees who report being reemployed in the first ten months after layoff in PME surveys report finding a new job falling into one of these work categories.

Figure A.2: UI replacement rates and wage density at layoff

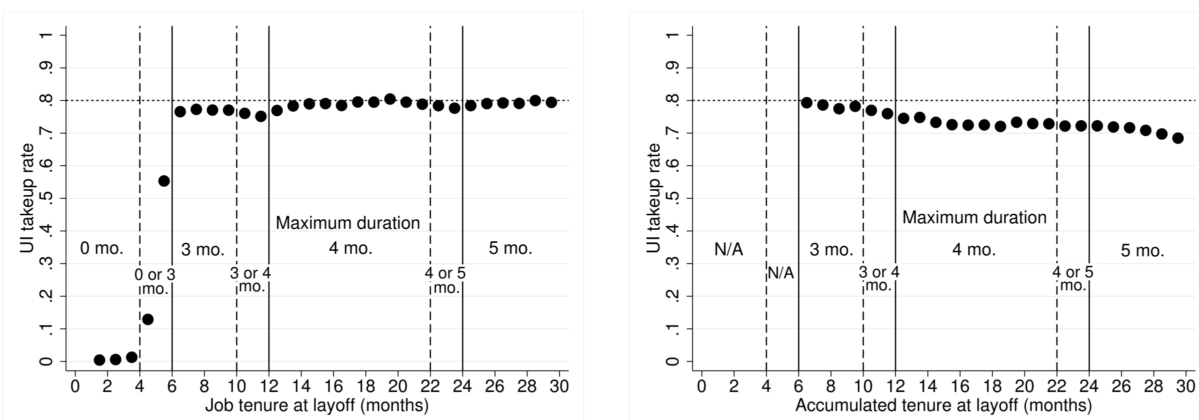


The black line displays the replacement rates in the Brazilian UI program as a function of the average wage in the three months prior to layoff w, expressed in multiples of the minimum wage. Replacement rates vary as follows: 0.8 if $w < 1.65$; $\frac{(0.8)(1.65)+(0.5)(w-1.65)}{w}$ if $1.65 \leq w \leq 2.75$; $\frac{1.87}{w}$ if $w \geq 2.75$. Moreover, UI benefits cannot be inferior to one minimum wage. The grey lines display the density of the wage distribution at layoff for a random sample of all UI takers (solid line), and for a random sample of UI takers with more than 24 months of tenure at layoff (dotted line), 18–54 years old, laid off between 1995 and 2009. Replacement rates are high at the bottom of the wage distribution, but all our results are robust to excluding workers with very high or very low replacement rates.

Figure A.3: UI takeup rates

(a) Workers with 1 formal job in previous 36 months

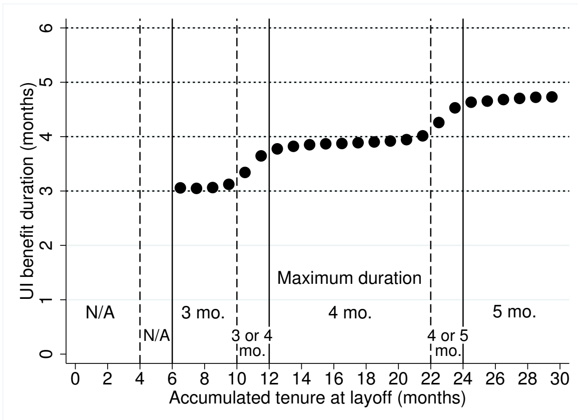
(b) Workers with 2+ formal jobs in previous 36 months, and eligible for UI



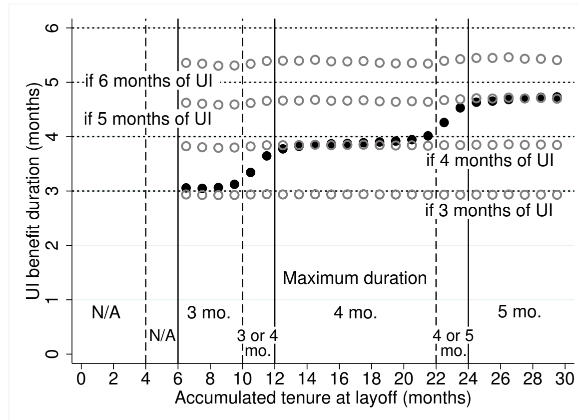
Panels (a) and (b) display UI takeup rates for two random samples of private formal employees, 18–54 years old, laid off between 1995 and 2009. The sample in panel (a) includes workers who had a single formal job in the previous 36 months. Their maximum UI duration only depends on job tenure at layoff. The sample in panel (b) includes workers who had several formal jobs in the previous 36 months. Their maximum UI duration depends on their accumulated tenure across all formal jobs in the previous 36 months, conditional on having at least 16 months between a worker's layoff date and the layoff date of her last successful application and on having more than six months of job tenure at layoff. This is why there is no observation with accumulated tenure below six months. Both panels show that takeup is relatively high among eligible workers in Brazil, around 80%. It is also smooth through the 12–month and 24–month eligibility cutoffs. Some workers with less than six months of job tenure in our data are also eligible for UI because of two rules explained in the text.

Figure A.4: Average benefit duration for UI takers (robustness)

(a) Actual benefit duration



(b) Actual vs. counterfactual benefit duration

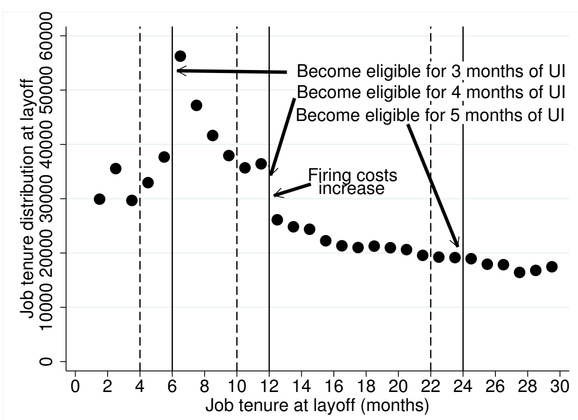


(workers with several formal jobs in the previous 36 months and eligible for UI).

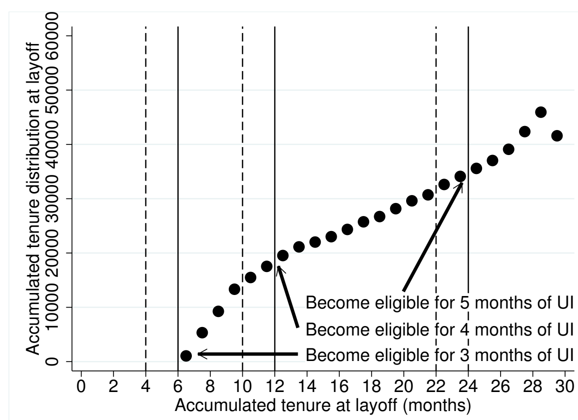
The figure replicates Figure III for a random sample of UI takers, 18–54 years old, laid off between 1995 and 2009, who had several formal jobs in the previous 36 months. Their maximum UI duration depends on their accumulated tenure across all formal jobs in the previous 36 months, conditional on having at least 16 months between a worker's layoff date and the layoff date of her last successful application and on having more than six months of job tenure at layoff. This is why there is no observation with accumulated tenure below six months. There is some ambiguity in UI eligibility at some tenure levels because of two rules explained in the text. As in Figure III, average benefit duration is almost exactly equal to maximum benefit duration and our counterfactual benefit durations imply that workers would mechanically draw most additional UI payments following an increase in maximum UI duration. Bootstrapped 95% confidence intervals are too tight to be presented in the figure.

Figure A.5: Job tenure distribution at layoff

(a) Workers with 1 formal job in previous 36 months



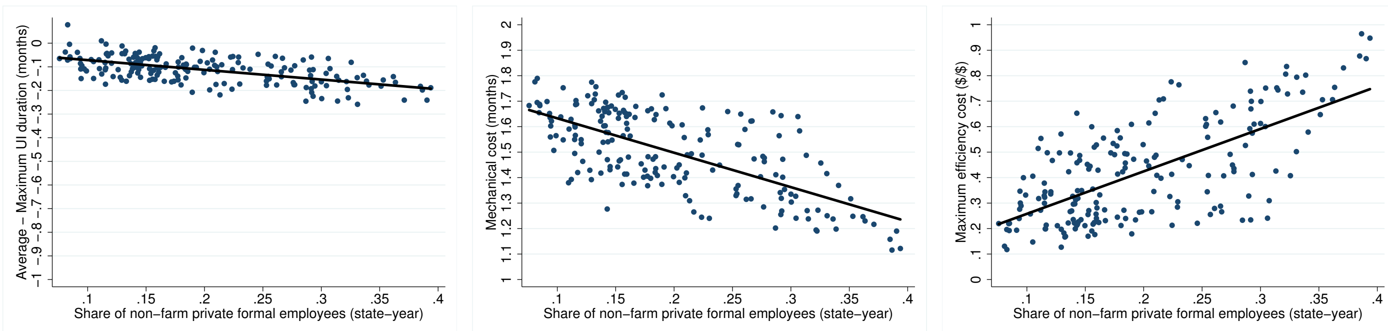
(b) Workers with 2+ formal jobs in previous 36 months, and eligible for UI



Panels (a) and (b) display the distribution of (accumulated) job tenure at layoff for the same two random samples as in Figure A.3. The maximum duration of workers in panel (a) only depends on job tenure at layoff. The maximum duration of workers in panel (b) depends on the accumulated tenure in all formal jobs in the previous 36 months, conditional on having at least 16 months between a worker's layoff date and the layoff date of her last successful application and on having more than six months of job tenure at layoff. This is why there is no observation with accumulated tenure below six months. In panel (a), layoff rates increase at exactly six months of job tenure, when formal employees become eligible for UI. Interestingly, layoff rates also decrease at exactly 12 months of job tenure, when firing costs increase. 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 (unpaid wages, overtime compensation, etc.). Layoff rates at other cutoffs for UI eligibility are smooth in the figures. Finally, note that layoff rates also increase at another cutoff for UI eligilibility, when workers have just 16 months between their layoff date and the layoff date of their last successful application (not shown).

Figure A.6: Benefit duration and consequences of a hypothetical two–month increase in UI benefits

(a) Average - maximum benefit duration (b) Mechanical cost (c) Maximum efficiency cost

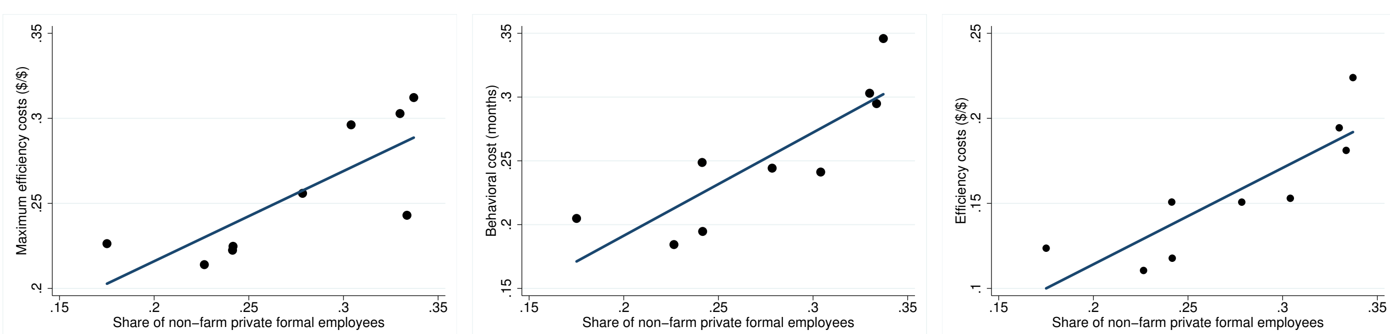


(all UI takers)

Panels (a), (b), and (c) use a random sample of all UI takers, combining the samples used in Figures III and A.4. Each observation is a state average in a given year. Panel (a) shows that average benefit duration is close to maximum benefit duration in every state and year. Panels (b) and (c) display the mechanical cost and the maximum efficiency cost of a hypothetical two–month increase in maximum benefit duration. We assume that workers would draw one extra month of UI (resp. two extra months of UI) if they are not formally reemployed within one month (resp. two months) of exhausting their regular UI benefits. It is then straightforward to estimate the mechanical cost, the maximum behavioral cost, and the maximum efficiency cost. A two–month increase in maximum benefit duration would have been mechanically costly in every state and year, implying that the maximum efficiency costs would have been limited. The mechanical cost (resp. the maximum efficiency cost) is smaller (resp. larger) in labor markets with a larger share of non–farm private formal employees because of the larger increase in hazard rates of formal reemployment at benefit exhaustion (see Figure IV).

Figure A.7: Heterogeneous impact of the 1996 temporary UI extension across treatment cities

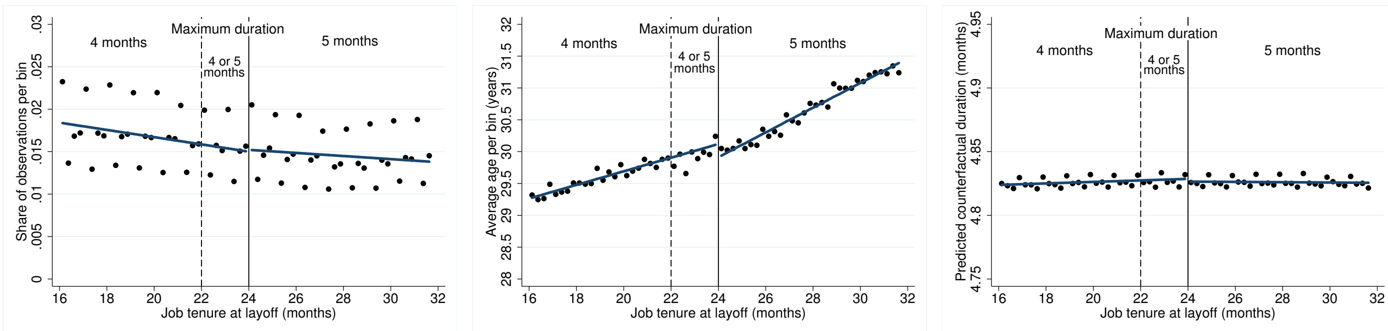
(a) Maximum efficiency cost (b) Behavioral cost (c) Efficiency cost



We compare survival rates in control and treatment years (as in Figure VI) in each treatment city to provide measures of the costs of the 1996 UI extension. These measures are then plotted against the city share of non–farm private formal employees (PNAD, average 1995–1997) in panels (a)–(c). The maximum efficiency cost is relatively small in every city, from 21 cents to 32 cents per $1 reaching mechanical beneficiaries. This is because the underlying mechanical cost (not shown) is large, from 1.49 to 1.67 months. The behavioral cost ranges from .18 to .35 month. As a result, the efficiency cost ranges from 11 cents to 23 cents per $1 reaching mechanical beneficiaries. The maximum efficiency cost is larger in cities with more formal employment and this translates into a larger efficiency cost. However, we cannot make any statistical inference with a cross–section of nine treatment cities. Our second empirical strategy (RD) addresses this limitation.

Figure A.8: Validity of the regression discontinuity design around the 24–month tenure cutoff

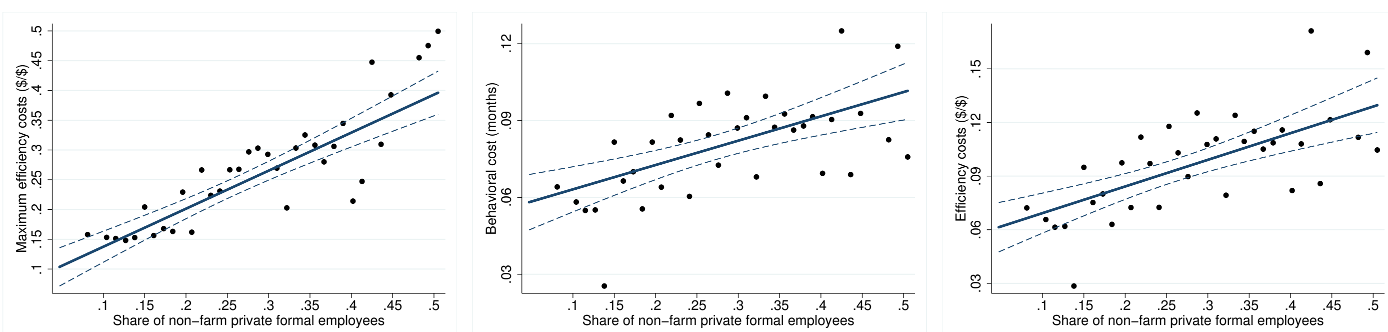
(a) Density of layoffs | (b) Age | (c) Predicted counterfactual duration if 5 months of UI (selection)



Panels (a), (b), and (c) display the graphs behind three estimates in Table A.6, which performs validity checks for a RD design around the 24–month tenure cutoff. Fitted lines on each side of the cutoff are obtained as in Figure VII. Panel (a) displays the density of job tenure at layoff by week of tenure. There is no evidence of an increase in the density of layoffs around the 24–month cutoff. Layoff rates vary across weeks within month, motivating our clustering of standard errors by week of tenure. Table A.6 found a statistically significant difference in average age at the cutoff. Panel (b) shows that there is no visible discontinuity in the underlying data and that any discontinuity is economically insignificant. Panel (c) assesses the degree of potential bias from the small amount of selection found in Table A.6. We estimate the effect of covariates on our main outcome of interest, the counterfactual benefit duration displayed in Figure VIIb, $y = X\phi$, where X denotes a rich set of observed characteristics, including demographics, wages, sector of activity, and state and year fixed effects. Some of these characteristics, such as wages, are endogenous outcomes and are thus likely to also capture unobserved characteristics. We then plot the predicted outcome for each observation using the estimated $\widehat{\phi}$ vector, averaged by week of tenure. It is smooth through the 24–month cutoff.

Figure A.9: RD estimates and formal employment

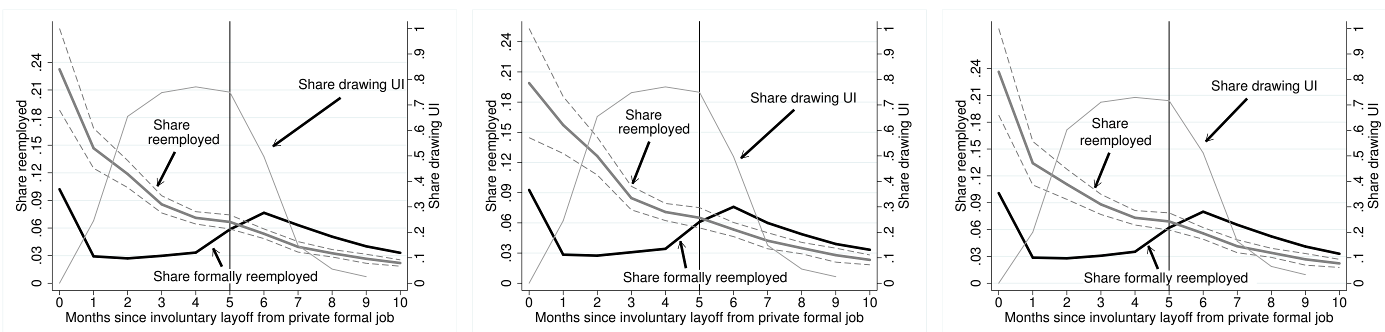
(a) Maximum efficiency cost | (b) Behavioral cost | (c) Efficiency cost



Panels (a), (b), and (c) present some first evidence of a systematic correlation between our RD estimates and formal employment. Specifically, we grouped each state–year pair by level of formal employment (share of non–farm private formal employees) and we obtained estimates of the mechanical cost, maximum efficiency cost, behavioral cost, and efficiency cost by group with their standard errors as in Table III. Standard errors for nonlinear functions of coefficient estimates are obtained by the delta method. The fitted line and confidence intervals then come from WLS regressions of our estimates on our measure of formal employment. The maximum efficiency cost is relatively small because the mechanical cost is large at every level of formal employment (not shown). Importantly, the maximum efficiency cost increases with formal employment. This increase is in part translated into an increase in the actual efficiency cost. The behavioral cost (resp. the efficiency cost) is relatively small but it doubles (resp. triples) from the lowest to the highest levels of formal employment.

Figure A.10: Overall (formal+informal) vs. formal reemployment for displaced formal employees (robustness)

(a) Reweighting the PME sample (b) First 5 months of every year (c) Only observations after 2007



Panels (a), (b), and (c) display robustness checks for Figure VIII. In panel (a), we reweight the PME sample such that it compares better to the administrative sample on observables (DiNardo, Fortin and Lemieux, 1996). We use year, calendar month, metropolitan area, gender, education (9), sector of activity in the lost job (21), and race (5) fixed effects, as well as 2nd order polynomials in age and tenure. Note that the sampling is different in RAIS (observations selected at layoff) and PME (observations selected at month *x* since layoff), so our application of DiNardo, Fortin and Lemieux (1996) is imperfect. Yet, it is reassuring that the results are similar after reweighting. Patterns of formal reemployment and UI collection in Figure VIII are based on workers displaced in the first five months of every year (see section 1.3). Patterns of overall reemployment are instead obtained for workers displaced throughout the year. We show here that patterns of overall reemployment are similar if we only use workers displaced in the first five months of every year (panel b). We also show that patterns of overall reemployment, formal reemployment, and UI collection are similar if we use workers laid off throughout the year after 2007, when our UI data no longer suffers from the same limitation (panel c).

Table A.1: Formal employment and the costs of unemployment insurance (robustness I)

		Only including share of non–farm formal employees			
		$\widehat{\gamma}_{form.employee}$			
Outcomes	Mean	(1)	(2)	(3)	(4)
		Only years after 2002			
UI takeup rate	.7579	-.1368***	-.1517***	-.2626***	-.0763**
		(.0386)	(.0439)	(.0221)	(.0322)
		N=1,154,247			
Average benefit duration (months)	4.338	-.3094***	-.2554***	-.3308***	-.3683***
		(.0321)	(.028)	(.0144)	(.0564)
Mechanical cost of a 2–month increase in max. UI duration (months)	1.366	-.8288***	-.6858***	-.7271***	-.7138***
		(.0554)	(.0651)	(.0316)	(.083)
		N=874,857			
		Replacement rate between 20% and 80%			
UI takeup rate	.7397	-.0595*	-.0987**	-.2006***	-.0219
		(.0318)	(.044)	(.0165)	(.0405)
		N=816,217			
Average benefit duration (months)	4.429	-.3334***	-.1793***	-.2577***	-.2487***
		(.0359)	(.0244)	(.0126)	(.0666)
Mechanical cost of a 2–month increase in max. UI duration (months)	1.384	-.8669***	-.5014***	-.6298***	-.5812***
		(.0542)	(.0426)	(.0277)	(.0797)
		N=603,718			
		Excluding states with highest and lowest share of formal employees			
UI takeup rate	.7548	-.1116*	-.1424**	-.2833***	-.076
		(.0634)	(.0679)	(.0351)	(.0526)
		N=1,000,174			
Average benefit duration (months)	4.336	-.282***	-.1939***	-.2817***	-.2416**
		(.0457)	(.0407)	(.0239)	(.1015)
Mechanical cost of a 2–month increase in max. UI duration (months)	1.423	-.9792***	-.7521***	-.7311***	-.639***
		(.0971)	(.0817)	(.0442)	(.1228)
		N=754,920			
Year fixed effects		No	Yes	Yes	Yes
State fixed effects		No	No	Yes	Yes
Other controls		No	No	No	Yes

s.e. clustered by state (27) in parentheses (significance levels: * 10%, ** 5%, ***1%). Sample, outcomes, and specifications as defined in Table I. The table shows that the results in the top panel of Table I are robust to focusing on a period of sustained economic growth (2002–2009), to excluding displaced formal workers with very high or very low replacement rates, and to excluding the least and most formal Brazilian states.

Table A.2: Formal employment and the costs of unemployment insurance (robustness II)

Outcomes	Mean	$\widehat{\gamma}_{form.employee}$ $-\widehat{\gamma}_{unemployed}$ (1)	$\widehat{\gamma}_{form.employee}$ $-\widehat{\gamma}_{inf.workers}$ (2)	$\widehat{\gamma}_{form.employee}$ $-\widehat{\gamma}_{inf.employee}$ (3)	$\widehat{\gamma}_{form.employee}$ $-\widehat{\gamma}_{self-employed}$ (4)
		Including more detailed labor market composition (specifications with year and state fixed effects, and other controls)			
		Without individual controls			
UI takeup rate	.7474	.1772	.0169	-.0986	.1893
		(.1379)	(.1203)	(.1431)	(.1339)
		N=1,461,760			
Average benefit duration (months)	4.351	-1.058***	-.5174***	-.4861***	-.5645***
		(.2174)	(.0929)	(.1346)	(.1364)
Mechanical cost of a 2–month increase in max. UI duration (months)	1.413	-2.182***	-.9906***	-.8358***	-1.224***
		(.3029)	(.116)	(.1655)	(.182)
		N=1,092,590			
		Only years after 2002			
UI takeup rate	.7579	.059	-.0436	-.0748	.0031
		(.1212)	(.0832)	(.1158)	(.0909)
		N=1,154,247			
Average benefit duration (months)	4.338	-.8314***	-.5299***	-.6029***	-.4198***
		(.1512)	(.0916)	(.0863)	(.15)
Mechanical cost of a 2–month increase in max. UI duration (months)	1.366	-2.215***	-1.047***	-1.13***	-.9221***
		(.2824)	(.1314)	(.1397)	(.2243)
		N=874,857			
		Replacement rate between 20% and 80%			
UI takeup rate	.7397	.2302	-.0033	-.0377	.0467
		(.1534)	(.0648)	(.0992)	(.1051)
		N=816,217			
Average benefit duration (months)	4.429	-.8787***	-.3414**	-.2249	-.5112***
		(.1812)	(.1657)	(.2262)	(.1521)
Mechanical cost of a 2–month increase in max. UI duration (months)	1.384	-2.097***	-.7111***	-.4469**	-1.096***
		(.3008)	(.1235)	(.1951)	(.2159)
		N=603,718			
		Excluding states with highest and lowest share of formal employees			
UI takeup rate	.7548	.1239	-.0933	-.1804*	.0266
		(.1232)	(.0822)	(.1024)	(.102)
		N=1,000,174			
Average benefit duration (months)	4.336	-.6193***	-.3827***	-.3777**	-.3897***
		(.1443)	(.125)	(.1711)	(.1242)
Mechanical cost of a 2–month increase in max. UI duration (months)	1.423	-1.98***	-.9737***	-.8529***	-1.142***
		(.2884)	(.1403)	(.2114)	(.1881)
		N=754,920			

s.e. clustered by state (27) in parentheses (significance levels: * 10%, ** 5%, ***1%). Sample, outcomes, and specifications as defined in Table I. The table shows that the results in the bottom panel of Table I are robust to not including individual controls, to focusing on a period of sustained economic growth (2002–2009), to excluding displaced formal workers with very high or very low replacement rates, and to excluding the least and most formal Brazilian states.

Table A.3: Sample distribution for the 1996 temporary UI extension

Year	Month	Control	Rio de Janeiro	Other treatment	All
1995	April	.3	.23	.47	38,427
	May	.31	.23	.46	43,973
1996	April	.33	.22	.45	34,722
	May	.36	.2	.44	35,112
1997	April	.32	.23	.46	40,810
	May	.33	.23	.45	40,953
All	Both	.32	.22	.45	233,997

The table displays the distribution of the sample of analysis for the 1996 temporary UI extension (see Table II) across areas and years. Treatment areas are relatively larger in every year, constituting 68% of the sample (22% of the sample is composed of workers from Rio de Janeiro).

Table A.4: Sample composition for the 1996 temporary UI extension

Variable	Year	Control	Rio de Janeiro	Other treatment	$T-C$	(s.e.)
Male	1995&1997	.6581	.6747	.669	.0128	(.0121)
	1996	.6626	.6709	.6602	.0009	(.014)
Age	1995&1997	32.52	34.47	33.28	1.153***	(.3276)
	1996	33.26	34.73	33.81	.8352***	(.2965)
Years of education	1995&1997	7.333	7.642	7.421	.1612	(.1368)
	1996	7.163	7.483	7.371	.2428*	(.1378)
Tenure	1995&1997	59.47	62.75	58.65	.5401	(1.806)
	1996	63.54	63.91	63.28	-.0657	(2.478)
Log. real wage	1995&1997	6.966	6.965	6.931	-.0241	(.0899)
	1996	6.999	6.992	6.966	-.0247	(.0944)
Replacement rate	1995&1997	.4701	.4744	.4831	.0101	(.0289)
	1996	.4767	.4847	.4923	.0133	(.03)
Services	1995&1997	.3008	.4486	.3642	.0915**	(.0362)
	1996	.2926	.4629	.3755	.1106***	(.0403)
Trade	1995&1997	.2597	.2379	.2498	-.0138	(.0198)
	1996	.2543	.2472	.2543	-.0023	(.0213)
Industry	1995&1997	.3778	.2501	.308	-.0891	(.0565)
	1996	.3909	.2321	.3028	-.1105*	(.0592)
Firm size $<$ 10 employees	1995&1997	.2803	.2453	.2479	-.0333**	(.0149)
	1996	.2866	.2503	.2658	-.0257	(.0191)
Firm size $\geq$ 100 employees	1995&1997	.3784	.4077	.4148	.034	(.0239)
	1996	.3927	.3981	.3874	-.0019	(.0282)
Unemployment rate (state)	1995&1997	.0466	.0571	.0469	.0037	(.0053)
	1996	.0433	.0566	.0473	.0069	(.0052)
Share non–farm private	1995&1997	.2432	.2919	.2002	-.0125	(.0312)
formal employees (state)	1996	.2373	.282	.202	-.0099	(.0282)

s.e. clustered by metropolitan area (29) in parentheses (significance levels: * 10%, ** 5%, ***1%). The table displays the composition of the sample of analysis for the 1996 temporary UI extension (see Table II) across areas and years. The last two columns display estimates of simple differences between control and treatment areas in control or treatment years with their standard errors. There are a few differences between workers from control and treatment areas, but these differences appear every year. Workers in treatment areas are more likely to be older and to come from the service sector. We find no differences in measures of unemployment and formal employment at the state level (the nine historical metropolitan areas and the Federal District are the only urban areas separately identified in yearly PNAD surveys).

Table A.5: Difference–in–difference results for the 1996 temporary UI extension (robustness)

	Outcomes			
	UI takeup rate (1)	Regular UI duration (months) (2)	Extended UI duration (months) (3)	Extended UI duration vs. counterfactual (months) (4)
	Controlling for individual characteristics			
TreatArea × Year1996	-.0168	-.0004	1.859***	.2378***
	(.0149)	(.0031)	(.0118)	(.0201)
	N=233,997		N=171,790	
	Replacement rate between 20% and 80%			
TreatArea × Year1996	-.0135	.0013	1.864***	.2646***
	(.0182)	(.0035)	(.0117)	(.0247)
	N=167,025		N=127,110	
	Excluding Rio de Janeiro			
TreatArea × Year1996	-.007	-.0025	1.857***	.256***
	(.0178)	(.0031)	(.0172)	(.0241)
	N=182,075		N=135,601	
	Using only 1995 as control year			
TreatArea × Year1996	-.011	-.0007	1.858***	.2634***
	(.0159)	(.0036)	(.0127)	(.019)
	N=152,234		N=110,095	
	Using only 1997 as control year			
TreatArea × Year1996	-.0201	.0015	1.86***	.2275***
	(.0168)	(.0037)	(.0126)	(.0251)
	N=151,597		N=112,454	
	Placebo experiment around following election year (2000)			
TreatArea × Year2000	-.0042	.0018	.0249	-.002
	(.0136)	(.0033)	(.0235)	(.0115)
	N=219,124		N=171,474	

s.e. clustered by metropolitan area (29) in parentheses (significance levels: * 10%, ** 5%, ***1%). Sample, outcomes, and specifications as defined in Table II. The table shows that the results in Table II are robust to controlling for individual characteristics, to excluding displaced formal workers with very high or very low replacement rates, to excluding workers from Rio de Janeiro (largest treatment area), and to using either one of our control years. Moreover, a mirror analysis around the next local election in 2000 (placebo experiment) finds no effect (using similarly selected workers laid off in April and May 1999, 2000, and 2001). Individual characteristics in the top panel include year, metropolitan area, calendar separation month, sector of activity (21), education (4), gender, and firm size (10) fixed effects, as well as 4th order polynomials in age, tenure and log real wage before layoff.

Table A.6: Validity checks for the regression discontinuity around the 24–month tenure cutoff

	Mean	Job tenure $\geq$ 24 months	
	$20 \leq Tenure < 22$	(1)	(2)
Number of observations per tenure bin	37,738	502.9	4,060
		(5,048)	(6,804)
		N=55	N=40
Male	.5988	-.0046	-.0026
		(.005)	(.0068)
Age	29.78	-.202***	-.2058***
		(.0488)	(.0575)
Years of education	8.592	.0089	-.0112
		(.0184)	(.0226)
Log. real wage	6.636	.0351	.0553
		(.0397)	(.0509)
Replacement rate	.7035	-.0108	-.0175
		(.0123)	(.0158)
Service	.3363	.0045	.0056
		(.0044)	(.0047)
Trade	.3657	.0184**	.0041
		(.0091)	(.0105)
Industry	.2397	-.0201**	-.0063
		(.0083)	(.0097)
Firm size < 10 employees	.4279	-.0005	-.0094
		(.0228)	(.031)
Firm size $\geq$ 100 employees	.2074	-.002	-.0008
		(.015)	(.0209)
Share non–farm private formal employees	.2852	-.0009	.0005
		(.0026)	(.0034)
		N=1,964,426	N=1,408,999
Predicted counterfactual benefit duration	4.833	-.0005	-.0007
		(.002)	(.0027)
		N=1,644,695	N=1,179,534
Tenure window		16–32	18–30

s.e. clustered by week of tenure in parentheses (significance levels: * 10%, ** 5%, ***1%). The table displays validity checks for a RD design around the 24–month tenure cutoff. In particular, we check whether the layoff density and the means of observable characteristics trend smoothly with job tenure through the 24-month cutoff (Lee, 2008). We consider tenure windows of either eight months or six months around the cutoff (col. 1 and 2, respectively). The first row checks for a discontinuity in the density of job tenure at layoff (by week of tenure) at the cutoff using the specification in equation (10). The coefficients are small and not statistically significant. The next rows check for a discontinuity in sample composition at the cutoff using the specification in equation (11). Estimates are neither economically nor statistically significant for gender, years of education, log real wages, replacement rates, firm size, the share of non–farm private formal employees (state–level), and for whether a worker comes from the construction or the service sector. Estimates are statistically significant, but only with the wider tenure window, for whether a worker comes from the trade or the industrial sector. Estimates are statistically, but not economically, significant for age. The last row assesses the degree of potential bias from the small amount of selection. We estimate the effect of covariates on our main outcome of interest, the counterfactual benefit duration displayed in Figure VIIb, $y = X\phi$. X include year, state, calendar separation month, sector of activity (21), education (4), gender, and firm size (10) fixed effects, as well as 4th order polynomials in age and log real wage before layoff. We then predict the outcome for each observation using the estimated $\widehat{\phi}$ vector and we regress it by WLS using the specification in equation (11). The estimated coefficients are close to zero. We conclude that any discontinuity in reemployment behaviors at the cutoff can be attributed to the causal effect of maximum benefit duration.

Table A.7: Overall regression discontinuity results for the 24–month tenure cutoff (robustness)

	UI takeup rate (1)	UI duration up to 4 months (months) (2)	Extended UI duration (months) (3)	Extended UI duration vs. counterfactual (months) (4)
		With individual controls		
Job tenure $\geq$ 24 months	.0071	.0048***	.8949***	.0824***
	(.0099)	(.0011)	(.004)	(.0026)
	N=1,964,426		N=1,644,695	
		Tenure window 18–30		
Job tenure $\geq$ 24 months	.0159	.0064***	.8881***	.0798***
	(.0119)	(.0017)	(.0056)	(.0048)
	N=1,408,999		N=1,179,534	
		Only years after 2002		
Job tenure $\geq$ 24 months	.0067	.006***	.8979***	.0893***
	(.0096)	(.0016)	(.0043)	(.0038)
	N=1,544,491		N=1,299,048	
		Replacement rate between 20% and 80%		
Job tenure $\geq$ 24 months	.0087	.0054***	.882***	.0777***
	(.0097)	(.0013)	(.0049)	(.004)
	N=1,035,298		N=860,061	
		Excluding states with highest and lowest share of formal employees		
Job tenure $\geq$ 24 months	.0077	.0048***	.898***	.0829***
	(.0099)	(.0014)	(.0044)	(.0042)
	N=1,361,566		N=1,145,133	

s.e. clustered by week of tenure in parentheses (significance levels: * 10%, ** 5%, ***1%). Sample, outcomes, and specifications as defined in Table III. The table shows that the results in Table III are robust to controlling for individual characteristics, to using a smaller tenure window, to focusing on a period of sustained economic growth (2002–2009), to excluding displaced formal workers with very high or very low replacement rates, and to excluding the least and most formal Brazilian states. Individual characteristics in the top panel include year, state, calendar separation month, sector of activity (21), education (4), gender, and firm size (10) fixed effects, as well as 4th order polynomials in age and log real wage before layoff.

Table A.8: Formal employment and regression discontinuity results (robustness I)

	UI takeup rate [mean]	UI takeup rate [RD]	Mechanical cost (months)	Max. efficiency cost ($/$)	Behavioral cost (months)	Efficiency cost ($/$)
	(1)	(2)	(3)	(4)	(5)	(6)
Only including the share of non–farm private formal employees						
Without individual controls						
At the mean	.8296***	.008	.8301***	.255***	.0804***	.0969***
	(.0083)	(.0095)	(.0019)	(.0035)	(.0031)	(.0039)
Δ form. employee	-.1759***	-.0023	-.2373***	.4966***	.0839***	.1291***
	(.0203)	(.0218)	(.0152)	(.0249)	(.0201)	(.0255)
	N=1,996,050			N=1,670,505		
Allowing for heterogeneous effects with individuals' characteristics						
At the mean	.8296***	.0042	.8301***	.255***	.0864***	.1041***
	(.0083)	(.0217)	(.0019)	(.0035)	(.0091)	(.0109)
Δ form. employee	-.0079	-.0565	-.249***	.5046***	.0584	.1018**
	(.0398)	(.0353)	(.0268)	(.0521)	(.0377)	(.0487)
	N=1,996,050			N=1,670,505		
Tenure window 18–30						
At the mean	.8202***	.0157	.8319***	.2544***	.0814***	.0979***
	(.0107)	(.0121)	(.0026)	(.0048)	(.0033)	(.0042)
Δ form. employee	-.0028	-.0253	-.2802***	.5538***	.1162***	.1733***
	(.0235)	(.021)	(.0224)	(.0399)	(.0254)	(.0318)
	N=1,408,999			N=1,179,534		
Only years after 2002						
At the mean	.8337***	.0086	.8097***	.294***	.0889***	.1098***
	(.0083)	(.0097)	(.0018)	(.0035)	(.0028)	(.0036)
Δ form. employee	-.0799**	-.0025	-.2628***	.5797***	.0817***	.1369***
	(.0368)	(.0252)	(.0213)	(.0389)	(.0251)	(.0322)
	N=1,569,187			N=1,319,354		
Replacement rate between 20% and 80%						
At the mean	.8213***	.0089	.833***	.2528***	.0795***	.0954***
	(.0081)	(.0095)	(.0021)	(.0039)	(.0028)	(.0036)
Δ form. employee	.0158	-.0145	-.2232***	.3975***	.0735***	.1141***
	(.0253)	(.0205)	(.0195)	(.038)	(.0221)	(.0272)
	N=1,052,917			N=874,367		
Excluding states with highest and lowest share of formal employees						
At the mean	.8325***	.0097	.8336***	.2482***	.0844***	.1012***
	(.0088)	(.0101)	(.0026)	(.0044)	(.0031)	(.004)
Δ form. employee	-.1059***	.028	-.2861***	.5528***	.1615***	.2293***
	(.0381)	(.03)	(.0222)	(.0356)	(.023)	(.029)
	N=1,383,417			N=1,163,097		

s.e. clustered by week of tenure in parentheses (significance levels: * 10%, ** 5%, ***1%). Sample, outcomes, and specifications as defined in Table IV. The table shows that the results in the top panel of Table IV are robust to not controlling for individual characteristics, to allowing RD estimates to vary with individuals' characteristics, to using a smaller tenure window, to focusing on a period of sustained economic growth (2002–2009), to excluding displaced formal workers with very high or very low replacement rates, and to excluding the least and most formal Brazilian states. The top panel still includes year and state fixed effects. The second panel includes the following individual characteristics (in addition to year, state, and calendar separation months fixed effects), both directly and interacted with the linear controls in tenure on each side of the cutoff, as well as with the indicator for observations above the cutoff: gender fixed–effects, a continuous measure of education, three categories of firm size, four categories of sector of activity, and linear controls in age, tenure, log real wage before layoff, and UI replacement rate.

Table A.9: Formal employment and regression discontinuity results (robustness II)

	Including more detailed labor market composition					
	UI takeup rate [mean]	UI takeup rate [RD]	Mechanical cost (months)	Max. efficiency cost ($/$)	Behavioral cost (months)	Efficiency cost ($/$)
	(1)	(2)	(3)	(4)	(5)	(6)
			Without individual controls			
Δ form. employee	.1636	.1134	-.9522***	2.002***	.5187***	.8077***
−Δ unemployed	(.1144)	(.132)	(.1085)	(.2135)	(.0031)	(.215)
Δ form. employee	.1155**	-.0164	-.4796***	1.038***	.2662***	.3714***
−Δ inf. worker	(.0562)	(.0628)	(.0354)	(.0784)	(.0518)	(.0621)
Δ form. employee	.0873	-.1128	-.2458***	.722***	-.0758	-.0628
−Δ inf. employee	(.0621)	(.0776)	(.0594)	(.1313)	(.1096)	(.1369)
Δ form. employee	.1435*	.0791	-.7347***	1.391***	.6058***	.8229***
−Δ self–employed	(.0738)	(.0876)	(.0856)	(.1418)	(.0785)	(.1033)
	N=1,996,050			N=1,670,505		
			Allowing for heterogeneous effects with workers' characteristics			
Δ form. employee	.1636	.0799	-1.014***	2.061***	.4961***	.7584***
−Δ unemployed	(.1144)	(.1419)	(.104)	(.2125)	(.0135)	(.2305)
Δ form. employee	.1155**	-.0581	-.4233***	.9615***	.1756***	.2762***
−Δ inf. worker	(.0562)	(.0772)	(.0347)	(.0804)	(.0619)	(.0736)
Δ form. employee	.0873	-.1414*	-.3083***	.8023***	.0123	.0602
−Δ inf. employee	(.0621)	(.0841)	(.0654)	(.1337)	(.1117)	(.1396)
Δ form. employee	.1435*	.0337	-.5647***	1.165***	.358***	.5174***
−Δ self–employed	(.0738)	(.1119)	(.0924)	(.1442)	(.0865)	(.1129)
	N=1,996,050			N=1,670,505		
			Tenure window 18–30			
Δ form. employee	.1503	.1475	-1.049***	2.211***	.6755***	.9469***
−Δ unemployed	(.1355)	(.1622)	(.1461)	(.2931)	(.0032)	(.2766)
Δ form. employee	.2049***	-.1163	-.5476***	1.063***	.3134***	.4439***
−Δ inf. worker	(.0769)	(.0863)	(.045)	(.0985)	(.0612)	(.0766)
Δ form. employee	.1728***	-.2247**	-.3158***	.6241***	-.1006	-.0841
−Δ inf. employee	(.0642)	(.0956)	(.0856)	(.177)	(.1471)	(.1842)
Δ form. employee	.2365*	-.0106	-.7904***	1.522***	.7234***	.9718***
−Δ self–employed	(.1225)	(.1195)	(.1227)	(.2194)	(.1321)	(.1737)
	N=1,408,999			N=1,179,534		

s.e. clustered by week of tenure in parentheses (significance levels: * 10%, ** 5%, ***1%). Sample, outcomes, and specifications as defined in Table IV. The table shows that the results in the bottom panel of Table IV are robust to not controlling for individual characteristics, to allowing RD estimates to vary with individuals' characteristics, and to using a smaller tenure window. The top panel still includes year and state fixed effects. The second panel includes the following individual characteristics (in addition to year, state, and calendar separation months fixed effects), both directly and interacted with the linear controls in tenure on each side of the cutoff, as well as with the indicator for observations above the cutoff: gender fixed–effects, a continuous measure of education, three categories of firm size, four categories of sector of activity, and linear controls in age, tenure, log real wage before layoff, and UI replacement rate.

Table A.10: Formal employment and regression discontinuity results (robustness III)

	Including more detailed labor market composition					
	UI takeup rate [mean] (1)	UI takeup rate [RD] (2)	Mechanical cost (months) (3)	Max. efficiency cost ($/$) (4)	Behavioral cost (months) (5)	Efficiency cost ($/$) (6)
Only years after 2002						
Δ form. employee	.2674**	.1697	-1.068***	2.098***	.6746***	.9908***
−Δ unemployed	(.1101)	(.1403)	(.1364)	(.2857)	(.0028)	(.2848)
Δ form. employee	.1509**	-.069	-.4379***	1.032***	.2152***	.3268***
−Δ inf. worker	(.0592)	(.068)	(.0365)	(.0872)	(.0549)	(.0697)
Δ form. employee	.3233***	-.2132**	-.4382***	1.135***	.149	.2447*
−Δ inf. employee	(.0791)	(.097)	(.0506)	(.1252)	(.1079)	(.1356)
Δ form. employee	-.02	.0738	-.4232***	.888***	.2808***	.4062***
−Δ self–employed	(.072)	(.0962)	(.0873)	(.1803)	(.0998)	(.1317)
	N=1,569,187			N=1,319,354		
Replacement rate between 20% and 80%						
Δ form. employee	.0108	.0879	-.9172***	1.973***	.5824***	.8142***
−Δ unemployed	(.1808)	(.192)	(.1436)	(.2793)	(.0029)	(.2845)
Δ form. employee	.1911***	.0399	-.4374***	.8192***	.3665***	.4932***
−Δ inf. worker	(.0704)	(.0775)	(.0767)	(.1259)	(.0885)	(.1116)
Δ form. employee	.2717**	-.0756	-.168	.4744**	-.0663	-.0605
−Δ inf. employee	(.1227)	(.1333)	(.1041)	(.2002)	(.1757)	(.2193)
Δ form. employee	.1141	.15	-.7056***	1.191***	.7807***	1.028***
−Δ self–employed	(.0865)	(.0962)	(.1566)	(.2668)	(.1709)	(.2252)
	N=1,052,917			N=874,367		
Excluding states with highest and lowest share of formal employees						
Δ form. employee	.3782**	.1633	-.9393***	1.824***	.6161***	.8627***
−Δ unemployed	(.1466)	(.149)	(.1179)	(.2508)	(.0031)	(.2508)
Δ form. employee	.025	-.0274	-.475***	.9698***	.3087***	.4304***
−Δ inf. worker	(.0661)	(.0726)	(.0428)	(.0859)	(.0596)	(.0755)
Δ form. employee	.1043	-.1046	-.2678***	.6614***	-.0411	-.0168
−Δ inf. employee	(.0735)	(.092)	(.0578)	(.1191)	(.0967)	(.1206)
Δ form. employee	-.0706	.0656	-.7303***	1.352***	.7277***	.9703***
−Δ self–employed	(.13)	(.1313)	(.0971)	(.1711)	(.1125)	(.1471)
	N=1,383,417			N=1,163,097		

s.e. clustered by week of tenure in parentheses (significance levels: * 10%, ** 5%, ***1%). Sample, outcomes, and specifications as defined in Table IV. The table shows that the results in the bottom panel of Table IV are robust to focusing on a period of sustained economic growth (2002–2009), to excluding displaced formal workers with very high or very low replacement rates, and to excluding the least and most formal Brazilian states.

Table A.11: Long–term effects of increases in maximum benefit duration

	Months employed (months) in next 2 years	Laid off again in in next 2 years	Log real wage (R$) at formal reemployment	Log real wage (R$) if formally employed in Dec. 2 years later	
	(1)	(2)	(3)	(4)	(5)
			1996 temporary UI extension		
TreatArea × Year1996	-1.227***	-.0154***	.0019	-.035**	-.0161
	(.1087)	(.0028)	(.0132)	(.0137)	(.0146)
Mean (T. areas, C. years)	7.53	.1	6.56	6.71	6.71
	N=171790	N=171790	N=102850	N=68318	N=68318
			24–month tenure cutoff		
Job tenure $\geq$ 24 months	-.2199***	-.0059***	-.0031	-.0076	.0022
	(.0553)	(.0014)	(.0056)	(.009)	(.0082)
Mean ($20 \leq Tenure < 22$)	7.54	.12	6.49	6.69	6.69
	N=1475267	N=1475267	N=955938	N=603419	N=603419

In parentheses, s.e. clustered by metropolitan area (29) in the top panel and by week of tenure in the bottom panel (significance levels: * 10%, ** 5%, ***1%). Outcomes are conditional on takeup. Samples as in Tables II and III. The table displays the impacts of the 1996 temporary UI extension (two months) and of the increase in maximum benefit duration at the 24–month tenure cutoff (one month) on longer–term outcomes. The outcome in column (1) is the total number of months a worker was formally employed in the two years following layoff. The outcome in column (2) is a dummy for whether a worker experienced a new layoff from a private formal job in the two years following layoff. Outcomes in columns (3)–(5) are conditional on formal reemployment within 2 years. The outcomes are the logarithm of the real wage at formal reemployment (col. 3) and in December two years after layoff if formally employed then (col. 4 and 5). We have data on monthly wages for December only; our other wage measure is the average wage over the employment relationship in a given year. The regression in column (5) includes fixed effects for the (endogenous) duration out of formal employment. The sample is restricted to workers laid off before January 2009 so that we observe two years post–layoff for all workers. Increasing maximum benefit duration decreases the number of months of formal employment in the two years after layoff, but also the share of workers experiencing a new layoff from the formal sector (one must be formally reemployed to be laid off again). These results motivate the choice of a steady state budget constraint in Section 2 (infinite horizon). We find no impact on wages. The coefficient in column (4, top panel) is closer to zero and no longer significant in column (5, top panel). The difference may be due to tenure effects as treated beneficiaries delayed formal reemployment. Exchange rate: R1.9\simeq$US$1 (in R$ of 2000).

Table A.12: Validation of counterfactual approach

	Extended UI duration (months) (measured with UI data) (1)	Extended UI duration (months) (inferred from reemployment data) (2)
	1996 temporary UI extension	
TreatArea × Year1996	1.859***	1.851***
	(.0125)	(.0156)
Mean (T. areas, C. years)	4.97	4.97
	N=171,790	
	24–month tenure cutoff	
Job tenure $\geq$ 24 months	.8949***	.9109***
	(.0043)	(.0024)
Mean ($20 \leq Tenure < 22$)	4.01	3.96
	N=1,644,695	

In parentheses, s.e. clustered by metropolitan area (29) in the top panel and by week of tenure in the bottom panel (significance levels: * 10%, ** 5%, ***1%). Outcomes are conditional on takeup. Samples and specifications as in Tables II and III. The table tests the accuracy of our approach used throughout the paper that relies on the timing of workers' formal reemployment to construct counterfactual benefit durations had workers been eligible for longer UI benefits. The table displays estimates of the impact of increasing maximum benefit duration on average benefit duration either measured with UI data (col. 1), as in column (3) of Tables II and III, or inferred from the timing of workers' formal reemployment (col. 2). We accurately predict changes in average benefit duration using the timing of workers' formal reemployment.

A.2 A model of job–search with informal work opportunities

We present here a model of endogenous job–search with informal work opportunities that fits within our conceptual framework. As discussed in Chetty (2006), the set of sufficient statistics capturing our tradeoff of interest is robust to many assumptions regarding the underlying model of job search (e.g., introducing heterogeneity or other margins of behaviors), as long as an envelope condition applies to the agents' problem. To simplify derivations and notations, we first assume a fixed horizon of T periods, but we set up the problem such that the budget constraint of the social planner is consistent with the steady state budget constraint (1). In particular, we assume that UI taxes are levied on workers who do not lose their formal job, as in Chetty (2006) and Kroft (2008). We later show how the results carry on to an infinite horizon model.

A.2.1 The finite horizon model

Workers' problem. Assume a population of formal employees of measure 1 living for T periods. At the beginning of period 1, they lose their formal job with some probability q. Workers who do not lose their formal job stay employed until T, earning wage w^f, and paying tax τw^f each period. Their per–period utility is $v\left(w^f(1-\tau)\right)$. $v(.)$ is assumed to be strictly concave. In this setup, the average number of contribution periods to the UI system for a given layoff (D^f in section 2) is $\frac{[(1-q)T]}{q}$. Upon layoff, workers become unemployed and eligible for a fixed level of UI benefits $b_t \equiv rw^f$, with replacement rate r, for a maximum duration of P periods. The incidence of taxes and benefits is assumed to fall on workers.

Upon layoff, an unemployed worker faces a decision tree similar to the one in Figure IIa. First, she must decide each period how much effort e at a cost $\theta z_e(e)$ to invest in finding a new job. Search efforts are normalized to correspond to job–finding probabilities. Cost functions are assumed to be convex. With probability $1-e$, she remains unemployed. She finds a formal job with probability pe and an informal job (self–employed or informal employee) with probability $(1-p)e$. Upon finding an informal job, she can keep searching for a formal job with effort f at a cost $\theta z_f(f)$. She finds a formal job with probability pf. Working informally, she earns wage $w^i < w^f$. She can always keep searching for a formal job at the same cost $\theta z_f(f)$ in subsequent periods. Upon finding a formal job, she can try hiding the new formal job (delay the signature of the working card). She is successful with probability h at hiding costs $\theta_h z_h(h)$. Individuals who are unemployed, working informally, or hiding a new formal job draw UI benefits b in the first P periods after layoff.[64]

[64] The two views on informality correspond to (i) low formal search costs, low hiding costs and a small wage differential, or (ii) a low value of p, high formal search costs, and a large wage differential.

Formally, the value function of being unemployed at the start of a period J_t^u solves:

$$J_t^u = \max_{e_t} (1-e_t) V_t + (1-p) e_t J_t^i + p e_t J_t^f - \theta z_e(e_t)$$

where J_t^i is the value function of having an informal job in period t with the option to keep searching for a formal job. It solves:

$$J_t^i = \max_{f_t} p f_t J_t^f + (1-p f_t) X_t - \theta z_f(f_t)$$

where J_t^f is the value function of having a formal job in period t with the option to try hiding it. It solves:

$$J_t^f = \max_{h_t} h_t Y_t + (1-h_t) Z_t - \theta_h z_h(h_t)$$

V, X, Y, and Z are respectively the value function of being unemployed, working informally, hiding a formal job, and being formally employed in a given period (after outcomes are realized). We have:

$$\begin{aligned}
V_t &= v(o+b_t) + J_{t+1}^u \\
X_t &= v\left(w^i + b_t\right) + J_{t+1}^i \\
Y_t &= v\left(w^f + b_t\right) + J_{t+1}^f \\
Z_t &= v\left(w^f\right) + Z_{t+1}
\end{aligned}$$

where $b_t = b$ for $t = 1 \ldots P$ and $b_t = 0$ otherwise. We assume that the unemployed have a minimum consumption level o. The workers' problem is to maximize J_1^u by choosing optimal levels of search and hiding efforts in each period until (declared) formal reemployment.[65] At an optimum, we have:

$$\begin{aligned}
\theta z_e'(e_t) &= p J_t^f + (1-p) J_t^i - V_t \\
\theta z_f'(f_t) &= p J_t^f - p X_t \\
\theta_h z_h'(h_t) &= Y_t - Z_t
\end{aligned}$$

The solution to this dynamic problem determines the survival rates out of formal employment S_t, and therefore the average UI benefit duration, $B \equiv \sum_{t=1}^{P} S_t$. Survival rates out of formal employment are the sum of the shares of displaced formal employees who are unemployed (U_t), working informally (I_t), or hiding a formal job (H_t) at the end of period t (with $U_0 = 1$, $I_0 = 0$, and $H_0 = 0$).

[65] Simulations in Chetty (2008) suggest that this class of models is well defined.

The share formally reemployed in a given period is $[U_{t-1}(pe_t + (1-p)e_t p f_t) + I_{t-1} p f_t + H_{t-1}](1-h_t)$.

Following Schmieder, von Wachter and Bender (2012), we assume that P can be increased by a fraction of 1 such that a marginal change in P can be analyzed. A marginal change in P then corresponds to a marginal change in b_{P+1}, the benefit amount after regular UI exhaustion, times b. In particular, we obtain the following comparative statics for a one–period change in b_{P+1}:

$$\frac{dh_{P+1-n}}{db_{P+1}} > 0, \quad \frac{df_{P+1-n}}{db_{P+1}} < 0, \quad \frac{de_{P+1-n}}{db_{P+1}} < 0$$

where $0 \leq n \leq P$. Increasing P reduces search efforts and increases hiding efforts both in the affected period and in earlier periods. These behavioral responses, which increase survival rates out of formal employment until the maximum benefit duration, increase average benefit duration through a *behavioral* cost: $\sum_{t=1}^{P+1} \frac{dS_t}{dP} = \sum_{t=1}^{P+1} \frac{dS_t}{db_{p+1}} b_{p+1}$. Increasing P also increases average benefit duration through a *mechanical* cost: S_{P+1}.

Social planner's problem. The problem of the social planner is to choose the maximum benefit duration P that maximizes welfare W such that a balanced–budget constraint holds:

$$\begin{aligned} \max_P W &= qJ_1^u + (1-q)Tv\left(w^f(1-\tau)\right) \\ \text{s.t. } \tau &= rB\frac{q}{[(1-q)T]} \end{aligned}$$

As workers choose search efforts optimally, we can use the envelope theorem to solve the planner's problem. The welfare effect of increasing P by one period is (first–order condition):

$$\frac{dW}{dP} = q\, b\, S_{P+1}\left(\frac{U_{P+1}}{S_{P+1}} g^{U_{P+1}} + \frac{S_{P+1} - U_{P+1}}{S_{P+1}} g^{IH_{P+1}}\right) - (1-q)\, Tw^f\, \frac{d\tau}{dP}\, g^E$$

where $g^{U_{P+1}}$, $g^{IH_{P+1}}$, and g^E are the social value of \$1 for the average unemployed mechanical beneficiary (UI exhaustee), the average mechanical beneficiary working informally or hiding a formal job, and the average UI contributor (formal employee), respectively. Formally, we have:

$$\begin{aligned} g^{U_{P+1}} &= v'(o + b_{P+1}) \\ g^{IH_{P+1}} &= \frac{I_{P+1}v'\left(w^i + b_{P+1}\right) + H_{P+1}v'\left(w^f + b_{P+1}\right)}{I_{P+1} + H_{P+1}} \\ g^E &= v'\left(w^f(1-\tau)\right) \end{aligned}$$

We aggregate individuals working informally and hiding a new formal job together because they

are not separately identified in survey data. Reorganizing, we obtain:

$$\frac{dW}{dP} = q\,r\,w^f\ S_{P+1}\left(\frac{U_{P+1}}{S_{P+1}}g^{U_{P+1}} + \frac{S_{P+1}-U_{P+1}}{S_{P+1}}g^{IH_{P+1}}\right) - q\,r\,w^f\ \left(S_{P+1} + \sum_{t=1}^{P+1}\frac{dS_t}{dP}\right)\ g^E$$

$$\frac{d\widetilde{W}}{dP} = \frac{dW/dP}{g^E w^f} = qr\,S_{P+1}\left[\left(\frac{U_{P+1}}{S_{P+1}}\frac{g^{U_{P+1}}-g^E}{g^E} + \frac{S_{P+1}-U_{P+1}}{S_{P+1}}\frac{g^{IH_{P+1}}-g^E}{g^E}\right) - \widetilde{\eta}\right]$$

where $\widetilde{\eta} \equiv \sum_{t=1}^{P+1}\frac{dS_t/dP}{S_{P+1}}$ is the ratio of the behavioral cost to the mechanical cost. It measures the efficiency costs of increasing UI maximum benefit duration. Welfare effects are positive if the *social value of insurance*, the first of the two terms in brackets, exceeds the measure of efficiency.

Efficiency and informality. Some parameters of our model capture features often associated with a labor market's degree of informality. For instance, in a labor market where the share of formal employees increases and the share of informal workers decreases, it might become easier to find a formal job ($p\uparrow$) or harder to hide a new formal job ($\theta_h\uparrow$), or formal wages may increase ($w^f\uparrow$). In contrast, when the share of unemployed workers decreases, it might simply become easier to find any job ($\theta\uparrow$). In our model, we obtain the following comparative statics:

$$\begin{array}{lll}
\frac{dh_t}{dp} = 0, & \frac{df_t}{dp} > 0, & \frac{de_t}{dp} > 0 \\
\frac{dh_t}{d\theta_h} < 0, & \frac{df_t}{d\theta_h} < 0, & \frac{de_t}{d\theta_h} < 0 \\
\frac{dh_t}{dw^f} < 0, & \frac{df_t}{dw^f} > 0, & \frac{de_t}{dw^f} > 0 \\
\frac{dh_t}{d\theta} = 0, & \frac{df_t}{d\theta} < 0, & \frac{de_t}{d\theta} < 0
\end{array}$$

The mechanical cost decreases, and thus the maximum behavioral cost increases, when it becomes easier to find a formal job ($p\uparrow$) or formal wages increase ($w^f\uparrow$). When it becomes harder to hide a new formal job ($\theta_h\uparrow$), hiding efforts decrease, but job–search efforts may also decrease as one of the benefits of finding a formal job (the possibility to hide it and cumulate formal wage and UI benefits) is reduced. The mechanical cost will decrease and the maximum behavioral cost will increase if the former effect dominates. Finally, the mechanical cost increases when it becomes simply harder to find any job ($\theta\uparrow$). Empirically, we find that the mechanical cost increases when the share of formal employees increases and either the share of informal workers or the share of unemployed workers decreases.

Comparative statics for the behavioral cost depend more strongly on parametic assumptions, e.g. the sign of the third derivative of the cost functions (Schmieder, von Wachter and Bender, 2012). We estimate it in the data.

A.2.2 The infinite horizon model

Consider the discrete time infinite horizon model where a representative agent cycles in and out of formal employment as in section 2. Denote ω_t the agent's state in period t and n_{ω_t} the probability that the agent is in state ω in period t. Because UI benefits are limited in time and the agent can be unemployed, working informally, hiding a formal job, or working formally, there are many possible states: (i) formally employed, (ii) unemployed without UI benefits, (iii) unemployed with UI benefits in period h=1,2,...,P since layoff, (iv) working informally without UI benefits, (v) working informally with UI benefits in period h=1,2,...,P since layoff, (vi) hiding a formal job without UI benefits, (vii) hiding a formal job with UI benefits in period h=1,2,...,P since layoff. In each state, the agent consumes c_ω and invests search efforts e_ω (0 if employed), f_ω (0 if found a formal job), and hiding efforts h_ω (0 if formally employed). The search and hiding efforts, and the layoff probability, determine the transition matrix between states from one period (ω_{t-1}) to the next (ω_t), given the model in section 2. Taking the UI program $\{b, P, \tau\}$ as given, the agent chooses search efforts to maximize the expected utility:

$$\mathbb{E}_1 \sum_{t=1}^{+\infty} \delta^t \left\{ \sum_{\omega_t} n_{\omega_t} u\left(c_{\omega_t}\right) - \sum_{\omega_{t-1}} n_{\omega_{t-1}} \left[\theta z_e\left(e_{\omega_{t-1}}\right) + \theta z_f\left(f_{\omega_{t-1}}\right) + \theta_h z_h\left(h_{\omega_{t-1}}\right)\right] \right\}$$

where $\delta < 1$ is the discount factor and $\mathbb{E}_1$ is the mathematical expectation given the agent's information in period 1. In the steady state of this dynamic model, all variables are constant (n_ω, c_ω, e_ω, f_ω, h_ω) and determine D^f, D^o, and B, the average duration of a formal job, the average duration of a spell out of formal employment, and the average benefit duration, respectively. Given UI benefits b, the planner's problem in the steady state is to choose P to maximize the agents' per–period utility given the per–period budget constraint (1). Using the envelope theorem, we obtain the first–order condition (2). We could assume that there are M types of agents to introduce preferences for redistribution beyond the insurance motive.

B Web Appendix

B.1 Institutional details

B.1.1 The Brazilian UI program

Unemployment insurance was first introduced in March 1986, but with a very small scope. A more complete UI program was established in the 1988 Constitution and approved in January 1990. The law created the Workers' Support Fund (FAT), financed by firms' payments of a .65% tax on total sales. The fund is managed by a committee (CODEFAT) composed of representatives of the government, unions, and employers, and was designed to finance both the UI program and active labor market policies. In June 1994, Law 8900 reformed the UI program, giving it its current format. The 1994 UI legislation also enabled the committee to extend UI for some groups of workers (workers in specific regions and/or sectors of the Brazilian economy) for up to two months without approval of Congress. The only restriction is that expenditures generated by the additional payments should not cost more than 10% of the UI fund's liquidity reserves.

B.1.2 Brazilian labor legislation

The Brazilian labor code (Consolidação das Leis do Trabalho - CLT) was created in 1943, consolidating existing labor laws. Two major revisions were implemented since then: in 1964, when the military regime restricted the power of labor unions, and in the 1988 Constitution, when workers' benefits were increased and workers' rights to organize were reintroduced. CLT is very broad and detailed, containing more than 900 articles (Gonzaga, 2003). Under Brazilian labor legislation, hiring a formal worker is costly. Payroll taxes are high, including 20% for Social Security contributions; 8% deposited in the worker's severance account (see below); and 7.8% for funding an array of programs (training, education, land reform, etc.). Formal workers are also entitled to receive at least the minimum wage, a 13th monthly wage, 30 days of paid leave per year remunerated at 4/3 of the average monthly wage, a maternity leave of 120 days, an overtime rate of 50% for hours exceeding 44 hours a week, etc.

B.1.3 Job security legislations

Despite having a very restrictive labor legislations, job and worker turnover rates are very high in Brazil. Dismissal costs are close to the average of other Latin American countries, but many authors argue that the design of job security programs in Brazil creates perverse incentives that stimulate labor turnover (Amadeo and Camargo, 1996; WorldBank, 2002; Gonzaga, 2003).

As in Europe and other Latin American countries, there are job security provisions in Brazil in addition to UI.

Severance payment accounts. Since 1966, the main component of job security is the FGTS (Fundo de Garantia por Tempo de Serviço) system, a seniority fund scheme. Employers must deposit 8% of a worker's monthly wage into an individual account, managed by Caixa Econômica Federal, a state bank. Deposits earn interest but real rates of return are negative. Employees can only access the account upon layoff or retirement (usually). In the case of layoff, employers must pay a fine to the worker equivalent to 40% of the amount deposited during the worker's tenure at the firm (an additional 10% is paid to the government since 2001).

Advance notice of layoff. The other important component of job security legislation in Brazil is advance notification. The first three months of employment are considered a probationary period in Brazil. Employers laying off workers with more than three months of tenure must provide a worker with a one–month advance notice. Since 2011, workers have been entitled to an advance notice that increases from one to three months depending on seniority. During this month, wages cannot be reduced and employers must allow a worker up to two hours a day to look for a new job.

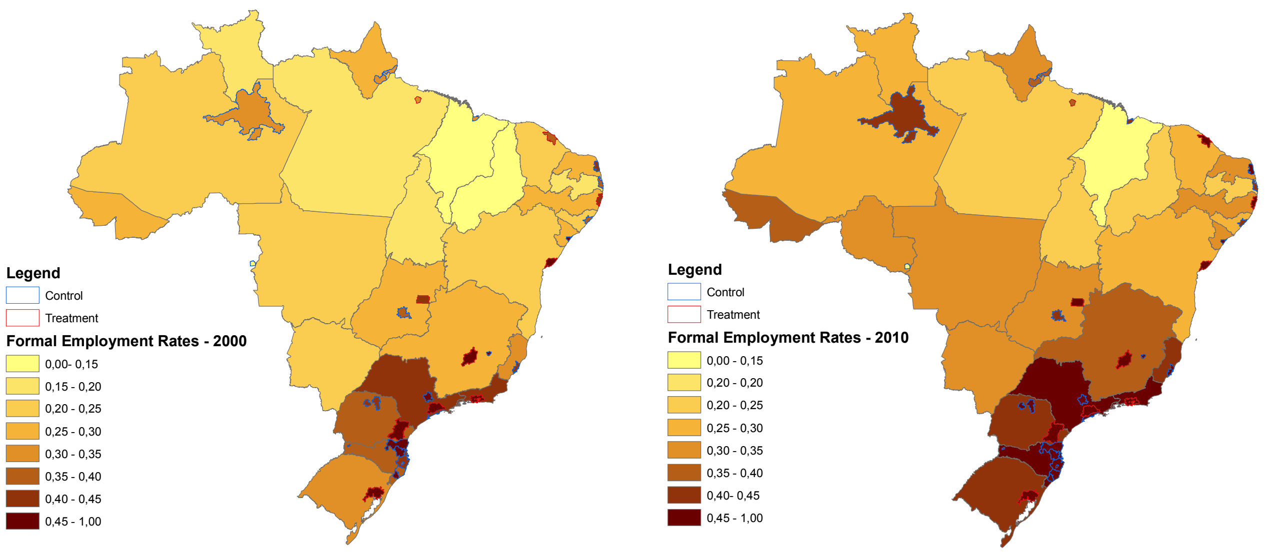
Mediation meeting. Termination of an employment contract for workers with more than 12 months of tenure must be overseen by a union or a Labor Ministry representative. This increases firing costs because of the administrative burden it imposes and because of firms' often imperfect compliance with workers' dues (unpaid wages, overtime compensation, etc.).

B.2 Figures and Tables

Figure B.1: Formal employment rates in Brazil (states and relevant areas for the 1996 temporary UI extension)

(a) Formal employment rates (2000 census)

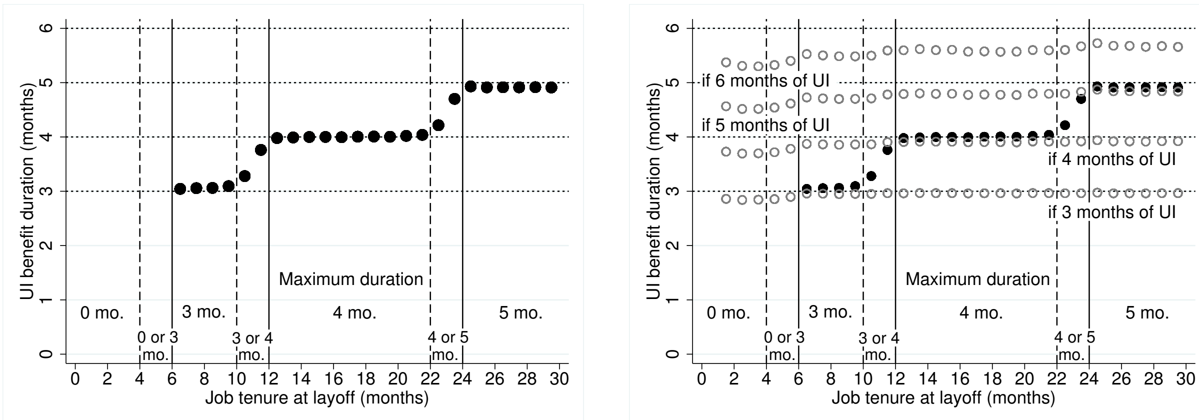
(b) Formal employment rates (2010 census)



The maps display the variation in the share of non–farm private formal employees across states in Brazil (grey lines) using the 2000 (panel a) and 2010 censuses (panel b). The maps show that there is a lot of variation in formal employment rates across states. The North and the Northeast are poorer and less formal. Brazil experienced rapid economic growth in the last decade. Formal employment rates increased across the country (darker shades on panel b) but not uniformly. We also show the variation across cities included in the sample of analysis for the 1996 temporary UI extension (blue lines identify control areas, red lines identify treatment areas). Control and treatment areas are similarly spread across the country and span a similar range of formal employment rates.

Figure B.2: Average benefit duration for UI takers (robustness II)

(a) Actual benefit duration | (b) Actual vs. counterfactual duration

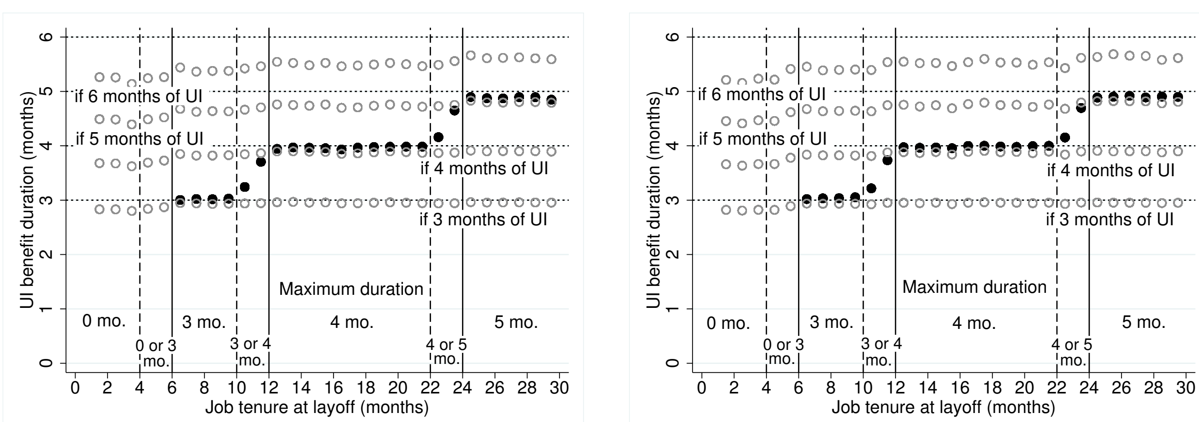


(workers with a single formal job in the previous 36 months, and who had replacement rates between 20% and 80%).

The figure replicates Figure III, excluding UI takers with replacement rates below 20% and above 80%. As in Figure III, average benefit duration is almost exactly equal to maximum benefit duration and our counterfactual benefit durations imply that workers would mechanically draw most additional UI payments following an increase in maximum UI duration. The patterns in Figure III are thus not driven by UI takers with very high replacement rates. Bootstrapped 95% confidence intervals are too tight to be presented in the figure.

Figure B.3: Robustness to selection of workers laid off in the first five months of the year (UI benefit duration)

(a) Actual vs. counterfactual benefit duration if layoff in first 5 months (after 2007) | (b) Actual vs. counterfactual benefit duration if layoff in last 7 months (after 2007)

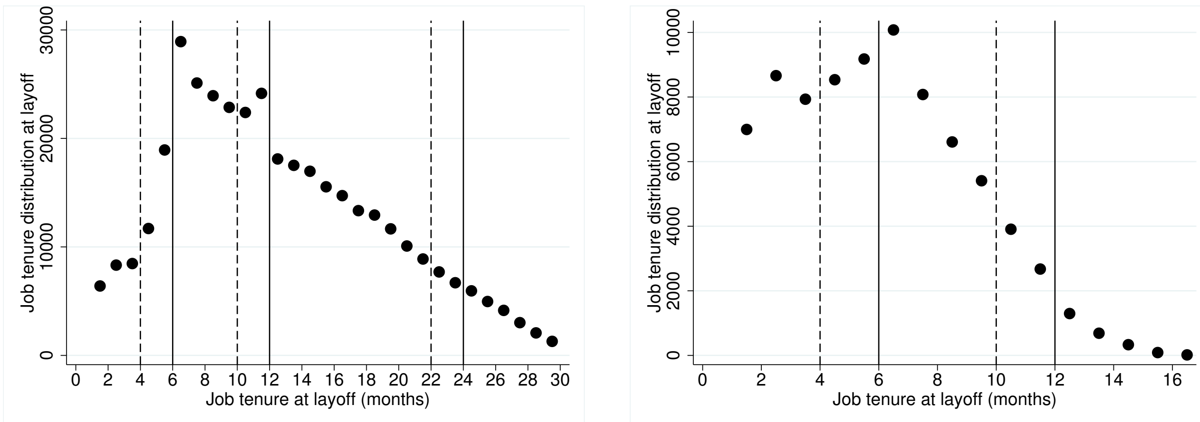


Most results in the paper are based on samples of workers displaced in the first five months of every year because of the limitation in our UI data discussed in section 1.3. We show here that the patterns displayed in Figure III are unlikely to be driven by this sample restriction. In particular, we reproduce Figure IIIb for a similarly selected sample, comparing workers displaced in the first five months of the year (a) and in the next seven months of the year (b) after 2007, when our UI data no longer suffer from the same limitation. The two panels are almost identical. Bootstrapped 95% confidence intervals are too tight to be presented in the figures.

Figure B.4: UI eligibility and layoff rates (suggestive evidence)

(a) Workers with 2+ formal jobs in previous 36 months, and eligible for UI (16–month rule)

(b) Workers with 2+ formal jobs in previous 36 months, but not eligible for UI (16–month rule)



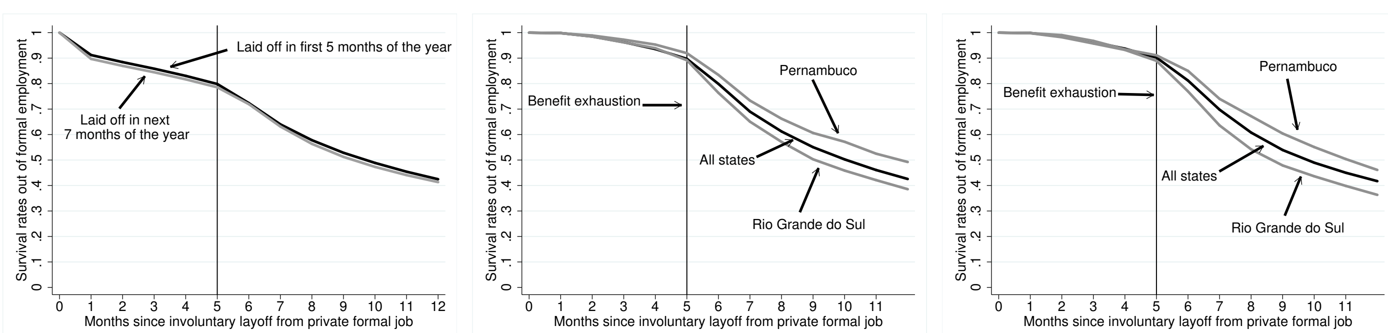
Panels (a) and (b) provide some additional evidence that UI may increase layoff rates of eligible workers, but suggest that such an effect is reduced when layoffs become more costly. In particular, we reproduce Figure A.5a for workers who had several formal jobs in the previous 36 months. We do so separately for workers who are eligible for UI (a) and not eligible for UI (b) because of the 16–month rule (there must be at least 16 months between a worker's layoff date and the layoff date of her last successful application). The increase in layoff rates at six months of job tenure, when formal employees can become eligible for UI, and the decrease in layoff rates at 12 months of job tenure, when firing costs increase, are limited to eligible workers.

Figure B.5: Robustness to selection of workers laid off in the first five months of the year (formal reemployment rates)

(a) Survival rates out of formal employment (2002–2009, unconditional on UI takeup)

(b) Survival rates out of formal employment if layoff in first 5 months (after 2007, UI takers)

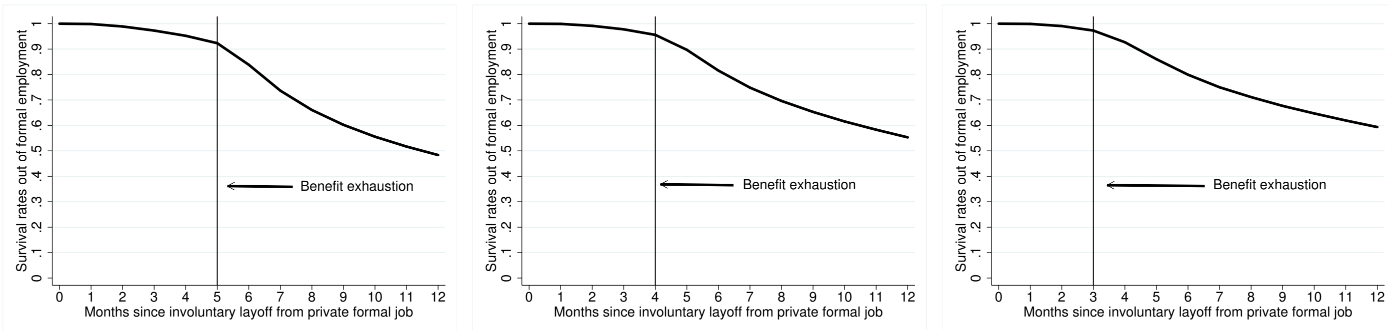
(c) Survival rates out of formal employment if layoff in last 7 months (after 2007, UI takers)



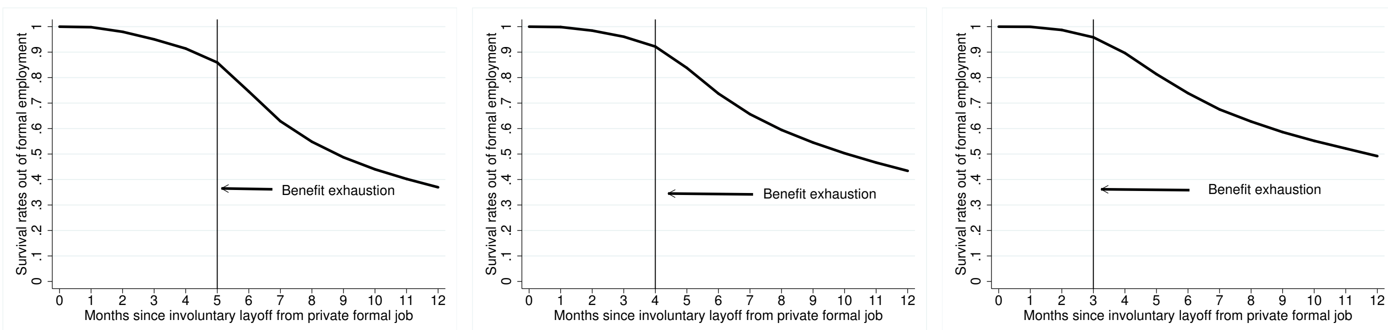
Most results in the paper are based on samples of workers displaced in the first five months of every year because of the limitation in our UI data discussed in section 1.3. We show here that the patterns displayed in Figure IV are unlikely to be driven by such a sample restriction. First, we reproduce the main pattern in Figure IVb without restricting the sample to UI takers (a). This allows us to compare survival rates out of formal employment for workers displaced in the first five months of the year and in the next seven months of the year. They are almost identical. Second, we reproduce Figure IVb for a similarly selected sample comparing workers displaced in the first five months of the year (b) and in the next seven months of the year (c) after 2007, when our UI data no longer suffer from the same limitation. The two panels are almost identical.

Figure B.6: Survival out of formal employment by type

(a) 1 job, tenure$\geq$ 24 (39.3%) (b) 1 job, $12 \leq$ tenure$<$ 22 (12.6%) (c) 1 job, $6 \leq$ tenure$<$ 10 (10.4%)

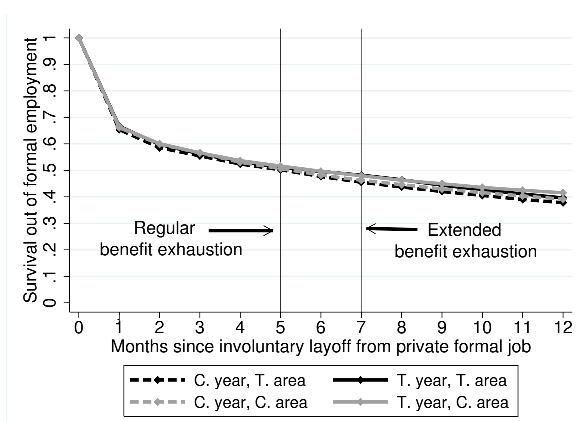


(d) 2+ jobs, acc. tenure$\geq$ 24 (22%) (e) 2+ jobs, $12 \leq$ acc. tenure$<$ 22 (13.9%) (f) 2+ jobs, $6 \leq$ acc. tenure$<$ 10 (1.64%)



Sample shares in parentheses. Panels (a)–(f) display survival rates out of formal employment for random samples of UI takers, 18–54 years old, laid off between 2002 and 2009. They show that the main pattern in Figure IVb, obtained for workers with more than 24 months of job tenure at layoff, holds when we consider other worker categories. In particular, we consider (i) workers who had one vs. several formal jobs in the previous 36 months and (ii) workers eligible for three, four, or five months of UI (based on their job tenure or accumulated job tenure at layoff). In all cases, survival rates stay high when workers are eligible for UI; they start decreasing faster after benefit exhaustion (spike); but they remain relatively high even after benefit exhaustion (low formal reemployment rates). In all samples, we exclude observations with (accumulated) tenure below six months, between 10 and 12 months and between 22 and 24 months, for which we measure maximum benefit duration imperfectly (see text).

Figure B.7: Survival out of formal employment for the 1996 temporary UI extension (UI non–takers)



The figure displays survival rates out of formal employment in each month after layoff for UI non–takers 18–54 years old, who had more than 24 months of job tenure at layoff, in control and treatment cities and in control and treatment years. In our sample, UI takeup occured several months before the UI extension was first discussed (see Figure Vb). Survival rates out of formal employment for UI non–takers present no differential trend, supporting our identifying assumption of a common trend absent the UI extension.

Table B.1: Individual heterogeneity in the costs of unemployment insurance

Covariates	UI takeup rate (1)	Average benefit duration (months) (2)	Mechanical cost (months) (3)	Max. behavioral cost (months) (4)
Male	-.0654***	-.0324*	.0303	.0021
	(.0219)	(.0175)	(.0287)	(.0427)
Age	-.0006***	0	.0058***	-.0058***
	(.0002)	(.0001)	(.0005)	(.0006)
Years of education	-.009***	-.003***	-.0024***	.0053***
	(.0008)	(.0003)	(.0009)	(.0011)
Tenure	-.0002***	.0006***	.0009***	-.0015***
	(.0001)	(0)	(.0001)	(.0001)
2+ formal jobs	-.0699***	-.1364***	-.2566***	.393***
	(.0032)	(.0131)	(.015)	(.0278)
Log. real wage	-.0448***	-.0192***	.0152	.004
	(.0024)	(.0031)	(.0094)	(.012)
Replacement rate	-.0136**	.0103**	.131***	-.1412***
	(.0062)	(.0046)	(.0147)	(.0156)
Services	.0318***	.0424***	.0738***	-.1162***
	(.0044)	(.0045)	(.0078)	(.0118)
Trade	.1051***	.0604***	.0705***	-.1309***
	(.0057)	(.0041)	(.0081)	(.0114)
Industry	.0924***	.0539***	.0588***	-.1127***
	(.0068)	(.0033)	(.013)	(.0155)
Firm size $<$ 10 employees	-.0205***	.0067***	.0565***	-.0632***
	(.0017)	(.0019)	(.0055)	(.0068)
Firm size $\geq$ 100 employees	-.0173***	-.0342***	-.0262***	.0604***
	(.0035)	(.0036)	(.0053)	(.0079)
Eligible for 4 months of UI	.0207***	.9743***	-.1075***	.1332***
	(.0019)	(.0022)	(.0054)	(.0073)
Eligible for 5 months of UI	-.0024	1.877***	-.2986***	.4215***
	(.0024)	(.0067)	(.0066)	(.0124)
	N=1,461,760		N=1,092,590	

s.e. clustered by state (27) in parentheses (significance levels: * 10%, ** 5%, ***1%). Sample and outcomes as defined in Table I. The table displays estimated coefficients for individual controls from a simplified version (in terms of the covariates included) of the specification used in the bottom panel of Table I. In particular, the specification here uses year, state, calendar separation month, and gender fixed–effects as in Table I, but it uses a continuous measure of education, three categories of firm size, four categories of sector of activity, and linear controls in age, tenure, log real wage before layoff, and UI replacement rate. It also includes fixed effects for whether a worker had a single vs. several formal job(s) in the previous 36 months and for workers' maximum benefit duration. Males are less likely to take up UI. However, they are not faster at finding a new formal job afterward, in contrast to construction workers (omitted sector) and those who had several formal jobs in the recent past. Conditional on takeup, workers with higher replacement rates and those displaced from smaller firms are slower at finding a new formal job. Finally, the maximum efficiency cost of longer UI benefits increases with workers' current maximum benefit duration because the mechanical cost decreases and the maximum behavioral cost increases.

Table B.2: Individual heterogeneity in the results of the 1996 temporary UI extension

	Outcomes			
	UI takeup rate [mean]	UI takeup rate [DD]	Mechanical cost (months)	Behavioral cost (months)
Covariates	(1)	(2)	(3)	(4)
Omitted group	.7521***	.0299	1.603***	.297***
	(.0339)	(.0313)	(.0366)	(.0353)
Male	-.0407*	-.0334	-.0913***	.0179
	(.0217)	(.0223)	(.0199)	(.0185)
Age	-.0043***	.0024**	.0022**	-.0021**
	(.0011)	(.0012)	(.0011)	(.001)
Years of education	-.0066***	-.0003	.0072***	-.0064***
	(.0018)	(.0017)	(.0023)	(.0024)
Tenure	-.0005**	.0002	.0012***	-.0008***
	(.0002)	(.0003)	(.0002)	(.0003)
Log. real wage	-.2281***	.0682	.2347***	-.1412***
	(.0454)	(.0481)	(.0382)	(.0416)
Replacement rate	-.6354***	.3152*	.8318***	-.4415***
	(.1648)	(.1759)	(.1306)	(.1389)
Services	-.0161	-.0073	.0535	-.063*
	(.0341)	(.0338)	(.0343)	(.0333)
Trade	.0661**	-.0249	.0112	-.0103
	(.0258)	(.0266)	(.0333)	(.0307)
Industry	.0956***	-.0578*	.052	-.0198
	(.0306)	(.0314)	(.0517)	(.0529)
Firm size $<$ 10 employees	-.0181	-.0254	.0855***	-.0544***
	(.0197)	(.0235)	(.0151)	(.0179)
Firm size $\geq$ 100 employees	-.0657***	.0304	-.0043	-.0578***
	(.0247)	(.0252)	(.0182)	(.0198)
	N=233,997		N=171,790	

s.e. clustered by metropolitan area (29) in parentheses (significance levels: * 10%, ** 5%, ***1%). Sample and outcomes as defined in Table II. The table displays estimated coefficients for individual controls from a simplified version (in terms of the covariates included) of the specification used in the top panel of Table A.5. In particular, the specification here uses treatment year, treatment area, and gender fixed–effects, a continuous measure of education, three categories of firm size, four categories of sector of activity, and linear controls in age, tenure, log real wage before layoff, and UI replacement rate. The individual heterogeneity in the mechanical cost is not always consistent with patterns found in Table B.1, which were obtained for workers laid off in the whole country. Efficiency costs are slightly larger for males and smaller for older, more educated, and more tenured workers. There is a nonlinear relationship with wages (replacement rates are decreasing in wages) and firm size at layoff.

Table B.3: Individual heterogeneity in the regression discontinuity results

	Outcomes			
	UI takeup rate [mean]	UI takeup rate [DD]	Mechanical cost (months)	Behavioral cost (months)
Covariates	(1)	(2)	(3)	(4)
Omitted group	.7696***	.006	.8494***	.1091***
	(.0247)	(.0251)	(.0105)	(.0136)
Male	-.0469**	.0099	.04***	-.0189
	(.0229)	(.0238)	(.013)	(.0194)
Age	-.0001	.0001	.0028***	-.0006**
	(.0002)	(.0002)	(.0002)	(.0003)
Years of education	-.0079***	.0001	-.0027***	.0014*
	(.0006)	(.0007)	(.0005)	(.0008)
Log. real wage	-.0376***	.018***	-.011	.0269**
	(.0056)	(.0061)	(.008)	(.0106)
Replacement rate	.0078	.0312	-.017	.0936***
	(.0208)	(.0223)	(.0251)	(.0269)
Services	.0237***	.0027	.036***	-.013
	(.0061)	(.0078)	(.0062)	(.0088)
Trade	.0886***	.0025	.039***	-.0071
	(.0055)	(.0069)	(.0051)	(.0068)
Industry	.0784***	.0013	.0358***	-.0079
	(.0058)	(.0075)	(.0059)	(.0092)
Firm size $<$ 10 employees	-.0242***	.0018	.0286***	-.0073**
	(.0074)	(.0079)	(.0029)	(.0034)
Firm size $\geq$ 100 employees	.0018	-.0148	-.0088**	-.0206***
	(.0076)	(.0094)	(.0041)	(.0075)
	N=1,996,050		N=1,670,505	

s.e. clustered by week of tenure in parentheses (significance levels: * 10%, ** 5%, ***1%). Sample and outcomes as defined in Table IV. The table displays estimated coefficients for individual controls from the specification used in the second panel of Table A.9. The individual heterogeneity in the mechanical cost is consistent with patterns found in Table B.1. Efficiency costs are slightly smaller for males and older workers and slightly larger for more educated workers. There is a nonlinear relationship with wages (replacement rates are decreasing in wages) and firm size at layoff.

Table B.4: Robustness to selection of workers laid off in the first five months of the year (overall regression discontinuity results)

	UI takeup rate (1)	UI duration up to 4 months (months) (2)	Extended UI duration (months) (3)	Extended UI duration vs. counterfactual (months) (4)
	Laid off in the first five months of the year			
Job tenure $\geq$ 24 months	.013	.0091***	.9021***	.1039***
	(.0099)	(.0023)	(.0038)	(.0054)
Mean ($20 \leq Tenure < 22$)	.83	3.93	3.97	4.71
	N=640,943		N=536,197	
	Laid off in the next seven months of the year			
Job tenure $\geq$ 24 months	.0155	.0128***	.9003***	.115***
	(.0109)	(.0034)	(.0052)	(.0064)
Mean ($20 \leq Tenure < 22$)	.8	3.93	3.98	4.72
	N=836,020		N=674,959	

s.e. clustered by week of tenure in parentheses (significance levels: * 10%, ** 5%, ***1%). Samples, outcomes, and specifications as defined in Table III. Most results in the paper are based on samples of workers displaced in the first five months of every year because of the limitation in our UI data discussed in Section 1.3. We show here that the results in Table III are unlikely to be driven by such a sample restriction. In particular, we compare our RD estimates for workers displaced in the first five months of the year (top panel) and in the next seven months of the year (bottom panel) after 2007, when our UI data no longer suffer from the same limitation. The results are very similar in both panels.

Table B.5: Earnings and disposable income levels by reemployment status (re–weighted prior to layoff)

	Average net earnings vs. prior to layoff (R$495.1)		Average net disposable income vs. prior to layoff (R$316.6)	
	(1)	(2)	(3)	(4)
Formally reemployed before UI exhaustion	-.1586***	-.1763***	-.0815***	-.0848***
(upon reemployment, Δ%)	(.0297)	(.0259)	(.0311)	(.0285)
Formally reemployed around UI exhaustion	-.282***	-.2965***	-.1541***	-.1549***
(upon reemployment, Δ%)	(.0365)	(.0319)	(.0371)	(.0342)
Formally reemployed after UI exhaustion	-.2916***	-.3082***	-.1261***	-.1177***
(upon reemployment, Δ%)	(.0422)	(.0399)	(.0464)	(.0437)
Informally reemployed before UI exhaustion	-.4675***	-.4442***	-.3417***	-.287***
(upon reemployment, Δ%)	(.0234)	(.0223)	(.026)	(.0237)
Informally reemployed around UI exhaustion	-.4755***	-.4425***	-.3225***	-.2624***
(upon reemployment, Δ%)	(.0236)	(.0226)	(.0267)	(.0248)
Informally reemployed after UI exhaustion	-.4932***	-.483***	-.3083***	-.2635***
(upon reemployment, Δ%)	(.0286)	(.0285)	(.0382)	(.0361)
Unemployed before UI exhaustion [a]			-.5431***	-.5499***
(Δ%)			(.0127)	(.0117)
Unemployed around UI exhaustion [b]			-.5163***	-.526***
(Δ%)			(.0154)	(.0139)
Unemployed after UI exhaustion [c]			-.5216***	-.5405***
(Δ%)			(.0165)	(.0155)
	N=6,470		N=27,578	
Including controls	No	Yes	No	Yes

s.e. clustered by individual and obtained by the delta method in parentheses (significance levels: * 10%, ** 5%, ***1%). Sample, outcomes, and specifications as in Table V. We reweight the sample such that it compares better to the sample of workers who are observed formally employed in the month prior to layoff (DiNardo, Fortin and Lemieux, 1996). We use year, calendar month, metropolitan area, gender, education (9), sector of activity (21), and race (5) fixed effects, as well as 2nd order polynomials in age and tenure. The results are similar after reweighting. Exchange rate: R$1.9≃US$1 (in R$ of 2000). [a] 34% have no disposable income. [b] 31% have no disposable income. [c] 30% have no disposable income.

Table B.6: Earnings and disposable income levels by reemployment status (re–weighted RAIS)

	Average net earnings vs. prior to layoff (R$502.4)		Average net disposable income vs. prior to layoff (R$314.9)	
	(1)	(2)	(3)	(4)
Formally reemployed before UI exhaustion	-.1272***	-.1498***	-.0499	-.0642
(upon reemployment, Δ%)	(.0425)	(.0376)	(.0474)	(.0438)
Formally reemployed around UI exhaustion	-.2529***	-.2661***	-.1349***	-.1482***
(upon reemployment, Δ%)	(.0421)	(.0381)	(.0444)	(.0411)
Formally reemployed after UI exhaustion	-.2628***	-.2862***	-.0782	-.0915
(upon reemployment, Δ%)	(.0551)	(.0531)	(.0724)	(.07)
Informally reemployed before UI exhaustion	-.4224***	-.4171***	-.2891***	-.2522***
(upon reemployment, Δ%)	(.0454)	(.0443)	(.0388)	(.037)
Informally reemployed around UI exhaustion	-.4494***	-.4025***	-.3106***	-.263***
(upon reemployment, Δ%)	(.0369)	(.0351)	(.0375)	(.0342)
Informally reemployed after UI exhaustion	-.5105***	-.4786***	-.3123***	-.2723***
(upon reemployment, Δ%)	(.0392)	(.038)	(.0422)	(.0396)
Unemployed before UI exhaustion [a]			-.5156***	-.534***
(Δ%)			(.0182)	(.0162)
Unemployed around UI exhaustion [b]			-.4937***	-.5152***
(Δ%)			(.0206)	(.0193)
Unemployed after UI exhaustion [c]			-.4977***	-.5304***
(Δ%)			(.0222)	(.0206)
	N=6,470		N=27,578	
Including controls	No	Yes	No	Yes

s.e. clustered by individual and obtained by the delta method in parentheses (significance levels: * 10%, ** 5%, ***1%). Sample, outcomes, and specifications as in Table V. We reweight the PME sample such that it compares better to the administrative sample on observables (DiNardo, Fortin and Lemieux, 1996). We use year, calendar month, metropolitan area, gender, education (9), sector of activity (21), and race (5) fixed effects, as well as 2nd order polynomials in age and tenure. Note that the sampling is different in RAIS (observations selected at layoff) and PME (observations selected at month x since layoff), so our application of DiNardo, Fortin and Lemieux (1996) is imperfect. Yet, it is reassuring that the results are similar after reweighting. Exchange rate: R$1.9≃US$1 (in R$ of 2000). [a] 34% have no disposable income. [b] 30% have no disposable income. [c] 29% have no disposable income.

Table B.7: Disposable income levels and selection into reemployment

	Average net disposable income (R$)			
	(levels)	(difference)	(levels)	(difference)
	(1)	(2)	(3)	(4)
Formally reemployed before UI exhaustion	159.3***	172.1***	158.9***	171.4***
(upon reemployment vs. unemployed)	(12.82)	(11.78)	(11.73)	(11.58)
Informally reemployed before UI exhaustion	63.25***	94.95***	85.41***	96.61***
(upon reemployment vs. unemployed)	(10.12)	(8.211)	(8.904)	(8.338)
Formally reemployed around UI exhaustion	119***	148.2***	125.3***	149***
(upon reemployment vs. unemployed)	(14.04)	(10)	(12.85)	(9.895)
Informally reemployed around UI exhaustion	56.57***	88.73***	82.77***	91.46***
(upon reemployment vs. unemployed)	(11.48)	(17.86)	(10.03)	(17.51)
Formally reemployed after UI exhaustion	142.9***	149***	146.9***	150.3***
(upon reemployment vs. unemployed)	(19.18)	(15.34)	(18.53)	(15.16)
Informally reemployed after UI exhaustion	67.83***	99.57***	89.73***	103***
(upon reemployment vs. unemployed)	(14.28)	(10.9)	(12.99)	(10.89)
	N=26,031			
Including controls	No	No	Yes	Yes

s.e. clustered by individual and obtained by the delta method in parentheses (significance levels: * 10%, ** 5%, ***1%). Sample, specifications, and outcomes as in Table V. The table investigates the role of selection and the ability of our individual controls to mitigate this issue in Table V. We use the fact that we observe disposable income for unemployed and reemployed workers in the previous month while they are all unemployed. We adopt a specification similar to (14) and we compare estimates that use the level vs. the change in disposable income in the left–hand side. The regression sample is slightly smaller because we must exclude workers who are observed prior to layoff. Controls include year, metropolitan area, calendar separation month, gender, education (9), sector of activity in the lost job (21), and race (5) fixed effects, as well as second–order polynomials in age and tenure. Importantly, our results are consistent with a simple model of liquidity constraints often used to motivate the need for UI (Chetty, 2008). Indeed, we find that workers who are reemployed informally in any period, and workers who are reemployed formally around UI exhaustion, had systematically lower levels of disposable income in the previous month than workers who remained unemployed. For instance, the specification in levels underestimates individual gains from finding an informal job by about 12.5% (R$30). Our individual controls capture about two thirds of that difference. Exchange rate: R$1.9≃ US$1 (in R$ of 2000).

Table B.8: Disposable income levels by reemployment status (North–East vs. other areas)

	Average net disposable income vs. prior to layoff			
	North–East (R$239.2)		Other areas (R$355.7)	
	(1)	(2)	(3)	(4)
Formally reemployed before UI exhaustion	.044	.007	-.0623	-.0794*
(upon reemployment, Δ%)	(.0732)	(.0668)	(.0462)	(.041)
Formally reemployed around UI exhaustion	.084	.0622	-.1661***	-.1646***
(upon reemployment, Δ%)	(.1225)	(.1141)	(.0471)	(.0421)
Formally reemployed after UI exhaustion	.0277	.0022	-.1054*	-.1149*
(upon reemployment, Δ%)	(.1138)	(.1063)	(.0642)	(.0609)
Informally reemployed before UI exhaustion	-.3608***	-.2948***	-.3333***	-.2877***
(upon reemployment, Δ%)	(.0525)	(.0495)	(.0341)	(.0311)
Informally reemployed around UI exhaustion	-.2687***	-.2146**	-.3287***	-.2766***
(upon reemployment, Δ%)	(.0975)	(.0887)	(.0373)	(.033)
Informally reemployed after UI exhaustion	-.2045	-.1689	-.3209***	-.2792***
(upon reemployment, Δ%)	(.1587)	(.1457)	(.0448)	(.0401)
Unemployed before UI exhaustion [a]	-.5415***	-.5713***	-.5171***	-.5344***
(Δ%)	(.0309)	(.0278)	(.0202)	(.0174)
Unemployed around UI exhaustion [b]	-.5368***	-.5642***	-.4853***	-.5111***
(Δ%)	(.0316)	(.0292)	(.0244)	(.0217)
Unemployed after UI exhaustion [c]	-.5416***	-.5641***	-.5061***	-.5327***
(Δ%)	(.0363)	(.0327)	(.023)	(.0216)
	N=6,340		N=21,238	
Including controls	No	Yes	No	Yes

s.e. clustered by individual and obtained by the delta method in parentheses (significance levels: * 10%, ** 5%, ***1%). Sample, outcomes, and specifications as in Table V. PME only covers six metropolitan areas and thus does not allow us to study how our results in Table V may vary with labor market composition. Nevertheless, we show here that our estimated disposable income gaps are relatively similar in the two metropolitan areas in the North–East of Brazil (Recife and Salvador), which are poorer and have lower shares of formal employees, and in the four other areas, which are richer and have higher shares of formal employees (Belo Horizonte, Porto Alegre, Rio de Janeiro, and São Paulo). Controls include year, metropolitan area, calendar separation month, gender, education (9), sector of activity in the lost job (21), and race (5) fixed effects, as well as second–order polynomials in age and tenure. Disposable income gaps are slightly smaller for workers informally reemployed before and around UI exhaustion and slightly larger for unemployed workers in the North–East. We also estimate that the share of unemployed vs. informally employed UI exhaustees is relatively similar in the two groups of cities (slightly more unemployed in the North–East, not shown). All together, we find no clear evidence pointing towards a larger or smaller social value of insurance in the two groups of cities. Exchange rate: R$1.9≃US$1 (in R$ of 2000). [a] 39% and 32% have no disposable income in the North–East and other regions, respectively. [b] 36% and 29% have no disposable income in the North–East and other regions, respectively. [c] 34% and 28% have no disposable income in the North–East and other regions, respectively.