Faculdade de Economia,
Administração e Contabilidade
de Ribeirão Preto
Universidade de São Paulo

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

Prof. Dr. Gustavo Mauricio Gonzaga

Av. Bandeirantes, 3900 - Monte Alegre - CEP: 14040-905 - Ribeirão Preto - SP
Fone (16) 3602-4331/Fax (16) 3602-3884 - e-mail: cebelima@usp.br site: www.fearp.usp.br

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Faculdade de Economia, Administração e Contabilidade de Ribeirão Preto
Avenida dos Bandeirantes,3900
14040-905 Ribeirão Preto – SP

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

François Gerard (UC Berkeley) Gustavo Gonzaga (PUC–Rio)

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**Abstract**

It is widely believed that the presence of a large informal sector increases the efficiency costs of social programs in developing countries. We develop a simple theoretical model of optimal unemployment insurance (UI) that specifies the efficiency–insurance tradeoff in the presence of informal job opportunities. We then combine the model with evidence drawn from 15 years of uniquely comprehensive administrative data to quantify the social costs of the UI program in Brazil. We first show that exogenous extensions of UI benefits 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 of the extra benefits were actually received by claimants who did not change their employment behavior. Consequently, only a fraction of the cost of UI extensions was due to perverse incentive effects and the efficiency costs were thus relatively small — only 20% as large as in the US, for example. Using variation in the relative size of the formal sector across different regions and over time in Brazil, we then show that the efficiency costs of UI extensions are actually *larger* in regions with a larger formal sector. Finally, we show that UI exhaustees have relatively low levels of disposable income, suggesting that the insurance value of longer benefits in Brazil may be sizeable. In sum, the results overturn the conventional wisdom, and imply that efficiency considerations in fact become *more* relevant as the formal sector expands.

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# 1 Introduction

The enforcement of tax compliance and social program eligibility is a major challenge in developing countries, where the informal sector accounts for 40% of GDP and 55% of the labor force (average in both Brazil and Latin America; Schneider et al., 2010; Perry et al., 2007). 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 (moral hazard).[1]

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 leave (or not join) the formal sector lack a theoretical framework to interpret this evidence in terms of 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 develop a simple theoretical model of optimal unemployment insurance in the presence of informal job opportunities 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 credible empirical strategies. 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 (re)employed. It has recently been adopted or considered in a

[1] *"Because checking benefit eligibility imposes large informational and institutional demands, particularly under abundant and diverse employment opportunities in the unobservable informal sector, the resulting weak monitoring would make the incentive problem of the standard UI system much worse"* (Robalino et al., 2009). The authors of this policy paper are the current and the former Labor Team leaders at the Social Protection anchor of the World Bank. The same concern applies to many different types of social programs. For example, welfare programs do not typically deny benefits to the formally employed but they condition transfers on income *as observed by the government*. Because informal wages are easier to hide, such programs create similar incentives.

[2] For instance, several papers investigate the impact of the Mexican *Seguro Popular* program, which extended health care coverage to the informally employed, on the size of the formal sector. They find mixed results (Azuara and Marinescu, 2011; Campos–Vasquez and Knox, 2008; Bosch and Campos–Vasquez, 2010; Aterido et al., 2011).

number of developing countries.[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] Brazil also constitutes a uniquely well–suited empirical setting because it offers wide variation in formal employment rates across space and time.[5] This allows us to estimate how the efficiency costs of UI extensions change with the relative size of the formal labor market.

We begin by adapting the canonical Baily model of optimal UI in two ways (Baily, 1978; Chetty, 2006). We introduce informal work opportunities and we consider extensions of the maximum benefit duration instead of changes in benefit levels (Schmieder et al., 2012). We show that the efficiency costs of UI extensions are captured by a pseudo–elasticity ($\widetilde{\eta}$), the ratio of a *behavioral* effect to a *mechanical* effect.[6] The former measures the cost of UI extensions due to behavioral responses. Beneficiaries may delay formal reemployment to draw additional benefits. The latter measures the cost absent any behavioral response. Beneficiaries who would not be formally reemployed after UI exhaustion in absence of the extension draw additional benefits without changing their behavior. The ratio measures the fraction of social spending lost through behavioral responses. A UI extension increases welfare if the social value of the income transfer to UI exhaustees exceeds $\widetilde{\eta}$.

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 beneficiary returns to a formal job after regular UI benefits are exhausted. This allows us to estimate the mechanical effect of UI extensions. We estimate the behavioral effect using two empirical strategies: a politically–motivated UI extension (difference–in–difference) and a tenure–based eligibility cutoff (regression

[3]Currently some form of UI exists in Algeria, Argentina, Barbados, Brazil, Chile, China, Ecuador, Egypt, Iran, Turkey, Uruguay, Venezuela and Vietnam (Vodopivec, 2009; Velásquez, 2010). Mexico, the Philippines, Sri Lanka, and Thailand have been considering its introduction.

[4]See Acevedo et al. (2006), Robalino et al. (2009), and Vodopivec (2009). 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. The new Jordanian program, for instance, designed in consultation with the World Bank, is a forced savings scheme to which workers contribute when formally employed. "UI benefits" drawn by a worker in excess of what she contributed over her lifetime must be paid back at retirement.

[5]The variation in formal employment rates across Brazilian states over our 15 years of data covers the existing variation across Latin American countries today. Private–sector formal employment rates are strongly correlated with income per capita. The variation in income per capita across Brazilian states is very large, ranging from the levels in China in the poorest state to Poland in the richest (http://www.economist.com/content/compare-cabana).

[6]This is a common result in public finance. The mechanical effect on government revenues of increasing the income tax, for instance, corresponds to the tax base ex-ante. The behavioral effect corresponds to the change in the tax base due to the tax increase. Their ratio, equal to the marginal deadweight burden of the tax increase, captures efficiency costs (Saez et al., 2012). Our measure of efficiency and our welfare formula apply to a broad class of models as long as an envelope condition applies to the agents' problem (Chetty 2006).

discontinuity). Finally, we use longitudinal survey data to estimate overall (formal and informal) reemployment rates and to provide evidence for the social value of the extended benefits.

This paper has four main findings. First, beneficiaries respond to UI incentives. Formal reemployment rates spike at UI exhaustion and this spike shifts completely following exogenous UI extensions. Because we find no such spike in overall reemployment rates, the response comes from beneficiaries (re)employed in the informal sector. Second, formal reemployment rates are on average very low even after UI exhaustion. Most beneficiaries draw extra benefits without changing their behavior. Extending UI by 2 months, from 5 to 7 months, mechanically increases average benefit duration by 1.7 months in Brazil. As a result, the behavioral effect is small compared to the mechanical effect. Our largest estimate of $\widetilde{\eta}$ is around .2, less than one fifth of estimates for the US (Katz and Meyer, 1990). Third, we find a positive relationship between formal employment rates and how rapidly beneficiaries return to a formal job after UI exhaustion (the spike). This result holds in the cross–section, using variation across regions over time, and controlling for a rich set of worker characteristics. It implies that the mechanical effect of UI extensions decreases with formal employment. In contrast, the behavioral effect may increase when more beneficiaries are formally reemployed rapidly after UI exhaustion. We find that the behavioral effect does increase with formal employment rates, both in the cross–section (difference–in–difference) and using the variation across regions over time (regression discontinuity). Thus, contrary to the prevailing belief, the efficiency costs of UI extensions are relatively small in a context of high informality and in fact rise with the size of the formal labor market. Last, we find that UI exhaustees have low levels of disposable income compared to similar workers prior to layoff and that a significant share of them remain unemployed. Incorporating these findings in our framework, we find that welfare effects from extending UI are likely positive and may be sizeable.

This paper extends a large theoretical and empirical literature on social insurance in developed countries.[7] The closest paper to ours is perhaps Schmieder et al. (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

[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.

periodically, but formal employment is persistently low in developing countries and is expected to rise with economic development. We also derive a new formula for the welfare effects of UI extensions, which takes into account the nature of labor markets in developing countries.

Further, we contribute to a growing literature at the intersection of public finance and development.[8] First, a theoretical literature argues that efficiency considerations force governments to resort to alternative, second–best, policies where enforcement is weak and informality is high (e.g. Gordon and Li, 2009). However, there is little empirical evidence on the impact of typical policies in such countries. We find that the efficiency costs of a common social program are low in Brazil even though informality is prevalent.[9] Second, several studies investigate how to use available information to best target social benefits (e.g. Alatas et al., 2012). We extend our analysis to investigate whether the government could use available information to target monitoring toward workers with larger behavioral responses. We show that our pseudo–elasticity $\widetilde{\eta}$ also captures incentives to introduce job–search monitoring of UI beneficiaries. Small efficiency costs may thus rationalize the lack of monitoring in Brazil until 2011. We find that the government could use information at hand to achieve some improvements in targeting. Nevertheless, most of the heterogeneity in behavioral responses is not easily captured by observable characteristics. As a result, incentives to introduce monitoring are low for all worker categories.

The two main complementary views on labor informality in developing countries shed light on why our findings might prevail (Perry et al., 2007). In the traditional "exclusion" view, formal jobs are associated with high search costs (Harris and Todaro, 1970; Fields, 1975; Zenou, 2008). The mechanical effect is large and the behavioral effect small because workers are unable to find a formal job rapidly. A decrease in formal search costs would then reduce the mechanical effect but increase the behavioral effect if beneficiaries still have the option to work informally. This rationalizes the finding that efficiency costs rise with formal employment rates. In the "exit" view, workers are voluntarily informal to avoid paying for benefits they may not value (Maloney, 1999; Levy, 2008). The mechanical effect is large and the behavioral effect small because workers are unwilling to return to a formal job rapidly with or without UI. Beneficiaries working informally do not need insurance in this case.

[8]See, for example, Niehaus and Sukhtankar (2012), Olken and Singhal (2011) or Pomeranz (2012).

[9]Similarly, Kleven and Waseem (2012) find that (intensive–margin) taxable income elasticities are low in Pakistan even though tax evasion is widespread.

Finally, our approach and findings contribute to the nascent empirical literature investigating the impact of social programs in countries with high informality.[10] Existing studies do not typically link their results to standard public finance theoretical frameworks, complicating interpretation. In contrast, we use such a framework to guide our empirical analysis; we provide new empirical evidence that allows us to estimate efficiency costs directly; and we evaluate the resulting welfare effects of the program. We are also the first paper to empirically estimate how behavioral responses to a program vary with the size of the formal sector. In so doing, our results overturn the conventional wisdom that social programs are particularly distortive in the presence of informal work opportunities. Whether to extend UI is not a question of efficiency in our setting; it depends on whether society values redistributing resources to UI exhaustees. Efficiency considerations may become more relevant as the formal sector expands.

The remainder of this paper is structured as follows. Section 2 provides some background and describes our data. Section 3 presents the conceptual framework that guides our analysis. Section 4 estimates the mechanical effect of UI extensions. Section 5 exploits two empirical strategies to estimate the behavioral effect and efficiency costs of UI extensions. Section 6 uses survey data to estimate overall reemployment rates and disposable income of UI exhaustees. We then incorporate our results in our framework and evaluate welfare effects. Section 7 concludes.

# 2 Background and Data

## 2.1 Labor markets in Latin America and Brazil

Labor markets in Latin America and elsewhere are characterized by the coexistence of formal employees and informal workers. Formal employees typically work in jobs with strict regulation of working conditions (overtime pay, firing costs) and relatively high payroll taxes. In exchange, they

[10] In addition to previously cited papers, Bérgolo and Cruces (2010), Camacho et al. (2009), and Gasparini et al. (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 non–OECD countries (IADB, in progress). We are aware of three working papers on UI in Brazil that are mostly descriptive (Cunningham, 2000; Margolis, 2008; Hijzen, 2011). A related literature investigates the impact of UI in macro-labor models with an informal sector (Zenou, 2008; Ulyssea, 2010; Robalino et al, 2011; Meghir et al., 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 workers 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.

are entitled to a series of benefits (pensions, disability, unemployment insurance) that they may or may not value. 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 the self–employed (mostly unskilled). The same firm may hire both formal and informal employees.[11] Longitudinal survey data reveal that many workers transit between formal and informal labor statuses over the course of their lives (Bosch and Maloney, 2010). Formal wages are on average higher, though there is a lot of heterogeneity; some informal workers (mostly the self–employed) are likely better off than in their alternative options in the formal sector (Botelho and Ponczek, 2011). The two main views on informality, that informal workers are excluded from formal jobs or that they voluntarily avoid formal employment, are recognized as complementary (Perry et al, 2007).

In contrast to other developing countries, formal employment is well–defined in Brazil. All workers have an individual working card that employers sign upon formal hiring. Brazilian labor laws are among the strictest in the region. Payroll taxes amount to over 35% of wages. Firing costs are also high.[12] In 2009, 42% of working adults were formal private–sector employees, 23% informal employees and 24% self–employed. Brazil is an extremely diverse country, however. Formal employment rates and GDP per capita across Brazilian states over our sample years range from the bottom to the top of the cross–country distributions in South America today. Figure 1 shows that average formal employment rates by state in two recent time periods strongly correlate with GDP per capita. In the cross–sections, formal employment rates increase by over 25 percentage points from the poorer to the richer states. In the last decade, both income per capita and formal employment rates also increased, but not uniformly. We use this variation to investigate how the efficiency costs of UI extensions change with the relative size of the formal labor market.[13]

## 2.2 The Brazilian Unemployment Insurance program

The Brazilian UI program has been in place since the mid–1980s and is quite sizeable. UI expenditures amount to 2.5% of total eligible payroll, more than three times the corresponding US figure

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

[12] We provide more information on labor legislation in the Appendix.

[13] Appendix Figure C.1 displays the same variation on maps of Brazil. We focus on formal employment rates as they capture both variation in employment and in its formality. Unemployment dropped from 13% to 7% over the last decade but unemployment is often poorly measured compared to formal employment.

(www.dol.gov). Workers involuntarily displaced from a private formal job with at least 6 months of tenure at layoff are eligible for 3 to 5 monthly UI payments. Maximum benefit duration depends on accumulated tenure over the 3 years prior to layoff. In particular, workers with more than 24 months of tenure at layoff are eligible for 5 months of UI, after a 30–day waiting period. Our main source of exogenous variation is a temporary UI extension that took place in 1996. As our data start in 1995, we cannot observe workers' formal employment history in the previous 3 years. Therefore, we restrict attention to workers with more than 24 months of tenure at layoff; for these workers, tenure at layoff is a sufficient statistic for their UI eligibility.

Benefit levels are means–tested. Replacement rates start at 100% at the bottom of the wage distribution but are down to 60% for workers who earned three times the minimum wage.[14] There was no monitoring of beneficiaries' job–search efforts before 2011. Workers applied in person for UI benefits in the first month only. Payments were then automatically made available for withdrawal at Caixa Economica, an official bank, every 30 days as long as the worker's name did not appear in a database where employers monthly report new hirings (CAGED, Labor Ministry).[15]

### 2.3 Data

In this paper, we mainly exploit two very large restricted access administrative datasets covering one and a half decades of Brazil's recent history. RAIS (Relação Anual de Informações Sociais) is a longitudinal matched employee–employer dataset covering by law the universe of formally employed workers, including public employees. All tax-registered firms have to report every worker formally employed at some point during the previous calendar year.[16] Every observation in RAIS is a worker–establishment pair in a given year. It includes information on wage, tenure, age, gender, education, sector of activity, establishment size and location, hiring and separation dates, and reason for separation. Because every worker is uniquely identified over time, we observe all spells in formal employment and between formal jobs for each individual. We currently have data from

[14] The full schedule is presented in Appendix Figure C.2. Our results hold if we exclude beneficiaries with very high and very low replacement rates.

[15] In Brazil, unemployment insurance is financed through a sales tax (.65% tax on firms' total sales).

[16] 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 has also been increasingly used by ministries administering other social programs to monitor formal job take-ups. RAIS actually has better coverage of formal employment than the data used by the UI agency (Ministerio do Trabalho e Emprego, 2008). Accordingly, we observe a few formally reemployed workers still collecting UI. As a consequence, our results slightly overestimate efficiency costs.

1995 to 2010; at the end of 2009, there were 41 million formal employees.

We are the first researchers to be granted access to the second administrative dataset, the Unemployment Insurance registry. It includes the month and amount paid for every UI payment made from 1995 to 2012. There were over 9 million different beneficiaries in 2009. Beneficiaries are identified with the same ID number as in RAIS. The UI registry has a few limitations. About 2% of ID numbers are missing for the earlier years in the data. Moreover, if the benefit collection period of a given worker spanned two different years, UI payments from the second year were not reported in the data before 2006. We thus restrict attention to workers who start collecting benefits in the first 6 months of the year to avoid truncation issues in UI spells.[17]

Finally, we exploit monthly labor force surveys (PME, Pesquisa Mensal de Emprego) to measure disposable income for UI exhaustees.[18] The longitudinal structure of the surveys also allows us to estimate hazard rates of overall (formal and informal) reemployment for displaced formal employees.

## 3 Costs and Benefits of UI extensions: a framework

This section presents the conceptual framework that guides our empirical analysis. We build on the canonical Baily model for the optimal social insurance benefit levels in the presence of moral hazard (Baily, 1978). We introduce informal work opportunities in a dynamic model of endogenous job–search (Chetty, 2008; Schmieder et al., 2012). We then show that the efficiency costs of UI extensions are captured by a pseudo–elasticity, defined as the ratio of a *behavioral* effect to a *mechanical* effect.[19] The former measures the increase in benefit duration due to behavioral responses. The latter measures the increase in benefit duration absent any behavioral response. The ratio measures the fraction of social spending lost through behavioral responses. A UI extension increases welfare if the social value of the income transfer to UI exhaustees exceeds this pseudo–elasticity. We focus on the intuition for the main results. The model and its derivations are described in the Appendix.

*Agent's Problem.* The model describes optimal behavior of a representative worker who cycles

[17]Formal reemployment patterns based on RAIS are similar for workers displaced throughout the year.

[18]Surveys collected by the Instituto Brasileiro de Geografia e Estatística — IBGE. See Section 6 for more description.

[19]As discussed in Chetty (2006), the measure of efficiency costs and the welfare formula derived in such a model are robust to relaxing many assumptions (such as introducing heterogeneity) or to introducing other margins of behaviors (endogenous savings accumulation and depletion, reservation wages, spousal labor supply, human capital decisions, job–search quality) as long as an envelope condition applies to the agents' problem. We discuss mechanisms beyond the scope of our framework (e.g. general equilibrium effects) in Section 6.

in and out of formal employment. It captures both views on informal labor markets. On the one hand, formal jobs may be associated with high search costs (Fields, 1975; Zenou, 2008). On the other hand, informal jobs may be attractive (Maloney, 1999). The worker faces a fixed layoff probability $q$ in the formal sector such that, on average, she stays employed $D^f = \frac{1}{q}$ periods. She earns formal wage $w^f$ each period. Upon layoff, she becomes unemployed and eligible for UI for a maximum benefit duration of $P$ periods. UI benefits $b_t$ are defined as $b_t \equiv r_t w^f$, with replacement rate $r_t = r$ for period $t = 1, 2, ..., P$ after layoff, and $r_t = 0$ otherwise.

While unemployed, she decides each period how much overall search effort $e$ at a cost $z(e)$ 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 does not find a job and stays unemployed. With probability $e$, she finds a job. She can increase her probability of returning to a formal job by investing formal search effort $f$ at a cost $\theta z(f)$. She thus finds a formal job with probability $ef$ and an informal job with probability $e(1 - f)$. She earns wage $w^i < w^f$ when working informally and can always search for a formal job at a cost $\theta z(f)$ in subsequent periods. The choice situation is illustrated in panel (a) in Figure 2. The traditional view of informality implies high values of $\theta$ (high formal search costs). The more recent view corresponds to low values of $\theta$ and small wage differentials.[20] We introduce enforcement in the model by assuming that informal jobs are detected by the government with probability $p$. If detected, an informal worker falls back into unemployment and loses her UI benefits. In many developing countries, detection probabilities $p$ are low. Both the unemployed and the "undetected" informally employed draw UI benefits in the first $P$ periods after layoff.

The workers' problem is to choose optimal levels of search intensity of both types in each period until formal reemployment. The solution to this dynamic problem determines the survival rate out of formal employment $S_t$ in each period $t$ after layoff, and thus the average duration between formal employment spells $D^u$ and the average benefit duration $B \equiv \sum_{t=0}^{P} S_t$.

[20]Our model describes the situation of a representative worker. Informal wages could be less than formal wages in part because workers would be willing to take lower paid informal jobs while drawing UI benefits. The literature finds that the traditional view better applies to informal employees and the more recent view to the self–employed (Bosch and Maloney, 2010). We find that most of the beneficiaries (re)employed in the informal sector are informal employees (67.5%) rather than self–employed (using labor force survey data). Our main conclusions are unaffected if workers receive heterogeneous wage offers in both sectors. Workers with high informal wages would simply never return to a formal job (as long as $p$ is low). They would draw UI benefits, but would not change their behavior in response to UI extensions, and would therefore not generate efficiency costs. Our main conclusions are also robust to assuming that formally reemployed workers can pay some convex evasion costs to hide their new formal job.

*Mechanical and behavioral effects of a UI extension.* Following Schmieder et al. (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 $b$ ($\equiv rw^f$).

Extending the maximum UI duration by one period ($dP$) increases average benefit duration through two effects. This is illustrated in panel (b) in Figure 2. First, there is a mechanical effect. In absence of the extension, some workers would not have been formally reemployed after regular UI exhaustion. These workers (unemployed or informally employed) will draw the additional benefits without changing their behavior, increasing the average benefit duration $B$ by $S_{P+1}$. Second, there is a behavioral effect, the increase in average benefit duration due to behavioral responses. Extending UI benefits reduces incentives to be formally reemployed. It reduces both overall search effort ($e \downarrow$) and formal search effort ($f \downarrow$) in period $P + 1$, but also in earlier periods as it reduces future utility losses from not finding a formal job rapidly. As a consequence, it increases average benefit duration $B$ by $\sum_{t=0}^{P+1} \frac{dS_t}{dP}$. The cost of extending UI is the sum of the behavioral and the mechanical effects times the benefit level.

*Planner's Problem.* The social planner's objective is to choose the maximum benefit duration $P$ that maximizes welfare $W$, which is a weighted sum of individual utilities, such that a balanced–budget constraint holds. In the steady state, a share $\frac{D^f}{D^f+D^u}$ is formally employed each period, a share $q\frac{D^f}{D^f+D^u}$ becomes eligible for UI, and a share $q\frac{D^f}{D^f+D^u}B$ draws UI benefits. UI taxes $\tau$ are typically levied on formal employees.[21] A balanced–budget constraint must then satisfy:

$$\frac{D^f}{D^f + D^u}\tau w^f = q\frac{D^f}{D^f + D^u}Bb$$

$$\tau = qrB \tag{1}$$

Given $q$ and $r$, equation (1) shows that changes in UI costs, and the resulting UI tax rate $\tau$, are driven by changes in average benefit duration $B$.[22]

[21] As the incidence of sales and labor taxes are likely similar, the fact that UI is financed through a sales tax in Brazil does not alter the analysis.

[22] In particular, the change in the overall duration out of formal employment $D^u$, following a UI extension, has no additional effect on the UI budget constraint. If $D^u \uparrow$, it reduces the number of individuals paying UI taxes, but also the number of future beneficiaries. The two effects on the UI budget cancel out in the steady–state. We adopt a steady–state approach (infinite horizon) because in Brazil a significant share of the formally reemployed are laid off again in the following months in Brazil. We show empirically that UI extensions not only reduce the number of

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

$$\frac{dW}{dP} = q\frac{D^f}{D^f + D^u}\ S_{P+1}\ b\ g^{U_{P+1}} - \frac{D^f}{D^f + D^u}\ w^f\ \frac{d\tau}{dP}\ g^E$$
$$\frac{dW}{dP} = q\frac{D^f}{D^f + D^u} r w^f\ S_{P+1}\ g^{U_{P+1}} - q\frac{D^f}{D^f + D^u} r w^f\ \left[S_{P+1} + \sum_{t=0}^{P+1}\frac{dS_t}{dP}\right]\ g^E \tag{2}$$

The first term in equation (2) is the welfare gain of the $S_{P+1}$ displaced formal employees who would not have been formally reemployed absent the extension and now receive an additional benefit $b$ (mechanical effect). $g^{U_{P+1}}$ denotes the average social value of \$1 for these UI exhaustees. Because of the envelope theorem, the behavioral response of UI beneficiaries does not create additional welfare gains. To satisfy the budget constraint, the UI tax $\tau$ on formal wages must increase to finance the cost of the UI extension, or the sum of the behavioral and the mechanical effects. The second term in equation (2) captures the welfare loss from the tax increase for formal employees. $g^E$ denotes the average social value of \$1 for formal employees. The social values, $g^{U_{P+1}}$ and $g^E$, depend on individuals' marginal utilities and on social planner preferences towards redistribution.

Reorganizing, we obtain:

$$\frac{1}{\frac{D^f}{D^f+D^u}}\frac{dW/dP}{g^E w^f} = qr\ S_{P+1}\left[\frac{g^{U_{P+1}} - g^E}{g^E} - \widetilde{\eta}\right] \tag{3}$$

where $\widetilde{\eta} \equiv \sum_{t=0}^{P+1}\frac{dS_t}{dP}/S_{P+1}$ is the ratio of the behavioral effect to the mechanical effect. Dividing by $\frac{D^f}{D^f+D^u}w^f g^E$, equation (3) expresses the welfare effects of a UI extension in terms of a money metric, the welfare gains from a percentage increase in the formal wage. Equation (3) shows the trade–off between insurance and efficiency. The first term in brackets, the *social value of insurance* $\frac{g^{U_{P+1}}-g^E}{g^E}$, measures the social value of transferring \$1 from the average taxpayer to the average UI exhaustee. The second term, the pseudo–elasticity $\widetilde{\eta}$, measures the resources lost for each \$1 transferred to UI exhaustees.[23] If the average social value of \$1 is 20% larger for UI exhaustees

months formally employed in the 2 years after layoff but also the share experiencing a new layoff from the formal sector (Table C.8). Chetty (2008) and Schmieder et al. (2012) assume instead that new jobs are never lost. Therefore, their model emphasizes the impact of UI extensions on the overall duration out of formal employment $D^u$ because of a reduction in UI tax revenues. 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 show that this assumption holds for the group of workers we consider.

[23] In the Baily model, the ratio of the behavioral to the mechanical effect corresponds to an elasticity.

than for taxpayers, a UI extension increases welfare as long as less than 20 cents are lost through behavioral responses for each \$1 transferred to UI exhaustees. At an optimum, these two terms must be equal.

That the ratio of a behavioral to a mechanical effect measures efficiency costs is a common result in public finance. The mechanical effect on government revenues of increasing the income tax, for instance, corresponds to the tax base ex-ante. The behavioral effect corresponds to the change in the tax base due to the tax increase. Their ratio, equal to the marginal deadweight burden of the tax increase, captures efficiency costs (Saez et al., 2012).[24] Neither the social value of insurance nor the pseudo–elasticity $\widetilde{\eta}$ are structural parameters. Evaluating equation (3) around the existing UI program, however, provides a local welfare test.

*Connecting theory to the data.* To estimate efficiency costs $\widetilde{\eta}$, we do not need to observe responses in overall ($e$) and formal search efforts ($f$) separately. The relevant combined response, formal reemployment, is recorded in administrative data. We capture the mechanical effect by estimating the exhaustion rate of regular UI benefits and how rapidly beneficiaries return to a formal job after regular UI exhaustion (Section 4). We capture the behavioral effect by estimating the change in the survival rates out of formal employment following an exogenous UI extension, up to the new maximum benefit duration (Section 5). We investigate the social value of insurance using longitudinal survey data (Section 6). These latter data also allow us to estimate overall reemployment rates and compare them to formal reemployment rates. Differences must be due to beneficiaries (re)employed in the informal sector.

*Efficiency, welfare and informality.* A 13–week UI extension has been estimated to increase regular benefit duration, up to 26 weeks, by 1 week (Card and Levine, 2000) and total benefit duration by 2.1–3 weeks (Katz and Meyer, 1990) on average in the US. Katz and Meyer (1990) estimate that 43% of the increase in benefit duration is due to a mechanical effect, or $\widetilde{\eta} > 1$.

How would the cost of UI extensions differ in labor markets with a smaller formal sector? The conventional wisdom is that UI formal work disincentives (moral hazard) will be exacerbated. Many workers will delay formal reemployment and choose to work informally while drawing benefits. This

[24] If part of the behavioral effect is due to costless reporting behaviors, it generates no efficiency cost. In this case, the measure of efficiency cost we derive is an upper bound. However, it is unlikely that misreporting entails no cost for both workers and employers. From panel (b) in Figure 2, the efficiency costs of UI extensions are likely increasing in the existing maximum benefit duration. The efficiency costs of a marginal UI extension at 5 months of UI thus provide upper–bounds for the efficiency costs of a marginal UI extension at shorter durations.

is possible because the probability of being detected working informally, $p$, is low. The behavioral effect will be large, increasing both total costs and efficiency costs. This line of thinking assumes, however, that workers would be formally reemployed rapidly absent UI (small mechanical effect) and that there is a strong link between informality levels and the size of the response at the margin.

Instead, low formal employment rates may indicate high formal search costs ($\theta \uparrow$, traditional view) or low returns from formal search ($w^f \downarrow$, more recent view). In either case, workers will not be formally reemployed rapidly absent UI. The mechanical effect will be large. The behavioral effect, in contrast, will be small. A given change in benefits, for instance, has a smaller impact on formal search effort when formal search costs are high. A decrease in formal search costs will then (i) *reduce the mechanical effect* and (ii) *increase the behavioral effect* if beneficiaries still have the option to work informally. This rationalizes our empirical findings.[25] Both cases are cost–equivalent; yet, the social value of insurance is likely small if UI exhaustees are informally employed with significant income levels. Our empirical evidence suggests the opposite: UI exhaustees have low levels of disposable income compared to formal employees prior to layoff.

### 3.1 Extension: job–search monitoring

The pseudo–elasticity $\widetilde{\eta}$ is also related to government incentives to introduce job–search monitoring. When extending UI benefits, suppose the government can pay $\alpha$ per beneficiary to monitor and enforce prior (first–best) formal reemployment levels. Without anticipation behaviors, the gain from monitoring is to reduce the behavioral effect to zero and save on benefit payments ($b * Behavioral$). The cost is that the government must pay $\alpha$ for every worker who would not have been formally reemployed anyway ($\alpha * Mechanical$). $\widetilde{\eta}$ thus provides a bound on the maximum price a government should be willing to pay for this perfect monitoring technology:

$$\alpha \leq \widetilde{\eta} b$$

If $\widetilde{\eta}$ is low, it is optimal for the government not to monitor job search. In practice, monitoring is imperfect, so the behavioral effect is never reduced to zero. But governments may be able to target

[25]The comparative statics are discussed in the Appendix. The ability to work informally may also decrease ($p \uparrow$) when formal employment rates rise. In this case, both the mechanical effect (more difficult to work outside the formal sector) and the behavioral effects (more costly to delay formal reemployment) may decrease with ambiguous effects on economic efficiency. The relationship between efficiency and formal employment rates is thus an empirical question.

monitoring towards workers with larger $\widetilde{\eta}$. In the empirical analysis, we investigate to what extent we can predict which workers have larger $\widetilde{\eta}$, using information readily available to the government.

# 4 Estimating the mechanical effect of UI extensions

In the previous section, we established that the efficiency costs of UI extensions depend on a pseudo–elasticity defined as the ratio of a behavioral effect (the cost of UI extensions due to behavioral responses) to a mechanical effect (the cost absent any behavioral response). The first step of our empirical analysis estimates this mechanical effect for beneficiaries eligible for 5 months of UI in Brazil. By observing their formal reemployment rates after UI exhaustion, we measure how many beneficiaries would draw additional UI payments following a hypothetical 2–month UI extension, absent any behavioral response. We also estimate how the mechanical effect varies with the relative size of the formal labor market using variation in formal employment rates across regions and time. We find that (i) the mechanical effect is large and (ii) it decreases with formal employment rates.

We proceed as follows. First, we draw a random sample of workers eligible for 5 months of UI in every year between 1995 and 2009. Our sample includes full–time private–sector formal employees 18–54 years old with more than 24 months of tenure at layoff. Because of data limitations detailed in Section 2, we use only workers laid off between January and June. We oversample less formal labor markets to have enough observations at low levels of formal employment.

Second, we use workers' formal reemployment patterns to measure how many additional UI payments they would mechanically draw following a hypothetical 2–month UI extension. We assume that workers who exhaust their regular UI benefits and are not formally reemployed within 1 month (resp. 2 months) of regular UI exhaustion would draw 1 extra payment (resp. 2 extra payments).[26] The mechanical effect for a given beneficiary is the difference between her hypothetical extended benefit duration and her regular (no extension) benefit duration. We use individual data in order to control for composition effects across labor markets.[27]

[26] Define $month_{regUI}$, the month a beneficiary exhausts her regular benefits. Define $month_{back}$, the month a beneficiary returns to a formal job. Formally, this variable is defined as:

$$\mathbb{1}\,(\text{exhaust regular UI benefits}) \times \sum_{j=1}^{2} \mathbb{1}\,(month_{back} > month_{regUI} + j)$$

In Table C.7, we use actual UI extensions and test (successfully) whether we accurately predict the increase in average benefit duration using workers' formal reemployment patterns after regular UI exhaustion in this way.

[27] For example, women's share of the formal labor force is positively correlated with formal employment rates.

Third, we construct yearly formal employment rates for 137 mesoregions (*mesorregiões*), the second largest geographical subdivision in the country (after the 27 states), defined as groups of spatially articulated municipalities with similar socio-economic characteristics. Because mesoregions are not identified in yearly surveys, we use RAIS data to construct formal employment rates. We divide the average number of formal employees by official population estimates (IBGE) in each year in each mesoregion.[28]

Finally, for individual $i$ in mesoregion $m$ in year $t$, we regress:

$$y_{i,m,t} = \alpha_m + \beta_t + \gamma \; FormalEmploymentRate_{m,t} + X_{i,m,t} + \epsilon_{i,m,t} \tag{4}$$

Our main outcome of interest is the mechanical effect of a hypothetical 2–month UI extension. We also consider other outcomes to better describe benefit collection and reemployment patterns in Brazil: UI take–up, regular benefit duration, and the probability of staying out of formal employment more than 7 months after layoff. We present results from specifications with and without year fixed effects ($\beta_t$), mesoregion fixed effects ($\alpha_m$) and a rich set of individual controls ($X_{i,m,t}$). Standard errors are clustered by mesoregion.

## 4.1 Graphical results

Figure 3 illustrates our main results. It displays formal reemployment patterns for workers eligible for 5 months of UI after losing a formal job in 2009 in Pernambuco, a poor state with low formal employment rates, or Rio Grande do Sul, a richer state with higher formal employment rates. Hazard rates of formal reemployment are below 4% a month in both states while workers draw UI benefits. They spike to 12%–18% a month after UI exhaustion, increasing relatively more in Rio Grande do Sul. Formal reemployment rates stay quite low, however, even after UI exhaustion. About 40% of workers are still out of formal employment 12 months after layoff. The spike in formal reemployment at UI exhaustion suggests a clear behavioral response to the incentives created by the UI program.[29] We show in Section 5 that the spike is completely shifted following exogenous UI extensions. Nevertheless, the size of the behavioral effect is small compared to the mechanical

[28] We also use state–level formal employment rates (27 states) obtained from yearly household survey data (PNAD, Pesquisa Nacional por Amostra de Domicilios collected by IBGE). Results are unchanged.

[29] Such a spike is not observed in most developed countries (Card et al., 2007b). van Ours and Vodopivec (2006) find a sizeable spike in Slovenia.

effect. If UI had been extended by 2 months in Figure 3, most beneficiaries (70%–80%) would have mechanically collected additional UI payments, absent any behavioral response. Efficiency costs are thus small. Because the spike is larger, the mechanical effect is smaller and the behavioral effect larger in Rio Grande do Sul. This suggests that efficiency costs rise with formal employment rates.

More systematic results are shown on Figure 4. Each observation is a state average in a given year from 2002 to 2009. The left panel displays the relationship between regular benefit duration for workers eligible for 5 months of UI and state–level formal employment rates. Average benefit duration decreases slightly with formal employment rates but remains very high at any level. Beneficiaries draw on average 4.85 to 4.95 months of UI. In comparison, beneficiaries eligible for 26 weeks of UI in the US drew on average 16 weekly UI payments over the same period (www.dol.gov). Average benefit duration is much higher in Brazil. High exhaustion rates have also been documented in Argentina (IADB, in progress) and China (Vodopivec and Tong, 2008).

The right panel displays the relationship between the mechanical effect of a hypothetical 2–month UI extension for the same workers and state–level formal employment rates. Formal reemployment rates increase after UI exhaustion but remain low. As a consequence, extending UI by 2 months would be costly in Brazil absent any behavioral response. The mechanical effect varies from 1.75 months in states with low formal employment rates to 1.4 months in states with high formal employment rates. The relationship is negative because the magnitude of the spike in formal reemployment after UI exhaustion increases with the relative size of the formal labor market.[30]

## 4.2 Regression results

We turn to a regression analysis to further investigate the relationship between the mechanical effect of UI extensions and formal employment rates. This allows us to control for general time trends, fixed differences across labor markets, and composition effects.

Table 1 reproduces the estimated coefficients on formal employment rates by mesoregion ($\gamma$) for different outcomes and different specifications of equation (4). The mechanical effect of a hypothetical 2–month UI extension (row 3) is high on average, at 1.67 months. The mechanical effect is large because most beneficiaries exhaust their 5 months of UI (regular benefit duration is

[30]If this equilibrium relationship is intuitive, it is nevertheless not trivial. Higher formal employment rates in a given labor market could also be due to lower separation rates in the formal sector, higher separation rates in the informal sector, or higher formal reemployment rates on average but not specifically in the first months after layoff.

4.93 on average, row 2) and because 73% of beneficiaries are still out of the formal sector 7 months after layoff (row 4). The mechanical effect decreases with formal employment rates. Estimates are larger in absolute value when using the full variation in formal employment rates (column 1), but they are similar when we include year fixed effects or both year and mesoregion fixed effects (columns 2 and 3). The relationship is not due to fixed differences across regions; it holds for marginal changes in formal employment rates. Moreover, the relationship is not simply due to composition effects. Controlling for a rich set of covariates, including wage and sector of activity, has no effect on our results (column 4). This latter estimate implies that increasing formal employment rates by 30 percentage points increases the mechanical effect of a hypothetical 2–month UI extension by .2 month or 12% (and regular benefit duration by only 1%).

A concern is that UI take–up is also correlated with formal employment rates (row 1), potentially creating selection issues when we consider only UI takers as above. The negative relationship in columns (1) and (2) likely implies negative selection (UI takers are relatively less likely to return rapidly to a formal job) while the positive relationship in columns (3) and (4) likely implies positive selection (UI takers are relatively more likely to return rapidly to a formal job).[31] Yet, our main results are consistent across specifications and are robust to the inclusion of a rich set of individual controls. Such a concern is thus limited.

Our results hold using state–level formal employment rates, using only years after 2002, or including only mesoregions with average formal employment rates between the $5^{\text{th}}$ and the $95^{\text{th}}$ percentile (Appendix Table C.1). Taken together, they show that beneficiaries' propensity to return rapidly to a formal job after UI exhaustion is systematically higher where the formal sector is relatively larger, and it rises with formal employment rates. As a consequence, the mechanical effect of a UI extension decreases with formal employment rates, but the *potential* behavioral effect increases. There cannot be much distortion if beneficiaries are unwilling or unable to join the formal sector rapidly. How much of this *potential* behavioral effect translates into an *actual* behavioral effect is a question we address in the next section.

[31] UI take–up is high in Brazil: on average 86% of our eligible workers collect a first UI payment rapidly after layoff. The negative relationship is due to the 30–day waiting period: if the propensity to be formally reemployed increases with formal employment rates, workers are less likely to stay out of the formal sector in the first 30 days. More surprisingly, the relationship becomes positive when mesoregion fixed effects are included. Take–up rates were increasing over time and increased more where formal employment rates increased relatively more. We are currently investigating potential mechanisms behind this correlation.

## 5 Estimating the behavioral effect of UI extensions

In this section, we use exogenous variation in maximum benefit duration to estimate the behavioral effect of UI extensions. We show that (i) the spike in formal reemployment at benefit exhaustion is fully shifted following a UI extension, (ii) the behavioral effect is small, however, compared to the mechanical effect, and efficiency costs are thus limited, and (iii) the behavioral effect increases with formal employment rates and, combined with a smaller mechanical effect, efficiency costs therefore rise with formal employment rates. Our first empirical strategy illustrates all these results using a temporary 2–month UI extension in 1996 (difference–in–difference) and cross–sectional variation in the relative size of the formal sector across cities. Our second empirical strategy, a tenure–based discontinuity in eligibility, confirms our results. It provides local variation in maximum benefit duration (1–month) in every year and in every labor market. This allows us to show that our results hold using variation in formal employment rates across regions over time.

### 5.1 The 1996 temporary UI extension

Beneficiaries who exhausted their regular UI benefits between September and November 1996 in specific urban areas were eligible for 2 additional months of UI. Importantly, the UI extension was politically motivated and the differential implementation was unrelated to local labor market conditions. A UI extension for the city of São Paulo was proposed to the President by Jose Serra, a politician from the same political party (PSDB) who was struggling in his run for mayor of São Paulo that year. Jose Serra justified his proposal by the rising unemployment in the city. In response, workers' representatives defended a UI extension in all cities, arguing that "unemployment is increasing everywhere, not only where the PSDB candidate is doing badly" (Folha de São Paulo, 08/22/1996). This proposition was rejected because a national extension would have cost more than the budget threshold to avoid parliamentary process. As a compromise, the UI extension was implemented in the 9 historical metropolitan areas of the country and the Federal District.[32] Unemployment was mildly increasing in 1996; it was higher in 1997 when no extension took place.

[32]Bélem, Belo Horizonte, Curitiba, Fortaleza, Porto Alegre, Recife, Rio de Janeiro, Salvador, and São Paulo. *"the choice of the first 9 metropolitan regions* (in the 1970s) *was more related to the objective of developing an urban system in the country 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), translation by the authors.

The timeline of the experiment is summarized in Figure 5. On August 14 1996, the extension was first proposed. It was adopted a week later, on August 21 to start on September 1, 33 days before the first round of local elections. Formal employees displaced in April or May, and eligible for 5 months of UI, learned in August that they would be eligible for 2 additional months of UI after exhaustion of their regular benefits. No extra UI payment would be paid after December 31, so workers laid off in June could only draw 1 additional month of UI. The timing guarantees that workers could not be strategically laid off. It may also prevent us from estimating anticipation behaviors in the first months after layoff. In practice, nearly 100% of beneficiaries exhausted their full 5 months of UI in these years. There is thus no room for anticipation to matter.

We adopt a difference–in–difference strategy. Our sample includes full–time private–sector formal employees 18–54 years old, laid off in April or May, and eligible for 5 months of regular UI benefits (more than 24 months of tenure at layoff). We use 1995 and 1997 as control years. We have 9 treated areas since we exclude São Paulo to reinforce the exogeneity of our cross–sectional variation. We define 20 control areas, using all the urban centers granted the status of metropolitan area since 1996. In total, we have about 230,000 workers. There are a few differences between workers from control and treatment areas but these differences appear every year. Treatment and control areas are spread over the country and spanned a similar range of formal employment rates in these years. The distribution and composition of the sample are presented in Appendix Tables C.2 and C.3.[33]

#### 5.1.1 Graphical results

Our results can be seen graphically. Figure 6 displays survival rates out of formal employment and hazard rates of formal reemployment for UI takers in control and treatment areas in 1995, 1996, and 1997. Lines traced each other very closely in control areas or control years. But in 1996, in treatment areas, the spike in formal reemployment at regular UI exhaustion shifted by exactly 2 months. An additional 15% of workers were out of formal employment 7 months after layoff. 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 (Appendix Figure C.4).

[33]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 also displayed on a map in Figure C.3.

Figure 7 presents similar graphs for two treatment cities, Recife (Pernambuco) and Porto Alegre (Rio Grande do Sul), with formal employment rates around 24% and 35% at the time, respectively. In Recife and Porto Alegre in control years, hazard rates of formal reemployment at regular UI exhaustion spiked at 8% and 12%, respectively. In both cities, the spike shifted by exactly 2 months in 1996. Therefore, the mechanical effect of the UI extension was smaller but the behavioral effect larger in Porto Alegre, the city with a relatively larger formal sector.

### 5.1.2 Regression results

In the regression analysis, we estimate the following difference–in–difference specification for individual $i$ from metropolitan area $m$ in year $t$:

$$y_{i,m,t} = \alpha_m + \beta_t + \gamma \left[Year1996_t * TreatArea_m\right] + \epsilon_{i,m,t} \tag{5}$$

where $\alpha$ is a metropolitan area fixed effect and $\beta$ a year fixed effect. $\gamma$ is a difference–in–difference estimator for the impact of the UI extension on outcome $y$ under a common–trend assumption. Estimates of $\gamma$ are reported in Table 2. $\epsilon$ is an error term clustered by metropolitan area.[34] We consider two outcomes using only the UI registry data, regular UI duration (first 5 months) and total benefit duration (up to 7 months, columns 2 and 3). We also verify that we do not find an effect on UI take–up, a decision taken before the extension was announced (column 1). The behavioral effect is the difference between the total benefit duration of treated workers and the benefit duration of the same workers had they not responded to the incentives of the UI extension (their mechanical effect). To capture such a counterfactual, we construct a new variable (columns 4 and 5) using workers' formal reemployment patterns to infer how many UI payments they would have collected had they all been eligible for 7 months of UI. If they exhausted regular UI benefits, we assume that workers not formally reemployed within 1 month of exhaustion (resp. 2 months) would have collected 1 extra payment (resp. 2 extra payments). The mean in control years captures the mechanical effect of the UI extension; the difference–in–difference measures the behavioral effect.[35]

[34] Significance levels are similar if we bootstrap t–statistics by resampling our 29 clusters.

[35] Define $month_{regUI}$, the month a beneficiary exhausts her regular benefits. Define $month_{back}$, the month a beneficiary returns to a formal job. Formally, this variable is defined as:

$$\mathbb{1}\,(\text{exhaust regular UI benefits}) \times \sum_{j=1}^{2} \mathbb{1}\,(month_{back} > month_{regUI} + j)$$

To estimate how the behavioral effect varies with formal employment rates, we use the following specification for the same outcome:[36]

$$\begin{aligned} y_{i,m,t} =& \alpha_m + \beta_t + \gamma \left[Year1996_t * TreatArea_m\right] + \delta \left[Year1996_t * FormalEmploymentAbove_m\right] \\ &+ \zeta \left[Year1996_t * TreatArea_m * FormalEmploymentAbove_m\right] + X_{i,m,t} + \epsilon_{i,m,t} \end{aligned} \tag{6}$$

Both $\gamma$ and $\zeta$ are reported in column (5). They capture the behavioral effect in areas with below average formal employment rates and the differential effect in areas with above average formal employment rates, respectively.

We find no effect on UI take–up or regular benefit duration. At the time, beneficiaries collected on average 4.98 months out of their 5 months of UI. We would thus not have been able to find an effect on regular benefit duration even if beneficiaries had learned about the extension upon layoff. The extension increased benefit duration by 1.87 months in treatment areas in 1996. We estimate that only 13% of that increase, .25 month, is due to a behavioral effect (column 4). Indeed, had they been eligible for 7 UI payments, beneficiaries in control years would have collected 1.58 (6.56-4.98) additional months of UI absent any behavioral response (mechanical effect). The behavioral effect is 40% larger, .08 month, in areas with a relatively larger formal sector. We use our estimates to quantify the efficiency costs $\widetilde{\eta}$ in the bottom panel in Table 2. Because of the large mechanical effect, $\widetilde{\eta}$ is relatively small, ranging from .12 to .175. In comparison, Katz and Meyer (1990) estimate $\widetilde{\eta} > 1$ following a 3–month UI extension in the US. Efficiency costs increase by 45% from areas with low to high formal employment rates; the mechanical effect decreases 5% and the behavioral effect increases by 40%.

Results are identical if we include a rich set of individual controls, if we exclude observations from Rio de Janeiro, if we restrict attention to workers with replacement rates between 20% and 80%, and if we use formal employment rates linearly (Appendix Table C.4).[37] They are also robust to using either one of the control years (not shown).

In Appendix Table C.7, we test (successfully) whether we accurately predict in this way the increase in average benefit duration using workers' formal reemployment patterns after regular UI exhaustion.

[36]The indicator for above average formal employment does not enter the specification directly because we average formal employment rates over the 3 years. Our measures are based on yearly household surveys, PNAD, representative at the national level. We average out formal employment rates over the years to increase the number of observations per metropolitan area in the surveys.

[37]We favor specifications using 2 formal employment categories because of the small number of metropolitan areas.

In the Appendix (Table C.8), we show that the UI extension decreased the number of months of formal employment in the 2 years after layoff but also the probability that workers experience a new layoff from the formal sector. These results motivate the steady state budget constraint in Section 3. We also find no effect on subsequent match quality in the formal sector (wage).

#### 5.1.3 Extension: Who is more likely to respond to UI incentives?

Our pseudo–elasticity $\widetilde{\eta}$ also captures the maximum price the government should be willing to pay for a perfect job–search monitoring technology ($\widetilde{\eta}b$, see Section 3). The above results imply that the government can use local formal employment rates to predict where efficiency costs from UI extensions will be larger, and target monitoring accordingly. The maximum price it should be willing to pay remains low, however, at .175$b$ per beneficiary. But the government has more information about UI beneficiaries than where they reside. We investigate here to what extent it can use that information to predict which workers are more likely to respond to UI incentives.

We adopt a two–stage procedure. First, we look for whether the prevalence of a *signal* suggestive of a higher responsiveness to UI incentives differs by workers' categories. The spike in formal reemployment at regular UI exhaustion is an obvious candidate. We estimate the propensity to be formally reemployed in the 2 months after regular UI exhaustion using workers' characteristics $X$ and excluding observations from treatment areas in treatment year:

$$P\left(\text{Formally reemployed in the 2 months after UI exhaustion} = 1|X\right) = F\left(X\beta\right) \tag{7}$$

We use a logit model in Table 3, but we obtain similar results with a linear probability model. Second, we test whether workers with a higher predicted propensity have larger behavioral responses to UI extensions. In practice, we estimate the impact of the 1996 temporary extension on benefit duration separately by quartile of the predicted propensity. Our approach thus aggregates workers' characteristics and looks for heterogeneous treatment effects.

The upper panel of Table 3 shows estimates from a simplified specification of equation (7) to highlight some of the relevant heterogeneity. Marginal effects at the mean are lower for females, older and more educated workers, and workers with more tenure at layoff. Firm size and wages at layoff have non–linear marginal effects. In our two–stage procedure, we use workers' characteristics more flexibly. We include fixed effects by year (3), metropolitan area (29), education (9), sector of activity (50) and firm size (9). We also include $4^{\text{th}}$ order polynomials in tenure, age and log real wages. Estimates of $\widetilde{\eta}$ are then obtained in the same way as in Table 2 by quartile of the predicted propensity. The results are displayed in the bottom panel of Table 3. Because quartiles are based on a constructed variable, we obtain standard errors through bootstrapping of the two–stage procedure by resampling clusters.

Our *signal* is informative: the behavioral effect increases by 100% from the first to the fourth quartile, from .16 to .34 month. Because the mechanical effect also decreases (by construction), $\widetilde{\eta}$ increases by 145% from the first to the fourth quartile, from .09 to .23. The government can use information at hand to achieve some improvements in targeting. Nevertheless, the maximum price it should be willing to pay for a perfect monitoring technology remains quite low, at $.23b$ per beneficiary in the fourth quartile. Most of the heterogeneity is not easily captured by observable characteristics. This result may rationalize the absence of job–search monitoring in Brazil over our sample period since it is unlikely that a government would use more sophisticated specifications.[38]

### 5.2 A tenure–based discontinuity in eligibility

Using the 1996 temporary UI extension, we showed that there is a behavioral effect of UI extensions but that it amounts to a small share of the increase in benefit duration. The resulting efficiency costs are thus small. We also established that efficiency costs rise with formal employment rates, based on cross–sectional variation across labor markets. Our second empirical strategy confirms these findings. Moreover, it allows us to show that the relationship between efficiency costs and formal employment rates holds using variation across regions over time. In Brazil, maximum benefit duration depends on accumulated tenure over the 3 years prior to layoff or since the last UI payments. Workers with more than 6, 12, and 24 months of accumulated tenure are eligible for 3, 4, and 5 months of UI, respectively. As discussed in the Appendix (Figure C.5), the distribution of tenure at layoff is only continuous around the third cutoff. In this section, we exploit the change in eligibility around this cutoff in a regression discontinuity design. This provides us with local variation in maximum benefit duration (1–month) in every year and in every labor market.

[38] A linear probability model provides similar results. $\widetilde{\eta}$ would tend to infinity for a group of workers always formally reemployed right after UI exhaustion.

### 5.2.1 Sample selection

Because accumulated tenure is measured with noise, we focus on formal workers who had no other formal job in the previous 3 years.[39] In this sample, workers with more than 24 months and less than 22 months of tenure at layoff are eligible for 5 months and 4 months of UI, respectively. Workers with tenure between 22 and 24 months are eligible for either 4 or 5 months of UI because of the following two rules. There is a mandatory 1-month advance notice of layoff in Brazil. Most firms lay off workers immediately, paying an extra monthly wage. Others keep workers employed during the period. We cannot separately identify these two groups of firms and the advance notice period counts for UI eligibility. Moreover, 15 days of tenure count as 1 month for UI eligibility.

Our sample includes full–time private–sector formal employees 18–54 years old, laid off between 1997 and 2009. It has more than 3 million workers. We consider workers with tenure at layoff between 15 and 36 months.[40] Because of data limitations detailed in Section 2, we again use only workers laid off between January and June. A worker with 24 months of tenure at layoff in our sample must then have been hired between January and June, while a worker with 22 months of tenure at layoff must have been hired between March and August. Our identifying assumption is that the distribution of workers' characteristics is continuous in tenure at layoff, conditional on hiring and separation calendar months. We thus avoid issues related to seasonality.[41]

### 5.2.2 Graphical results

Our results are easily presented graphically. Panel (a) in Figure 8 displays actual benefit duration by tenure at layoff around the 24–month cutoff. Most workers collected all the UI payments for which they were eligible. Average benefit duration was thus constant and close to 4 months of UI for tenure levels below 22 months.[42] It increased to above 4.85 months for workers with 24 months of tenure. As expected, benefit duration for workers with tenure between 22 and 24 months lay in

[39] We are currently trying to tackle the following issues to replicate our results without this last selection condition. Because of missing worker IDs in the UI data, we cannot precisely measure accumulated tenure since the last UI payments. Because of specific rules (see main text above), tenure in a formal job as accounted for UI eligibility purposes is weakly higher than tenure as measured in our data. This noise increases with each previous employment.

[40] We cannot use observations prior to 1997 as we must observe workers' formal employment history in the previous 3 years. Our results are similar when we add workers with tenure between 12 and 15 months at layoff. These workers may be negatively selected given the discontinuity in the tenure distribution around 12 months shown in Figure C.5.

[41] Our results are identical without controlling for hiring and separation calendar months but the distribution of covariates appear affected by seasonality patterns.

[42] A very few beneficiaries supposedly eligible for 4 months of UI collected 5 months of UI.

between. In the regression analysis, we simply exclude these observations.

Extending UI by 1 month increased average benefit duration by .9 month. To estimate the share of this increase due to a behavioral effect, we adopt the same approach as for the 1996 temporary UI extension. We construct a new variable, plotted in panel (b) in Figure 8, using workers' formal reemployment patterns to infer how many UI payments they would have collected had they all been eligible for 5 months of UI. If they exhausted the first 4 months of UI, we assume that workers not formally reemployed within 1 month of UI exhaustion would have collected 1 extra payment. Observations to the left of the cutoff include only a mechanical effect. Observations to the right of the cutoff include both a mechanical effect and a behavioral effect. The discontinuity shows the behavioral effect.[43] It amounts to .08 month or only 9% of the total increase in benefit duration. Beneficiaries would have mechanically collected 4.8 UI payments if eligible for $5^{\text{th}}$ month of UI.

How these effects vary across labor markets with different formal employment rates is illustrated in Figure 9. It presents monthly hazard rates of formal reemployment for workers with tenure at layoff between 20 and 22 months (eligible for 4 months of UI) and between 24 and 26 months (eligible for 5 months of UI) in Pernambuco and Rio Grande do Sul. On average between 2002 and 2009, formal employment rates were 15 percentage points higher in Rio Grande do Sul than in Pernambuco. The spike in formal reemployment rates at UI exhaustion is clearly shifted by 1 month in both states. Because formal reemployment rates were higher, the mechanical effect of a 1–month UI extension was smaller and the behavioral effect larger in Rio Grande do Sul.

#### 5.2.3 Validity checks

We present validity checks supporting our identification strategy before turning to the regression analysis. Results in Table 4 are obtained by estimating the following local linear specification:

$$x_i = \alpha_0 + \beta \ \mathbb{1}(T_i \geq 0) + \gamma T_i + \delta \ \mathbb{1}(T_i \geq 0) * T_i + Z_i + \epsilon_i \tag{8}$$

[43] Define $month_{regUI}$, the month a beneficiary exhausts her $4^{\text{th}}$ month of UI benefits. Define $month_{back}$, the month a beneficiary returns to a formal job. Formally, this variable is defined as:

$$\mathbb{1}\left(\text{draw } 4^{\text{th}} \text{ UI benefits}\right) \times \sum_{j=1}^{1} \mathbb{1}\left(month_{back} > month_{regUI} + j\right)$$

In Appendix Table C.7, we test (successfully) whether we accurately predict in this way the increase in average benefit duration using workers' formal reemployment patterns after regular UI exhaustion.

where $x_i$ is some characteristic of worker $i$ and $T_i = Tenure - 24$ is the forcing variable. $\epsilon$ is an error term clustered by week of tenure. $Z_i$ includes only fixed effects for hiring and separation calendar months. Our coefficient of interest, $\beta$, would capture any discontinuous change in the value of covariates at the tenure cutoff. Estimates of $\beta$ are reported in Table 4. We perform a similar regression for the number of observations by week–of–tenure bin on each side of the cutoff (row 1). We exclude observations with tenure between 22 and 24 months but the results are similar in the overall sample. We consider the full tenure window around the cutoff in column (1) and a smaller tenure window — 18 to 30 months — in column (2). Estimates of $\beta$ are neither economically nor statistically significant for gender, age, log wages, replacement rates, sectors of activity, firm size, local formal employment rates, and the number of observations per tenure bin. One estimate is marginally significant for years of education in column (1), but it is economically insignificant (.03 year). Appendix Figure C.6 graphically confirms our identifying assumption. The results below are identical when we control for individual characteristics.

### 5.2.4 Regression results

To quantify the average impact of a 1–month UI extension at the tenure cutoff, we estimate similar local linear regressions as in equation (8):

$$y_i = \alpha + \beta \ \mathbb{1}(T_i \geq 0) + \gamma \ T_i + \delta \ \mathbb{1}(T_i \geq 0) * T_i + Z_i + \epsilon_i \tag{9}$$

where $y_i$ is an outcome of interest. $\beta$ captures a discontinuous impact at the tenure cutoff. Estimates of $\beta$ are reported in Table 5. We consider similar outcomes as for the 1996 temporary UI extension using only the UI registry data: UI take–up, benefit duration censored at 4 months of UI, and total benefit duration (columns 1–3). We use the variable plotted in panel (b) in Figure 8 to estimate the increase in benefit duration due to a behavioral effect (column 4). $\alpha$ measures the mechanical effect; $\beta$ measures the behavioral effect. In Table 5, we use the larger tenure window and exclude observations with tenure between 22 and 24 months.

Average benefit duration for workers eligible for 4 months of UI was around 3.96 months. We estimate an increase of .91 month at the eligibility cutoff. The behavioral effect amounts to .08 month or 9% of the total increase in benefit duration. Interestingly, we even find a very small

(.005 month) effect on benefit collection of the first 4 UI payments. This suggests some limited anticipation behaviors. Our results are robust to controlling for individual characteristics, to using a smaller tenure window, to considering only years after 2002, to restricting attention to workers with replacement rates between 20% and 80%, and to including only mesoregions with average formal employment rates between the 5$^{\text{th}}$ and the 95$^{\text{th}}$ percentile (Appendix Table C.5).[44]

We investigate how the behavioral effect and the resulting efficiency costs vary with local formal employment rates, using the following specification:

$$\begin{aligned}
y_{i,m,t} = {} & \alpha_m + \omega_t + \beta\ \mathbb{1}(T_{i,m,t} \geq 0) + \gamma\ T_{i,m,t} + \delta\ \mathbb{1}(T_{i,m,t} \geq 0) * T_{i,m,t} \\
& + \zeta\ \mathit{FormalEmploymentRates}_{m,t} + \kappa\ \mathit{FormalEmploymentRates}_{m,t} * \mathbb{1}(T_{i,m,t} \geq 0) \\
& + \psi\ \mathit{FormalEmploymentRates}_{m,t} * T_{i,m,t} \\
& + \xi\ \mathit{FormalEmploymentRates}_{m,t} * \mathbb{1}(T_{i,m,t} \geq 0) * T_{i,m,t} + Z_{i,m,t} + \epsilon_{i,m,t}
\end{aligned} \tag{10}$$

where $\alpha_m$ and $\omega_t$ are mesoregion and year fixed effects. We use demeaned formal employment rates linearly to fully exploit the cross–sectional and time variation. Our outcome of interest is the same outcome as in column (4) in Table 5. $\beta$ measures the average behavioral effect at the tenure cutoff. $\zeta$ and $\kappa$ measure how the mechanical and behavioral effects vary with formal employment rates, respectively. We report estimates of $\beta$, $\zeta$, and $\kappa$ in Table 6 for specifications without fixed effects, with only year fixed effects, with both year and mesoregion fixed effects, and with the addition of a rich set of individual controls (columns 1–4). We use formal employment rates by mesoregion as in Section 4.

We estimate a systematic negative relationship between the mechanical effect and formal employment rates and a systematic positive relationship between the behavioral effect and formal employment rates. These relationships are not due to fixed characteristics of labor markets. They are identical using variation over time across regions (column 3 compared to column 2). The results are not due to simple composition effects. They are identical controlling for a rich set of individual characteristics, including wage and sector of activity (column 4 compared to column 3).

[44] In Appendix Table C.8, we find that the number of months of formal employment in the 2 years after layoff decreased at the cutoff as did the probability that workers experience a new layoff from the formal sector. We also find no effect on subsequent match quality in the formal sector (wage). These results confirm our findings using the 1996 temporary UI extension.

Our results are also robust to using formal employment rates by state, to using a smaller tenure window, to considering only years after 2002, to restricting attention to workers with replacement rates between 20% and 80%, and to including only mesoregions with average formal employment rates between the $5^{\text{th}}$ and the $95^{\text{th}}$ percentile (Appendix Table C.6).

Finally, the bottom panel in Table 6 uses estimates from column (4) to quantify the efficiency costs of the UI extension, $\widetilde{\eta}$. The efficiency costs are low at any level of formal employment (around .1 at the sample mean) because most of the cost of extending UI is not due to distortions. Efficiency costs are increasing, however, with formal employment rates. Moving from 15 percentage points below to 15 percentage points above the sample mean ($25^{\text{th}}$ percentile and $99^{\text{th}}$ percentile of the mesoregion–by–year distribution), increases efficiency costs by 73%; it increases the behavioral effect by 56% and decreases the mechanical effect by 10%.

# 6 The benefits of UI extensions and welfare simulations

We have established that (i) UI extensions are costly in Brazil but generate small efficiency costs from moral hazard (formal work disincentives), and (ii) efficiency costs rise with formal employment rates. As derived in Section 3, we can evaluate welfare effects of UI extensions locally by comparing the efficiency costs and the social value of the income transfer to UI exhaustees. In this section, we investigate this social value using available survey data. We then evaluate welfare effects.

## 6.1 Social value of insurance and welfare effects of UI extension

We derive welfare effects from a marginal UI extension in Section 3 as:

$$\frac{1}{\frac{D^f}{D^f+D^u}} \frac{dW/dP}{g^E w^f} = qr\ S_{P+1} \left[ \frac{g^{U_{P+1}} - g^E}{g^E} - \widetilde{\eta} \right] \tag{11}$$

The *social value of insurance* $\frac{g^{U_{P+1}}-g^E}{g^E}$ corresponds to the social value of transferring \$1 from the average taxpayer (with marginal social value $g^E$) to the average UI exhaustee (with marginal social value $g^{U_{P+1}}$). It includes both the relative need for income support for UI exhaustees compared to taxpayers (ratio of marginal utilities), and social planner preferences towards redistribution. A UI extension increases welfare if the social value of insurance exceeds the pseudo–elasticity $\widetilde{\eta}$, which measures efficiency costs. To investigate the social value of insurance, we proceed in three steps.

First, we distinguish between our two types of UI exhaustees, the unemployed and the informally reemployed. They may have different need for income support. Define $O$ as the share of unemployed UI exhaustees. The social value of insurance can be written as:

$$\frac{g^{U_{P+1}} - g^E}{g^E} = O\frac{g^{O_{P+1}} - g^E}{g^E} + (1-O)\,\frac{g^{I_{P+1}} - g^E}{g^E} \tag{12}$$

where $g^{O_{P+1}}$ and $g^{I_{P+1}}$ are the social values of \$1 for unemployed and informally reemployed UI exhaustees, respectively. We estimate $O$ using longitudinal survey data.

Second, the value of insurance can be decomposed as follows (Baily, 1978; Chetty, 2006):[45]

$$\frac{g_P^J - g^E}{g^E} = \gamma\frac{c^E - c_P^J}{c^E}, \quad \text{with J=O,I.} \tag{13}$$

where $\frac{c^E - c_P^J}{c^E}$ corresponds to the mean consumption gap between taxpayers and UI exhaustees of type $J$. $\gamma$ captures both an average coefficient of relative risk aversion and social planner preferences toward redistribution. A high value of $\gamma$, or large consumption gaps, increases the social value of insurance. There is no data on consumption or savings for UI beneficiaries in Brazil. Instead, using the same longitudinal survey data, we measure average disposable income for the formally employed and the two types of UI exhaustees in order to approximate these consumption gaps. Finally, we calibrate the social value of insurance for different values of $\gamma$.

## 6.2 Are UI exhaustees unemployed or informally reemployed?

We rely on urban labor force surveys to estimate the share of unemployed UI exhaustees $O$ (Pesquisa Mensal de Emprego, PME, 2003–2010). PME has the same structure as the *Current Population Surveys* (CPS) in the US. Households enter the sample for 2 periods of 4 consecutive months, 8 months apart from each other. PME covers the six largest urban areas of Brazil and is used to compute official unemployment rates. Each survey asks for the labor market status of every household member above 10 years old, information on wage, and tenure in the job. Formality is captured by asking whether her employer signed the respondent's working card. The unemployed

[45] The decomposition assumes that third derivatives of utility functions are small.

(whether or not they are searching for a job) are asked about their labor status and tenure in the last job, the reason for separation, and the length of their unemployment spell (in months).

Using consecutive interviews, we can estimate the job–finding probability in the subsequent month given respondents' unemployment duration. We estimate these hazard rates of overall reemployment (formal and informal) by maximum likelihood.[46] We want a likelihood function flexible enough to capture a possible spike in overall reemployment rates. We therefore assume a piece–wise constant hazard functions with 6 parameters, accounting for different hazard rates in months 0, 1–2, 3–4, 5–6, 7–8, and 9–10. Our likelihood function also corrects for a stock sampling issue within month. Define $\lambda_m$ as the daily hazard rate constant over month $m = 0, 1, ..., 10$ since layoff. Assume a respondent is interviewed on day $b \in [0, 30]$ within month $m$. She can only be observed on day $b$ if she survived $b$ days without a job, given that she already survived $m$ months. Define $k(b)$ as the distribution of interviews over days within a month. Finally, define $d_{i,m} = 1$ if individual $i$, unemployed since month $m$, is reemployed by the time of the subsequent interview. The likelihood for a given observation is thus:

$$\begin{aligned} L_{i,m} = d_{i,m} \int_0^{30} \left[1 - exp\left(-\left(30 - b\right)\lambda_m - b\lambda_{m+1}\right)\right] \frac{k\left(b\right) exp\left[-b\lambda_m\right]}{\int_0^{30} k\left(s\right) exp\left[-s\lambda_m\right] ds} db \\ + \left(1 - d_{i,m}\right) \int_0^{30} \left[exp\left(-\left(30 - b\right)\lambda_m - b\lambda_{m+1}\right)\right] \frac{k\left(b\right) exp\left[-b\lambda_m\right]}{\int_0^{30} k\left(s\right) exp\left[-s\lambda_m\right] ds} db \end{aligned} \tag{14}$$

Our sample includes individuals 18–54 years old who were full–time private–sector formal employees with more than 24 months of tenure at layoff in their last job (eligible for 5 months of UI).[47] We have 30,749 observations contributing to the likelihood function. Our sample cannot be conditioned on UI take–up because survey questions do not cover UI benefits. In the estimations, we assume that interviews are uniformly distributed, $k(b) = 1/30$. Estimations are performed using sampling weights and clustering standard errors by individual.

The estimated monthly hazard rates of overall reemployment are displayed in panel (a) in Figure 10. Point estimates start at 22% in the first month after layoff and decrease to stabilize at around 18% from month 3 onwards. We display in the same graph hazard rates of formal

[46] For workers who find a job, we are unable to estimate later transitions to other jobs because questions about past unemployment spells are not asked in that case and the panel is too short.

[47] Although samples are representative of the overall labor force in the six metropolitan areas, this does not guarantee that they are representative of the unemployed labor force with more than 2 years of tenure in the last formal job.

reemployment using a random sample of similarly selected workers in our administrative data. Formal reemployment rates are higher than in previous Figures in the first few months because the sample is not conditioned on UI take–up. They are particularly high during the 30–day waiting period. As usual, they spike after month 5 since layoff (from .04 to .14). They are, however, systematically smaller than overall reemployment rates. Many UI beneficiaries are thus informally reemployed.[48] Confidence intervals rule out the existence of a large spike in overall reemployment. Because we established that the spike in formal reemployment is driven by UI incentives, most of the behavioral effect of UI extensions is due to beneficiaries (re)employed in the informal sector ($f$ margin in the model of Section 3).

Panel (b) in Figure 10 displays the corresponding survival rates. We estimate that about 30% of workers are unemployed 1 month after typical UI exhaustion. In comparison, 65% are still out of formal employment. Therefore, even if informal reemployment is prevalent, a significant share of UI exhaustees remains unemployed. Our estimate is similar to exhaustion rates of the 26 weeks of UI in the US (around 35%). We use these estimates in our simulation and assume that 46% of UI exhaustees are unemployed.

### 6.3 Relative need of income support

Labor status does not directly provide information on UI exhaustees' relative need for income support. Beneficiaries (re)employed in the informal sector may earn a low wage, while the unemployed may have family members with a high income. Using the same surveys and sample as above, we measure average disposable income for the formally employed before layoff (typical UI contributors), the unemployed around UI exhaustion, and the informally reemployed. We observe the informally reemployed and their disposable income only upon reemployment. We assume that they have similar income levels around UI exhaustion. Disposable income is defined as household income per capita per month, with an equivalence scale of 1/2 for children. In Section 5, we estimated that efficiency costs were larger in labor markets with higher formal employment rates. Ceteris paribus, this decreases welfare effects of UI extensions. A greater need for insurance, however, may compensate for larger efficiency costs.[49] We therefore divide our sample in two groups: the two

[48]Among the workers reemployed but not as formal employees, the surveys reveal that 30.5% are self–employed, 2% are employers, and 67.5% are informal employees.

[49]For instance, the need for insurance may be greater if there are fewer informal employment opportunities.

metropolitan areas from the poorer, less formal Northeast and the four metropolitan areas from the richer, more formal South/Southeast.

First, we re–estimate our maximum likelihood separately for each group. Formal employment rates were on average .28 and .36 over the period in the Northeast and the South/Southeast, respectively. We obtain comparable shares of unemployed UI exhaustees in the two groups (.48 and .43, respectively). This share is in fact slightly larger in the less formal labor markets. These results are reported in the first two rows in Table 7.

Second, we measure average disposable income separately for each group. They are systematically higher in the South/Southeast. But disposable income ratios between labor statuses are very similar across groups. Average disposable income for the informally reemployed is 36% and 34.5% smaller than for formal employees prior to layoff. Corresponding average disposable income for the unemployed UI exhaustees is 54% and 51% smaller. These figures reveal large disposable income gaps, including for the informally reemployed. Average levels for the unemployed UI exhaustees may understate the need for income support: 37% and 30% of them, respectively, have no source of household income at all. None of the estimates provided in Table 7 offer any evidence of a greater need for income support among UI exhaustees from more formal labor markets. If anything, relative income gaps are always larger in the poorer, less formal Northeast.

### 6.4 Welfare simulations

We now use our results to evaluate the welfare effects of a UI extension in our context. Table 8 displays welfare effects of a marginal UI extension (in bold) obtained from evaluating equation (11). Welfare effects are measured in terms of an equivalent percentage change in total payroll of eligible formal employees. We use estimates of efficiency costs from Table 2 (low formal=.12, high formal=.175). The social value of insurance is calibrated using the decompositions in equations (12)–(13) and disposable income ratios from Table 7 for different values of $\gamma$, which captures both an average coefficient of relative risk aversion and social planner preferences towards redistribution. We use the same social value of insurance in labor markets with different formal employment rates because we did not find evidence of differential disposable income ratios in Table 7. For a given value of $\gamma$ (column 1), the table displays the corresponding social value of insurance (column 2) and the resulting welfare effects in labor markets with relatively high and relatively low formal employment rates (columns 3 and 4). Alternatively, without relying on our calibration, the table displays the welfare effects for a given social value of insurance.[50]

Welfare effects are positive unless the social value of insurance is very low. For $\gamma = 1$ (social value of \$1 is 42% higher for UI exhaustees), extending UI benefits by 1 month has a similar effect on welfare as increasing wages of eligible formal employees by .31%–.4%. Welfare effects are 29% ($\frac{.4-.31}{.31}$) higher in labor markets with low formal employment rates because of smaller efficiency costs. Because the efficiency costs are at most .175, welfare effects are positive as long as the social value of \$1 is 17.5% larger for UI exhaustees than for individuals contributing to the UI budget (or the marginal cost of public fund). A similar lower bound on the social value of insurance for a UI extension to increase welfare would be above 100% in the US, using estimates from Katz and Meyer (1990, $\widetilde{\eta} > 1$). Chetty (2008) estimates that the social value of \$1 in the US is 150% larger for UI beneficiaries at the start of their unemployment spell than for employed individuals, because of high risk aversion. Welfare effects in Table 8 are equivalent to raising wages of eligible formal employees by 1.68%–1.84% for this social value.[51] Incorporating our empirical findings in our framework, the welfare effects of a UI extension are thus likely positive and may be sizeable.

### 6.5 Discussion

Using a conceptual framework to guide our analysis, we have established that UI extensions in Brazil impose small efficiency costs due to formal work disincentives (moral hazard) and are likely welfare–enhancing. We discuss here some limitations of our framework.

First, our measure of efficiency costs entails both an income and a substitution effect. Separating them (Card et al., 2007a) provides information on the welfare gains from social insurance programs (Chetty, 2008). Yet, conditional on a given social value of insurance, welfare consequences of extending UI depend solely on the ratio of the behavioral to the mechanical effect in a large class of models, as long as an envelope condition applies to the agents' problem (Chetty, 2006).

Second, layoffs may increase when UI benefits increase. We followed the literature and ab-

[50] We use the average monthly layoff rate taking into account incomplete UI take–up ($q = .0291 * .86$) and the average replacement rate ($r = .65$).

[51] If individuals have significant liquid savings to deplete when unemployed (which is not the case in the US; Chetty, 2008), lower values of $\gamma$ should be considered. The availability of liquid savings decreases local relative risk aversion (Chetty and Szeidl, 2007). Even if we assume that the social value of redistributing \$1 towards the informally reemployed is nil, welfare effects are positive as long as the social value of \$1 is 35% larger for unemployed UI exhaustees than for individuals contributing to the UI budget (available from the authors).

stracted from this margin since the optimal policy is to introduce experience rating. Blanchard and Tirole (2006), however, show that full experience rating may not be optimal if firms are credit–constrained. Patterns in Appendix Figure C.5 suggest that UI affects layoffs at low tenure. Existing institutions appear sufficient to prevent such responses for the workers we considered.

Third, there may be relevant general equilibrium effects even though it is unclear whether welfare effects would be smaller taking those into account. Welfare effects would increase in the presence of search externalities, but likely decrease in wage bargaining models (Landais et al., 2012). Entitlement effects could attract workers to the formal sector if they value UI (Hamermesh, 1979). In contrast, UI taxes may be more distortive in poorer countries. To our knowledge, there is no empirical evidence on the relative magnitude of these two mechanisms even in developed countries. Almeida and Carneiro (2012) show that labor inspections targeting non–compliance with mandated benefits by formal firms increased formal employment in Brazil. Workers appear willing to trade off lower wages for mandated benefits, including benefits related to job–loss risk (severance payments).

Fourth, there may be externalities attached to moral hazard induced informal employment. There is no consensus, however, on the magnitude or sign of such externalities.[52] If there was a consensus, the welfare formula (11) could include a third component: the impact of UI extensions on informal employment multiplied by the social cost (value) of the externality.

Finally, a welfarist perspective may not be an accurate positive theory of governments. If governments consider their budget as fixed, for instance, our results are reversed. UI extensions are costly in our context and they become relatively cheaper, even if more distortive, when formal employment rates increase.

# 7 Conclusion

This paper estimates the efficiency costs of UI extensions in a context where informal labor is prevalent by combining a model of optimal social insurance and an unusually rich dataset on Brazilian UI beneficiaries over 15 years. The main results are that the efficiency costs of UI extensions are rather small, but that they rise with the relative size of the formal labor market.

[52] Informal employment is often viewed as generating negative externalities (Levy, 2008). One could argue in our case, however, that a behavioral effect caused by beneficiaries working informally generate positive externalities compared to a behavioral effect caused by beneficiaries not working at all (as in more developed countries).

These findings run counter to widespread claims in policy circles that heightened concerns of moral hazard preclude the expansion of unemployment insurance in countries with a large informal sector.

Because Brazil contains regions with such widely divergent levels of income and labor market formality, we are optimistic about the external validity of our study. In fact, understanding the relationship between efficiency and formality in other settings is an exciting avenue for future research. We also discuss some mechanisms besides moral hazard and beyond the scope of our framework (e.g. general equilibrium effects) that could, in theory, increase or decrease efficiency costs from UI extensions. More research is needed to evaluate their empirical relevance.

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 (2012) points in the same direction: intensive–margin taxable income elasticities are small in Pakistan even though evasion is widespread. Of course, the expansion of social policies may be driven instead by the political process and by policymakers' preferences for redistribution (Acemoglu and Robinson, 2008).

Second, governments' lack of strict enforcement strategies is typically considered to be an issue, even though little is known about enforcement costs. We find that the absence of job–search monitoring for UI beneficiaries in Brazil may have been rational. The recent introduction of job–search requirements may provide evidence on the cost–effectiveness of monitoring in this context.

Finally, our results suggest that weak governmental institutions may become even more policy relevant when a country's economy develops and its formal employment sector expands. Fiscal and social policies should adjust to these changing circumstances in rapidly developing countries — such as China and Brazil — and may be best partially decentralized to subnational governments, given how local labor market conditions affect the efficiency costs of social programs.

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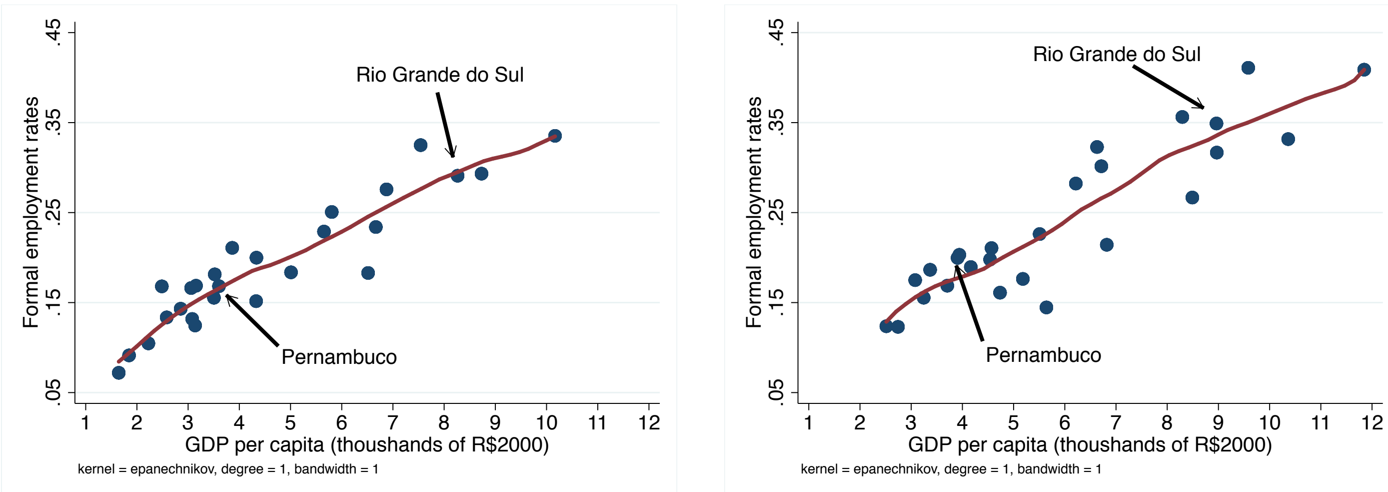
Figure 1: Formal employment rates and GDP per capita in Brazil

(a) Average by state (1995–2002) (b) Average by state (2003–2009)



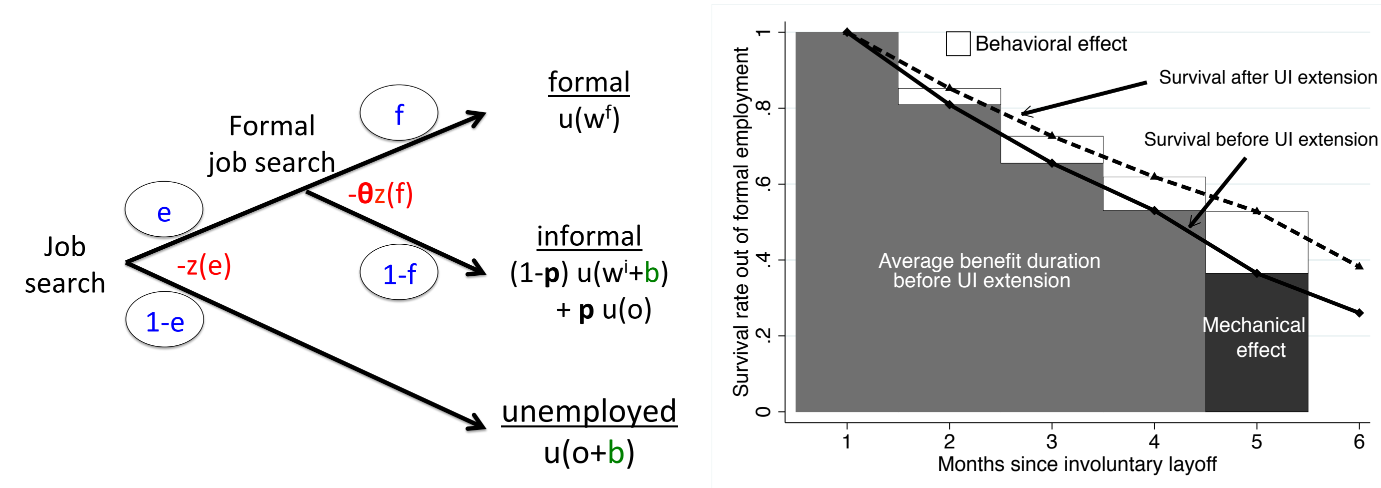

Both panels display formal employment rates and GDP per capita by state in Brazil. Variables are averaged over the period 1995–2002 in panel (a) and over the period 2003–2009 in panel (b). Formal employment rates by state (private–sector formal employees within the 18–54 years old population) are obtained from yearly household surveys (PNAD). GDP per capita by state (R$1.9≈US$1 in 2000) are obtained from Instituto Brasileiro de Geografia e Estatística (IBGE).

In the cross-sections, formal employment rates strongly correlate with GDP per capita and there is wide variation across states. Brazil experienced high economic growth in the second time period. Both GDP per capita and formal employment rates increased overall, but increases were not uniform across areas. We obtain similar patterns when we include public employees.

Figure 2: Connecting theory to the data

(a) Per–period choice situation of a dynamic endogenous job–search model with informal work opportunities

(b) Mechanical and behavioral effects of a UI extension



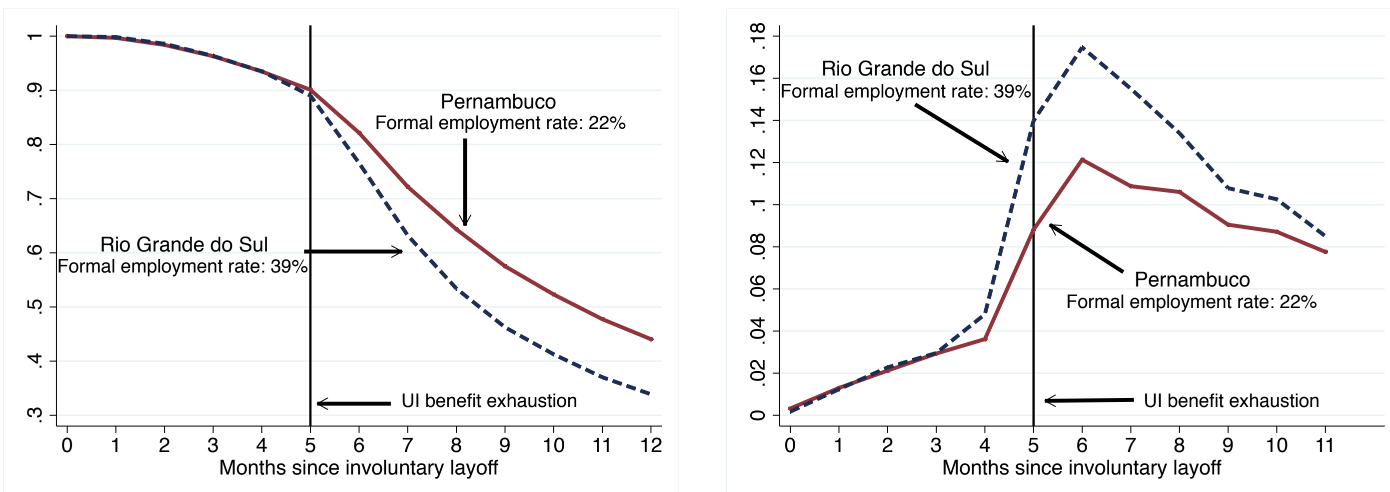

Panel (a) describes the choice situation that displaced formal employees face each period before formal reemployment in our dynamic model of endogenous job–search. While unemployed, a worker decides how much effort $e$ at a cost $z(e)$ to invest in finding a new job. Search efforts are normalized to correspond to job–finding probabilities. With probability $1-e$, she does not find a job and stays unemployed. With probability $e$, she finds a job. She can increase her probability of returning to a formal job by investing formal search effort $f$ at a cost $\theta z(f)$. She thus finds a formal job with probability $ef$ and an informal job with probability $e(1-f)$. She earns wage $w^i < w^f$ when working informally. She can always search for a formal job at the same cost $\theta z(f)$ in subsequent periods (she starts from the "formal job search" node). We further assume that informal jobs are detected by the government with probability $p$. If detected, an informal worker falls back into unemployment and loses her UI benefits. Both the unemployed and the "undetected" informally employed draw UI benefits $b$ in the first $P$ periods after layoff. The unemployed have a minimum consumption level $o$. The solution to this dynamic problem determines survival rates out of formal employment and therefore the average benefit duration.

Panel (b) represents graphically the mechanical and behavioral effects on average benefit duration of extending UI by one period (UI costs are average benefit duration times the benefit level). Workers not formally reemployed 1 month after regular UI exhaustion before the extension draw the extra payment without changing their behavior (mechanical effect). A UI extension also reduces incentives to be formally reemployed ($e \downarrow, f \downarrow$). As a result, survival rates, and average benefit duration, may increase both during the period of extension and in earlier periods (behavioral effect). The ratio of the behavioral to the mechanical effect (pseudo–elasticity $\widetilde{\eta}$) captures efficiency costs (Saez et al., 2012), because it measures the fraction of social spending lost through behavioral responses. A UI extension increases welfare if the social value of the income support provided to UI exhaustees exceeds $\widetilde{\eta}$.

Figure 3: Reemployment patterns of UI beneficiaries in Brazil

(a) Survival out of formal employment

(b) Hazard of formal reemployment




Full–time private–sector formal employees 18–54 years, eligible for 5 months of UI after having been laid off in 2009 in Pernambuco (or Rio Grande do Sul), a poor (rich) state with relatively low (high) formal employment rates in the Northeast (South) of Brazil. The sample is restricted to UI takers. The left panel displays survival rates out of formal employment in the months following layoff. The right panel displays the corresponding monthly hazard rates of formal reemployment.

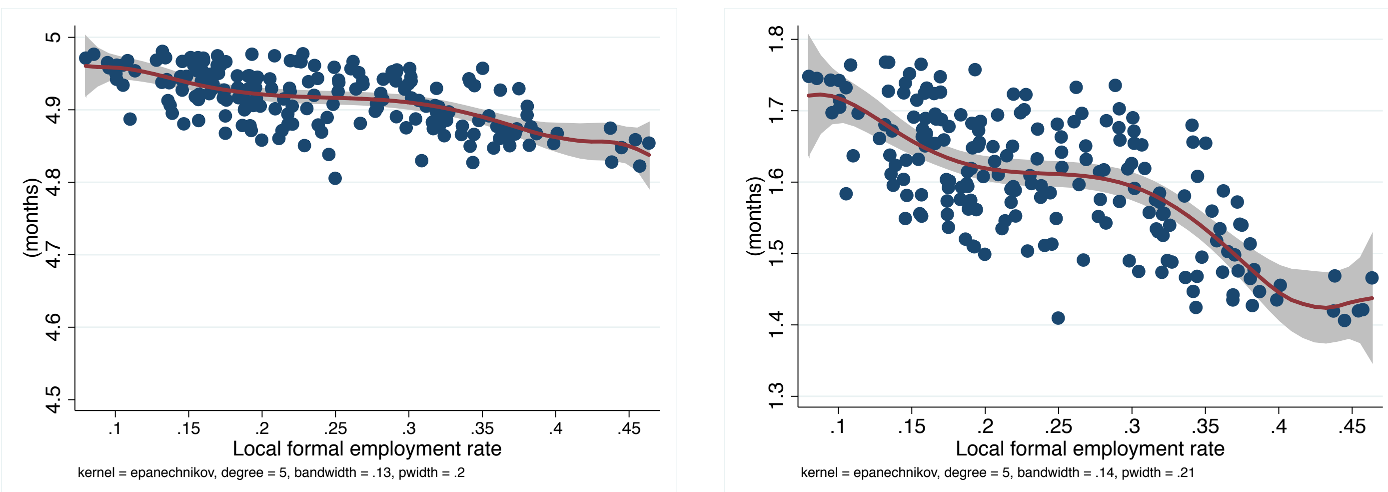
While workers draw UI benefits, formal reemployment rates are very low in both states. After UI exhaustion, they spike and increase relatively more in the state with higher formal employment rates. This suggests a clear behavioral response to UI incentives, larger in Rio Grande do Sul. Formal reemployment rates stay quite low, however, even after UI exhaustion: about 40% of workers were still out of formal employment 12 months after losing their formal job.

If UI had been extended by 2 months for these workers, most of them (70%–80%) would have mechanically collected additional UI payments, without changing their behavior. In Section 5, we find that the spike in formal reemployment at benefit exhaustion is completely shifted following actual UI extensions. In Section 6, we find that there is no such spike in overall reemployment rates; the behavioral effect is thus driven by informally reemployed beneficiaries. Nevertheless, the size of this behavioral effect is small compared to the mechanical effect, and so the efficiency costs are limited. The spike being larger, the mechanical effect is smaller and the behavioral effect larger in Rio Grande do Sul. This suggests that efficiency costs rise with formal employment rates.

Figure 4: Regular benefit duration and mechanical effect of a hypothetical 2–month UI extension

(a) Regular benefit duration

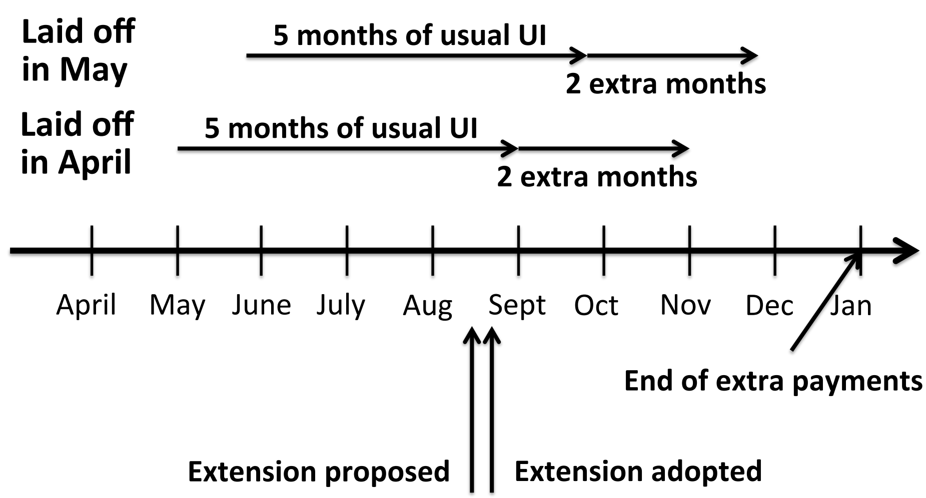
(b) Mechanical effect of a hypothetical 2–month UI extension




Full–time private–sector formal employees 18–54 years old, laid off between 2002 and 2009, and eligible for 5 months of UI. The sample is restricted to UI takers. Observations are averaged out by state–year. The left panel displays the relationship between regular benefit duration and formal employment rates (PNAD surveys). The right panel displays the relationship between the mechanical effect of a hypothetical 2–month UI extension (the increase in benefit duration absent any behavioral response) and formal employment rates (PNAD surveys).

Regular benefit duration decreases slightly with formal employment rates but remains very high at any level. Beneficiaries draw on average 4.85 to 4.95 months of UI (out of a maximum of 5 months). Formal reemployment rates increase after regular UI exhaustion but remain low. As a consequence, extending UI by 2 months would be costly in Brazil absent any behavioral response. The mechanical effect varies from 1.75 months in states with low formal employment rates to 1.4 months in states with high formal employment rates. The relationship with formal employment is negative because formal reemployment rates after regular UI exhaustion increase with local formal employment rates.

Figure 5: Timeline of the 1996 temporary UI extension

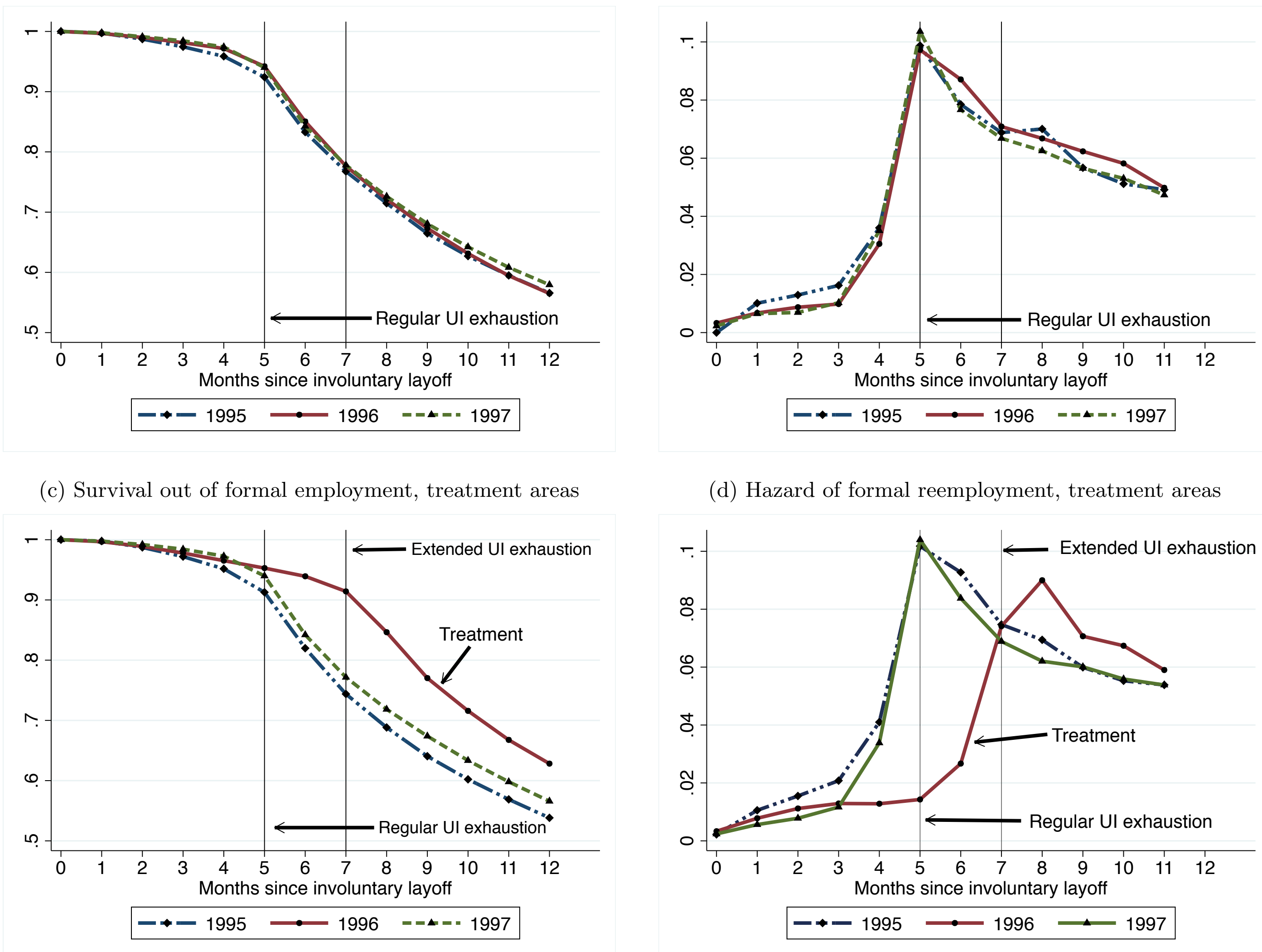



Private–sector formal employees laid off in April and May 1996 who exhausted their 5 months of regular UI benefits between September and November 1996 in treated metropolitan areas were eligible for 2 additional UI payments. The UI extension was proposed on Aug and was adopted on Aug 21 to start on September 1, 33 days before the first round of local elections. No extra payments would be paid after December 31.

Figure 6: The 1996 temporary UI extension, impacts on formal reemployment

(a) Survival out of formal employment, control areas

(b) Hazard of formal reemployment, control areas




(c) Survival out of formal employment, treatment areas

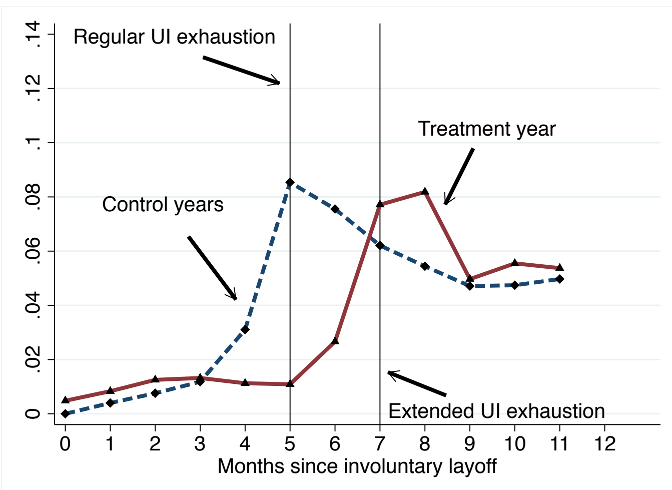
(d) Hazard of formal reemployment, treatment areas

Full–time private–sector formal employees 18–54 years old from the largest metropolitan areas of Brazil (São Paulo excluded), laid off in April or May 1995, 1996, and 1997, and eligible for 5 months of UI benefits. In 1996, in treated metropolitan areas, these workers were eligible for 7 months of UI benefits. The sample is restricted to UI takers. The left panels display the survival rates out of formal employment after layoff in each year for displaced workers from control (panel a) and treatment areas (panel c). The right panels display the hazard rates of formal reemployment after layoff in each year for displaced workers from control (panel b) and treatment areas (panel d).

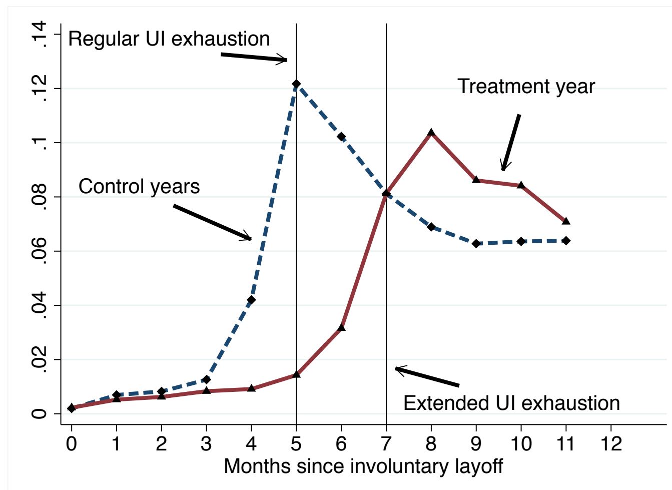
In control areas or in control years, survival rates out of formal employment and hazard rates of formal reemployment followed each other closely. But in 1996, the spike in formal reemployment rates at regular benefit exhaustion shifted by exactly 2 months. As a consequence, an additional 15% of workers was still out of formal employment 7 months after layoff.

Figure 7: The 1996 temporary UI extension, impacts in different metropolitan areas

(a) Recife (Pernambuco, formal employment rate 24%)




(b) Porto Alegre (Rio Grande do Sul, formal employment rate 35%)

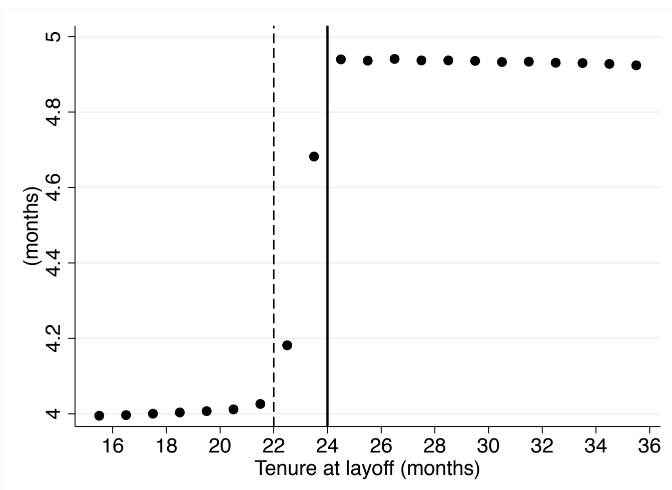



Full–time private–sector formal employees 18–54 years old from the largest metropolitan areas of Brazil (São Paulo excluded), laid off in April or May 1995, 1996, and 1997, and eligible for 5 months of UI benefits. In 1996, in treated metropolitan areas, these workers were eligible for 7 months of UI benefits. The sample is restricted to UI takers. The left panel displays the hazard rates of formal reemployment after layoff in control and treatment year for displaced workers from Recife, a treatment metropolitan area (state of Pernambuco), where formal employment rates were relatively low (24%). The right panel displays the hazard rates of formal reemployment after layoff in control and treatment year for displaced workers from Porto Alegre, a treatment metropolitan area (state of Rio Grande do Sul), where formal employment rates were relatively high (35%). Formal employment rates are obtained from yearly household surveys (PNAD).

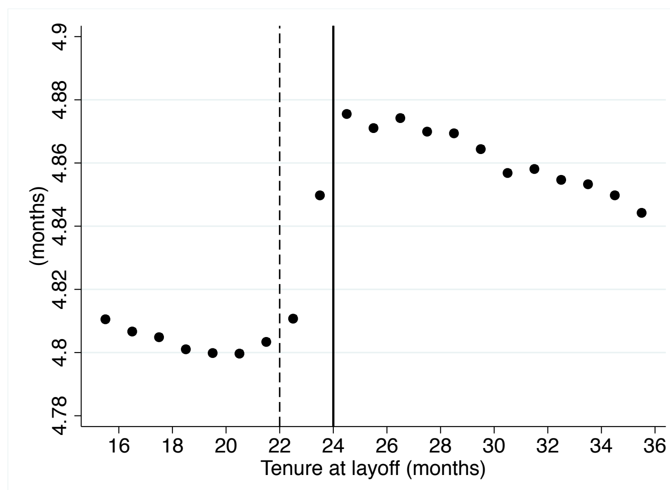
Hazard rates of formal reemployment at regular UI exhaustion spiked to 8% and 12% in control years in Recife and Porto Alegre, respectively. In both metropolitan areas, the spike shifted by exactly 2 months in 1996. Because the spike was larger in Porto Alegre, the mechanical effect of the UI extension was smaller and the behavioral effect larger in Porto Alegre.

Figure 8: Regression discontinuity design, impacts on UI benefit duration

(a) Actual UI duration




(b) Extended UI duration compared to counterfactual

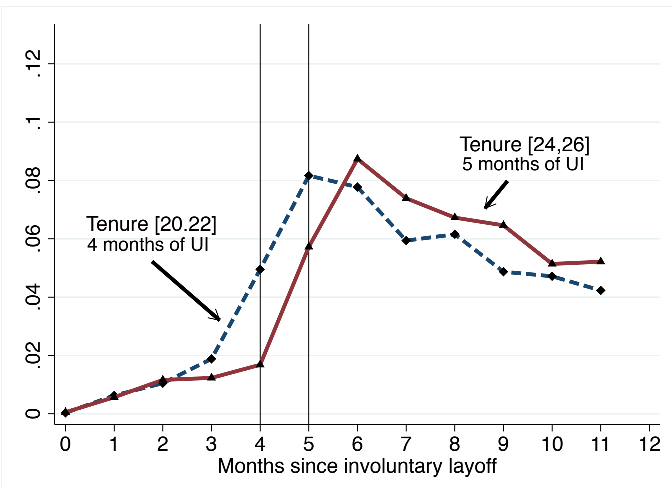



Full–time private–sector formal employees 18–54 years old, laid off between 1997 and 2009. Workers with more than 24 months of tenure at layoff were eligible for 5 months of UI. Workers with less than 22 months of tenure at layoff were eligible for 4 months of UI. Workers with tenure between 22 and 24 months at layoff were eligible for either 4 or 5 months of UI (see text). Outcomes are averaged out by month of tenure. The sample is restricted to UI takers. Panel (a) plots the actual benefit duration by tenure at layoff. The discontinuity at the tenure cutoff shows the effect of a 1–month UI extension on average benefit duration. Panel (b) displays the mechanical and behavioral effects of the 1–month UI extension. Observations to the left of the cutoff show the mechanical effect, the increase in benefit duration if beneficiaries eligible for 4 months of UI did not change their behavior but could have collected a $5^{\text{th}}$ month of UI. Observations to the right of the cutoff show the actual benefit duration for beneficiaries eligible for 5 months of UI. The discontinuity at the tenure cutoff shows the behavioral effect. The scale of the y–axis is 10 times smaller in panel (b).

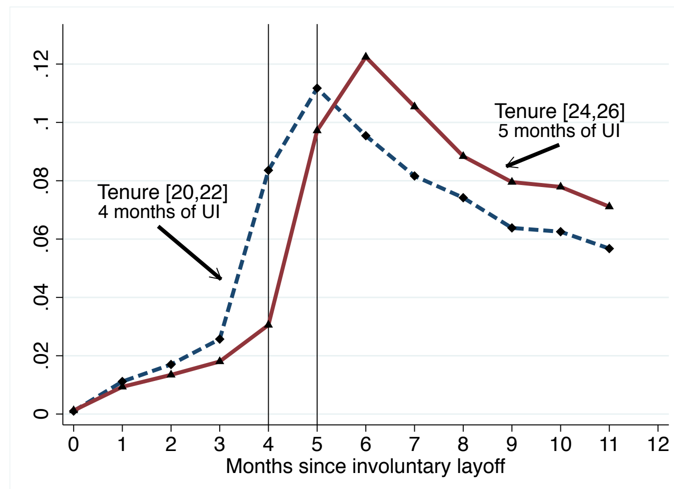
Average benefit duration increases by .9 month following a 1–month UI extension (panel a). Most of the increase is driven by the mechanical effect. The behavioral effect only amounts to .08 month (panel b).

Figure 9: Regression discontinuity design, hazard rates of formal reemployment

(a) Pernambuco (average formal employment rate: .19)




(b) Rio Grande do Sul (average formal employment rate: .34)




Full–time private–sector formal employees 18–54 years old, laid off between 2002 and 2009. Workers with more than 24 months of tenure at layoff were eligible for 5 months of UI. Workers with less than 22 months of tenure at layoff were eligible for 4 months of UI. Workers with tenure between 22 and 24 months at layoff were eligible for either 4 or 5 months of UI (see text). The sample is restricted to UI takers. The left panel displays hazard rates of formal reemployment after layoff for displaced workers from Pernambuco, a state where formal employment rates were relatively low (19%, average over 2002–2009). The right panel displays hazard rates of formal reemployment after layoff for displaced workers from Rio Grande do Sul, a state where formal employment rates were relatively high (34%, average over 2002–2009). Formal employment rates are obtained from yearly household surveys (PNAD). The solid (red) lines display hazard rates for beneficiaries eligible for 5 months of UI (tenure at layoff above 24 months). The dash (blue) lines display hazard rates for beneficiaries eligible for 4 months of UI (tenure at layoff below 22 months).

In both states, the spike in formal reemployment rates at benefit exhaustion is clearly shifted by 1 month for beneficiaries eligible for 5 months of UI. Because formal reemployment rates were higher, the mechanical effect of a 1–month UI extension was smaller and the behavioral effect larger in Rio Grande do Sul.

Figure 10: Comparing formal and overall reemployment patterns

(a) Hazard rates

(b) Survival rates


Both panels compare *formal* reemployment rates from administrative data (RAIS) and *overall* (formal and informal) reemployment rates estimated by maximum likelihood using monthly urban labor force surveys (PME, see text). The samples include displaced formal employees (full–time private–sector 18–54 years old) laid off between 2003 and 2009 in the six largest metropolitan areas of Brazil (coverage of PME surveys) and eligible for 5 months of UI (more than 24 months of tenure). Because surveys do not include information on UI takeup, the samples are not restricted to UI takers.

Overall reemployment rates are much higher than formal reemployment rates. They present no clear spike around benefit exhaustion: most of the spike (behavioral effect) must be due to behavioral responses from informally reemployed beneficiaries. We estimate that about 30% of workers remain unemployed 1 month after UI exhaustion, while 65% are not yet back to the formal sector. The difference must be made up of informally reemployed beneficiaries.

Table 1: Mechanical effect of UI extensions and formal employment rates

| | | Coefficient on formal employment rates | | | |
|---|---|---|---|---|---|
| **Outcomes** | Mean | (1) | (2) | (3) | (4) |
| UI take-up | .8601 | -.1469*** | -.2134*** | .142*** | .1355*** |
| | | (.0301) | (.0278) | (.0548) | (.0525) |
| Regular benefit duration | 4.934 | -.3091*** | -.1713*** | -.2082*** | -.1715*** |
| | | (.0555) | (.0341) | (.0689) | (.0591) |
| Mechanical effect of a | 1.667 | -.9191*** | -.5999*** | -.7211*** | -.6683*** |
| hypothetical 2–month UI extension | | (.1612) | (.1145) | (.2116) | (.2053) |
| More than 7 months | .7316 | -.4973*** | -.372*** | -.3761*** | -.3744*** |
| without formal job | | (.0923) | (.0795) | (.1198) | (.1245) |
| Observations | | 2,901,159 | 2,901,159 | 2,901,159 | 2,901,159 |
| Year fixed effects | | No | Yes | Yes | Yes |
| Mesoregion fixed effects | | No | No | Yes | Yes |
| Other controls | | No | No | No | Yes |

s.e. clustered by mesoregion (137 clusters). Significance levels: * 10%, ** 5%, ***1%. Random sample of full–time private–sector formal employees 18–54 years old, laid off between 1995 and 2009, and eligible for 5 months of UI. The table displays the coefficients from regressing various outcomes (listed in the left–hand–side column) on yearly formal employment rates by mesoregions. Outcomes, other than take–up, are conditional on take–up (take–up regressions use 3,870,398 observations). Column (2) includes year fixed effects. Column (3) includes year and mesoregion fixed effects. Column (4) adds dummies for (calendar) separation month, sector of activity, education, gender, and firm size, as well as 4th order polynomials in age, tenure and log real wage before layoff.

Our main outcome of interest is the mechanical effect of a hypothetical 2–month UI extension, the increase in benefit duration absent any behavioral response (row 3; the construction of the outcome is detailed in the text). Extending UI from 5 to 7 months would be costly in Brazil because 1.67 additional months of UI would be collected on average, absent any behavioral response. The mechanical effect is large because most beneficiaries exhaust their 5 months of UI (regular benefit duration is 4.93 on average, row 2) and because 73% of beneficiaries are still out of the formal sector 7 months after layoff (row 4). The mechanical effect decreases with formal employment rates. The relationship is not due to fixed differences across areas but holds for marginal changes in formal employment rates (columns 3). The relationship is not simply due to composition effects (column 4). The estimate in column (4) implies that increasing formal employment rates by 30 percentage points increases the mechanical effect by .2 month or 12% (and regular benefit duration by only 1%). We present many robustness checks in Table C.1.

A concern is that UI take–up is also correlated with formal employment rates (row 1), potentially creating selection issues. The negative relationship in columns (1) and (2) likely implies negative selection while the positive relationship in columns (3) and (4) likely implies positive selection (we discuss these correlations in the text). Yet such a concern is limited: our main results are consistent across specifications and are robust to the inclusion of a rich set of individual controls.

Table 2: Difference–in–difference results for the 1996 temporary UI extension

| | UI take–up | Regular UI duration | Extended UI duration | Extended UI duration compared to counterfactual | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| TreatArea × Year1996 | -.0178 | -.0003 | 1.867*** | .2469*** | .1933*** |
| | (.0153) | (.0024) | (.0137) | (.0207) | (.0174) |
| TreatArea × Year1996 × Formality rate > average | | | | | .0779** |
| | | | | | (.0328) |
| Mean (treatment area, control years) | .74 | 4.98 | 4.98 | 6.56 | 6.56 |
| Observations | 229,878 | 171,407 | 171,407 | 171,407 | 171,407 |

| | **Mechanical and behavioral effects of the UI extension** | | |
|---|---|---|---|
| | Mech. effect (month) | Beh. effect (month) | $\widetilde{\eta}$ |
| Formality rate < average | 1.641*** | .1964*** | .1197*** |
| | (.0142) | (.0179) | (.0118) |
| Formality rate > average | 1.558*** | .2726*** | .175*** |
| | (.0165) | (.0284) | (.0195) |

s.e. clustered by metropolitan area (29 clusters). Significance levels: * 10%, ** 5%, ***1%. The sample includes full–time private–sector formal employees 18–54 years old from the largest metropolitan areas of Brazil (São Paulo excluded), laid off in April or May 1995, 1996, and 1997, and eligible for 5 months of UI benefits. In 1996, in treated metropolitan areas, these workers were eligible for 7 months of UI benefits.

In the top panel, the table displays estimates of the difference–in–difference estimator for various outcomes (listed above each column). The regressions include dummies for (calendar) separation month, year, and metropolitan area. Outcomes in columns (2)–(5) are conditional on take–up. Column (1) shows that there is no treatment effect on UI take–up, a decision taken before the UI extension was announced. Column (2) shows that there is no treatment effect on regular UI duration (5 months). Because beneficiaries were already drawing 4.98 months without the UI extension, there was no room to increase regular UI duration. Column (3) shows that the UI extension increased benefit duration by 1.87 months on average. But column (4) shows that only .25 month is due to a behavioral effect (the construction of the outcome in column 4 is detailed in the text). Beneficiaries in control years would have collected 1.58 months (6.56 − 4.98) absent any behavioral response, had they been eligible for the extension. Column (5) shows that the behavioral effect is larger in metropolitan areas with higher formal employment rates. In the last regression, the dummy for above–average formal employment rates is absorbed by the metropolitan area fixed effects. The regression includes this dummy interacted with a dummy for the treatment year and interacted with the difference–in–difference indicator (reported). We present many robustness checks in Table C.4.

In the bottom panel, the table displays estimates of the behavioral and the mechanical effects. To be able to estimate the mechanical effect, we use regressions without metropolitan area fixed effects. Regressions include dummies for (calendar) separation month, treatment year, treatment area, above–average formal employment rates, and the latter dummy interacted with the dummies for treatment year and treament area. Regressions also include the difference–in–difference indicator directly and interacted with the dummy for above average formal employment rates. The mechanical effect is obtained as the linear combination of all the coefficients, except the difference–in–difference estimators, from a regression using the outcome of column (4) minus the same linear combination of coefficients from a regression using the outcome of column (2). The behavioral effect is the linear combination of the difference–in–difference estimators from a regression using the outcome of column (4). The pseudo–elasticity $\widetilde{\eta}$, the ratio of the behavioral to the mechanical effect, measures efficiency costs (s.e. are obtained by the delta method). Estimates show that the mechanical effect is large and decreases with formal employment rates, and that the behavioral effect increases with formal employment rates. The resulting efficiency costs are small because most of the increase in benefit duration is not due to behavioral responses, but the efficiency costs rise with formal employment rates.

Table 3: Who is more likely to respond to UI incentives?

| **Formally reemployed in the 2 months after regular UI exhaustion (logit)** | | | |
|---|---|---|---|
| Male | .0447*** | Firm size $\geq$ 100 | -.0231*** |
| | (.0025) | employees | (.0027) |
| Age | -.0015*** | Tenure | -.0002*** |
| | (.0001) | | (0) |
| Years of education | -.0011*** | Log real wage | -.0598*** |
| | (.0004) | | (.0071) |
| Firm size < 10 | -.0155*** | Replacement rate | -.2023*** |
| employees | (.0028) | | (.022) |

| **Mechanical and behavioral effects by quartile of the predicted propensity to be formally reemployed in the 2 months after regular UI exhaustion** | | | |
|---|---|---|---|
| | Mech. effect (month) | Beh. effect (month) | $\widetilde{\eta}$ |
| First quartile: | 1.731*** | .1591*** | .0919*** |
| | (.0164) | (.0171) | (.0106) |
| Second quartile: | 1.644*** | .2208*** | .1343*** |
| | (.0226) | (.0238) | (.016) |
| Third quartile: | 1.584*** | .2518*** | .159*** |
| | (.0205) | (.0271) | (.0185) |
| Fourth quartile: | 1.479*** | .3365*** | .2275*** |
| | (.035) | (.0408) | (.0316) |

Significance levels: * 10%, ** 5%, ***1%. The sample includes full–time private–sector formal employees 18–54 years old from the largest metropolitan areas of Brazil (São Paulo excluded), laid off in April or May 1995, 1996, and 1997, and eligible for 5 months of UI benefits. In 1996, in treated metropolitan areas, these workers were eligible for 7 months of UI benefits.

In the top panel, the table displays marginal effects at the mean (robust s.e. in parenthesis) from regressing a dummy for being formally reemployed rapidly after regular UI exhaustion (spike) on available beneficiaries' characteristics (logistic regressions). Observations from treatment areas in 1996 are excluded. The table highlights some of the relevant heterogeneity.

This information is used in the bottom panel in two steps. First, we perform the same logistic regression on a more flexible function of the same covariates, including fixed effects by year, (calendar) separation month, metropolitan area (29), education (9), sector of activity (50), gender, and firm size (9), as well as 4th order polynomials in tenure, age and log real wage before layoff (not shown). Then, we perform the difference–in–difference estimation separately for each quartile of the predicted propensity. In the bottom panel, the table displays the resulting estimates of the behavioral effects, the mechanical effects, and their ratio ($\widetilde{\eta}$), which measure efficiency costs (Table 2 describes how we obtain these estimates). Standard errors are clustered by metropolitan areas and are obtained by bootstrapping of the two–stage procedure (1000 replications). The results show that the government could use readily available information to identify workers more likely to respond to UI incentives, and target job–search monitoring accordingly. Nevertheless, the maximum price it should be willing to pay for a perfect monitoring technology ($\widetilde{\eta}b$, see text) remains low ($.23b$). Most of the heterogeneity is not easily captured by observable characteristics ($\widetilde{\eta}$ would tend to infinity for a group of workers always formally reemployed right after UI exhaustion).

Table 4: Validity of the regression discontinuity design

| | Mean | Coefficient on cutoff indicator | |
|---|---|---|---|
| | $20 \leq Tenure < 22$ | (1) | (2) |
| Observations per bin | 44972 | 679.4 | 1328 |
| | | (5236) | (7894) |
| Male | .5947 | .0065 | .0003 |
| | | (.0052) | (.0077) |
| Age | 29.62 | .067 | -.0063 |
| | | (.0518) | (.065) |
| Years of education | 8.522 | -.0347* | -.0229 |
| | | (.0208) | (.028) |
| Log real wage | 6.608 | .0359 | .0652 |
| | | (.033) | (.05) |
| Replacement rate | .7066 | -.0107 | -.0214 |
| | | (.0106) | (.0157) |
| Commerce | .3654 | .004 | -.0022 |
| | | (.0074) | (.01) |
| Services | .3362 | -.0047 | .0036 |
| | | (.0036) | (.0052) |
| Industry | .2399 | .0002 | .0007 |
| | | (.0067) | (.0098) |
| Firm size $\geq$ 100 employees | .2071 | -.0046 | .0095 |
| | | (.0147) | (.0222) |
| Firm size $<$ 10 employees | .43 | .0026 | -.0119 |
| | | (.0218) | (.034) |
| Formal jobs per inhabitants in mesoregion | .2097 | -.0005 | -.0002 |
| | | (.0013) | (.002) |
| Observations | | 3,065,724 | 1,648,581 |
| Tenure window (months) | | 15–36 | 18–30 |

s.e. clustered by week of tenure. Significance levels: * 10%, ** 5%, ***1%. Full–time private–sector formal employees 18–54 years old, laid off between 1997 and 2009. Those with less than 22 months of tenure at layoff were eligible for 4 months of UI. Those with more than 24 months of tenure at layoff were eligible for 5 months of UI (extension). Those with tenure between 22 and 24 months were eligible for 4 or 5 months of UI (see text) and are excluded from the regressions.

The table displays the coefficients from regressing various workers' characteristics (listed in the left–hand column) on a dummy for having more than 24 months of tenure at layoff (tenure cutoff). The outcome in the first row is the number of observations by tenure bin to test for the smoothness of the tenure distribution at layoff (forcing variable). Column (1) uses observations with tenure at layoff between 15 and 36 months. Column (2) uses a smaller tenure window around the cutoff (18 to 30 months). The regressions include fixed effects for (calendar) separation and hiring months and linear controls in tenure on each side of the cutoff. We find no evidence of a discontinuous change in the value of the covariates or the number of observations at the tenure cutoff. The only (marginally) significant coefficient is economically insignificant (.03 years of education). These estimates support the validity of the regression discontinuity design. Figure C.6 in the Appendix graphically confirms our results.

Table 5: Overall regression discontinuity results

| | UI take–up | UI duration up to 4 months | Extended UI duration | Extended UI duration compared to counterfactual |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Tenure≥24 months | .0312 | .0055*** | .9091*** | .0821*** |
| | (.0226) | (.0013) | (.0043) | (.0037) |
| Mean 2002–2009 | .74 | 3.96 | 4 | 4.77 |
| ($20 \leq Tenure < 22$) | | | | |
| Observations | 3,065,724 | 2,302,058 | 2,302,058 | 2,302,058 |

s.e. clustered by week of tenure. Significance levels: * 10%, ** 5%, ***1%. Full–time private–sector formal employees 18–54 years old, laid off between 1997 and 2009. Those with less than 22 months of tenure at layoff were eligible for 4 months of UI. Those with more than 24 months of tenure at layoff were eligible for 5 months of UI (extension). Those with tenure between 22 and 24 months were eligible for 4 or 5 months of UI (see text) and are excluded from the regressions.

The table displays the coefficients from regressing various outcomes (listed above each column) on a dummy for having more than 24 months of tenure at layoff (tenure cutoff). The sample includes observations with tenure at layoff between 15 and 36 months. The regressions include fixed effects for (calendar) separation and hiring months and linear controls in tenure on each side of the cutoff. Outcomes in columns (2)–(4) are conditional on take–up. Column (1) finds no significant effect on UI take–up. Column (3) shows that the 1–month UI extension increased benefit duration by .91 month on average at the cutoff. But column (4) shows that only .08 month is due to a behavioral effect (the contruction of the outcome in column 4 is detailed in the text). Beneficiaries with tenure levels below the cutoff would have collected .81 month (4.77-3.96), had they been eligible for the extension. Column (2) suggests that there was some very limited behavioral response in the first 4 months of benefit collection. We present many robustness checks in Table C.5.

Table 6: Regression discontinuity results and formal employment rates

| | Extended UI duration compared to counterfactual | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Tenure≥24 months | .0819*** | .0833*** | .0832*** | .0831*** |
| | (.0033) | (.0033) | (.0032) | (.0026) |
| Formal employment rate in mesoregion | -.5619*** | -.3363*** | -.4198*** | -.3883*** |
| | (.0188) | (.017) | (.0248) | (.0242) |
| Tenure≥24 months * Formal employment rate | .1142*** | .1102*** | .1191*** | .1207*** |
| | (.0221) | (.0209) | (.0207) | (.0198) |
| Observations | 2,302,058 | 2,302,058 | 2,302,058 | 2,302,058 |
| Year fixed effects | No | Yes | Yes | Yes |
| Mesoregion fixed effects | No | No | Yes | Yes |
| Other controls | No | No | No | Yes |

| | **Mechanical and behavioral effects of the UI extension** | | |
|---|---|---|---|
| | Mech. effect (month) | Beh. effect (month) | $\widetilde{\eta}$ |
| Formal employment rate = $mean - .15$ | .8705*** | .065*** | .0747*** |
| | (.0037) | (.0037) | (.0043) |
| Formal employment rate = $mean$ | .8269*** | .0831*** | .1005*** |
| | (.0018) | (.0026) | (.0033) |
| Formal employment rate = $mean + .15$ | .7833*** | .1012*** | .1292*** |
| | (.0036) | (.0042) | (.0057) |

s.e. clustered by week of tenure. Significance levels: * 10%, ** 5%, ***1%. Full–time private–sector formal employees 18–54 years old, laid off between 1997 and 2009. Those with less than 22 months of tenure at layoff were eligible for 4 months of UI. Those with more than 24 months of tenure at layoff were eligible for 5 months of UI (extension). Those with tenure between 22 and 24 months were eligible for 4 or 5 months of UI (see text) and are excluded from the regressions. The sample includes observations with tenure at layoff between 15 and 36 months.

In the top panel, coefficients in the first row capture the average behavioral effect at the cutoff of a 1–month UI extension. Coefficients in the second row capture how the mechanical effect of a 1–month UI extension varies with formal employment rates. Coefficients in the third row capture how the behavioral effect varies with formal employment rates. The regressions include fixed effects for (calendar) separation and hiring months, linear controls in tenure on each side of the discontinuity, and these controls interacted with formal employment rates (the construction of the outcome is described in the text). Column 2 includes year fixed effects. Column 3 includes year and mesoregion fixed effects. Column 4 adds dummies for sector of activity, gender, education, and firm size, as well as a 4th order polynomials in age and log real wage before layoff. Across specifications, the average behavioral effect is around .08 month, the mechanical effect is decreasing and the behavioral effect increasing with formal employment rates. These relationships are not due to fixed differences across areas but hold for marginal changes in formal employment rates (column 3). These relationships are not simply due to composition effects (column 4). We present many robustness checks in Table C.6.

The bottom panel uses the specification in column (4) and provides estimates of the behavioral effect and the mechanical effect at different levels of formal employment rates. Estimates are obtained following a similar procedure as in Table 2. The pseudo–elasticity $\widetilde{\eta}$, the ratio of the behavioral to the mechanical effect, measures efficiency costs (s.e. are obtained by the delta method). Estimates show that the mechanical effect is large and decreases with formal employment rates, and that the behavioral effect increases with formal employment rates. The resulting efficiency costs are small because most of the increase in benefit duration is not due to behavioral responses, but the efficiency costs rise with formal employment rates.

Table 7: Disposable income by labor status

| | Northeast | South/Southeast |
|---|---|---|
| Formal employment rates | .279 | .364 |
| Share unemployed if UI exhaustee | .478 | .434 |
| **Disposable Income (in R$ of 2000)** | | |
| Formal employees | 342 | 514 |
| Formal employees before layoff | 248 | 362 |
| Formally reemployed in first months after layoff | 244 | 330 |
| Informally reemployed in first months after layoff | 159 | 237 |
| Unemployed around UI exhaustion | 114[a] | 177[b] |

Data from monthly urban labor force surveys covering the 6 largest metropolitan areas of Brazil (PME, 2003–2010). Sample restricted to full–time private–sector formal employees 18–54 years old with more than 24 months of tenure or to similar workers who lost their formal job with more than 24 months of tenure at layoff. These workers were eligible for 5 months of UI. Northeastern metropolitan areas include Recife and Salvador; Southern and Southeastern metropolitan areas include Belo Horizonte, Rio de Janeiro, São Paulo, and Porto Alegre. Disposable income is measured as household income per capita per month with an equivalence scale of 1/2 for children. Minimum wage in 2000: R$200 per month.

Northeastern metropolitan areas had lower formal employment rates (row 1). We estimate (through maximum likelihood, see text) that the share of unemployed UI exhaustees was similar or even higher in the Northeast (row 2). Disposable income was systematically lower for unemployed UI exhaustees or the informally reemployed in the first months after layoff (while eligible for UI). Disposable income were also systematically lower in the Northeast. The gap in disposable income between unemployed UI exhaustees and formal employees before layoff was similar, however ($\frac{114}{248}$ and $\frac{177}{362}$). We only observe disposable income for the informally reemployed upon reemployment. Assuming that they have similar disposable income around UI exhaustion, the gap in disposable income between informally reemployed UI exhaustees and formal employees before layoff was also similar ($\frac{159}{248}$ and $\frac{237}{362}$). [a] 37% have no disposable income. [b] 30% have no disposable income.

Table 8: Calibrated welfare gains from a marginal UI extension

| | | Welfare effects | |
|---|---|---|---|
| Risk aversion + redistribution ($\gamma$) | Social value of insurance | low formal employment rates | high formal employment rates |
| .25 | .1 | **-.02** | **-.09** |
| .42 | .175 | **.08** | **0** |
| .75 | .31 | **.26** | **.18** |
| 1 | .42 | **.4** | **.31** |
| 2 | .84 | **.96** | **.84** |
| 3.57 | 1.5 | **1.84** | **1.68** |

The table displays welfare effects of a marginal UI extension (in bold) obtained from evaluating equation (11). Welfare effects are measured in terms of an equivalent percentage change in the total payroll of eligible formal employees. The *social value of insurance* ($\frac{g^{U_{P+1}}-g^E}{g^E}$) captures the relative social value of \$1 for the average UI exhaustee (recipient) compared to the average formal employee before layoff (taxpayer). A value of 1.5 means that the social value of \$1 is 150% larger for UI exhaustees. A UI extension increases welfare if the social value of insurance exceeds the pseudo–elasticity $\widetilde{\eta}$ measuring efficiency costs. We use estimates of efficiency costs from Table 2 (low formal=.12, high formal=.175). The social value of insurance is calibrated using the decompositions in equations (12)–(13) and disposable income ratios from Table 7 for different values of $\gamma$ (see text). $\gamma$ captures both an average coefficient of relative risk aversion and social planner preferences towards redistribution. The social value of insurance is high if UI exhaustees have relatively little disposable income or if $\gamma$ is high. We use the same social value of insurance in labor markets with different formal employment rates because we do not find evidence of differential disposable income ratios in Table 7.

For a given value of $\gamma$, the table displays the corresponding social value of insurance and the resulting welfare effects in labor markets with relatively high and relatively low formal employment rates. Alternatively, without relying on our calibration, the table displays the welfare effects for a given social value of insurance.

Because efficiency costs are small, welfare effects are positive unless the social value of insurance is very low. Welfare effects are positive as long as the social value of \$1 is 17.5% higher for UI exhaustees than for the formally employed. For high values of $\gamma$ (or high social value of insurance), welfare effects may be sizeable. For $\gamma = 1$ (social value of \$1 is 42% higher for UI exhaustees), extending UI benefits by 1 month has a similar effect on welfare as increasing wages of eligible formal employees by .31%–.4%. Welfare effects decrease with formal employment rates (a difference of 29% for $\gamma = 1$) because of increased efficiency costs.

# A Appendix: Background

## A.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.

Workers involuntarily displaced from a private formal job with at least 6 months of tenure at layoff are eligible for 3 to 5 monthly UI payments. Maximum benefit duration depends on the number of months of formal employment in the 36 months prior to layoff, $T_{36}$—3 months of UI if $T_{36} \in [6, 12)$, 4 months of UI if $T_{36} \in [12, 24)$ and 5 months of UI if $T_{36} \geq 24$. There is a 30–day waiting period before a first UI payment can be collected. The replacement rate is constant until exhaustion of benefits and is means–tested, starting at 100% at the bottom of the wage distribution. Benefits can be used discontinuously over a period of 16 months after which a worker is again eligible for the full maximum benefit duration.

## A.2 Brazilian labor legislation

The Brazilian labor code (Consolidação das Leis do Trabalho - CLT) was created in 1943. Two major revisions were implemented since then: i) in 1964, when the military regime restricted the power of labor unions; and ii) 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 (see Gonzaga, 2003 for a full description). 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 one 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.

## A.3 Job protection institutions

Despite having a very restrictive labor legislation, job and worker turnover rates are very high when compared to other countries. 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; World Bank, 2002; Gonzaga, 2003).

*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 are adjusted monthly but real rates of return are negative. Employees can usually only access the account upon layoff or retirement. In the case of layoff, employers currently must pay a fine equivalent to 50% of the amount deposited during the worker's tenure at the firm (40% is paid to the worker and, since 2001, 10% is paid to the government).

*Advance notice of layoff.* The other important component of job security legislation in Brazil is advance notification. The first 3 months of employment are considered a probationary period in Brazil. Employers laying off workers with more than 3 months of tenure must provide a worker with a 1–month advance notice.[53] During this month, wages cannot be reduced and employers must allow a worker up to 2 hours a day to look for a new job.

*Mediation meeting.* Any layoff of workers with more than 12 months of tenure must be signed by a representative of the Labor Ministry (or the unions) who verifies that workers received all payments they were entitled to. This increases oversight of the layoff process and constitutes a significant administrative burden, as officials are unable to visit every worksite each month.

# B Appendix: A model of job–search with informality

We develop a model of endogenous job–search with informal work opportunities to highlight the tradeoff between insurance and efficiency faced by a social planner deciding on the maximum UI benefit duration. 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 only on workers who do not lose their formal job (Chetty, 2006; Kroft, 2008). We later show how the intuition carries on to an infinite horizon model. The measure of efficiency cost and the welfare formula we derive are robust to relaxing many assumptions of the model (e.g. introducing heterogeneity) or to adding other margins of endogenous behaviors, as long as an envelope condition applies to the agents' problem (Chetty, 2006).

*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 $\tau w^f$ each period. Their per–period utility is $u\left(w^f\left(1-\tau\right)\right)$. $u(.)$ is assumed to be strictly concave.[54] In this setup, the average number of contribution periods to the UI system for a given layoff ($D^f$ in Section 3)

[53] Since 2011, workers have been entitled to an advance notice that increases from 1 to 3 months depending on seniority.

[54] Allowing for different utility functions in different labor statuses does not affect our main conclusions.

is thus $\frac{[(1-q)T]}{q}$. Upon layoff, workers become unemployed and eligible for UI for $P$ periods. UI benefits $b_t$ are defined as $b_t \equiv r_t w^f$, with replacement rate $r_t = r$ for period $t = 1...P$ after layoff, and $r_t = 0$ otherwise.

While unemployed, a worker decides each period how much effort $e$ at a cost $z(e)$ 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 does not find a job and stays unemployed. With probability $e$, she finds a job. She can increase her probability of returning to a formal job by investing formal search effort $f$ at a cost $\theta z(f)$. She thus finds a formal job with probability $ef$ and an informal job with probability $e(1-f)$. Working informally, she earns wage $w^i < w^f$. She can always search for a formal job at the same cost $\theta z(f)$ in subsequent periods. To introduce enforcement in the model, we further assume that informal jobs are detected by the government with probability $p$. If detected, an informal worker falls back into unemployment and loses her UI benefits. Both the unemployed and the "undetected" informally employed draw UI benefits $b$ in the first $P$ periods after layoff. The unemployed have a minimum consumption level $o$. The traditional view of informality implies high values of $\theta$ (high formal search costs). The more recent view corresponds to low values of $\theta$ and small wage differentials. In many developing countries, detection probabilities $p$ are low. When investigating the social planner problem below, we thus abstract from this and set $p = 0$.

The value function of being unemployed at the start of a period $J_t^o$ solves:

$$J_t^o = \max_{e_t} (1-e_t) U_t + e_t J_t^i - z(e_t)$$

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

$$J_t^i = \max_{f_t} (1-f_t) Z_t + f_t V_t - \theta z(f_t)$$

$V$, $Z$ and $U$ are respectively the value function of being formally employed, informally employed or unemployed in a given period (after job search has occurred). We have:

$$\begin{aligned}
V_t &= u\left(w^f\right) + V_{t+1} \\
Z_t &= (1-p)\left[u\left(w^i + b_t\right) + J_{t+1}^i\right] + p\left[u(o) + J_{t+1}^o\right] \\
U_t &= u(o + b_t) + J_{t+1}^o
\end{aligned}$$

where $b_t = b$ for $t = 1...P$ and $b_t = 0$ otherwise ($o$ is a minimum subsistence level).[55]

The workers' problem is to maximize $J_1^o$ by choosing optimal levels of search intensity of both

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

types in each period until formal reemployment. At an optimum, we have:

$$V_t - Z_t = \theta z'(f_t)$$
$$J_t^i - U_t = z'(e_t)$$

Define $O_t$ and $I_t$ as the share of displaced formal employees unemployed and informally reemployed at the end of period $t$, with $O_0 = 1$ and $I_0 = 0$. The hazard of formal reemployment in a given period is $O_{t-1}e_t f_t + I_{t-1}f_t$. The solution to this dynamic problem determines the survival rate out of formal employment and therefore the average UI benefit duration, $B$.

To illustrate the mechanisms discussed in the paper, we obtain the following comparative statics for one-period changes in the parameters, assuming $O_{t-1}$ and $I_{t-1}$ fixed.[56] The behavioral effect is obtained by the derivative of the search efforts with respect to $b_t$ $(O_{t-1}\frac{de_t f_t}{db_t} + I_{t-1}\frac{df_t}{db_t})$. The change in the behavioral effect following a change in a parameter $\kappa$ is obtained by the derivate of this behavioral effect with respect to the parameter $(O_{t-1}\frac{d^2 e_t f_t}{db_t d\kappa} + I_{t-1}\frac{d^2 f_t}{db_t d\kappa})$. The change in the mechanical effect following a change in a parameter is obtained by the derivative of the search efforts with respect to the parameter $(O_{t-1}\frac{de_t f_t}{d\kappa} + I_{t-1}\frac{df_t}{d\kappa})$.

The hazard of formal reemployment decreases with an increase in UI benefits (behavioral effect):

$$\frac{df_t}{db_t} < 0, \quad \frac{de_t}{db_t} < 0$$

The hazard of formal reemployment increases when formal search costs decrease (mechanical effect↓); the impact of an increase in UI benefits is exacerbated when formal search costs decrease (behavioral effect↑)

$$\frac{df_t}{d\theta} < 0, \quad \frac{de_t}{d\theta} < 0, \frac{d^2 f_t}{db_t d\theta} > 0, \frac{d^2 e_t}{db_t d\theta} > 0$$

The hazard of formal reemployment increases when formal wages increase (mechanical effect↓); the impact of an increase in UI benefits is exacerbated when formal wages increase relatively (behavioral effect↑)

$$\frac{df_t}{dw^f} > 0, \quad \frac{de_t}{dw^f} > 0, \frac{d^2 f_t}{db_t dw^f} = 0, \frac{d^2 e_t}{db_t dw^f} < 0$$

The impact of an increase in the detection probability $p$ on the hazard of formal reemployment is ambiguous: it discourages overall search but encourages formal search conditional on searching for a formal job. Likewise, the impact of an increase in UI benefits on overall search effort is exacerbated

[56] There is very little room for anticipation behaviors to matter, so the assumption is not restrictive for the Brazilian case. The impact of multi-period changes in the parameters includes cross–period effects whose signs will depend more heavily on functional form assumptions.

but the impact on formal search effort is reduced.

$$\frac{df_t}{dp} > 0, \quad \frac{de_t}{dp} < 0, \frac{d^2 f_t}{db_t dp} > 0, \frac{d^2 e_t}{db_t dp} < 0$$

*Social planner's problem.* Following Schmieder et al. (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 $b$.

To derive a welfare formula, we follow Saez (2002) and assume that there are $M$ types of individuals in our population indexed by $m = 1, ..., M$, in proportion $h_m$, whose utilities enter the social welfare function with weight $\mu_m$. Define $S_t$, the average survival rate out of formal employment in period t.

$$S_t = \int_m S_{m,t} h_m dm = \int_m \left[O_{m,t} + I_{m,t}\right] h_m dm$$

We have $B = \sum_{t=0}^{P} S_t$, the average benefit duration. The problem of the social planner is to choose the maximum benefit duration $P$ that maximizes the social welfare function such that a balanced–budget constraint holds:

$$\begin{aligned} &\max_P W = q \int_m \mu_m J_{1,m}^o h_m dm + (1-q)T \int_m \mu_m u_m \left(w^f \left(1-\tau\right)\right) h_m dm \\ &\text{s.t. } \tau = rB \frac{q}{[(1-q)T]} \end{aligned}$$

The mechanical and behavioral effects of a marginal UI extension are then:

$$\begin{aligned} Mechanical &= S_{P+1} \\ Behavioral &= \sum_{t=0}^{P+1} \frac{dS_t}{dP} \\ \frac{dB}{dP} &= Mechanical + Behavioral \end{aligned}$$

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

$$\begin{aligned} \frac{dW}{dP} &= q \ b \ S_{P+1} \ g^{U_{P+1}} - T\left(1-q\right) w^f \ \frac{d\tau}{dP} \ g^E \\ \frac{dW}{dP} &= q \ r \ w^f \ \ S_{P+1} \ g^{U_{P+1}} - q \ r \ w^f \ \frac{dB}{dP} \ g^E \\ \frac{dW/dP}{w^f g^E} &= q \ r \ S_{P+1} \ \left[\frac{g^{U_{P+1}} - g^E}{g^E} - \widetilde{\eta}\right] \end{aligned}$$

where $\widetilde{\eta} = \sum_{t=0}^{P+1} \frac{dS_t}{dP} / S_{P+1}$. $g^{U_{P+1}}$ and $g^E$ are the social value of \$1 for the average UI exhaustee and the average UI contributors, respectively.

$$g^{U_{P+1}} = \frac{1}{S_{P+1}} \int_m \mu_m \left[ O_{m,P+1} u'_m \left( o + b_{P+1} \right) + \left( I_{m,P+1} \right) u'_m \left( w^i + b_{P+1} \right) \right] h_m dm$$
$$g^E = \int_m \mu_m u'_m \left( w^f \left( 1 - \tau \right) \right) h_m dm$$

*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 3. Denote $\omega_t$ as the agent's labor status in period $t$ and $n_{\omega_t}$ the probability that the agent is in labor status $\omega$ in period $t$. Because UI benefits are limited in time and the agent can work in both the formal and informal sectors, there are many possible labor statuses: (i) formally employed, (ii) informally employed without UI benefits, (iii) informally employed with UI benefits in period h=1,2,...,P since layoff from the formal sector, (iv) unemployed without UI benefits, (v) unemployed with UI benefits in period h=1,2,...,P since layoff from the formal sector. In each labor status, the agent consumes $c_\omega$ and invests search efforts $e_\omega$ (0 if employed) and $f_\omega$ (0 if formally employed). The search efforts and the layoff probability determine the transition matrix between labor statuses from one period ($\omega_{t-1}$) to the next ($\omega_t$) given the model in Section 3. Taking the UI program $\{b, P, \tau\}$ as given, the agent chooses search efforts to maximize the expected utility:

$$\mathbb{E}_0 \sum_{t=0}^{+\infty} \delta^t \left\{ \sum_{\omega_t} n_{\omega_t} u \left( c_{\omega_t} \right) - \sum_{\omega_{t-1}} n_{\omega_{t-1}} \left[ z \left( e_{\omega_{t-1}} \right) + \theta z \left( f_{\omega_{t-1}} \right) \right] \right\} \tag{15}$$

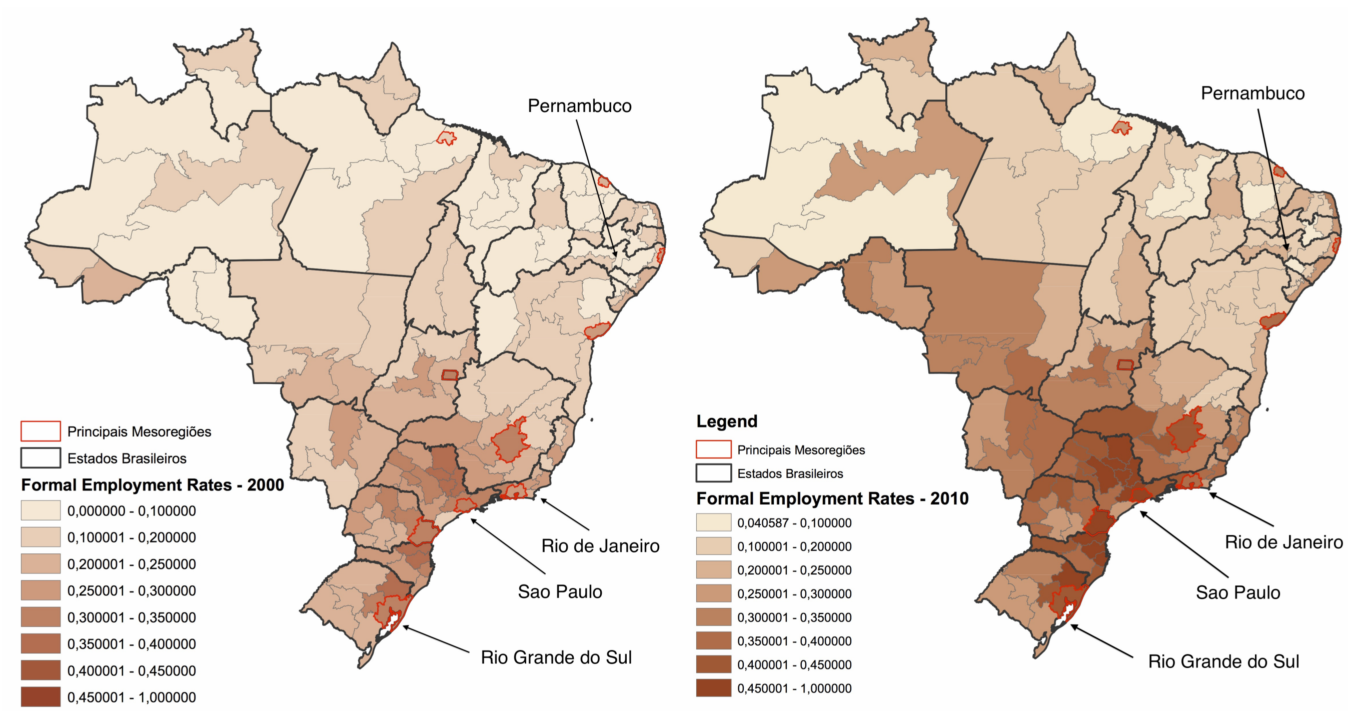
where $\delta < 1$ is the discount factor and $\mathbb{E}_0$ is the mathematical expectation given the agent's information in period 0.

In the steady state of this dynamic model, all variables are constant ($n_\omega$, $c_\omega$, $e_\omega$, $f_\omega$) and determine $D^f$, $D^u$, and $B$, the average length of a formal employment spell, of a spell out of formal employment, and of a benefit collection spell, respectively. Given UI benefits $b$, the planner's problem in 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 can assume that there are $M$ types of individuals as above to introduce preferences for redistribution beyond the insurance motive.

# C Appendix: Figures and Tables

Figure C.1: Geographical distribution and evolution of formal employment rates in Brazil

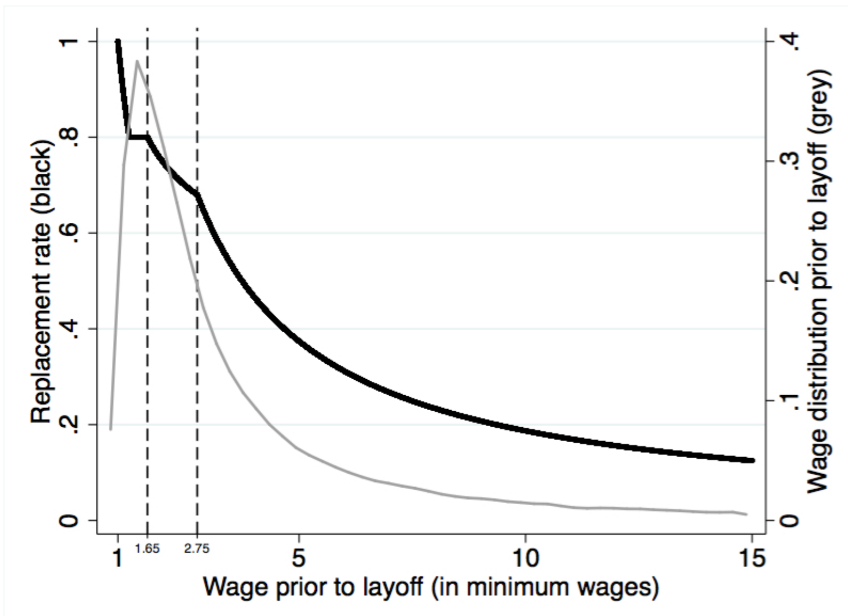
(a) Formal employment rates, 2000 census (b) Formal employment rates, 2010 census




The maps display the variation in formal employment rates (shades of color) across space in Brazil, based on the 2000 census (panel a) and the 2010 census (panel b). The darker lines identify state boundaries. The thinner lines identify mesoregion boundaries, the next geographical subdivisions in Brazil. Mesoregions that include one of the 9 historical metropolitan areas of the country or the Federal District are highlighted.

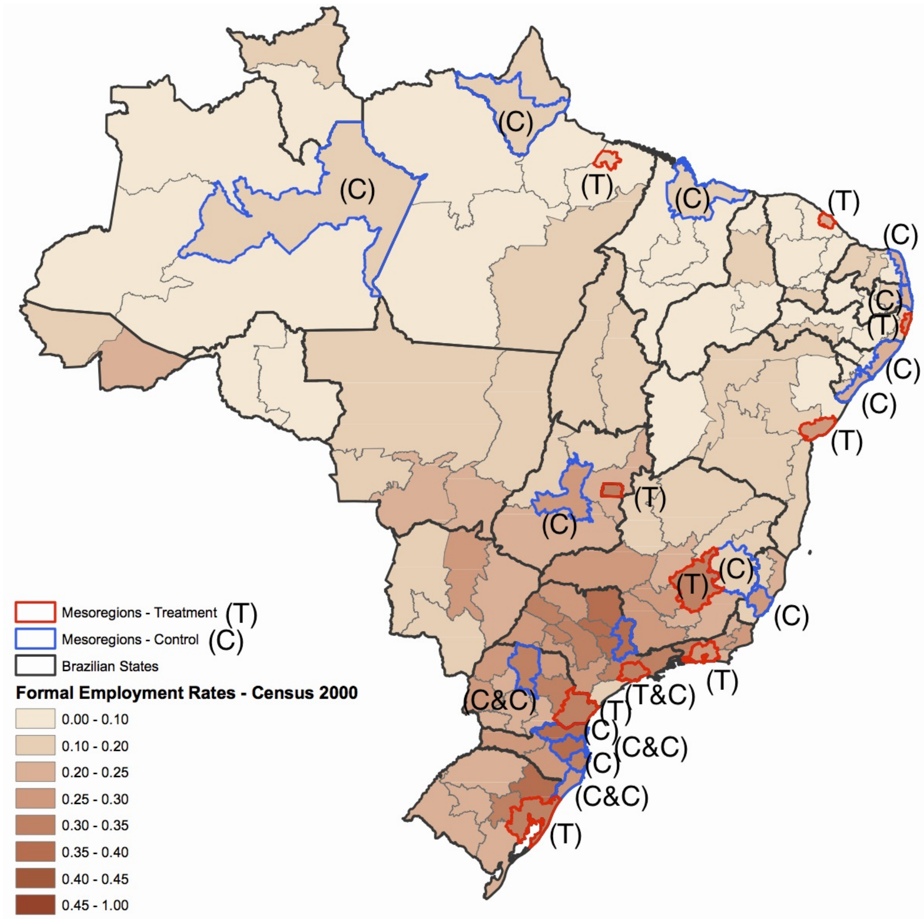
The maps show that there is tremendous variation in formal employment rates across states in Brazil. The North and the Northeast are poorer and less formal. There is also variation within state, however. Brazil experienced rapid economic growth in the last decade. Formal employment rates increased across the country (darker shades of color on panel b) but not uniformly so.

Figure C.2: Replacement rate in the Brazilian UI program




The black line displays the replacement rate of UI benefits as a function of the wage in the lost job (expressed in minimum wages). The grey line displays the density of the wage distribution at layoff. UI benefits cannot be inferior to the minimum wage. Since 1994, replacement rates depend on the wage (in multiples of minimum wage) prior to layoff $w$ 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 $w \geq 2.75$. The kernel density corresponds to the wage distribution at layoff for a random sample of 10,000 displaced formal employees (eligible for 5 months of UI) in each year between 1995 and 2009.

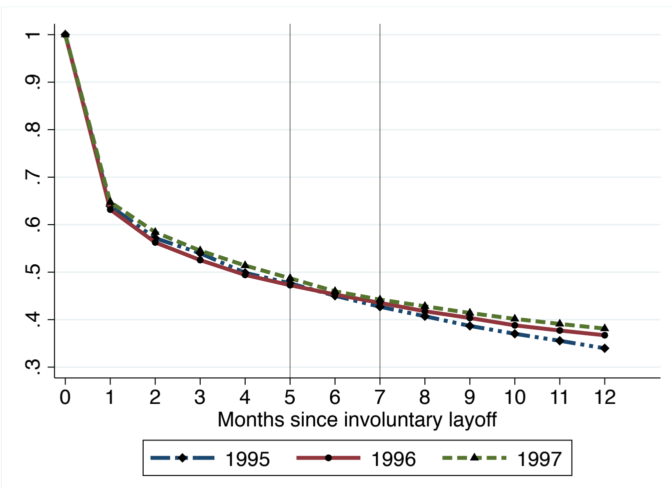
Figure C.3: Geographical distribution of control and treatment areas for the 1996 temporary UI extension



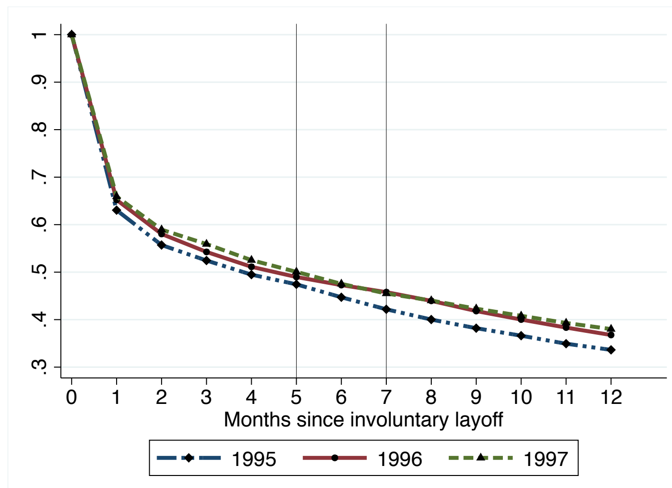

Highlighted in red and blue are mesoregions that include treatment (T) and control (C) metropolitan areas for the 1996 temporary UI extension. Treatment and control areas are similarly spread over the country and span a similar range of formal employment rates.

Figure C.4: Test of the common trend assumption for the 1996 temporary UI extension

(a) Survival out of formal employment for UI non–takers, control



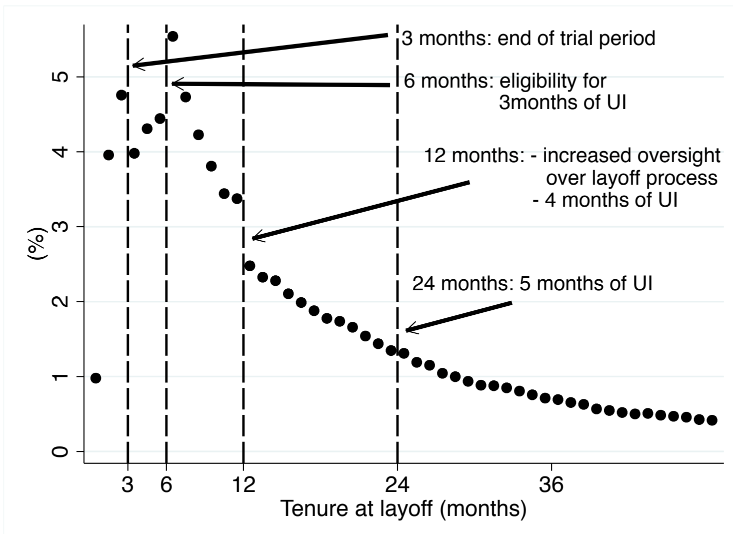

(b) Survival out of formal employment for UI non–takers, treatment




Full–time private–sector formal employees 18–54 years old from the largest metropolitan areas of Brazil (São Paulo excluded), laid off in April or May 1995, 1996, and 1997, and eligible for 5 months of UI benefits. The sample includes only workers who did not take up UI benefits. These workers should not have been affected by the 1996 temporary UI extension (the take–up decision was taken before the UI extension was announced). The left panel displays survival rates out of formal employment after layoff in each year for displaced workers from control metropolitan areas. The right panel displays survival rates out of formal employment after layoff in each year for displaced workers from treatment metropolitan areas.

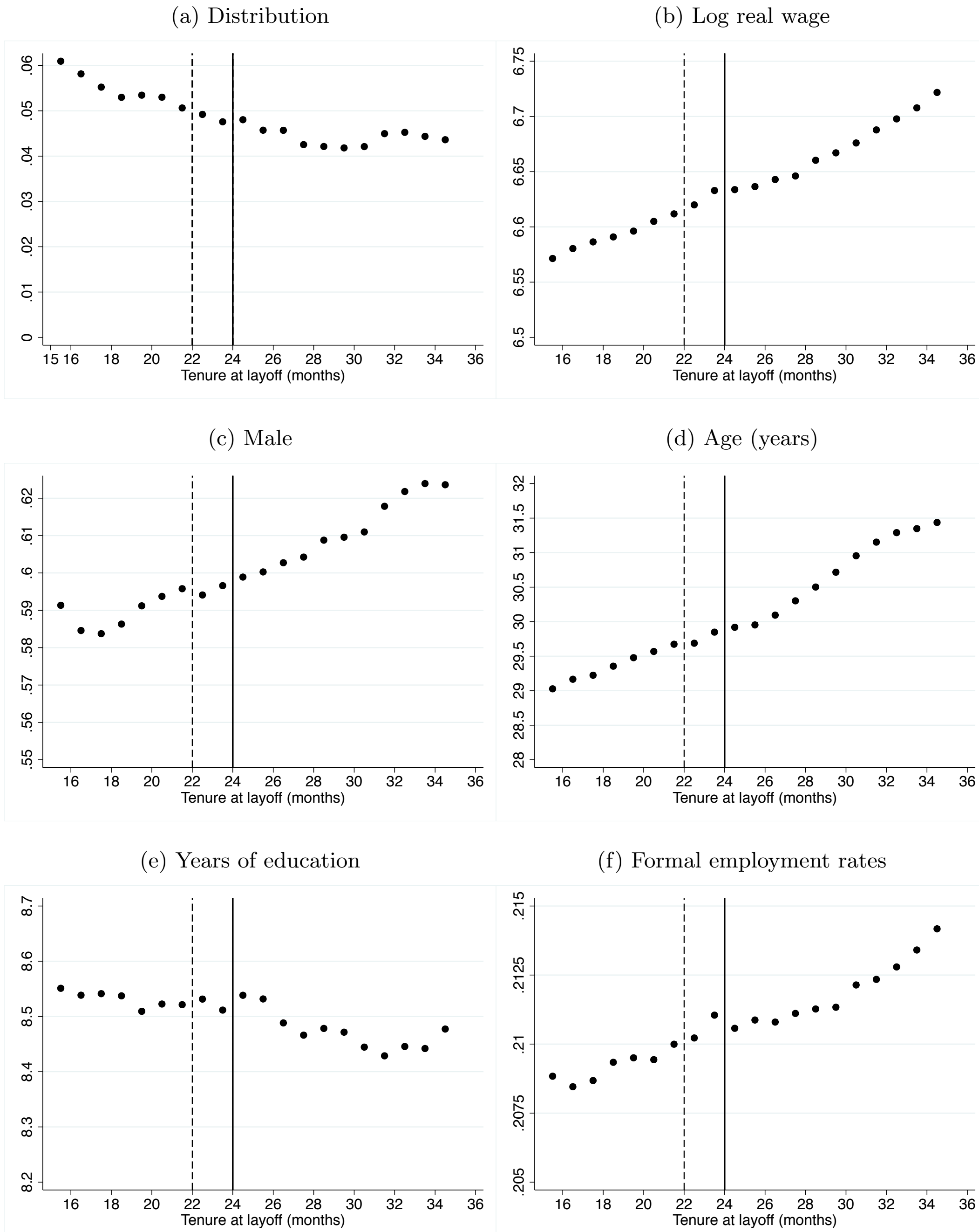
We find no sign of differential trends in treatment areas in treatment year. This supports our identifying assumption that trends would have been similar for UI beneficiaries in the absence of the UI extension.

Figure C.5: Tenure distribution at layoff and tenure–based discontinuities in UI eligibility




Private–sector formal employees with more than 6 months, 12 months, and 24 months of tenure at layoff are eligible for 3 months, 4 months, and 5 months of UI respectively (if they had no other job in the previous 3 years). These tenure–based discontinuities in eligibility provide potential regression discontinuity designs. The tenure density, however, is not continuously distributed across the first two relevant tenure cutoffs. The upward jump in the density at 6 months may be due to the absence of experience rating of UI benefits in Brazil. The discontinuity at 12 months cannot be due to the increase in UI benefits a worker is now eligible for. Indeed, the layoff density jumps downward. In Brazil, firing costs are discontinuously increased at 3 months of tenure (end of probationary period) and 12 months of tenure (administrative burden and oversight of the layoff process). Firms clearly react to changes in firing costs by adjusting their layoff decisions. The tenure density at layoff is continuous beyond one year of tenure, in particular around the last relevant tenure cutoff (24 months). The higher firing costs that firms are facing at those tenure levels, the higher value of such jobs for workers, and the additional scrutiny over the layoff process appear sufficient to prevent responses at the layoff margin, even in the absence of experience rating.

Figure C.6: Validity of the regression discontinuity design




Full–time private–sector formal employees 18–54 years old, laid off between 1997 and 2009. Workers with more than 24 months of tenure at layoff were eligible for 5 months of UI. Workers with less than 22 months of tenure at layoff were eligible for 4 months of UI. Workers with tenure between 22 and 24 months at layoff were eligible for either 4 or 5 months of UI (see text). Outcomes are averaged out by month of tenure. Panel (a) plots the share of observations by tenure bin. The other panels plot (b) the logarithm of the real wage in the lost job, (c) the share male, (d) age, (e) years of education, and (f) formal employment rates in the mesoregion in the year of layoff.

There is no discontinuity in the tenure distribution at layoff at the 24–month cutoff. There is also no clear discontinuity in the value of covariates at the 24–month tenure cutoff. This visually confirms regression results in Table 4.

Table C.1: Robustness of the relationship between the mechanical effect of UI extensions and formal employment rates

| | | Coefficient on formal employment rates | | | |
|---|---|---|---|---|---|
| **Outcomes** | Mean | (1) | (2) | (3) | (4) |
| | | **Using state–level formal employment rates** | | | |
| UI take-up | .8622 | -.09** | -.1559*** | .1602 | .1555 |
| | | (.041) | (.039) | (.1016) | (.0964) |
| Regular benefit duration | 4.938 | -.2695*** | -.1329*** | -.2089** | -.1679** |
| | | (.0311) | (.0178) | (.0869) | (.076) |
| Mechanical effect of a | 1.687 | -.8061*** | -.4938*** | -.7764*** | -.7247*** |
| hypothetical 2–month UI extension | | (.0797) | (.0589) | (.2294) | (.2234) |
| | | **Years after 2002** | | | |
| UI take-up | .8773 | -.1817*** | -.2018*** | -.0135 | .0096 |
| | | (.0333) | (.0326) | (.1454) | (.1454) |
| Regular benefit duration | 4.907 | -.3263*** | -.2423*** | -.5467*** | -.4341*** |
| | | (.0546) | (.0463) | (.1367) | (.1254) |
| Mechanical effect of a | 1.605 | -.9726*** | -.7574*** | -1.231*** | -1.195*** |
| hypothetical 2–month UI extension | | (.1523) | (.1326) | (.2046) | (.1926) |
| | | **Mesoregions with average formal employment rates between $5^{th}$ and $95^{th}$ percentile** | | | |
| UI take-up | .8644 | -.093** | -.1885*** | .1452** | .1368** |
| | | (.0374) | (.042) | (.0569) | (.0547) |
| Regular benefit duration | 4.935 | -.4071*** | -.2121*** | -.1969*** | -.1605*** |
| | | (.0513) | (.0294) | (.0723) | (.0619) |
| Mechanical effect of a | 1.671 | -1.186*** | -.7385*** | -.6605*** | -.6034*** |
| hypothetical 2–month UI extension | | (.137) | (.0948) | (.2112) | (.2017) |

Significance levels: * 10%, ** 5%, ***1%. Random samples of full–time private–sector formal employees 18–54 years old, laid off between 1995 and 2009, and eligible for 5 months of UI. The table presents robustness checks for the results in Table 1. The table displays coefficients from regressing the same outcomes on formal employment rates (see Table 1 for a description of the outcomes and the baseline specifications).

The top panel (s.e. clustered by 27 states) uses formal employment rates by state from yearly household surveys (PNAD). The middle panel (s.e. clustered by 137 mesoregions) uses only workers laid off between 2002 and 2009. The bottom panel (s.e. clustered by 124 mesoregions) excludes mesoregions with average formal employment rates over the period below the $5^{th}$ and above the $95^{th}$ percentile.

Results in this table confirm results from Table 1: the mechanical effect of a hypothetical 2–month UI extension is high on average but it decreases with formal employment rates.

Table C.2: Distribution of sample across areas and years for the 1996 temporary UI extension

| Year | Month | Control | Rio | Other Treat | Total |
|---|---|---|---|---|---|
| 1995 | April | .3 | .23 | .47 | 37,819 |
| | May | .31 | .23 | .46 | 43,387 |
| 1996 | April | .33 | .22 | .45 | 33,994 |
| | May | .36 | .2 | .44 | 34,453 |
| 1997 | April | .32 | .23 | .46 | 40091 |
| | May | .33 | .22 | .45 | 40,134 |
| All | All | .32 | .22 | .46 | 229,878 |

Full–time private–sector formal employees 18–54 years old from the largest metropolitan areas of Brazil (São Paulo excluded), laid off in April or May 1995, 1996, and 1997, and eligible for 5 months of UI benefits. In 1996, in treated metropolitan areas, these workers were eligible for 7 months of UI benefits. The table presents the distribution of our sample across control and treatment metropolitan areas (Rio de Janeiro and other treatment areas), and control and treatment years.

Table C.3: Composition of sample across areas and years for the 1996 temporary UI extension

| Variable | Year | Control | Rio | Other Treat | Treat–Control | |
|---|---|---|---|---|---|---|
| Male | 1995&1997 | .6543 | .6737 | .6649 | .0135 | (.0122) |
| | 1996 | .654 | .6664 | .6547 | .0044 | (.0138) |
| Age | 1995&1997 | 32.15 | 34.04 | 32.94 | 1.155*** | (.308) |
| | 1996 | 32.82 | 34.33 | 33.4 | .8707*** | (.2827) |
| Years of education | 1995&1997 | 7.299 | 7.606 | 7.392 | .1643 | (.1362) |
| | 1996 | 7.14 | 7.418 | 7.336 | .2219 | (.1405) |
| Log real wage | 1995&1997 | 6.927 | 6.922 | 6.895 | -.023 | (.0888) |
| | 1996 | 6.951 | 6.937 | 6.921 | -.0253 | (.0923) |
| Replacement rate | 1995&1997 | .4789 | .4834 | .4912 | .0096 | (.029) |
| | 1996 | .4881 | .4965 | .5026 | .0126 | (.03) |
| Commerce | 1995&1997 | .2647 | .2424 | .2542 | -.0145 | (.02) |
| | 1996 | .2602 | .2534 | .2606 | -.0019 | (.0215) |
| Services | 1995&1997 | .303 | .449 | .3657 | .0902** | (.0358) |
| | 1996 | .2984 | .4619 | .378 | .1062*** | (.0403) |
| Industry | 1995&1997 | .3689 | .2452 | .3015 | -.0861 | (.0564) |
| | 1996 | .3772 | .2262 | .2927 | -.1056* | (.06) |
| Firm size $\geq 100$ | 1995&1997 | .3692 | .4026 | .4088 | .0376 | (.0238) |
| | 1996 | .377 | .3892 | .3796 | .0056 | (.0297) |
| Firm size $< 10$ | 1995&1997 | .2847 | .2496 | .2506 | -.0344** | (.0152) |
| | 1996 | .2961 | .2549 | .271 | -.0302 | (.0206) |
| Share formally employed | all | .3175 | .3121 | .3043 | -.0106 | (.0233) |

s.e. clustered by metropolitan area (29 clusters) in parenthesis. Significance levels: * 10%, ** 5%, *** 1%. Full–time private–sector formal employees 18–54 years old from the largest metropolitan areas of Brazil (São Paulo excluded), laid off in April or May 1995, 1996, and 1997, and eligible for 5 months of UI benefits. In 1996, in treated metropolitan areas, these workers were eligible for 7 months of UI benefits. The table presents the composition of our sample across control and treatment metropolitan areas (Rio de Janeiro and other treatment areas), and control and treatment years. There are some differences between treatment and control metropolitan areas but these differences appear in treatment and control years.

Table C.4: Robustness of the difference–in–difference results for the 1996 temporary UI extension

| | UI take–up | Regular UI duration | Extended UI duration | Extended UI duration compared to counterfactual | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| | | | **Controlling for individual characteristics** | | |
| TreatArea * 1996 | -.0178 | -.0008 | 1.867*** | .2427*** | .1909*** |
| | (.0151) | (.0024) | (.0129) | (.02) | (.0195) |
| TreatArea * 1996 | | | | | .0749** |
| * formality > average | | | | | (.0333) |
| | | | **Replacement rate between 20% and 80%** | | |
| TreatArea * 1996 | -.0141 | .0007 | 1.871*** | .2654*** | .2009*** |
| | (.0182) | (.003) | (.0132) | (.0251) | (.025) |
| TreatArea * 1996 | | | | | .0929** |
| * formality > average | | | | | (.0406) |
| | | | **Excluding Rio de Janeiro** | | |
| TreatArea * 1996 | -.0084 | -.0003 | 1.869*** | .2579*** | .1939*** |
| | (.0166) | (.0029) | (.0185) | (.0239) | (.0175) |
| TreatArea * 1996 | | | | | .1117*** |
| * formality > average | | | | | (.0258) |
| | | | **Using formal employment rates linearly** | | |
| TreatArea * 1996 | -.0178 | -.0003 | 1.867*** | .2469*** | .2492*** |
| | (.0153) | (.0024) | (.0137) | (.0207) | (.015) |
| TreatArea * 1996 | | | | | 1.084*** |
| * formality rate | | | | | (.2463) |

s.e. clustered by metropolitan area (29 clusters). Significance levels: * 10%, ** 5%, ***1%. The sample includes full–time private–sector formal employees 18–54 years old from the largest metropolitan areas of Brazil (São Paulo excluded), laid off in April or May 1995, 1996, and 1997, and eligible for 5 months of UI benefits. In 1996, in treated metropolitan areas, these workers were eligible for 7 months of UI benefits. The table presents robustness checks for the results in Table 2. The table displays estimates of the difference–in–difference estimator for various outcomes (listed above each column; see Table 2 for a description of the outcomes and the baseline specifications).

The top panel includes dummies for education, sector of activity, gender, and firm size, as well as 4th order polynomials in tenure, age, and log real wage before layoff. The second panel restricts the sample to workers with replacement rates between 20% and 80%. The third panel excludes observations from Rio de Janeiro, more than 20% of our sample. The bottom panel uses formal employment rates entered linearly (demeaned) instead of a categorical variable.

Results in this table confirm results from Table 2: a 2–month UI extension increased benefit duration by 1.87 months on average, but only .25 month is due to a behavioral effect. The behavioral effect is larger in metropolitan areas with higher formal employment rates at the time.

Table C.5: Robustness of the overall regression discontinuity results

| | UI take–up | UI duration up to 4 months | Extended UI duration | Extended UI duration compared to counterfactual |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | | **Controlling for individual characteristics** | | |
| Tenure≥24 months | .0305 | .0059*** | .9098*** | .0829*** |
| | (.0224) | (.0011) | (.004) | (.0026) |
| | | **Tenure at layoff between 18 and 30 months** | | |
| Tenure≥24 months | .0469 | .0048*** | .8974*** | .0762*** |
| | (.033) | (.0018) | (.006) | (.0052) |
| | | **Years after 2002** | | |
| Tenure≥24 months | .0373 | .008*** | .9123*** | .094*** |
| | (.0237) | (.0018) | (.0043) | (.0041) |
| | | **Replacement rate between 20% and 80%** | | |
| Tenure≥24 months | .0306 | .0053*** | .8957*** | .0779*** |
| | (.021) | (.0014) | (.0047) | (.0043) |
| | | **Mesoregions with average formal employment rates between 5$^{th}$ and 95$^{th}$ percentile** | | |
| Tenure≥24 months | .0321 | .0052*** | .9124*** | .0832*** |
| | (.0227) | (.0016) | (.0047) | (.0043) |

s.e. clustered by week of tenure. Significance levels: * 10%, ** 5%, ***1%. Full–time private–sector formal employees 18–54 years old, laid off between 1997 and 2009. Those with less than 22 months of tenure at layoff were eligible for 4 months of UI. Those with more than 24 months of tenure at layoff were eligible for 5 months of UI (extension). Those with tenure between 22 and 24 months were eligible for 4 or 5 months of UI (see text) and are excluded from the regressions. The table presents robustness checks for the results in Table 5. The table displays the coefficients from regressing various outcomes (listed above each column) on a dummy for having more than 24 months of tenure at layoff (tenure cutoff; see Table 5 for a description of the outcomes and the baseline specifications).

The top panel includes dummies for year, mesoregion, sector of activity, gender, education, and firm size, as well as a 4th order polynomials in age and log real wage before layoff. The second panel considers a smaller tenure window around the 24–month tenure cutoff. The third panel uses only workers displaced between 2002 and 2009. The fourth panel restricts the sample to workers with replacement rates between 20% and 80%. The bottom panel excludes mesoregions with average formal employment rates over the period below the 5$^{th}$ and above the 95$^{th}$ percentile.

Results in this table confirm results from Table 5: a 1–month UI extension increased benefit duration by .91 month on average at the cutoff, but only .08 month is due to a behavioral effect.

Table C.6: Robustness of regression discontinuity results interacted with formal employment rates

| | Extended UI duration compared to counterfactual | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Tenure at layoff between 18 and 30 months** | | | | |
| Tenure≥24 months | .0765*** | .0772*** | .0771*** | .0781*** |
| | (.0047) | (.0048) | (.0046) | (.0038) |
| Formal employment rate in mesoregion | -.5932*** | -.3612*** | -.4341*** | -.3951*** |
| | (.0288) | (.0278) | (.0395) | (.0385) |
| Tenure≥24 months * Formal employment rate | .1414*** | .1339*** | .14*** | .1412*** |
| | (.0331) | (.0311) | (.0313) | (.028) |
| **Years after 2002** | | | | |
| Tenure≥24 months | .0908*** | .0901*** | .0899*** | .0895*** |
| | (.0038) | (.0039) | (.0037) | (.0033) |
| Formal employment rate in mesoregion | -.5626*** | -.39*** | -.8535*** | -.8279*** |
| | (.0228) | (.0211) | (.0622) | (.0624) |
| Tenure≥24 months * Formal employment rate | .1051*** | .1044*** | .1118*** | .1139*** |
| | (.027) | (.0258) | (.0255) | (.0258) |
| **Replacement rate between 20% and 80%** | | | | |
| Tenure≥24 months | .0766*** | .0808*** | .0807*** | .0793*** |
| | (.0041) | (.0039) | (.0038) | (.0032) |
| Formal employment rate in mesoregion | -.5872*** | -.2386*** | -.3427*** | -.3225*** |
| | (.0334) | (.0329) | (.0396) | (.0393) |
| Tenure≥24 months * Formal employment rate | .1234*** | .1262*** | .1369*** | .1366*** |
| | (.0366) | (.0356) | (.0359) | (.0353) |
| **Using state–level formal employment rates** | | | | |
| Tenure≥24 months | .0766*** | .0804*** | .0802*** | .0787*** |
| | (.0042) | (.0039) | (.0039) | (.0033) |
| Formal employment rate in mesoregion | -.6338*** | -.2299*** | -.5799*** | -.5732*** |
| | (.0281) | (.0279) | (.0506) | (.0498) |
| Tenure≥24 months * Formal employment rate | .167*** | .165*** | .1682*** | .1691*** |
| | (.031) | (.0298) | (.0297) | (.0291) |
| **Mesoregions with average formal employment rates between $5^{th}$ and $95^{th}$ percentile** | | | | |
| Tenure≥24 months | .0868*** | .0879*** | .0877*** | .0879*** |
| | (.004) | (.0038) | (.0037) | (.003) |
| Formal employment rate in mesoregion | -.5558*** | -.3365*** | -.6097*** | -.5895*** |
| | (.0179) | (.0181) | (.0338) | (.0323) |
| Tenure≥24 months * Formal employment rate | .1727*** | .1681*** | .1686*** | .1725*** |
| | (.0217) | (.0218) | (.0218) | (.0214) |
| Year fixed effects | No | Yes | Yes | Yes |
| Mesoregion fixed effects | No | No | Yes | Yes |
| Other controls | No | No | No | Yes |

s.e. clustered by week of tenure. Significance levels: * 10%, ** 5%, ***1%. The table presents robustness checks for the results in Table 6 (where the outcome and the baseline specifications are described). The top panel uses a smaller tenure window. The second panel uses only workers displaced between 2002 and 2009. The third panel uses only workers with replacement rates between 20% and 80%. The fourth panel uses state–level instead of mesoregion–level formal employment rates. The bottom panel excludes mesoregions with average formal employment rates over the period below the $5^{th}$ and above the $95^{th}$ percentile. Results in the table confirm results from Table 6: the average behavioral effect is around .08 month (first row), the mechanical effect is decreasing (second row) and the behavioral effect (third row) increasing with formal employment rates.

Table C.7: Testing the accuracy of our counterfactual approach

| | Extended UI duration (using only UI data) (1) | Extended UI duration (inferred from formal reemployment pattern) (2) |
|---|---|---|
| | **1996 temporary UI extension** | |
| TreatArea * 1996 | 1.867*** | 1.846*** |
| | (.0137) | (.0168) |
| Mean (control years) | 4.98 | 4.98 |
| Observations | 171,407 | 171,407 |
| | **Tenure–based discontinuity in eligibility** | |
| Tenure≥24 months | .9091*** | .9095*** |
| | (.0043) | (.0027) |
| Mean 2002–2009 ($20 \leq Tenure < 22$) | 4 | 3.96 |
| Observations | 2,302,058 | 2,302,058 |

Significance levels: * 10%, ** 5%, ***1%. The table provides a test for the accuracy of our approach using workers' formal reemployment patterns to construct the counterfactual benefit duration of control beneficiaries (mechanical effect) when estimating the behavioral effect of UI extensions throughout the paper. The table displays estimates of the difference–in–difference estimator (1996 temporary UI extension, treated beneficiaries eligible for 7 months of UI instead of 5 months, s.e. clustered by metropolitan area in parenthesis) and the regression discontinuity estimator (tenure–based discontinuity in eligibility, treated beneficiaries eligible for 5 months of UI instead of 4 months, s.e. clustered by week of tenure) for various outcomes (listed above each column). The regressions in the top panel include include (calendar) separation month, year, and metropolitan area fixed effects (see Table 2 for a full description of the sample). The regressions in the bottom panel include fixed effects for (calendar) separation and hiring months and linear controls in tenure on each side of the discontinuity (see Table 5 for a full description of the sample). Outcomes are conditional on take–up.

Column (1) estimates treatment effects on the actual benefit duration using data from the UI registry data. Formally, in the top panel, the outcome is defined as: $\sum_{i=1}^{5} \mathbb{1}\left(\text{draw i}^{\text{th}} \text{ UI payment}\right) + \sum_{i=6}^{7} \mathbb{1}\left(\text{draw i}^{\text{th}} \text{ UI payment}\right)$. Formally, in the bottom panel, the outcome is defined as: $\sum_{i=1}^{4} \mathbb{1}\left(\text{draw i}^{\text{th}} \text{ UI payment}\right) + \sum_{i=5}^{5} \mathbb{1}\left(\text{draw i}^{\text{th}} \text{ UI payment}\right)$. The treatment increased average benefit duration by 1.87 months in the top panel and .91 month in the bottom panel.

Column (2) shows that the treatment effects on the actual benefit duration in column (1) can be well approximated using workers' formal reemployment patterns after UI exhaustion (coefficients are very similar in columns 1 and 2). Define $month_{regUI}$, the month a beneficiary exhausts her "regular" benefit duration (5 months in the top panel, 4 months in the bottom panel). We assume that a beneficiary who exhausts her regular UI benefits and is not formally reemployed within 1 month (resp. 2 months) of regular UI exhaustion would draw 1 extra payment (resp. 2 extra payments) if she is eligible. Define $month_{back}$, the month a beneficiary returns to a formal job. Formally, in the top panel, the outcome is defined as:

$$\sum_{i=1}^{5} \mathbb{1}\left(\text{draw i}^{\text{th}} \text{ UI payment}\right) + \left[\mathbb{1}\left(\text{draw 5}^{\text{th}} \text{ UI payment}\right) \times TreatArea \times Year1996 \times \sum_{j=1}^{2} \mathbb{1}\left(month_{back} > month_{regUI} + j\right)\right]$$

Formally, in the bottom panel, the outcome is defined as:

$$\sum_{i=1}^{4} \mathbb{1}\left(\text{draw i}^{\text{th}} \text{ UI payment}\right) + \left[\mathbb{1}\left(\text{draw 4}^{\text{th}} \text{ UI payment}\right) \times \mathbb{1}\left(\text{Tenure} \geq 24 \text{ months}\right) \times \sum_{j=1}^{1} \mathbb{1}\left(month_{back} > month_{regUI} + j\right)\right]$$

Table C.8: Long–term effects of UI extensions

| | Months employed (in next 2 years) | New layoff (in next 2 years) | Log real wage (at formal reemployment) | Log real wage (if formal in Dec. 2 years later) | Log real wage (if formal in Dec. 2 years later) |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| | | | **1996 temporary UI extension** | | |
| TreatArea * 1996 | -1.211*** | -.0148*** | .0016 | -.0343*** | -.0164 |
| | (.11) | (.0027) | (.0112) | (.0114) | (.0122) |
| Mean (control years) | 8.97 | .1 | 6.55 | 6.71 | 6.71 |
| Observations | 171,407 | 171,407 | 103,452 | 68,589 | 68,589 |
| | | | **Tenure–based discontinuity in eligibility** | | |
| Tenure≥24 months | -.1999*** | -.0071*** | .0024 | -.0061 | .0041 |
| | (.0478) | (.0017) | (.0052) | (.0074) | (.0068) |
| Mean 2002–2008 ($20 \leq Tenure < 22$) | 8.58 | .13 | 6.5 | 6.7 | 6.7 |
| Observations | 2,073,090 | 2,073,090 | 1,348,187 | 858,940 | 858,940 |

Significance levels: * 10%, ** 5%, ***1%. The table displays estimates of the difference–in–difference estimator (1996 temporary UI extension, treated beneficiaries eligible for 7 months of UI instead of 5 months, s.e. clustered by metropolitan area in parenthesis) and the regression discontinuity estimator (tenure–based discontinuity in eligibility, treated beneficiaries eligible for 5 months of UI instead of 4 months, s.e. clustered by week of tenure) for various long–term outcomes (listed above each column). The regressions in the top panel include dummies for (calendar) separation month, year, and metropolitan area (see Table 2 for a full description of the sample). The regressions in the bottom panel include dummies for (calendar) separation and hiring months, and linear controls in tenure on each side of the discontinuity (see Table 5 for a full description of the sample). Outcomes are conditional on take–up.

The outcome in column (1) is the number of months working as a formal employee in the two years after layoff; in column (2) is a dummy for experiencing at least 1 new layoff from the formal sector in the two years after layoff. Outcomes in column (3)–(5) are conditional on formal reemployment withintwo2 years after layoff. The outcome in column (3) is the logarithm of the real wage in workers' first new formal job; in column (4) is the logarithm of the real wage earned by a worker if she is formally employed in December two years after layoff. The regression in column (5) include fixed effect for the (endogenous) duration out of formal employment.

UI extensions reduce formal employment beyond the UI benefit collection period (column 1), but also reduce the occurence of (frequent) new layoffs from the formal sector (column 2; one must be formally reemployed to be displaced again). This supports our steady state approach in Section 3: delaying formal reemployment beyond the benefit duration does not have a strong effect on the UI budget because it also reduces future spells of UI collection. In the remaining columns, we find no impact of UI on susbsequent wages. In the top panel, the coefficient in column (4) is smaller and no longer significant in column (5). The difference may be due to tenure effects as treated beneficiaries delayed formal reemployment.