Online Appendix

for "Informal Labor and the Efficiency Cost of Social Programs:

Evidence from Unemployment Insurance in Brazil"

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Contents

A   Discussion of other papers

As written in footnote 11 in the Introduction, we discuss here Gonzalez-Rozada and Ruffo (2016) and earlier working papers on UI in Brazil (Cunningham, 2000; Margolis, 2008; Hijzen, 2011).

A.1   Gonzalez-Rozada and Ruffo (2016)

Gonzalez-Rozada and Ruffo (2016), who developed their paper in parallel to our work, also estimate the impact of changes in UI benefits on formal reemployment outcomes in Argentina, and analyze their implications through the lenses of an optimal UI model. However, their empirical analysis do so through proportional hazard models, which leads to two main limitations.

First, UI does not have a proportional impact on hazard rates of formal reemployment. As we show, UI has a much larger effect on hazard rates around UI benefit exhaustion and the UI literature has moved away from proportional hazard models. An important implication of their approach is that it is difficult to translate their findings into the correct measure of efficiency cost.

Second, and relatedly, by comparing the proportional impact of UI on hazard rates of formal reemployment in Argentina and in other countries with lower informality, Gonzalez-Rozada and Ruffo (2016) draw the wrong conclusion regarding the relative size of the moral hazard and efficiency cost of UI in these countries. More specifically, they conclude that:

> "*Elasticities of finding rates with respect to UI level are relatively high compared with estimates from other countries, which is reasonable because we observe only formal re-employment and because UI implies higher incentives to search for informal jobs, thus inducing a higher response to UI level. This relates to the observation that under high informality, the moral hazard effect is stronger.*" (page 213)

The elasticity of job-finding rates, however, is not the relevant measure of efficiency cost or moral hazard for UI. The moral hazard problem with insurance is when the probability of being in the insured state increases because of the insurance. The insured state with UI is being without a formal job (up until the end of the potential UI duration). So a more relevant measure of moral hazard would be the elasticity of the duration without a formal job or the efficiency cost formula in the paper. As we show in the paper, both are particularly small in our setting, even though we also find large changes in hazard rates in Brazil in percentage terms. This is because hazard rates are very low to begin with, such that larger percentage changes in hazard rates can in fact imply smaller percentage changes in the insured state in Brazil than in countries with lower informality.

A.2 Earlier working papers on UI in Brazil

As we discuss below, earlier working papers on the effect of UI in Brazil suffer from shortcomings due to limitations in the datasets used and the lack of credible identification strategies.

A.2.1 Hijzen (2011)

Hijzen (2011) attempts to estimate the impact of UI and severance payment on non-employment duration and on transitions from non-employment to formal employment, informal employment and self-employment, using longitudinal data from PME surveys between 2003 and 2010.

To estimate the impact of UI, the author uses a difference-in-difference strategy comparing differences in the hazard rate of returning to employment for displaced workers from the formal vs. the informal sector, and for displaced workers who had more than six months of tenure vs. less than six months of tenure in their last job. The idea is that a worker needs to be displaced from the formal sector and needs to have at least six months of tenure in her last job to be eligible for UI.

This approach has several limitations. First, PME surveys do not have any information on the takeup of UI benefits. Second, not all displaced formal workers with more than six months of tenure at layoff are eligible for UI. There must also be at least 16 months between a worker's layoff date and the layoff date of her last successful application to the UI system. This information is not available in PME. Third, it is not possible to know the potential UI duration of displaced formal workers with around six months of tenure in their last job. It depends on a displaced formal worker's accumulated tenure across all formal jobs in the 36 months prior to layoff. PME surveys only provide information about tenure in the last job for non-employed workers. Finally, such a strategy assumes that displaced formal workers are not selectively laid off with more than six months of tenure at layoff. We show in Section D.1 in this Appendix that there is a discontinuous jump in the density of tenure at layoff for displaced formal workers at six months of tenure.

To estimate the impact of severance payment (FGTS), the author uses another difference-in-difference strategy comparing differences in the hazard rate of returning to employment for displaced workers from the formal vs. the informal sector, and for displaced workers who had 24 to 48 months of tenure vs. more than 48 months of tenure in their last job. The idea is that only displaced formal workers contributed to their FGTS account, and thus received severance payments upon layoff, and that workers with longer tenure at a firm accumulated more funds in their FGTS accounts, and thus received a larger severance payment upon layoff.

This approach has several limitations. First, PME surveys do not have any information on the takeup of severance payments. Second, FGTS payments increase linearly with tenure, so there is no discontinuous difference in the severance payments received by workers with less than vs. more than 48 months of tenure. It is unlikely that any differential outcome for displaced formal workers

with longer tenures at layoff are only due to differences in severance payments.

A.2.2 Margolis (2008)

Margolis (2008) also attempts to estimate the impact of UI eligibility on non-employment duration and on transitions from non-employment to formal employment, informal employment, and self-employment using PME surveys (from 2003 to 2007). The empirical strategy uses a competing risks duration model with a baseline duration that shifts depending on the sector of previous job (formal and informal), UI eligibility, and potential UI duration (three, four, and five months of UI).

This approach has several limitations. First, the author assumes that all eligible displaced formal workers take up their UI benefits. We know that takeup is only around 80%. Second, he assumes that all displaced formal workers with more than six months of tenure at layoff are eligible for UI. As discussed above, this is not true. Third, he assumes that potential UI duration is a function of tenure in the last job and not a function of the accumulated tenure in all formal jobs in the last 36 months. As discussed above, this is not true. Finally, the author only compares groups of workers with different tenure levels at layoffs, but does not exploit any discontinuous change in UI benefits. The author only deals with selection issues by controlling for unobserved heterogeneity, using two mass points for the distribution of unobserved heterogeneity.

A.2.3 Cunningham (2000)

Cunningham (2000) attempts to estimate the impact of UI eligibility using a natural experiment: the change in UI legislation that took place in 1994. Before 1994, workers who had a formal job for 15 or more months in the previous 24 months were eligible for up to four months of UI. After 1994, the potential UI duration varies from three to five months of UI, according to the current schedule: workers who had a formal job for 6-11 (resp. 12-23, more than 24) months in the previous 36 months are entitled to up to three (resp. four, five) months of UI.

To estimate the impact of the change in potential UI duration on non-employment duration, reemployment wages, and sector of activity (formal and informal), the author uses a difference-in-difference strategy comparing differences in outcomes before and after the 1994 law change for workers affected by the change (treatment group) and those not affected by the change (control group). She uses repeated cross-sections from PNAD (the annual household survey) for the years 1992, 93, 95, 96 and 97 (PNAD was not carried out in 1994). Since PNAD does not have information on dates of UI application and actual UI takeup, she constructs treatment and control groups combining information on tenure in the previous job and formal-informal status.

In the preferred specification, the author uses a treatment group composed of formal workers with more than 24 months of tenure in the previous job and a control group composed of formal

workers with less than six months of tenure in the previous job. The treatment group was entitled to four months of UI before the legislation change and five months of UI after the change. The control group was not affected by the change because it was not eligible for UI in either periods.

This approach has several limitations. First, PNAD does not have any information on the actual takeup of UI benefits. She assumes that eligibility criteria are applied correctly and all eligible workers actually take up UI benefits. As discussed above, this is not true. Second, UI eligibility is noisy as PNAD is a cross-section with only retrospective and thus imprecise information on previous job tenure. Third, the author compares groups of workers with very different tenure levels at layoffs (more than 24 and less than 6 months of tenure), not exploiting any discontinuous change in UI benefits. She shows evidence that observables in the control group are very different from the treatment group. Finally, and most importantly, the use of repeated cross-sections around 1994 in a difference-in-difference strategy is valid only under implausible assumptions. In particular, 1994 was the year of the implementation of the Real stabilization plan, which reduced monthly inflation from 40% in the first half of 1994 to 1.5% in 1995. Any before-after comparison around that time is unlikely to isolate the impact of a change in the UI legislation; it is unlikely that treatment and control groups constructed based on tenure and informal status in the previous job would have responded similarly to the broader changes in the economic environment (parallel-trend assumption).

B Appendix for Section I

As discussed in Section I in the paper, the sufficient statistics formula in the paper applies generally, as long as workers internalize all consequences of their choices except on the UI budget (Chetty, 2006). This is why we only present a specific model here (and not in the paper), which is a simple extension of the dynamic model of job-search and consumption with liquidity constraints in Card et al. (2007) and Chetty (2008). First, this allows us to show more concretely how we obtain the sufficient statistics formulas that capture the usual efficiency-insurance tradeoff with UI. In so doing, we consider both a policy that changes the potential UI duration (as in the paper) and a policy that changes the UI benefit level. Second, the model presented here is also the model that we use for the simulations presented in Section I, for which we provide more details below.

B.1 A simple model of job-search with informal work opportunities

The model is in discrete time, starts in period $t = 0$, and ends in period $t = T$. A continuum of workers of mass 1 is laid off from a formal job at the start of period 0, and are eligible for UI benefit level b for a maximum of P periods (so we have $b_t = b$ for $t = 0, ..., P - 1$, and $b_t = 0$ otherwise), as long as they remain without a formal job. Once formally reemployed, workers earn a net wage $w^F - \tau$ in each period until T, where τ is a tax financing the UI system.

Job search. Each displaced formal worker i decides in each period t how much search effort $h_{i,t}$ to exert as long as she remains without a formal job. The effort level is normalized to correspond to her reemployment probability in period $t + 1$, with $h_{i,t} \in [0, 1]$. The utility cost of effort $\psi_i(h_{i,t})$ is assumed to be separable, increasing, and strictly convex. The worker's probability to remain without a formal job in period t equals the survival rate $S_{i,t} \equiv \Pi_{t'=0}^{t-1}(1 - h_{i,t'})$, with $S_{i,0} = 1$. In practice, we only observe the population average $S_t \equiv \int S_{i,t} \, di$.

Informal work opportunities. We allow displaced formal workers to generate some income endogenously by engaging in informal work activities ($l_{i,t} \geq 0$) while not yet formally reemployed. Specifically, we assume that each displaced formal worker who is not yet formally reemployed can earn additional income $l_{i,t} \cdot w^l$ (where w^l captures an informal wage) at a utility cost $\phi(l_{i,t})$ that is also assumed to be separable, increasing, and strictly convex.[1]

Other income We also allow workers to have another *exogenous* source of income $y_{i,t}$ to finance their consumption, e.g, the shared earnings of other household members.

Inter-temporal consumption. Each worker decides in each period t how much to borrow or save (at interest rate r) given her employment status. We consider time-separable preferences (with discount factor δ) and assume that worker i's per-period utility from consumption $v(c)$ is increasing and strictly concave. A worker starts with some asset level $a_{i,0}$ in period 0 and borrowing

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[1] The variable $l_{i,t}$ could also capture other (costly) income-generating activities, such as added-worker effects.

constraints prevent her from running down her assets below $\bar{a}_i$ at any time. The worker's savings decisions determine her consumption level when remaining without a formal job $c_{i,t}^{nf}$ and when formally reemployed $c_{i,t}^{f}$, respectively. While we cannot observe a worker's contingent consumption plan, we can observe average levels of consumption, for example at different lengths of the duration without a formal job, $\bar{c}_t^{fn} = \int \left(S_{i,t} \cdot c_{i,t}^{fn} \right) di$.

Agents' problem. For a given UI program, each displaced worker chooses how much to search and how much to consume in order to maximize her expected utility subject to the budget and borrowing constraints. Her choices depend on her assets and on the time spent without a formal job in addition to the UI program. In particular, the value function $U_{i,t}$ for a worker who remains without a formal job in period t can be written as follows:

$$U_{i,t}(a_{i,t}) = \max_{a_{i,t+1}, h_{i,t}, l_{i,t}} \nu(c_{i,t}^{nf}) - \phi(l_{i,t}) - \psi_i(h_{i,t}) + \delta \cdot [h_{i,t} \cdot V_{i,t+1}(a_{i,t+1}) + (1 - h_{i,t}) \cdot U_{i,t+1}(a_{i,t+1})]$$

where $a_{i,t}$ is the asset level at the start of period t and $V_{i,t+1}$ is the value function of being formally employed next period. The value function $V_{i,t}$ of being formally employed in period t is simply:

$$V_{i,t}(a_{i,t}) = \max_{a_{i,t+1}} \nu(c_{i,t}^{f}) + \delta \cdot V_{i,t+1}(a_{i,t+1})$$

The consumption path must also satisfy the borrowing constraint $a_{i,t} > \bar{a}_i$, as well as the following inter-temporal budget constraints and budget balance conditions at the end of life:

$$c_{i,t}^{nf} = a_{i,t} + y_{i,t} + b_t + l_{i,t} \cdot w^l - \frac{a_{i,t+1}}{(1+r)} \text{ for } t = 0...T-1 \qquad c_{i,t}^{nf} = a_{i,t} + y_{i,t} + b_t + l_{i,t} \cdot w^l \text{ for } t = T$$

$$c_{i,t}^{f} = a_{i,t} + y_{i,t} + w^f - \tau - \frac{a_{i,t+1}}{(1+r)} \text{ for } t = 1...T-1 \qquad c_{i,t}^{f} = a_{i,t} + y_{i,t} + w^f - \tau \text{ for } t = T$$

The first order conditions for a worker who remains without a formal job in period t are:

$$\frac{\partial \nu(c_{i,t}^{nf})}{\partial c_{i,t}^{nf}} = (1+r) \cdot \delta \cdot \left[h_{i,t} \cdot \frac{\partial \nu(c_{i,t+1}^{f})}{\partial c_{i,t+1}^{f}} + (1 - h_{i,t}) \cdot \frac{\partial \nu(c_{i,t+1}^{nf})}{\partial c_{i,t+1}^{nf}} \right]$$

$$\frac{\partial \psi_i(h_{i,t})}{\partial h_{i,t}} = \delta \cdot [V_{i,t+1}(a_{i,t+1}) - U_{i,t+1}(a_{i,t+1})]$$

$$\frac{\partial \phi_i(l_{i,t})}{\partial l_{i,t}} = w^l \frac{\partial \nu(c_{i,t}^{nf})}{\partial c_{i,t}^{nf}}$$

The first order condition for a worker formally reemployed is simply:

$$\frac{\partial v(c_{i,t}^f)}{\partial c_{i,t}^f} = (1+r) \cdot \delta \cdot \frac{\partial v(c_{i,t+1}^f)}{\partial c_{i,t+1}^f}$$

B.2 Sufficient statistics formulas

A common approach in the UI literature to evaluate the incentive-insurance trade-off with UI reforms is to express it in terms of sufficient statistics that can be estimated empirically.

Government's problem. The balanced-budget equation for the UI program is (as in other papers, we ignore time discounting for now, i.e., $1+r = \delta = 1$):

$$\tau \cdot \left(T - D^{NF}\right) = b \cdot D^B$$

where $D^B = \sum_{t=0}^{P-1} S_t$ is the average *paid UI duration* and $D^{NF} = \sum_{t=0}^{T} S_t$ is the average *duration without a formal job*. The government's problem is to maximize welfare:

$$W = \int U_{i,0} \, di.$$

We assume that the social welfare function is differentiable.

Sufficient statistics formulas. We now derive sufficient statistics formulas for the welfare effect of changes in the UI benefit level (db) and in the potential UI duration (dP), separately.

First, consider a marginal change in the UI benefit level, b:

$$\frac{dW}{db} = \int \frac{\partial U_{i,0}}{\partial b} \, di + \frac{d\tau}{db} \int \frac{\partial U_{i,0}}{\partial \tau} \, di$$

$$= D^B \cdot \mathbb{E}^B \left[\frac{\partial v(c_{i,t}^{nf})}{\partial c_{i,t}^{nf}} \right] - \left[D^B + b \cdot \frac{dD^B}{db} + \tau \cdot \frac{dD^{NF}}{db} \right] \cdot \mathbb{E}^F \left[\frac{\partial v(c_{i,t}^f)}{\partial c_{i,t}^f} \right]$$

where the first equality arises from the application of the envelope theorem. The two expectations $\mathbb{E}^B \left[\frac{\partial v(c_{i,t}^{nf})}{\partial c_{i,t}^{nf}} \right]$ and $\mathbb{E}^F \left[\frac{\partial v(c_{i,t}^f)}{\partial c_{i,t}^f} \right]$ are the average marginal utility of consumption over the periods that workers would be drawing UI benefits in absence of the reform (the target beneficiaries for an increase db) and the average marginal utility of consumption when formally reemployed, respectively. Dividing by $\mathbb{E}^F \left[\frac{\partial v(c_{i,t}^f)}{\partial c_{i,t}^f} \right]$ to obtain a money metrics and by D^B to normalize by the mechanical effect of the reform, we obtain an expression for the welfare effect per \$1 spent on

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target beneficiaries:

$$
\frac{\mathbb{E}^B\left[\frac{\partial v(c_{i,t}^{nf})}{\partial c_{i,t}^{nf}}\right] - \mathbb{E}^F\left[\frac{\partial v(c_{i,t}^{f})}{\partial c_{i,t}^{f}}\right]}{\mathbb{E}\left[\frac{\partial v(c_{i,t}^{F})}{\partial c_{i,t}^{f}}\right]} - \left[\frac{b \cdot (dD^B)/db)}{D^B} + \frac{\tau \cdot (dD^{NF}/db)}{D^B}\right]
$$

$$
= \frac{\mathbb{E}^B\left[\frac{\partial v(c_{i,t}^{nf})}{\partial c_{i,t}^{nf}}\right] - \mathbb{E}^F\left[\frac{\partial v(c_{i,t}^{f})}{\partial c_{i,t}^{f}}\right]}{\mathbb{E}\left[\frac{\partial v(c_{i,t}^{F})}{\partial c_{i,t}^{f}}\right]} - \left[\eta_{D^B,b} + \frac{D^{NF}}{T - D^{NF}} \cdot \eta_{D^{NF},b}\right]
$$

The first term captures the value of insurance and the second term the efficiency cost. The first line shows that the efficiency cost of an increase in the UI benefit level can be expressed as the ratio of the behavioral effect to the mechanical effect on the UI budget. The second line shows how that efficiency cost can also be expressed in terms of behavioral elasticities, i.e., the elasticity of the average paid UI duration ($\eta_{D^B,b}$) and of the average duration without a formal job ($\eta_{D^{NF},b}$).

Second, consider a marginal change in the potential UI duration, P, which is equivalent to a marginal change in b_P multiplied by b (Schmieder et al., 2012):

$$
\frac{dW}{dP} = \int \frac{\partial U_{i,0}}{\partial P}\,di + \frac{d\tau}{dP}\int \frac{\partial U_{i,0}}{\partial \tau}\,di
$$

$$
= b \cdot S_P \cdot \mathbb{E}^P\left[\frac{\partial v(c_{i,P}^{nf})}{\partial c_{i,P}^{nf}}\right] - \left[b \cdot S_P + b \cdot \frac{dD^B}{dP}\Big|_B + \tau \cdot \frac{dD^{NF}}{dP}\right] \cdot \mathbb{E}^F\left[\frac{\partial v(c_{i,t}^{f})}{\partial c_{i,t}^{f}}\right]
$$

where $\frac{dD^B}{dP}\big|_B$ is the behavioral effect on the average paid UI duration and the expectation $\mathbb{E}^P\left[\frac{\partial v(c_{i,P}^{nf})}{\partial c_{i,P}^{nf}}\right]$ is the average marginal utility of consumption for workers who remain without a formal job at the end of the potential UI duration (i.e., in period P) in absence of the reform (the target beneficiaries for an increase dP). Dividing by $\mathbb{E}^F\left[\frac{\partial v(c_{i,t}^{f})}{\partial c_{i,t}^{f}}\right]$ to obtain a money metrics and normalizing by the mechanical effect ($b \cdot S_P$), we obtain an expression for the welfare effect per \$1 spent on target beneficiaries:

$$
\frac{\mathbb{E}^P\left[\frac{\partial v(c_{i,P}^{nf})}{\partial c_{i,P}^{nf}}\right] - \mathbb{E}^F\left[\frac{\partial v(c_{i,t}^{f})}{\partial c_{i,t}^{f}}\right]}{\mathbb{E}^F\left[\frac{\partial v(c_{i,t}^{f})}{\partial c_{i,t}^{e}}\right]} - \left[\frac{\frac{dD^B}{dP}\big|_B}{S_P} + \frac{\tau}{b} \cdot \frac{\frac{dD^{NF}}{dP}}{S_P}\right],
$$

which is the formula in equation (1) in the paper.

B.3 Model simulations

We make a few additional assumptions for the model simulations in Section I.

Time and job search. We assume that time periods correspond to months, such that $h_{i,t}$ corresponds to the probability of reemployment from one month to the next. The search cost function is assumed to take the form: $\psi_i(h_{i,t}) = \kappa_i \cdot h_{i,t}^{1+\theta}/(1+\theta)$. We assume that the cost of search κ_i varies across two worker types: a proportion s_1 of workers is of the higher-cost type (κ_1) and a proportion $(1-s_1)$ is of the lower-cost type (κ_2). With this functional form, the first-order condition for search effort $h_{i,t}$ becomes: $\kappa_i \cdot h_{i,t}^{\theta} = \delta \cdot [V_{i,t+1}(a_{i,t+1}) - U_{i,t+1}(a_{i,t+1})]$, showing that θ is the inverse of the elasticity of search effort with respect to the net gain from reemployment in period $t+1$.

Utility of consumption. We assume that the per-period utility of consumption takes the form $v(c_{i,t}) = (c_{i,t}^{1-\gamma} - 1)/(1-\gamma)$, where γ is the coefficient of relative risk aversion. We assume $\gamma = 2$. We also assume a monthly discount factor of $\delta = .995$ (equivalent to a 6% yearly discount rate).

Informal work opportunities. We assume that the utility cost of informal work takes the form: $\phi(l_{i,t}) = \chi \cdot l_{i,t}^{1+\lambda}/(1+\lambda)$, where χ is a scaling factor for the cost of informal work and λ is the inverse of the elasticity of informal work with respect to the utility gain from working informally (with this functional form, the first-order condition for informal work $l_{i,t}$ becomes: $\chi \cdot l_{i,t}^{\lambda} = w^l \cdot \partial v(c_{i,t}^{nf})/\partial c_{i,t}^{nf}$). We set $w^l = w^f$ in the model simulation for simplicity, but we impose the restriction $l \in [0,1]$ such that informal earnings cannot exceed formal earnings.

Other income. We assume that workers' other source of income to finance their consumption $y_{i,t}$ is constant over time. Specifically, we assume that workers live with another adult who earns the formal wage w^f in all periods. Moreover, we assume that the two of them pool all their financial resources, such that workers have an income of at least $w^f/2$ in all periods.

Saving and borrowing. Workers are assumed to have assets at layoff equal to one monthly wage w^f, they cannot borrow against their future income, and any savings yield zero interest, $r = 0$.

UI benefits. In the simulations in which displaced workers are eligible for UI, we assume that all of them take up UI and that the UI benefit level is $b = .69 * w^f$ (a replacement rate of 69%). In the simulations, we assume in turn no UI benefits, a potential UI duration of $P = 4$ months (benefits paid in $t = 0, ..., 3$), and a potential UI duration of $P = 5$ months (benefits paid in $t = 0, ..., 4$).

Simulations. For a given vector of parameters ξ, the model is solved by backwards induction.[2] We choose the vector of parameters $(\kappa_1, \kappa_2, s_1, \theta, \chi, \lambda)$ for "convenience" (we do not estimate these parameters, they are calibrated) in the sense that it allows us to illustrate the points that we make in Section I nicely. For the simulations in which we increase the cost of finding a formal job, we double the values of both κ_1 and κ_2. For the simulations in which we increase the cost of finding a formal job, we double the values of both κ_1 and κ_2. For the simulations in which we decrease

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[2]The Matlab code used in DellaVigna et al. (2017), shared by Johannes Schmieder, formed the basis for our code.

the cost of working informally, we decrease χ by 80%. These changes in model parameters were chosen such that survival rates are comparable between the two sets of simulations and broadly in line with the formal reemployment rates that we observe in the data.

C   Appendix for Section II

We provide here some additional details for Section II of the paper.

C.1   Full schedule of UI benefit levels

The UI benefit level in Brazil depends on a displaced formal worker's average wage in the three months prior to layoff and ranges from 1 minimum wage to 1.87 minimum wages. We show here the full schedule of UI benefit levels. Define w the displaced formal worker's average nominal wage in the three months prior to layoff, expressed in multiples of the prevailing minimum wage (mw). Her UI benefit level (b) is then calculated as follows:

- $b = 1\ mw$ if $w < 1.25$

- $b = .8\ w$ if $1.25 \leq w < 1.65$

- $b = 1.32\ mw + .5\ (w - 1.65)$ if $1.65 \leq w < 2.75$

- $b = 1.87\ mw$ if $w \geq 2.75$

Figure C.1 displays the relationship between w and b graphically, as well as the relationship between w and the replacement rate (b/w). As discussed in the paper, replacement rates are particularly high at the bottom of the wage distribution, but all our results are robust to excluding workers with very high or very low replacement rates.

Figure C.1: UI benefit level and UI replacement rate in Brazil

(a) UI benefit level

(b) UI replacement rate
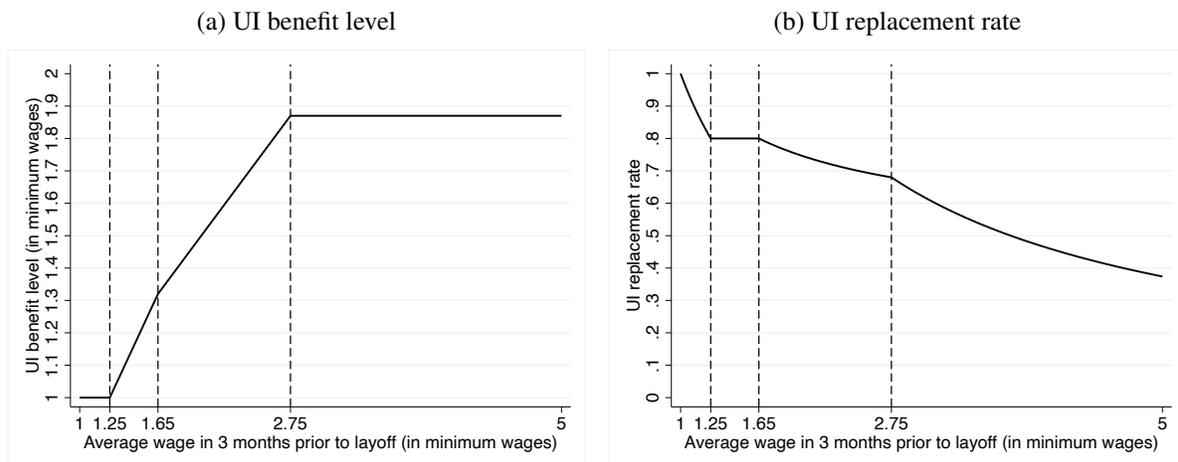
Notes: The figure displays the UI benefit level in panel (a) and the UI replacement rate in panel (b) for the Brazilian UI program. They are a function of a displaced formal worker's average nominal wage in the three months prior to layoff, expressed in multiples of the prevailing minimum wage.

C.2 Efficiency cost formula for a UI program funded by a sales tax

A departure from the conceptual framework in the paper is that UI is financed by a .65% tax on firms' sales in Brazil. We consider instead the case of a tax on formal workers, which is the main source of funding for UI in other countries, including developing countries (Velásquez, 2010). As we argue in the paper, a tax on formal workers is the more interesting case conceptually. We thus use Brazil as an empirical setting to estimate and illustrate the efficiency cost of increases in UI benefits in a context of high informality as derived in a benchmark framework. We explain here why this is unlikely to matter a lot for our conclusion that the efficiency cost is relatively low.

In the paper, the sufficient statistics formula is:

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

Moreover, using the UI budget constraint: $\tau \times (T - D^{NF}) = b \times D^B$, we have $\frac{\tau}{b} = \frac{D^B}{T - D^{NF}}$. When we implement the formula we assume $\frac{D^B}{T - D^{NF}} = .086$ (this is the average number of UI beneficiaries per formal employee between 2005 and 2009), such that we obtain an efficiency cost equal to: $\frac{.126 + .086 \times .389}{.735}$. As pointed out by Schmieder and von Wachter (2017), for a fixed payroll tax and a fixed UI replacement rate, $\frac{\tau}{b}$ is the ratio of the payroll tax rate to the UI replacement rate. Using the average UI replacement rate in our RD sample (79%), our assumption of $\frac{\tau}{b} = .086$ is thus equivalent to assuming a payroll tax rate of 6.8% (we chose to err on the conservative side when choosing the factor scaling the behavioral effect on the duration without a formal job).

With a sales tax, we can assume that the UI budget constraint becomes: $\tau_s \times Y(T - D^{NF}) = b \times D^B$, where τ_s is a sales tax and we allow taxable sales Y to possibly depend on the duration formally employed after layoff $T - D^{NF}$. If workers staying longer without a formal job does not affect taxable sales, the impact of behavioral responses on the duration without a formal job does not matter anymore for the sufficient statistics formula. In that case, we would have:

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

where u'^F is now the marginal utility of those bearing the incidence of the sales tax. The efficiency cost would become $\frac{.126}{.735} = .171$ and we could still conclude that this is much lower than the ratio of the behavioral effect on the paid UI duration to the mechanical effect from prior studies for the U.S. Moreover, this ratio is also decreasing with informality rates in our empirical analysis (the numerator is decreasing while the denominator is increasing).

If workers staying longer without a formal job affect taxable sales (e.g., because workers spend

less time producing any formal output), the impact of behavioral responses on the duration without a formal job would matter in the following way for the sufficient statistics formula:

$$\frac{dW/dP}{b \times S_P \times u'^F} = \left(\frac{u'^P - u'^F}{u'^F}\right) - \left(\frac{\frac{dD^B}{dP}|_B}{S_P} + \frac{\frac{\tau_s}{b} \times \frac{dY}{d(T-D^{NF})} \times \frac{dD^{NF}}{dP}}{S_P}\right)$$

So with a sales tax of .65%, the efficiency cost would be the same as in the paper if we had: $\frac{dY}{d(T-D^{NF})} = \frac{.086}{.0065} \times b = 13.2 \times b$, i.e., if delaying formal reemployment by one month implied a reduction in taxable sales equivalent to more than 13 times the value of the monthly UI benefit payment received by the worker. It seems fair to view this as a conservative estimate.

C.3 Additional details on the data

We provide below more information about the four main datasets used in the paper: the matched employee-employer data (*RAIS*), the UI registry, the yearly household surveys (*PNAD*), and the monthly labor force surveys (*PME*).

C.3.1 RAIS

RAIS is a matched employee-employer dataset covering by law the universe of formal employees, including public employees. Every year, in March, tax-registered firms must report every worker formally employed during the previous calendar year. An observation in RAIS thus corresponds to a formal employment spell in a given year. An example of the form that firms must fill and send to the Brazilian Labor Ministry can be found here: http://www.rais.gov.br/sitio/download.jsf#gdPrazo

For this project, we were granted access to a version of the RAIS dataset with unmasked worker, firm, and establishment identifiers by the Brazilian Labor Ministry for the years 1995 to 2010. The set of variables available in the dataset varies slightly from year to year. We describe here the variables used in the paper.

Variables:

- PIS: This is a worker identifier, similar to a social security number. It is available in all years.

- CNPJ: This is an establishment identifier. It is available in all years. The first eight digits of the CNPJ also uniquely identify the firm that the establishment belongs to.

- Year: This variable records the prevailing year. It is available in all years.

- Tenure: This variable records the job tenure of a worker from the date of hiring, which is potentially in a previous year, until either the date of separation if the spell ended during the year or December 31st of the prevailing year if the spell did not end during the year. It is available in all years. Tenure is recorded in tenth of a month (e.g., 6.1 months of tenure).

- Age: This variable records the age of a worker, either at separation or on December 31st of the prevailing year. It is available in all years. Age is recorded in years.

- Gender: This variable records the gender of a worker. It is available in all years.

- Education: This variable records the education level of a worker, either at separation or on December 31st of the prevailing year. It is available in all years. Education is recorded as a categorical variable identifying different education levels: no education, incomplete first part of primary school, complete first part of primary school, incomplete second part of primary school, complete primary school, incomplete middle school, complete middle school, incomplete high school, complete high school, incomplete college, complete college. We transform this variable in a "years of education" variable using mid-values in each schooling category range. For instance, we assign a value of 2 years for "incomplete first part of primary school," which would be in the range 1-3 years of schooling. We also create another variable with 5 education categories (incomplete primary school, completed primary school, completed middle school, completed high school, completed college), which we use as controls in some of our regressions (as dummies).

- Sector of activity: There are two variable recording the sector of activity of a given establishment. The first variable captures an aggregate sector definition, which only uses 24 categories. The second variable captures a detailed sector definition, which is comparable to usual sector definitions in other countries (7-digit code). The two variables are available in all years. The categorization for the second variable was changed in 2006. We use the first variable to create another variable with 4 sector categories (construction, industry, commerce, and services), which we use as controls in some of our regressions (as dummies).

- Establishment size: This variable records the number of workers at an establishment on December 31st of the prevailing year. It is available in all years. Size is recorded as a dummy variable identifying 10 different ranges: zero workers, up to 4 workers, between 5 and 9 workers, between 10 and 19 workers, between 20 and 49 workers, between 50 and 99 workers, between 100 and 249 workers, between 250 and 499 workers, between 500 and 999 workers, more than 1000 workers. We include a dummy for each of the 10 categories when we control for establishment-size dummies in our regressions. We also use this variable to identify small establishments with fewer than 10 employees and large establishments with more than 100 employees.

- Location: There are two variables recording the location of a given establishment in the prevailing year. The first variable records the state where the establishment is located. There are 27 states in Brazil. The second records the municipality where the establishment is located. There are 5500 municipalities in Brazil. Both of these variables are available in all years. The boundaries of some of the Brazilian municipalities have been re-drawn between 1995 and 2010.

- Reason for separation: This variable records the reason for separation if the spell ended during the year. It is available in all years. Reason for separation is recorded as a dummy variable

identifying different reasons for separation. We focus in the paper on separations without just cause (involuntary) initiated by the employer given the eligibility criteria of the Brazilian UI program.

- Hiring date: This variable records the hiring date of a worker. It is only available since 2002. In previous years, the dataset includes a variable recording the hiring month of a worker if she was hired during the prevailing year. We use this variable to construct several outcome variables in the paper. We also use this variable to construct a dummy variable for the calendar month of hiring (January, ..., December), and we include a dummy for each of the 12 calendar months when we control for calendar month fixed effects for the month of hiring in some of our regressions.

- Separation date: This variable records the separation date of a worker. It is only available since 2002. In previous years, the dataset includes a variable recording the separation month of a worker, if the separation took place during the prevailing year. We use this variable to construct several outcome variables in the paper. We also use this variable to construct a dummy variable for the calendar month of separation (January, ..., December), and we include a dummy for each of the 12 calendar months when we control for calendar month fixed effects for the month of separation in some of our regressions.

- Wage: There are four variables recording wages. There are two variables recording the average monthly wage of a worker over the employment spell during the year, the first one in nominal terms and the second one in multiples of the minimum wage. There are also two variables recording the monthly wage of a worker in December of the prevailing year, again in nominal terms and in multiples of the minimum wage. We obtain real wages by dividing nominal wages by a consumer price index (IPCA); we use either the average of the values of the price index over the employment spell or the value of the price index in December. We use the average wage to approximate the wage measure used to calculate a displaced worker's statutory UI benefit level and UI replacement rate. We include a 4th order polynomial in the (demeaned) logarithm of the average wage in real terms when we control for wages in some of our regressions. We use the December wage when we investigate the impact of an increase in potential UI duration on wages for workers formally employed several months after displacement. This is because the December wage is more comparable across workers (it is not an average over spells of different lengths for different workers). Note that wage measures include all pecuniary compensations (bonuses, etc.).

- Contract type: This variable records the type of contract. It is available in all years. It allows us to focus on workers with the most common type of contract in the Brazilian formal sector (CLT): urban workers with an open-ended contract at an incorporated business.

- Legal form: This variable categorizes the legal form of the firm as assigned by the tax authority. The codes correspond to a juridical classification of firms, which depends on how firms are formally organized (state firms, private firms, public administration, foundations, non-profit, etc). It is available in all years. It allows us to focus on workers displaced from private-sector firms.

- Contracted hours: This variable records the number of hours per week according to the contract between the employee and the employer. It is available in all years. It allows us to focus on full-time workers.

Notes:

We only use data from 2002 to 2010 in the paper in order to measure hiring and separation dates precisely. In a previous version of this paper, we used data from earlier years as well. We then had to impute/predict hiring and separation days within a month. Results were comparable. Therefore we focus on the more recent years to avoid creating unnecessary noise in measures of hiring and separation dates, and thus in measures of the duration without a formal job.

C.3.2 UI registry

The UI registry includes individual-level data from the Brazilian UI program. For this project, we were granted access to a version of the UI registry data with unmasked worker identifiers by the Brazilian Labor Ministry for the years 1995 to 2012. The registry includes two datasets: an application dataset and a payment dataset. In the first dataset, an observation is a UI spell. In the second dataset, an observation is a UI payment. The set of variables available in both datasets varies slightly from year to year. We describe here the main variables used in the paper.

Variables from the application dataset:
- PIS: This is a worker identifier, similar to a social security number. It is available in all years.
- UI spell identifier: This is an identifier for the UI spell which allows us to match information between the application and payment datasets for a given UI spell. It is available in all years.
- Situation/status: There are two variables recording the current status of a UI application. They are available in all years. It allows us to know whether an applicant was deemed eligible for UI.
- Wage: There are three variables recording the wage of the applicant in each of the three months before layoff. They are available in all years.
- Hiring date: This variable records the hiring date of the applicant. It is available in all years.
- Separation date: This variable records the separation date of the applicant. It is available in all years.
- Application date: This variable records the application date to the UI system. It is available in all years.
- Habilitation date: This variable records the date at which the applicant was deemed eligible by the UI system. It is available in all years.
- Takeup date: This variable records the date at which the applicant became a UI beneficiary. It is available in all years.

Unfortunately, the dataset does not tell us the potential UI duration that the applicant was deemed eligible for by the UI system at application.

Variables from the payment dataset:

- PIS: This is a worker identifier, similar to a social security number. It is available in all years.

- UI spell identifier: This is an identifier for the UI spell which allows us to match information between the application and payment datasets for a given UI spell. It is available in all years.

- Situation: This is a variable recording the situation of a given UI payment. It is available in all years. It allows us to know whether a given monthly payment was withdrawn or not by the UI beneficiary.

- Sequence number: This is a variable recording whether a given UI payment is the first payment, second payment, etc., in a given UI spell. It is available in all years.

- UI benefit level: This is a variable recording the value of the UI payment. It is available in all years.

- Type: This is a variable recording whether a given UI payment is part of a regular UI spell (given a worker's regular potential UI duration) or part of a temporary UI extension. There were a few temporary UI extensions over the years covered by our data. The variable is available in all years.

- Emission date: This variable records the date at which a UI payment was emitted (made available to a beneficiary). It is available since 2005. In previous years, we only have information about the month and year of emission.

- Payment date: This variable records the date at which a UI payment was actually paid (withdrawn). It is available since 2005. In previous years, we only have information about the month and year of payment.

Notes:

We primarily use data from 2005 to 2010 in the paper in order to measure not only hiring and separation dates precisely, but also UI payment dates precisely. This is important when we construct our measures of takeup and of actual and counterfactual paid UI duration. However, in robustness checks, we also show that all our results are robust if we use data prior to 2005. In this case, we must impute the day of UI emission and the day of UI payment within a month. We proceed as follows. We regress the day of emission (or payment) on a fully saturated set of calendar-month of emission (or payment) dummies (Jan, ..., Dec) and separation day (within a month) dummies, using data from 2005 to 2010. We then use the model to predict the day of emission (or payment) in all years from 2002 to 2010. We estimate such models separately for the first monthly benefit, the second monthly benefit, etc., in a UI spell.

C.3.3 PNAD

We use the microdata of Brazilian yearly household surveys (Pesquisa Nacional de Amostra por Domicílios, PNAD), which are conducted by the Brazilian Institute of Geography and Statistics (IBGE), to measure labor market informality in each of the 27 Brazilian states. PNAD surveys were conducted in one week in September (usually the third week) every year between 2002 and 2009, the period covered by our analysis (PNAD surveys were not conducted in 2010 because of the decennial census). Households are sampled from all 27 Brazilian states and the sampling is such that the surveys are representative at the state level.[3] Finally, PNAD surveys are repeated cross-sections and do not include any longitudinal component.

The microdata of PNAD surveys are organized in two datasets, the first one at the household level, and the second one at the individual level. Information is provided for all residents of the household. In the paper, we use the individual-level data and the variables described below. In PNAD surveys, the respondent for a given household (typically, the household head) is asked about labor market and income information for every household member above ten years old in the previous month. To guarantee confidentiality, survey respondents cannot be matched to other datasets. So, respondents have no incentive to lie for questions related to labor market informality.

Variables from the individual-level data:
- Year: This variable records the prevailing year. It is available in all years.
- State: This variable records the state where an individual resides. It is available in all years.
- Survey weight: This variable records the weight attached to a given individual in the survey sampling strategy. It is available in all years. We use this variable to make sure that the statistics that are based on PNAD surveys in the paper are representative at the state or national levels.
- Age: This variable records the age of an individual. It is available in all years.
- Gender: This variable records the gender of an individual. It is available in all years.
- Labor force participation: This variable records whether an individual participates in the labor force (i.e., is employed or unemployed). It is available in all years.
- Employed: This variable records whether an individual is working/employed if she participates in the labor force (vs. is unemployed). It is available in all years.
- Agricultural employment: This variable records whether an individual works in the agricultural sector. It is available in all years. We use this variable to identify non-farm workers.
- Employment status: This variable records the specific employment status of an employed individual. It is generated by the IBGE using answers to a series of questions in the survey and it is available in all years. In particular, the variable identifies: employees of private-sector firms

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[3]Within a state, the surveys are also representative at the level of metropolitan areas in the nine states that created metropolitan areas before 1988. There are only nine such metropolitan areas.

with the working card signed (i.e., formal employees), employees of the military forces and public servants, employees of private-sector firms without the working card signed (i.e., informal employees), domestic employees, self-employed workers, employers, and unpaid workers.

- Income: This variable records the income of an individual during the previous month from all income sources. It is available in all years. It is recorded in nominal terms. We convert it in real terms using the consumer price index prevailing in September in each year.

Notes:

We describe the composition of the labor force between 2005 and 2009 in Figure 1 in the paper using the above variables. For instance, the share of non-farm private-sector formal employees is constructed by computing the share of adults (between 19 and 54 years old) at the national level in each year, who are employees of a private-sector firm with the working card signed and are non-farm workers. We take the average of this variable over our five-year period. The share of non-farm informal workers is constructed similarly by considering adults who are employees of a private-sector firm without the working card signed or self-employed workers. The share of non-farm workers with other non-farm employment is constructed by considering adults with any of the other categories of the employment status variable. The share of unemployed workers is constructed by considering adults who are unemployed according to the employed variable. The share of farm workers is constructed by considering adults who work in the agricultural sector according to the agricultural employment variable. In Figure 1a, these shares are averaged over our five-year period and divided by the average share of individuals who participate in the labor force. For Figure 1b, we construct the average over our five-year period of the shares of non-farm private-sector formal employees and informal workers by state and divide these shares by the average share of adults who participate in the labor force and are not farm workers in each state.

We also use the above variables to construct the average state-level informality rates used in our empirical analysis. For instance, we construct a variable recording the share of informal workers among adults who participate in the labor force and are not farm workers in the state and year of layoff for all displaced formal workers in our RD sample. The average informality rate in Figure 6 is then the average of this variable among the displaced workers in the RD sample for that state.

C.3.4 PME

We also use the microdata of Brazilian monthly labor force surveys (Pesquisa Mensal de Emprego, PME), which are conducted by the IBGE in the six largest metropolitan areas of Brazil (São Paulo, Rio de Janeiro, Belo Horizonte, Porto Alegre, Salvador, and Recife). The sampling is such that the surveys are representative at the metropolitan-area level in each month. The surveys were conducted every month between 2003 and 2010, the end of the period covered by our analysis. As

discussed below, we cannot use data from PME surveys conducted prior to 2003 because the set of questions was different (and not suitable for our analysis) prior to 2003. In contrast to PNAD surveys, PME surveys have a rotating panel structure. Households are surveyed for two periods of four consecutive months, eight months apart from each other. Interviews are spread evenly within a month and households are always interviewed in the same week of the month (IBGE, 2007).

Variables:

- State: This variable records the state where an individual resides. It is available in all months. There is at most one of the six metropolitan areas in a given state, so this variable also identifies the metropolitan area.

- Control and series numbers: These variables taken together uniquely identify a household. They are used to create a household ID number, which allows us to track households across the eight months in the panel. It is available in all months.

- Panel ID number: This variable identifies the survey panel that a given household belongs to. It is available in all months. The rotating panel structure of PME is such that, every month, two panel groups leave the sample (each one corresponding to 1/8 of the sample). As a result, half of the sample remains the same for every 12-month period.

- Rotational group ID: This variable identifies the rotational group within a survey panel that a given household belongs to. It is available in all months.

- Individual identifier: This variable records the number attached to a given individual within a household. It allows us to track an individual over time, and not just a household. It is constructed using the methodology described in Ribas and Soares (2008). It is available in all months.

- Interview number: This variable records the interview number (each household enters the panel for at most 8 monthly interviews) for a given household. It is available in all months.

- Survey month: This variable records the survey month. It is available in all months.

- Survey year: This variable records the survey year. It is available in all months.

- Survey weight: This variable records the weight attached to a given individual in the survey sampling strategy. It is available in all months. We use this variable to make sure that the results based on PME surveys in the paper are representative at the level of the six metropolitan areas.

- Gender: This variable records the gender of an individual. It is available in all months.

- Year of birth: This variable records the year of birth of a given individual. It is available in all months.

- Month of birth: This variable records the month of birth of a given individual. It is available in all months.

- Day of birth: This variable records the day of birth of a given individual. It is available in all months. Note that we use the year/month/day of birth variables to calculate an individual's age when we restrict our sample to adults who were between 19 and 54 years old at layoff.

- Education: There are several variables recording the education level of an individual. They are available in all months. We use these variables to create an education variable that includes the same categories as the education variable in RAIS, described above.

- Employed: This variable records whether an individual is employed in the reference week. It is available in all months. It records separately individuals who are employed, unemployed, and not in the labor force.

- Employment status if employee: This variable records the type of employment for individuals who are employees in the reference week. It is available in all months. It records separately employees in the private sector with their working card signed (i.e., formal employees), employees in the private sector without their working card signed (i.e., informal employees), employee in the public sector, employee of the military forces.

- Employment status if working but not as employee: This variable records the type of employment for individuals who are working but not as employees in the reference week. It is available in all months. It records separately self-employed workers and employers.

- Other employment status: This variable records other type of employment in the reference week. It is available in all months. It records separately domestic workers, individuals working for own production, and unpaid workers.

Note: All the above variables allow us to construct a variable "Employment status" similar to the variable existing in PNAD surveys.

- Sector of activity: This variable records the sector of activity for employed individuals. It is available in all months. The variable uses a different categorization than the sector variables in RAIS. We aggregated the categories for this variable in order to create a variable with the same categorization as the first sector variable in RAIS.

- Earnings: This variable records the monthly earnings from an individual's main job. It is available in all months. It is recorded in gross terms, before taxes statutorily levied on individuals. We transform it in net earnings, by deducting income and social security taxes paid by employees according to the official rates and thresholds existent in each month. We apply this transformation only to the earnings of formal employees, such that gross and net earnings are the same for individuals with another employment status. The variable is recorded in nominal terms. We convert it in real terms using the consumer price index prevailing in each month.

- Total earnings: This variable records the total monthly earnings from an individual. It is available in all months. It is recorded in gross terms, before taxes statutorily levied on individuals. We transform it in net total earnings, by deducting income and social security taxes paid by employees according to the official rates and thresholds existent in each month. We apply this transformation only to the total earnings of formal employees, such that gross and net total earnings are the same for individuals with another employment status. As a result, we likely underestimate the net total

earnings of formal employees, as they may have secondary sources of earnings from informal jobs. The variable is recorded in nominal terms. We convert it in real terms using the consumer price index prevailing in each month.

- Laid off: This variable records whether an individual was separated from a job within the reference period for individuals who are non-employed at the time of the interview. It is available in all months.

- Non-employment duration: This variable records the time since an individual was separated from her last job (years and months) for individuals who are non-employed at the time of the interview. It is available in all months.

- Separation reason: This variable records the reason of separation for their last job for individuals who are non-employed at the time of the interview. It is available in all months. Importantly for us, it records whether an individual was laid off (i.e., involuntary separation).

- Employment type in last job: This variable records the employment type of their last job (employee, self-employed, domestic worker, etc), for individuals who are non-employed at the time of the interview. It is available in all months.

- Employed in public sector in last job: This variable records whether an individual was employed by the public sector or the military forces in her last job, for individuals who are non-employed at the time of the interview. It is available in all months.

- Formally employed in last job: This variable records whether an individual was formally employed in her last job (they had their working card signed), for individuals who are non-employed at the time of the interview. It is available in all months.

- Job tenure in last job: This variable records the job tenure in the last job for individuals who are non-employed at the time of the interview. It is available in all months. Importantly for us, it records whether an individual had more than two years of tenure at layoff.

- Contract type in last job: This variable records the contract type (temporary vs. open-ended) in the last job for individuals who are non-employed at the time of the interview. It is available in all months.

- Sector of activity in last job: This variable records the sector of activity of their last job for individuals who are non-employed at the time of the interview. It is available in all months. We aggregated the categories for this variable in order to create a variable with the same categorization as the first sector variable in RAIS.

Note: All the above variables allow us to identify non-employed workers involuntarily laid off from a formal job with more than two years of job tenure at layoff and who had an open-ended contract, and to know the time they have spent non-employed since layoff.

Notes:

We cannot use data prior to 2003 because questions about the non-employment duration and

the characteristics of the last job were only asked to individuals who reported to be actively looking for a job at the time of the interview. Therefore, using this subset of non-employed individuals to estimate survival rates in non-employment and hazard rates of reemployment for *all* non-employed individuals (not only for those actively looking for a job) would likely be biased.

The structure of PME surveys and the variables recorded in PME surveys allow us to estimate survival rates in non-employment and hazard rates of overall reemployment (formal or informal) for displaced formal workers. First, we observe individuals at given non-employment durations (less than 1 month, 1 month, 2 months, 3 months, etc.) in their first to third and fifth to seventh interview. We can thus observe whether they are reemployed in the following month. Second, for non-employed individuals, we know the characteristics of their last job, and so we know whether they are displaced formal workers eligible for UI.

In contrast, the structure of PME surveys and the variables recorded in PME surveys do not allow us to estimate survival rates without a formal job and hazard rates of formal reemployment. First, we do not observe individuals at given durations without a formal job. For an individual who is non-employed, we know how long the individual has been without a job, and thus without a formal job if her last job was a formal job. However, for an individual who is informally employed, we don't know how long the individual has been without a formal job, because we don't know anything about her previous jobs. Using individuals who are laid off from a formal job in their first interview, we could in theory construct their duration without a formal job up to three months since layoff even if they find an informal job thanks to the panel structure of PME surveys, but not beyond three months since layoff. We could of course see whether they are formally employed in their fifth interview, but there is a gap of eight months between the fourth and the fifth interview and we wouldn't know whether they have been working formally in the interval. So we cannot, for instance, measure the hazard rate of formal reemployment five months after layoff, because we never know who has been displaced from a formal job since four months among the informally employed individuals (we only know this among the non-employed).

Finally, we can only compute the potential UI duration accurately for non-employed displaced formal workers who had more than two years of tenure in their last job. This is because PME surveys do not record any information on UI takeup or on other prior formal jobs that displaced formal workers may have had in the 36 months prior to their layoff (we also don't know whether it has been at least 16 months between their last layoff and the previous layoff for which they applied for UI). For displaced formal workers with more than two years of tenure at layoff, job tenure at layoff is enough information to know that they are eligible for five months of UI.

D   Appendix for Section III

We provide here some additional details for Section III of the paper.

D.1   Distribution of tenure at layoff around the other eligibility cutoffs

Figure D.1 shows the distribution of observations by tenure at layoff for a 10%-random sample of workers selected in the same way as for the sample used in Section III (883,136 layoffs), with the exception that the sample includes workers who had between 0 and 36 months of tenure at layoff (we restrict attention to workers who had between 16 and 30 months of tenure in Section III). It shows that the distribution is smooth around the 24-month eligibility cutoff, which is the cutoff that we exploit in Section III. However, it also shows that the distribution is not smooth around the two other eligibility cutoffs.

The layoff density is not smooth – it increases discontinuously – around the 6-month cutoff, i.e., when workers become eligible for UI. The decision to lay off workers recently hired at a firm may be more responsive to UI eligibility. In fact, Gerard et al. (2020) show that the layoff density also increases discontinuously at another eligibility cutoff for such workers, when they reach 16 months between their layoff date and the layoff date of their last successful application to UI.

The layoff density is also not smooth – it decreases discontinuously – around the 12-month cutoff, which is when workers become eligible for a fourth month of UI, but also (and more importantly given that the density decreases) when firing costs increase. Termination of an employment contract for workers with more than 12 months of tenure must be overseen by a union or a Labor Ministry representative. This increases firing costs because of the administrative burden it imposes and because of firms' often imperfect compliance with workers' dues.[4]

In contrast, the layoff density appears smooth around the 24-month cutoff, which is when workers become eligible for a fifth month of UI. There is no other policy varying discontinuously at this cutoff. Moreover, the decision to lay off workers with relatively high tenure at a firm (or to report them as laid off) may not be very responsive to variation in their UI benefits upon layoff, even in the absence of experience-rating of UI benefits. They may have accumulated job-specific human capital and firing costs are relatively large for these workers.

D.2   Additional validity checks for the RD design

Figure D.2 presents validity checks for the RD design, in a similar way as Figure 3 in the paper, but for additional variables. In particular, it shows that there is no visible change in the composition

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[4]The layoff density also decreases discontinuously at 3 months of tenure, which is when firing costs increase as well because the first three months of tenure constitute a probationary period in Brazil.

Figure D.1: Share of observations by tenure bin at layoff
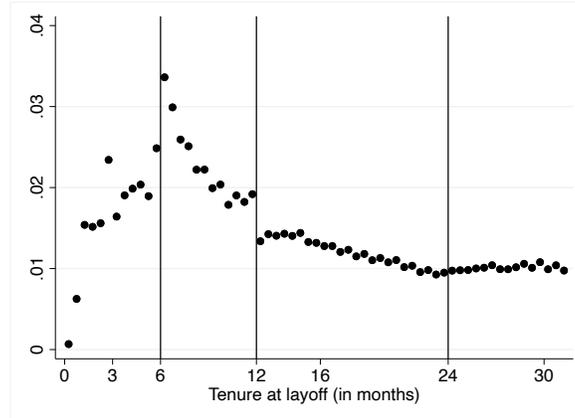
Notes: The figure displays the distribution of tenure at layoff for a sample similar to the one used in Section III, with the exception that it does not impose any restriction based on the tenure level at layoff. Tenure levels are aggregated by .5-month. The three vertical lines indicate tenure levels at which displaced formal workers become eligible for three, four, and five months of UI.

of our RD sample in terms of real monthly wage at layoff, sector of activity of the establishment that the worker was laid off by (construction is the omitted category), and size of the establishment (medium-sized firms is the omitted category) that the worker was laid off by.

D.3 Generalization of key patterns behind our main RD results

Figure D.3 shows that the key patterns behind our main RD results (see Section III.C in the paper) generalize beyond the RD sample by displaying hazard rates of formal reemployment and survival rates without a formal job for different samples of displaced formal workers.

In panels (a) and (b), we use a 10%-random sample of layoffs taking place between 2005 and 2009 and involving workers with more than 24 months of tenure at layoff (845,591 layoffs). These workers were eligible for 5 months of UI at layoff. We display patterns for all displaced workers, for UI takers, and for UI takers taking UI in their first month of eligibility, separately. Panels (a) and (b) show that rates of formal reemployment are comparable between that sample and the RD sample. Panel (a) also shows that the increase in hazard rates after month 5 for workers eligible for five months of UI is even sharper if we focus on workers taking up UI in month 1 after layoff, who actually exhaust their UI benefits in month 5.

In panels (c) and (d), we use a 5%-random sample of layoffs taking place between 2005 and 2009 (1,533,991 layoffs). We display patterns for all displaced workers and for UI takers, separately. These workers were eligible for 0, 3, 4, or 5 months of UI at layoff. Panels (c) and (d) show that rates of formal reemployment are comparable between that sample and the RD sample.

Panels (a)-(d) thus show that the finding that workers return slowly to a formal job even after

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Figure D.2: Additional evidence supporting the validity of the RD design

(a) Real monthly wage (in logs)

(b) Laid off from service sector

(c) Laid off from commercial sector

(d) Laid off from industrial sector

(e) Laid off from small establishment (less than 10 employees)

(f) Laid off from large establishment (more than 100 employees
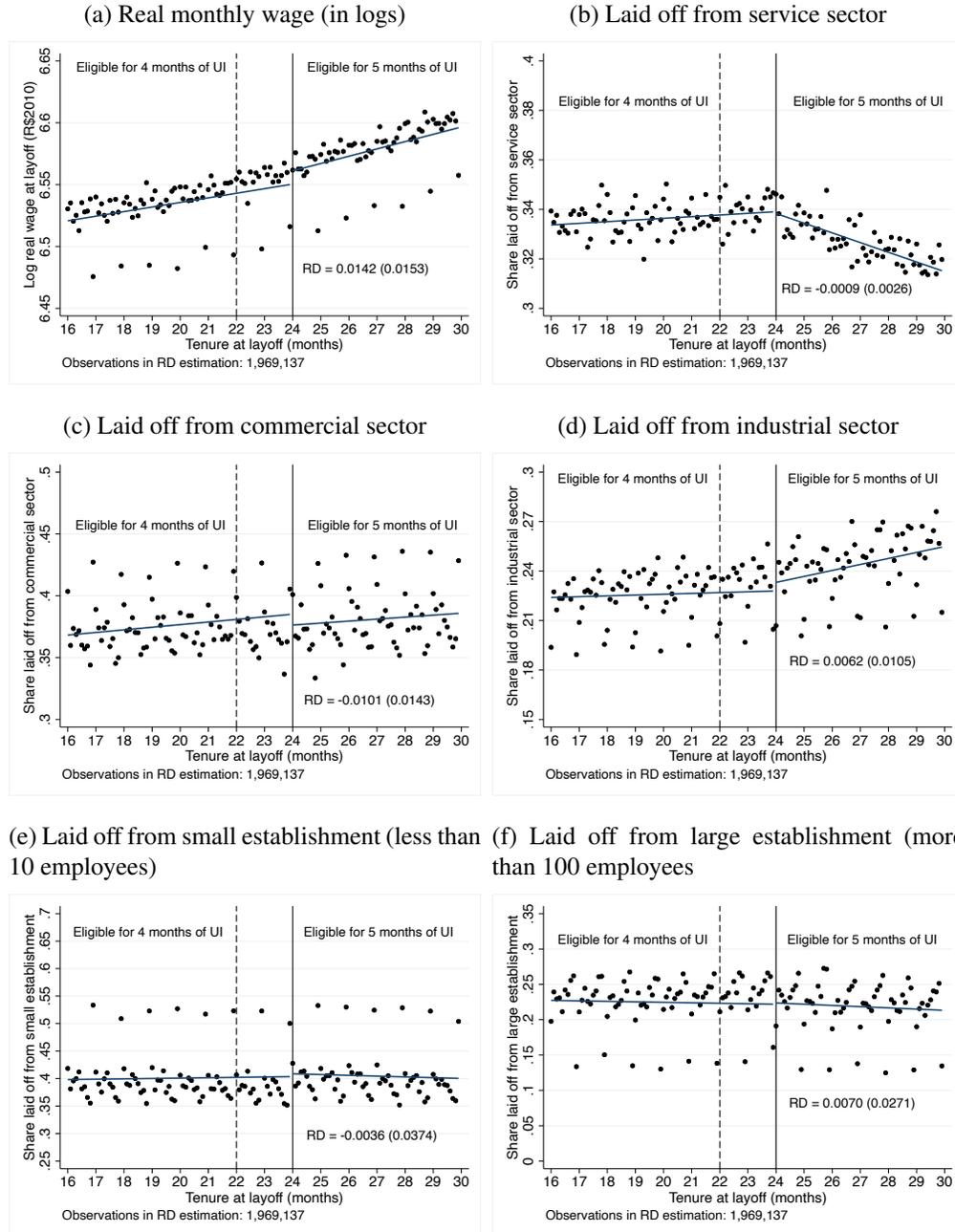
Notes: The figure provides additional evidence supporting the validity of the RD design. Panels (a)-(f) display averages of a series of workers' characteristics by tenure level (.1 month bins). The line below the cutoff (resp. above the cutoff) is estimated using an edge kernel and observations in a bandwidth of six months below 22 months of tenure (resp. above 24 months of tenure). The estimated β, the difference between the two lines at the cutoff, is displayed in each panel with its standard error (in parenthesis). Panels (a)-(f) show that there is no change in the composition of the sample around the cutoff in terms of real monthly wage at layoff, sector of activity of the establishment that the worker was laid off by, and size of the establishment that the worker was laid off by.

UI exhaustion is not specific to our RD sample. Panels (e) and (f) show that it is also unlikely due to some long-term effect of UI eligibility in earlier months. We use a 25%-random sample of separations taking place between 2005 and 2009 and involving workers with more than 24 months of tenure at separation but who were fired for cause (27,814 separations). These workers were not eligible for UI. Hazard rates of formal reemployment decrease monotonically in the months after layoff for these workers, which is consistent with the fact that they were not eligible for UI. Importantly for our purpose, rates of formal reemployment are similarly low in that sample. For instance, the survival rate without a formal job 12 months after separation is even higher for workers with more than 24 months of tenure at separation if they were fired for cause (see panel f) than if they were laid off (see panel b, considering all workers for that comparison).

D.4   Key patterns for workers returning or not to their previous employer

Figure D.4 shows that the increase and peak in formal reemployment after UI benefit exhaustion that we document in Section III in the paper is not driven by workers returning to their previous employer. We use the same 10%-random sample of layoffs taking place between 2005 and 2009 and involving workers with more than 24 months of tenure at layoff used in Figures D.3a and D.3b above. These workers were eligible for 5 months of UI at layoff. Figure D.4 displays the shares finding a new formal job in each month divided between workers returning to their previous employer and those hired by a new employer. Panel (a) considers all displaced workers and panel (b) considers UI takers. The share finding a new formal job increases and peaks after UI exhaustion both for workers returning to their previous employer and for those hired by a new employer. The pattern is not particularly more pronounced for workers who return to the same employer.

D.5   RD estimates for hazard and survival rates in each month since layoff

Figure D.5 displays RD estimates for the one-month increase in potential UI duration at the 24-month tenure cutoff for the survival rate without a formal job and for the hazard rate of formal reemployment in each month since layoff. We use the same sample of UI takers and the same specification as in Figure 5 in the paper. For a given level of the duration without a formal job, the regression outcomes are an indicator for whether a worker's duration without a formal job was longer than this level and an indicator for whether a worker's duration without a formal job was equal to this level (the sample in this case is restricted to workers whose duration without a formal job was longer or equal to this level) for the results presented in panels (a) and (b), respectively.

The largest difference in the survival rates are in months 5 and 6 after layoff as expected given the patterns in Figure 4 in the paper. There is no difference in survival rates anymore three years after layoff. This is why the impact on the average duration without a formal job censored at

Figure D.3: Generalization of key patterns behind main RD results

(a) Hazard rate of formal reemployment (workers with more than 24 months of tenure)

(b) Survival rate without a formal job (workers with more than 24 months of tenure)
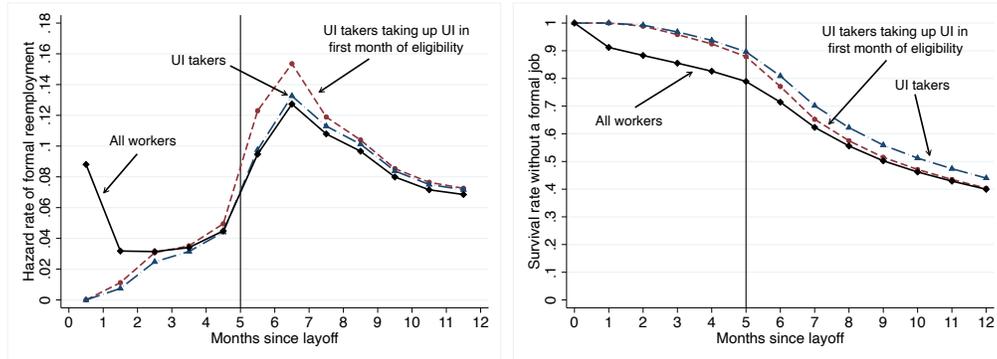
(c) Hazard rate of formal reemployment (random sample of workers)

(d) Survival rate without a formal job (random sample of workers)

(e) Hazard rate of formal reemployment (workers with more than 24 months of tenure but fired for cause, and thus not eligible for UI)

(f) Survival rate without a formal job (workers with more than 24 months of tenure but fired for cause, and thus not eligible for UI)
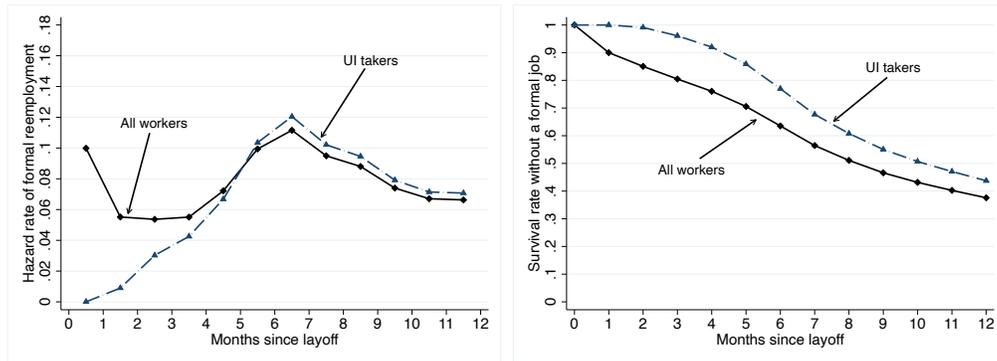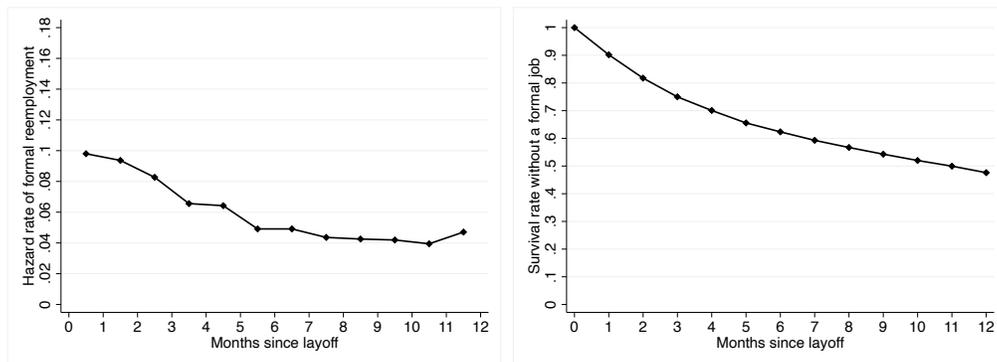

Notes: The figure shows that the key patterns behind our main RD results generalize beyond the RD sample by displaying hazard rates of formal reemployment and survival rates without a formal job for different samples. In panels (a) and (b), we use a 10%-random sample of layoffs taking place between 2005 and 2009 and involving workers with more than 24 months of tenure at layoff (845,591 layoffs). These workers were eligible for 5 months of UI at layoff. We display patterns for all displaced workers, for UI takers, and for UI takers taking UI in their first month of eligibility, separately. In panels (c) and (d), we use a 5%-random sample of layoffs taking place between 2005 and 2009 (1,533,991 layoffs). We display patterns for all displaced workers and for UI takers, separately. These workers were eligible for 0, 3, 4, or 5 months of UI at layoff. In panels (e) and (f), we use a 25%-random sample of separations taking place between 2005 and 2009 and involving workers with more than 24 months of tenure at separation but who were fired for cause (27,814 separations). These workers were not eligible for UI.

Figure D.4: Share finding a new formal job with the same vs. a different employer

(a) All workers

(b) UI takers
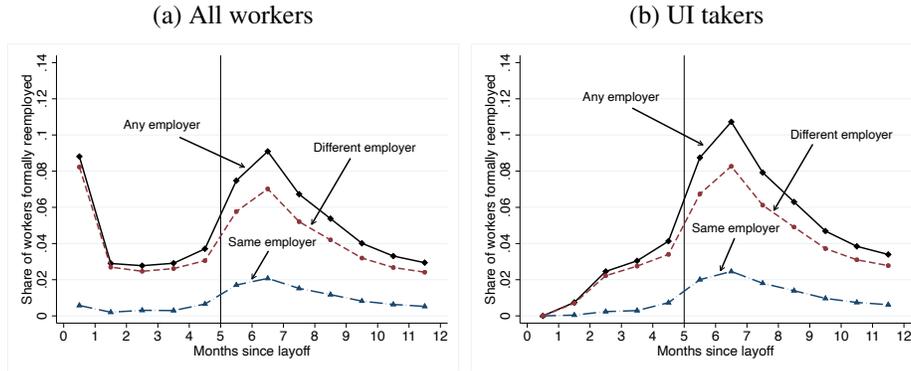
Notes: The figure shows that the increase and peak in formal reemployment after UI benefit exhaustion that we document in Section III in the paper is not driven by workers returning to their previous employer. We use the same 10%-random sample of layoffs taking place between 2005 and 2009 and involving workers with more than 24 months of tenure at layoff used in Figures D.3a and D.3b. These workers were eligible for 5 months of UI at layoff. The figure displays the shares finding a new formal job in each month divided between workers returning to the same employer and those hired by a new employer. Panel (a) considers all displaced workers and panel (b) considers UI takers.

three years after layoff, which we display in Figure 5d, captures the behavioral effect in full. The differences in the hazard rates are consistent with the differences in the survival rates.

Figure D.5: RD estimates on hazard and survival rates in each month

(a) Survival rate without a formal job

(b) Hazard rate of formal reemployment
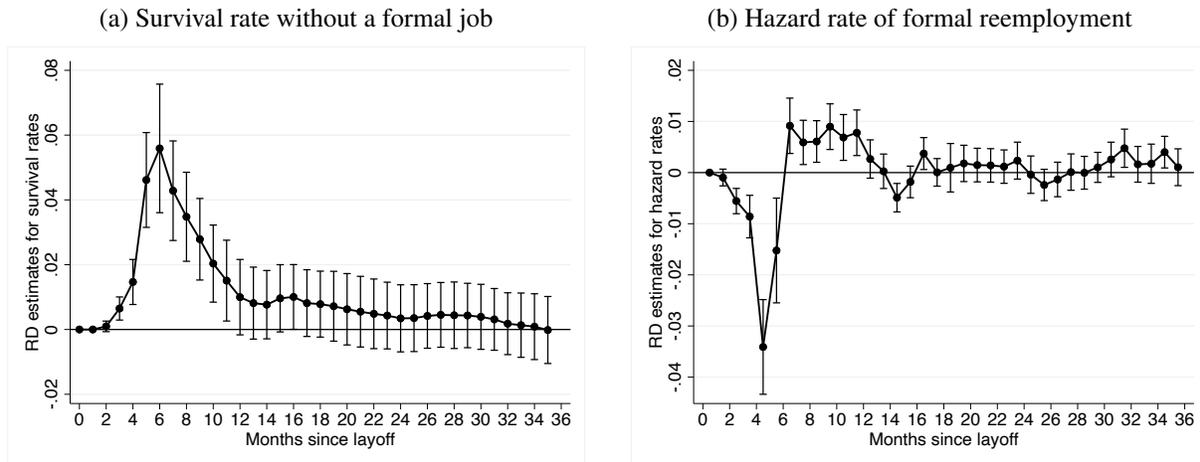
Notes: The figure displays RD estimates (and 95% confidence intervals) for the survival rate without a formal job (panel a) and for the hazard rate of formal reemployment (panel b) in each month since layoff. We use the same sample of UI takers and the same specification as in Figure 5 in the paper. For a given level of the duration without a formal job, the regression outcomes are an indicator for whether a worker's duration without a formal job was longer than this level and an indicator for whether a worker's duration without a formal job was equal to this level (the sample in this case is restricted to workers whose duration without a formal job was longer or equal to this duration level) for the results presented in panels (a) and (b), respectively.

D.6 Imputation of UI payment dates when using layoffs since 2002

In the robustness checks in the paper (see Table 1 and Figure 7), we use layoff since 2002 to maximize sample size. However, when using data prior to 2005, we do not know the precise date for the emission and withdrawal of UI payments, only the year and calendar month. To address this issue, we impute a day of emission (resp. withdrawal) within the month of emission (resp. withdrawal). We first estimate a simple model using data starting in 2005, in which we regress the day of emission (resp. withdrawal) within a month on a fully saturated set of fixed effects for calendar month of emission (resp. withdrawal) and separation day within a month. We estimate such models separately for the first UI payment, the second UI payment, etc. We then use the estimated model to predict days of emission (resp. withdrawal) for all UI payments, including those prior to 2005. Figure D.6 shows that we obtain similar patterns of UI benefit collection when we use the same sample as in Figures D.3a and D.3b (after 2005) with actual vs. imputed payment dates.

Figure D.6: UI benefit collection using actual vs. imputed payment dates.

(a) Using actual payment dates   (b) Using imputed payment dates
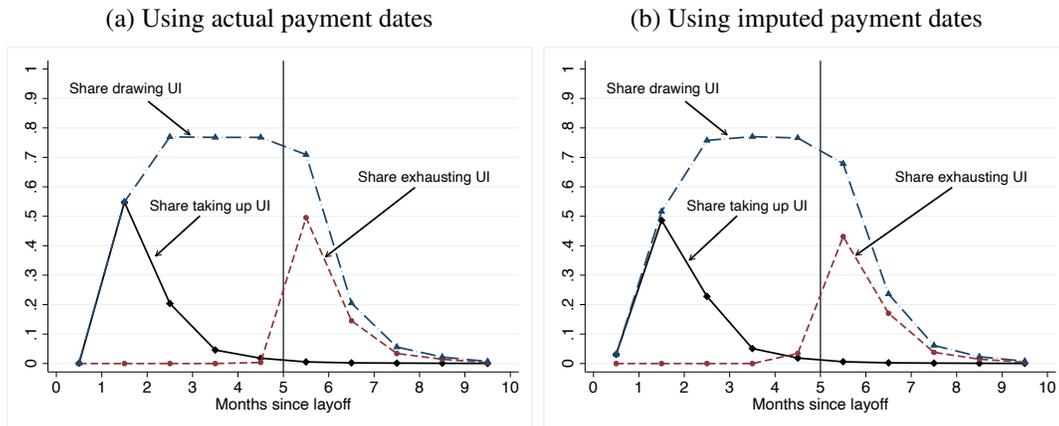
Notes: The figure shows that we obtain similar patterns of UI benefit collection when we use the same sample as in Figures D.3a and D.3b (after 2005) with actual vs. imputed payment dates. It displays the share taking up UI, drawing UI, and exhausting UI in each month since layoff using the actual payment dates in panel (a) and the imputed payment dates in panel (b).

D.7 RD estimates varying the bandwidth size

Table D.1 presents RD estimates varying the bandwidth size. Panel A first reproduces the estimates in Table 1B in the paper, using data since 2002 and a six-month bandwidth. Panels B-D then present estimates using data since 2002 but considering a five-month bandwidth, a four-month bandwidth, and a three-month bandwidth, respectively. As mentioned in the paper, the point estimates for the behavioral effects are decreasing with smaller bandwidths. The associated estimate

for the efficiency cost decreases from $.2 per $1 reaching mechanical beneficiaries in panel A to $.187 per $1, $.167 per $1, and $.146 per $1 in panels B, C, and D, respectively. Thus, by focusing on the six-month bandwidth in the paper, we may be over-estimating the efficiency cost.

D.8   Alternative empirical strategy: 2008 UI benefit extension

We confirm here the results from the RD analysis in the paper by using the other source of quasi-experimental variation in potential UI duration in Brazil, namely temporary extensions of UI benefits. The UI managing board, which is composed of representatives of employers, unions, and the government, is allowed to extend UI benefits temporarily by up to two months for selected groups of workers in Brazil, as long as the extension does not affect the financial stability of the UI fund. A few UI extensions took place in the last 20 years. We focus here on the last extension covered by our data for two reasons. First, it took place after 2005 and our data are more complete after 2005. Second, other recent extensions were much more targeted, e.g., to a very small number of sectors in decline (e.g. the shoe industry) or to municipalities hit by natural disasters.

D.8.1   Institutional details

The economic downturn in 2008-2009 was not as severe in Brazil as in other countries. Yet, by the end of 2008, there were concerns that the international crisis was reaching Brazil because of an increase in the number of layoffs. In November 2008, the Labor Minister announced that he would propose a UI extension to the UI managing board for workers "eventually" hit by the international crisis (Folha de São Paulo, November 19th, 2008). In early January 2009, he mentioned again the possibility of such an extension "but with no rush, probably in March" (Folha de São Paulo, January 7th, 2009). In February 2009, the managing board issued a resolution specifying that sectors and regions with one-month, two-month, three-month, and twelve-month averages in net formal employment generation lower than in previous years could become eligible for a UI extension. Finally, a resolution passed on March 30th 2009, which extended UI benefits by two months for formal employees laid off in December 2008 from a list of 42 sector-state pairs. We verified that all eligible sector-state pairs did in fact satisfy the pre-specified criteria using monthly formal employment generation data from the Labor Ministry (CAGED) between 2004 and 2009.

However, not all sector-state pairs satisfying the criteria were included in the list. In fact, a leading newspaper argued at the time against "measures benefiting the most organized sectors with the most powerful union lobbies." As we show below, industrial sectors were much more likely to become eligible conditional on satisfying the selection criteria. The same article also denounced that "restricting the universe of beneficiaries to those laid off in December 2008 sounds, to say the least, arbitrary" (Folha de São Paulo, March 25th, 2009).

Table D.1: RD estimates varying the bandwidth size

	A. Using a 6-month bandwidth		
	Actual paid UI duration (months) [1]	Counterfactual paid UI duration (months) [2]	Duration without a formal job censored at 3 years (months) [3]
Tenure ≥ 24 months	0.8519 (0.0080)	0.1150 (0.0062)	0.3720 (0.1453)
# Observations	2,667,223	2,667,223	1,880,941

	B. Using a 5-month bandwidth		
	Actual paid UI duration (months) [1]	Counterfactual paid UI duration (months) [2]	Duration without a formal job censored at 3 years (months) [3]
Tenure ≥ 24 months	0.8466 (0.0094)	0.1118 (0.0070)	0.2999 (0.1633)
# Observations	2,189,248	2,189,248	1,543,584

	C. Using a 4-month bandwidth		
	Actual paid UI duration (months) [1]	Counterfactual paid UI duration (months) [2]	Duration without a formal job censored at 3 years (months) [3]
Tenure ≥ 24 months	0.8382 (0.0114)	0.1060 (0.0078)	0.1856 (0.1839)
# Observations	1,725,025	1,725,025	1,216,133

	D. Using a 3-month bandwidth		
	Actual paid UI duration (months) [1]	Counterfactual paid UI duration (months) [2]	Duration without a formal job censored at 3 years (months) [3]
Tenure ≥ 24 months	0.8249 (0.0148)	0.0991 (0.0088)	0.0820 (0.2258)
# Observations	1,278,328	1,278,328	900,263

Notes: The table presents robustness checks for the RD analysis. Panel A first reproduces the estimates in Table 1B in the paper, using data since 2002 and a six-month bandwidth. Panels B-D then present estimates using data since 2002 but considering a five-month bandwidth, a four-month bandwidth, and a three-month bandwidth, respectively.

We estimate the impacts of the policy through a difference-in-differences. The relative arbitrariness in treatment assignment is possibly exogenous to our outcomes of interest. We evaluate endogeneity concerns by showing how our estimates vary with our choice of control groups. There is also an external validity concern. Our results may not be representative of the impacts of a similar policy in "normal" times because we study sector-state pairs that were experiencing relatively low formal employment generation. We address this concern by showing that the levels of relevant outcomes in our control groups are similar to average levels prevailing at other times in Brazil.

D.8.2   Data construction

For our sample of analysis, we begin by selecting layoffs that took place in November and December 2008 and for which the displaced formal worker had more than 24 months of tenure at layoff. We focus on workers with more than 24 months of tenure at layoff because they were eligible for five months of regular UI benefits. Therefore, workers laid off in December 2008 with more than 24 months of tenure learned that they were eligible for the UI extension at least one month prior to exhausting their regular UI benefits. This is not the case for workers with shorter potential UI duration. Our treatment group consists of workers laid off in December 2008 from sector-state pairs eligible for the UI extension. Note that the sectoral definition used for treatment assignment is the same as the first sector variable in RAIS. Our preferred control groups consist of similar workers laid off in November 2008 from the same sector-state pairs, and those laid off in November and December 2008 from other sector-state pairs that experienced one-month, two-month, and three-month averages in net formal employment generation lower than in previous years. The resulting sample includes 140,798 layoffs; 22,899 of them form the treatment group. We only add the 12-month average criterion as a sample restriction for our controls groups as a robustness check because it reduces our sample by two thirds. We also chose November 2008 as control month rather than January 2009 for two reasons. First, it allows us to observe all workers for two years after layoff since our data end in 2010. Second, some workers laid off in January 2009, as well as workers laid off in December 2008 from an extended list of sector-state pairs, became eligible for the UI extension through another resolution on May 27th. We exclude sector-state pairs from this extended list when constructing our control groups. We do not study the impacts of the May resolution because even eligible workers with more than 24 months of tenure at layoff may not have learned about their eligibility at least one month prior to exhausting their regular UI benefits.

D.8.3   Graphical evidence

Figure D.7 presents graphical evidence for the impacts of the UI extension. Panel (a) displays the share of UI takers drawing one to seven months of UI in our treatment group and in our preferred

control groups. The share drawing one to five months of UI is undistinguishable across groups, which supports a common-trend assumption. The share exhausting their fifth month of UI (regular potential UI duration) is also comparable as in Section III in the paper (around 86%). In the treatment group, 73% and 66.9% of UI takers drew a sixth and a seventh month of UI, respectively, while almost none did so in control groups. The treatment was thus assigned according to the official rules. Yet, eligible workers may have been only partially aware of the policy. Panel (a) shows the ratio between the share of UI takers who drew a UI payment and the share who were issued that UI payment. This ratio is very close to one for the regular UI benefits in the treatment group, but it is only equal to .92 for the additional UI benefits. We thus have some non-compliance: some workers were issued additional UI payments because they remained without a formal job after exhausting their regular UI benefits, but never collected them.

Panel (b) displays the survival rate without a formal job and the hazard rate of formal reemployment in each month since layoff for UI takers in the same treatment and control groups. Patterns are similar for the control groups and for the workers eligible for five months of UI in Figure 4 and Figure 8 in the paper. There is thus no evidence that workers in our sample were having a harder time at finding formal employment than comparable workers at other times in Brazil. In contrast, the increase and peak in formal reemployment rates after regular benefit exhaustion is shifted by exactly two months in the treatment group. These behavioral responses imply an increase of ten percentage points in the share of UI takers who remained without a formal job seven months after layoff. Nevertheless, this share is high even in the control groups, at about 70%. Therefore, most workers would have drawn the additional benefits without changing their behavior and the behavioral effect on the paid UI duration will be relatively small compared to the mechanical effect.

D.8.4 Empirical strategy

We estimate the impacts of the UI extension using the following difference-in-differences specification for worker i laid off in month m of year t from sector r in state s:

$$
\begin{aligned}
y_{i,m,t,r,s} =\; & \alpha + \beta \times December2008_{m,t} + \gamma \times EligibleSectorState_{r,s} \\
& + \delta \times (December2008_{m,t} \times EligibleSectorState_{r,s}) + \varepsilon_{i,m,t,r,s},
\end{aligned}
\tag{1}
$$

where $December2008_{m,t}$ and $EligibleSectorState_{r,s}$ identify a worker laid off in December 2008 and a worker from an eligible sector-state pair, respectively. The coefficient δ is a difference-in-differences estimator for the impact of the UI extension on outcome y under a common-trend assumption. ε is an error term clustered at the sector-state level. We present results for this main specification in Table D.2. We also present results for alternative specifications in Tables D.3 and D.4. In particular, we consider specifications (a) in which we include state fixed effects, sector

Figure D.7: Graphical evidence for the impact of the temporary UI extension

(a) UI benefit collection   (b) Formal reemployment
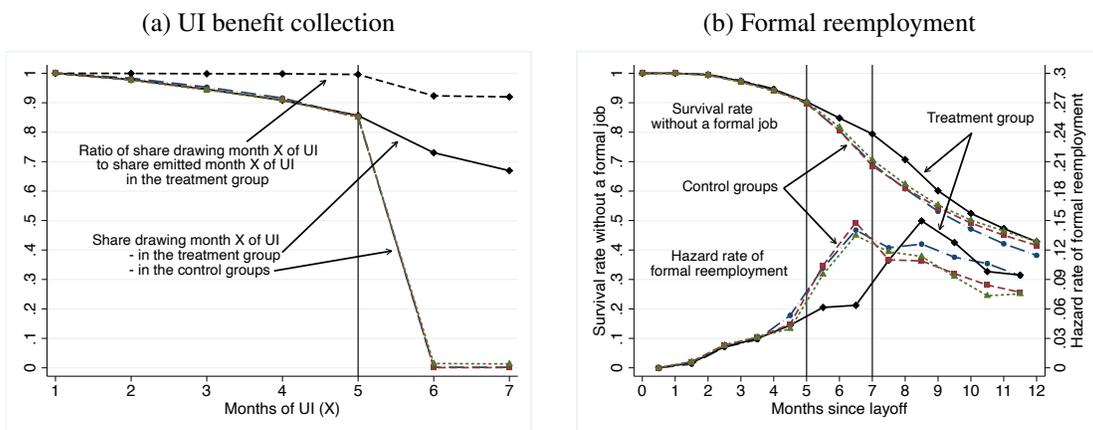
Notes: The figure displays graphical evidence for the impacts of the temporary UI extension. Workers in the treatment group were laid off in December 2008 with more than 24 months of tenure from sector-state pairs eligible for the UI benefit extension. They were eligible for seven months of UI. The control groups consists of workers with more than 24 months of tenure (i) laid off in November 2008 from the same sector-state pairs and (ii) laid off in November and December 2008 from other sector-state pairs that experienced lower one-month, two-month, and three-month averages in net formal employment generation than in previous years. Workers in the control groups were all eligible for five months of UI. Panel (a) displays the share of UI takers drawing their first month of UI, second month of UI, etc. for the treatment group and the control groups. It also displays the ratio between the share of UI takers who drew their first month of UI, second month of UI, etc. and the share who were issued their first month of UI, second month of UI, etc. in the treatment group. Panel (b) displays the survival rate without a formal job and the hazard rate of formal reemployment in each month since layoff for UI takers in the treatment group and in the control groups.

fixed effects, and a rich set of individual controls;[5] (b) in which we remove criteria on net formal employment generation when selecting our control groups; (c) in which we add the criterion on the 12-month average net formal employment generation when selecting our control groups; (d) in which we include workers laid off in October 2008 in our control groups; (e) in which we include workers laid off in November and December 2007 from the same sector-state pairs as in our main specification and adopt a triple-difference specification; and (f) in which we limit the sample to workers with replacement rates between 20% and 80%.

We consider similar outcome variables as for the RD design, including the counterfactual benefit duration $\widetilde{D_i^B}$. With perfect treatment assignment and compliance, $\widetilde{D_i^B}$ would equal paid UI duration D_i^B in the treatment group. In the control groups, $\widetilde{D_i^B}$ would capture the sum of the regular benefit duration and of the mechanical effect, i.e., the additional UI payments that a worker would draw without changing her behavior if she was eligible for the UI extension. The impact of the extension on $\widetilde{D_i^B}$ would thus isolate the behavioral effect. The differential impact on D_i^B and $\widetilde{D_i^B}$ would isolate the mechanical effect. Figure D.7a shows that treatment assignment is not a problem, but that we have 8% of non-compliers. In that case, $\widetilde{D_i^B}$ would overestimate D_i^B in the treatment group. However, the impact on $\widetilde{D_i^B}$ and the difference in the impacts on D_i^B and $\widetilde{D_i^B}$ would still recover the average behavioral and mechanical effects of the policy. Such impacts would underestimate behavioral and mechanical effects for compliers, but not the ratio between the behavioral and the mechanical effects, which is our measure of efficiency.[6]

D.8.5 Main results

Table D.2 displays the regression results. Panel A first provides evidence supporting a common-trend assumption. In particular, we test for a difference-in-differences in sample size and in worker characteristics: age, gender, education, replacement rate, and UI takeup, which is a pre-determined characteristic here given the timing of the policy variation. We present results for additional worker characteristics and for alternative specifications in Table D.3, which also documents average characteristics in eligible and ineligible sector-state pairs. Workers in eligible sector-state pairs are much more likely to come from industrial sectors (93.4% vs. 13.1%), and thus from larger establishments. They are more likely to be male, to have higher tenure, and to have higher wages and thus lower replacement rates (.65 vs. .72). We find no difference-in-differences in worker characteristics for our main specification in Table D.2, but we find a significant increase in the number of observations. Importantly, the latter result is not robust. The estimated coefficient is smaller, insignificant, or even negative when we consider alternative specifications in Table

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[5]We include fixed effects for state (27), sector (24), gender (2), education (5), and establishment size (10), as well as 4th order polynomials in age, in tenure, and in the logarithm of the real monthly wage before layoff (demeaned).

[6]Rescaling behavioral and mechanical effects by the compliance rate (92%) leave their ratio unchanged.

D.3. Moreover, the sign and magnitude of our impacts of interest (see below) are unrelated to the sign and magnitude of this coefficient across specifications. In contrast, the absence of clear difference-in-differences in worker characteristics is a robust result. The only consistent difference-in-differences, which is not always significant, is in the share of workers from industrial sectors. Yet, we show below that our estimated impacts of interest are similar if we include sector fixed effects and other control variables. In sum, results in panel A support our empirical strategy.

Next, we restrict attention to UI takers and turn to our outcomes of interest in panel B, which are similar to the outcomes considered for the RD analysis in the paper. We estimate an overall effect on the average paid UI duration of 1.364 months. However, this increase is mostly due to a mechanical effect. Indeed, we estimate an increase in the average counterfactual paid UI duration, which captures the behavioral effect, of only .163 month. The mechanical effect is thus equal to $1.364 - .163 = 1.201$ months. We also estimate an increase in the duration without a formal job censored at two years after layoff of .484 month. This captures the behavioral effect on the duration without a formal job in full because we find no impact on the survival without a formal job two years after layoff. Finally, we find no effect on the probability to be formally employed two years after layoff or on the wage among those formally employed then.

Results are of comparable magnitudes for the robustness checks in Table D.4. We note that relaxing all restrictions on net formal employment generation for our choice of control sector-state pairs leads to larger estimates of behavioral responses. This suggests that the choice of control groups in our main specification addresses endogeneity concerns in treatment assignment. If anything, our preferred estimates may still be upper bounds: adding the criterion on the 12-month average net formal employment generation leads to smaller estimates of behavioral responses.

Results from our main specification imply an efficiency cost of $\frac{.163 + .082 \times .484}{1.364 - .163} = .168$ or 16.8 cents per \$1 reaching mechanical beneficiaries. In this calculation, we approximate the scaling coefficient $\frac{\tau}{b}$ in equation (1) in the paper by $\frac{\tau}{b} = .082$, which is the average ratio of the number of UI beneficiaries to the number of private formal employees in any given month in 2008. Moreover, the robustness checks in Table D.4 imply an efficiency cost ranging from 15.2 cents to 22.7 cents per \$1 reaching mechanical beneficiaries. Results from our second empirical strategy thus confirm results from the RD design in Section III in the paper.

Table D.2: Main results for the 2008 UI benefit extension

A. Validity checks	Share of observations by month of layoff and sector-state pair	Age (years)	Male (dummy)	Years of education	Statutory UI replacement rate	UI takeup
	[1]	[2]	[3]	[4]	[5]	[6]
EligibleSectorState x December2008	0.0020	0.0794	-0.0040	0.0036	-0.0019	-0.0097
	(0.0010)	(0.1643)	(0.0145)	(0.0900)	(0.0074)	(0.0109)
Constant	0.0155	32.45	0.6216	9.364	0.7278	0.8483
	(0.0058)	(0.3704)	(0.0231)	(0.1048)	(0.0099)	(0.0156)
# Observations	140,798	140,798	140,798	140,798	140,798	140,798

B. Impacts (UI takers only)	Actual paid UI duration (months)	Counterfactual paid UI duration (months)	Survival without a formal job 2 years after layoff	Duration without a formal job censored at 2 years (months)	December 2 years later Formally employed (dummy)	December 2 years later Real monthly wage if formally employed (R$, in logs)
	[1]	[2]	[3]	[4]	[5]	[6]
EligibleSectorState x December2008	1.364	0.1626	-0.0005	0.4840	0.0105	0.0069
	(0.0363)	(0.0343)	(0.0057)	(0.1392)	(0.0078)	(0.0186)
Constant	4.690	6.022	0.2388	12.42	0.5863	6.920
	(0.0217)	(0.0361)	(0.0050)	(0.1198)	(0.0056)	(0.0298)
# Observations	122,403	122,403	122,403	122,403	122,403	122,403

Notes: The table displays difference-in-differences estimates for the two-month increase in potential UI duration from the 2008 UI benefit extension, using a sample of displaced formal employees who had more than 24 months of tenure at layoff and who were typically eligible for five months of UI. Workers laid off in December 2008 from eligible sector-state pairs were instead eligible for seven months of UI (see text). Specifications include a dummy for workers laid off in December 2008 and a dummy for workers laid off from eligible sector-state pairs. Panel A presents supportive evidence for the common-trend assumption by testing for a difference-in-differences in relative sample size and in average worker characteristics (we use the same variables as for the validity checks in the RD analysis in Figure 3 in the paper). Panel B presents treatment effects on similar outcomes as for the RD analysis in Figure 5 in the paper.

Table D.3: Additional validity checks for the 2008 UI benefit extension

	Mean Control	Mean Treatment	DD [1]	DD [2]	DD [3]	DD [4]	DD [5]
Share of observations by month	0.0136	0.0061	0.0020	0.0010	0.0004	0.0126	-0.0026
of layoff and sector-state pair	[0.0129]	[0.0037]	(0.0010)	(0.0008)	(0.0004)	(0.0054)	(0.0012)
Male (dummy)	0.6067	0.6818	-0.0040	0.0106	0.0154	0.0029	0.0321
	[0.4885]	[0.4658]	(0.0145)	(0.0140)	(0.0226)	(0.0126)	(0.0138)
Age (years)	32.47	32.42	0.0794	-0.0860	0.3052	0.1850	-0.1137
	[8.609]	[8.450]	(0.1643)	(0.1547)	(0.1955)	(0.1568)	(0.2131)
Years of education	9.403	9.265	0.0036	0.0000	0.0143	-0.0063	0.1153
	[2.659]	[2.586]	(0.0900)	(0.0960)	(0.1083)	(0.0862)	(0.0797)
Tenure (months)	47.77	52.08	-0.0968	-0.4222	-1.360	0.6895	-1.249
	[23.40]	[25.60]	(0.8429)	(0.7328)	(0.9078)	(0.5610)	(0.8774)
Real monthly wage (R$, in logs)	6.825	7.044	0.0046	0.0096	0.0200	0.0368	0.0300
	[0.4594]	[0.5112]	(0.0223)	(0.0218)	(0.0272)	(0.0214)	(0.0208)
Statutory UI replacement rate	0.7247	0.6530	-0.0019	-0.0039	-0.0040	-0.0125	-0.0105
	[0.1512]	[0.1795]	(0.0074)	(0.0073)	(0.0089)	(0.0074)	(0.0067)
Laid off from service sector	0.3218	0.0121	0.0155	-0.0008	0.0002	0.0149	0.0158
(dummy)	[0.4672]	[0.1093]	(0.0105)	(0.0133)	(0.0025)	(0.0133)	(0.0141)
Laid off from commercial	0.5247	0.0540	0.0092	0.0298	0.0223	0.0121	0.0013
sector (dummy)	[0.4994]	[0.2261]	(0.0118)	(0.0121)	(0.0323)	(0.0147)	(0.0124)
Laid off from industrial sector	0.1312	0.9339	-0.0261	-0.0324	-0.0458	-0.0235	-0.0194
(dummy)	[0.3376]	[0.2485]	(0.0138)	(0.0141)	(0.0336)	(0.0149)	(0.0112)
Laid off from establishment	0.3768	0.1471	-0.0055	0.0093	-0.0141	-0.0100	-0.0045
with less than 10 employees	[0.4846]	[0.3542]	(0.0125)	(0.0127)	(0.0174)	(0.0101)	(0.0181)
Laid off from establishment	0.2633	0.5236	0.0102	0.0082	0.0172	0.0197	0.0074
with more than 100 employees	[0.4404]	[0.4995]	(0.0262)	(0.0262)	(0.0332)	(0.0211)	(0.0210)
# Observations			140,798	270,163	60,697	209,855	253,730
Lower 1/2/3-month averages			Yes	No	Yes	Yes	Yes
Lower 12-month average			No	No	Yes	No	No
Including layoffs in October 2008			No	No	No	Yes	No
Including layoffs in Nov-Dec 2007			No	No	No	No	Yes

Notes: The table presents additional supportive evidence for the common-trend assumption by testing for a difference-in-differences in relative sample size and in average worker characteristics for additional worker characteristics and alternative specifications. The means in the control and treatment groups, as well as the estimates in column (1) are based on the same samples and specifications as in Table D.2. For the specifications in the other columns, we remove criteria on net formal employment generation when selecting our control groups (column 2), we add the criterion on the 12-month average net formal employment generation when selecting our control groups (column 3), we include workers laid off in October 2008 in our control groups (column 4), and we include workers laid off in November and December 2007 from the same sector-state pairs as in our main specification and adopt a triple-difference strategy (column 5). In the latter case, the specification includes fixed effects for eligible sector-state pairs, for the month of December, for the year 2008, the two-way interactions between these dummies, and the three-way interaction (reported coefficient).

Table D.4: Robustness of main results for the 2008 UI benefit extension

		UI takeup	Actual paid UI duration (months)	Counterfactual paid UI duration (months)	Duration without a formal job censored at 2 years (months)
		[1]	[2]	[3]	[4]
A. Including controls	EligibleSectorState x December2008	-0.0065 (0.0088)	1.367 (0.0335)	0.1681 (0.0297)	0.4816 (0.1120)
	# Observations	140,798	122,403	122,403	122,403
B. No restriction on net employment changes	EligibleSectorState x December2008	0.0070 (0.0080)	1.389 (0.0354)	0.1882 (0.0339)	0.5669 (0.1339)
	# Observations	270,163	231,875	231,875	231,875
C. Additional restriction on net employment changes	EligibleSectorState x December2008	0.0070 (0.0099)	1.374 (0.0412)	0.1475 (0.0465)	0.4775 (0.2498)
	# Observations	60,697	54,237	54,237	54,237
D. Including October as control month	EligibleSectorState x December2008	-0.0028 (0.0063)	1.368 (0.0359)	0.1578 (0.0339)	0.3751 (0.1407)
	# Observations	209,855	181,190	181,190	181,190
E. Triple difference using data from 2007	EligibleSectorState x December2008	-0.0266 (0.0194)	1.401 (0.0394)	0.2208 (0.0447)	0.5755 (0.1688)
	# Observations	253,730	216,752	216,752	216,752
F. Replacement rate between 20% and 80%	EligibleSectorState x December2008	-0.0082 (0.0082)	1.350 (0.0441)	0.1544 (0.0431)	0.5106 (0.1749)
	# Observations	89,695	77,351	77,351	77,351

Notes: The table displays robustness checks for the main results in Table D.2, using alternative specifications. In panel A, we include fixed effects for state (27), sector (24), gender (2), education (5), and establishment size (10), as well as 4th order polynomials in age, in tenure, and in the logarithm of the real monthly wage before layoff (demeaned). In panel B, we remove criteria on net formal employment generation when selecting our control groups. In panel C, we add the criterion on the 12-month average net formal employment generation when selecting our control groups. In panel D, we include workers laid off in October 2008 in our control groups. In panel E, we include workers laid off in November and December 2007 from the same sector-state pairs as in our main specification and adopt a triple-difference strategy. In panel F, we limit the sample to workers with replacement rates between 20% and 80% (panel F). The specification in panel E includes fixed effects for eligible sector-state pairs, for the month of December, for the year 2008, the two-way interactions between these dummies, and the three-way interaction (reported coefficient).

E   Appendix for Section IV

We provide here some additional details for Section IV of the paper.

E.1   Generalization of key patterns behind our main results

Figure E.1 shows that the key patterns behind our heterogeneity results with informality rates generalize beyond the RD sample by presenting graphs similar to some of the graphs in Figure 6 in the paper for different samples. In all graphs, state-level estimates are plotted against average state-level informality rates. Lines display the result of a WLS regression of these estimates on informality rates weighting them by the inverse of their standard error squared (with 95% confidence intervals). The estimated slope is displayed with its standard error (in parenthesis).

In panels (a)-(d), we use the same 10%-random sample of layoffs taking place between 2005 and 2009 and involving workers with more than 24 months of tenure at layoff (845,591 layoffs) as in Figures D.3a and D.3b. These workers were eligible for 5 months of UI at layoff. Panel (a) shows that there is no correlation between UI takeup and informality rates. Panels (b)-(d) then restrict attention to UI takers. Panel (b) shows that the average paid UI duration is large in every state and is increasing in informality rates. Providing UI is thus more costly in labor markets with higher informality. Panel (c) shows that the mechanical effect of a hypothetical one-month increase in potential UI duration is also large in every state and is increasing in informality rates. Finally, panel (d) displays the average duration without a formal job censored at three years after layoff. Consistent with the pattern for the mechanical effect, it shows that displaced workers return slower to a formal job in labor markets with higher informality, even in absence of longer UI benefits.

In panel (e), we use the same 5%-random sample of layoffs taking place between 2005 and 2009 (1,533,991 layoffs) as in Figures D.3c and D.3d. Panel (e) displays the average duration without a formal job censored at three years after layoff for UI takers. It shows that displaced workers return slower to a formal job in labor markets with higher informality in this sample too.

Panel (f) shows that the fact that displaced workers return slower to a formal job in labor markets with higher informality, even in absence of longer UI benefits, is unlikely due to some long-term effect of UI eligibility in earlier months. We use the same 25%-random sample of separations taking place between 2005 and 2009 and involving workers with more than 24 months of tenure at separation but who were fired for cause (27,814 separations) as in Figures D.3e and D.3f. These workers were not eligible for UI, but panel (f) shows that they return slower to a formal job in labor markets with higher informality. The estimated slope is even steeper in that sample.

Figure E.1: Generalization of key patterns behind our heterogeneity results with informality rates

(a) UI takeup rate (workers with more than 24 months of tenure)

(b) Average paid UI duration (UI takers with more than 24 months of tenure)
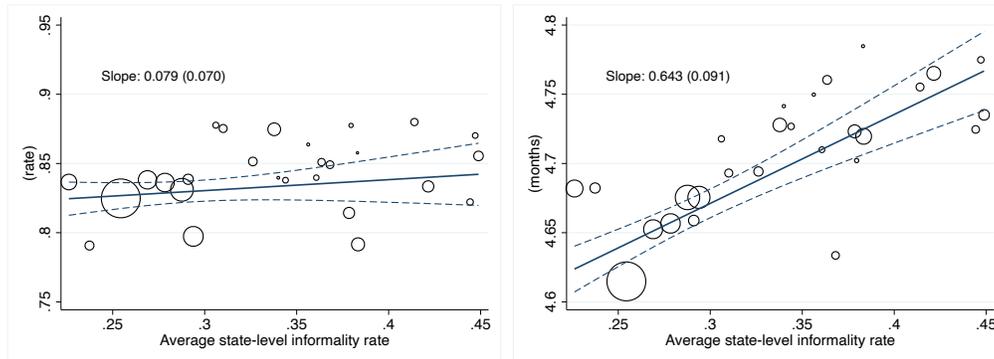
(c) Mechanical effect of hypothetical one-month increase in potential UI duration (UI takers with more than 24 months of tenure)

(d) Average duration without a formal job censored at 3 years (UI takers with more than 24 months of tenure)
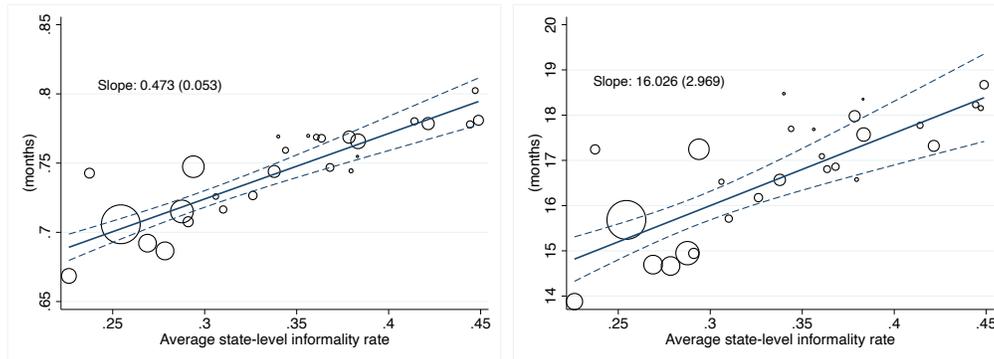
(e) Average duration without a formal job censored at 3 years (random sample of UI takers)

(f) Average duration without a formal job censored at 3 years (workers with more than 24 months of tenure but fired for cause)
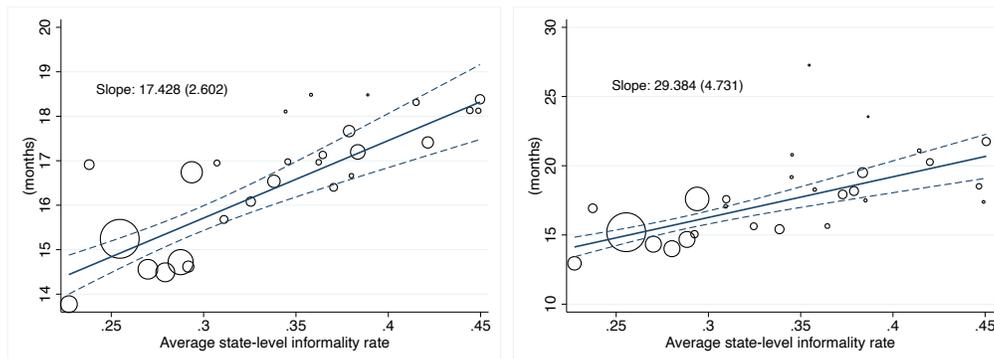
Notes: The figure shows that the key patterns behind our heterogeneity results with informality rates generalize beyond the RD sample by presenting graphs similar to graphs in Figure 6 in the paper for different samples. In all panels, state-level estimates are plotted against average state-level informality rates. Lines display the result of a WLS regression of these estimates on informality rates weighting them by the inverse of their standard error squared (with 95% confidence intervals). The estimated slope is displayed with its standard error (in parenthesis). In panels (a)-(d), we use the same sample as in Figures D.3a and D.3b. These workers were eligible for 5 months of UI at layoff. In panel (e), we use the same sample as in Figures D.3c and D.3d. These workers were eligible for 0, 3, 4, or 5 months of UI at layoff. In panel (f), we use the same sample as in Figures D.3e and D.3f. These workers were not eligible for UI at layoff.

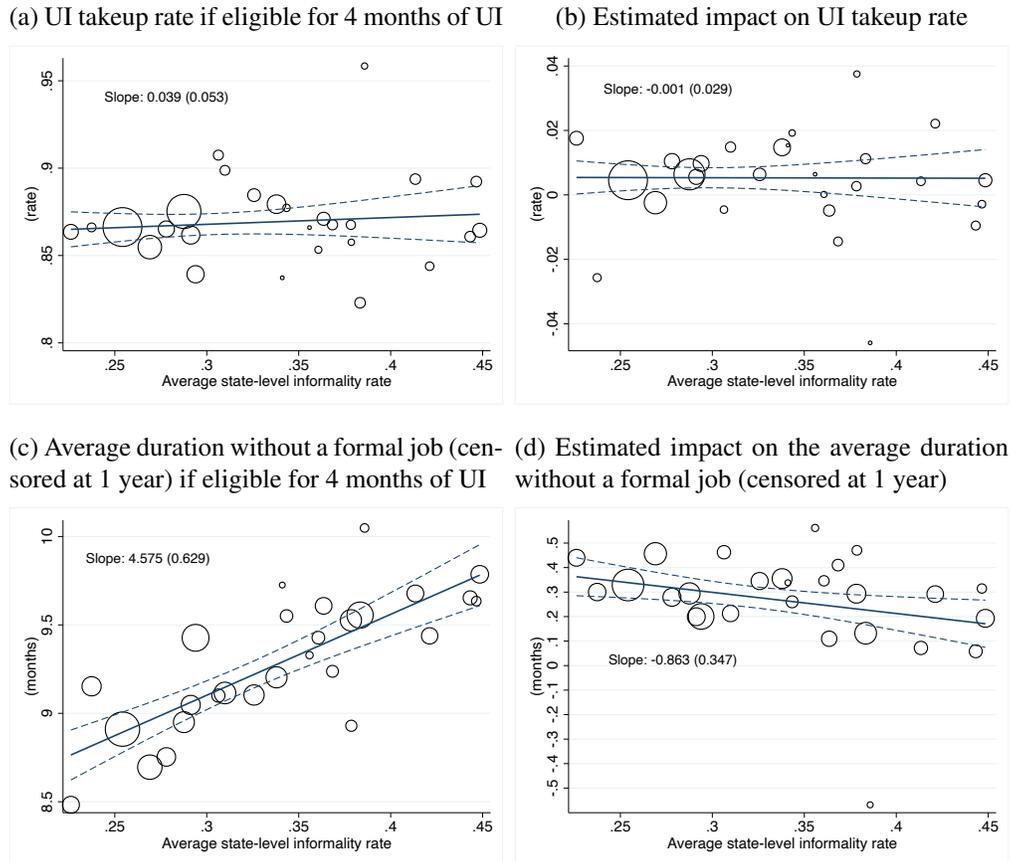## E.2   Graphs for additional outcome variables

Figure E.2 displays similar graphs as in Figure 6 in the paper for two additional outcome variables. Panels (a) and (b) consider the specification for UI takeup, displaying the estimated constant α and the estimated β, i.e., the estimated impact of the increase in potential UI duration. They show that there is no systematic correlation between UI takeup, or the impact on UI takeup, and informality rates. Panels (c) and (d) consider the specification for the duration without a formal job censored at 1 year after layoff, displaying the estimated constant α and the estimated β, i.e., the estimated impact of the increase in potential UI duration. As in Figure 6e in the paper, where we consider the duration without a formal job censored at three years after layoff, panel (c) shows that displaced workers return slower to a formal job in labor markets with higher informality, even in absence of longer UI benefits. As in Figure 6f in the paper, panel (d) shows that the behavioral effect on the average duration without a formal job censored at one year after layoff is decreasing in informality rates. The estimated slope is significant, in contrast to the results for the average duration without a formal job censored at three years after layoff displayed in Figure 6f in the paper.

Figure E.3 displays the average number of UI beneficiaries per formal employee in each state between 2005 and 2009, which we use to approximate the ratio $\frac{\tau}{b}$ in equation (1) in the paper. State-level values of that ratio are plotted against average state-level informality rates. Lines display the result of an OLS regression of these estimates on informality rates (with 95% confidence intervals). The estimated slope is displayed with its standard error (in parenthesis). Figure E.3 shows that the ratio is slightly increasing with informality rates (but not significantly so).

E.3   Additional graphs for robustness checks

Figures E.4, E.5, E.6, E.7, E.8 present results similar to those in Figure 6 in the paper but for each of the robustness checks considered in Table 1 and Figure 7 in the paper. In so doing, we can show that the slope is significant for the overall effect on the average paid UI duration if we include layoffs since 2002. Moreover, we can show that the slope is significant for the behavioral effect on the average paid UI duration (censored at three years after layoff) if we include layoffs since 2002.

Figure E.2: Heterogeneity of RD results with informality rates for additional variables

(a) UI takeup rate if eligible for 4 months of UI

(b) Estimated impact on UI takeup rate

(c) Average duration without a formal job (censored at 1 year) if eligible for 4 months of UI

(d) Estimated impact on the average duration without a formal job (censored at 1 year)

Notes: The figure displays results from estimating the same RD specifications as in Figure 5 in the paper for each of the 27 Brazilian states, separately. State-level estimates are plotted against average state-level informality rates. Lines display the result of a WLS regression of these estimates on informality rates weighting them by the inverse of their standard error squared (with 95% confidence intervals). The estimated slope is displayed with its standard error (in parenthesis). Panels (a) and (b) consider the specification for UI takeup, displaying the estimated constant α and the estimated β, i.e., the estimated impact on the UI takeup rate. Panels (c) and (d) consider the specification for the duration without a formal job censored at 1 year after layoff, displaying the estimated constant α and the estimated β, i.e., the estimated impact on the average duration without a formal job censored at 1 year after layoff.

Figure E.3: Average number of UI beneficiaries per formal employee between 2005 and 2009
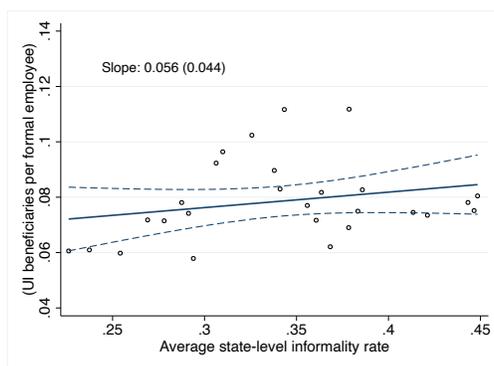
Notes: The figure displays the average number of UI beneficiaries per formal employee in each state between 2005 and 2009, which we use to approximate the ratio $\frac{\tau}{b}$ in equation (1) in the paper. State-level values of that ratio are plotted against average state-level informality rates. Lines display the result of an OLS regression of these estimates on informality rates (with 95% confidence intervals). The estimated slope is displayed with its standard error (in parenthesis).

Figure E.4: Heterogeneity of RD results with informality rates (including layoffs since 2002)

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

(b) Overall effect on average paid UI duration

(c) Behavioral effect on average paid UI duration

(d) Mechanical effect on average paid UI duration

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

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

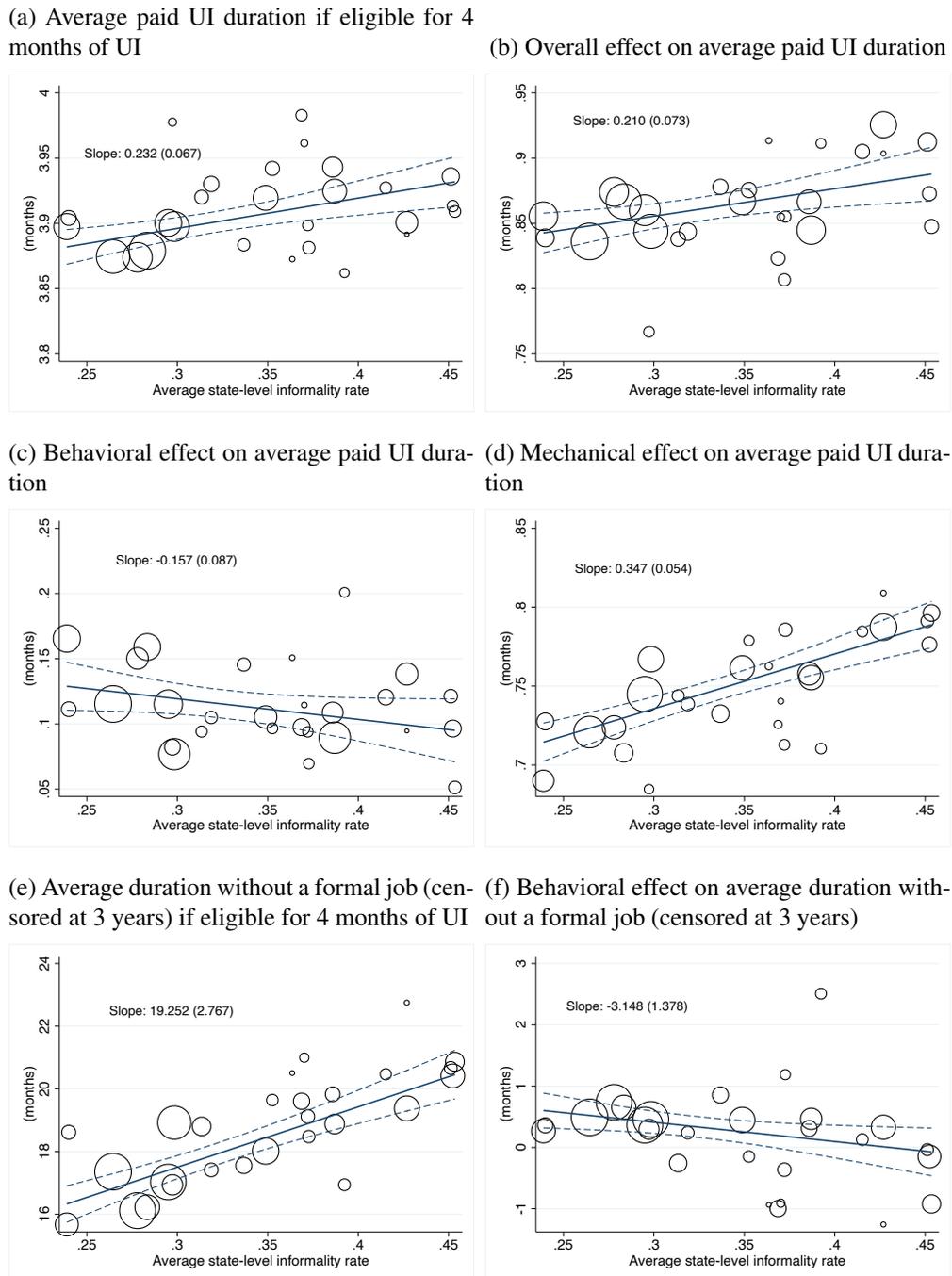*Notes:* The figure present results similar to those in Figure 6 in the paper but including layoffs since 2002, as in Figure 7b in the paper. It displays results from estimating the same RD specifications as in Table 1B for each of the 27 Brazilian states, separately. State-level estimates are plotted against average state-level informality rates. Lines display the result of a WLS regression of these estimates on informality rates weighting them by the inverse of their standard error squared (with 95% confidence intervals). The estimated slope is displayed with its standard error (in parenthesis).

Figure E.5: Heterogeneity of RD results with informality rates (using a 4-month bandwidth)
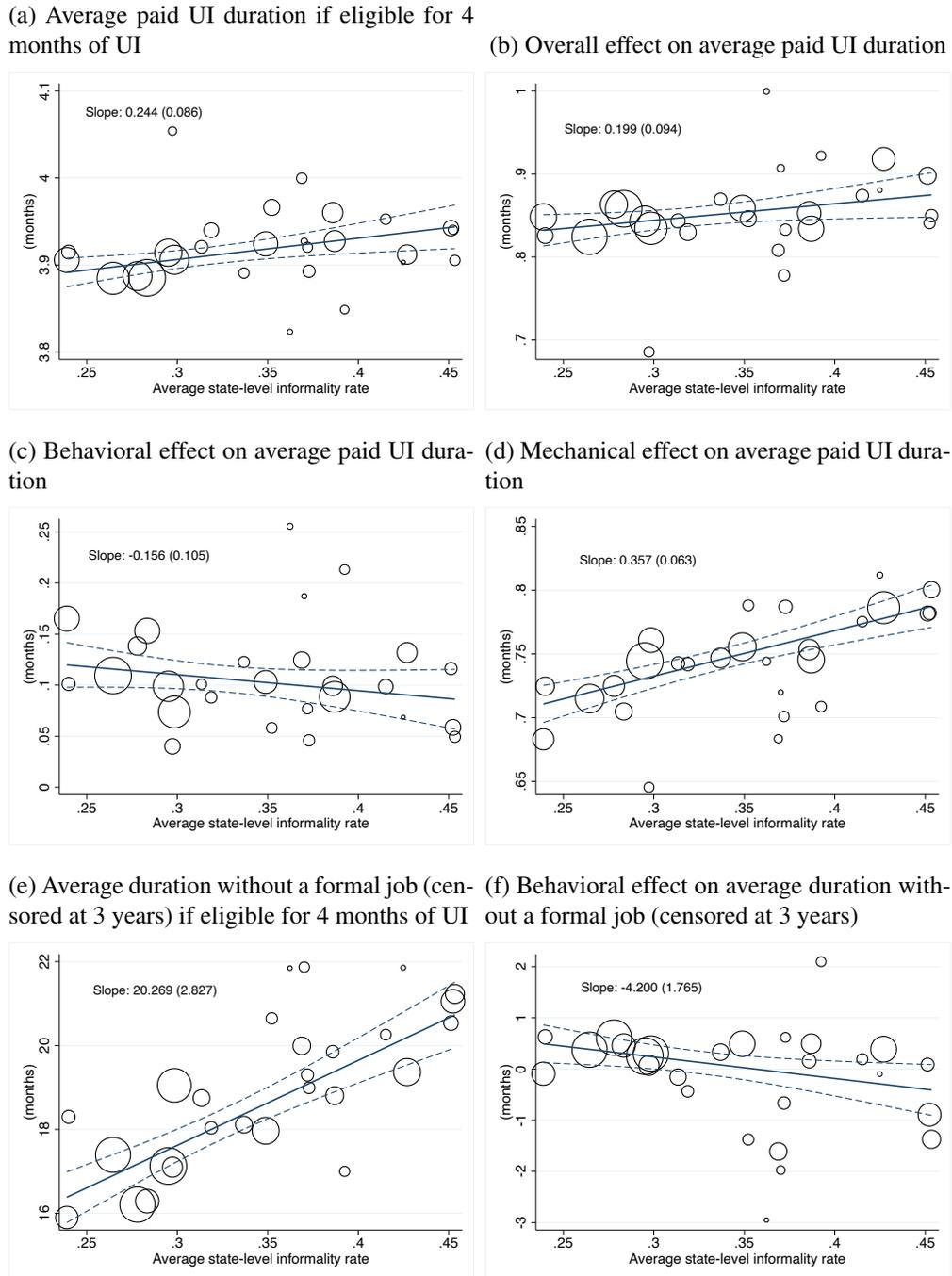
(a) Average paid UI duration if eligible for 4 months of UI

(b) Overall effect on average paid UI duration

(c) Behavioral effect on average paid UI duration

(d) Mechanical effect on average paid UI duration

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

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

Notes: The figure present results similar to those in Figure 6 in the paper but including layoffs since 2002 and using a 4-month bandwidth around the cutoff, as in Figure 7c in the paper. It displays results from estimating the same RD specifications as in Table 1C for each of the 27 Brazilian states, separately. State-level estimates are plotted against average state-level informality rates. Lines display the result of a WLS regression of these estimates on informality rates weighting them by the inverse of their standard error squared (with 95% confidence intervals). The estimated slope is displayed with its standard error (in parenthesis).

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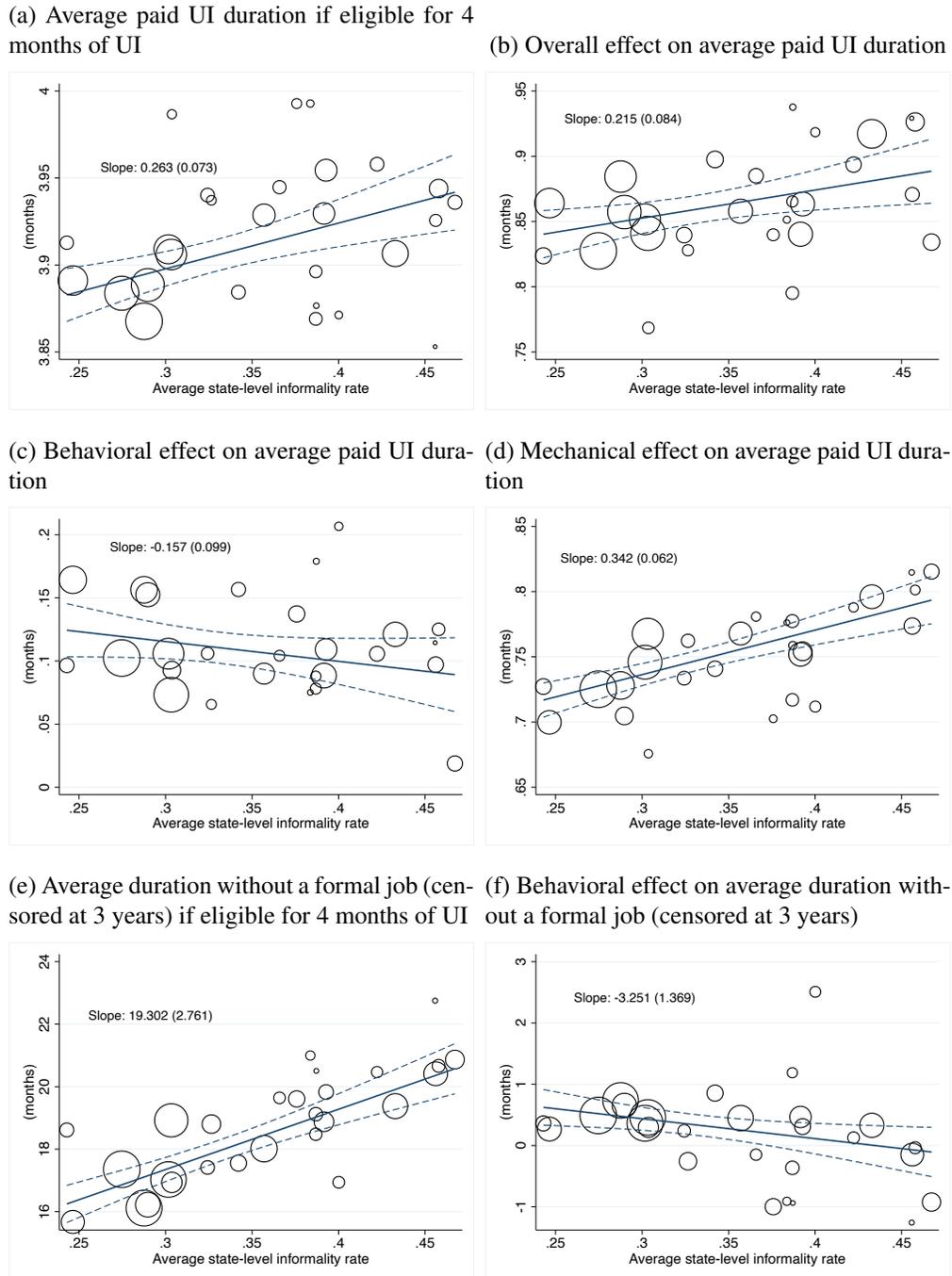Figure E.6: Heterogeneity of RD results with informality rates (excluding layoffs after 2007)

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

(b) Overall effect on average paid UI duration

(c) Behavioral effect on average paid UI duration

(d) Mechanical effect on average paid UI duration

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

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

Notes: The figure present results similar to those in Figure 6 in the paper but including layoffs since 2002 and excluding layoffs after 2007, as in Figure 7d in the paper. It displays results from estimating the same RD specifications as in Table 1D for each of the 27 Brazilian states, separately. State-level estimates are plotted against average state-level informality rates. Lines display the result of a WLS regression of these estimates on informality rates weighting them by the inverse of their standard error squared (with 95% confidence intervals). The estimated slope is displayed with its standard error (in parenthesis).

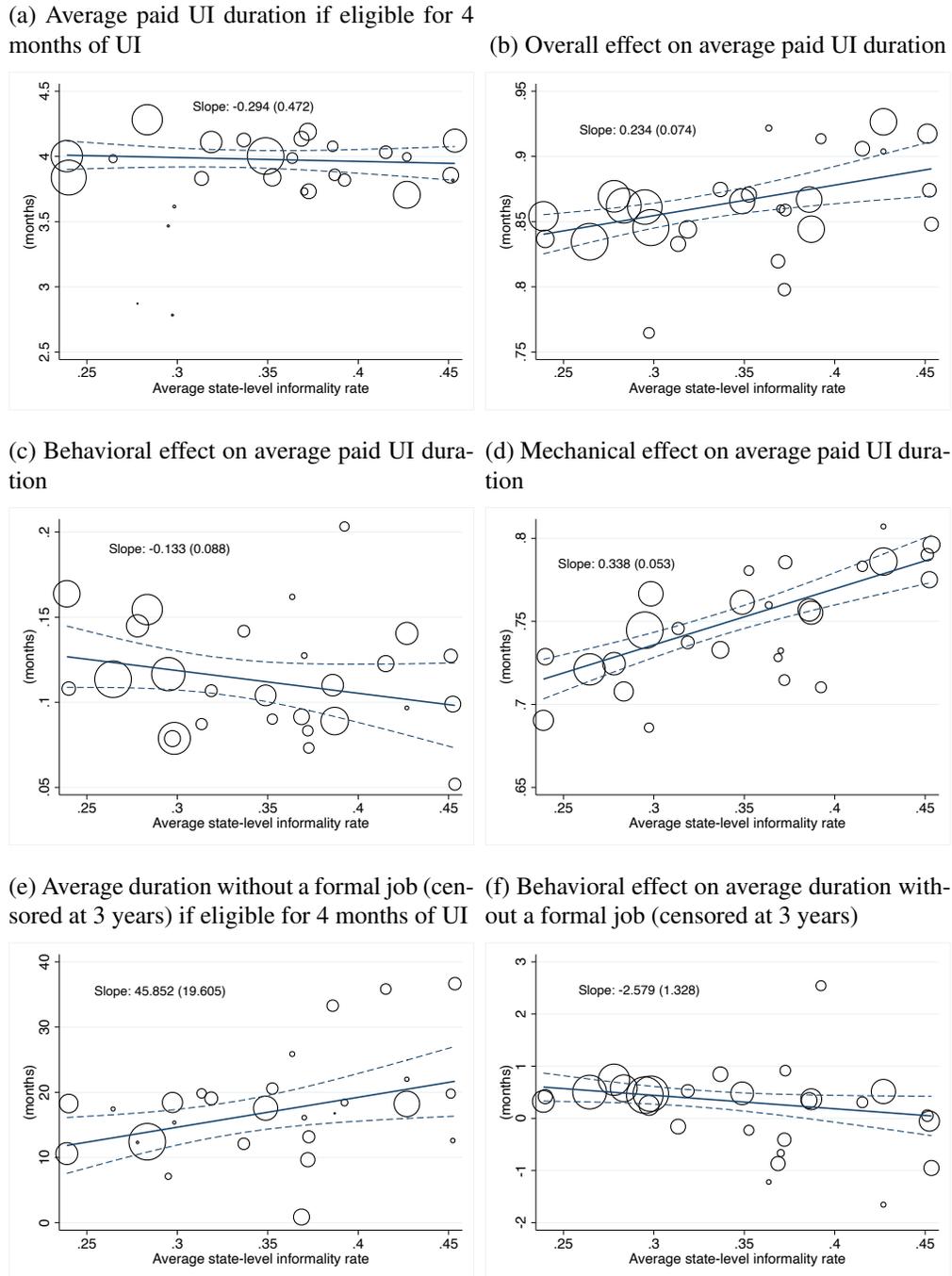Figure E.7: Heterogeneity of RD results with informality rates (controlling for worker characteristics)

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

(b) Overall effect on average paid UI duration

(c) Behavioral effect on average paid UI duration

(d) Mechanical effect on average paid UI duration

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

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

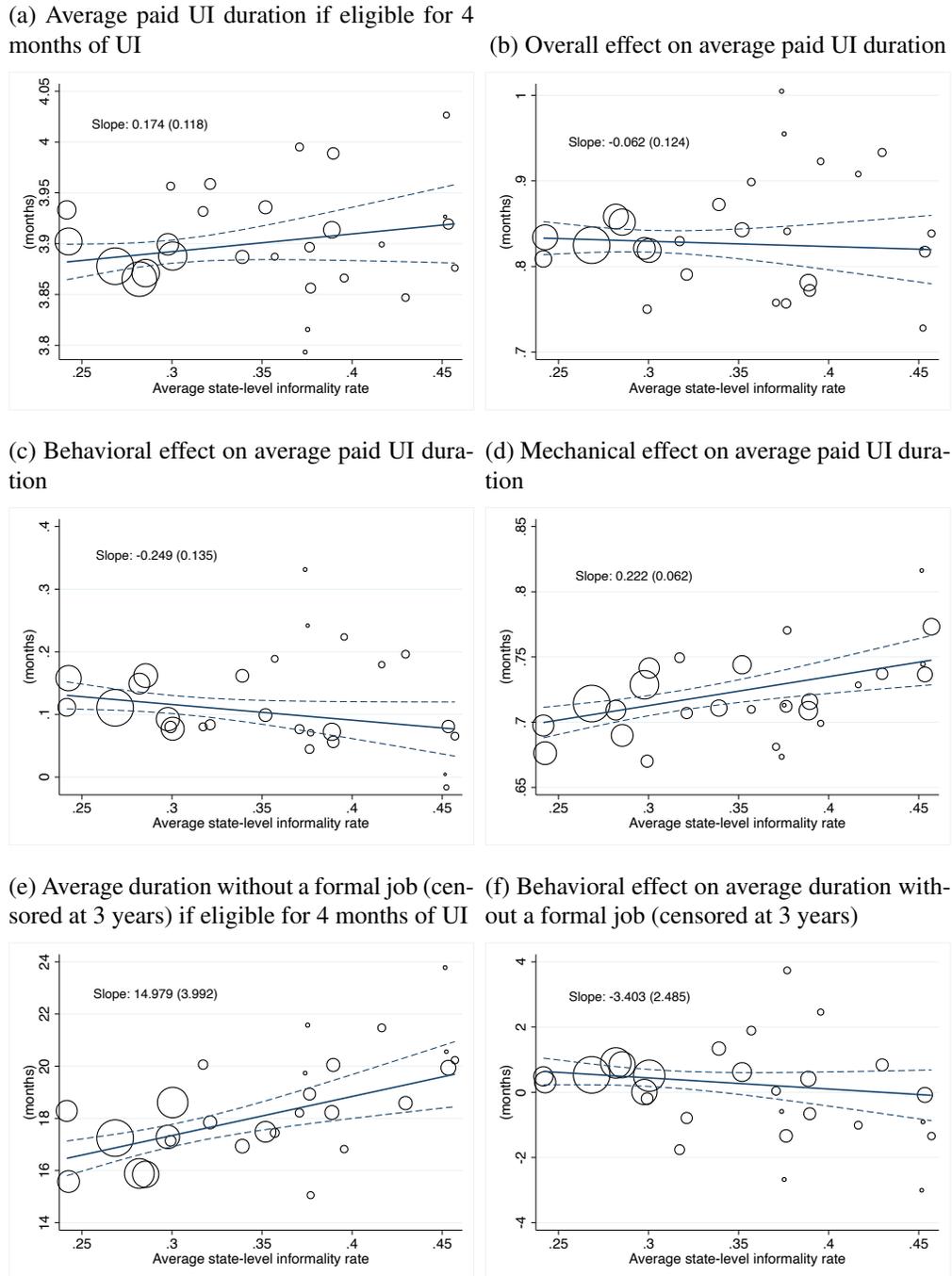*Notes:* The figure present results similar to those in Figure 6 in the paper but including layoffs since 2002 and including a rich set of individual controls (see paper for details), as in Figure 7e in the paper. It displays results from estimating the same RD specifications as in Table 1E for each of the 27 Brazilian states, separately. State-level estimates are plotted against average state-level informality rates. Lines display the result of a WLS regression of these estimates on informality rates weighting them by the inverse of their standard error squared (with 95% confidence intervals). The estimated slope is displayed with its standard error (in parenthesis).

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Figure E.8: Heterogeneity of RD results with informality rates (Excluding workers with statutory UI replacement rates above 80% and below 20%)

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

(b) Overall effect on average paid UI duration

(c) Behavioral effect on average paid UI duration

(d) Mechanical effect on average paid UI duration

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

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

Notes: The figure present results similar to those in Figure 6 in the paper but including layoffs since 2002 and excluding workers with statutory UI replacement rates above 80% and below 20%, as in Figure 7f in the paper. It displays results from estimating the same RD specifications as in Table 1F for each of the 27 Brazilian states, separately. State-level estimates are plotted against average state-level informality rates. Lines display the result of a WLS regression of these estimates on informality rates weighting them by the inverse of their standard error squared (with 95% confidence intervals). The estimated slope is displayed with its standard error (in parenthesis).

E.4 Alternative regression analysis

In this section, we confirm the results in Section IV in the paper (and the robustness checks presented above in this Web Appendix) using another regression analysis.

E.4.1 Specification and motivation

We estimate heterogeneous effects with informality rates directly by using a variant of the RD specification used in the paper, in which all the right-hand-side variables are interacted with informality rates (demeaned). We enter informality rates only linearly because a linear relationship seems to be a sensible first approximation based on the patterns in Figure 6 in the paper.

This approach has three advantages. First, it allows us to estimate correlations with informality rates holding (un)employment rates constant (also interacting all the right-hand-side variables with unemployment rates). Informality and unemployment rates are correlated in Brazil and in other middle-income countries (Bosch and Maloney, 2010). The results in the paper could thus in theory come from underlying correlations with unemployment rates, which have already been studied in contexts of lower informality (Kroft and Notowidigdo, 2016; Schmieder et al., 2012). In particular, we estimate the following specification:

$$
\begin{aligned}
y_{i,s,t} = {} & \alpha + \beta \times \mathbb{1}(T_i \geq 0) + f(T_i) + \gamma_{inf} \times InformalityRate_{i,s,t} \\
& + \delta_{inf} \times InformalityRate_{i,s,t} \times \mathbb{1}(T_i \geq 0) + \zeta_{inf} \times InformalityRate_{i,s,t} \times f(T_i) \\
& + \gamma_{unemp} \times UnemploymentRate_{i,s,t} + \delta_{unemp} \times UnemploymentRate_{i,s,t} \times \mathbb{1}(T_i \geq 0) \\
& + \zeta_{unemp} \times UnemploymentRate_{i,s,t} \times f(T_i) + \gamma_{other} \times OtherRate_{i,s,t} \\
& + \delta_{other} \times OtherRate_{i,s,t} \times \mathbb{1}(T_i \geq 0) + \zeta_{other} \times OtherRate_{i,s,t} \times f(T_i) + \varepsilon_{i,s,t} \qquad (2)
\end{aligned}
$$

where $OtherRate = 1 - InformalityRate - UnemploymentRate - PrivateFormalEmploymentRate$. The omitted labor force category is the rate of private-sector formal employment. Thus, the coefficients γ_{inf} and δ_{inf} capture variation in the informality of the private sector, controlling for variation in unemployment rates or in the size of the rest of the labor force.

Second, it allows us to consider more disaggregated measures of the informality rate prevailing in a given labor market. The average state-level informality rates used in Section IV in the paper may not best capture the characteristics of the relevant labor market for workers in a given state because of variation over the years and across workers. Therefore, we use state-level rates that vary along two important dimensions: year and gender (this is why they are indexed by i, s, t).[7] It

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[7]All rates are demeaned and measured using PNAD surveys which are representative of each state s in each year t. We measure the informality rate for each state-year-gender combination, as the share of informal workers in the

is well known that labor markets became more formal over our sample period in Brazil. Moreover, average labor market composition still differs greatly by gender in Brazil. For instance, rates of labor force participation and of private-sector formal employment remain much lower for women.

Third, it allows us to control for year fixed effects, state fixed effects, as well as a rich set of individual controls. Controlling for year fixed effects shows that our correlations with informality rates are not simply due to overall trends in Brazil. Controlling for year and state fixed effects shows that our correlations with informality rates are not simply due to fixed differences across states. Controlling for workers characteristics shows that our correlations with informality rates are not simply due to the fact that workers in different states have different observables.

E.4.2   Main results

Table E.1 presents our main results. We include layoffs since 2002 as in Table 1B in the paper to maximize sample size. This is because controlling for state and year fixed effects absorbs a lot of the variation in informality rates necessary to identify the coefficients γ_{inf} and δ_{inf} in equation (2).

Table E.1 displays results for the actual paid UI duration, the counterfactual paid UI duration, and the duration without a formal job censored at three years after layoff in columns (1)-(3), respectively. It presents estimates of the average impact (β), of the correlation between the level of the outcome at the cutoff coming from below and informality rates (γ_{inf}), and of the correlation between the average impact and informality rates (δ_{inf}). Standard errors are clustered at the level of the running variable for the first coefficient, at the state level for the second coefficient, and at both levels (two-way clustering) for the third coefficient. The table also presents estimates of the same correlations with unemployment rates as a benchmark (γ_{unemp} and δ_{unemp}). All other aspects of our estimations are similar as in Table 1 in the paper (bandwidth, kernel, local linear regression).

Panel A does not include any controls or year and state fixed effects as in Figure 6 in the paper. Results for the average impact are similar to the results in Figure 5 and Table 1 in the paper. The overall effect on the average paid UI duration is .853 month, the behavioral effect on the average paid UI duration is .116 month (so the mechanical effect is .737 month), and the behavioral effect on the average duration without a formal job is .44 month. These estimates imply an efficiency cost of \$.209 per \$ reaching mechanical beneficiaries (compared to \$.217 in the paper).

Column (1) shows that the average paid UI duration at the cutoff for workers eligible for four months of UI is increasing in informality rates as in Figure 6a in the paper (γ_{inf}). Moreover, the overall effect on the average paid UI duration is increasing in informality rates as in Figure 6b in the paper (δ_{inf}). Providing UI is thus more costly in labor markets with higher informality. However, this is entirely driven by a larger mechanical effect. Indeed, column (2) shows that the

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non-farm labor force of the state in that year for male or female adults. The unemployment rate and the rate of "other" jobs are measured similarly.

Table E.1: Heterogeneity with informality rates (alternative regression analysis)

	A. No other controls, state or year fixed effects			B. Adding year fixed effects and other controls			C. Adding state and year fixed effects, and other controls		
	Actual paid UI duration (months)	Counterfactual paid UI duration (months)	Duration without a formal job censored at 3 years (months)	Actual paid UI duration (months)	Counterfactual paid UI duration (months)	Duration without a formal job censored at 3 years (months)	Actual paid UI duration (months)	Counterfactual paid UI duration (months)	Duration without a formal job censored at 3 years (months)
	[1]	[2]	[3]	[1]	[2]	[3]	[1]	[2]	[3]
Tenure ≥ 24 months	0.8529	0.1164	0.4395	0.8509	0.1143	0.4072	0.8509	0.1144	0.4185
	(0.0073)	(0.0048)	(0.0981)	(0.0069)	(0.0032)	(0.0710)	(0.0069)	(0.0032)	(0.0675)
Informality rate	0.2627	0.5072	12.82	0.1944	0.3994	9.944	0.3534	0.5807	4.570
	(0.0593)	(0.0618)	(1.827)	(0.0453)	(0.0488)	(2.704)	(0.0764)	(0.0958)	(1.999)
Tenure ≥ 24 months × informality rate	0.1219	-0.1202	-2.034	0.1269	-0.1134	-1.843	0.1270	-0.1129	-1.830
	(0.0493)	(0.0473)	(0.5135)	(0.0557)	(0.0569)	(0.4181)	(0.0574)	(0.0598)	(0.9706)
Unemployment rate	0.5210	1.016	39.89	0.3738	0.8483	31.85	0.6600	1.078	20.39
	(0.1395)	(0.1779)	(6.048)	(0.1169)	(0.1409)	(4.936)	(0.1453)	(0.2061)	(5.202)
Tenure ≥ 24 months × Unemployment rate	-0.2217	-0.7117	-1.207	-0.2164	-0.7029	-1.129	0.2106	-0.7001	-1.336
	(0.0743)	(0.1471)	(1.173)	(0.0713)	(0.1375)	(1.757)	(0.0787)	(0.1449)	(2.340)
# Observations	2,667,223	2,667,223	1,880,941	2,667,223	2,667,223	1,880,941	2,667,223	2,667,223	1,880,941

Notes: The table displays results from estimating variants of equation (2) using the same sample as in Table 1B in the paper (including layoffs since 2002). All specifications use a six-month bandwidth around the cutoff and an edge kernel. They include a fixed effect for a tenure level above the cutoff, linear controls in tenure (normalized to the cutoff) on each side of the cutoff, linear controls in the (demeaned) rate of informality, of unemployment, and of "other" employment (private-sector formal employment is the omitted category), and these latter linear controls interacted with the tenure variables (the fixed effect and the two linear controls). Other controls include fixed effects for calendar month of hiring (12) and separation (12), sector (4), gender (2), education (5), and establishment size (10), as well as 4th order polynomials in age and in the logarithm of the real monthly wage before layoff (demeaned).

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behavioral effect on the average paid UI duration is decreasing in informality rates as in Figure 6c in the paper. The mechanical effect is thus increasing in informality rates as in Figure 6d in the paper. Consistent with this result, column (3) shows that the average duration without a formal job at the cutoff for workers eligible for four months of UI is increasing in informality rates as in Figure 6e in the paper. Finally, column (3) shows that the behavioral effect on the average duration without a formal job is in fact decreasing in informality rates as in Figure 6f in the paper. All these estimates are statistically significant.

Taken together, these results imply that the efficiency cost (and the elasticity of the duration without a formal job) is decreasing in informality rates. They also imply that the results in Figures 6 and 7 in the paper hold when we use variation in the informality of the private sector controlling for variation in unemployment rates and in the size of the rest of the labor force.

Panels B and C in Table E.1 show that these correlations with informality rates are not simply due to national trends, to differences in observable worker characteristics, or to fixed differences across states. Specifications in Panel B include year fixed effects and the same individual controls as in the robustness check in Figure 7e in the paper. Specifications in Panel C further add state fixed effects. The signs of the correlations with informality rates remain identical in Panels B and C. The magnitudes of the correlations between the average impacts and informality rates (δ_{inf}) also remain very similar. The magnitudes of the correlations between the levels of the outcome at the cutoff coming from below and informality rates (γ_{inf}) remain similar for the paid UI duration outcomes but becomes smaller for the duration without a formal job. All these estimates are statistically significant (one coefficient is only significant at the 10% level when adding state fixed effects).

In sum, the results in Table E.1 support the results in Section IV in the paper and the conclusion that the efficiency cost may in fact be lower in labor markets with higher informality.

F   Appendix for Section V

We provide here some additional details for Section V of the paper.

F.1   Descriptive statistics for the administrative and PME samples used in Section V.A

Table F.1 provides descriptive statistics for the administrative and PME samples used in Section V.A in the paper. Column (1) displays the mean (standard deviation in brackets) of worker characteristics in the administrative sample. Columns (2) and (3) display the estimated mean (using survey weights; standard errors clustered at the individual level in parenthesis) of the same characteristics for workers in the PME sample. Column (2) only considers non-employed workers observed in their first month after layoff in PME. They should be more similar to the workers in the administrative sample. Column (3) considers all the observations used in our maximum likelihood estimation (a given worker can appear more than once in the PME sample). Note that the replacement rate can only be measured in the administrative sample because we have no information about the wage at layoff in PME surveys.

In the administrative sample, the average displaced formal worker is more likely to be male (62%), is 33.8 years old, has 9.3 years of education, has 4.3 years of tenure, has a relatively high replacement rate (64%), is more likely to have lost a job from the service sector (43.7%) than from the commercial sector (28.3%), the industrial sector (23%), or the construction sector (5%). Finally, only 11.9% of layoffs took place in the two metropolitan areas in the Northeast, which are poorer than the other four metropolitan areas covered by the sampling scheme of PME surveys.

The sample of non-employed workers observed in their first month after layoff in PME surveys looks relatively similar to the administrative sample. The average worker is slightly less likely to be male (57.6%), has a comparable age (33.4 years old), is slightly more educated (9.7 years), and has slightly more tenure at layoff (4.9 years). The share coming from the Northeast is also comparable (12.3%). The shares of layoffs from the service and construction sectors are very similar, but we have more layoffs from the commercial sector and fewer layoffs from the industrial sector in PME surveys. All these comparisons also apply when we consider the sample in column (3).

Given some of the differences between the administrative and PME samples, it is reassuring that the patterns of overall reemployment estimated using PME are similar when we reweight the sample in column (3) such that it compares better to the administrative sample (see below).

Table F.1: Descriptive statistics for the administrative and PME samples used in Section V.A

	Administrative sample [1]	PME sample (first month after layoff) [2]	PME sample (all observations) [3]
Male (dummy)	0.6217 [0.4850]	0.5758 (0.0122)	0.5566 (0.0060)
Age (years)	33.77 [8.622]	33.42 (0.2203)	33.73 (0.1089)
Years of education	9.296 [3.043]	9.689 (0.0799)	9.674 (0.0390)
Tenure (years)	4.323 [3.490]	4.969 (0.0999)	5.049 (0.0544)
Statutory UI replacement rate	0.6404 [0.2256]		
Laid off from service sector (dummy)	0.4368 [0.4960]	0.4306 (0.0123)	0.4563 (0.0058)
Laid off from commerical sector (dummy)	0.2830 [0.4504]	0.2226 (0.0099)	0.2187 (0.0047)
Laid off from industrial sector (dummy)	0.2300 [0.4208]	0.3024 (0.0113)	0.2886 (0.0053)
Laid off from construction sector (dummy)	0.0502 [0.2184]	0.0443 (0.0050)	0.0364 (0.0020)
Laid off from the Northeast (dummy)	0.1185 [0.3232]	0.1227 (0.0060)	0.1285 (0.0028)
# Observations	3,393,055	2,536	19,904

Notes: The table provides descriptive statistics for the administrative and PME samples used in Section V.A in the paper. Column (1) displays the mean (standard deviation in brackets) of worker characteristics in the administrative sample. Columns (2) and (3) display the estimated mean (using survey weights; standard errors clustered at the individual level in parenthesis) of the same characteristics for workers in the PME sample. Column (2) only considers non-employed workers observed in their first month after layoff in PME. Column (3) considers all the observations used in our maximum likelihood estimation (a given worker can appear more than once in the PME sample). Note that the replacement rate can only be measured in the administrative sample because we have no information about the wage at layoff in PME surveys.

F.2 Robustness checks for the results in Section V.A

Figure F.1 replicates the patterns of overall and formal reemployment in Figures 8a-8c in the paper, but reweighting the PME sample such that it compares better to the administrative sample based on observable worker characteristics (DiNardo et al., 1996). We combine the administrative and PME samples and estimate a logit model for the probability that an observation belongs to the administrative data ($Prob_{RAIS}$). As predictors, we use dummies for years of layoff, calendar months of layoff, education levels (we use the education information in PME to create the same categorical variable as in RAIS), gender, sectors of activity (we use the sector information in PME to create the same variable as the first sector variable in RAIS), and state, as well as a second-order polynomial in age and tenure (we aggregate tenure by years, because PME does not include more detailed tenure information). We then re-estimate the hazard rates of finding any new job in PME by maximum likelihood using as weights $\frac{\widehat{Prob_{RAIS}}}{1-\widehat{Prob_{RAIS}}}\frac{Npme}{Nrais}$, where $Nrais$ is the number of observations in the administrative sample and $Npme$ is the number of observations in our PME sample.

The estimated patterns of overall reemployment are almost identical with this alternative weighting scheme. So the findings in Figures 8a-8c in the paper are robust to using an alternative weighting scheme. However, this scheme does not constitute an "ideal" set of weights. For instance, it is unclear that we would like to reweight individuals who are non-employed for five months such that they compare better to the universe of displaced formal workers: individuals who remain non-employed for five months may be different from the average displaced formal worker. This is the reason why we present the results using survey weights in the paper.

## F.3 Additional results supporting the evidence in Section V.B

In Figure 8d in the paper, we present results for the specification in equation (3) in the paper both omitting and including the same rich set of worker characteristics used for the reweighting of the PME sample above. As we explain in the text of Section V.B:

> "*This is to address possible concerns of selection bias with the specification in equation (3). We would, for instance, underestimate the relative earnings of workers reemployed informally if their earnings were below the average before layoff. The controls that we include can explain most of the cross-sectional difference in earnings between formal employees and informal workers in the overall population (see Appendix F3). Thus, we would expect them to have a strong influence on our results in Figure 8d if these results were severely affected by selection bias.*"

We present here the analysis mentioned in that quote from the paper. Specifically, we consider all the observations in PME between 2005 and 2010 where an individual is between 19 and 54

Figure F.1: Same patterns as in Figures 8a-8c but reweighting the PME sample such that it compares better to the administrative sample

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

(b) Survival rate without a formal job vs. survival rate in non-employment
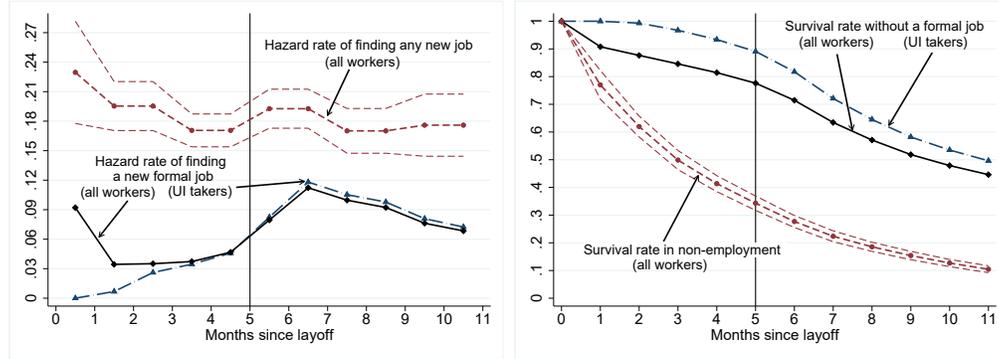
(c) Share finding a new formal job vs. share finding any new job
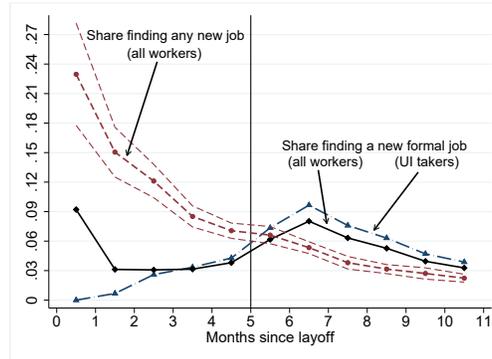
Notes: The figure displays similar graphs as in Figures 8a-8c in the paper, but reweighting the PME sample such that it compares better to the administrative sample based on observable worker characteristics.

years old and is a private-sector formal employee or an informal worker (self-employed or informal employee). We then estimate the following specification:

$$y_{i,t} = \alpha + \beta \times Informal_{i,t} + \gamma \times X_{i,t} + \varepsilon_{i,t}, \tag{3}$$

where $Informal_{i,t}$ identifies individuals i who are informal workers in a given month t. Formal employees constitute the omitted group. Table F.2 displays the estimated coefficient β divided by the average net earnings of formal employees. The specification in column (1) does not include any controls, while the specification in column (2) includes the controls. Standard errors are clustered by individual and estimations use survey weights.

Without controls, we estimate that informal workers have earnings levels that are on average 13.9% lower than formal employees. With controls, this difference drops to 1.47%. The controls

Table F.2: Evidence that the controls used for Figure 8d can explain most of the cross-sectional difference in earnings between formal employees and informal workers in the overall population.

	Net earnings if informal worker vs. formal employee	
	No controls [1]	With controls [2]
Informal worker	-0.1387 (0.0043)	-0.0147 (0.0040)
# Observations	2,157,161	2,157,161

Notes: The table shows that the controls that we use for Figure 8d in the paper can explain most of the cross-sectional difference in earnings between formal employees and informal workers in the overall population. It displays the results from estimating the specification in equation (3), diving the estimated coefficient β by the average net earnings of formal employees. The specification in column (1) does not include any controls, while the specification in column (2) includes the controls. Standard errors are clustered by individual and estimations use survey weights.

can thus explain most of the cross-sectional difference in earnings between formal employees and informal workers in the overall population.

F.4 Replicating Figure 8d adding the average UI benefit level from a comparable sample of workers in the administrative data

A caveat with the estimates in Figure 8d in the paper is that they do not account for the UI benefits that workers can receive while eligible for UI. We thus gauge the role of UI benefits in Figure F.2. Specifically, we replicate the results in Figure 8d but using as outcome the net household earnings to which we add the average UI benefit level from a comparable sample of workers in the administrative data. We do not show estimates for workers reemployed in a formal job as these workers stopped being eligible for UI. We show estimates for the 10 months after layoff as in Figure 8d for simplicity, but workers in the sample were only eligible for five months of UI.

Figure F.2 shows that adding the average UI benefit level from a comparable sample of workers in the administrative data brings workers who remain non-employed to 85%-90% of household earnings before layoff, and to 105%- 110% for those reemployed informally. Without estimating its efficiency cost, it thus appears unlikely that increasing the UI benefit level would generate large welfare gains (non-employed workers may be able to self-insure against 10%-15% income losses).

Figure F.2: Replicating Figure 8d adding the average UI benefit level from a comparable sample of workers in the administrative data
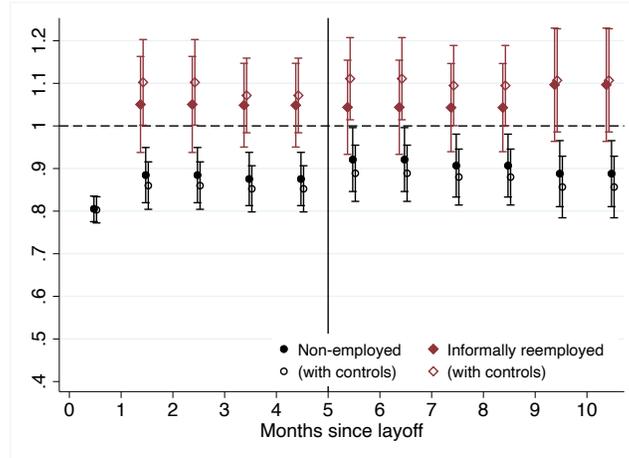
Notes: The figure replicates Figures 8d in the paper using as outcome the sum of the net household earnings and of the average UI benefit level from a comparable sample of workers in the administrative data. We do not show estimates for workers reemployed in a formal job as these workers stopped being eligible for UI. We show estimates for the 10 months after layoff as in Figure 8d for simplicity, but workers in the sample were only eligible for five months of UI.

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