Social Insurance under Imperfect Monitoring

Labor market and welfare impacts of the Brazilian UI program*

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Abstract

Social insurance programs, including Unemployment Insurance (UI), have been adopted in many countries where informal employment is prevalent and monitoring of eligibility imperfect. Whether social insurance programs can increase welfare in such a context often remains an open question. To address this issue, we study the Brazilian UI program. Social insurance trades off welfare gains from providing income support and efficiency costs from distorting behaviors. Imperfect monitoring may both exacerbate behavioral responses and, with the possibility to work informally, reduce the need for insurance. Using matched employee–employer data, and two complementary empirical strategies, we estimate the impacts of UI extensions on program and labor market outcomes and their efficiency costs. We find large percentage reductions in hazard rates of formal reemployment, in particular around benefit exhaustion. However, because hazard rates are very low, UI has little impact on formal reemployment and efficiency costs amount to only 5%–11% of the cost of extending benefits. Using survey data, we further estimate that 35% of our sample of job–losers is actually unemployed after 5 months of UI, a figure comparable to the US. In our normative framework, these results imply that even a low social value of insurance is consistent with an increase in welfare from extending UI. We obtain that welfare effects from the existing UI program are likely to be positive and may be sizeable.

JEL classifications: I38, J65, O17

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1 Introduction

Social insurance accounts for a smaller share of GDP or government spendings in poorer countries and this share is strongly associated with income per capita (Chetty and Finkelstein, 2011). Consistent with this relationship,[1] social insurance and transfer programs have been expanding in middle–income and developing countries. Those expansions have generated a large body of empirical work to assess programs' impacts on several outcomes.[2] However, results from this literature are seldom easily translated into a cost–benefit framework. Therefore, whether conditions were met for those programs to increase welfare in implementing countries often remains an open question.

Major concerns for the implementation of social insurance and transfer programs in poorer countries are twofold. First the nature of the labor market is different, with often a large informal sector escaping oversight from government agencies. Second, monitoring of beneficiaries' eligibility may be very imperfect (Camacho and Conover, 2011). These concerns are related when eligibility depends on labor status (Levy, 2008). Because of these specific aspects, costs and benefits of a given program are likely to differ from those in richer countries. Social insurance programs, such as Unemployment Insurance (UI), trade off welfare gains from providing income support and efficiency costs from distorting behaviors. On the one hand, with the possibility to work informally, a weak capacity to monitor eligibility status may reduce the need for income support of the average UI beneficiary. On the other hand, it may exacerbate behavioral responses to the program's incentives and increase efficiency costs if potential beneficiaries are more willing and better able to adjust their eligibility status.[3] Whether this precludes specific programs to be welfare–enhancing is an empirical question that crucially depends on credible estimates of efficiency costs.

We focus on UI for two reasons. First, UI has been adopted (Figure 1) and is on the agenda in a growing number of countries sharing with Brazil widespread informal employment and difficult monitoring of individuals' labor status.[4] Yet there is little empirical evidence on costs and benefits of UI in such contexts. Second, recent work on optimal social insurance has revived normative tests to assess welfare consequences from providing UI (Baily, 1978; Chetty, 2006 and 2008). Those tests rely on estimating particular *sufficient* statistics (Chetty, 2009). Among existing social insurance programs, UI is therefore particularly well–suited for our purpose because there exists a framework connecting empirical and welfare analysis.

[1]The exact timing of expansions of social programs is probably related to political economy considerations: the first Unemployment Insurance law in Brazil, for example, was enacted one year after the end of the military dictatorship.

[2]For the Mexican health care program *Seguro Popular* only: Azuara and Marinescu, 2011; Barros, 2008; Campos–Vasquez and Knox, 2008; Bosch et al., 2010; Aterido et al., 2011

[3]"The lack of appropriate administrative capacity to effectively monitor continuing eligibility (...) implies that the moral hazard problem (...) could be particularly prominent." (Vodopivec, 2008)

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

In this paper, we investigate impacts of UI extensions on program and labor market outcomes in Brazil. In particular, we estimate a measure of efficiency costs directly derived from a normative framework. To do so, we use matched longitudinal employee–employer data covering the universe of formal employment (the sector monitored by the UI agency) and two complementary empirical exercises that are based on credible identification strategies (described below). We then use our results and available survey data to evaluate welfare effects from UI extensions in a context of imperfect monitoring of beneficiaries' labor status.

More specifically, we proceed in four steps. First, we adapt a typical normative framework from the literature (Chetty, 2006; Kroft, 2008; Schmieder et al, 2011) which highlights the trade–off between efficiency costs from distorting behaviors and social gains from providing income support. A UI extension affects UI costs by increasing mean covered duration, the actual duration of benefit collection, through: (i) a mechanical effect, as extra benefits are offered to UI exhaustees, and (ii) a behavioral effect, as incentives to find a job are reduced. The share of the increase due to the behavioral effect is a relative measure of efficiency costs as behavioral responses increase UI costs but have no first–order effects on individuals' utility (envelope argument). The mechanical effect is the potential source of welfare gains as it transfers income without distorting reemployment behavior. How large those gains are depends on the social value of such a transfer. To evaluate whether a UI extension (and thus the existing program) increases welfare, we must (i) recover estimates of both the mechanical and the behavioral effects and (ii) discuss the social value of transferring income to UI exhaustees.

Second, we turn to the matched employee–employer data to estimate program and labor market outcomes from UI extensions in Brazil, in particular the mechanical and behavioral effects. We find that the hazard of reemployment in the formal sector more than doubles after benefit exhaustion for workers eligible for 5 months of benefits, a large spike not observed in richer countries (Card et al., 2007). A politically–motivated temporary UI extension provides us with an ideal experiment to study the impact of maximum benefit duration on this spike in formal reemployment. Indeed our sample of workers learned about the extension shortly before exhausting regular benefits. To estimate behavioral responses to UI extensions earlier in the unemployment spell (anticipation behavior), we also exploit a tenure–based discontinuity.

We find large percentage reductions in hazard rates of formal reemployment when benefits are extended. In particular, the spike at benefit exhaustion is entirely due to the UI program. We also find evidence that individuals are forward–looking and adjust their behavior early in the unemployment spell. However, because monthly hazard rates of formal reemployment are very low even after benefit exhaustion (below 10%), the behavioral effect amounts to only 5%–11% of the total increase in mean covered duration.[5] Contrary to a prior that imperfect monitoring may

[5]High survival rates outside the formal sector also imply that providing UI in Brazil is costly as most beneficiaries exhaust their benefits.

exacerbate behavioral responses, UI appears to have little impact on formal reemployment and efficiency costs are actually low (the same statistic is estimated at 57% for the US — Katz and Meyer, 1990).

Our findings are actually consistent with predominant views on the prevalence of informal employment. Indeed, reemployment rates in the formal sector could be low if either formal jobs are more difficult to secure (Fields, 1975) or informal employment is more attractive (Maloney, 1999). If efficiency costs are low, the social value of providing income support to UI exhaustees need not be as high for a UI extension to increase welfare.[6] However, this social value is likely to depend on the reason why formal reemployment rates are so low. Indeed, the need for income support may be small if most beneficiaries are working informally while collecting benefits. In a third step, we rely on available longitudinal labor force surveys to investigate these aspects. Using a maximum likelihood estimation adapted to the sampling of the surveys, we estimate that about 35% of our sample of formal job losers is unemployed at benefit exhaustion, a figure comparable to the US. Therefore a significant number of formal job losers is actually unemployed at benefit exhaustion, although some UI exhaustees are likely to be working in jobs undetected by the UI agency. We also document large disposable income gaps between formally employed individuals and formal job losers at benefit exhaustion.

Finally, we use our results and evaluate welfare effects from a UI extension. First, we bound the social value of providing insurance to UI exhaustees consistent with an increase in welfare. This social value is small (in absolute terms and using the US as benchmark) because of the low efficiency costs. As many UI exhaustees are unemployed, the result holds even if we adopt the strong assumption that there is no gain from transferring income to informally reemployed UI exhaustees. Following Baily (1978), we then impose some structure on the social value such that it depends on relative risk aversion (γ) and the disposable income gaps we estimated. We then simulate welfare effects for typical values of γ. A marginal UI extension increases welfare for the whole range of values considered and the gains can be sizeable, even if we apply the same assumption as above.

Our normative approach follows the literature on optimal unemployment insurance by considering a single policy instrument—maximum benefit duration. We also abstract from the impact of UI extensions on layoff rates, as experience–rating of benefits is the usual prescription to prevent responses at the layoff margin. We further discuss how several mechanisms, absent from our analysis and potentially important in developing countries, may affect our welfare results. Nevertheless, our analysis indicates that a UI program may increase welfare even in the presence of informal employment and imperfect monitoring of UI beneficiaries' labor status.

In our empirical analysis, we also investigate impacts of UI extensions beyond the maximum benefit duration by following workers for up to 2 years after layoff. We estimate that extending

[6] The same argument applies to recessions (Schmieder et al., 2011): in recessions, reemployment rates are lower and the behavioral effect is smaller compared to the mechanical effect, increasing welfare effects from extending UI.

UI increases the duration between two formal jobs, and reduces the time spent formally employed. However, it also reduces the share of individuals experiencing a new layoff from the formal sector. A typical UI program taxes eligible individuals while working to provide benefits upon layoff. Revenues thus depend on average formal employment duration before layoff. Our estimates imply that extending UI would have no effect on this measure in subsequent periods.[7] We finally consider measures of job quality but do not find systematic effects on job–matching.

Our paper contributes to three main strands in the literature. First, we contribute to the empirical literature on unemployment insurance by studying the impact of UI extensions in a different context. We are aware of only a few working papers, developed in parallel to our work, which attempt to causally estimate the impact of UI on some labor market outcomes in non–OECD countries.[8] To our knowledge, we are the first to estimate the impact of UI on mean covered duration and UI costs. We are also the first to causally estimate the impact of UI on a spike in reemployment at benefit exhaustion. Second, we contribute to the empirical literature on optimal unemployment insurance by showing that a UI program may increase welfare even if monitoring of UI beneficiaries' labor status is very imperfect. Informal employment is not limited to developing countries, but it is there more prevalent (Schneider and Enste, 2000). Finally, our paper contributes to the literature on the impact of social insurance and transfer programs in middle–income and developing countries. As mentioned above, the prevalence of informal employment and imperfect monitoring of beneficiaries' eligibility status have been a central theme in this literature (Levy, 2008). We are the first paper that uses a cost–benefit framework to guide our empirical analysis of program and labor market outcomes, in particular the estimation of a direct measure of efficiency costs, and to evaluate welfare effects of a social insurance program in this context. We consider such an approach essential as many countries considering adopting or extending social insurance programs share similar characteristics and policy should be based on empirical results interpretable in terms of efficiency costs and welfare.

Our findings also relate to the literature on labor markets in developing countries. The traditional view considered informal and formal jobs as segmented labor markets (Fields, 1975). Recent work argues that informal employment may be voluntary (Maloney, 1999). Our normative framework does not rely on a specific view of the labor market. We acknowledge that informal employment may be voluntary and bound our welfare effects by assuming that welfare gains from providing income support to the informally reemployed are nil. A few papers in this literature investigate the impact of UI in macro models of the labor market (Zenou, 2008; Ulyssea, 2010) but their modeling of the incentives created by UI is often coarse. Another literature investigates the consequence of employment shocks in developing countries (Chetty and Looney, 2006 and 2007)

[7] Contrary to the models used in Chetty (2008) and Schmieder et al. (2011).

[8] See IADB (in progress) for example. We are aware of 3 working papers on UI in Brazil but they are mostly descriptive (Cunningham, 2000; Margolis, 2008; Hijzen, 2011).

stressing the potential benefits of insurance programs as it appears more difficult for households to smooth consumption in developing countries. We complement this literature by estimating the costs of providing insurance and by combining both costs and benefits to evaluate welfare effects. Finally, our paper is part of a small but growing literature at the intersection of Development Economics and Public Finance (Gordon and Li, 2005).

The rest of the paper is organized as follows. Section 2 adapts a framework to discuss costs and benefits of UI and evaluate welfare effects from UI extensions. Section 3 provides some background and describes the Brazilian UI program and our data. Section 4 turns to the data by first recovering formal reemployment survival and hazard rates for potential UI beneficiaries. We then exploit two complementary empirical strategies to study the impact of UI extensions on program and labor market outcomes, and their efficiency costs. Section 5 investigates the insurance role of UI using survey data. Section 6 uses our results to evaluate welfare effects. Section 7 concludes.

2 Cost and Benefits of a UI program

This section introduces a framework adapted from the optimal UI literature (Baily, 1978; Chetty, 2006 and 2008; Kroft, 2008; Schmieder et al., 2011). We start from a typical budget equation and decompose the impact of UI extensions on UI costs into: (i) a mechanical effect, as additional benefits are now offered to UI exhaustees, and (ii) a behavioral effect, as incentives to find a job are reduced. We then derive a *sufficient statistics* formula (Chetty, 2009) to evaluate welfare effects. In this framework, the relative size of the behavioral effect is a measure of efficiency costs (envelope argument). This welfare decomposition guides our subsequent empirical analysis: it requires us to (i) recover estimates of both the mechanical and the behavioral effects and (ii) discuss the social value of transferring income to UI exhaustees. Our framework is consistent with a job–search model with formal and informal employment opportunities (see Appendix). However, it does not rely on a specific model of the labor market.

1. Budget equation. A typical UI program taxes working eligible individuals to provide benefits upon layoff for a limited period. A budget–balanced UI program with replacement rate $r = \frac{b}{w^F}$ (b, UI benefits; w^F, formal wage) and maximum benefit duration of P periods must satisfy:

$$D^F \tau w^F = D^B r w^F \tag{1}$$

where τ is the tax levied on formal wage w^F, D^F is the average duration of formal employment before layoff, and D^B is mean covered duration, the average duration of actual benefit collection. The right–hand side in (1) is average costs and the left–hand side average revenues per UI spell. Lower reemployment rates in jobs monitored by the UI agency increase mean covered duration, UI costs, and the necessary tax rate. High layoff rates reduce formal employment duration before layoff, reduce UI revenues, and increase the necessary tax rate.

2. Cost of extending UI. An extension in maximum benefit duration P increases mean covered duration, and UI costs, through: (i) a mechanical effect, as additional benefits are now offered to UI exhaustees, and (ii) a behavioral effect,[9] as incentives to find a job are reduced. Those two effects are decomposed in Figure 2. Illustrative survival functions out of the formal sector are drawn for a maximum benefit duration of 4 and 5 months ($P = 4, 5$). Higher survival rates when benefits are extended capture the reduced incentives to find a formal job. The black area corresponds to mean covered duration (D^B) before the UI extension. The mechanical effect corresponds to the white area: individuals surviving out of the formal sector after benefit exhaustion would collect the additional benefit even if they do not change their behavior. The grey area corresponds to the behavioral effect, the change in mean covered duration due to behavioral responses.

If $S(t)$ is the survival rate out of formal employment at the start of period t, we have from (1):

$$\frac{d\tau}{dP} = \frac{d\left(r\frac{D^B}{D^F}\right)}{dP} = \frac{r}{D^F}\frac{dD^B}{dP} = \frac{r}{D^F}\left[S(P) + \sum_{t=0}^{P}\frac{dS(t)}{dP}\right] \tag{2}$$

where the first term in brackets is the mechanical effect and the second term the behavioral effect. Equation (2) assumes that UI benefits are sufficiently experience–rated. This is a common assumption in the literature because experience rating is the usual normative prescription to prevent responses of layoff rates and formal employment duration to UI.[10] Equation (2) also assumes that average duration of formal employment before layoff is not affected by the impact of UI extensions on the duration out of the formal sector. Such a channel matters in models where individuals contribute to the UI system upon reemployment but new jobs are never lost (Chetty, 2008; Schmieder et al., 2011). In practice, layoff risk upon reemployment is not nil. Duration out of the formal sector affects the overall time a worker spends in the formal sector but it does not affect average duration of formal employment before layoff (and average UI revenues) if the layoff rate is unchanged.[11]

How would UI costs compare in countries with widespread informal employment and imperfect monitoring of beneficiaries' labor status? Reemployment rates in the formal sector are likely to be lower and behavioral responses smaller if either formal jobs are difficult to secure (Fields, 1975) or informal jobs are relatively attractive (Maloney, 1999), as derived in a job–search model in the Appendix. Extending benefits would then be costly, mostly through the mechanical effect. Behavioral responses could be larger if the *formality* of a given reemployment status is easily adjusted: imperfect monitoring may allow individuals to hide new formal jobs while collecting UI,

[9] The relevant effects are at the macro level. As most empirical variation is at the micro level, estimates give an upper bound in the presence of search externalities (see Landais et al., 2011).

[10] We find suggestive evidence that layoff rates may respond to UI eligibility in Brazil. Yet, existing institutions appear sufficient to prevent such responses for the sample of workers we consider.

[11] In an infinite horizon model with 2 states (employment–unemployment) and constant transition probabilities, a decrease in the probability to leave unemployment reduces equilibrium employment but also the number of transitions to unemployment (layoff) as the latter is proportional to the former. Employment duration before layoff is unaffected.

as revealed by a lot of anecdotal evidence in Brazil.[12] Moreover, potentially large income effects in poorer countries (Chetty and Looney, 2006 and 2007) would predict high reemployment rates and a large behavioral response. Because of this ambiguity, our paper addresses the question empirically.

3. Welfare effects of extending benefits. Adopting a welfarist approach and assuming agents behave optimally, we can derive a sufficient statistics formula to evaluate welfare effects from UI extensions, taking the rest of the environment as given. Welfare gains from extending benefits result from the additional benefit rw^F collected by individuals if they do not change their behavior, corresponding to the mechanical effect—$S(P)$. If we define g_P^U as the social marginal welfare weight on UI exhaustees, the social value of giving \$1 to the average UI exhaustee, we have:

$$dW_G = g_P^U S(P) rw^F$$

Welfare losses from extending UI result from the tax increase necessary to balance the UI budget. Let's define g^E as the social marginal welfare weight on contributing individuals, typically the social value of giving \$1 to the average formal worker. As formal workers spend on average D^F periods contributing before collecting benefits, we have that:

$$dW_L = -g^E D^F w^F \frac{d\tau}{dP} = -g^E rw^F \frac{dD^B}{dP} = -g^E rw^F \left[S(P) + \sum_{t=0}^{P} \frac{dS(t)}{dP} \right]$$

The average welfare effect (per UI spell) of extending UI is the sum of gains and losses. We can divide each term by g^e to express welfare in a money metrics (Chetty, 2008). To obtain a formula for the aggregate welfare effects, we multiply by the number of layoffs in a given period divided by total formal payroll over the period (or $\frac{q}{w^F}$, with q the layoff rate). Welfare effects are then expressed as a percentage increase in total formal payroll.

$$\widetilde{dW} = \frac{q}{w^F} rw^F \frac{dW_G - dW_L}{g^E} = qr \frac{dD^B}{dP} \left[\frac{S(P)}{dD^B/dP} \left(\frac{g_P^U - g^E}{g^E} \right) - \frac{\sum_{t=0}^{P} \frac{dS(t)}{dP}}{dD^B/dP} \right] \tag{3}$$

The share of the cost increase due to the behavioral is a relative measure of efficiency costs and enters negatively in (3): the behavioral effect increases UI spendings but has no first–order effect on the welfare of UI beneficiaries (envelope argument). The mechanical effect enters positively as it

[12] Our favorite quote from Yahoo Answers! found on March 3rd, 2011.

Q: I am starting in a new firm (...) and there are 5 days left to receive the other month of my unemployment insurance. If I am registered before I get my next payment do I lose the UI this month? (...)

R: In general, when receiving unemployment insurance benefits, employees talk to the employer asking to sign the working card after receiving all UI payments, as a "favor". Most firms agree without any problem. Some do not, fearing that the Labor Ministry finds out - fines for an unregistered worker are huge. Talk to your boss and say: "Look, I would like to negotiate with you, Sir, about registering my working card. The thing is that I still have the last month of my UI to receive on day "X" and I would not like to miss it. Would you mind registering my card after that date?" My sister and my friends already have done this a lot... they never faced any problem. (...) This is so common nowadays.

corresponds to a non–distortionary income transfer from contributing individuals to UI exhaustees. The social value of such a transfer is captured by $\frac{g_P^U - g^E}{g^E}$. We can estimate every element of (3), besides the social value. As in other sufficient statistics formula, those elements are not deep structural parameters independent of each other. For example, large income effects lead to large behavioral responses but also reveal a high social value of insuring UI exhaustees (Chetty, 2008). Nevertheless the formula can be used as a local empirical test around the existing UI program: $d\widetilde{W} > 0$ implies that a marginal UI extension, and the existing UI program, increases welfare.

To evaluate welfare effects in this framework, we must (i) estimate both the mechanical and the behavioral effect, what we do in Section 4, and (ii) discuss the social value of transferring income to UI exhaustees. Given an estimate of efficiency costs, we can bound the social value consistent with an increase in welfare and simulate welfare effects for specific social values, using (3). To make further progress, we exploit longitudinal labor force survey data in Section 5 to estimate the share of UI exhaustees actually unemployed, $O(P)$. Indeed, if most UI exhaustees are voluntarily reemployed in jobs undetected by the UI agency, the need for income support may be limited. We then decompose social value and mechanical effect as:

$$d\widetilde{W} = qr\frac{dD^B}{dP}\left[\frac{O(P)}{dD^B/dP}\left(\frac{g_P^O - g^E}{g^E}\right) + \frac{I(P)}{dD^B/dP}\left(\frac{g_P^I - g^E}{g^E}\right) - \frac{\sum_{t=0}^{P}\frac{dS(t)}{dP}}{dD^B/dP}\right] \tag{4}$$

with g_P^O and g_P^I the social marginal welfare weight on unemployed and informally reemployed UI exhaustee and $I(P)$ the share of individuals still informally reemployed after benefit exhaustion. Using (4), we can impose the strong assumption that there is no gain from transferring income to informally reemployed UI exhaustees and recover a new bound on the social value of insuring unemployed UI exhaustees consistent with $d\widetilde{W} > 0$. There is no clear prediction as to how this social value would compare in poorer countries, not mentioning the fundamental subjectivity it encloses. Individuals may particularly benefit from insurance schemes if smoothing consumption is more difficult in poorer countries (Chetty and Looney, 2006 and 2007). Higher costs of taxing formal workers in developing countries —even for benefit taxes (Levy 2008), can be captured by a higher value of \$1 to contributing individuals (g^e). In least developed settings, formal jobs may be so rare and short–lived that workers do not value insurance.

Finally, we can follow Baily (1978) and assume a social welfare function with equal Pareto weights and a unique utility function such that:

$$\frac{g_P^{O,I} - g^E}{g^E} \simeq \gamma\frac{c^E - c^{O,I}}{c^E} \tag{5}$$

with consumption levels c and γ the coefficient of relative risk aversion. In section 5, we estimate disposable income gaps between UI exhaustees and the formally employed to approximate consumption gaps. We can then simulate welfare effects for different values of γ.

Our cost–benefit framework abstracts from several dimensions that we cannot address empirically. First, this is a partial equilibrium analysis: the envelope argument is not sufficient, for example, in the presence of search frictions and wage bargaining. Search frictions may be particularly large in labor markets with a large informal sector (Ulyssea, 2010). Landais et al. (2011) extends the analysis to incorporate such general equilibrium considerations and obtain that welfare effects from UI are likely to be larger. Three additional remarks deserve our attention:

A. Longer–term impacts. Extending UI may improve welfare through subsequent job quality. We investigate this channel empirically in Section 4 but we find no systematic impact.

B. Informality as externality. The impact of UI extensions on informal employment does not affect welfare directly when behavioral responses have only second–order effects. Informal employment might generate negative externalities (Gordon and Li, 2005; Levy, 2008) and therefore the impact of UI extensions on informal employment may be first-order (in the Appendix, we derive a welfare formula allowing for an externality). The sign of such an impact is ambiguous (see below). Therefore, it is unknown a priori if UI may exacerbate or mitigate a potential externality. Extending UI may also attract informal workers to the formal sector. If those new workers face higher layoff rates or smaller reemployment rates than average, it will create fiscal externalities.[13] Instead, extending UI may push workers away from the formal sector because of the (benefit) tax increase necessary to finance such an extension (Levy, 2008).

C. Informality, moral hazard and income effects. The recent literature on optimal UI focuses on disentangling substitution (moral hazard) and income (liquidity) effects (Chetty, 2008). Both substitution and income effects reduce formal reemployment rates. Therefore the sign of the impact of UI extensions on formal reemployment rates is uninformative. However, the substitution effect pushes individuals towards informal employment. Therefore, a negative impact of UI extensions on informal reemployment rates would reveal that any substitution effect is dominated by a larger income effect (if negative). This is derived in the Appendix. To our knowledge, existing data are not rich enough to tackle such a question, at least in Latin America: samples are too small, panels are too short, and little effort is devoted to UI programs.[14]

3 Institutions, Data and Background

Before turning to the empirical analysis, we describe here the Brazilian UI program and our data as well as relevant background information on the Brazilian labor market.

[13] Importantly, informal workers newly attracted to the formal sector must be different in such a way as the mere impact of attracting additional workers to the formal labor force would not affect the UI budget equation.

[14] Currently, monthly survey data in Brazil do not ask about UI benefits and information about previous jobs are only collected for the unemployed. In Argentina a question about UI benefits is only asked to unemployed workers.

3.1 Institutions

A. The Brazilian UI program

Contrary to other developing countries, Brazil has an experience of more than 20 years with UI and a sizeable UI program. From 1995 to 2002, on average per year, the program provided benefits to 4.5 million new beneficiaries and 19.9 million UI checks were issued. Total benefit payments amounted to 2.5% of the total payroll of eligible workers, more than three times the corresponding US figure. UI is available for workers from the private sector[15] who are (i) fired without a justified reason, (ii) with at least 6 months of formal work prior to dismissal, and (iii) with no other form of labor income. In practice, the last condition is only enforced if reemployed as a formal employee. Each month, applications are compared to a database tracking hiring and layoffs as reported by firms on a monthly basis (CAGED). Benefits are granted for a month if individuals do not reappear in the data.[16] Due to little incentives to timely report hirings, this database has a coverage 15% smaller than the data we use, reported yearly (Ministério do Trabalho e Emprego, 2008).

There is a 30–day waiting period before a first UI payment can be collected. Maximum benefit duration depends on the number of months of formal employment in the 36 months prior to dismissal, 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 $T_{36} \geq 24$. The replacement rate is constant until exhaustion of benefits and is means–tested, starting at 80% at the bottom of the wage distribution.[17] Benefits can be used discontinuously over a period of 16 months after which a worker is again eligible for the full maximum benefit duration. This complicates assessment of eligibility for a given worker in our data. Finally, Brazil is uncommon in the sense that UI is financed through firms' payments of a .65% tax on total sales.[18]

B. Job protection institutions

Other institutions in Brazil are aimed at protecting formal employees from labor demand shocks.
Severance payment accounts. Employers must deposit 8% of a worker's monthly wage in a severance payment account (FGTS). Employees can usually only access the account upon layoff or retirement. In case of layoff, employers must pay a fine equivalent to 50% of the amount deposited.
Advance notice of layoff. 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. 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

[15]Any formal worker must have their "working card" signed by their employer. In Brazil, the definition of the formal sector is thus somewhat clearer than in other countries.

[16]Since 1990, on average, more than 98% of applications are successful (Ministério do Trabalho e Emprego, 2010).

[17]The complete schedule is presented in Figure A.1 in the Appendix.

[18]If UI is financed through a sales tax, the social marginal welfare weight on contributing individuals is the social value of $1 for the average consumer of formal goods rather than the average formal worker. It is unclear how those compare. In theory, they should be equal if governments equalize the marginal cost of public funds across sources.

a representative of the Labor Ministry (or the Unions) who verifies workers received all payments they were entitled to. This increases oversight over the layoff process and constitutes a significant administrative burden, as officials are unable to visit every worksite each month.

3.2 Data

A. Matched employee–employer data (RAIS)

Our main data, RAIS (Relação Anual de Informações Sociais), is derived from Brazil's labor force records gathered annually by the Labor Ministry. RAIS is a longitudinal linked employee–employer dataset covering by law the universe of formally employed workers. It includes public sector employees but by design misses employers, informal workers, and the self–employed. All tax-registered firms have to report every worker formally employed at some point during the previous calendar year. RAIS' main purpose is to administer a federal wage supplement (Abono Salarial) offering the equivalent of a monthly minimum wage to low–wage workers formally employed during the calendar year. There are thus incentives for truthful reporting. RAIS has also been increasingly used by ministries administrating other programs —UI or conditional cash transfers— to monitor formal job take-ups. There are (rarely issued) fines for firms that fail to report. The Labor Ministry estimates that information on more than 90% of formal workers were reported throughout the 1990s (Muendler, 2008). Moreover, RAIS has a better coverage than the data used by the UI agency to monitor beneficiaries' labor status. We can currently access RAIS data from 1994 to 2002, although the 1994 data does not identify the reason for separation. In that period, RAIS covers more than 30 million formal workers a year.

Every observation in RAIS is a worker–establishment pair in a given year. Workers, firms and establishments are uniquely identified over time and geographical areas. Every observation includes the following variables: (i) hiring and separation months and reason for separation; (ii) average monthly earnings and December earnings; (iii) sector (2 and 3 digits) codes; (iv) tenure when an employment relationship ends and as of December 31st (ex: tenure=6.1 months); (v) location of the establishment (municipality); (vi) contract type, contracted hours and legal form of the establishment; (vii) education, age, gender and nationality. The Appendix details how we use the data to recover duration between two formal jobs.

Not having access to actual UI records, we can only precisely assess UI eligibility for the subsample of workers with indeterminate–length contracts, more than 24 months of tenure in the lost job and recorded as involuntarily laid off.[19] We also restrict our attention to private sector employees, as public employees are not eligible. Those workers are eligible for 5 months of UI benefits. Any

[19] Individuals with less than 24 months of tenure might be eligible for 5 months of UI instead of 4 if they accumulated tenure in a previous job without collecting benefits in between. Individuals with less than 16 months of tenure might not be eligible at all if they collected benefits before starting in this job.

impact estimated captures Intention–To–Treat as we do not observe the actual take–up of UI.[20] Workers with more than 24 months of tenure are clearly selected but are also more likely to value UI as they have adjusted their expectations to being formally employed.

B. Urban labor force surveys (PME)

Brazilian urban labor force surveys (PME, 2003–2010) have the same structure as the CPS. Households enter the sample for 2 periods of 4 consecutive months (interviewed in the same week of the month) 8 months apart from each other. PME covers the six largest urban areas of Brazil and is used to compute official unemployment rates in Brazil. Each survey asks for labor market status of every household member above 10 years old, information on monthly wage and tenure in the job. A specific question asks if the respondent's working card was signed by her employer in the current job, a definition of formality in Brazil and a condition to appear in RAIS. Moreover, individuals who do not report any employment are asked about their labor status and tenure in the last job, the reason for separation and the length of their non–employment spell.[21]

3.3 Background

A. Informality prevalent.

As in many developing countries, a large share of the labor force in Brazil is working as self–employed (mostly undeclared) or informal employees. From 2003–2010, only 55% of working male 18–54 years old were formal employees. This pattern holds broadly across demographic groups (70% of working male young college graduates are formal employees —own calculations using PME). Firms in our data are also likely to employ unregistered workers. Fines for doing so are high (about 2.6 minimum wages) but the risk of detection is very low (Cardoso and Lage, 2007). According to the 2002 World Bank's Investment Climate survey in Brazilian manufacturing, only 70% of firms with more than 100 employees and 35% of firms with less than 20 employees were inspected at least once in the previous year. Asked about the share of unregistered workers a similar firm would employ, the same survey reveals a median answer of 30% for small firms (own calculations).

B. Formal jobs short–lived.

[20] There is no available take–up statistics. If we divide the number of new beneficiaries by the number of eligible layoffs with more than 6 months of tenure in the previous month in RAIS, we obtain a figure close to 95%. Very few UI beneficiaries have less than 6 months of tenure in their previous job according to official statistics (Marinho, 2010). Such an exercise is only suggestive but others estimate similarly high take–up rates (Hijzen, 2011). From Section 2, the mechanical effect corresponds to $S(P)$, the share of job losers surviving outside the formal sector up to period P. If take–up was only $0 < M < 1$, then the relevant mechanical effect would be $MS^M(P)$ where $S^M(P)$ is the share of UI takers surviving outside the formal sector up to period P. We expect $S^M(P) > S(P)$, therefore given the likely high take–up, any bias is probably very small. We have no way to estimate $S^M(P)$ directly. Moreover, non–takers are likely to value insurance less. Therefore, if the mechanical effect may be smaller in the presence of imperfect take–up, the social value of providing UI is likely to be higher and the effect on welfare is thus ambiguous.

[21] Labor force surveys were greatly improved in 2002.

Private formal jobs tend to be less stable in poorer countries.[22] Tenure levels are lower, separation rates higher and layoff rates much higher in Brazil than in the US.[23] Most formal workers in Brazil are young with low levels of education and tenure, a population associated with less stable employment (Maloney, 2003). Goods and services produced in the formal sector are also likely to face more volatile (mainly foreign) demand in developing countries. In addition, particular institutions of the labor market —firing costs increasing with tenure and severance payment accounts only accessible upon layoff— may also cause high rates of experimentation on the employer side and a higher propensity to record a job–separation as a layoff (Gonzaga, 2003). We find suggestive evidence that layoff rates may respond to UI eligibility. Yet, existing institutions appear sufficient to prevent such response for the sample of workers we focus on (see Section 4.3).

4 The Impact of UI extensions

In this section, we use our administrative data to recover hazard rates of formal reemployment and survival rates outside the formal sector for workers eligible for 5 months of UI. Hazard rates more than double after benefit exhaustion but are generally low. We then estimate the impacts of UI extensions by exploiting two complementary sources of exogenous variation. We investigate both program outcomes, in particular the mechanical and behavioral effects of UI extensions, and longer–term outcomes on formal employment up to 2 years after layoff. Finally, we summarize our results and compare them with existing estimates from the literature.

4.1 Formal reemployment after layoff

Using the hiring, separation and tenure information in our data, we can recover formal reemployment hazard rates, and survival rates out of the formal sector.[24] Figure 3a presents monthly formal reemployment hazard rates for a group of workers eligible for 5 months of UI benefits after a waiting period of 30 days following layoff. Aggregate hazard rates are higher during the waiting period (month 0), then much lower when UI benefits can be collected. Strikingly, hazard rates more than double after benefit exhaustion (month 5) and stay higher for a few months. This spike suggests that UI affects job–search behavior, a behavioral effect we estimate in the next section. Moreover, no such spike is observed for workers ineligible for UI.[25] We are the first to document this pattern

[22] See Kaplan et al. (2007) and Schaffner (2001) for a similar point in Mexico and Colombia, respectively.

[23] 20%–25% of formal male workers have more than 10 years of tenure in Brazil; the corresponding figure is around 35%–40% in the US (own calculations using RAIS and estimates from Farber, 2010). Monthly separation and layoff rates are around 3% and 1.1% in the US, respectively. The corresponding figures in Brazil are 4% for separations and 3% for layoffs (own calculations using RAIS and estimates from Davis et al, 2006).

[24] Codes to construct the duration data are available upon request.

[25] For example, we find no such spike for public sector employees, private sector workers with small tenure levels, and private sector workers laid off for justified reasons (available upon request).

in Brazil; such a large spike is not observed in typical developed countries.[26]

However, the behavioral effect is likely to be small compared to the mechanical effect of extending UI. Indeed, formal reemployment rates as recorded by government agencies are very low in Brazil. As shown on Figure 3b, 80% of our sample exhaust their 5 months of UI benefits. Moreover only 50% find a new job as formal employee within the year.[27] Such a pattern is mostly unrelated to UI as low reemployment rates in the formal sector are also observed after benefit exhaustion and for workers ineligible for UI.[28] This pattern is not limited to Brazil: in Argentina, 80% of UI beneficiaries eligible for 4 months of UI do exhaust their benefits (IADB, in progress).[29] Formal reemployment rates are lower for female, older and less educated workers but are low even for young male college graduates (not shown). Providing UI in this context is costly but efficiency costs are likely to be small. A significant share of UI exhaustees may be reemployed in jobs undetected by the UI agency (something we estimate in Section 5) but UI appear to have little influence on their decision to do so.

4.2 A politically–motivated temporary UI extension

In August 1996, a temporary legislation increased maximum benefit duration by 2 months in the existing *official metropolitan areas*[30] of Brazil. Additional benefits could only be collected between September 1st and December 31st. This relatively short window of time prevents manipulation of the pool of workers eligible for the extra benefits. In sum, any worker who exhausted her regular UI benefits before November 1996, and was not reemployed in the formal sector, could receive up to 2 additional months of UI benefits. The timeline is illustrated on Figure 4.

Media coverage reveals a clear electoral motive behind the temporary legislation. 1996 was a local–election year with rounds in October and November. An extension of UI limited to São Paulo was proposed to the President on August 14th by José Serra. A former minister, he was running for mayor of São Paulo and argued that unemployment was rising. One day later, the

[26] Van Ours and Vodopivec (2006) find a similar spike in Slovenia. Ferrada (2011) finds some evidence of such a spike in Chile and IADB (in progress) in Uruguay. Card et al. (2007) reviews the literature in richer countries.

[27] In this sense, the Brazilian context is very different from the Slovenian one analyzed in Van Ours and Vodopivec (2006). In their sample, similar workers are nearly all securing a formal job within a year.

[28] Public sector employees, private sector workers with small tenure levels, and private sector workers laid off for justified reasons (available upon request).

[29] In China, most beneficiaries exhaust their UI benefits too (Vodopivec and Tong, 2008). Personal communications with Eric Verhoogen reveal that in similar administrative Mexican data (IMSS), about 35% of workers aged 36–45 have only spent half of a 12–years window in formal employment.

[30] *(...)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 of the authors.

Labor Minister called a meeting of the board managing the UI fund. At the meeting, the three worker representatives defended a UI extension for all Brazilian cities, arguing that "*unemployment is increasing everywhere, not only* (referring to Serra) *where the PSDB candidate is doing badly* (Folha de São Paulo, August 22^{nd}, 1996)." This proposal was denied because a national extension would have required Congressional approval, as it would have cost more than the legal threshold for speedy approvals of UI extensions. An extension restricted to the 9 official metropolitan areas and the Federal district was agreed on as a compromise.

Arguably, the 1996 temporary UI extension was thus politically motivated, unanticipated and its geographical restriction was not related to local labor market conditions. Nevertheless to reinforce the exogeneity of our cross–sectional variation, we drop São Paulo from the sample of analysis. Unemployment rates were higher in 1996 than in 1995 but not than in 1997. Benefits were not extended in such a way in any other year with higher unemployment rates until 2009. This natural experiment creates exogenous variation in treatment across periods and geographical areas, calling for a difference-in–difference (diff–in–diff) strategy. Potentially affected workers learned about the treatment shortly before regular benefit exhaustion. We can thus estimate the causal impact of maximum benefit duration on the spike in formal reemployment at benefit exhaustion.

4.2.1 Sample selection

In our matched employee–employer data, we can only precisely assess UI eligibility for workers with more than 2 years of tenure (see Section 3.2), so we limit our sample to this group. To minimize information issues and maximize take–up of the extra benefits, we also restrict attention to workers who would have collected their last UI payment in September or October in absence of a temporary extension. Indeed, those workers could have collected two additional months of benefits in 1996 without experiencing any gap in UI payments. We use both 1995 and 1997 as control years. Since 1996, other urban centers have received the status of metropolitan area. We use them as control group.[31] We have 9 treated areas (excluding São Paulo) and 20 control areas. In short, our sample includes any worker aged 18 to 54, with more than 2 years of tenure in their lost job (eligible for 5 months of regular UI) who lost a private formal job in April or May 1995, 1996 or 1997.

Tables A.1 and A.2 in the Appendix present the distribution and composition of our sample across treatment–control areas and years. We have about 240,000 workers. Treatment areas represent 70% of the sample as treatment was assigned to larger urban areas. Treatment areas have a larger service sector, a smaller industrial sector and more college graduates. Average age is also higher in treatment areas. But those differences appear in both control and treatment years. About 67% of our sample is male, mean age is 33 years old, education levels are low with nearly half the

[31] Many urban areas were awarded the status of metropolitan area between 1996 and 2002. Given that the set of municipalities composing any given metropolitan area has been modified over the years, we only consider municipalities that were part of a metropolitan area in 1996 or when the status of metropolitan area was first granted.

sample having not completed 8th grade, and mean replacement rates are around 50%.

4.2.2 Graphical evidence

Impacts of the temporary UI extension can be seen graphically. Figure 5 depicts survival rates out of formal employment. Survival functions in control areas and years trace each other closely, with survival rates slightly lower in 1995. In 1996, the survival function in treatment areas departs from the other ones after September. This results in an increase in the survival rate 7 months after layoff (when the last extended benefit is collected) of about 6.8 percentage points. Figure 5 supports our identification strategy: except for the treatment period, trends are similar across years and areas.

Treatment impacts on hazard rates of formal reemployment are visible on Figure 6. In 1996, the spike around regular benefits exhaustion[32] disappears in treatment areas. Hazard rates are lower until extended benefits are exhausted. The spike is thus entirely due to the UI program. We estimate below aggregate formal reemployment rates to be 50% lower in the 2 months between exhaustion of regular and extended benefits. Using the sample of workers laid off in May (who learned about the extension at least one month before exhausting their regular benefits), we can estimate reductions in reemployment rates in anticipation of the additional benefits. In month 4 after layoff, their reemployment rate is 19% lower on average in our preferred specification. In the Appendix, we present additional graphical evidence and some robustness checks. In particular, a similar shift in formal reemployment hazard rates is also visible for temporary layoffs but not for a similar sample of workers laid off later in the same years or laid off in the same months in the next local–election year. Moreover, hazard rates of formal reemployment six months after layoff (the spike) in each metropolitan area in control and treatment years are only statistically different from each other in treatment areas and they are so in every one of them. Our treatment impact is not confounded with labor market size as one treatment (resp. control) area —Belém (resp. Campinas)— was smaller (resp. larger) than some control (resp. treatment) areas.[33]

4.2.3 Benefit collection outcomes

We display regression results for three sets of benefit collection outcomes in Table 1: (i) survival rates 5 and 7 months after layoff corresponding to collection of the last regular and the last extended benefits (S_5, S_7), (ii) mean covered duration for regular UI benefits conditional on take–up ($\sum_{t=1...5} S_t/S_1$) and mean covered duration of the 2 extended UI payments conditional on exhaustion of regular benefits ($\sum_{t=6...7} S_t/S_5$), and (iii) aggregate monthly reemployment hazard rates between exhaustion of regular and extended benefits (h_5, h_6) for the whole sample and also prior to regular benefit exhaustion (h_4) for the May sample.

[32] Due to the 30–days waiting period, individuals who survive until month 5 after layoff, collect their last UI payment at the beginning of that month.

[33] Figures A.2, A.3, A.4 and A.5, respectively.

For the first two sets of outcomes, we estimate the following OLS specifications for individual i, in metropolitan area m and year t:

$$y_{i,m,t} = \alpha_m + \beta Year1996_t + \gamma \left[Year1996_t \bullet TreatArea_m\right] + \delta X_{i,m,t} + \epsilon_{i,m,t} \tag{6}$$

where α_m is a metropolitan area fixed effect and $X_{i,m,t}$ is a full set of flexible controls for age, tenure, log wages, education, sector and gender. γ captures a diff–in–diff estimate for a given outcome y and is reported in Table 1. $\epsilon_{i,m,t}$ is an individual error term.

For the aggregate monthly hazard rates, we estimate the following logit specifications:[34]

$$log\frac{h_{i,m,t}}{1-h_{i,m,t}} = \alpha_m + \beta Year1996_t + \gamma \left[Year1996_t \bullet TreatArea_m\right] + \delta X_{i,m,t} + \mu_{i,m,t} \tag{7}$$

where h is an indicator for being reemployed in the month considered (4^{th}, 5^{th} or 6^{th} month since layoff) if not formally reemployed before then. $\mu_{i,m,t}$ is an individual error term.

We present specifications varying the control years and the set of controls, and we cluster standard errors by metropolitan areas. Table 1 reveals that 67% of the sample is still out of formal employment 7 months after layoff in control areas in 1996. Using both control years and the full set of controls, we estimate an increase of about 6.8 percentage points due to the temporary extension. If two additional UI payments had been offered in control areas in 1996, the average worker exhausting regular UI benefits would have mechanically collected 1.75 additional UI payments. We estimate an increase of about .12 additional UI payments in response to the temporary extension. The behavioral effect thus amounts to less than 7% of the increase in mean covered duration. Those estimates are very robust across specifications and highly significant. Because workers learned late about the temporary extension, we find no significant effect on exhaustion of (or mean covered duration for) regular UI benefits.

Estimates from our hazard model are also very robust and significant across specifications. We estimate a percentage reduction in aggregate hazard rates of about 52%–54.5% (baseline .07–.0892) when additional benefits can be collected.[35] Additionally for the sample of workers laid off in May, we estimate reductions in aggregate hazard rates in month 4 —in anticipation of any extended benefit— of about 18.7% (baseline .04). This latter estimate is larger when using 1995 (24%) and lower when using 1997 (10%) as control years. Our estimates of the behavioral effect are thus lower–bounds as individuals could not adjust behavior directly upon layoff. Our second empirical strategy allows us to recover such anticipated responses.

In the Appendix, we replicated these results (i) collapsing data at the area–month level,[36] (ii) through a triple–difference strategy including workers laid off in July, August and September in the same years and the same areas, and (iii) limiting our sample to workers with replacement rates

[34] They are equivalent to standard proportional hazard models when hazard rates are low as in Brazil.

[35] Because baseline rates are low, we can approximate percentage changes as $1 - exp(\gamma)$.

[36] We have very unbalanced clusters.

above 50% as they have more incentives to collect UI benefits. Results are very similar: estimated impacts are slightly larger when collapsing the data or restricting the sample to individuals with higher replacement rates but are smaller with the triple–difference strategy (Table A.3). Performing our OLS specifications separately for different categories of workers also reveal that the temporary extension had an effect for each category (gender, age, education, sector). Estimates are smaller for older and more educated workers (Table A.4).

4.2.4 Reemployment outcomes

We present results on longer–term reemployment outcomes applying the same OLS specifications in Table 2. The first 3 columns show results for the whole sample and the last 3 columns for the sample of workers potentially affected by the UI extension (not reemployed by September).

As seen on Figure 5, survival rates in treatment areas in 1996 did not catch up immediately with survival rates in control years after exhaustion of the additional benefits. Moreover survival rates in control areas in 1996 decrease faster in later months than survival rates in control years. Therefore, we estimate very significant longer–term impacts of the temporary extension on the sample of affected workers.[37] Restricting attention to the 2 years following layoff, we estimate a 1.86 percentage point decrease in the share ever formally reemployed (baseline 63%), a .70 month increase in the duration out of formal employment (baseline 13.65 months), and a .47 month decrease in the number of months of formal employment in the 2 years following layoff (baseline 8.06 months, column 4). Given that workers must be formally reemployed to be laid off again from the formal sector, we also estimate a decrease of about 1.71 percentage point in the share of workers experiencing at least one new layoff. Our estimates imply a positive impact of .0652 month on the subsequent average duration of formal employment before layoff (corresponding to D^F in our model), using our preferred specification (column 4).[38] Therefore, at least when layoff risk is high, there is no evidence that UI extensions decrease average contributions to the UI budget per UI spell.[39] Finally, we find no evidence of an impact on log real wage if employed in December 31$^{\text{st}}$, 2 years after layoff.[40]

4.3 Tenure–based discontinuities in UI eligibility

Because workers learned late about the temporary UI extensions, the empirical strategy above does not allow us to estimate impacts of UI extensions on formal reemployment rates directly upon layoff. To estimate such anticipation behaviors, we turn to a second empirical strategy.

[37] We interpret these results with caution as the common–trend assumption is a stronger assumption in the long–run.

[38] Using mean values for the control group in 1996 as baseline, this is obtained as: $\frac{8.061-.4708}{.2824-.0171} - \frac{8.061}{.2824}$.

[39] Results collapsing data at the area–month level, adopting a triple difference strategy or restricting the sample to workers with replacement rates above 50% are very similar and available upon request.

[40] We also found no evidence of an impact on other characteristics of the first new job (available upon request).

Tenure–based discontinuities in maximum benefit duration in Brazil potentially provides us with a regression discontinuity design. A necessary condition for such an identification strategy to be valid is that the forcing variable (tenure at layoff) must be continuously distributed across the relevant tenure threshold (6, 12 and 24 months of tenure). As shown in Figure 7, this tenure density is not continuous and varies systematically with some institutions of the labor market. In particular, it is not continuous across the first 2 relevant tenure thresholds for UI eligibility (6 and 12 months). The upward jump in the density at the 6–month threshold might be due to the absence of experience rating of UI benefits in Brazil: UI eligibility gives employees and employers a surplus to negotiate over in case of layoff.[41] The discontinuity around the 12–month threshold 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 over the layoff process). Firms clearly react to changes in firing costs by adjusting their layoff decisions. We can apparently exploit (verified below) the 24–month tenure threshold as the tenure density at layoff is continuous beyond 1 year of tenure. 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 may be sufficient to prevent responses on the layoff margin even in the absence of experience rating.

4.3.1 Sample selection

Although we have information on the whole history of formal employment starting in 1994, we do not observe if a worker collected UI benefits in the past and we can thus only imperfectly assess eligibility for workers with less than 2 years of tenure (see Section 3.2). We can avoid this issue and the fuzziness it induces in our design by focusing on (selected) workers laid off from 1997 on, and who did not appear in another job in previous years.[42] For those workers, only tenure in the lost job defines the UI benefits they are eligible for. Our sample of analysis consists of any worker aged 18 to 54, involuntarily laid off by a private formal firm between 1997 and 2000 from the same 30 metropolitan areas considered in our first empirical strategy (São Paulo included). We include workers who had between 15 and 36 months of tenure at job–loss.[43] We also consider a smaller tenure window (18–30 months). Individuals with tenure levels below (resp. above) the 24–month threshold are eligible for 4 months (resp. 5 months) of UI. According to the 1994 UI legislation, a partial month of tenure may count as a full month for UI eligibility purpose. Therefore we exclude

[41] The tenure distribution at separation in the case of quits (not shown) is perfectly smooth over its whole support.

[42] Because maximum benefit duration depends on the number of months of formal employment in the 36 months prior to dismissal, we cannot sample workers laid off before 1997 (we need at least 3 years of data prior to the layoff). We do not consider layoffs occuring after 2000 because we want to follow individuals up to 2 years after layoff.

[43] The slope of the tenure density changes closer to the 12–month threshold on Figure 7, suggesting selection.

workers with tenure levels between 23 and 24 months from the analysis.[44] Finally, as we do not observe the actual take–up of benefits, we estimate the reduced form effect of being eligible for an additional month of UI benefits.

4.3.2 Graphical evidence

Figure 8 presents formal reemployment hazard rates for 2 samples of workers with tenure levels at layoff just below and just above the 24–month threshold. In both cases, hazard rates are low while collecting benefits, but jump up after benefit exhaustion. Formal reemployment hazard rates are visibly lower in months 2, 3 and 4 after layoff for workers eligible for a 5$^{\text{th}}$ month of UI benefits. Figure 9 presents graphical evidence for the effect of an extra month of UI at the tenure discontinuity. Panel (a) shows that aggregate formal reemployment hazard rates, just prior to collection of the extra benefit, jump down at the tenure threshold by about 44%. Panel (b) reveals that survival rates out of formal employment are higher by about 2.5 percentage points in month 5 after layoff when individuals can collect their extra benefit.

4.3.3 Benefit collection outcomes

Let T_i be the tenure level at layoff falling in tenure bin b. We obtain estimates below by regressing a variable of interest for individual i (y_i) on a constant, an indicator for having tenure levels above 24 months ($1(T_i \geq 24)$) and two parametric polynomials in tenure, one on each side of the tenure threshold ($f_1(T_i), f_2(T_i)$):

$$y_i = \alpha_0 + \beta_1 1(T_i \geq 24) + f_1(T_i) + 1(T_i \geq 24) f_2(T_i) + \delta X_i + \epsilon_i \tag{8}$$

X_i includes controls for age and log wages (4$^{\text{th}}$ order polynomials), gender, education, sector, metropolitan areas, and separation month (dummies). ϵ_i is an individual error term. The impact of being eligible for an additional month of UI benefits is captured by $\beta_1 + f_2(24)$. We display results for two sets of benefit collection outcomes in Table 3: (i) survival rates 1, 4 and 5 months after layoff corresponding respectively to take–up of benefits (given the 30–days waiting period), and potential collection of a 4th and a 5th month of UI benefits (S_1, S_4, S_5), and (ii) mean covered duration conditional on take–up for UI offering up to 4 and 5 months of benefits ($\sum_{t=1...4} S_t/S_1$, $\sum_{t=1...5} S_t/S_1$). In the Table, we consider polynomials of degrees 1 and 2 for a tenure window of 15–36 months and of degree 1 for a smaller tenure window (18–30 months).[45] Standard errors are clustered by quarter–month tenure bins.

[44] Including them reduce our estimated impacts as workers with those tenure levels tend to behave as being eligible for 5 months of UI (see Figure 9).

[45] We do not present results with higher orders given the risk of over–fitting with such small windows and the fact that the Akaike Information Criterion is higher when we increase the degree of the polynomial.

In Figure 10, we also present graphically OLS results for survival rates at each month since layoff as well as results from the following logit model for hazard rates (we chose the specification leading to the largest estimated impacts as our point is that impacts are low):

$$log\frac{h_i}{1-h_i} = \alpha_0 + \beta_1 1(T_i \geq 24) + f_1(T_i) + 1(T_i \geq 24)f_2(T_i) + \delta X_i + \mu_i \tag{9}$$

where h is an indicator for being reemployed in the month considered (month 0 to 12 after layoff) if not formally reemployed before then. μ_i is an individual error term.

Figure 10 reveals that workers eligible for a 5$^{\text{th}}$ month of UI have slightly lower aggregate reemployment hazard rates from the first month they collect benefits and the percentage reduction increases throughout the spell: about 18.5% in months 2 and 3 since layoff and about 44% in month 4 since layoff, prior to collecting the extra UI payment. Those large reductions in aggregate hazard rates translate into a small increase in the share of workers collecting the extra benefit of about 2.5 percentage points (S_5). More systematically, Table 3 shows no effect on benefit take–up and small significant effects on collection of the 4$^{\text{th}}$ month of UI. The increase of 2.5 percentage points in collection of the 5$^{\text{th}}$ month of UI is robust across specifications and very significant. Including covariates has no effect on the coefficients. Those effects result into small significant behavioral effects on mean covered duration up to 4 and 5 months of about .014 month and .042 month, respectively. The latter amounts to 5% of the total increase in mean covered duration.

We present heterogeneous impacts on mean covered duration in the Appendix (Table A.5). Treatment effects are found for each category and are larger for individuals with higher replacement rates and lower levels of education. We find a surprisingly large effect for older workers, but because of our sample selection, those older workers are a small and very selected group.

4.3.4 Sample description and validity of the design

To check the validity of the regression discontinuity design, we systematically test for the absence of any discontinuous change in the number (aggregating observations by quarter–month bins) and composition (gender, age, education, log wages, replacement rates, and sectors) of observations on each side of the discontinuity. We use the same OLS specifications as above. Results are presented in Table 4. The sample differs from the sample for the temporary UI extension: it has more female workers, younger individuals, less workers with very low education levels, less workers in the industrial sector, and higher replacement rates. Estimates using quadratic tenure controls or the shorter tenure window support the validity of the design even though specifications using the larger tenure window and only linear tenure controls reveal some significant differences. Those latter specifications being less flexible, they are more likely to be affected by observations further away from the discontinuity. We thus interpret Table 4 as an overall validation of our empirical strategy. Moreover, our results of interest on Table 3 are similar with the different specifications and with or without inclusion of the set of controls.

4.3.5 Reemployment outcomes

We present results on longer–term reemployment outcomes applying the same OLS specifications in Table 5. Extending UI benefits led to longer–term impacts. Restricting attention again to the 2 years following layoff, we find a .18 month increase in the duration out of formal employment (baseline 15.33 months), and a .12 month decrease in the number of months of formal employment in the 2 years following layoff (baseline 6.2 months, not significant at conventional levels). We also estimate a decrease of about .8 percentage point in the share of workers experiencing at least one new layoff from the formal sector (baseline 27.5%). Our estimates imply a positive impact of .22–.35 month (baseline 22.55 months) on the subsequent average formal employment duration before layoff (corresponding to D^F in our model).[46] Therefore, once again, we find no evidence that UI extensions decrease average contributions to the UI budget per UI spell. We find some evidence of a positive impact on log real wage if employed on Dec 31st 2 years after layoff. However those results are too sensitive to specifications and to the inclusion of controls to be fully conclusive (the same is true for characteristics of the first new job— results available upon request).

4.4 Summary of results

We now summarize and compare our results with the existing literature.

4.4.1 Benefit collection outcomes

Even though contexts and sample selections differ in some important respects, UI extensions in our two empirical strategies have very similar impacts on the shape of the hazard function of reemployment in the formal sector. The spike at benefit exhaustion is entirely due to the UI program and individuals are forward–looking. Several papers (Card and Levine, 2000; Card et al., 2007a; Schmieder et al., 2011) have found evidence of anticipation behaviors in developed countries.

As derived in section 2, the relevant impact for the UI budget and for welfare is the change in mean covered duration, and to which extent it is due to a behavioral rather than a mechanical effect. We could estimate the mechanical effects by observing survival rates for workers unaffected by the UI extensions. We directly estimated the behavioral effect in our regression discontinuity design. Our estimates of the behavioral effect in the temporary UI extension are lower–bounds, as they do not allow for behavioral responses directly upon layoff. Therefore, we simulate how mean covered duration would have changed had workers known about the UI extension upon layoff. In particular, we extrapolate estimates of anticipation behavior we obtained with the May sample, on hazard rates from the start of the UI benefits collection period (as in Card and Levine, 2000).

[46] Using mean values for workers with tenure levels just below the 24–month threshold as baseline and applying our estimates from the last 3 columns of Table 5. For the last column of Table 5, this is obtained as: $\frac{6.204-.1164}{.2751-.0079} - \frac{6.204}{.2751}$.

To gauge the role of anticipation, we also present results mimicking the actual timing of the 1996 temporary policy. Results from the simulations are displayed on Table 6.[47]

Formal reemployment rates being low, the mechanical effect is large: the control group would have collected an extra 1.555 UI payment if benefits had been extended by 2 months. The behavioral effect (with anticipations) implies a .1 month increase in mean covered duration when benefits are extended by 1 month, or about 11% of the overall increase. The behavioral effect estimated in the regression discontinuity analysis is smaller (.043 month or about 5% of the increase in mean covered duration) because formal reemployment rates are smaller in this sample. Both empirical strategies reveal that large percentage changes in formal reemployment hazard rates have only limited effects on mean covered duration, in particular compared to the mechanical effect. Few papers actually estimate impacts on mean covered duration. For the US, Katz and Meyer (1990) estimates an increase in mean covered duration of 2.1 to 3 weeks when 13 additional weeks are offered. The marginal behavioral effect in their study ranges from .092 to .132 month (43% of the increase is due to a mechanical effect). Therefore if the behavioral effect in absolute terms does compare to our estimates, the striking difference is that it would amount to 57% of the total increase in mean covered duration. Card and Levine (2000) studies a temporary UI extension in New Jersey in 1996 but only estimates the impact on mean covered duration of regular benefits and therefore only estimates anticipated responses to increased benefits. They obtain that a 13–week UI extension increases mean covered duration of regular benefits (the first 26 weeks) by 1 week, or a marginal effect of .077 month. This is much larger than our estimates (.014 or .025 depending on the empirical strategy) but consistent with the estimates from Katz and Meyer (1990). Schmieder et al. (2011) uses variations in maximum benefit duration in Germany for workers above 42 years old. They estimate that increases in mean covered duration range from a third to a fourth of the increase in maximum benefit duration. This is similar to what Katz and Meyer (1990) finds for the US and much smaller than our estimates but benefits are offered for a longer period in Germany. Unfortunately, they do not report the share of the increase due to behavioral responses.

4.4.2 Longer–term outcomes

We estimated impact of UI extensions on overall duration outside the formal sector 3–4 times larger than the effect on mean covered duration. This is because reemployment rates did not increase

[47] As the regression discontinuity analysis reveals that there is no sign of an effect in the first month of benefit collection, such an extrapolation may actually overestimate the behavioral effect on mean covered duration. The Baseline scenario is obtained through non–parametric estimation of the hazard function for the control group in 1996. We then apply our estimates for the impact of a UI extension on aggregate reemployment rates and recover survival rates and mean covered duration. In the tables, we study mean covered duration conditional on take–up as it is similar to what authors have studied in the US but the relevant statistics is actually unconditional mean covered duration. Using unconditional mean covered duration instead slightly reduces our estimates.

sharply enough after exhaustion of the extended benefits for survival rates to recover rapidly. In absolute terms, this is much larger than estimates from Schmieder et al. (2011, marginal effect .1–.13 month) for Germany or Card et al. (2007a, marginal effect .1 month) for Austria. However, it is actually smaller in terms of elasticity because average duration between 2 formal jobs is also longer in Brazil. Nevertheless, it is interesting that workers delaying formal reemployment in response to UI extensions do not reappear in the formal sector right after exhaustion of the extended benefits, as anecdotal evidence would have suggested. We even find some negative impact on the probability to be ever formally reemployed in the 2 years after layoff. This result is only significant for the 1996 temporary extension and it relies on the common–trend assumption to be valid in the long–run, but estimates imply that a marginal UI extension announced shortly before exhaustion of regular benefits reduces this probability by about 1.4% (point estimates for the regression discontinuity range from .5%–1.5%). Importantly, because the impact on overall duration is larger than the impact on covered duration, this is not due to moral hazard while collecting benefits. We cannot provide much insight on the underlying mechanisms but our results echo an argument that UI might help transit to self–employment in Brazil where starting capital is small (Cunningham, 2000).

A larger duration out of the formal sector reduces the time spent formally employed and the share experiencing a new layoff from the formal sector. Combining both results, we find non–negative impacts on average formal employment duration before layoff, the relevant margin for UI revenues. This is consistent with simple theories (see section 2) but we are not aware of any paper actually estimating this statistics. Finally, we do not find conclusive evidence of an effect of UI extensions on subsequent job characteristics in the formal sector. This is also the case in Card et al. (1997), Schmieder et al. (2011) and van Ours and Vodopivec (2008).

5 Labor status of UI exhaustees

So far, we estimated that efficiency costs from UI extensions were small. In the normative framework of Section 2, this implies that the social value of transferring income to UI exhaustees need not be high for a UI extension to increase welfare. However, in a context of prevalent informal employment and imperfect monitoring of labor status, many UI exhaustees may be reemployed informally or self–employed reducing the need for income support. In this section, we thus estimate the share of UI exhaustees actually unemployed using Brazil's monthly urban labor force surveys (PME) from 2003 to 2010. Using 2 consecutive interviews, we can estimate the job–finding probability in the subsequent month given respondents' duration in non–employment.[48] We estimate transitions out of non–employment by maximum likelihood for individuals laid off from a formal job with more than 2 year of tenure (for comparability with the samples used in Section 4). We then use our

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

results to estimate the share still unemployed by the time they exhaust UI benefits.[49]

In the surveys, respondents are asked about month since layoff (and not number of weeks as in CPS). Therefore we assume that the hazard function out of non–employment is piece–wise constant over each 30–days period. We define λ_m as the daily hazard rate constant over a 30–days period, $m = 0..10$ months since layoff. We also have to correct for a stock sampling issue within month. Suppose a respondent is interviewed on day $b \in [0, 30]$ within month m since layoff. The probability that she is found reemployed in the subsequent interview is thus:

$$P\left(30m + b < T \leq 30m + 30 + b\right) = 1 - exp\left(-\left(30 - b\right)\lambda_m - b\lambda_{m+1}\right)$$

Moreover, she can only be observed on day b if she survived b days without a job, given that she survived already m months. If we define $k(b)$, as the distribution of interviews over days within a month, the probability that she is surveyed on day b if unemployed within month m is:

$$P\left(b|Unemployed, m\right) = \frac{k\left(b\right) exp\left[-b\lambda_m\right]}{\int_0^{30} k\left(s\right) exp\left[-s\lambda_m\right] ds}$$

Define $d_{i,m} = 1$ if individual i unemployed since month m is reemployed in the subsequent interview. The likelihood for a given observation is thus:

$$\begin{aligned} L_{i,m} = d_{i,m} \int_0^{30} \left[1 - exp\left(-\left(30 - b\right)\lambda_m - b\lambda_{m+1}\right)\right] \frac{k\left(b\right) exp\left[-b\lambda_m\right]}{\int_0^{30} k\left(s\right) exp\left[-s\lambda_m\right] ds} db \\ + \left(1 - d_{i,m}\right) \int_0^{30} \left[exp\left(-\left(30 - b\right)\lambda_m - b\lambda_{m+1}\right)\right] \frac{k\left(b\right) exp\left[-b\lambda_m\right]}{\int_0^{30} k\left(s\right) exp\left[-s\lambda_m\right] ds} db \end{aligned} \quad (10)$$

In the estimations, we assume that interviews are uniformly distributed over the month, $k(b) = 1/30$. For simplicity, we also set $\lambda_i = \lambda_{i+1}$ for $i = 1, 3, 5, 7, 9$ such that we only estimate 6 parameters.[50] Once point estimates for the parameters are obtained, the probability to be unemployed 5 months after layoff is:

$$P\left(T \geq 30 * 5\right) = exp\left(-30(\lambda_0 + 2\lambda_1 + 2\lambda_3)\right)$$

Estimations are performed using the sampling weights and clustering standard errors at the individual levels. Table 7 presents our results. Point estimates for job–finding rates hazard rates are higher early in the non–employment spell in particular in the first month. Without our stock sampling correction we would have thus underestimated job–finding rates. Our results imply that 35% of workers are still unemployed by the time they exhaust UI benefits —a figure comparable to

[49] Samples are representative of the overall labor force in the six largest metropolitan areas in Brazil, which does not guarantee that they are representative of the non–employed labor force with more than 2 years of tenure in the last job.

[50] The whole purpose of the stock sampling correction is to avoid underestimating the hazard rate in the first 30 days after layoff. We also performed our estimation without this assumption: estimates typically alternate a low and high value of the lambda's but results for the share still unemployed at benefit exhaustion are unchanged.

UI exhaustion rate— and 29% one month later. Thus not every UI beneficiary is working informally while collecting benefits, but a significant share may be doing so. 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.

Another way to inform about the social value of transferring income to UI exhaustees is to consider net earnings and disposable income for formal employees and potential UI exhaustees.[51] Table 8 shows that average monthly earnings are about R$744 for formal workers with more than 2 years of tenure. Workers who did find a new job as formal employees before benefit exhaustion are earning R$448 in their first month of employment. Workers who did find a new job outside the covered formal sector are earning R$150 less and enjoying R$90 less in disposable income or about half the level of disposable income of formal workers with more than 2 years of tenure. The average level of disposable income of the unemployed around benefit exhaustion is about a third of the level of disposable income of formal workers with more than 2 years of tenure, but 30% had no other income at all in the household. We do not have information on actual consumption and saving levels, but smoothing consumption in case of income shocks tends to be more difficult in developing countries (see Chetty & Looney, 2006, for employment shocks). Therefore Table 8 suggests that the social value of transferring income to UI exhaustees is likely to be positive.

We combine these results with our estimates of efficiency costs to discuss welfare in the next section.

6 Welfare effects of UI extensions

We estimated that UI extensions have a large impact on mean covered duration but mostly through a mechanical effect. Therefore, efficiency costs are actually low in Brazil. Welfare effects may still be negative if the social value of transferring income to UI exhaustees is small, for example if many UI exhaustees are reemployed informally. We estimated that the share of UI exhaustees still unemployed is around 35%, a figure comparable to the US. In this section, we evaluate welfare effects of UI extensions by feeding our results into the sufficient statistics formula derived in Section 2. Given that our estimates apply to workers with more than 2 years of tenure, we only consider this specific group. Because the only statistics we cannot provide an estimate for —the social value of transferring income to UI exhaustees— is intrinsically subjective, we proceed in two steps.

1. Bounding the social value consistent with positive welfare effects.

From equation (3), we can recover the minimum social value of transferring income to UI exhaustees consistent with positive welfare effects. Our estimates of efficiency costs range from 5% to 11%. Therefore, welfare effects from UI extensions would be positive if the social value of $1 is 5.3%

[51] In R$ of 2002. Minimum wage was R$200 in 2002 and the average exchange rate was 2.92R$/US$. Disposable income is household income per capita using equivalence scales of $\frac{1}{2}$ for children.

to 12.3% larger for UI exhaustees than for individuals contributing to the UI budget (typically the formally employed). As a comparison, Chetty (2008, for the US) estimates that the social value of $1 is 150% larger for UI beneficiaries at the start of their unemployment spell than for employed individuals. The social value consistent with positive welfare effects in our context is thus rather small. Yet, some UI exhaustees may be reemployed in jobs undetected by the UI agency by the time they exhaust benefits, potentially reducing the need for income support. Using equation (4), we can recover the minimum social value of transferring income to *unemployed* UI exhaustees consistent with positive welfare effects, assuming that the social value of transferring income to the informally reemployed is nil. This assumption may seem extreme given that the informally reemployed experience lower levels of disposable income than the formally (re–)employed (Table 8). Nevertheless, we are interested in providing bounds and Maloney (1999) argues that non–wage benefits from informal self–employment may be important. With this assumption and a share of 29% still unemployed 1 month after benefit exhaustion —$O(P)$ in the formula—, welfare effects are positive if the social value of transferring income to *unemployed* UI exhaustees is at least 14%–33%.

To evaluate arguments that the prevalence of informal employment and imperfect monitoring of beneficiaries may preclude a social insurance program such as UI to increase welfare, we also use the US as a benchmark. Katz and Meyer (1990) estimates a similar measure of efficiency costs from UI extensions at about 57% in the US. As a consequence, the social value should be larger than 100% in the US for a UI extension to be welfare–improving. Therefore, extending comparable UI programs—5 months in Brazil, 6 months in the US— may be more likely to increase welfare in our context. Importantly, this holds even if we assume no gain from transferring income to UI exhaustees reemployed in jobs undetected by the UI agency. Even though the UI program is relatively costly in Brazil, it is potentially very beneficial.[52]

2. Approximating consumption using disposable income.
Imposing more structure on our normative framework, we can combine (4) and (5) to estimate the social value for typical levels of relative risk aversion. In Table 9, we simulate welfare effects using such a derivation and average measures of disposable income from Table 8 as consumption proxies —we do not observe consumption. If individuals have significant liquid savings to deplete when unemployed,[53] we should consider smaller values for the risk aversion parameter as the availability of liquid savings decreases local relative risk aversion (Chetty and Szeidl, 2006). For the informally employed, we only have information for the first period of reemployment so we assume those disposable income levels still hold around benefit exhaustion. We obtain $I(P)$ as $S(P) - O(P)$. Finally, to bound our results, we also consider the case where there is no social value of transferring income to informally reemployed UI exhaustees. We use estimates of efficiency costs from both

[52]Imperfect take–up of benefits would reduce welfare gains but take–up is unlikely to be lower in Brazil than in the US as it is believed to be very high there (Hijzen, 2011).

[53]It is not the case in the US (Chetty, 2008).

empirical strategies and present the social value corresponding to each risk aversion parameter.

Unless risk aversion is particularly low,[54] the simulations suggest that a UI extension increases welfare. Welfare gains could be sizeable: with a coefficient of relative risk aversion of 1 and no gains from transferring income to informally reemployed UI exhaustees, extending benefits from 5 to 6 months is equivalent to raising total formal payroll by .086%–.13% for the group of workers we consider. These results indicate that imperfect monitoring of UI beneficiaries' eligibility does not preclude a UI program, comparable to the US system, to increase welfare.

7 Conclusion

Our paper investigates the impact of UI extensions on labor market outcomes and welfare in Brazil, where informal employment is prevalent and monitoring of labor status imperfect. We estimate that extending UI reduces hazard rates of formal reemployment by about 50% when collecting extra benefits. Individuals are forward–looking as hazard rates are also reduced earlier in the UI spell. Nevertheless, such a behavioral effect amounts to only 5%–11% of the total increase in mean covered duration —a measure of efficiency costs— because reemployment rates in the formal sector are low, even after benefit exhaustion. Therefore, contrary to a prior that imperfect monitoring may exacerbate behavioral responses, we estimate efficiency costs to be small. This result and the fact that a significant share of UI exhaustees are actually unemployed imply that welfare effects from extending benefits (and thus from the UI program itself) are likely to be positive in our normative framework. Imperfect monitoring could lead to larger distortions if potential beneficiaires were more willing and better able to adjust their labor status in response to UI incentives. But typical views on the presence of a large informal sector (Fields, 1975; Maloney, 1999) are actually consistent with such small efficiency costs. Social insurance programs, such as UI, may thus increase welfare in this context.

Why then have only a few middle–income and developing countries, most of them sharing a similar context, implemented UI programs so far? First, if social insurance tends to expand with economic development, the actual timing of expansions is often related to political economy considerations. But the list of countries implementing or considering the implementation of UI is growing.[55] Second, many countries have historically relied on job protection rather than worker protection institutions. Heckman and Pages (2004) argues that such regulations appeared "*much earlier in the development process* as a *low–cost way (from the point of government fiscal authorities) of providing social insurance.*" Third, even benefit taxes are often viewed as more distortionary in poorer countries (Levy, 2008). A higher cost of taxation can be captured in our normative

[54] For example if households had more liquid savings or easier access to credit in Brazil than in the US.

[55] At the extreme, if formal jobs are *too* rare and unstable, workers would not value insurance. A similar argument might justify UI benefits increasing with tenure duration as layoff risk decreases with tenure.

framework through a lower social value of transferring income away from contributing individuals. Evidence of tax avoidance abound. However, there is very little evidence on the relevant elasticities with respect to tax rates in poorer countries. Moreover, recent studies show that such an elasticity might not be as high as previously thought.[56] This is a promising avenue for future research.

We have not considered externalities sometimes associated with informal employment. In theory, UI may actually decrease informal reemployment rates if income effects are large. We find that UI extensions increase overall duration outside formal employment, and the impact is larger than on mean covered duration. This is consistent with an increase in informality but existing data are not rich enough to directly tackle this question. However, externalities would have to be sizeable to reverse our conclusion that the existing UI program likely increases welfare for our sample of Brazilian workers. Our normative framework also abstract from other reasons why an envelope argument does not apply, such as search frictions and wage bargaining. In this specific case, welfare effects from UI may be even higher (Landais et al., 2011).

The usual normative prescription to prevent responses to UI at the layoff margin is the full experience–rating of benefits. Generally, the recent optimal UI literature (our paper included) then abstracts from this margin. In practice, experience–rating may be particularly relevant in countries where monitoring is imperfect, as manipulating layoffs may be easier. However, if the social cost of levying layoff taxes is higher (if firms are credit–constrained), it may not be optimal to have full experience–rating of benefits (Blanchard and Tirole, 2006). The prevalence of high firing costs in developing countries suggests that experience–rating of benefits is a feasible option. Moreover, we find suggestive evidence that existing institutions may already prevent such responses.

Although our results have been obtained for a specific program, for a particular country and group of workers, our analysis indicates that the prevalence of informal employment and the imperfect monitoring of beneficiaries' eligibility need not preclude social insurance programs to increase welfare. The relevant patterns for our results in the case of UI —low formal reemployment rates and a significant share of workers still unemployed a few months after layoff— are likely to prevail in other settings, in particular in Latin America. Considering workers attached to the formal labor force is also a natural first–step as only workers who adjusted to their employment situation would value insurance. Understanding if our findings apply for different programs and in different settings remains an important topic for future research.

[56]Kleven and Waseem (2011) finds a small elasticity at the intensive margin in Pakistan. Studies of the impacts of Seguro Popular, a Mexican program extending health care to the informally employed, find small effects at the extensive margin compared to the large positive shock on informal wages implied by the program (Azuara and Marinescu, 2011; Barros, 2008; Campos–Vasquez and Knox, 2008; Bosch et al., 2010; Aterido et al., 2011).

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Table 1: Diff–in–diff results for benefit collection outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Mean	Control years					
	Control	both	1995	1997	both	1995	1997
S(5)	.7946	.0044	.0059	.001	.005	.0054	.0023
		(.0094)	(.0103)	(.0102)	(.0095)	(.0104)	(.0099)
S(7)	.673	.0667***	.0706***	.0608***	.068***	.0716***	.0619***
		(.0098)	(.0106)	(.011)	(.01)	(.011)	(.0106)
$\sum_{t=1...5} S(t)/S(1)$	4.674	-.0085	-.0045	-.0158	-.0092	-.0068	-.0155
		(.014)	(.0134)	(.0163)	(.0148)	(.0143)	(.0165)
$\sum_{t=6...7} S(t)/S(5)$	1.758	.1207***	.1255***	.1154***	.1218***	.1283***	.1145***
		(.0124)	(.0133)	(.0129)	(.0124)	(.0138)	(.0124)
		Whole sample (logit)					
h(5)	.0892	-.7293***	-.7437***	-.7132***	-.7357***	-.7592***	-.7114***
		(.0728)	(.0779)	(.0737)	(.0736)	(.0817)	(.0709)
h(6)	.0701	-.7705***	-.802***	-.7305***	-.7878***	-.8295***	-.7396***
		(.0596)	(.0708)	(.0549)	(.06)	(.072)	(.0541)
		May only (logit)					
h(4)	.0444	-.2065**	-.2809***	-.1086	-.2067**	-.2825***	-.1053
		(.0819)	(.0889)	(.0925)	(.081)	(.0888)	(.0907)
h(5)	.0847	-.7749***	-.7846***	-.7645***	-.7836***	-.8013***	-.7635***
		(.0856)	(.0902)	(.0918)	(.0876)	(.0952)	(.089)
h(6)	.0615	-.7476***	-.7771***	-.7089***	-.7708***	-.8097***	-.7268***
		(.0605)	(.0668)	(.0719)	(.0627)	(.0695)	(.0724)
Other controls		No	No	No	Yes	Yes	Yes

Results for the 1996 temporary UI extension. The top quadrant shows impacts on survival rates out of the formal sector and covered duration. As eligible beneficiaries learned about the extension shortly before exhaustion of regular benefits (month 5 after layoff), the impacts are observed only late in the UI spell. The second quadrant shows how aggregate formal reemployment hazard rates are affected (in month 5 and 6 after layoff —the spike). The bottom quadrant restricts attention to workers who learned about the extension earlier in their UI spell and could already adjust job–search in month 4 after layoff (anticipation behavior).

Workers aged 18 to 54 from the largest metropolitan areas of Brazil (São Paulo excluded), eligible for 5 months of maximum benefit duration and who lost a private formal job in April or May 1995, 1996 and 1997. In 1996, in treatment areas, they were eligible for up to 7 months of UI. Sample described in Tables A.1 and A.2 in the Appendix. s.e. clustered by Metropolitan Area (29 clusters). Significance levels: *10%, **5%, ***1%. Metropolitan Area fixed effects included. Other controls include 4th order polynomials in (normalized) tenure, age and log wage, and dummies for education levels, sector and gender.

Table 2: Diff–in–diff results for reemployment outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Whole sample			Surviving out up to September	
	Mean			Control years			
Outcome	Control	both	1995	1997	both	1995	1997
Reemployed	.6316	-.0162***	-.0164**	-.0145**	-.0186***	-.0206***	-.0161***
within 2 years		(.0055)	(.007)	(.0066)	(.0045)	(.0064)	(.0057)
Non–employment	13.65	.6007***	.6369***	.5229***	.6996***	.7822***	.6104***
duration (max 24m)		(.1662)	(.2015)	(.18)	(.1097)	(.142)	(.1096)
Months employed in	8.061	-.4488***	-.4896***	-.3674**	-.4708***	-.5202***	-.4148***
2 years after layoff		(.1427)	(.1665)	(.1513)	(.0949)	(.1111)	(.1036)
At least 1 new layoff in	.2824	-.019***	-.0283***	-.0082	-.0171***	-.0252***	-.0083
2 years after layoff		(.0065)	(.0093)	(.0069)	(.0059)	(.0087)	(.0055)
December employment	.4446	-.0031	.0003	-.006	-.0043	-.0024	-.0067
2 years after layoff		(.0054)	(.0067)	(.006)	(.0056)	(.006)	(.0065)
December log real wage	5.742	.0126*	.0105	.0158	.0042	-.0005	.01
2 years after layoff		(.0071)	(.0094)	(.0133)	(.0076)	(.0104)	(.0124)

Results for the 1996 temporary UI extension. The table shows longer–term reemployment outcomes beyond the covered duration. As eligible beneficiaries learned about the extension shortly before September 1996, we restrict attention to individuals not formally reemployed by September in columns (5)–(7). Eligible beneficiaries in treatment areas in 1996 stayed longer out of the formal sector, spent less time in the formal sector in the following 2 years but are then also less likely to have experienced a new layoff from the formal sector (and thus less likely to collect again UI benefits). We find no effect on wages. Sample as in Table 1 (see notes). s.e. clustered by Metropolitan Area (29 clusters). Significance levels: *10%, **5%, ***1%. Additional controls for (normalized) log wages, age, tenure (4th order polynomials), metropolitan area, education, sector and gender (dummies).

Table 3: Regression discontinuity results for benefit collection outcomes

	Mean T<24	(1)	(2)	(3)	(4)	(5)	(6)
S(1)	.939	-.0021	.0002	.0002	-.0024	-.0005	-.0009
		(.0026)	(.005)	(.0033)	(.0018)	(.0034)	(.0024)
S(4)	.8744	.0066**	.008*	.0094***	.0065***	.0066*	.0079***
		(.0026)	(.0043)	(.0031)	(.0021)	(.004)	(.0028)
S(5)	.8306	.024***	.0248***	.0248***	.0247***	.0244***	.0256***
		(.0021)	(.0034)	(.0027)	(.0021)	(.0042)	(.0028)
$\sum_{t=1...4} S(t)/S(1)$	3.867	.0139***	.0147***	.017***	.0133***	.0122***	.0147***
		(.0027)	(.0037)	(.0029)	(.0025)	(.004)	(.0028)
$\sum_{t=1...5} S(t)/S(1)$	4.752	.0416***	.041***	.0433***	.0419***	.039***	.043***
		(.0039)	(.0064)	(.0045)	(.0036)	(.0063)	(.0041)
Tenure controls		Linear	Quadratic	Linear	Linear	Quadratic	Linear
Other controls		No	No	No	Yes	Yes	Yes
Tenure window		15–36	15–36	18–30	15–36	15–36	18–30

Results for a regression discontinuity based on tenure in the lost job (T): workers with more than 24 months of tenure are eligible for up to 5 months of UI (instead of 4 months). The first outcome (survival up to month 1 after layoff) is a measure of UI take–up (waiting period of 30 days). The next 2 outcomes measures potential exhaustion of the benefits available for individuals *on the left* and *on the right* of the discontinuity, respectively. Similarly, the last 2 outcomes are measures of covered duration. Extended benefits lead to small but significant impacts on formal reemployment rates, even before the extra benefit is collected. Controlling for covariates has no effect on our results.

See the text for sample selection. Sample described in Table 4. s.e. clustered at the quarter–month tenure level. Significance levels: *10%, **5%, ***1%. Other controls include metropolitan area, separation month and quarter–month fixed effects, 4th order polynomials in age and log wage and dummies for education levels, industry and gender. Full sample has 748246 observations.

Table 4: Validity of the regression discontinuity design

	Mean T<24	(1)	(2)	(3)
Observations per bin	10497	695.1	328.9	582.1
		(890.8)	(1629)	(1206)
Male	.5747	.0105**	.0064	.011
		(.0052)	(.0117)	(.0076)
Age	27.58	-.117*	.0033	-.0142
		(.0668)	(.1246)	(.0906)
Less than 8th grade	.4194	.0028	.0117	.0076
		(.0059)	(.0095)	(.0074)
8th to 10th grade	.3512	.004	-.0048	-.0012
		(.0034)	(.0059)	(.0048)
High school graduates	.206	-.0045	-.003	-.0034
		(.0041)	(.0059)	(.0051)
College graduates	.0229	-.0022	-.0036	-.0029
		(.0014)	(.0025)	(.0018)
Log wages	5.609	-.0072	.0177	.0314
		(.0402)	(.0745)	(.0553)
Replacement rate	.6015	.0018	-.0041	-.0091
		(.0126)	(.0233)	(.0174)
Commerce	.3078	-.0093*	-.008	-.0109*
		(.005)	(.0088)	(.0062)
Construction	.0841	-.0023	-.0028	-.0014
		(.0032)	(.0064)	(.0045)
Services	.3987	.0005	.0006	.0045
		(.0057)	(.0111)	(.008)
Industry	.2095	.0111*	.0102	.0078
		(.0059)	(.0124)	(.0085)
Tenure controls		Linear	Quadratic	Linear
Tenure window		15–36	15–36	18–30

Description of the sample and validity check for the regression discontinuity in Table 3. With more flexible specifications in columns (2)–(3), there does not appear to be any significant change in the number and composition of observations on each side of the discontinuity (our results in Table 3 are unaffected when controls are included).
See the text for sample selection. s.e. clustered at the quarter–month tenure level. Significance levels: *10%, **5%, ***1%. No additional controls. Full sample has 748246 observations.

Table 5: Regression discontinuity results for reemployment outcomes

Outcome	Mean T<24	(1)	(2)	(3)	(4)	(5)	(6)
Reemployed	.562	-.0024	-.0087*	-.006	-.003	-.0075	-.0041
within 2 years		(.0035)	(.0051)	(.0045)	(.003)	(.0052)	(.004)
Non–employment	15.33	.1593**	.2415**	.216**	.1642***	.2064*	.1728**
duration (max 24m)		(.0704)	(.1032)	(.0863)	(.0616)	(.1127)	(.0785)
Months employed in	6.204	-.0892	-.178	-.1554*	-.0921	-.1428	-.1164
2 years after layoff		(.0663)	(.1193)	(.0853)	(.0593)	(.1126)	(.077)
At least 1 new layoff in	.2751	-.008*	-.0083	-.0071	-.0082***	-.0089**	-.0079**
2 years after layoff		(.0043)	(.0065)	(.0054)	(.0028)	(.0041)	(.0035)
December employment	.3336	-.0003	-.0012	-.0009	0	.0003	.001
1 year after layoff		(.003)	(.0048)	(.0038)	(.0026)	(.0046)	(.0034)
December employment	.3573	-.0007	-.0048	-.0037	-.0001	-.003	-.0009
2 years after layoff		(.0033)	(.0061)	(.0049)	(.0027)	(.0052)	(.0038)
December log real wage	5.714	-.0049	-.0056	-.0065	-.0047	.0012	-.002
1 year after layoff		(.0085)	(.0178)	(.0129)	(.0042)	(.0067)	(.0054)
December log real wage	5.923	.0078	.0039	.0084	.0074*	.0123*	.011***
2 years after layoff		(.0077)	(.0154)	(.0102)	(.0041)	(.0063)	(.0041)
Tenure controls		Lin.	Quad.	Lin.	Lin.	Quad.	Lin.
Other controls		No	No	No	Yes	Yes	Yes
Tenure window		15–36	15–36	18–30	15–36	15–36	18–30

Longer–term results for the regression discontinuity in Table 3. Workers eligible for an extra month of UI stayed longer out of the formal sector, spent less time in the formal sector in the following 2 years but are then also less likely to have experienced a new layoff from the formal sector (and thus less likely to collect again UI benefits). Results on wages are not consistent enough to be fully conclusive.

See text for sample selection. s.e. clustered at the quarter–month tenure level. Significance levels: *10%, **5%, ***1%. Other controls include metropolitan area, separation month and quarter–month fixed effects, 4th order polynomials in age and log wage and dummies for education levels, industry and gender. Full sample has 748246 observations.

Table 6: Simulating the impact of a UI extension of 2 months

	(1)	(2)	(3)
	Baseline	1996 temporary extension	Adding anticipation
Surviving 4 months out (S_4)	.8457	.8457	.8599
Surviving 5 months out (S_5)	.8146	.8204	.8342
Surviving 7 months out (S_7)	.6888	.7585	.7713
Mean covered duration if 5 months offered	4.711	4.717	4.764
Mean covered duration if 7 months offered	6.266	6.391	6.465
Δ mean covered duration if 5 months offered		.0062	.0523
marginal behavioral effect		.0031	.0262
Δ mean covered duration if 7 months offered	1.555	1.679	1.754
marginal mechanical effect		.7776	.7776
marginal behavioral effect		.0621	.0992

Simulation of the impact of a 2–month UI extension (i) if workers learned late about the extra benefits —column (2), as in the 1996 extension— or (ii) if they had learned about the extension upon layoff—column (3), to gauge the importance of anticipation behaviors. The behavioral effect is only responsible for 11% of the increase in mean covered duration ($.0992/(.7776 + .0992)$), even if we have relatively large anticipation behaviors.

The Baseline is obtained through non–parametric estimation of the hazard function in Control Areas in 1996. The second column replicates results from the temporary extension by multiplying hazard rates in month 4, 5 and 6 by the percentage change estimated in those months in Table 1 (using full controls). The third column simulates the impact had workers learned about the UI extension upon layoff by also multiplying hazard rates in months 1 to 3 by the percentage change estimated in months 4 (anticipation) for the May sample in Table 1 (using full controls). As workers are more likely to adjust job–search closer to benefit exhaustion, this is likely to be an upper bound for the actual scenario we simulate (see Figure 10b).

Table 7: Maximum likelihood results for the hazard of leaving non–employment

Daily hazard rate constant per 30–days period	Coefficient	s.e.
λ_0	.0082508***	(.0006836)
$\lambda_{1,2}$	.0071817***	(.0003428)
$\lambda_{3,4}$	.0063421***	(.000227)
$\lambda_{5,6}$	.0067136***	(.0002787)
$\lambda_{7,8}$	.0069552 ***	(.0003371)
$\lambda_{9,10}$	.0064482***	(.0004648)

Data from monthly urban labor force surveys in the 6 largest metropolitan areas of Brazil (PME, 2003–2010). The maximum likelihood estimation uses equation (10) and information from consecutive interviews about the likelihood to be reemployed in period t if laid off since m months in period $t-1$. λ_m is the daily hazard rate of reemployment over the next month if still unemployed m months after layoff. The maximum likelihood controls for a stock sampling issue within month (see text). Sample restricted to individuals laid off from a formal private sector job with more than 2 years of tenure at layoff. Our results imply that 35% of workers are still unemployed by the time they exhaust UI benefits —a figure comparable to UI exhaustion rate— and 29% one month later.
s.e. clustered by individual (19346 individuals). Estimations using sampling weights. Significance levels: *10%, **5%, ***1%.

Table 8: Average earnings and disposable income by labor status (R$)

	Individual Earnings	Disposable Income
Formal private overall	744	500
Formal private prior to layoff	540	350
If reemployed as formal employee[a] before benefit exhaustion	448	322
If reemployed in uncovered sector[a] before benefit exhaustion	290	231
If still out of any employment around benefit exhaustion	0	172[b]

Description of earnings and disposable income for workers either formally employed with more than 2 years of tenure or for workers who were recently laid off from a formal job with more than 2 years of tenure. Earnings and disposable income for individuals still unemployed around benefit exhaustion and for individuals reemployed in jobs undetected by the UI agency are much lower than for the formally employed.

Own calculations using monthly urban labor force surveys covering the six largest metropolitan areas of Brazil (PME, 2003–2010). Estimations using sampling weights. Units are R$ of 2002. In 2002, the minimum wage was R$200. Sample restricted to individuals either formally employed since more than 2 years or who had more than 2 years of tenure when laid off from the formal sector (to mimic samples from RAIS in other Figures and Tables of the paper). Disposable income is defined as total family income divided by the number of family members using an equivalence scale of $\frac{1}{2}$ for children.

[a] Earnings and disposable income in the first reemployment month.

[b] 30 % have a value of 0.

Table 9: Aggregate welfare gains from a marginal UI extension

		Welfare gains	
Risk aversion	Social value	*Behavioral effect: 5%*	*Behavioral effect: 11%*
.5	*.2928*	.1711	.1039
1	*.5856*	.3786	.288
2	*1.171*	.7935	.6562
5	*2.928*	2.038	1.761

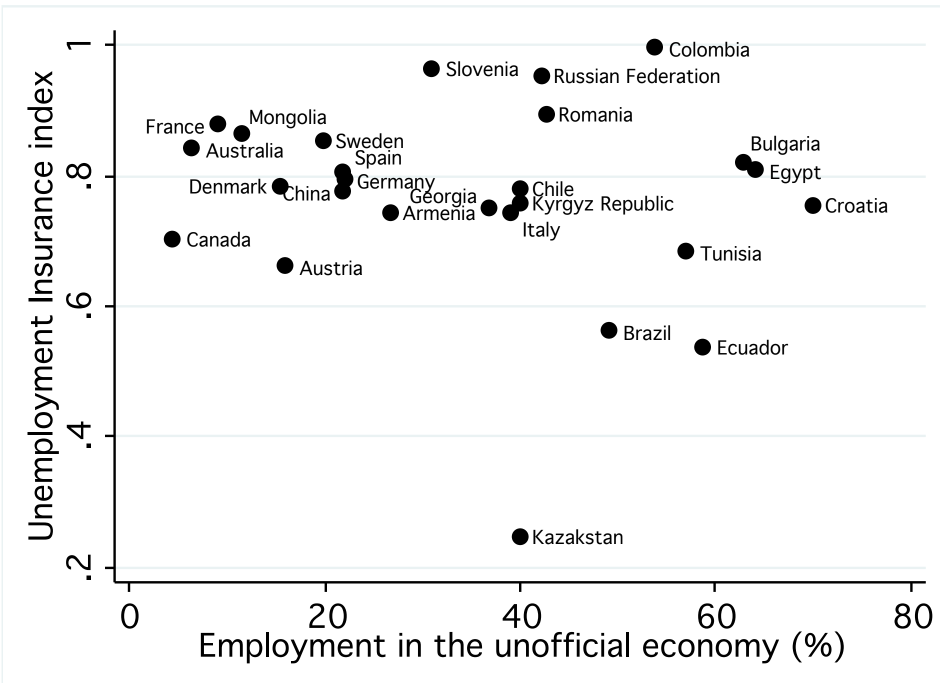
Unemployed UI exhaustees only

		Welfare gains	
Risk aversion	Social value	*Behavioral effect: 5%*	*Behavioral effect: 11%*
.5	*.328*	.0469	.003
1	*.656*	.1301	.0863
2	*1.312*	.2966	.2528
5	*3.28*	.7962	.7523

Welfare gains are expressed in terms of a percentage increase in formal payroll and are calculated combining equations (4) and (5). Welfare gains are calculated for our lowest and highest measures of the behavioral effect. The first quadrant measures the social value of transferring income to UI exhaustees by multiplying the relative risk aversion parameter by the drop in disposable income for all UI exhaustees independently of their labor status. The second quadrant assumes that the social value of transferring income to UI exhaustees reemployed in jobs undetected by the UI agency is nil. Welfare gains are positive even in this case for every level of risk aversion considered.

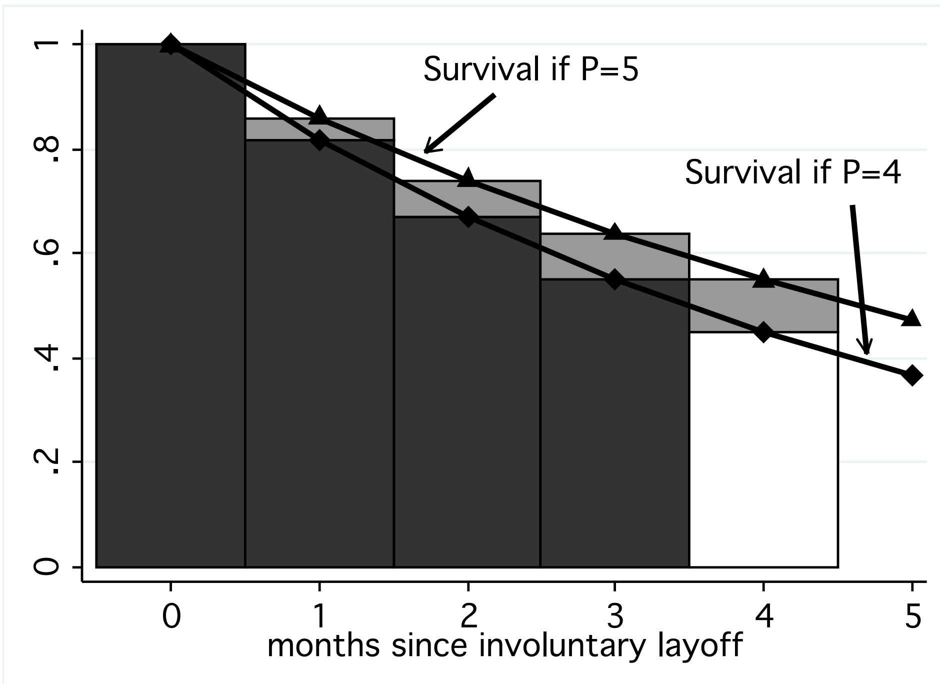
Calculations use layoff rates and replacement rates for covered workers with more than 2 years of tenure and disposable income figures from Table 8. Using disposable income to approximate consumption is valid if individuals have little savings to spend in case of non–employment as it appears to be the case in the US (Chetty, 2008). In case of positive available savings, one should consider smaller values of the coefficient of relative risk aversion in this table (Chetty and Szeidl, 2006).

Figure 1: Unemployment Insurance and Informal Employment



Data from Botero et al. (2004) can be found at http://iicg.som.yale.edu//. We include every country with a positive value for the unemployment insurance index and a value for the size of the unofficial economy. The former is a measure of the generosity of the UI program. Various countries with prevalent informal employment have adopted UI programs.

Figure 2: Impact on UI budget if maximum benefit duration (P) is extended from 4 to 5 months



$D^B_{P=4}$: mean covered duration if P=4

$S(4)$: survival out of formal employment 4 months after layoff (mechanical effect)

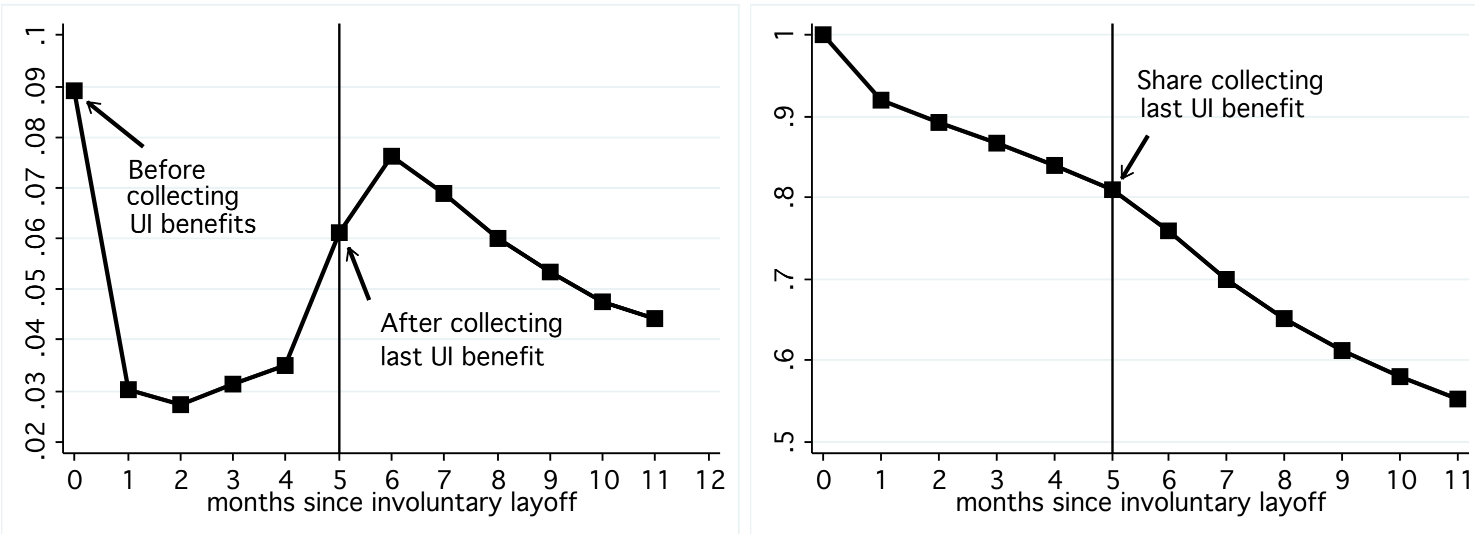
dD^B/dP: change in mean covered duration due to behavioral responses

Individuals who survive until t are collecting $t+1$ months of benefits prior to benefit exhaustion. Due to behavioral responses, formal reemployment rates are reduced when benefits are extended and the survival out of formal employment decreases at a slower rate. Before the extension, mean covered duration corresponds to the black area. Extending benefits increases mean covered duration through a mechanical effect, corresponding to the white area (the share of people who could collect the extra benefit if they do not change their behavior), and a behavioral effect, corresponding to the grey area (the change in mean covered duration due to reduced incentives to find a formal job).

Figure 3: Formal reemployment after an involuntary layoff from the formal sector

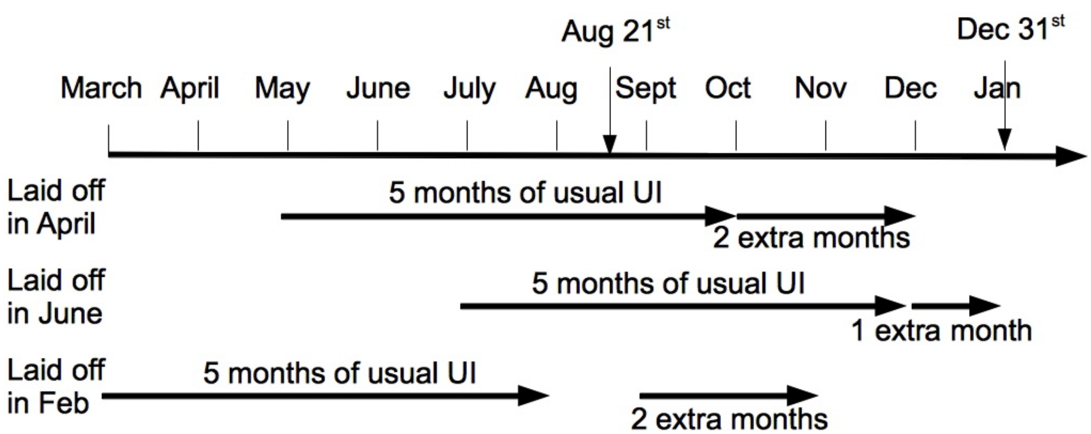
(a) Hazard rates of formal reemployment

(b) Survival rates out of the formal sector



Sample of workers eligible for 5 months of UI. Hazard rates are higher before collection of benefits (30–days waiting period) and jump up at benefit exhaustion but they are generally low. Indeed, survival rates out of the formal sector are very high. Administrative data (RAIS, see text). Sample limited to private formal workers with more than 2 years of tenure at layoff, aged 18 to 54, involuntarily laid off in 2000–2001 and working in the six largest urban areas of Brazil (coverage of urban labor force surveys —PME).

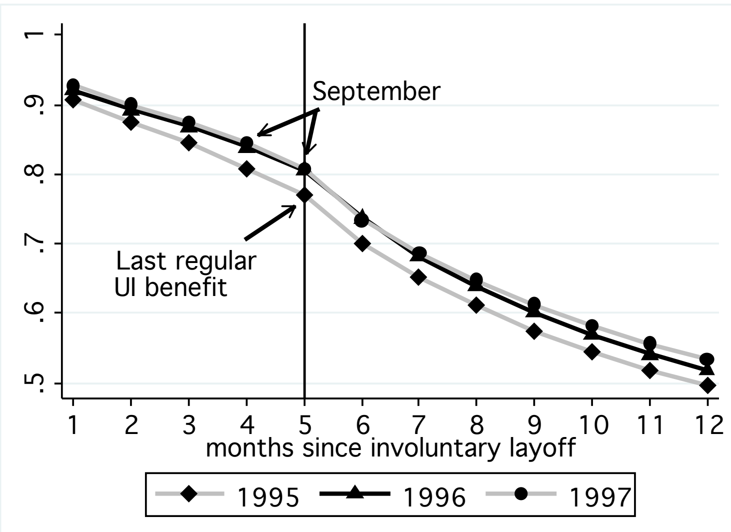
Figure 4: Timeline of the 1996 experiment



During the temporary UI extension in 1996, 2 additional UI payments were available for workers who had exhausted their regular UI benefits and had not found a new formal job by September 1st. Any additional UI payment had to be paid between September 1st and December 31st. The idea of a temporary extension was first mentioned in the media on August 14th and the extension was actually enacted on August 21st.

Figure 5: Diff–in–diff graphically (1996 temporary extension), proportion surviving out of formal employment

(a) Control areas



(b) Treatment areas

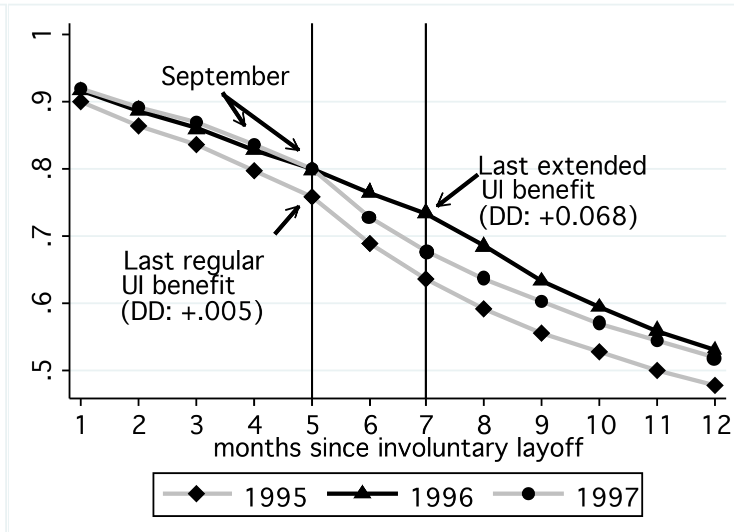
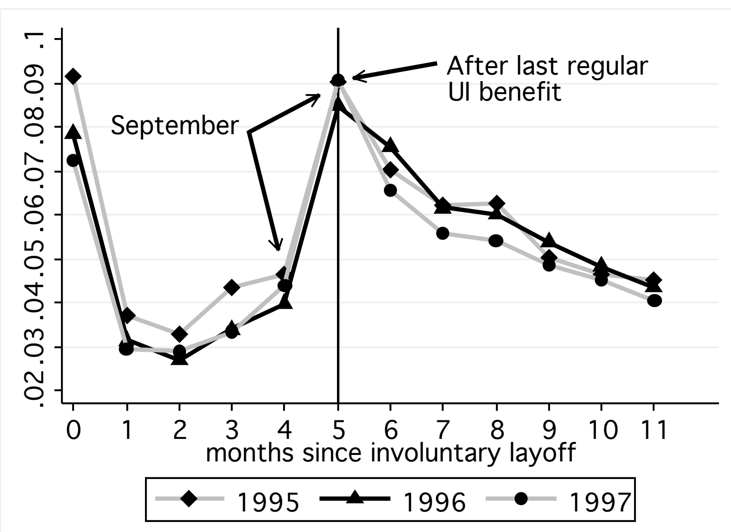
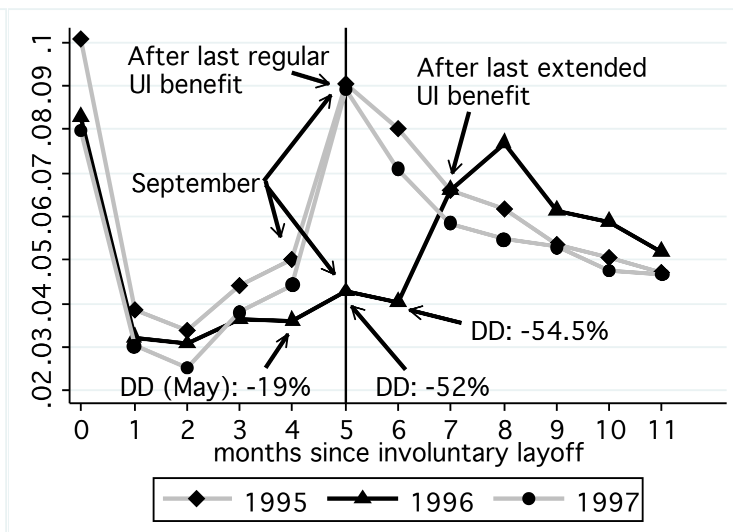


Figure 6: Diff–in–diff graphically (1996 temporary extension), hazard rate of formal reemployment

(a) Control areas

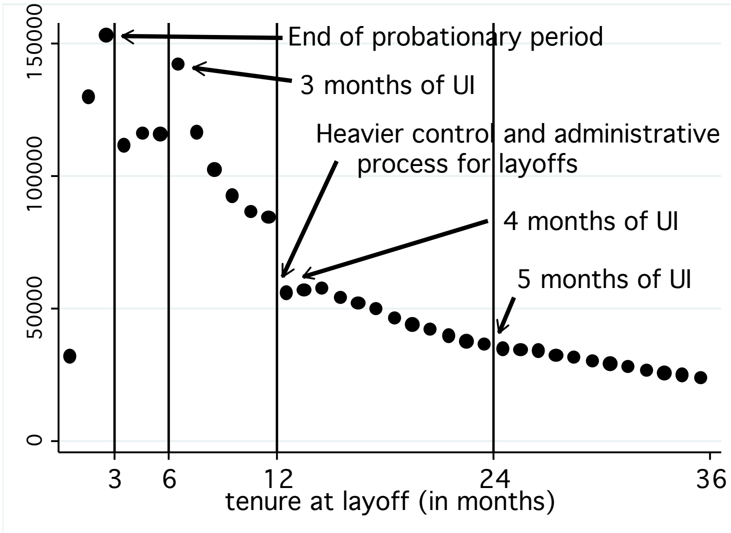


(b) Treatment areas



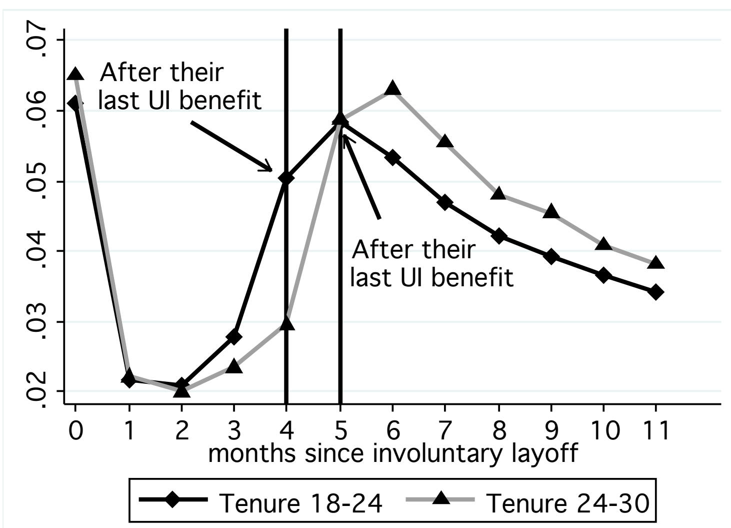
Workers aged 18 to 54 from the largest metropolitan areas of Brazil (São Paulo excluded), eligible for 5 months of maximum benefit duration and who lost a private formal job in April or May 1995, 1996 and 1997. In September 1996, in treatment areas, they learned that they would be eligible for up to 2 additional UI payments. After September, survival rates (resp. hazard rates) stay higher (resp. lower) in treatment areas in 1996, at least until exhaustion of the extended benefits. The spike in hazard rates at exhaustion of regular benefits entirely disappear when benefits are extended.

Figure 7: Tenure distribution at layoff



The figure presents the distribution of tenure at layoff for workers, aged 18 to 54, involuntarily laid off by a private formal firm between 1997 and 2000 (who did not appear in another formal employment in our data prior to 1997). Vertical lines indicate tenure thresholds related to UI or firing costs legislation. Workers with more than 6 months, 12 months or 24 months of tenure at layoff are eligible for 3, 4 or 5 months of UI, respectively. In Brazil, firing costs are discontinuously increased at 3 months of tenure (end of probationary period) and 12 months of tenure (administrative process for layoffs is made heavier). The distribution shifts down when firing costs increase and shifts up when workers are first eligible for UI but not at the other UI thresholds.

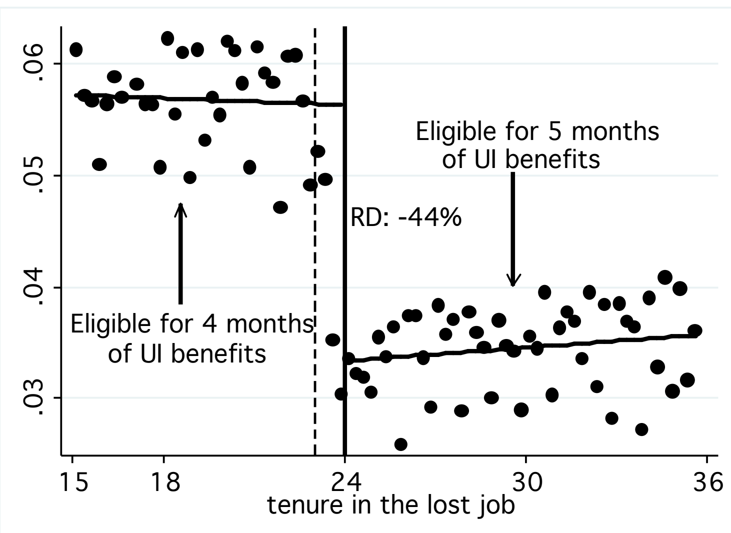
Figure 8: Formal reemployment hazard rates for workers with tenure levels at layoff around the 24–month discontinuity



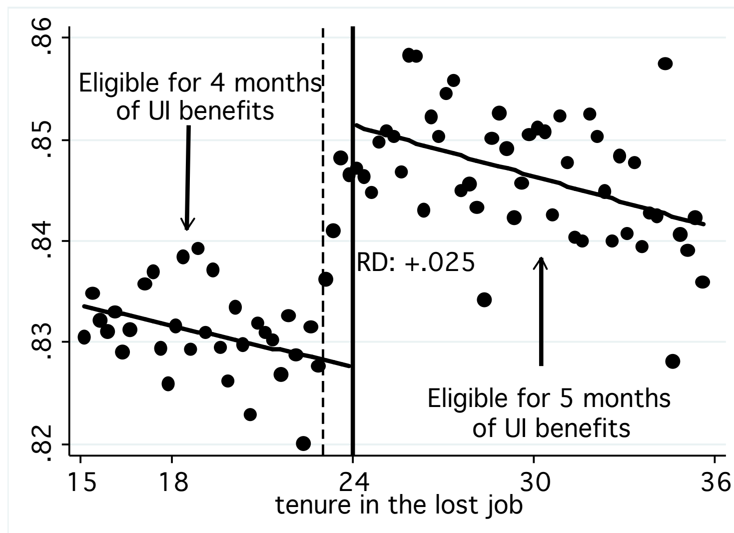
Workers with tenure levels at layoff between 18 and 24 months are eligible for 4 months of UI. Workers with tenure levels at layoff between 24 and 30 months are eligible for 5 months of UI. Hazard rates are visibly lower by month 2 after layoff for workers elegible for an extra month of UI; the spike is shifted by exactly one month. Sample for the regression discontinuity around the 24–month tenure threshold, restricted on tenure. See the text for sample selection.

Figure 9: Graphical evidence for a discontinuity around the 24–month tenure threshold

(a) Formal reemployment hazard rate in month 4 after layoff (before collecting the extra benefit)



(b) Survival rates out of formal employment in month 5 after layoff (exhaustion of the extra benefit)

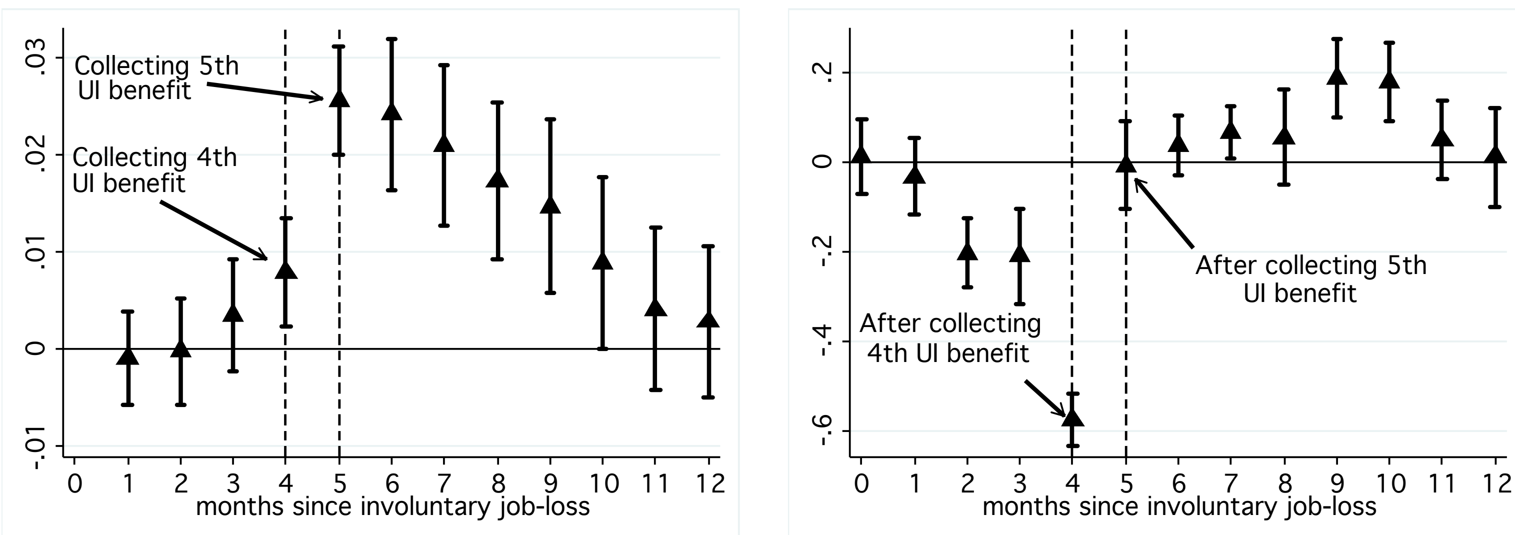


Workers with more than 24 months of tenure at layoff are eligible for a 5th month of UI. Their formal reemployment hazard rate is lower prior to collecting the extra benefit and they are more likely to survive up to 5 months after layoff, exhausting the extended benefits. See the text for sample selection. Observations are aggregated at quarter–month tenure levels. The eligibility status of workers with tenure between 23 and 24 months is unclear according to UI laws, those observations are thus not included in our analysis.

Figure 10: Regression discontinuity estimates around the 24–month tenure threshold

(a) Impact on survival rates out of formal employment (in percentage points, OLS specifications)

(b) Impact on aggregate formal reemployment hazard rates (coefficients from logit specifications)



Results for a regression discontinuity at the 24–month tenure threshold. See notes in Figure 9. Aggregate hazard rates are reduced when collecting the extra benefit but also earlier in the UI spell (anticipation behavior). Survival rates are higher between months 3 and 5 after layoff and then converge back when the extra benefit is exhausted. Estimates for aggregate hazard rates cannot be interpreted as impacts on individuals behaviors as they may include a selection component. Specifications for survival rates as in Table 3. Here we present our preferred specifications including linear controls in tenure and a tenure window of 18–30 months at layoff. See text for sample selection.

Table A.1: 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	39932
	May	.31	.23	.46	45732
1996	April	.31	.23	.46	37828
	May	.33	.21	.45	37628
1997	April	.32	.22	.45	38289
	May	.33	.22	.44	38783
All	All	.32	.23	.46	238192

We have 9 treatment areas (including Rio de Janeiro) and 20 control areas. The treatment areas are larger but the relative distribution is very stable across years.

Workers aged 18 to 54 from the largest metropolitan areas of Brazil (São Paulo excluded), eligible for 5 months of maximum benefit duration and who lost a private formal job in April or May 1995, 1996 and 1997.

A Appendix: Tables and Figures

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

		(1)	(2)	(3)	(4)	
Variable	Year	Control	Rio	Other Treat	Treat–Control	
Male	1995&1997	.656	.6748	.6707	.0161	(.0121)
	1996	.6653	.6778	.6653	.0041	(.0128)
Age	1995&1997	32.2	34.21	33.06	1.246***	(.3323)
	1996	32.97	34.39	33.45	.7894***	(.2806)
Less than 8th grade	1995&1997	.4585	.4342	.4673	-.0023	(.0251)
	1996	.4874	.4592	.4802	-.014	(.0242)
8th to 10th grade	1995&1997	.3118	.2937	.2771	-.0291***	(.0104)
	1996	.2968	.2863	.2618	-.027**	(.0119)
High school graduates	1995&1997	.2006	.2214	.2221	.0213	(.0215)
	1996	.1833	.2107	.215	.0303	(.0199)
College graduates	1995&1997	.0254	.0467	.0292	.0097**	(.0049)
	1996	.0262	.043	.0309	.0086**	(.004)
Tenure	1995&1997	57.62	60.89	57.62	1.087	(1.401)
	1996	61.33	61.25	60.59	-.526	(1.642)
Commerce	1995&1997	.2638	.2401	.2523	-.0156	(.0209)
	1996	.262	.2598	.2617	-.0009	(.0214)
Construction	1995&1997	.0617	.0618	.0782	.011	(.0115)
	1996	.0642	.0573	.0705	.002	(.009)
Industry	1995&1997	.3767	.2532	.3066	-.0879	(.0575)
	1996	.385	.2248	.2982	-.1107*	(.0592)
Services	1995&1997	.2977	.4449	.3629	.0925**	(.0361)
	1996	.2888	.4581	.3696	.1096***	(.0394)
Replacement rate	1995&1997	.4734	.4841	.4878	.0131	(.0285)
	1996	.4779	.4986	.4991	.021	(.0285)
Log wage	1995&1997	5.614	5.581	5.565	-.0437	(.088)
	1996	5.692	5.639	5.638	-.0536	(.0909)

The table presents sample means for several variables by area. Column (4) tests for a difference in means between treatment and control areas. There are significant differences between treatment and control areas (larger areas —treatment— have older, more educated job–losers more likely to have been employed in the service sector) but they appear in both control and treatment years.
Sample as in Table A.2. s.e. clustered by Metropolitan Area (29 clusters) in parenthesis. Significance levels in column (4): * 10%, ** 5%, *** 1%.

Table A.3: Robustness of diff–in–diff results for benefit collection outcomes

	(1)	(2)	(3)	(4)	(5)
	Collapsed[a] data	Triple[b] difference		Replacement[c] rate above .5	
S(5)	.0066	.0102	.0126	.0174	.0164
	(.0117)	(.0088)	(.0086)	(.012)	(.0119)
S(7)	.0713***	.0638***	.0665***	.0917***	.0909***
	(.0121)	(.0092)	(.0088)	(.012)	(.0125)
$\sum_{t=1...5} S(t)/S(1)$	.0086	-.0139	-.0109	.0103	.0092
	(.0184)	(.0154)	(.0154)	(.0123)	(.0123)
$\sum_{t=6...7} S(t)/S(5)$	.1222***	.1071***	.1086***	.137***	.1366***
	(.013)	(.0139)	(.0127)	(.0137)	(.0141)
	Whole sample (log odds ratio)	Whole sample (logit)			
h(5)	-.8015***	-.6749***	-.6848***	-.83***	-.8284***
	(.0979)	(.0858)	(.0812)	(.0864)	(.0889)
h(6)	-.8077***	-.6897***	-.7052***	-.9581***	-.9612***
	(.0708)	(.0682)	(.065)	(.0567)	(.0581)
	May only (log odds ratio)	May only (logit)			
h(4)	-.1345	-.1871**	-.1878**	-.162	-.1538
	(.1126)	(.0948)	(.091)	(.1327)	(.1364)
h(5)	-.8547***	-.7229***	-.7349***	-.8573***	-.8472***
	(.0997)	(.0956)	(.0922)	(.1209)	(.1228)
h(6)	-.7987***	-.6678***	-.6869***	-.9163***	-.917***
	(.0868)	(.0631)	(.0611)	(.0743)	(.0723)
Control years	both	both	both	both	both
Other controls	No	No	Yes	No	Yes

Robustness results for the 1996 temporary UI extension. Outcome variables are the same as in Table 1 and results are similar. s.e. clustered by Metropolitan Area (29 clusters). Significance levels: * 10%, ** 5%, ***1%. Other controls include 4th order polynomials in (normalized) tenure, age and log wage, and dummies for metropolitan areas, education levels, industry and gender.

[a] Data collapsed at the metropolitan–area by month–of–layoff level (to control for having very unbalanced clusters). OLS regressions of log odds ratio on collapsed data are equivalent to logit specifications using the micro data.

[b] Triple difference using workers laid off in July, Aug and Sep in the same year as additional controls.

[c] Sample restricted to workers with replacement rate above 50%.

Table A.4: Heterogenous diff–in–diff results for benefit collection outcomes

	(1)	(2)	(3)
	S(5)	S(7)	$\sum_{t=6...7} S(t)/S(5)$
All	.0044	.068***	.1218***
	(.0094)	(.01)	(.0124)
Male	0	.0624***	.1245***
	(.0114)	(.0114)	(.0127)
Female	.0152	.0804***	.1182***
	(.0113)	(.0132)	(.0156)
Repl. rate> .5	.0164	.0909***	.1366***
	(.0119)	(.0125)	(.0141)
Repl. rate<= .5	-.0043	.0453***	.1028***
	(.0124)	(.013)	(.0138)
Age 18–29	-.0067	.0628***	.1342***
	(.0093)	(.012)	(.0128)
Age 30–44	.0139	.0778***	.1268***
	(.0115)	(.0107)	(.0148)
Age 45–54	.014	.0549***	.0737***
	(.0138)	(.0121)	(.0188)
Less than 8th grade	.0064	.0801***	.1425***
	(.0123)	(.0127)	(.0162)
8th to 10th grade	-.0017	.0621***	.1218***
	(.0091)	(.0094)	(.0137)
High school graduates	.0049	.0504***	.0859***
	(.0103)	(.0109)	(.015)
Construction	.0376	.099***	.1338***
	(.0246)	(.0228)	(.029)
Commerce	.0016	.0815***	.1398***
	(.0088)	(.0114)	(.0146)
Industry	-.0059	.0619***	.1304***
	(.0134)	(.0179)	(.0209)
Services	.0131	.0602***	.0973***
	(.0157)	(.0145)	(.0106)

Heterogeneity results for the 1996 temporary UI extension. Coefficients for the treatment indicator from separate regressions by category using both control years. We find a significant impact on mean covered duration for each category.
Sample defined as in Table 1. Controls include 4th order polynomials in (normalized) tenure, age and log wage and dummies for metropolitan areas, education levels, sector and gender. s.e. clustered by Metropolitan Area (29 clusters). Significance levels: * 10%, ** 5%, ***1% .

Table A.5: Heterogeneity in regression discontinuity results for benefit collection outcomes

	(1)	(2)	(3)
	$\sum_{t=1...5} S(t)/S(1)$		
All	.0419***	.039***	.043***
	(.0036)	(.0063)	(.0041)
Male	.0428***	.0445***	.0476***
	(.005)	(.0079)	(.0066)
Female	.0402***	.032***	.0366***
	(.0056)	(.0101)	(.0071)
Repl. rate> .5	.0454***	.0432***	.0461***
	(.0046)	(.0087)	(.0057)
Repl. rate<= .5	.0332***	.0276*	.0348***
	(.0088)	(.016)	(.0117)
Age 18–29	.0482***	.0407***	.0479***
	(.0048)	(.0078)	(.0063)
Age 30–44	.027***	.0283**	.0292***
	(.0074)	(.0111)	(.0085)
Age 45–54	.0417***	.0798***	.0558***
	(.0146)	(.0266)	(.0196)
Less than 8th grade	.046***	.0512***	.0474***
	(.0053)	(.0101)	(.0073)
8th to 10th grade	.0426***	.0477***	.052***
	(.006)	(.0094)	(.0067)
High school graduates	.0347***	.0023	.0215
	(.0107)	(.018)	(.0134)
Construction	.047***	.0443*	.0643***
	(.0134)	(.0234)	(.0175)
Commerce	.0348***	.0412***	.0394***
	(.0069)	(.0107)	(.0088)
Industry	.0338***	.0011	.0197**
	(.0067)	(.01)	(.0091)
Services	.0501***	.0582***	.0549***
	(.0067)	(.0096)	(.0078)
Tenure controls	Linear	Quadratic	Linear
Tenure window	15–36	15–36	18–30

Heterogenity results for the regression discontinuity in Table 3. We replicate results in the last row —columns (4)–(6)— of Table 3 for mean covered duration and present coefficients from separate regressions by category. We find a significant impact for almost every category and specification.

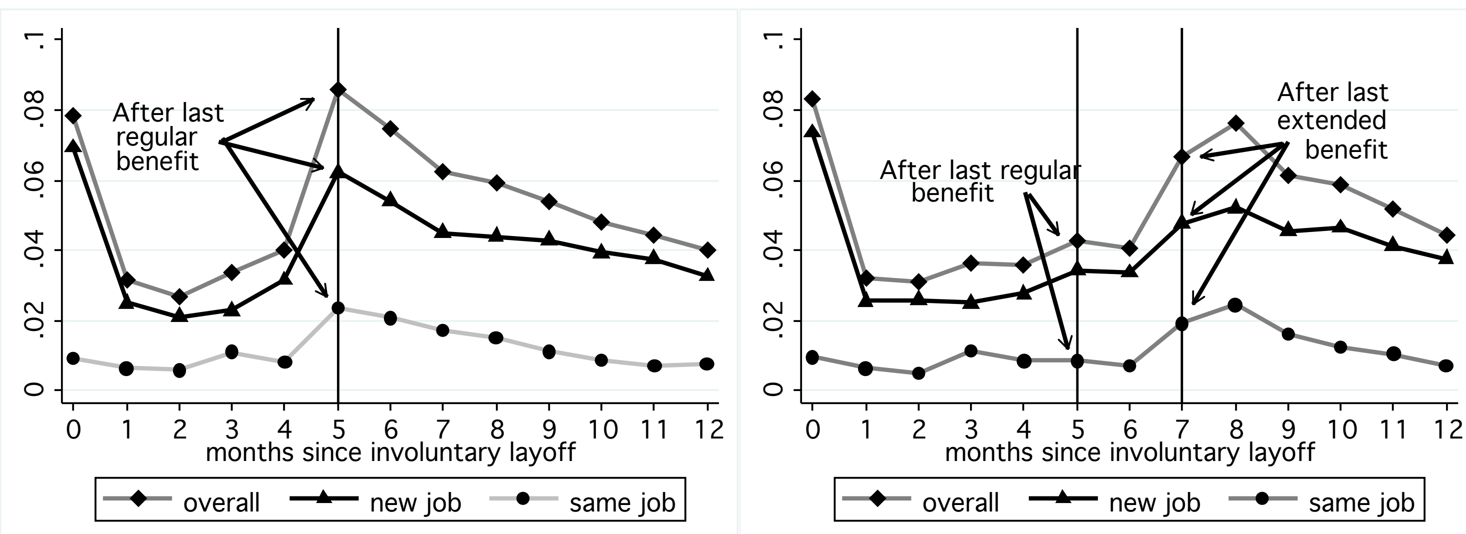
Sample as in Table 3. Full sample has 748246 observations. s.e. clustered at the quarter–month tenure level. Significance levels: * 10%, ** 5%, ***1%.

Figure A.1: Replacement rate in the Brazilian UI program

Replacement rates depend on the wage in the lost job w (expressed in minimum wages) 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 minimum wage was R$200 in 2002.

Figure A.2: Hazard rates of formal reemployment in 1996 (temporary UI extension) by destination

(a) Control Areas (b) Treatment Areas



The figures decompose overall hazard rates of formal reemployment into "same job" (if a worker's first new job is back in the same firm—temporary layoff) and "new jobs" (if a worker's first new job is with a different firm). The spike at exhaustion of regular benefits is shifted for both *destinations* in 1996 in treatment areas. Sample as in Figure 6 restricted to 1996 (treatment year).

Figure A.3: Robustness test (1996 temporary UI extension), hazard rates of formal reemployment for similar workers laid off in July, August and September in the same years and areas (not eligible).

(a) Control Areas (b) Treatment Areas

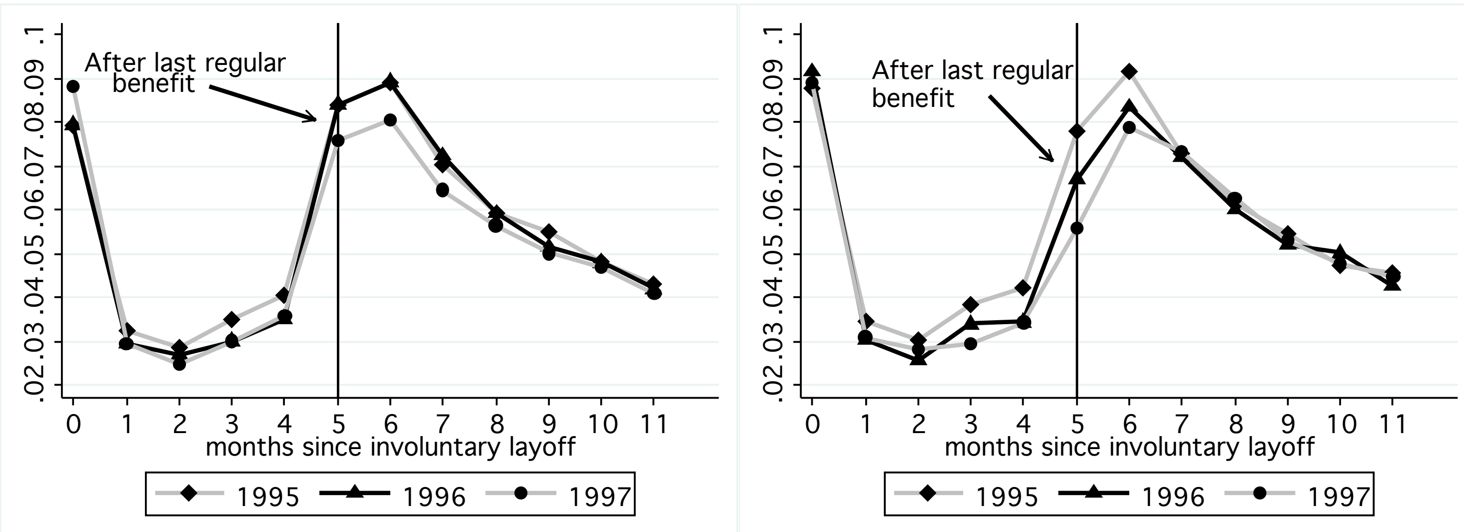
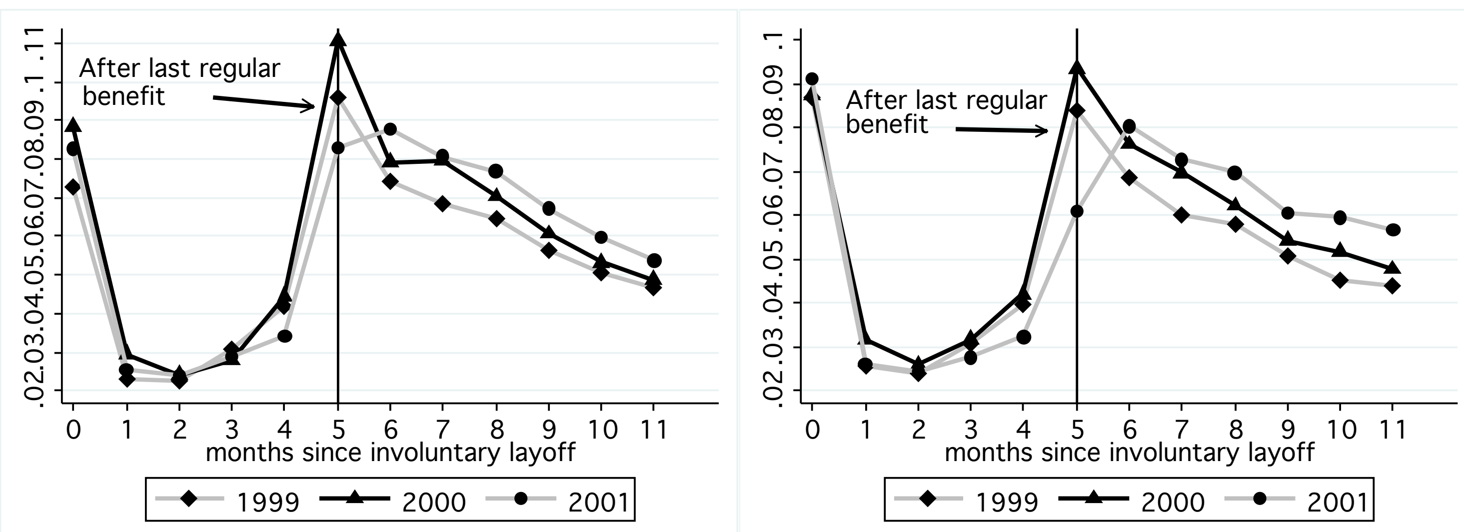


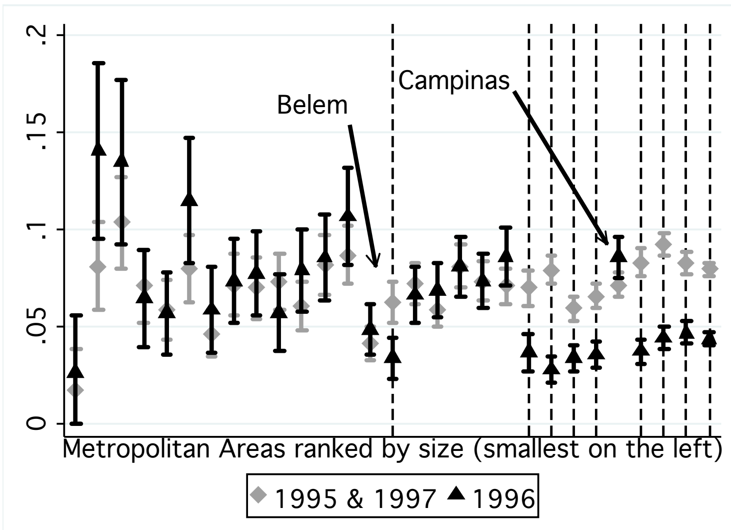
Figure A.4: Robustness test (1996 temporary UI extension), hazard rates of formal reemployment for similar workers laid off in the same months and areas in 2000, the next local election year (no UI extension).

(a) Control Areas (b) Treatment Areas



Those graphical robustness tests show that the pattern observed in treatment areas in 1996 in Figure 6 is neither observed in the next local election year nor for workers laid off later in the same years and areas (and thus not eligible). Sample as in Figure 6 except for differences mentioned in the titles.

Figure A.5: Hazard rate of formal reemployment six months after job–loss by metropolitan areas (1996 temporary extension).



The graph presents hazard rates of formal reemployment 6 months after layoff for every Metropolitan area in our sample and for treatment and control years separately. In month 6 after layoff, in 1996, additional UI payments could be collected in treatment areas. Metropolitan Areas are ranked by population, the smallest on the left. A vertical line identifies a treatment area. Sample as in Figure 6. 95% confidence intervals for the empirical hazard rates provided. Belem, one of the treatment areas was smaller population–wise than some of our control areas. Campinas, one of our control areas, was larger population–wise than some or our treatment areas. Nevertheless, hazard rates across control and treatment years within a metropolitan area are only statistically different from each other in treatment areas and they are so in every one of them.

B Appendix: History and Model

B.1 History of UI in Brazil

Unemployment insurance was first introduced in March 1986. 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 going through any parliamentary process. The only restriction is that expenditures generated by the additional payments should not cost more than 10% of UI fund's liquidity reserves. Since 1994, maximum benefit duration was temporarily extended twice (in 1996 and 2009).

B.2 A model of job–search with informality

We present here a simple search model with formal and informal employment opportunities to illustrate the discussions and derive the formulas from Section 2. This model follows models used in the optimal social insurance literature (Baily, 1978; Chetty, 2006; Kroft, 2008). Assume an agent living for T periods and starting in period 0 with assets A_0. In period 0, she might lose her formal job with some probability s. If she does not lose her job, she stays employed until T, earning wage w^F and paying tax τw^F each period. Her problem is deterministic and she will have the following utility in each period $t = 1...T$: $v\left(\frac{A_0}{T} + w^F(1-\tau)\right)$. If she does lose her job, she becomes unemployed and eligible for collecting UI benefits b for up to P periods unless she finds a new formal job. We assume for simplicity and coherence with discussions in Section 2 that taxes are only levied on workers who did not lose their formal job. In this setup, the number of periods of contributions to the UI system per layoff $\left(D^F\right)$ is thus $\frac{[(1-s)T]}{s}$. The balanced–budget equation for the UI program in this model is:

$$\tau = \frac{s}{[(1-s)\,T]} D^B \frac{b}{w^F}. \tag{11}$$

If she does lose her formal job in period 0, she becomes unemployed and can search for a new job. Given the prevalence of informal employment in most developing countries,[57] she can find with some probability both formal and informal jobs. For simplicity, we assume this search occurs sequentially within each period:[58] she first spends some effort looking for a formal job and, if unsuccessful, turns

[57] South Africa being an often-quoted exception.

[58] This approach abstracts from any direct "time" substitution between formal and informal search efforts as well as any search externality. The former effect should increase informal job search when formal job search goes down

to the informal sector. Additionally, we assume that an agent can keep searching for a formal job if informally employed. Both formal and informal jobs are assumed to survive until T. The value function of being unemployed at the start of a period solves:

$$J_t^O(A_t) = \max_{e_t^{OF}} e_t^{OF} V_t(A_t) + (1 - e_t^{OF})\Delta_t(A_t) - \theta^F \psi_{OF}(e_t^{OF}) \tag{12}$$

$$\Delta_t(A_t) = \max_{e_t^{OI}} e_t^{OI} Z_t(A_t) + (1 - e_t^{OI})U_t(A_t) - \theta^I \psi_{OI}(e_t^{OI}) \tag{13}$$

The value function of being informally employed at the start of a period solves:

$$J_t^I(A_t) = \max_{e_t^{IF}} e_t^{IF} V_t(A_t) + (1 - e_t^{IF})Z_t(A_t) - \theta^F \psi_{IF}(e_t^{IF}) \tag{14}$$

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). The e's are search efforts normalized to correspond to job-finding probabilities and the ψ's are strictly convex search cost functions. Upper scripts OF, OI and IF stand respectively for transitions from unemployment to formal employment, unemployment to informal employment and informal to formal employment. θ's are scale parameters for the cost functions. Finally, we have:

$$V_t(A_t) = \max_{A_{t+1}} v(A_t - A_{t+1} + w^F) + V_t(A_{t+1}) \tag{15}$$

$$Z_t(A_t) = \max_{A_{t+1}} z(A_t - A_{t+1} + w^I + b_t) + J_t^I(A_t) \tag{16}$$

$$U_t(A_t) = \max_{A_{t+1}} u(A_t - A_{t+1} + b_t) + J_t^O(A_t) \tag{17}$$

where $b_t = b$ for $t = 1...P$ and $b_t = 0$ otherwise and w^I stands for the per–period informal wage. The utility functions v, z and u are assumed to be strictly concave. Interior solutions for the search efforts in this model must satisfy:

$$e_t^{IF} : V_t(A_t) - I_t(A_t) = \theta^F \psi'_{IF}(e_t^{IF}) \tag{18}$$

$$e_t^{OI} : Z_t(A_t) - U_t(A_t) = \theta^I \psi'_{OI}(e_t^{OI}) \tag{19}$$

$$e_t^{OF} : V_t(A_t) - \Delta_t(A_t) = \theta^F \psi'_{OF}(e_t^{OF}) \tag{20}$$

B.2.1 Reemployment rates

The main channels discussed in the text, potentially affecting reemployment rates in the formal sector in developing countries, can be captured by the following comparative statics: $d\theta^F > 0$ (more difficult to find a formal job), $dw^I > 0$ (informal employment opportunities more attractive) and $dA < 0$ (less resources to cope with income shocks). For illustration, the following is obtained for one-period changes in the parameters.[59] Define O, F and I as the share of agents unemployed,

by the time constraint. The latter effect should have the opposite effect if the model captures the behavior of a representative agent because of search congestion.

[59]The impact on our outcomes of interest of multi-period changes in the parameters includes cross-period effects whose signs will depend more heavily on functional form assumptions.

formally employed and informally employed at the end of a given period. In the above model, we have that:

$$dF_t = O_{t-1} * de_t^{OF} + I_{t-1} * de_t^{IF}$$

Comparative statics gives the following results:

$$\frac{dF_t}{\theta^F} < 0, \quad \frac{dF_t}{w^I} < 0, \quad \frac{dF_t}{dA_t} < 0.$$

The share reemployed in the formal sector (F_t) decreases with higher search costs for a formal job and more attractive informal employment opportunities but increases if individuals have less resources to cope with unemployment. In Brazil, this latter channel is clearly dominated by others. The same is probably true in other developing countries but there is no theoretical reason for this to be the case.

B.2.2 Behavioral responses

Having $\frac{dx}{dP} = b\frac{dx}{db}$, we can show in our search model that:

$$\frac{dF_t}{db} < 0 \tag{21}$$

And differentiating again to highlight our 3 channels:

$$\frac{d^2F_t}{dPd\theta} > 0, \quad \frac{d^2F_t}{dPdw^I} > 0, \quad \frac{d^2F_t}{dPdA_t} > 0 \tag{22}$$

Therefore, higher formal search costs and more attractive informal employment opportunities (resp. less resources to cope with income shocks) lead to smaller (resp. larger) responses of formal reemployment rates to a UI extension.

B.2.3 Deriving a welfare formula

To derive a welfare formula, we follow Saez (2002) and assume that there are M types of individuals indexed by $m = 1, ..., M$ whose utilities enter the social welfare function with weight μ_m. In period 0, with probability s_m a formal worker (she) may become unemployed and can search for an informal and/or formal job as in the model described above. Her problem is to maximize $J_{0,m}^o$ given the parameters of the UI program and her asset level $A_{0,m}$. With probability $1 - s_m$, she keeps her formal job and has utility $v_m\left(\frac{A_{0,m}}{T} + w^f(1-\tau)\right)$. The problem of the social planner is to maximize the social welfare function from the point of view of period 0 over the parameters of the UI program $(\frac{b}{w^F}, P, \tau)$ such that the budget constraint holds (letters without the index m indicate population averages):

$$W_0 = \int_m s_m \mu_m J_{0,m}^O(b,\tau)\, dm + T\int_m (1-s_m)\mu_m v_m\left(\frac{A_{0,m}}{T} + w^F(1-\tau)\right) dm \tag{23}$$

$$\text{such that } \tau = \frac{s}{[(1-s)T]} D^B \frac{b}{w^F} \tag{24}$$

Applying the envelope theorem and the fact that $\frac{dx}{dP} = b\frac{dx}{db_P}$, we have the following formula for the welfare impact of a UI extension:

$$\frac{dW_0}{dP} = b\int_m s_m\mu_m \left[O_m(P)\frac{dJ^O_{P,m}(A^O_{P,m})}{db_P} + I_m(P)\sum_{i=0\ldots P}\mathbf{1}[e^{OI}_{i,m}=1]\frac{dJ^I_{P,m}(A^{I,i}_{P,m})}{db_P}\right]dm$$
$$-\frac{d\tau}{dP}Tw^F\int_m (1-s_m)\,\mu_m v'_m dm \tag{25}$$

Defining $S(P)$ as the survival rate out of formal employed at the end of period P, g^U_P and g^E as the social marginal welfare weight on the average individual out of formal employment at the end of period P and the average formally employed contributing to the UI system, we have:

$$g^U_P = \frac{1}{S(P)s}\int_m s_m\mu_m \left[O_m(P)\frac{dJ^O_{P,m}(A^O_{P,m})}{db_P} + I_m(P)\sum_{i=0\ldots P}\mathbf{1}[e^{OI}_{i,m}=1]\frac{dJ^I_{P,m}(A^{I,i}_{P,m})}{db_P}\right]dm$$
$$g^E = \frac{1}{1-s}\int_m (1-s_m)\,\mu_m v'_m dm$$

Taking derivatives of the budget equation, we have:

$$\frac{d\tau}{dP} = \frac{s}{[(1-s)T]}\frac{b}{w^F}\left[S(P) + \sum_{t=0}^{P}\frac{dS(t)}{dP}\right] \tag{26}$$

Substituting in the welfare formula, we can obtain the following expression for the welfare impact per layoff of a UI extension:

$$\frac{dW_0/dP}{s} = b\left[S(P)\left(g^U_P - g^E\right) - g^E\sum_{t=0}^{P}\frac{dS(t)}{dP}\right] \tag{27}$$

Reorganizing and dividing by g^E, we finally obtain:

$$\frac{dW_0/dP}{sg^E} = b\frac{dD^B}{dP}\left[\frac{S(P)}{dD^B/dP}\left(\frac{g^U_P - g^E}{g^E}\right) - \frac{\sum_{t=0}^{P}\frac{dS(t)}{dP}}{dD^B/dP}\right] \tag{28}$$

B.2.4 Informality as externality

If informal employment generates externalities, the objective function of the social planner can be modified as follows:

$$W_0 = \int_m s_m\mu_m J^O_{0,m}(b,\tau)\,dm + T\int_m (1-s_m)\mu_m v_m\left(\frac{A_{0,m}}{T} + w^F(1-\tau)\right)dm + \phi(\bar{I}) \tag{29}$$

where ϕ captures the externality cost from average informality rates $\bar{I}$. A negative externality would have $\phi(\bar{I}) < 0,\ \forall \bar{I}$. We can then derive our welfare formula as before and obtain:

$$\frac{dW_0/dP}{sg^E} = b\frac{dD^B}{dP}\left[\frac{S(P)}{dD^B/dP}\left(\frac{g^U_P - g^E}{g^E}\right) - \frac{\sum_{t=0}^{P}\frac{dS(t)}{dP}}{dD^B/dP}\right] + \frac{\phi'(\bar{I})}{sg^E}\frac{d\bar{I}}{dP} \tag{30}$$

The ratio $\frac{\phi'}{g^E}$ multiplying the impact of UI extensions on average informality rates provides a scale for the externality as it is the social value of marginal change in informality rates expressed in terms of the social value of \$1 to the formally employed. Yet we do not know a priori the sign of $\frac{d\bar{I}}{dP}$ as it is intrisically ambiguous for job–losers (see below). Moreover, a more generous UI program may attract workers to the formal sector while the (benefit) tax increase may push workers away from the formal sector. Therefore, not only is the scale of a potential externality difficult to quantify, but it is also not clear whether UI benefits will reinforce or mitigate such an externality.

B.2.5 Informality, substitution and income effects

As discussed in the text, both substitution and income effects of a UI extension will push formal reemployment rates down. In our search model, we have:

$$\frac{dF_t}{db_t}(<0) = \frac{dF_t}{dA_t}(<0) - \frac{dF_t}{dw_t^F}(>0) \tag{31}$$

This does not hold for informal employment rates as the substitution effect should push informal employment rates up:

$$\frac{dI_t}{db_t}(ambiguous) = \frac{dI_t}{dA_t}(ambiguous) - \frac{dI_t}{dw_t^F}(<0) \tag{32}$$

A negative sign for $\frac{dI_t}{db_t}$ would thus reveal that any positive substitution effect is dominated by a larger negative income effect, a valuable information about the social value of extended UI benefits. The sign of the income effect is itself ambiguous because income effects will decrease search efforts both when unemployed and when informally employed.