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Unemployment Insurance and the Duration of Employment: Theory and Evidence from a Regression Kink Design

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Abstract

Can unemployment insurance (UI) affect the behavior of employed workers and the duration of their employment spells? I apply a regression kink design to address this question using data from the Brazilian labor market. Exploiting the UI schedule, I find that a 1% higher potential benefit level increases job duration by around 0.35%. This result is driven by the fact that a higher potential benefit level reduces the probability of job quits, which are not covered by UI. I develop a simple model showing that the positive effect on employment duration implies that the optimal benefit is higher than otherwise and delivers a simple welfare formula based on sufficient statistics. A simple calibration exercise shows that this elasticity affects welfare with a similar magnitude as the well-known elasticity of unemployment duration to the benefit level.

JEL classification: I38, J65.

Keywords: Unemployment Insurance, Employment Duration, Regression Kink Design, Sufficient Statistics Welfare Analysis.

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1 Introduction

A large body of theoretical and empirical literature has studied a number of issues related to unemployment insurance (UI). Perhaps its most well-established result is that more generous benefits increase the duration of unemployment spells. However, the question of whether and how unemployment insurance affects the dynamic of employment spells has been much less commonly studied. This paper presents evidence on the existence of a causal link between the level of unemployment benefits and the duration of employment spells. I propose a novel identification strategy to investigate how job duration reacts to variations in the potential level of unemployment benefits to which workers are entitled in the case of a dismissal. Surprisingly, I find that a higher potential benefit level causes an increase in the duration of employment spells.

Despite the scarce empirical evidence, there are a number of straightforward reasons to suspect that the availability of UI may affect the duration of employment. First, higher unemployment benefit increases the value of unemployment for employed workers. Therefore, it reduces the incentives for these workers to put effort into keeping their jobs, thus potentially reducing the duration of employment. Second, in the majority of UI systems, only workers laid-off against their will are eligible for unemployment benefits. Therefore, it should decrease the incentives for workers to quit because it implies giving up unemployment benefits, especially if the reason for quitting is not starting a new job. Unlike the first mechanism, this leads workers to stay longer in their jobs. Third, most UI systems have minimum eligibility requirements (MER) in place, which usually require workers to be employed for a minimum length of time to be entitled to UI. Such a feature creates incentives for workers to hold their jobs until the minimum eligibility period, whereby it should increase the duration of employment spells. Moreover, in most countries, potential duration is an increasing function of tenure prior to the dismissal. Similarly to MER, this provides an incentive for workers to hold their jobs for longer periods.

All of these are simple theoretical predictions that do not rely on any unusual assumptions. However, the real question is whether one or more of these mechanisms are able to create any economically meaningful effect on the duration of employment spells. Note that such an effect could be positive or negative depending on which channel described above dominates. To answer this question avoiding the interference of confounding factors, I exploit the assignment rule of the UI benefit level in Brazil by implementing a regression kink design (RKD). This strategy leverages upon the kinked relationship between benefit level and (pre-displacement) earnings to assess the causal effect of potential benefit level on the duration of employment spells. To perform the analysis, I take advantage of eight years of linked employer-employee data covering the whole Brazilian formal labor market. I find that a 1% increase in potential benefit level causes employment spells to last 0.35% longer on average. This result is driven by a strong negative effect on quitting behavior. These findings show that unemployment insurance can create powerful incentives affecting the dynamics of employment spells.

Even though such a finding may be interesting *per se*, in itself it falls short of addressing the relevant policy question. The obvious remaining issue is whether this result holds any relevance for welfare. Does the fact that UI lengthens the time that workers spend employed have any implications for the optimal level of unemployment benefits? To address this question, I provide a simple yet general search model in partial equilibrium, where the duration of employment spells is endogenous to the incentives created by unemployment insurance. First, the model allows for the presence of informal jobs opportunities, which might be relevant for the analysis in the context of a developing country.[1] Second, it sheds light on the potential mechanisms through which UI can affect job duration. Third, and most importantly, the model reveals how such a margin impacts the optimal benefit level. It shows that when this effect is positive the optimal benefit level is higher than otherwise because higher benefit induces workers to

[1]Nevertheless, the general model also embodies the case where labor informality is small or absent.

contribute to the system for a longer period. This allows the policy maker to sustain a given benefit level by imposing a lower distortionary tax on employed workers. Therefore, the qualitative implication of this paper's key empirical finding is clear: the positive effect of UI on employment duration leads to a higher optimal benefit level.

One final important question is whether this effect is quantitatively relevant. To address this question, I take advantage of a simple welfare formula derived from the model and based on few sufficient statistics, which can be easily linked to the data. It generalizes the reduced-form welfare formula provided by Chetty (2008) in a way that it can deal with UI distortions on employment duration, while it also allows for the presence of informal labor. First, the formula shows that to evaluate policy optimality one only needs to observe outcomes from the formal labor market.[2] Second, and most importantly, a simple calibration exercise of local optimality shows that the effect of UI on job duration weighs on welfare with the same order of magnitude as the well-known elasticity of unemployment duration. Therefore, this result suggests that the effect of benefit level on the duration of employment spells can be, in quantitative terms, similarly relevant for policy as the typical effect on joblessness duration.

It is worth noting that there exists a small and relatively old body of literature studying the effects of UI on a few different aspects of unemployment inflow.[3] From the empirical perspective, the closest related contribution to this paper is Winter-Ebmer (2003) who studies how unemployment entry responds to a large increase in potential duration for

[2] This result is in line with Gerard and Gonzaga (2014), who study another aspect of UI in the presence of an informal labor market.

[3] Most of these studies use Canadian data and find that employment hazard rates spike exactly when employed workers qualify for unemployment benefits, which usually happens when workers reach a given job tenure [Baker and Rea Jr (1998); Christofides and McKenna (1995, 1996); Green and Sargent (1998) and Rebollo-Sanz (2012) for Spain]. Bingley et al. (2013) instead study the relationship between UI and wage dynamics. Using Danish data, they find that (voluntary) UI membership is associated with less heterogeneous wage growth and increased wage instability. Complementary evidence suggests that this is a result of moral hazard.

old workers in specific Austrian regions. He finds that unemployment inflow increases by 4-11% and argues that the effect seems to be driven by firms that aim to get rid of high-tenured and expensive old workers. A key difference from this paper is that such estimates are local to old workers (50-65 years old) who may use UI as a pathway to retirement and comprise the response to an unusually large extension of potential benefit duration (from 52 to 209 weeks). Interestingly, Lalive et al. (2011) assess the effects of a different policy change in Austria, finding that an extension in UI potential duration led to higher unemployment rate mostly due to a rise in unemployment inflow rather than outflow.

Taken together, the existing literature indicates that UI affects the dynamics of employment spells at least to some extent. One key question that is not directly answered in any of these studies is whether UI can affect the average time that workers spend employed in an economically significant way. This constitutes the first main contribution provided by this paper: using Brazilian data, it presents credible quasi-experimental evidence that higher potential benefit level can actually increase the duration of employment spells. Besides satisfying all of the standard tests from RD designs, these results are robust to permutation tests (Ganong and Jäger (2016)) and RK estimates in double and triple differences. This contribution is complemented by the finding that such responses are driven by a strong negative effect on quitting probabilities, while layoff hazard does not seem to be significantly affected.[4] Even though estimates on layoff probabilities are somewhat noisy, they do not lend support to the idea that when UI is not fully experience rated (as is the case in Brazil) it represents a relevant subsidy for firms to temporarily layoff workers (Feldstein, 1976, 1978; Topel, 1983, 1984). Moreover, these findings also highlight the notion that

[4] Using US survey data, Solon (1984) empirically studies the effect of entitling job quitters to UI and cannot identify any robust response on quitting rates. Instead, Light and Omori (2004) exploit cross-state and cross-year variations in UI benefit in the US, finding that an increase in UI benefits decreases job quits by a statistically significant but small amount. They argue that UI reduces quits because it provides fewer incentives for workers to perform on-the-job search in response to an expected layoff.

unemployment inflow may be substantially affected by UI incentives. This stresses the need for studies evaluating UI effects on unemployment outcomes to carefully assess whether the inflow into unemployment is exogenous to UI incentives in the given data.[5]

From a policy perspective, the existing (mostly empirical) literature provides little guidance on how UI effects on the dynamics of employment spells matter for the optimal benefit level. This paper's second main contribution is to provide a welfare formula for benefit level based on sufficient statistics that explicitly indicates how employment duration responses affect welfare. Moreover, such a result is robust to the presence of informal labor markets, which may hold prime interest for studying UI in developing countries.

The third key contribution of this work is putting together these empirical results with the theory to show that the welfare effect of this response on the duration of employment spells can be economically meaningful. From the theoretical perspective, this response weighs on welfare with the same weight as the well-known elasticity of unemployment duration to UI benefits. On the empirical spectrum, the estimated effect on job duration (≈ 0.35) falls within the range of estimates on the elasticity of unemployment duration to the benefit level found by previous studies (0.2-0.8, Card et al. (2015a)). Since both responses weigh equally on welfare, their relevance for the optimal benefit level are likely to be similar. It is also interesting to highlight that since the terms present in the welfare formula are sufficient statistics, these conclusions are not based on any specific model calibration; rather, they hold for any given set of primitives.

The remainder of this paper is structured as follows. The next section

[5]In the literature, studies often leverage upon UI assignment discontinuities to evaluate its effect on different aspects of unemployment spells. Such regression discontinuity designs are based on the assumption that the inflow into unemployment is exogenous to UI. While the findings presented here do not necessarily rule out the validity of the design, they call for a careful evaluation of the continuity condition on the density function and covariates required by any regression discontinuity design.

presents the model and the reduced-form welfare formula. In section 3, the institutional background is described and the identification strategy is presented. In section 4, estimation results are presented together with evidence on the validity of the econometric design. Section 5 provides a discussion of the empirical findings and a calibration exercise assessing how they link to welfare, and section 6 finally concludes.

2 Theory

The goal of this partial equilibrium model of labor supply is to study the potential mechanisms through which UI can affect the duration of employment spells and whether such margin matters for the optimal benefit level. The model generalizes the Baily-Chetty framework by introducing an employment stage with endogenous duration prior to unemployment and allowing for the presence of informal jobs (Baily (1978); Chetty (2006)).

2.1 The model

The model runs in discrete time and the agent's life lasts T periods {0, 1, ... , T-1}. For a matter of simplicity, I further assume that the agent's discounting rate and interest rates are equal to zero, as in Chetty (2008). In this economy, a representative worker starts the model employed with a wage equal to w and has to pay a tax τ, which finances the UI system. At the beginning of each period, the worker has to decide whether to quit his job or not, setting the decision variable $x_t \in \{0, 1\}$. In case he quits ($x_t = 1$), he remains out of the labor force until the end of the model and is not entitled to unemployment benefits. The value of quitting is randomly determined in each period according to some given probability distribution. In case the worker decides to keep his job ($x_t = 0$), he faces a layoff risk, which negatively depends on the level of effort e_t that he devotes to keeping his job. The idea is that workers can make costly decisions, which may help them to hold their jobs.

For instance, workers can decide how punctual they are or how willing they are to undertake extra hours. It can also be understood under the framework of a standard shirking model, whereby firms use the threat of firing to motivate workers to exert effort.[6] Work effort e_t is costly for the worker and its cost is given by the function $c(e_t)$, which is assumed to be continuous and convex ($c'(e_t) > 0$ and $c''(e_t) > 0$). Furthermore, without loss of generality, e_t is normalized in such a way that it directly represents the probability of a layoff. The problem of the employed worker is given by:

$$V_t(A_t) = \max_{A_{t+1} \geq L} v(A_t - A_{t+1} + w_t - \tau) + J^V_{t+1}(A_{t+1}) \tag{1}$$

$$J^V_t(A_t) = \max_{e_t, x_t}(1 - x_t)[e_t V_t(A_t) + (1 - e_t)J^U_t(A_t) - c(e_t)] + x_t Q_t \tag{2}$$

$$Q_t(A_t) = \max_{A_{t+1} \geq L} u(A_t - A_{t+1} + q_t) + Q^V_{t+1}(A_{t+1}) \tag{3}$$

V_t defines the value of the job that the worker has at the beginning of the model over time. A_t defines the worker's asset level at period t. Such a level is constrained by a lower bound L, which defines the maximum amount that the worker is able to borrow against the future. This implies that the model's credit market is imperfect and provides the rationale for UI provision. $v(.)$ defines the utility from consumption when formally employed. If the worker decides to quit his job, he moves into the quit state, which is an absorbing state of value Q_t. In case the worker does not quit, he keeps his job with probability e_t, which yields the value V_t. With probability $(1-e_t)$, he loses his job and becomes unemployed immediately at period t, which yields the value U_t.[7]

In the case where the worker is laid-off, he receives unemployment benefits

[6] However, it is worth noting that this model is silent with respect to the notion that variations in effort may affect firm productivity.

[7] A more intuitive and conventional assumption would be that a layoff at period t leads to unemployment at period t+1. However, here I shall assume that unemployment comes immediately for a matter of tractability of the model.

equal to $b_t < w_t$, provided that he has worked for at least k periods; otherwise, $b_t = 0$. This characterizes a minimum eligibility requirement (MER) for enjoying benefits, which is a typical feature of UI systems.[8] $u(.)$ defines his utility of consumption while out of the formal labor market. The unemployed agent searches for a new job, which can be in either the formal or informal labor market. In case he is formally re-employed, with wage w_t, his unemployment benefits are ceased. In case he finds an informal job, with wage w_t^i, he continues to collect unemployment benefits and can also continue to look for a formal job, which is assumed to provide a higher wage ($w_t > w_t^i$). This setup is in line with the idea that the government is unable to suspend UI benefits of the informally re-employed because it is almost impossible to identify such workers. The unemployed subsequently chooses his level of search effort s_t to find a new formal job, and the informal search effort z_t to find an informal job in case he does not succeed in the formal labor market. s_t is normalized to the probability that the worker finds a new formal job, while z_t is normalized to the probability that he finds an informal job conditional upon failing in the formal market. The cost of formal and informal search effort while unemployed are defined by $\psi(s_t)$ and $\phi(z_t)$, respectively. Both functions are assumed to be continuous and convex. For the sake of generality, formal search effort while informally employed f_t is allowed to have its own cost function $\omega(f_t)$, which is also assumed to be continuous and concave.

To summarize, with probability s_t the unemployed worker finds a new formal job, which immediately starts at period t and yields value E_t. With probability $(1 - s_t)z_t$, he finds an informal job and can continue to search for a formal job. If informally employed, the worker finds a new formal job with probability f_t. With probability $(1 - s_t)(1 - z_t)$, he fails to find a job at period t and remains unemployed, which yields the value U_t. His problem is

[8]More precisely, I am not aware of any UI system that does not require a minimum number of working months for workers to be granted UI benefits. Nevertheless, since k is a parameter that can take any value, the model also nests the case of systems that do not have MER.

given by:

$$J_t^U(A_t) = \max_{s_t, z_t} s_t E_t(A_t) + (1-s_t)(1-z_t)U_t(A_t) + (1-s_t)z_t I_t(A_t) - \psi(s_t) - \phi(z_t) \tag{4}$$

$$U_t(A_t) = \max_{A_{t+1} \geq L} u(A_t - A_{t+1} + b_t) + J_{t+1}^U(A_{t+1}) \tag{5}$$

$$I_t(A_t) = \max_{A_{t+1} \geq L} u(A_t - A_{t+1} + b_t + w_t^i) + J_{t+1}^I(A_{t+1}) \tag{6}$$

$$J_t^I(A_t) = \max_{f_t} f_t E_t(A_t) + (1-f_t)I_t(A_t) - \omega(f_t) \tag{7}$$

E_t is defined as the value of formal employment subsequent to unemployment. Following the same spirit as Chetty (2008), I assume this to be an absorbing state. This means that once an unemployed (or informally employed) worker finds a new formal job, he remains employed indefinitely. Furthermore, once reemployed, workers no longer have to contribute for UI since they no longer face any unemployment risk, whereby their jobs now last forever.

$$E_t(A_t) = \max_{A_{t+1} \geq L} v(A_t - A_{t+1} + w_t) + E_{t+1}(A_{t+1}) \tag{8}$$

The underlying idea of this setup is that the UI system can be properly represented by an initial period where employed workers contribute to the system, as well as a subsequent period where workers who have lost their jobs benefit from the insurance. This also seems to be the appropriate order of facts because any UI system requires workers to work first, only after which they can become eligible for UI. In other words, new entrants in the labor market are not entitled to benefits when they first start looking for a job. Therefore, in this model, for a matter of simplicity, the third state is neutral with respect to the UI system exactly because the initial employment and subsequent unemployment period are sufficient to capture the relevant features of the system. Making a link with the “real world”, once workers are reemployed after enjoying UI benefits, it works as if they were starting their first employment again, for all that matters for UI.

In summary, the model defines an economy with incomplete credit and insurance markets. All workers are employed at $t = 0$ with a net wage of $w_t - \tau$ and face a layoff risk, which negatively depends on their choice level of work effort, period after period. Workers are also allowed to quit their jobs and remain out of the labor force. If a worker is laid-off, he has to choose a level of formal and informal search to find a new job. While unemployed, he is entitled to UI benefits b_t, which last for a maximum of B periods, provided that he has worked for more than k periods (MER), otherwise he receives zero benefits. Finding a job in the informal sector allows the worker to receive the informal wage while continuing to collect UI benefits. Once the worker finds a new formal job, he falls into an absorbing state where his new job lasts indefinitely and he no longer has to contribute to the UI system.

UI Incentives and Job Duration In this setup, job duration essentially depends on two elements: the probability that the worker quits, remaining out of the labor market; and the work effort level, which determines the layoff probability. In order to understand how UI incentives affect job duration, we need to analyze the worker's quit and work effort decision problem. The agent quits whenever the expected value of remain in his job is lower than the expected value of quitting:

$$q_t = 1 \iff e_t V_t(A_t) + (1 - e_t) J_t^U(A_t) - c(e_t) < Q_t \tag{9}$$

An increase in benefit level does not directly affect the value of quitting (Q_t) because quits are not covered by UI. On the other hand, higher benefit raises the expected value of continuing to work because the employment state contains the case in which the worker is laid-off and granted unemployment benefits. Note that this applies even if the worker has not yet reached the minimum eligibility work period for UI because with some probability he will remain employed for sufficiently long to be eligible. Therefore, since UI raises the value of remaining employed while not directly affecting quitting payoffs, it reduces the worker's incentives to quit, which can lead to longer job duration. This constitutes the first mechanism in the model linking UI to job duration. Regarding UI and layoff probabilities, the worker chooses his effort level according to the following first-order condition:

$$c'(e_t) = V_t(A_t) - U_t(A_t) \tag{10}$$

The worker sets e_t in such a way that the marginal cost of effort $c'(e_t)$ equals the payoff of keeping his job, which is given by the difference between the value of employment and unemployment at time t. Prior to the moment in which the worker qualifies for UI (MER), a higher benefit level does not affect the current value of unemployment U_t because he would not receive UI benefits if laid-off at time t. However, a higher benefit affects the value of remaining employed because with some probability the worker's job will last sufficiently long for him to qualify for unemployment benefits. Thus, prior to MER, a higher benefit level provides an incentive for the worker to exert more effort into keeping his job, thus increasing his job duration. Once the worker reaches MER, more generous UI raises the value of unemployment and reduces the incentives for the worker to keep his job, thus decreasing job duration. More precisely, after MER, higher benefit level also raises the current employment value V_t through the probability that the worker is laid-off in subsequent periods. However, it increases the current value of unemployment U_t by a larger amount because its effect is not weighted by future layoff probabilities.

Therefore, in this model more generous UI can affect job duration through three distinct channels: first, it can increase duration by reducing quitting probabilities; second, it can also lengthen employment spells by providing incentives for workers to put effort into holding their jobs up to the moment in which they qualify for UI; and third, it can reduce the length of the employment spell by reducing the incentives for workers entitled to UI to keep their jobs. The overall effect of benefit level on average employment duration depends on the specific parametrization of the model, which determines which channels dominate. In the next section, this problem is addressed as an empirical question.

The role of informal job opportunities Since the empirical analysis below exploits data from a developing country where the informal labor market is large, it is worth discussing how labor informality may affect the interaction between UI and (formal) job duration. Abundant informal job opportunities increase the chances for UI beneficiaries to work informally while continuing to collect benefits. Hence, the more that workers are able to resort to informal rather than formal jobs, the more benefits that they should be able to collect. This implies that increasing the benefit level by $1 in an environment of high informality has a larger positive impact on the value of unemployment for UI eligible workers. Therefore, this means that the response of job duration to the benefit level should be stronger through all of the three channels described above.

For instance, increasing the benefit level more strongly diminishes the incentives for a worker to quit and give up potential UI in the presence of high labor informality. The reason is that on expectations, the worker who quits under high labor informality gives up more potential unemployment benefits because he is able to collect benefits for longer when having the chance to work informally. Since the three channels linking UI to job duration point in different directions, such a response under high informality may be stronger or weaker depending once again on the specific parametrization of the model.

2.2 The Reduced-Form Welfare Formula

I leave the complete solution to the worker's problem in each state of the model to the Appendix A.1 and A.2 and move on to the social planner's problem to derive the welfare formula. This subsection shows how UI effects on job duration matter for the optimal benefit level. Moreover, it reveals that it is enough to observe the worker's formal labor market outcomes to assess UI optimality. The social planner aims to maximize the agent's expected utility by choosing the level of unemployment benefits and a tax level τ levied on employed workers to finance the system. In principle, the profile of benefit level and duration could vary over time, although for a matter of simplicity I focus on "constant benefit, finite duration", as in Chetty (2008).[9] Therefore, here I assume that b_t is constant over time and that benefits last for a maximum of B periods.

The general social planner's problem is given by:

$$\max_{b,\tau} J_0^V(b,\tau) \tag{11}$$

$$s.t. \quad f^{UI} D_B b = D_E \tau \tag{12}$$

The goal of the social planner is to maximize J_0^V which defines the representative worker's expected utility, who is assumed to start the model employed. The constraint assures that the government budget is balanced. D_E describes the expected duration of the agent's (formal) employment at the beginning of the model. Only this duration matters for the government budget's revenue because, as previously stated, upon reemployment workers remain employed forever and no longer contribute to the system. D_B defines the expected time during which displaced individuals receive unemployment benefits, while f^{UI} is the fraction of workers receiving unemployment

[9] Chetty (2008) also remarks that most UI policies indeed provide constant benefits with a finite duration. This is also the case for Brazil, which is analyzed in the empirical section.

benefits. Such a fraction comprises those workers who are laid-off after meeting UI minimum eligibility requirements. Note that D_B is given by the average time that displaced workers take to find a new formal job, censored at the maximum UI duration. Thus, it also covers UI beneficiaries who are informally re-employed but continue to collect benefits. Therefore, the left-hand-side of the budget constraint in (12) denotes the expected cost of the policy, while the right-hand-side represents the expected amount received in taxes levied on employed workers.

At this point, it is possible to evaluate how a marginal change in the level of benefits impacts on welfare. In the same spirit as Chetty (2008), I assume that the consumption path during employment is constant since unemployment is unlikely to cause large losses in life cycle earnings. Furthermore, I assume that the worker's need for liquidity is independent of the period of displacement.[10] Together with the results from the agent's optimal choice of work and search effort, it is possible to derive the final welfare formula (see Appendix A.3 for details):

$$\frac{dW}{db} = f^{UI}\frac{D_B}{D_E}\left\{\rho - \left(\epsilon_{f^{UI},b} + \epsilon_{D_B,b} - \epsilon_{D_E,b}\right)\right\} \tag{13}$$

where $f^{UI} = \sum_{i=k}^{T-1}\left[\Pi_{j=0}^{i-1}(1-x_j)e_j\right](1-x_i)(1-e_i)$ is the share of laid-off workers eligible for UI due to MER and $\rho = -\frac{\frac{\partial s_0}{\partial A_0}|B}{\frac{\partial s_0}{\partial W_0}|B}$ is the liquidity-to-moral hazard ratio.

This formula shows the net welfare effect of increasing UI benefits by \$1 in comparison to the welfare gain from raising wages by \$1. In line with the traditional intuition from the Baily-Chetty framework, UI welfare effects are characterized by the trade-off between the benefits from providing liquidity

[10] Indeed, this is an implicit assumption in the Baily-Chetty framework, since the representative worker starts the model unemployed.

to unemployed workers and the efficiency costs from imposing higher taxes on employed workers to keep the budget balanced. The formula is derived in such a way that welfare benefits are captured by the liquidity-to-moral hazard ratio, as coined by Chetty (2008). Consumption smoothing benefits are captured by the ratio between UI liquidity effects on formal re-employment probabilities to the moral hazard effect on the same outcome. Note that this sufficient statistic does not depend on informal re-employment patterns. It is enough to observe workers' outcomes in the formal labor market to have a sufficient statistic assessing the extent to which UI benefits provide liquidity gains to unemployed workers. Such benefits are weighted by the fraction of unemployed workers actually eligible for UI, since those not meeting MER and job quitters do not receive UI benefits. This is an important result for the empirical section, which fully relies on data from the formal labor market.

The cost side of marginally raising the benefit level comes from the impact on the budget, which is to be balanced through higher distortionary taxes on employed workers. $\epsilon_{D_B,b}$ captures the behavioral response from higher benefits on the UI covered duration of non-formal employment, while $\epsilon_{D_E,b}$ captures the behavioral response from higher benefits on the duration of employment. Furthermore, there is also the behavioral response on the fraction of workers meeting MER, $\epsilon_{f^{UI},b}$. These last two terms capture exactly the distortionary effect of UI on job duration. Such margins affect welfare because they change the length of time during which individuals contribute to the system. If UI causes employment spells to be longer, the policy maker needs to impose less distortionary taxes on employed workers in order to finance the system. In other words, longer employment spells take the form of a positive externality to the government budget. By contrast, if UI leads to shorter employment spells, the government has to impose higher taxes on employed workers. This causes an additional welfare burden from increases in the benefit level. Whether the elasticity of employment duration to benefit level is positive or negative is an empirical question. The same idea extends to the UI effects on the share of workers qualifying to UI, which is also evaluated in the empirical section.

Finally, it is worth noting that the formula shows that $\epsilon_{D_E,b}$ affects welfare exactly with the same magnitude as $\epsilon_{D_B,b}$. Hence, it is enough to compare the size of these two elasticities to have an idea of their relative relevance for welfare.

3 Institutional Setup and Empirical Strategy

To recover the effect of benefit level on the duration of employment spells without the interference of confounding factors, I implement a regression kink design. The idea is to explore kinks on the policy rule, which conditions the benefit level on pre-displacement earnings in Brazil. Throughout this section, I introduce the main characteristics of the Brazilian UI system, explain the identification strategy and present the data.

3.1 UI and Institutional Setup in Brazil

The Brazilian unemployment insurance system is a federal program established in 1986. It offers temporary income for formal sector workers who are dismissed against their will and meet minimum eligibility requirements, namely: (i) having been employed in all of the last six months prior to the layoff; (ii) having no other source of income; and (iii) having not been granted UI benefits for the last sixteen months, counting from the date of the last layoff that enacted benefits. It is important to note that benefits are only granted to workers dismissed *without a just cause*, which is the most common type of dismissal in Brazil. Upon the payment of severance, by law employers are free to dismiss workers without a just cause.[11] Furthermore, even though dismissing *with a just cause* is less cost for employers, the conditions for this type of dismissal are very tight and it is very difficult to collect sufficient proof to back up cause.[12]

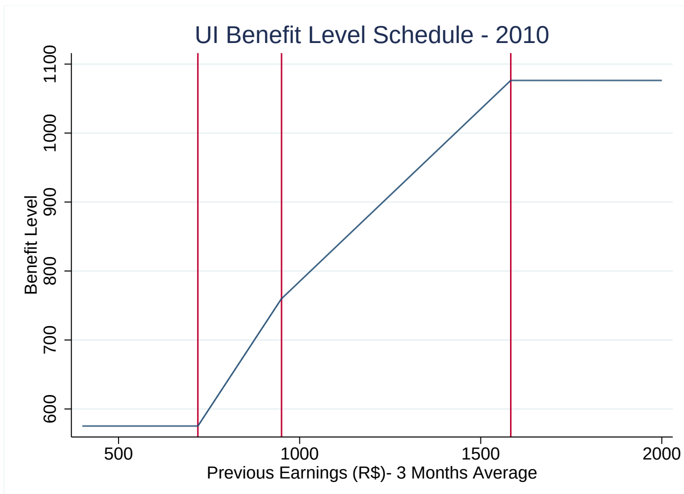
[11] Severance pay roughly equals 0.4 times the worker's monthly earnings per year of tenure.

[12] In general, workers can only be fired for cause if they: (a) are continuously absent from work (usually more than 30 days) ; (b) commit serious misconduct ; (c) go to work under

Furthermore, note that workers quitting their jobs are not entitled to benefits.

The benefit level is defined by a rule based on the three-month average earnings prior to the dismissal. Figure 1 presents the schedule with numbers from 2010 for illustrative purposes. The schedules for other years follow the same shape, although the kinks are located at different points. (See Appendix Figure 14).

Figure 1: Benefit Level Assignment Rule - Year 2010



Monthly benefits are a function of a reference wage, which is given by the average monthly earnings in the last three months prior to the dismissal. Up to a certain threshold, the benefit level equals 80% of the reference wage and is never below the minimum (monthly) wage. This feature gives rise to the first kink on the relationship between the reference wage and the benefit level. The second kink arises from the fact that after a given threshold, the marginal replacement ratio is lowered from 80% to 50%, thus generating the second kink point. Finally, there is a cap for the benefit level, which gives rise to the third kink point, as displayed in Figure 1.

These three non-linearities are the source of exogeneity, which is exploited to identify the effect of the benefit level on employment duration. The slope

the effect of alcohol; or (d) commit a large number of small infractions.

change of the policy rule relating the benefit level and the reference wage around the three points are 0.8, -0.3 and -0.5, respectively. From year to year, the shape of the relationship remains the same but all three kink points move. The schedule is increased yearly by the inflation rate in the previous year plus the average real growth rate of the economy in the two previous years. On average, these increased by roughly 8% per year in the period from 2005 to 2012. The time variation of the kink points is also exploited for identification as a robustness test. The details of this strategy are discussed in the results section.

Regarding the maximum duration of benefits, it is a function of the number of months worked in the last 36 months prior to the layoff. It starts from three months and is raised to four and five months for workers who have worked for more than 12 and 24 months in the reference period, respectively. Overall, the potential duration is roughly comparable to the US and shorter than in most European countries.

3.2 The Regression Kink Design

The idea of the regression kink design (RKD) is to exploit kinks in the relationship between an assignment variable and a treatment variable. These are the reference wage, based on previous earnings, and the level of unemployment benefits in this application, respectively. Such kinks are present in the relationship explained above and illustrated by Figure 1. The intuition of the strategy is that if the treatment variable has a causal effect on a given outcome variable, there should also be a kinked relationship between the outcome variable and the assignment variable. Therefore, in our context, if we expect benefit level to affect employment duration, there should also be a kinked relationship between employment duration and the reference wage at the same kink points marked in red in Figure 1.

The idea of this design is similar to a regression discontinuity design (RDD), although in this case there is not a discontinuity in the level of the assignment rule, but in its slope. The intuition of why it is able to identify causal effects of a policy derives from the fact that in the vicinity of the kink, subjects have the same pre-treatment characteristics, *on the margin*, but are assigned to different levels of treatment, *on the margin*.

The key assumption for the RKD is formalized by Card et al. (2015b) and requires that the density of the assignment variable is smooth conditional on observable characteristics around the kink point present in the policy assignment rule. As for RDDs, one crucial advantage for the credibility of this empirical strategy is that its key assumption has at least two testable implications: first, the empirical density function of the assignment variable must evolve smoothly around the threshold; and second, the conditional expectation function of any pre-determined characteristic is smooth around the threshold. In order to test and identify the presence of slope discontinuities in the data, I apply a local polynomial regression in the following parametric form:

$$Y_i = [\sum_{p=0}^{P} \gamma_p (w-k)^p + \beta_p (w-k)^p.D] \text{ where } |w-k| \leq h \tag{14}$$

where w is the reference wage centered around the kink point k, P is the polynomial order of the regression, h is the bandwidth used and D is a dummy variable taking the value 1 for $(w-k) \geq 0$. The estimate of interest is the slope change in the outcome variable at the kink point, which is identified by β_1. In the next section, I discuss in detail the choice of bandwidth and polynomial order.

3.3 Data

The data that I use in this paper comes from the *Relação Anual de Informaes Sociais* (RAIS). It is an administrative dataset covering all of the employment relationships in the Brazilian formal labor market. I have access to this data from 2005 to 2012. It contains detailed information on the characteristics of each labor contract such as the start and end date, type of labor contract, type of termination (layoff with or without cause, quit, etc), firm size at two different aggregation levels (branch and holding), municipality and industry; as well as information on workers, such as schooling, gender and average yearly earnings by each contract. Furthermore, it is possible to track workers and firms over time through an identification number.

4 The Effect of Benefit Level on Employment Duration

To assess the effect of benefit level on employment duration, I create three samples pooling data from all years around each of the yearly thresholds. I consider all permanent workers from the private sector who were employed on the first day when each yearly schedule is introduced. Since the schedule is updated again in the subsequent year, the duration of employment is constructed as the spell between the first day in which a yearly UI schedule is in place and the last day of the year. For instance, for the 2010 schedule, I consider all workers employed on the first day in which the schedule is valid, January 1^{st} (2010) in this case, and count for how long they were employed in the year, i.e. until December 31^{st}. In case a worker keeps his/her job until the last day of the year, the last day of work is set as December 31^{st}.[13] Summary statistics on the samples around each kink are displayed in

[13]However, note that from 2005 to 2009, the yearly schedule was respectively introduced on the first day of May, April, April, March and February. Therefore, in the years prior to these it would be possible to consider the duration until a date further than December 31^{st}. However, I decide to always use December 31^{st} as the last day of employment in the year due to the structure of the dataset, which is based on yearly mandatory information provided by the firms to the government authorities. This procedure avoids computing the duration of spells using data from two different years, which eliminates the risk of any possible endogeneity arising from the selection of firms that report the data for only one of the years.

appendix table B1.

A drawback from the dataset is that it provides only the worker's average monthly earnings for each year, while the assignment variable for the UI schedule is based on the average monthly earnings only in the three months prior to dismissal. Due to that limitation, I use the average monthly earnings in the year as the assignment variable for the RKD and need to expect that wages do not change too quickly within a given year. In case wage growth over the year is too steep, it is likely that the design would be compromised as the kink would be smoothed out and it would be difficult to identify any slope change in the data. Thus, such a possibility could only work in favor of the null hypothesis of no effect.

One further important condition for the success of the empirical design is that workers are informed with precision about the UI schedule. Information about the schedule is easily accessible on the internet. A simple google search for "value, unemployment insurance" leads to multiple benefit calculator websites. Further cheap information channels are unions and agencies of the Ministry of Labor. Moreover, Brazil is well known for its extremely high job turnover rate. First, this implies that a large number of workers have enjoyed unemployment benefits multiple times, making it more likely that they are informed about these incentives. Second, the high job turnover environment provides incentives for workers to learn about unemployment benefits, since layoff risk is generally not low. Therefore, while it would be difficult to believe that literally all employed workers know with precision the schedule, it seems reasonable to expect that most workers are informed about the UI schedule or can easily access this information when necessary. Moreover, even if a non-negligible number of workers are not informed, the empirical analysis would detect an effect of smaller magnitude than the actual effect. Such a case would resemble the situation of a fuzzy RDD, where the first stage is lower than one. This would imply that the following estimates represent a lower bound of the true effect of benefit level on job duration.

4.1 Density Smoothness

To evaluate whether the necessary conditions for the RKD hold, the density function of reference wages must evolve smoothly around the kink. Such condition could be violated if: (i) workers internalize UI benefits when deciding to enter their jobs, or (ii) workers are able to precisely manipulate their earnings. However, as shown by Card et al. (2015a) in a wage posting model, such threat is not justified if workers cannot perfectly predict the position of the kink or commit small optimization errors. Since the kink points change on a yearly basis at rate which depends on inflation and gdp growth, it is unlikely that workers can perfectly predict it. Moreover, the reference wage is based on earnings which are difficult to manipulate with precision. For example, workers cannot perfectly control the amount of extra hours they do, or take control over wage increases bargained at the union level.

To test this assumption, I extend the spirit of the McCrary (2008) density discontinuity test for RDD to check for the presence of a slope change in the density of the assignment variable. I create bins over the assignment variable and count the number of observations in each bin. Then, I run a regression as in equation (14) on the number of observations allowing for a slope change at the kink to test for the smoothness condition. I set the polynomial order of this regression to minimize the Akaike Criterion.[14] Figures 2 and 3 display the density of average monthly earnings and the manipulation test result for the first and second kink. The same results for kink 3 are reported in the Appendix Figures B15.[15] From visual inspection, the density function seems to move quite smoothly around all kink points and there does not seem to be any evidence of slope changes or strategic bunching. This impression is supported by the first-derivative tests reported in each graph, which do not allow one to reject the null hypothesis of smoothness in the density functions.

[14]This is in line with the procedure adopted by Card et al. (2015b)

[15] 2005 and 2006 are dropped from the analysis because there is mild evidence that results on employment duration may be driven by covariates. To avoid concerns that these years might drive the results in the full sample, I decide to drop these years from the analysis.

4.2 Graphical Evidence

A first and key piece of graphical evidence on how employment duration is affected by the benefit level comes from observing how this variable evolves over earnings within a (UI schedule) year. Figures 4-5 display this evidence for 2007 and 2012. Even though yearly data is relatively noisy, job duration seems to be relatively flat between the minimum wage and the first kink, whereby it starts increasing with earnings exactly at the first kink. Since the potential benefit level increases on the margin at this point, this graphical evidence suggests that benefit level is causing employment duration to increase. It is also worth highlighting that kink points changed in real terms by 45% between 2007 and 2012, suggesting that this is not a coincidence driven by one specific year. Regarding the second kink, duration seems to become slightly less steep right after the threshold for both years, suggesting again a positive effect of benefit on duration since the potential benefit level marginally decreases at that point. Around the third kink, the graph is somewhat less noisy and duration seems to evolve smoothly across the threshold, thus suggesting no effect.

At this point, I pool the data using all years from 2007 to 2012 around each of the thresholds. Such a procedure helps to reduce the noise in the data. These graphs for the first and second kink are displayed in Figures 6 and 7. The same pattern emerges as observed for 2007 and 2012: employment duration is apparently flat before the first kink and starts to increase just after it, while duration apparently increases less steeply in the vicinity after the second kink. Overall, these pieces of graphical evidence suggest that benefit level is causing job duration to increase around the first and second kink point. Nonetheless, it is important to note that these graphs are affected by noisy patterns to a fair extent and the evidence is somewhat less clear around the second kink. This stresses the need for a careful regression analysis, which is conducted in the next subsection.

To investigate whether any of these results are driven by kinks in pre-determined covariates, I build a linear prediction of job duration based on a rich set of pre-determined characteristics of workers and firms: age at hiring date, job tenure at the date of the yearly schedule introduction, employment duration at previous job, decile of firm size and previous firm size at two different aggregation levels, as well as dummies for whether the worker was recalled for this job and whether he/she was still facing the waiting period for UI due to prior benefit claim (16 month minimum period between UI claims), termination type at previous job, race, gender, weekly hours of work, years of schooling, industry and federal state, all interacted with year dummies. Note that the dataset allows me to recall relevant workers' previous job characteristics, which are likely to be very informative regarding their current labor market outcomes. Figures 8 and 9 show how this best linear prediction of job duration evolves around the first two kinks. These graphs do not seem to display any slope change around the threshold. If anything, predicted values around the second kink slightly bend upwards, which goes in the opposite direction of the effect suggested by the graphical analysis on actual job duration. Therefore, if anything, covariates lead to an underestimation of the results around the second kink. In any case, it is important to note that these predicted values in both graphs display incredibly less variation for the same range of the running variable with respect to actual job duration. This suggests that even if pre-determined covariates are driving part of the results on job duration, any potential bias is likely to be very small.

To gain further insights into the role of covariates, it is also useful to observe how pre-determined covariates themselves evolve around the threshold. For the sake of conciseness, these are shown in the Appendix Figures B10 and B14. As for the predicted job duration, these variables seem to display no slope change around any of the kink points.

Overall, I interpret these results as evidence that UI causes job duration to increase around the first and second kink and that imbalances in predetermined covariates cannot explain this effect. In order to gain some intuition on the potential drivers of this evidence on job duration, it is useful to analyze how the profile of quitting and firing probabilities, as well as the probability that a worker reaches MER evolve over monthly earnings. The evidence on quits around the first kink presented in Figures 10-11 is striking: in both years, the probability of job quits displays a clear negative slope change just at the threshold. This strongly suggests that UI benefit level causes job quits to decrease, which is perfectly coherent with the graphical impression that benefit level causes longer employment duration. Regarding the evidence around the other thresholds, quitting rate seems to decrease less steeply as earnings increase, which is coherent with a negative effect of benefit level on quits. However, this seems to be a relatively smooth process over the earnings profile. The pooled graphs displayed in Appendix Figures B7, B11 and B17 lead to the same impressions.

In terms of the probability of layoff and the probability that a worker remains in the job until MER (6 months), the evidence displayed in Appendix Figures B3-B6 is much less clear. Layoff probabilities seem to evolve relatively smoothly at all points. The share of workers reaching MER seems to display a mild positive slope change around kink 1 in 2007, while the pattern is less clear in 2012.

Put briefly, I interpret these results as suggestive evidence that higher benefit level causes job spells to last longer around the first and second threshold. Furthermore, this seems to be mainly driven by a strong negative effect on job quit decisions. Careful analysis of these impressions is carried out in the following subsections.

4.3 Bandwidths Choice

A key issue in RKD applications is the choice of the bandwidth and polynomial order, which essentially trades off bias and precision. The only bandwidth selector explicitly designed for the RKD is proposed by Calonico et al. (2014) -CCT-, where they develop a selector based on optimal mean square error and propose a bias robust confidence interval for RDDs in general. As suggested by Card et al. (2015a) -CLPW-, I consider this selector with and without its regularization term and also consider a rule of thumb based on Fan and Gijbels (1996) -FG.[16] A similar issue emerges to the one faced by CLPW arises: the results are not consistent across different bandwidth selectors, as reported in Appendix Table B8. For example, while all linear and quadratic CCT specifications in the first kink indicate positive and statistically significant elasticities[17], the results based on the FG selector point in the opposite direction and are nevertheless statistically significant. Even though one could argue that in theory, FG is a "sub-optimal" alternative and thus CCT estimates should be preferred, the same occurs in the second kink across CCT specifications. While linear CCT specifications suggest a negative effect, quadratic ones point otherwise.

Due to these inconsistencies across estimates, I adopt the same approach suggested by CPLW in order to choose a preferred specification (selector and polynomial order). The idea is to run Monte Carlo simulations with a data generation process that approximates the actual data around each kink, and evaluate the performance of each selector in estimating a given imposed kink value. Then, by empirically assessing the root mean square error (RMSE) through repeated simulations, it is possible to evaluate which specification performs better on average at estimating slope changes around each kink.

Based on these simulations, I show that all the proposed bandwidth selectors perform very poorly around the first and second kink. Root mean square errors arising from the simulations are rarely lower than 100% compared to the imposed kink value, which is set to deliver an elasticity of employment duration equal to 0.5. Moreover, further results show that estimates in the bandwidth range picked by these selectors yield high rates of

[16] The original selector contains a regularization term to avoid bandwidths that are "too large", as noted in Calonico et al. (2014). To implement the two CCT bandwidths, I use the CLPW adaptation of the software developed by Calonico et al. (2014), which optimizes computational time in large samples.

[17] For the CCT robust C.I. at the 10% level.

false positives in placebo points. As an alternative to these selectors, I use the same simulation procedure to test the performance of a range of fix bandwidths around each threshold. I show that specific fix bandwidths perform better in this data compared to the selectors described above. For example, these simulations show that using a fix bandwidth of R$75 (of monthly earnings) in the first kink yields a RMSE that is roughly 82% smaller than the one arising from a linear CCT specification (0.25 vs 1.39 as a proportion of the imposed kink value). The only exception are the results around the third kink, which indicate that a quadratic specification with the CCT bandwidth selector, without the regularization term, is the best performing choice.

I leave a detailed discussion on these procedures to Appendix B.3 and proceed to the results, which are based on the best performing specifications based on the Monte Carlo study, around each kink, for each variable. In the results, I also report 95% critical values based on permutation tests and use this as the main hypothesis test for the presence of kink points in the analysis below, as suggested by Ganong and Jäger (2016). They show that while standard local linear estimates severely suffer from type I error, the incidence of such errors is much closer to nominal in permutation tests. To further ensure the robustness of these results, I present estimates for a range of bandwidth choices around the preferred ones.

4.4 Estimation Results

Table 1 presents regression results together with permutation test critical values. These estimates confirm the graphical impression for the first two kinks and point to a positive elasticity of 0.32 and 0.37 respectively. Such coefficients are highly significant, and most importantly, they are significantly above permutation test critical values, which suggests that they are not spurious estimates arising from a smooth curvature in the relationship between employment duration and earnings. Since these thresholds are located relatively close to each other, it is also reassuring to note that they point to very similar elasticities. This again indicates that these are not spurious results arising from a global non-linear pattern in the data, especially considering that the policy kinks around these points have a different sign and magnitude (0.5 vs. -0.3). On the other hand, the results on the linear prediction of duration around the same points, despite being statistically significant, have much smaller magnitude, which indicates that variations in the rich set of covariates cannot explain the effect of benefit level on employment duration. While it points in the same direction of the effect at the first kink, calling for a somewhat more careful interpretation of the result, around the second kink it points in the opposite direction. Taking these together and observing that the estimated effect is stable around these close thresholds indicates that this should not be a cause for much concern. Regarding the third kink, the estimated elasticity is negative and statistically significant but lies within the range of critical values based on permutation tests. Observing the rather large range of these critical values ([-3.36,2.6]) leads to the conclusion that it would be difficult to identify any elasticity of reasonable size around this threshold.

Estimates on the possible channels driving this effect are displayed in Table 2. They point to a strong negative effect on quitting probabilities at the first and second kink, which is robust to permutation tests. These results indicate that a 1% higher benefit level reduces the unconditional probability that workers quit their jobs by 1.21% and 1.73% at these thresholds. Aside from the probability that the worker reaches MER at the second kink, it is not possible to reject the null for any other result. The estimates suggest that a 1% higher benefit increases this probability by 0.19% at kink 2. It is also worth noting that the range of permutation tests critical values for layoff probabilities around kink 2 is large. This suggests that it would be difficult to identify effects of reasonable size for this outcome at this threshold.

4.5 Robustness

Bandwidth Choice In order to evaluate whether positive elasticities at the first and second kink are driven by specific bandwidth choices, Figures 12 and 13 display regression results and t-statistics for a range of bandwidths, together with permutation tests critical values. In both cases, the estimates are statistically significant and exceed permutation tests critical values for a range of bandwidths. More importantly, they display some stability, which suggests that bias does not play a major role within this range (Ruppert, 1997). Note that when there are fewer than sufficient placebo estimates to draw 95% critical values, gray lines display the largest and smallest placebo estimate results.[18] In terms of the channel driving such an effect, Figures B20 and B22 display the same estimates on quitting probabilities at the first and second kink, while Figure B23 shows the results on MER at the second kink. Overall, they once again support the results based on the preferred bandwidths in Table 1.

RKD in double and triple difference Despite the evidence provided above, there could still be a concern that these results capture a simple pre-determined kinked or quadratic relationship between job duration and average earnings. To address this issue and test for the robustness of the estimates presented above, I apply a RKD in double differences (Landais, 2014), which explores the fact that each kink point changes every year in real terms. The idea is to compare the estimated slope change at the actual policy kink to the slope change estimate at the same point in previous years, when no actual policy kink was in place. Therefore, if the results from above are simply picking up a quadratic relationship between job duration and earnings, this strategy in double differences should point towards a null result. I use a three year lag to implement this procedure and I have to drop the year 2011 because there is too little variation between kink points for 2011-2008.[19] The final sample

[18] As the bandwidth increases, the number of possible placebo points becomes smaller because the data used in each placebo estimate should not overlap.

[19] Implementing this procedure requires a minimum interval of time between the treatment and placebo year to ensure sufficient distance between the policy kink in the treatment and

contains 2010 and 2012 as treatment years, as well as 2007 and 2009 as control years.

Finally, I propose a further extension of this test to assess whether the differences in slopes on job duration detected in the DD-RKD are indeed caused by the UI benefit schedule. I repeat the same procedure described above, but now adding a fake kink point to the analysis. This point is not an actual policy kink in the "treatment" or the "control" year. Thus, I perform a RKD in triple differences: it assesses the differences in slope changes between "treatment" and "control" year at an actual kink point, *vis-a-vis*, the differences in slope changes between the "treatment" and "control" year at a fake kink point, in which no policy kink was ever in place. The idea is to control for the fact that the overall curvature of the relationship between job duration and earnings may be changing over the years. In other words, it aims to control for the possibility that the differences in slope changes with three years distance are spurious. This fake point is set as close as possible to the actual kink point, albeit considering the constraint that some minimal bandwidth is required to run these regressions. The results for the RKD in double and triple differences are presented in Table 3. Estimates from both specifications at the first and second kink are similar in magnitude but larger and less precise compared to those found in the standard local linear regression.[20] This indicates that any suspicion that the latter may be driven by a bandwidth bias is not supported by the data. If anything, it suggests that bandwidth bias may be driving estimates downwards. Regarding the results from the same exercise on predicted job duration, they once again point to an effect of small magnitude. More precisely, the effect on this variable at the first kink almost vanishes and become statistically insignificant in triple differences, while at the second kink it is positive yet

placebo year; otherwise, bandwidths around the kink point used in the DD-RKD would be constrained by the vicinity, in real terms, to the actual kink point in the placebo year. This would lead for regression bandwidths that are too small and the results would likely pick noise.

[20] Aside from the result in triple differences at the second kink, which shows a similar effect to the one from the standard local linear regression.

small. Once again, taken together, the covariates seem unable to explain the effect of employment duration.

Employment duration over multiple spells Since the positive effect on duration is explained by a decrease in job quits, one could be suspicious that higher benefit level prevents only job-to-job quits and does not affect the total time that workers spend employed. Such an idea would be consistent with Light and Omori (2004), who argue that more generous UI leads workers to search less on the job when faced with the expectation of a layoff. If this is the mechanism in place, the theory proposed in section two would be jeopardized because UI benefits would not actually cause workers to contribute to the system for a longer period. The conclusion that a positive effect of the benefit level on job duration calls for more generous benefits is based exactly on the worker's behavioral response, which affects the government budget constraint.

Hence, in order to test whether a higher benefit level actually leads to workers remaining formally employed for longer, I repeat the exercise from above measuring duration as the total time that the worker spends employed over multiple spells. Thus, if the worker changes job within a UI schedule year, the time spent employed in the new job is also counted. If it is the case that UI simply prevents job-to-job quits, we would find zero effect on this measure around the thresholds. Table 4 presents these results. Point estimates on the elasticity of employment duration to benefit level barely display any change, thus supporting the hypothesis that higher benefit level actually causes workers to spend more time employed. This makes the hypothesis that the results are purely driven by job-to-job quitting behavior extremely unlikely.

4.6 High vs. low labor informality areas

As discussed in the theoretical section, the presence of informal job opportunities could affect the response of employment duration to the benefit level. In particular, it could strengthen one or more of the channels driving these responses. In order to gain some insights into the role informal jobs, I investigate how the results from above vary between areas of high and low labor informality. More specifically, I use data from the 2010 Brazilian Census to create a simply measure of informality that indicates the proportion of informal jobs among all existing jobs in each of the 5,565 Brazilian municipalities. I use this informality measure to split the sample in two, above and below the median, and subsequently estimate the effect of benefit level on duration in each subsample. Such an exercise takes advantage of the large heterogeneity across Brazilian municipalities. While in the low informality sample the median informality index equals 31.4%, in the high informality sample 67.8% of all jobs in the median municipality are informal. The results displayed in Table 5 show that employment duration is actually more responsive to benefit level in areas of low informality. Even though it is obviously not possible to directly attribute these differences to variations in informality, it suggests that such responses are not a special case of areas dominated by informal jobs.

5 Implications for Welfare

In the theoretical section, it is shown that the effect of the benefit level on employment duration directly affects the optimal benefit level. In particular, the elasticity of employment duration to benefit level weighs on welfare one to one with respect to the well-known elasticity of unemployment duration. Hence, one exercise to gain a sense on the economic relevance of the results from above is to compare them with the range of estimates of the same effect on unemployment duration found in the literature. The estimates in the previous section point to an elasticity of 0.32 and 0.37 at the first and second kink, respectively. These are clearly in the same range of the estimates on the effect of the benefit level on unemployment duration. Card et al. (2015a) provides a survey of both old and recent studies in Europe and the US, showing this elasticity to lie within the range (0.2,0.8).

Nonetheless, a reason for concern is that existing studies are based on developed countries and thus responses to search effort might possibly be different in the context of a developing country. However, Gerard and Gonzaga (2014) argues that in such a context this response is actually likely to be smaller exactly due to high labor informality. The reason is that in such contexts most unemployed workers do not come back to the formal labor market regardless, whereby their margin to react to UI incentives is actually small. Indeed, using Brazilian data, they show that behavioral distortions caused by UI extensions are rather small compared to those previously found for the US. Hence, this simple comparison indicates that UI effects on employment duration can be quantitatively relevant for welfare. It is worth noting that around the second kink, the results suggest that the fraction of workers qualifying for UI increases by 0.19% as a response to a 1% increase in benefit level. This effect counterbalances part of the effect on duration, as shown by the welfare formula in (13). Thus, around this threshold the impact of behavioral reactions of employed workers to be compared to the response of unemployment duration is 0.37 - 0.19 = 0.18. Despite being smaller, this remains a non-negligible effect in comparison to the range of elasticities of unemployment duration.

At this point, I perform a simple calibration exercise based on the welfare formula in (13). For this purpose, I estimate the remaining statistics from the formula and recall the average duration of benefits from Gerard and Gonzaga (2014), based on the registry of UI payments in Brazil. The liquidity-to-moral hazard ratio is set to 0.98, as estimated by Britto (2015) using the same data. Since estimates on $\epsilon_{D_B,b}$ are not available for Brazilian data, I set this value in the middle of the range recalled from the literature (0.5), as mentioned above. The results from this exercise are presented in Table 6 and suggest that raising the benefit level by \$1 yields a welfare gain of \$0.12 and \$0.09 around the first and second kink, respectively. Of course, $\epsilon_{D_B,b}$ is one critical missing value that is not estimated. Nevertheless, relaxing the assumption on this measure, it is possible to back-out that marginally increasing the benefit level yields a positive welfare effect as long as $\epsilon_{D_B,b}$ is lower than 1.3 and 1.16 around the first and second kink, respectively. Considering that these values are fairly large compared to those presented in the literature, this prompts the conclusion that the benefit level is most likely below optimality.

In any case, the main message from this simple exercise is that UI effects on the duration of employment can play a relevant role for welfare. The estimated effect lies within the range of the literature estimates on the elasticity of unemployment duration to benefit level (0.32/0.37 vs. [0.2,0.8]) and impacts welfare with the same weight. This strongly suggests that, at least in some context, policy makers should be aware of such an effect and take it into account in order to optimally set the level of unemployment benefits.

6 Conclusion

The main conclusion from this essay is that UI can affect the duration of employment spells in an economically meaningful way. Perhaps surprisingly, this effect is found to be positive in this data and driven by a strong negative effect on quitting rates. Such a finding is of direct interest to policy makers when evaluating the social costs and benefits of UI. Moreover, the results from above may be particularly relevant for the nascent literature of UI in developing countries and strengthen the case for UI provision in such contexts. They complement the findings of Gerard and Gonzaga (2014) who argues that despite the skepticism in the public policy debate, UI provision in developing countries may yield large welfare gains.

Whether similar findings extend to the context of developed countries is left as an open question. Nevertheless, it is worth noting that the empirical strategy proposed in this paper could be applied elsewhere since many UI systems follow a similar schedule type for benefit level. This could be an interesting direction for future research.

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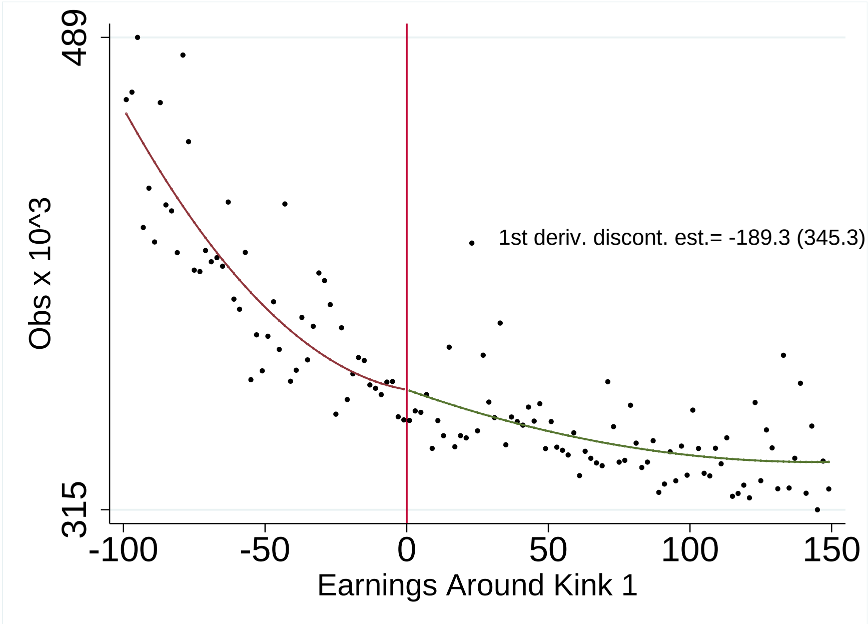
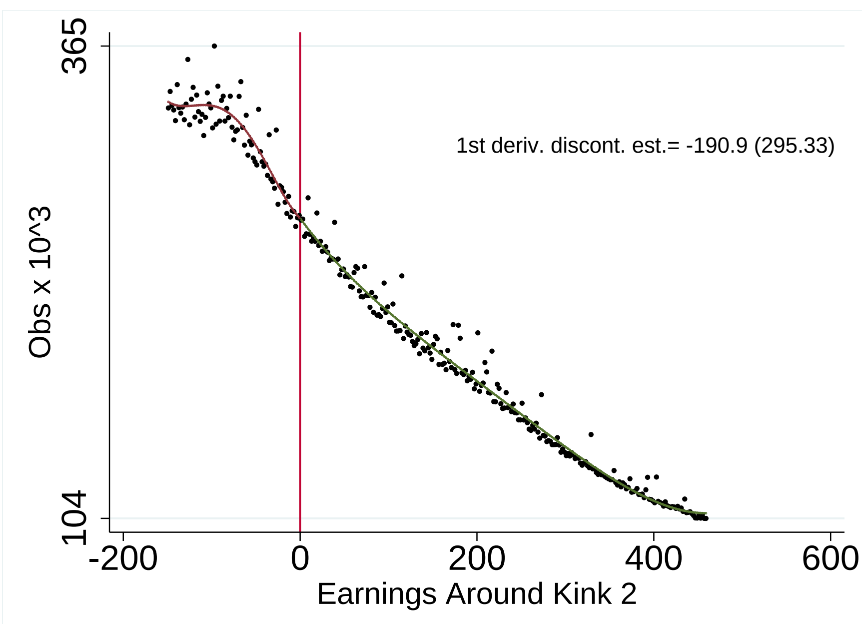
Figure 2: Density of Earnings Around Kink 1



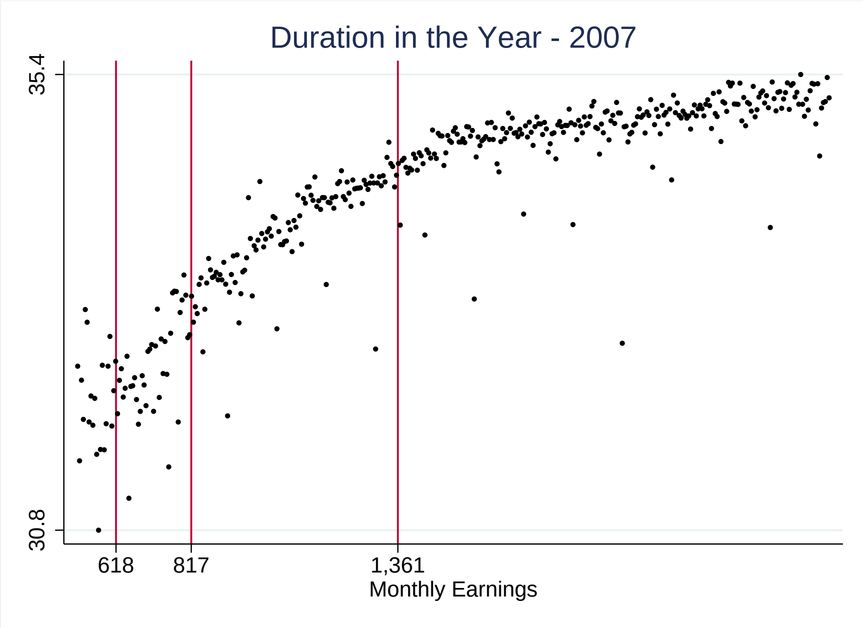
At each side of the kink, the density is approximated by the polynomial which minimizes the Akaike Criterion. The graph also displays the test statistics for the slope change of these polynomials at the kink. The sample is composed of data from all years from 2007 to 2012. See the text for details.

Figure 3: Density of Earnings Around Kink 2



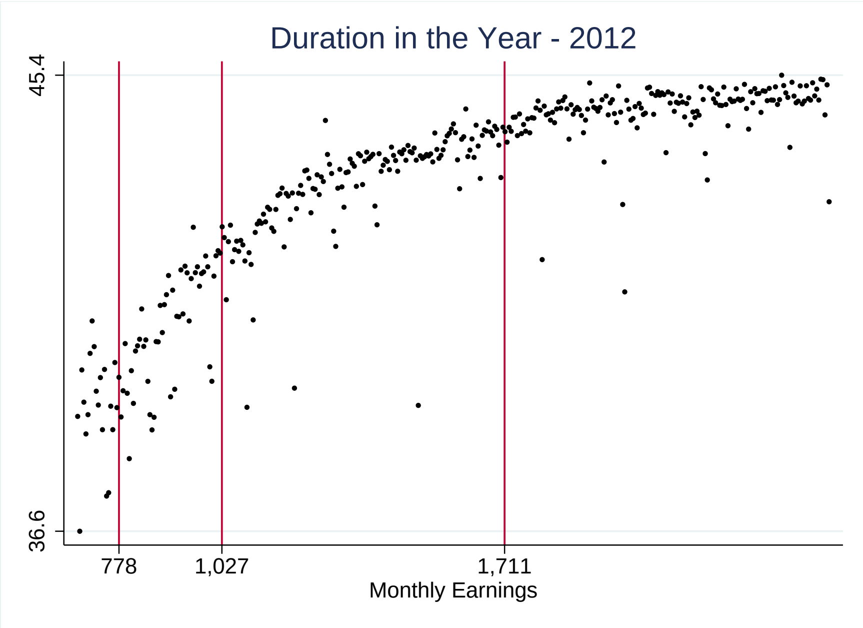
At each side of the kink, the density is approximated by the polynomial which minimizes the Akaike Criterion. The graph also displays the test statistics for the slope change of these polynomials at the kink. The sample is composed of data from all years from 2007 to 2012. See the text for details.

Figure 4: Employment Duration



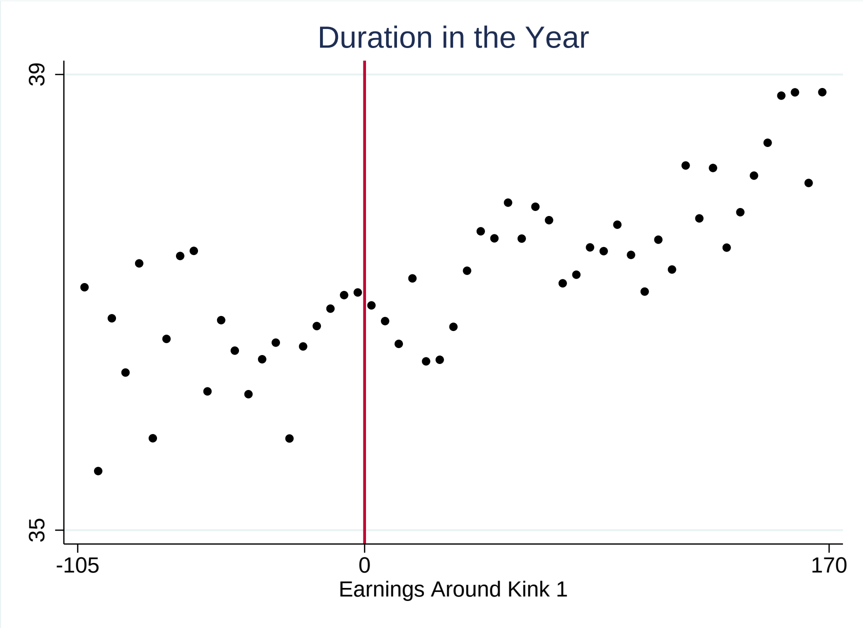
The graph displays how employment duration in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks.

Figure 5: Employment Duration



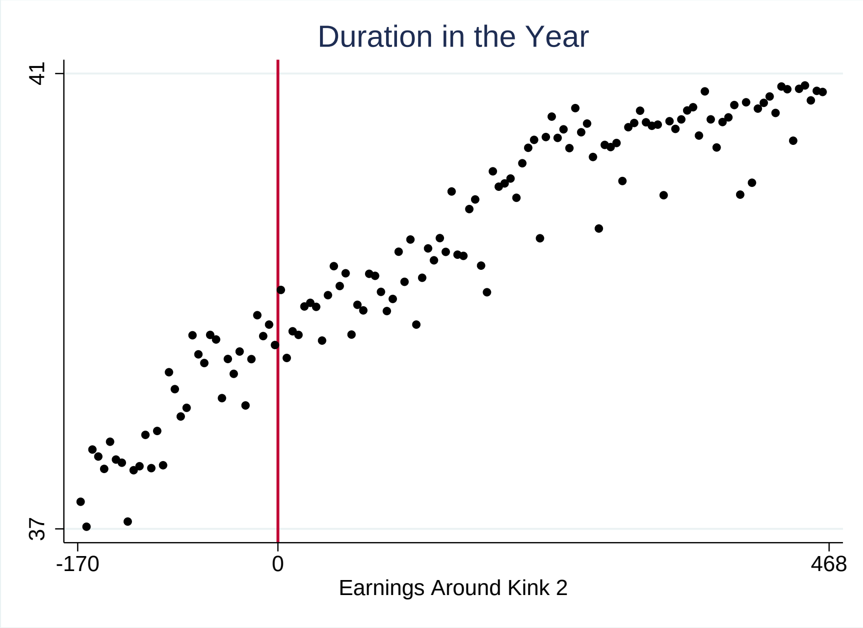
The graph displays how employment duration in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks.

Figure 6: Employment Duration Around Kink 1



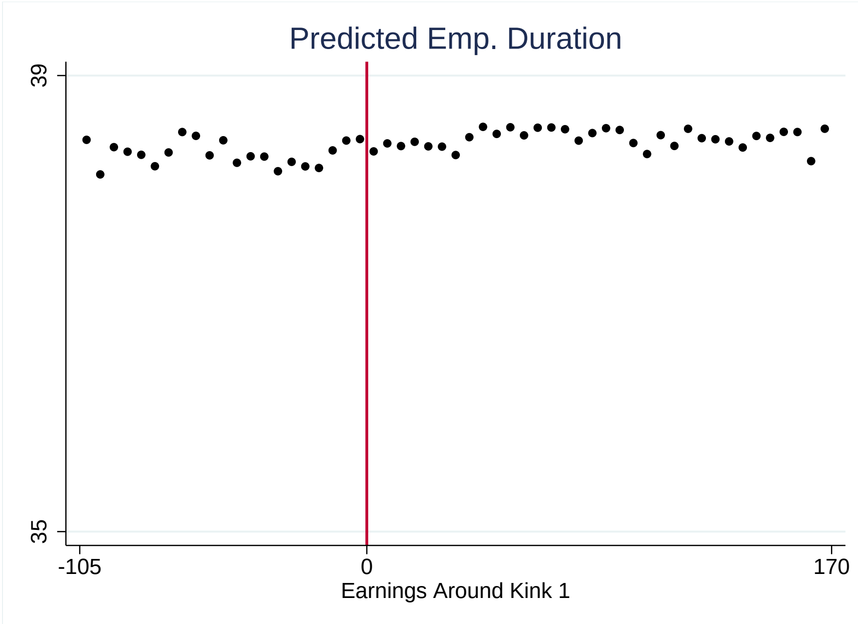
The graph displays how employment duration in the year evolves around the kink. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks.

Figure 7: Employment Duration Around Kink 2



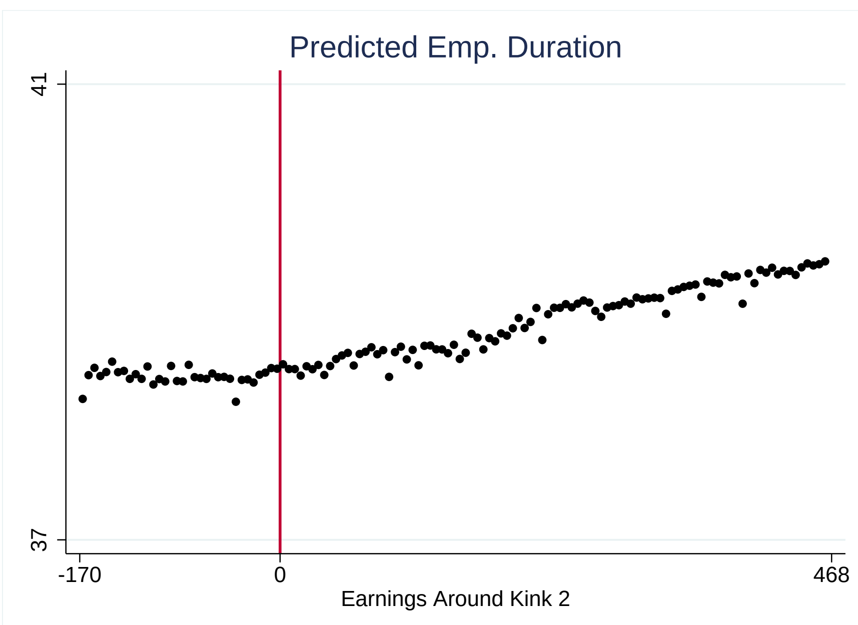
The graph displays how employment duration in the year evolve around the kink. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks

Figure 8: Predicted Employment Duration Around Kink 1



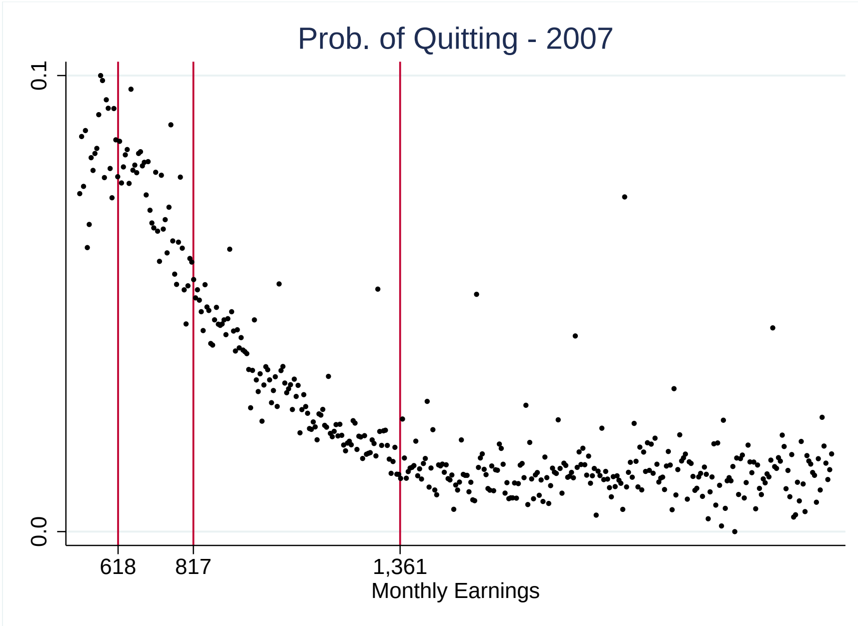
The graph displays how predicted employment duration in the year evolves around the kink. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks. See text for details on the construction of this predicted variabel.

Figure 9: Predicted Employment Duration Around Kink 2



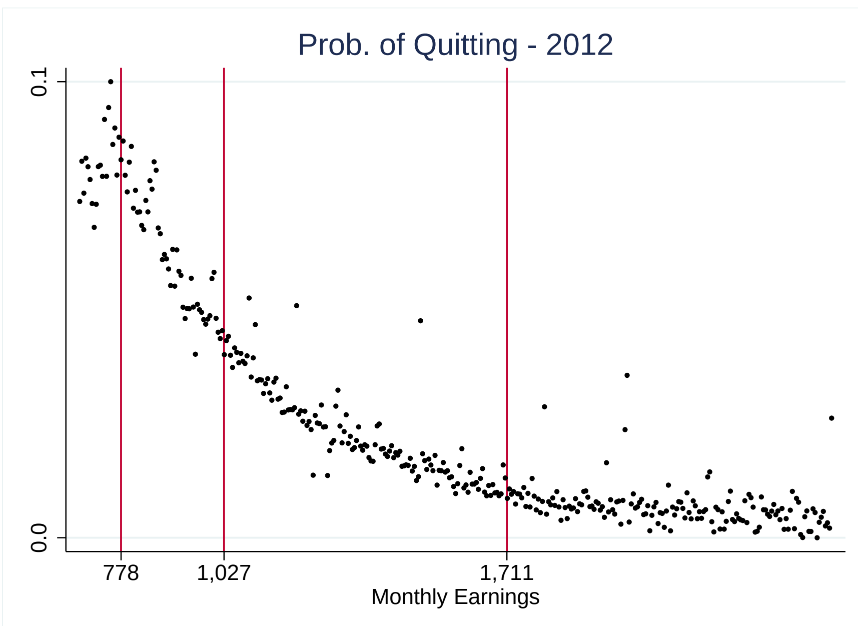
The graph displays how predicted employment duration in the year evolves around the kink. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks. See text for details on the construction of this predicted variabel.

Figure 10: Job Quits



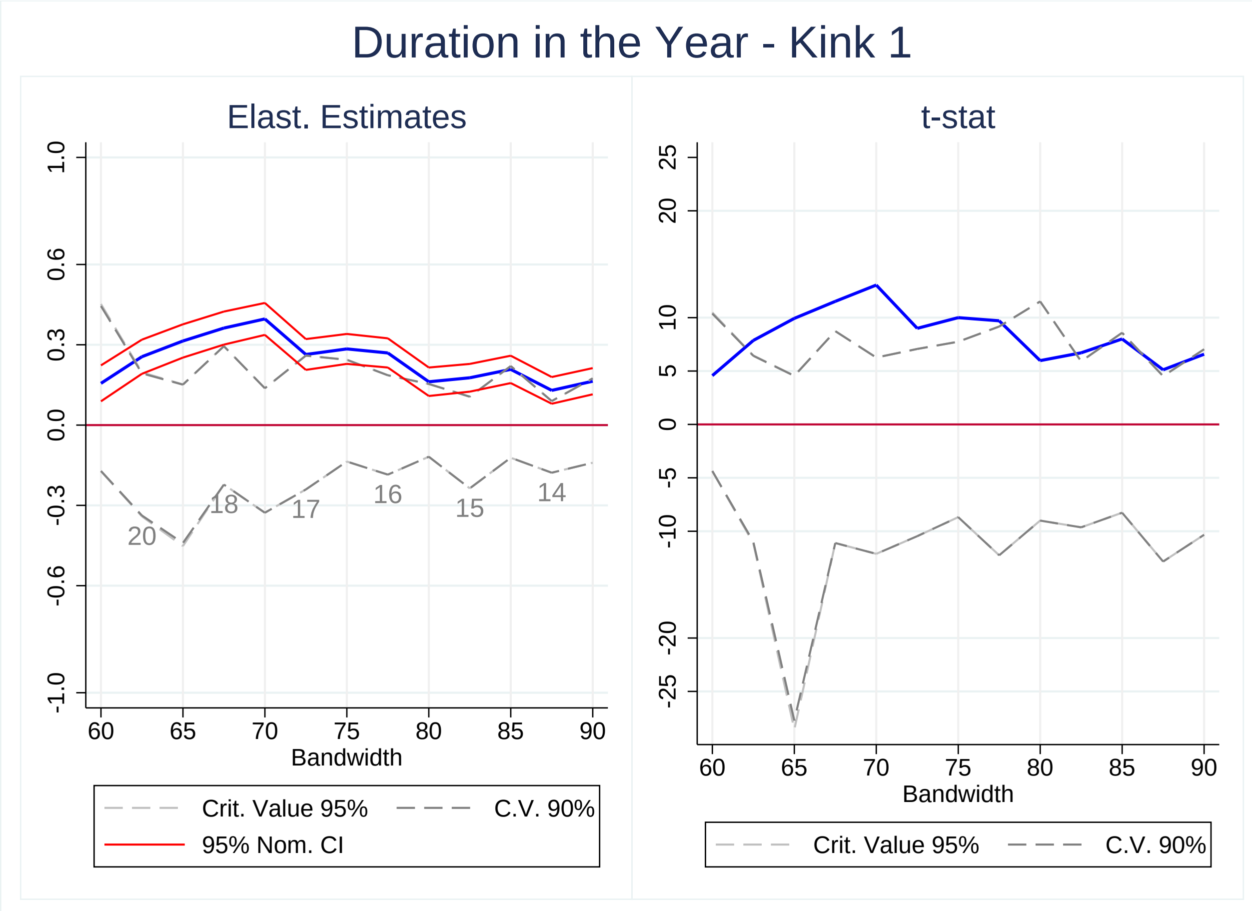
The graph displays how the prob. of quitting in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks.

Figure 11: Job Quits



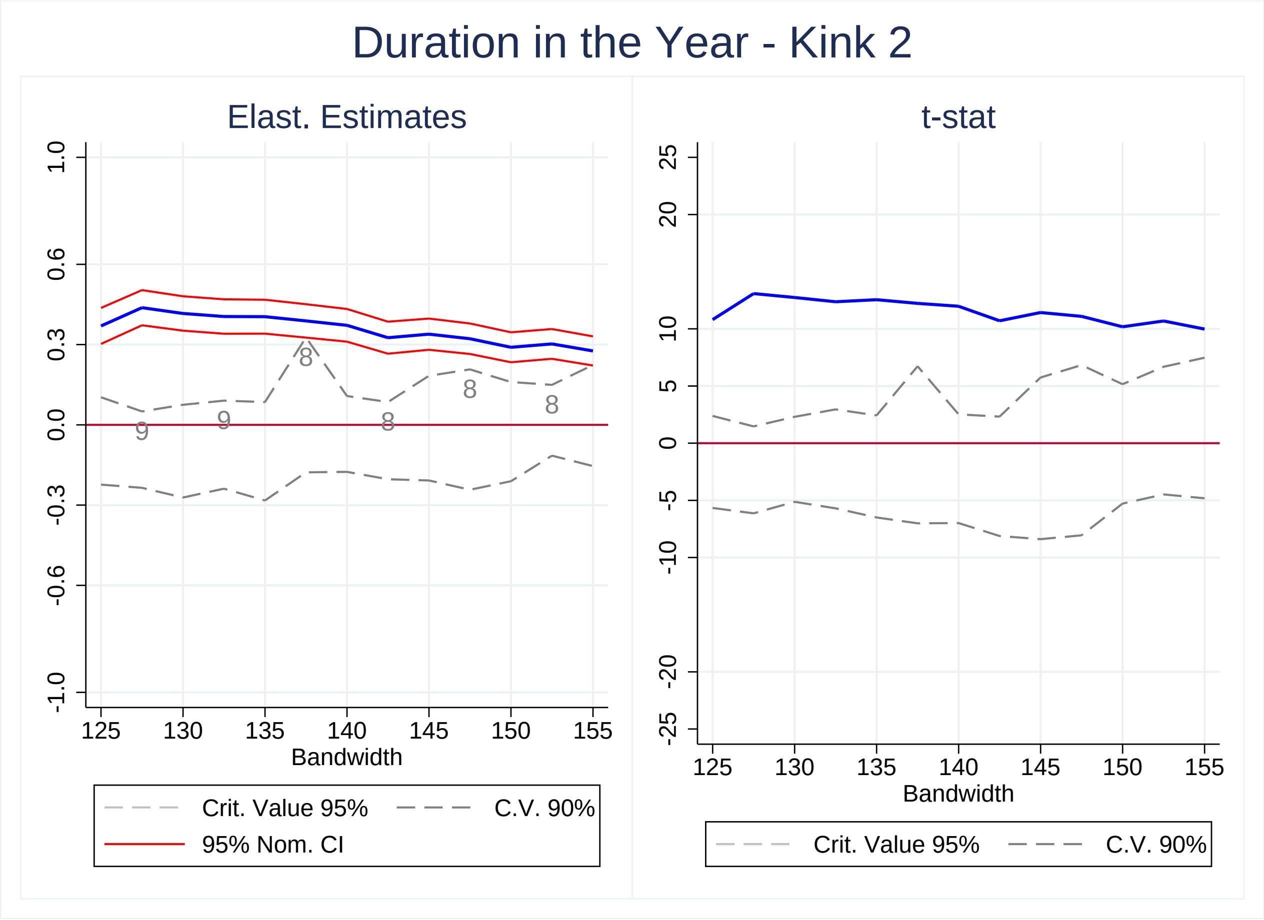
The graph displays how the prob. of quitting in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks.

Figure 12: RKD estimates with varying bandwidths and Permutation Test Critical Values - Kink 1



The graph displays elasticities estimates and t-statistics based on a local linear regressions around kink 1 for varying bandwidths (blue line), and nominal confidence intervals for these estimates (red line). The gray lines display the critical value in a two-sided test for rejecting the null hypothesis of zero effect, based on permutation tests along as many as possible placebo points located between the minimum wage and R$4000 (2012 prices). The gray number displays the number of placebo points for this test, according to the LLR bandwidth. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks. Standard errors are clustered at the firm level.

Figure 13: RKD estimates with varying bandwidths and Permutation Test Critical Values - Kink 2



The graph displays elasticities estimates and t-statistics based on a local linear regressions around kink 1 for varying bandwidths (blue line), and nominal confidence intervals for these estimates (red line). The gray lines display the critical value in a two-sided test for rejecting the null hypothesis of zero effect, based on permutation tests along as many as possible placebo points located between the minimum wage and R$4000 (2012 prices). The gray number displays the number of placebo points for this test, according to the LLR bandwidth. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks. Standard errors are clustered at the firm level.

Table 1: RKD Estimates - Elasticity of Employment Duration to Benefit Level

	Elast.	s.e.	t-stat	Perm. Test Crit. Values [min,max] coef.	Perm. Test Crit. Values [min,max] t-stat	Bandwidth
First kink						
Employment Duration	0.32	(.03)	11.6	[-0.14,0.17]	[-10.71,6.53]	75
Predicted Emp. Dur.	0.078	(.01)	8.0	[-0.05,0.03]	[-3.95,4.2]	75
Second kink						
Employment Duration	0.37	(.03)	12.0	[-0.17,0.1]	[-6.98,2.52]	140
Predicted Emp. Dur.	-0.10	(.01)	- 9.7	[-0.02,0.05]	[-2.83,3.04]	140
Third kink						
Employment Duration	-1.04	(.14)	- 7.6	[-3.36,2.6]	[-10.24,8.48]	62
Predicted Emp. Dur.	-0.05	(.01)	- 3.5	[-0.21,0.12]	[-3.15,4.41]	128

Note: The table displays RKD elasticities estimates based on local polynomial regressions as in equation 14. For each kink, based on the Monte Carlo study, preferred choices of polynomial degree and bandwidth selector are used. For the first and second kink, the preferred choice is a fixed bandwidth with a linear specification. For the third kink, the CCT bandwidth selector with quadratic polynomial is applied. The sample is composed of data from all years from 2007 to 2012. For each regression, permutation test 95% critical values are derived from a number of RKD estimates on placebo points where no policy kink is in place. Standard errors are clustered at the firm level.

Table 2: RKD Estimates - Benefit Level Effects on Quits, Layoffs and UI Entitlement

	Elast.	s.e.	t-stat	Perm. Test CV [min,max] Elast.	Perm. Test CV [min,max] t-stat	Bandwidth
First kink						
Prob. of Quitting	-1.21	(.09)	- 13.2	[-0.46,0.36]	[-6.67,5.56]	100
Prob. of Layoff	0.88	(.13)	6.8	[-1.16,1.04]	[-7.12,10.03]	45
Prob. Reach MER	-0.02	(.01)	- 2.6	[-0.04,0.07]	[-7.71,10.27]	80
Second kink						
Prob. of Quitting	-1.73	(.19)	- 9.2	[-0.8,1.15]	[-1.64,3.33]	110
Prob. of Layoff	-1.57	(.52)	- 3.0	[-9.73,9.04]	[-8.55,7.79]	30
Prob. Reach MER	0.19	(.02)	12.0	[-0.07,0.05]	[-9.42,3.13]	140

Note: The table displays RKD elasticities estimates based on local polynomial regressions as in equation 14. For each kink and outcome, preferred bandwidths based on Monte Carlo studies with a linear specification are used (see Appendix B.3 for details). The sample is composed of data from all years from 2007 to 2012. For a given bandwidth, permutation test 95% critical values are derived from a number of RKD estimates on placebo points where no policy kink is in place. Standard errors are clustered at the firm level.

Table 3: RKD in Double and Triple Differences - Elasticity of Employment Duration to Benefit Level

	DD-RKD		DDD-RKD	
	Elast.	Bandwidth	Elast.	Bandwidth
First kink				
Employment Duration	0.48	75	0.52	60
	(.04)		(.07)	
Predicted Emp. Duration	0.03	75	-0.03	60
	(.01)		(.02)	
Second kink				
Employment Duration	0.46	125	0.30	125
	(.07)		(.09)	
Predicted Emp. Duration	0.10	125	0.13	125
	(.02)		(.03)	

Note: The table displays elasticities estimates using a RKD in double and triple differences. Diiff-in-diff RKD results are based on the comparison of the slope change estimate at actual policy kinks with the estimated slope change three years before, when no actual policy kink was in place. Results in triple differences are given by the difference between the DD-RKD and the estimates from implementing the same DD-RKD in a point which had no actual policy kink in any year. Both strategies consider the policy kink of the years 2010 and 2012, and use as placebo kinks the same points on data in years 2007, and 2009. It is necessary to use a three years gap and drop some years in order to have a minimum bandwidth range around the kink points. Bandwidths are chosen as close as possible to the preferred choices for the standard RKD analysis, at each kink. Standard errors are displayed in parenthesis and clustered at the firm level.

Table 4: Elasticity of Total Employment Duration Over Multiple Spells

	Elast.	s.e.	t-stat	Perm. Test CV [min,max] Elast.	Perm. Test CV [min,max] t-stat	Bandwidth
First kink						
Employment Duration (*multiple spells*)	0.31	(.03)	11.6	[-0.14,0.17]	[-10.79,6.16]	75
Second kink						
Employment Duration (*multiple spells*)	0.37	(.03)	12.0	[-0.17,0.1]	[-7.3,2.57]	140

Note: The table displays RKD elasticities estimates based on local linear regressions as in equation 14. The dependent variable is the total time which the worker spends employed, over multiple spells, within the UI schedule year. The running variable is the average monthly earnings in the year in the job which the workers was employed at the schedule introduction. The sample is composed of data from all years from 2007 to 2012. For a given bandwidth, permutation test 95% critical values are derived from a number of RKD estimates on placebo points where no policy kink is in place. Standard errors are clustered at the firm level.

Table 5: Elasticity of Employment Duration to Benefit Level - High vs. Low Labor Informality Areas

Dep. Employment Duration				Perm. Test CV[min,max]		Bandwidth
	Elast.	s.e.	t-stat	Elast.	t-stat	
First kink						
High informality areas	0.18	(.04)	4.8	[-0.19,0.12]	[-8.9,4.62]	75
Low informality areas	0.44	(.03)	13.2	[-0.12,0.21]	[-6.6,5.68]	75
Second kink						
High informality areas	0.34	(.03)	10.6	[-0.2,0.15]	[-5.94,2.25]	140
Low informality areas	0.50	(.05)	10.0	[-0.14,0.07]	[-4.5,1.59]	140

Note: The table displays estimates for the elasticity based on the estimated slope change for each variable at each of the three kinks using equation (14). The sample is split by municipalities with labor informality above and below the median, according to the 2010 Census. The sample is composed of data from all years from 2007 to 2012. For a given bandwidth, permutation test 95% critical values are derived from a number of RKD estimates on placebo points where no policy kink is in place. Standard errors are clustered at the firm level.

Table 6: Welfare Calibrations

	First Kink	Second Kink
f^{UI}	0.93	0.93
D_b	18.6	18.6
D_E	112.6	133.5
ρ	0.98	0.98
$\epsilon_{f^{UI},b}$	0	0.19
$\epsilon_{D_E,b}$	0.32	0.37
$\epsilon_{D_B,b}$	0.50	0.50
$\frac{dW}{db}$	0.12	0.09

Note: The table presents the results of a calibration exercise based on the welfare formula in (13). All statistics are computed around each of the thresholds (+-R$1), except for D_B which is recovered from Gerard and Gonzaga (2014) and $\epsilon_{D_E,b}$ which is set at the average of the range found in the literature, based on Card et al. (2015b).

A Appendix - Theoretical Model *Only For Online Publication*

A.1 Benefit Level and the Choice of Search Effort by the Unemployed

I first characterize the agent's optimal choice of search effort and then analyze how this choice reacts to variations in the level of unemployment benefits. The analysis regards the case of unemployed workers who have to choose a level of search intensity in order to find a job either in the formal or informal labor market as stated in equation (4). First-order conditions are given by: [21]

$$\psi'(s_t) = E_t(A_t) - [(1-z_t)U_t(A_t) + z_t I_t(A_t)] \tag{15}$$

$$\phi'(z_t) = (1-s_t)[I_t(A_t) - U_t(A_t)] \tag{16}$$

The optimal level of search intensity either in the formal (s_t) or informal (z_t) labor market is simply the one where the marginal cost of search equals the net gain from finding a new job. In the formal labor market, such gains are given by the difference between the value of a new formal job $E_t(A_t)$ and the value of failing to find such job which is a weighted sum of the value of an informal job I_t and unemployment $U_t(A_t)$. In the informal labor market, the gains are given by the value difference of a new informal job $I_t(A_t)$ and unemployment $U_t(A_t)$.

Liquidity-to-moral hazard and benefit level At this point, it is possible to approach the question of how a small change in the benefit level affects the incentives to search. The goal is decomposing this response into a liquidity ($\frac{\partial s_t}{\partial a_t}$) and moral hazard component ($\frac{\partial s_t}{\partial w_t}$). From implicitly deriving first-order condition (15) with respect to b_t, w_t and a_t we have:

[21] Here we adopt the so-called "first-order approach" and assume $U_t(A_t)$ to be concave as in Chetty (2008), which shows that for plausible parameters non-concavity never arises.

$$\frac{\partial s_t}{\partial b_t}\psi''(s_t) = -(1-z_t)u'(c_t^U) - z_t u'(c_t^I) - \frac{\partial z_t}{\partial b_t}(I_t - U_t) \tag{17}$$
$$\frac{\partial s_t}{\partial w_t}\psi''(s_t) = v'(c_t^E) - \frac{\partial z_t}{\partial w_t}(I_t - U_t) \tag{18}$$
$$\frac{\partial s_t}{\partial a_t}\psi''(s_t) = v'(c_t^E) - (1-z_t)u'(c_t^U) - z_t u'(c_t^I) - \frac{\partial z_t}{\partial a_t}(I_t - U_t) \tag{19}$$

where a_t is an unconditional provision of liquidity relaxing the budget constrain, and c_t^E, c_t^I and c_t^U are the worker's consumption level during formal re-employment, informal re-employment and unemployment.

Implicitly deriving first order conditions (15) and (16) with respect to z_t and s_t gives:

$$\frac{\partial s_t}{\partial z_t}\psi''(s_t) = -(I_t - U_t) \tag{20}$$
$$\frac{\partial z_t}{\partial s_t}\phi''(z_t) = -(I_t - U_t) \tag{21}$$

And provides a relationship between optimal search effort in the formal and informal labor market:

$$\partial z_t = \partial s_t \sqrt{\frac{\psi''(s_t)}{\phi''(z_t)}} \tag{22}$$

Plugging this condition into (17),(18) and (19), and combining them yields the decomposition of benefit level effects on search:

$$\frac{\partial s_t}{\partial b_t} = \frac{\partial s_t}{\partial a_t} - \frac{\partial s_t}{\partial w_t} < 0 \tag{23}$$

This show that the core intuition provided by Chetty (2008) also holds in this generalized model in which job duration is endogenous and informal job opportunity are present. It highlights that the effect of UI benefits on (formal) search intensity is a mix between a moral hazard component ($\frac{\partial s_t}{\partial w_t}$) and a liquidity effect ($\frac{\partial s_t}{\partial a_t}$). The moral hazard regards the fact that unemployment benefits distort the payoff from leaving unemployment because as soon as the worker finds a new job, his benefits are ceased. Therefore, it directly decreases the net benefits of search which are given by $(w_t - b_t)$ and characterizes a substitution effect.[22] The liquidity effect, on the other hand, has to do with the ability the agent has to smooth consumption across states. It means that when workers are liquidity constrained, they search more intensely than they would if credit markets were complete. Once you provide these workers with UI benefits, they decrease their search intensity because now they are less liquidity constrained and thus can better smooth consumption across states.

T periods Liquidity-to-moral hazard decomposition This intuition can be generalized to decompose the effect of a benefit level increase over B periods. Let $x \in \{b, w, a\}$, $s \in \{0, 1, ..., T-1\}$, $\frac{\partial s_t}{\partial x}|s = \sum_{i=0}^{s-1} \frac{\partial s_t}{\partial x_{t+i}}$ and $\frac{\partial z_t}{\partial x}|s = \sum_{i=0}^{s-1} \frac{\partial z_t}{\partial x_{t+i}}$.

Exploiting FOCs (15) and (16) with envelope conditions, we have:

$$\frac{\partial s_t}{\partial x}|s = \frac{1}{\psi''(s_t)} \left\{ \frac{\partial E_t}{\partial x}|s - (1-z_t)\frac{\partial U_t}{\partial x}|s - z_t \frac{\partial I_t}{\partial x}|s - \frac{\partial z_t}{\partial x}|s(I_t - U_t) \right\} \quad (24)$$

$$\frac{\partial z_t}{\partial x}|s = \frac{1}{\phi''(z_t)} \left\{ (1-s_t)\frac{\partial I_t}{\partial x}|s - (1-s_t)\frac{\partial U_t}{\partial x}|s - \frac{\partial s_t}{\partial x}|s(I_t - U_t) \right\} \quad (25)$$

Notice that an unconditional liquidity (or wealth) increase is equivalent to increasing wages and benefit level because these imply in a liquidity gain covering all the possible states in which the worker may fall: formal employment, informal employment or unemployment. This implies:

$$\frac{\partial E_0}{\partial a}|_B = \frac{\partial E_0}{\partial w}|_B$$
$$\frac{\partial U_0}{\partial a}|_B = \frac{\partial U_0}{\partial w}|_B + \frac{\partial U_0}{\partial b}|_B$$
$$\frac{\partial I_0}{\partial a}|_B = \frac{\partial I_0}{\partial w}|_B + \frac{\partial I_0}{\partial b}|_B$$

From equation (24) and (25), combining cases where $x = \{b, w, a\}$, it is

[22] Technically, it also embodies a wealth effect as a variation in the net value of finding a job also affects life time wealth. However, in the context of unemployment benefits, such effect is arguably very low since the total amount of benefits are only a very small fraction of lifetime earnings.

possible to find:

$$\psi''(s_t)\frac{\partial s_t}{\partial b}|_B = \psi''(s_t)\frac{\partial s_t}{\partial a}|_B - \psi''(s_t)\frac{\partial s_t}{\partial w}|_B + \left[\frac{\partial z_t}{\partial a}|_B - \frac{\partial z_t}{\partial w}|_B - \frac{\partial z_t}{\partial b}|_B\right](I_t - U_t) \tag{26}$$

$$\phi''(z_t)\frac{\partial z_t}{\partial b}|_B = \phi''(z_t)\frac{\partial z_t}{\partial a}|_B - \phi''(z_t)\frac{\partial z_t}{\partial w}|_B + \left[\frac{\partial s_t}{\partial a}|_B - \frac{\partial s_t}{\partial w}|_B - \frac{\partial s_t}{\partial b}|_B\right](I_t - U_t) \tag{27}$$

Combining these conditions, it must be that:

$$\frac{\partial s_t}{\partial b} = \frac{\partial s_t}{\partial a}|_B - \frac{\partial s_t}{\partial w}|_B \tag{28}$$

These shows that benefit level increases over B periods can also be decomposed into liquidity and moral hazard effects.

A.2 Benefit Level and the Choice of Work Effort by the Employed

Here I approach the problem of how variations in benefit level affect the the choice of effort by the employed. Equation (2) states the problem faced by the employed worker. First, the worker decides whether or not quit his job. He quits whenever the value of the quitting state happens to be higher than the value of remain employed, given an optimal effort level. Thus:

$$x_t = 1 \iff Q_t > e_t^* V_t(A_t) + (1 - e_t^*)U_t - c(e_t^*) \tag{29}$$

$$x_t = 0 \iff Q_t \leq e_t^* V_t(A_t) + (1 - e_t^*)U_t - c(e_t^*) \tag{30}$$

In turn, the optimal effort level is characterized by the following first-order condition:[23]

[23] As for the problem of the unemployed worker, I take the "first-order" approach and assume $V_t(A_t)$ to be concave.

$$c'(e_t) = V_t(A_t) - U_t(A_t) \tag{31}$$

It shows that employed workers decide their level of effort by adjusting the marginal cost of effort to keep his job (left-hand-side of the equation) to the net gain of keeping their jobs, which is given by the difference between the value of employment and unemployment.

A.3 The Welfare Formula in the T Periods Model

$$\max_{b,\tau} J_0^V(b,\tau) = (1-x_0)[(1-e_0)J_0^U(b,\tau) + e_0 V_0(b,\tau) - c(e_0)] + x_0 Q_0 \tag{32}$$

$$s.t. f^{UI} D_B b = D_E \tau \tag{33}$$

Deriving with respect to benefit level:

$$\frac{dJ_0}{db} = (1-x_0)e_0 \frac{\partial V_0}{\partial b} - \frac{d\tau}{db}\left[(1-x_0)e_0 \frac{\partial V_0}{\partial w}\right] \tag{34}$$

Notice that $\frac{\partial J_0^U}{\partial b} = 0$ because workers laid-off in the first period are not eligible for UI and $\frac{\partial U_0}{\partial \tau} = 0$ because workers no longer collect UI taxes upon reemployment. Let $E_{0,T-1} v'(c_t^V)$ denote the unconditional average marginal utility while in the initial employed stage and D_E the respective expected duration of this spell. Then:

$$E_{0,T-1} v'(c_t^V) = \frac{1}{D_E}\left[(1-x_0)e_0 \frac{\partial V_0}{\partial w}\right] \tag{35}$$

Also:

$$(1-x_0)e_0 \frac{\partial V_0}{\partial b} = \sum_{i=k}^{T-1} l(i) \frac{\partial J_i^U}{\partial B_i} \tag{36}$$

where $\left[\Pi_{j=0}^{i-1}(1-x_j)e_j\right](1-x_i)(1-e_i)$ is the unconditional probability that the worker is laid-off at period i and $\frac{\partial J_i^U}{\partial B_i}$ is the effect of raising UI benefit level for workers entering unemployment at period i for B periods. It means that for the worker employed at the initial stage of the model, an increase in benefit level raises the value of his job by the effect it has on unemployment value weighted by the probability that he enters this state and is eligible for UI (which happens if his job lasts at least for k periods). In other words, higher benefit level increase the value of employment at $t = 0$ by raising the value of subsequent unemployment after minimum eligibility requirement, i.e., from period k.

Then, it implies:

$$\frac{dJ_0}{db} = \sum_{i=k}^{T-1}\left\{l(i)\frac{\partial J_i^U}{\partial B_i}\right\} - \frac{d\tau}{db}(D_E)E_{0,T-1}v'(c_t^V) \tag{37}$$

Normalize welfare by the gain from raising wages by \$ 1:

$$\frac{dJ_0}{dw} = (1-x_0)e_0\frac{\partial V_0}{\partial w} = (D_E)E_{0,T-1}v'(c_t^V) \tag{38}$$

Therefore:

$$\frac{dW}{db} = \frac{\frac{dJ_0}{db}}{\frac{dJ_0}{dw}} = \frac{\sum_{i=k}^{T-1}\left\{l(i)\frac{\partial J_i^U}{\partial B_i}\right\}}{(D_E)E_{0,T-1}v'(c_t^V)} - \frac{d\tau}{db} \tag{39}$$

For workers becoming unemployed at period i, it is true that:

$$\psi''(s_i)\frac{\partial s_i}{\partial b_i} = \cancelto{0}{\frac{\partial E_i}{\partial b_i}} - (1-z_t)\frac{\partial U_i}{\partial b_i} - z_t\frac{\partial I_i}{\partial b_i} - \frac{\partial z_i}{\partial b_i}(I_i - U_i) = \frac{1}{1-s_i}\frac{\partial J_i^U}{\partial b_i} - \frac{\partial z_i}{\partial b_i}(I_i - U_i) \tag{40}$$

Combining this with the relationship between optimal formal and informal search in (22), it follows that:

$$-m(i)\frac{\partial s_i}{\partial b_i} = \left\{(1-z_t)\frac{\partial U_i}{\partial b_i} + z_t\frac{\partial I_i}{\partial b_i}\right\} = \frac{1}{1-s_i}\frac{\partial J_i^U}{\partial b_i} \tag{41}$$

where $m(i) = \psi''(s_i) + \sqrt{\frac{\psi''(s_i)}{\phi''(z_i)}}(I_i - U_i)$
From the same reasoning, this formula can be extended to the case where benefit level is increased over B periods:

$$-m(i)\frac{\partial s_i}{\partial B_i} = \frac{1}{1-s_i}\frac{\partial J_i^U}{\partial B_i} \tag{42}$$

Then, it follows that:

$$\frac{dW}{db} = \frac{\sum_{i=k}^{T-1}\left\{l(i)\left(-m(i)(1-s_i)\frac{\partial s_i}{\partial B_i}\right)\right\}}{(D_E)E_{0,T-1}v'(c_t^V)} - \frac{d\tau}{db} \tag{43}$$

Now since:

$$\frac{\partial s_i}{\partial B_i} = \frac{\partial s_i}{\partial A_i}|B - \frac{\partial s_i}{\partial W_i}|B \tag{44}$$

We have:

$$\frac{dW}{db} = \frac{\sum_{i=k}^{T-1}\left\{l(i)\left[-m(i)(1-s_i)\frac{\partial s_i}{\partial W_i}|B\,(-\rho_i - 1)\right]\right\}}{(D_E)E_{0,T-1}v'(c_t^V)} - \frac{d\tau}{db} \tag{45}$$

where $\rho_i = -\frac{\frac{\partial s_i}{\partial A_i}|B}{\frac{\partial s_i}{\partial W_i}|B}$ is the liquidity to moral hazard ratio at period i.

Let $E_{i,i+B-1}v'(c_t^E)$ be the average marginal utility upon formal reemployment over the first B periods after becoming unemployed at $t = i$, and notice that:

$$E_{i,i+B-1}v'(c_t^E) = \frac{1}{B-D_B}\left(s_i\frac{\partial E_i}{\partial W_i}|_B + (1-s_i)(1-z_i)\frac{\partial U_i}{\partial W_i}|_B + (1-s_i)z_i\frac{\partial I_i}{\partial W_i}|_B\right) \tag{46}$$

From implicitly deriving FOC (15) with respect to W_i, it follows that:

$$\psi''(s_i)\frac{\partial s_i}{\partial W_i}|B = \left\{\frac{\partial E_i}{\partial W_i}|_B - \left((1-z_i)\frac{\partial U_i}{\partial W_i}|_B + z_i\frac{\partial I_i}{\partial W_i}|_B\right) - \frac{\partial z_i}{\partial W_i}(I_i - U_i)\right\} \tag{47}$$

Using again condition (22), it is true that:

$$m(i)\frac{\partial s_i}{\partial W_i}|B = \frac{1}{1-s_i}\left\{\frac{\partial E_i}{\partial W_i}|_B - \left(s_i\frac{\partial E_i}{\partial W_i}|_B + (1-s_i)(1-z_i)\frac{\partial U_i}{\partial W_i}|_B + (1-s_i)z_i\frac{\partial I_i}{\partial W_i}|_B\right)\right\} \tag{48}$$

$$= \frac{1}{1-s_i}\left\{Bv'(c_i^E) - (B-D_B)E_{i,i+B-1}v'(c_t^E)\right\} \tag{49}$$

This result in $\frac{dW}{db}$ implies:

$$\frac{dW}{db} = \frac{\sum_{i=k}^{T-1}\left\{l(i)\left[\left\{Bv'(c_i^E) - (B-D_B)E_{i,i+B-1}v'(c_t^E)\right\}(-\rho_i - 1)\right]\right\}}{(D_E)E_{0,T-1}v'(c_t^V)} - \frac{d\tau}{db} \tag{50}$$

Notice that from the government budget constraint:

$$\frac{d\tau}{db} = f^{UI}\frac{D_B}{D_E}\left\{1 + \epsilon_{f^{UI},b} + \epsilon_{D_B,b} - \epsilon_{D_E,b}\right\} \tag{51}$$

As in Chetty (2008), assume that the consumption path during employment is constant since unemployment is unlikely to cause large losses on life cycle earnings. This means that $E_{i,i+B-1}v'(c_t^E) = E_{0,T-1}v'(c_t^V) = v'(c_i^E)$, $\forall i$. Using this assumption and the result on (51), it implies that:

$$\frac{dW}{db} = \frac{D_B}{D_E}\left\{\sum_{i=k}^{T-1}[l(i)(\rho_i+1)] - f^{UI}\left[1 + \epsilon_{f^{UI},b} + \epsilon_{D_B,b} - \epsilon_{D_E,b}\right]\right\} \tag{52}$$

The term $\sum_{i=k}^{T-1} l(i)(\rho_i+1)$ is the weighted average of the liquidity-to-moral hazard ratio of a worker becoming unemployed at period $i > k$. If we assume the liquidity-to-moral ratio not to depend on the period of job loss, as is implicity in Chetty (2008), it is true that $\rho_i = \rho$. Then it follows the final welfare formula:

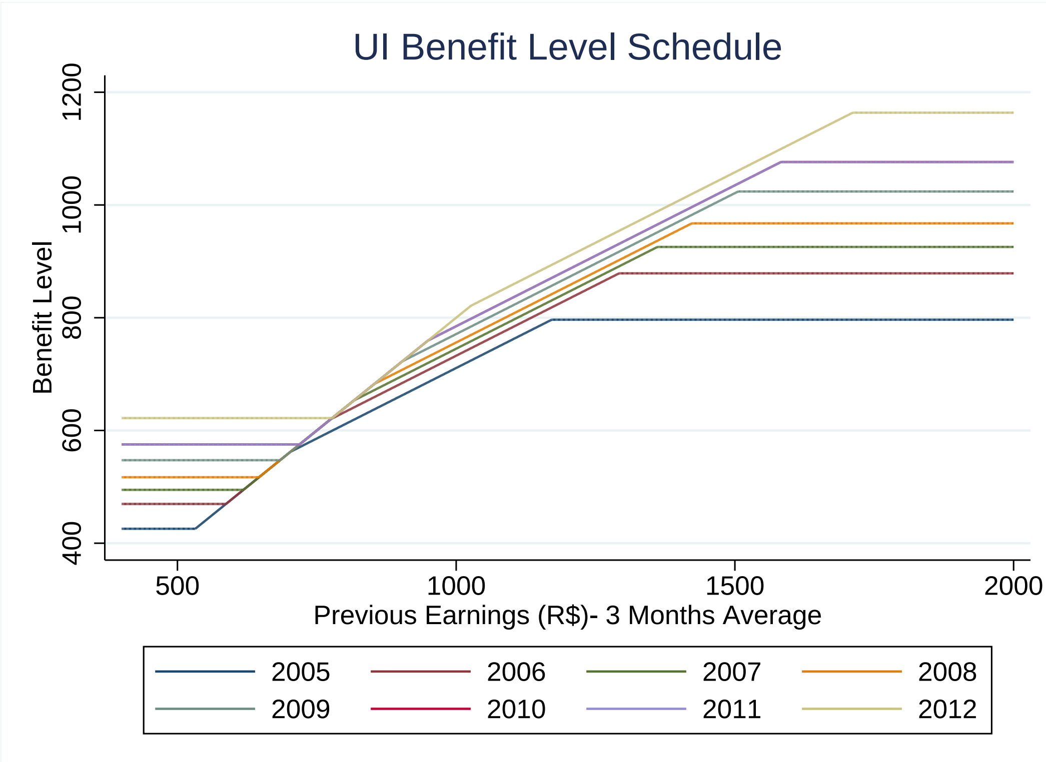
$$\frac{dW}{db} = f^{UI}\frac{D_B}{D_E}\left\{\rho - \left(\epsilon_{f^{UI},b} + \epsilon_{D_B,b} - \epsilon_{D_E,b}\right)\right\} \tag{53}$$

where $f^{UI} = \sum_{i=k}^{T-1}\left[\Pi_{j=0}^{i-1}(1-x_j)e_j\right](1-x_i)(1-e_i)$ is the share of laid-off workers eligible for UI due to MER.

B Appendix - Empirical Results *Only For Online Publication*

B.1 UI Schedule for all years

Figure 14: Benefit Level Assignment Rule - Year 2010 (2012 prices)



B.2 Summary Statistics

Table B1: Summary Statistics - Final Sample

Variable	Full Sample		Kink 1		Kink 2		Kink 3	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Monthly Earnings (R$)	1154	(668)	731	(118)	1012	(237)	1635	(711)
Potential Benefit Level (R$)	758	(200)	610	(73)	768	(141)	937	(131)
Employment Duration	38.0	(15.1)	37.3	(16.5)	39.2	(15.5)	40.7	(14.5)
Layoff	0.22	(0.42)	0.27	(0.45)	0.23	(0.42)	0.19	(0.39)
Quit	0.05	(0.23)	0.07	(0.26)	0.06	(0.23)	0.04	(0.20)
Reach Min. Eligibility Req.	0.96	(0.20)	0.94	(0.24)	0.96	(0.20)	0.97	(0.17)
Tenure at Schedule Introduction	154.9	(212.9)	114.3	(166.8)	144.1	(197.2)	195.0	(248.2)
Years of Schooling	10.5	(3.0)	10.2	(2.8)	10.4	(2.9)	11.0	(3.1)
Female	0.38	(0.49)	0.48	(0.50)	0.37	(0.48)	0.30	(0.46)
Firm Size (Branch)	575	(2995)	509	(2631)	534	(2874)	694	(3647)
Firm Size (Holding)	2404	(8875)	2007	(8305)	2214	(8382)	3035	(10092)
White Worker	0.62	(0.49)	0.56	(0.50)	0.62	(0.48)	0.66	(0.47)
Weekly Workload	42.8	(4.6)	43.0	(4.1)	42.9	(4.2)	42.5	(5.1)
Disable Worker	0.01	(0.09)	0.01	(0.09)	0.01	(0.09)	0.01	(0.10)
Observations (millions)	172.5		64.9		83.4		79.8	

Note: Summary statistics for the final sample composed of all years from 2007 to 2012. Duration variables are expressed in weeks.

B.3 Bandwidth Choice

The purpose of this section is to evaluate the performance of different bandwidth selectors. Accordingly, I evaluate the performance of the selector proposed by CCT, with and without its regularization term, and, as well as the FG rule of thumb, in its linear and quadratic forms. As discussed in the text, estimates based on these are not perfectly consistent across each other, as shown by Table B8. I provide two pieces of evidence supporting the idea that all these selectors pick bandwidths that are too narrow and that wider bandwidth choices are to be preferred in this specific application, aside from around the third kink.

The first one is based on the permutation test proposed by Ganong and Jäger (2016). The idea of the test is to estimate the slope change around as many placebo points as possible for a given bandwidth, comparing the result with the estimates at the actual policy kink. This procedure is useful to assess the robustness of results based on a given specification since one expects to find no slope change around placebo points. Of course, the assumption for this procedure to make sense is that such points are reasonable counterfactuals of the actual kink point. The authors provide strong evidence that their procedure should be the prime instrument for assessing whether a given RKD application yields enough power to detect economically meaningful results. I apply the procedure with a linear specification for a range of small bandwidths similar to those picked by the selectors around the first and second kink (between R\$2 and R\$30 of monthly earnings - Table B8) and plot the test critical values in Figures B1 and B2.[24] These results show that local linear estimates for such small bandwidths yield too many false positives on placebo points. Critical values displayed by the gray lines are remarkably large, particularly for bandwidths smaller than R\$15, which are most often picked by CCT specifications. Such a large range of critical values implies that it would not be possible to identify any reasonable elasticity, for instance, smaller than one, based on this narrow data range. Henceforth, the exercise indicates that such small bandwidths picked by any of these selectors do not yield enough power to detect moderate benefit level effects on employment duration.

The second piece of evidence favoring the choice of larger bandwidths is

[24]For the sake of conciseness, I only provide the results for the linear specifications. Using a quadratic specification yields similar results, which are available upon request.

based on a Monte Carlo simulation exercise, as suggested by CLPW. The idea is to create a data generation process (DGP) that approximates the actual data around each kink, imposes a given slope change at the threshold and assesses the performance of different specifications considering two main criteria: the root mean-squared error (RMSE) and coverage rates. Similar to CLPW, I set the data generation process (DGP) using an approximation of the data on job duration around the kink with a fifth order polynomial on each side of the threshold. The linear coefficient on the linear term to the right of the kink is held equal to zero for this approximation. Then, I set the DGP to equal the coefficients of this regression and impose a value on the linear term after the kink point to set the actual slope change. The DGP for the error term is based on the empirical distribution of the residuals in this regression. For each simulation, I set the slope change in order to have an elasticity of 0.5. I generate 100 samples of the same size as the actual data by sampling the running variables and errors with replacement.

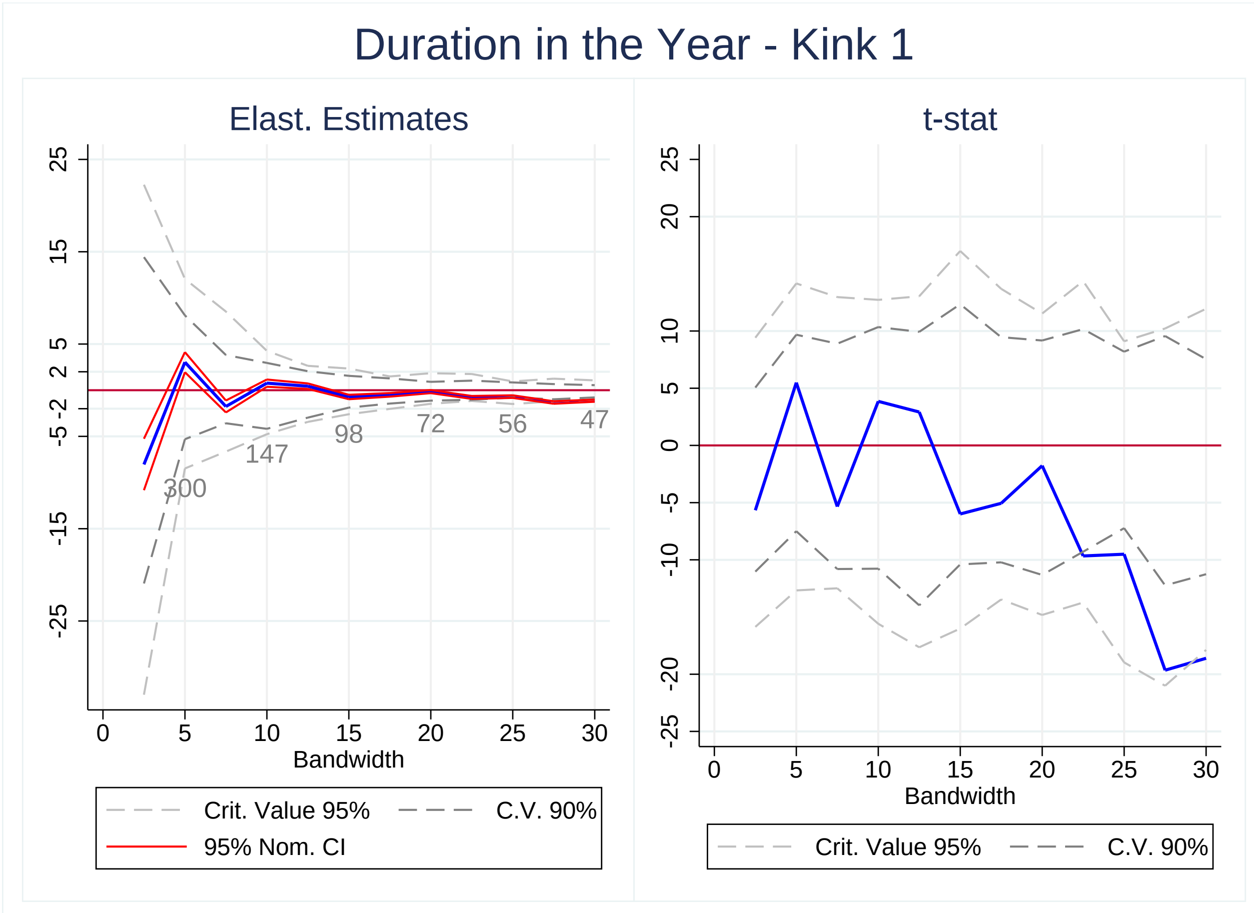
The results from these simulations for the first and second kink are shown in Tables B2 and B3. It is remarkable that RMSE with respect to the true kink value is never lower than 85%. Moreover, the coverage rates are most often well below the nominal rate (95%). Another pattern arising from this Monte Carlo study is that all the specifications seem to suffer from a strong negative bias, ranging from 0.12 to 1.38 times the actual kink size set for the simulations (0.5). Overall, these simulations suggest that these specifications perform poorly at estimating slope changes on employment duration.

I interpret the two results from above as strong evidence that such small bandwidth choices have unsatisfactory performance in this data and thus argue that the instability of the results in Table B8 is driven by the extremely small bandwidths chosen by these selectors. Such estimates do not provide much information concerning the existence of a kinked relationship between job duration and earnings or the size of the kink. In order to assess whether larger bandwidths yield better performance, I conduct again an identical Monte Carlo study to the one described above, albeit now using an array of fix bandwidth choices. I consider the widest possible range of symmetric bandwidths around all kinks. The results are displayed in Tabes B2, B3 and B4. The first thing to note is that they again provide compelling evidence that the range of small bandwidths picked by the selectors performs poorly. For the first and second kink, local linear estimates based on bandwidths smaller than R$50 always yield RMSEs that are larger than the actual slope change set in the DGP. Moreover, the null hypothesis is rejected too often with the wrong sign. On the other hand, larger bandwidths provide much lower RMSE around kink 1 and 2. Thus, I base the main regression analysis around these thresholds on bandwidths that minimize RMSE: R$75 and R$140 for kink 1 and 2, respectively. Coverage rates for kink 1 and 2 are poor for any range of bandwidths and seem to be driven by a overall negative bias, which is smaller but still substantial for larger bandwidths. Considering that the results based on these preferred specifications in Table 1 support the graphical intuition and point to a positive elasticity between employment duration and the benefit level, the negative bias indicated by the Monte Carlo study should not be a cause for much concern. If anything, the positive elasticities found in the preferred specifications constitute a lower bound of the actual effect.

Such a procedure is repeated for all other outcome variables in order to choose their preferred regression specification, as used to construct table 2.[25] The results from these further Monte Carlo studies are available upon request. As concerns estimates around the third kink, the CCT selector in a quadratic form minimizes RMSE in the simulations and performs better than any fix bandwidth. Therefore, I use this as a preferred specification around kink 3. It is remarkable that bandwidth selectors around this threshold tend to pick larger bandwidths, which may explain why their performance is much better compared to the first and second kink. A possible reasonable for this is that the global data range is much larger around kink 3, whereas around kink 1 and 2 it is limited by the vicinity between each other and the minimum wage. These selectors often pick smaller bandwidths as the global data range decreases, in a non transitive way.

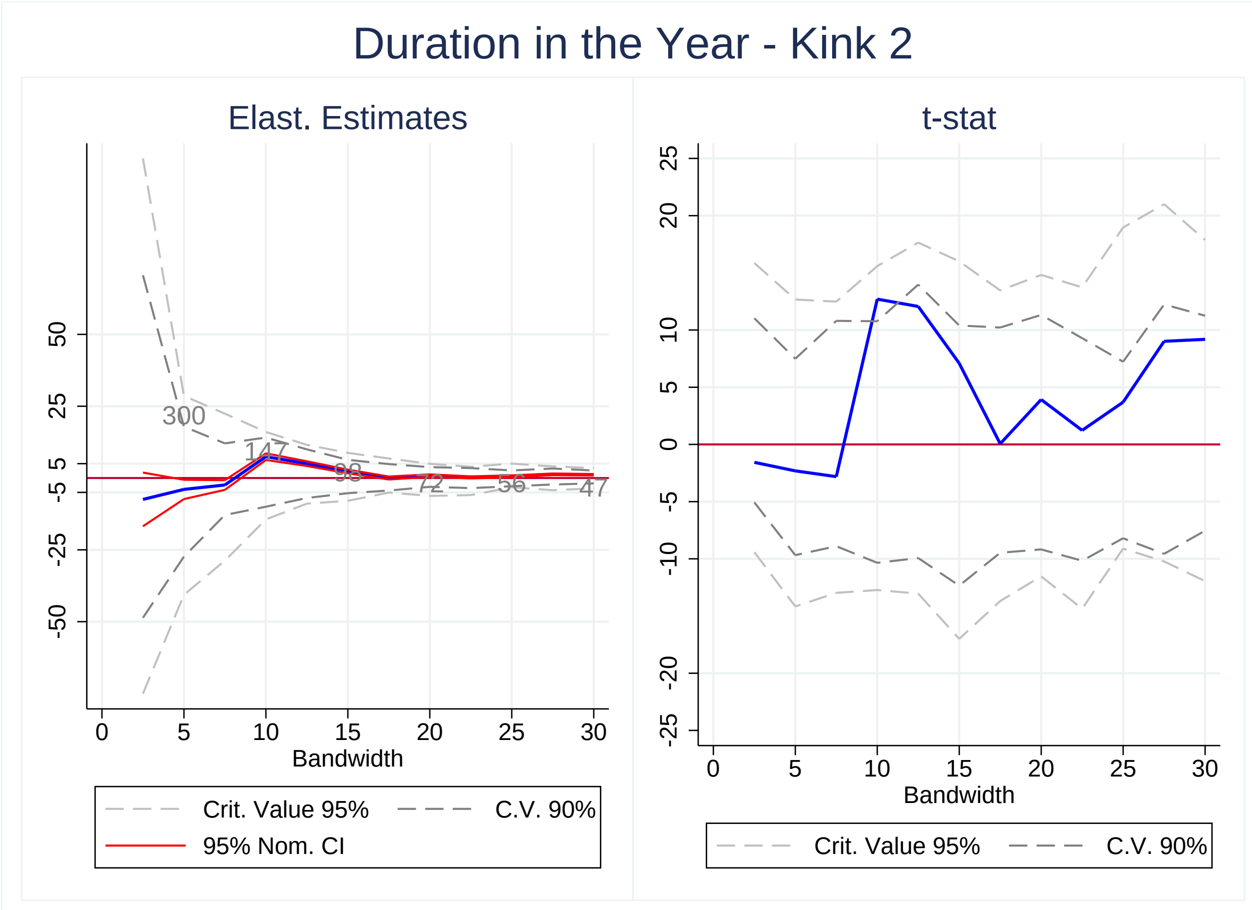
[25] Further results available upon request show that 100 simulations is enough to achieve reasonable convergence ratios. Also note that each Monte Carlo study undertaken for each variable, around each kink, is an extremely computational intense and time consuming task, especially because of the large data size.

Figure B1: RKD estimates with varying bandwidths and Permutation Test Critical Values
Small Bandwidths



The graph displays elasticities estimates and t-statistics based on a local linear regressions around kink 1 for varying bandwidths (blue line), and nominal confidence intervals for these estimates (red line). The gray lines display the critical value in a two-sided test for rejecting the null hypothesis of zero effect, based on permutation tests along as many as possible placebo points located between the minimum wage and R$4000 (2012 prices). The gray number displays the number of placebo points for this test, according to the LLR bandwidth. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks. Standard errors are clustered at the firm level.

Figure B2: RKD estimates with varying bandwidths and Permutation Test Critical Values
Small Bandwidths



The graph displays elasticities estimates and t-statistics based on a local linear regressions around kink 2 for varying bandwidths (blue line), and nominal confidence intervals for these estimates (red line). The gray lines display the critical value in a two-sided test for rejecting the null hypothesis of zero effect, based on permutation tests along as many as possible placebo points located between the minimum wage and R$4000 (2012 prices). The gray number displays the number of placebo points for this test, according to the LLR bandwidth. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks. Standard errors are clustered at the firm level.

Table B2: Monte Carlo Simulations - Bandwidth Selectors Performance - Kink 1

First Kink						
Bandwidth Selector	Average Bandwidth	RMSE over True Value	Coverage Rate	Bias over True Value	Rejects null correct sign	Rejects null wrong sign
linear						
CCT (no regularization)	7.5	1.50	0.25	-1.38	0.02	0.30
CCT - Bias Correction (no regularization)	15.2	1.13	0.62	-0.85	0.14	0.01
CCT (with regularization)	6.3	1.39	0.33	-1.25	0.10	0.13
CCT - Bias Correction (with regularization)	15.0	1.06	0.75	-0.75	0.13	0.02
FG	7.7	2.40	0.37	-1.29	0.13	0.31
FG - Bias Correction	7.7	6.53	0.80	-0.09	0.00	0.04
quadratic						
CCT (no regularization)	14.4	1.17	0.60	-0.85	0.05	0.08
CCT - Bias Correction (no regularization)	24.5	0.94	0.84	-0.33	0.01	0.00
CCT (with regularization)	13.0	1.07	0.67	-0.69	0.08	0.02
CCT - Bias Correction (with regularization)	23.1	0.95	0.92	-0.24	0.09	0.00
FG	10.3	2.56	0.89	-0.25	0.08	0.03
FG - Bias Correction	10.3	6.35	0.88	-0.28	0.05	0.04

Note: Results are based on 100 simulations. DGP is based on 5^{th} degree polynomial approximation of the actual data around this kink. The sample size for each simulation equals the actual sample size. The true elasticity is 0.5. Standard errors are clustered at the firm level.

Table B3: Monte Carlo Simulations - Bandwidth Selectors Performance - Kink 2

Second Kink						
Bandwidth Selector	Average Bandwidth	RMSE over True Value	Coverage Rate	Bias over True Value	Rejects null correct sign	Rejects null wrong sign
linear						
CCT (no regularization)	28.9	1.25	0.09	-1.19	0.06	0.45
CCT - Bias Correction (no regularization)	43.4	1.10	0.29	-0.98	0.07	0.09
CCT (with regularization)	18.8	1.17	0.14	-1.09	0.06	0.17
CCT - Bias Correction (with regularization)	41.4	1.04	0.36	-0.90	0.09	0.07
FG	9.2	1.21	0.80	-0.64	0.06	0.05
FG - Bias Correction	9.2	3.06	0.99	-0.42	0.04	0.00
quadratic						
CCT (no regularization)	36.0	1.01	0.49	-0.76	0.12	0.08
CCT - Bias Correction (no regularization)	56.9	0.85	0.77	-0.38	0.17	0.01
CCT (with regularization)	30.2	0.90	0.70	-0.57	0.13	0.01
CCT - Bias Correction (with regularization)	51.0	0.85	0.89	-0.26	0.16	0.00
FG	23.8	1.02	0.89	-0.41	0.09	0.03
FG - Bias Correction	23.8	2.41	0.93	-0.10	0.08	0.02

Note: Results are based on 100 simulations. DGP is based on 5^{th} degree polynomial approximation of the actual data around this kink. The sample size for each simulation equals the actual sample size. The true elasticity is 0.5. Standard errors are clustered at the firm level.

Table B4: Monte Carlo Simulations - Bandwidth Selectors Performance - Kink 3

Third Kink

Bandwidth Selector	Average Bandwidth	RMSE over True Value	Coverage Rate	Bias over True Value	Rejects null correct sign	Rejects null wrong sign
linear						
CCT (no regularization)	44.0	0.37	0.63	0.10	0.93	0.00
CCT - Bias Correction (no regularization)	135.1	0.30	0.90	0.00	1.00	0.00
CCT (with regularization)	40.7	0.37	0.75	0.08	0.92	0.00
CCT - Bias Correction (with regularization)	103.4	0.34	0.90	-0.01	0.93	0.00
FG	99.2	0.62	0.13	0.20	0.89	0.09
FG - Bias Correction	99.2	0.45	0.89	0.02	0.69	0.00
quadratic						
CCT (no regularization)	143.0	0.23	0.87	0.02	0.99	0.00
CCT - Bias Correction (no regularization)	220.6	0.28	0.95	-0.03	0.94	0.00
CCT (with regularization)	104.0	0.25	0.92	-0.01	1.00	0.00
CCT - Bias Correction (with regularization)	168.6	0.33	0.93	-0.03	0.93	0.00
FG	102.1	0.26	0.93	0.01	0.98	0.00
FG - Bias Correction	102.1	0.65	0.93	0.01	0.45	0.00

Note: Results are based on 100 simulations. DGP is based on 5^{th} degree polynomial approximation of the actual data around this kink. The sample size for each simulation equals the actual sample size. The true elasticity is 0.5. Standard errors are clustered at the firm level.

Table B5: Monte Carlo Simulation Results - Employment Duration

First Kink

Fix Bandwidth	RMSE over True Value	Coverage Rate	Bias over True Value	Rejects null correct sign	Rejects null wrong sign
10	1.801	0.00	-1.8	0.00	0.84
15	2.212	0.00	-2.2	0.00	1.00
20	2.385	0.00	-2.4	0.00	1.00
25	2.356	0.00	-2.4	0.00	1.00
30	2.208	0.00	-2.2	0.00	1.00
35	1.968	0.00	-2.0	0.00	1.00
40	1.679	0.00	-1.7	0.00	1.00
45	1.366	0.00	-1.4	0.00	1.00
50	1.065	0.00	-1.1	0.00	0.74
55	0.796	0.00	-0.8	1.00	0.00
60	0.569	0.00	-0.6	1.00	0.00
65	0.397	0.00	-0.4	1.00	0.00
70	0.292	0.00	-0.3	1.00	0.00
75	0.250	0.00	-0.2	1.00	0.00
80	0.261	0.00	-0.3	1.00	0.00
85	0.316	0.00	-0.3	1.00	0.00
90	0.395	0.00	-0.4	1.00	0.00
95	0.473	0.00	-0.5	1.00	0.00
100	0.524	0.00	-0.5	1.00	0.00

Note: Results are based on 100 simulations. DGP is based on 5^{th} degree polynomial approximation of the actual data around this kink. The sample size for each simulation equals the actual sample size. The true elasticity is 0.5. Standard errors are clustered at the firm level.

Table B6: Monte Carlo Simulation Results - Employment Duration

Second Kink

Fix Bandwidth	RMSE over True Value	Coverage Rate	Bias over True Value	Rejects null correct sign	Rejects null wrong sign
10	1.096	0.81	-0.8	0.09	0.00
20	1.360	0.00	-1.3	0.00	0.24
30	1.587	0.00	-1.6	0.00	0.98
40	1.682	0.00	-1.7	0.00	1.00
50	1.661	0.00	-1.7	0.00	1.00
60	1.558	0.00	-1.6	0.00	1.00
70	1.400	0.00	-1.4	0.00	1.00
80	1.223	0.00	-1.2	0.00	1.00
90	1.040	0.00	-1.0	0.00	0.29
100	0.872	0.00	-0.9	1.00	0.00
110	0.739	0.00	-0.7	1.00	0.00
120	0.644	0.00	-0.6	1.00	0.00
130	0.585	0.00	-0.6	1.00	0.00
140	0.569	0.00	-0.6	1.00	0.00
150	0.588	0.00	-0.6	1.00	0.00
160	0.629	0.00	-0.6	1.00	0.00
170	0.677	0.00	-0.7	1.00	0.00

Note: Results are based on 100 simulations. DGP is based on 5^{th} degree polynomial approximation of the actual data around this kink. The sample size for each simulation equals the actual sample size. The true elasticity is 0.5. Standard errors are clustered at the firm level.

Table B7: Monte Carlo Simulation Results - Employment Duration

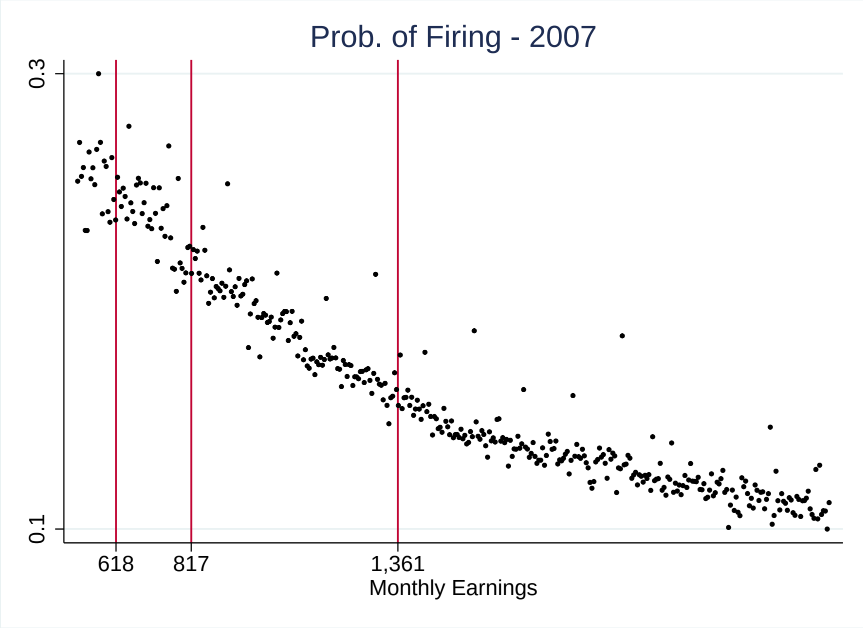
Third Kink

Fix Bandwidth	RMSE over True Value	Coverage Rate	Bias over True Value	Rejects null correct sign	Rejects null wrong sign
10	1.932	0.92	0.1	0.03	0.01
20	0.698	0.95	0.1	0.22	0.00
30	0.401	0.92	0.1	0.63	0.00
40	0.340	0.85	0.1	0.85	0.00
50	0.355	0.59	0.1	0.96	0.00
60	0.382	0.26	0.1	1.00	0.00
70	0.446	0.00	0.2	1.00	0.00
80	0.498	0.00	0.2	1.00	0.00
90	0.556	0.00	0.2	1.00	0.00
100	0.617	0.00	0.2	1.00	0.00
110	0.676	0.00	0.3	1.00	0.00
120	0.736	0.00	0.3	1.00	0.00
130	0.785	0.00	0.3	1.00	0.00
140	0.839	0.00	0.3	0.99	0.00
150	0.890	0.00	0.3	0.94	0.00
160	0.941	0.00	0.4	0.46	0.00
170	0.987	0.00	0.4	0.06	0.00
180	1.031	0.00	0.4	0.00	0.19
190	1.073	0.00	0.4	0.00	0.91
200	1.112	0.00	0.4	0.00	1.00

Note: Results are based on 100 simulations. DGP is based on 5^{th} degree polynomial approximation of the actual data around this kink. The sample size for each simulation equals the actual sample size. The true elasticity is 0.5. Standard errors are clustered at the firm level.

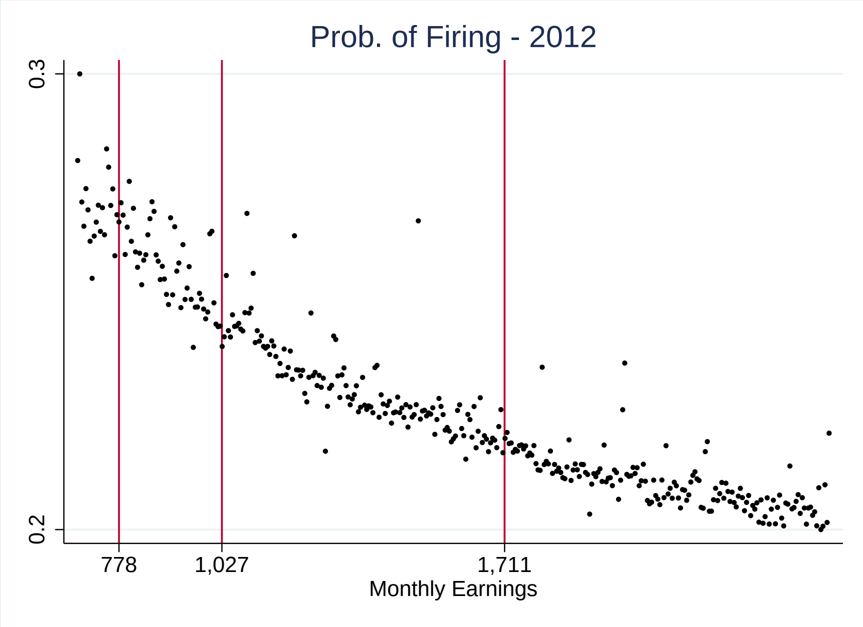
B.4 Complementary Results - Graphs by Year

Figure B3



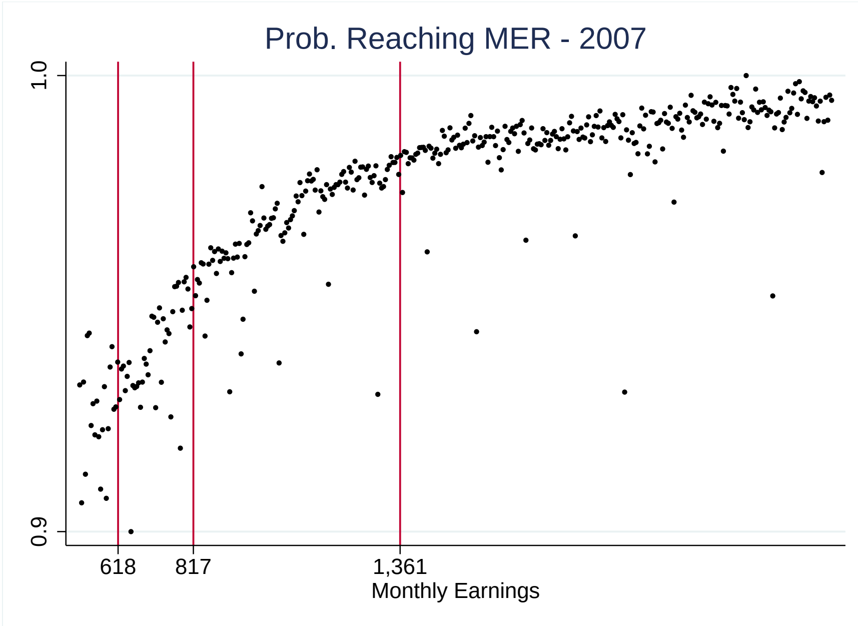
The graph displays how the prob. of layoff in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks

Figure B4



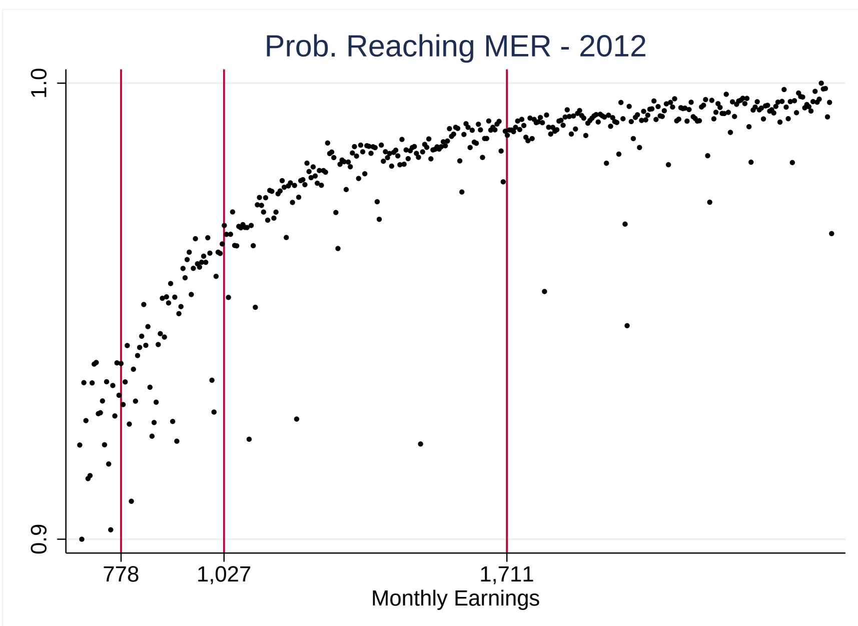
The graph displays how the prob. of layoff in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks

Figure B5



The graph displays how the prob. of reaching MER in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks

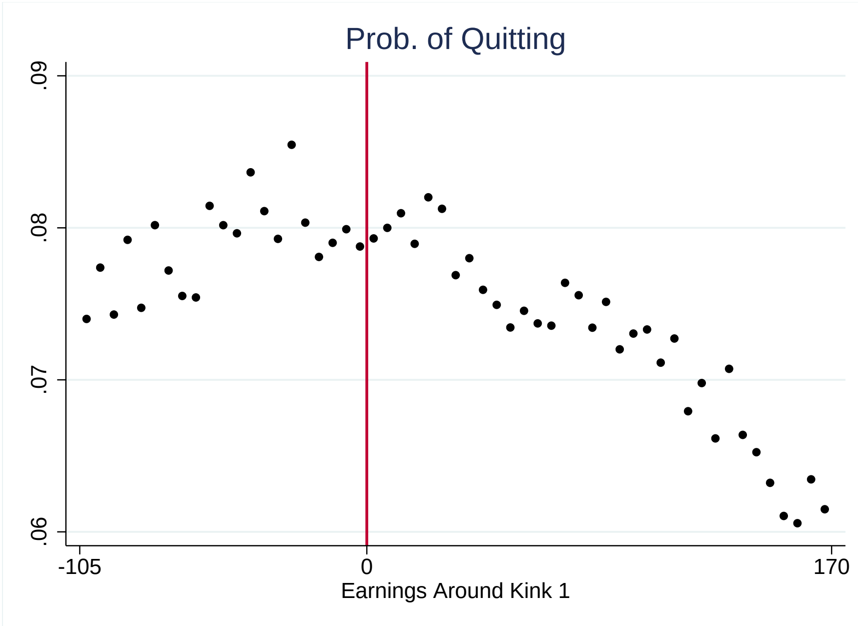
Figure B6



The graph displays how the prob. of reaching MER in the year evolves according to monthly average earnings, in 2012 prices. Duration is expressed in weeks

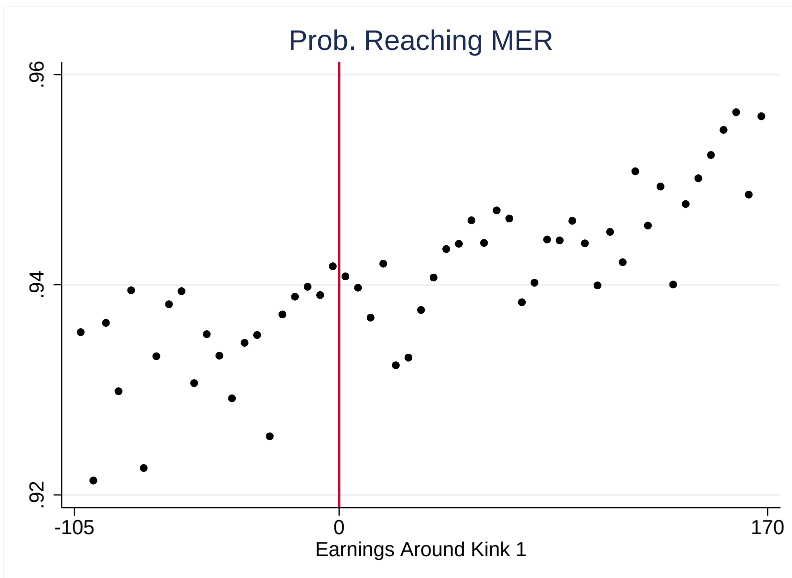
B.5 Complementary Results - Graphs Pooled Around Each Kink Point

Figure B7: Job Quits Around Kink 1



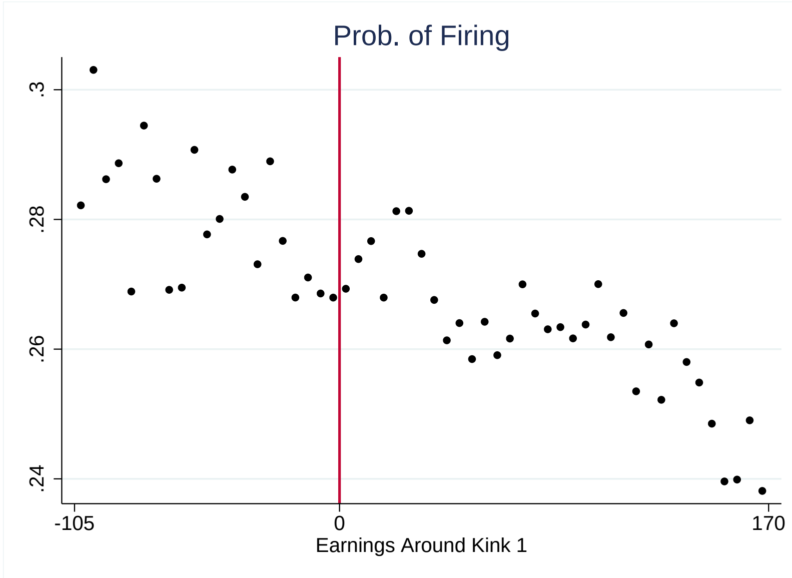
The graph displays how the prob. of quitting in the year evolves around the kink. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks.

Figure B8: Prob. Reaching MER Around Kink 1



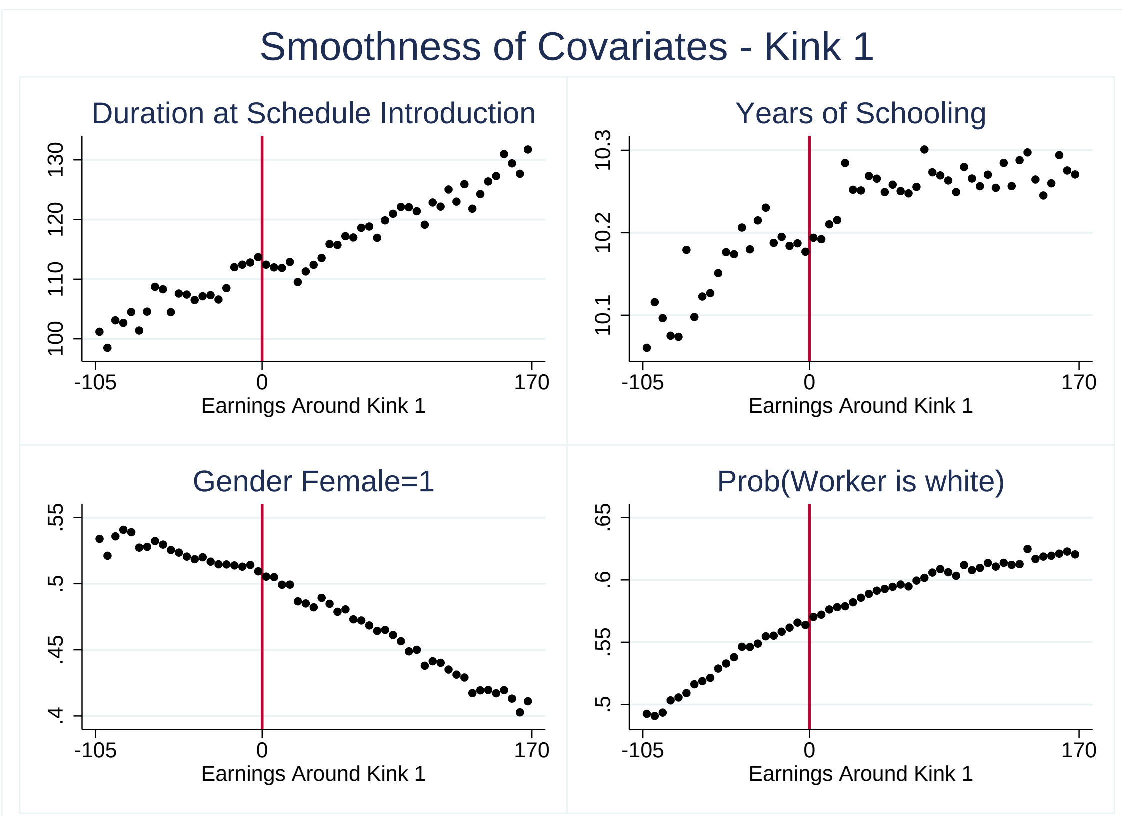
The graph displays how the prob. of reaching MER in the year evolves around the kink. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks.

Figure B9: Prob. of Layoff Around Kink 1



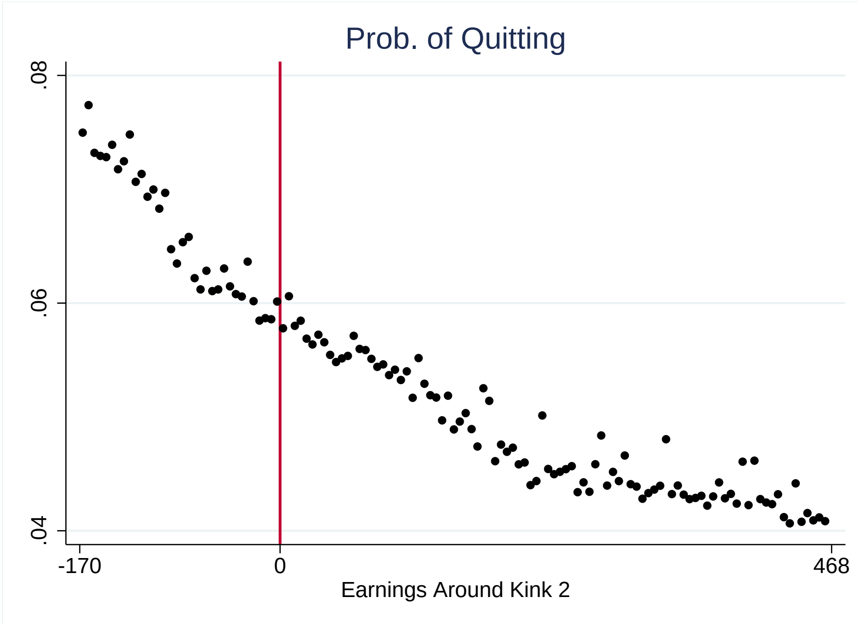
The graph displays how the prob. of layoff in the year evolves around the kink. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks.

Figure B10: Covariates Around Kink 1



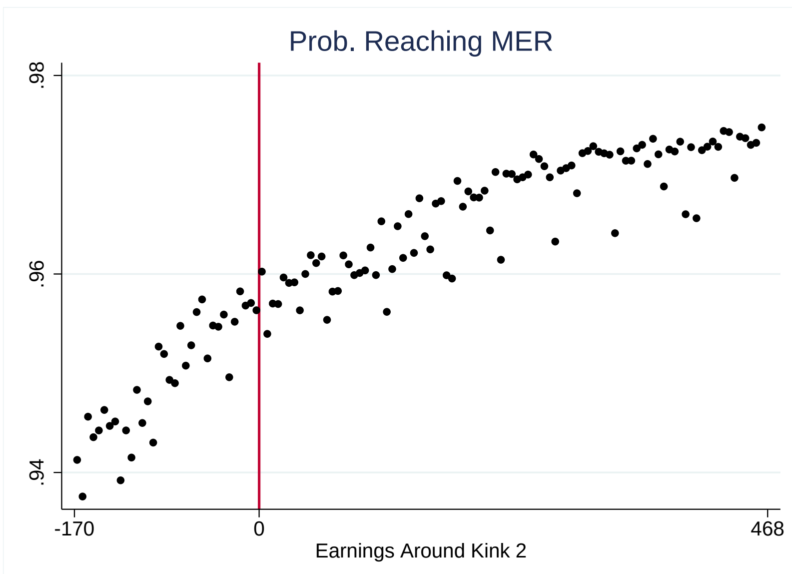
The graph displays how pre-determined covariates evolves around the kink. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks.

Figure B11: Job Quits Around Kink 2



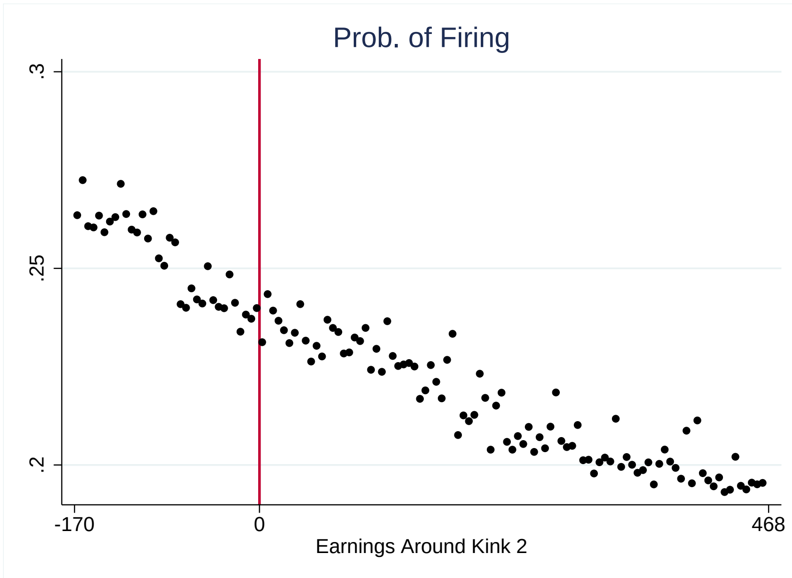
The graph displays how the prob. of quitting in the year evolves around the kink. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks.

Figure 12: Prob. Reaching MER Around Kink 2



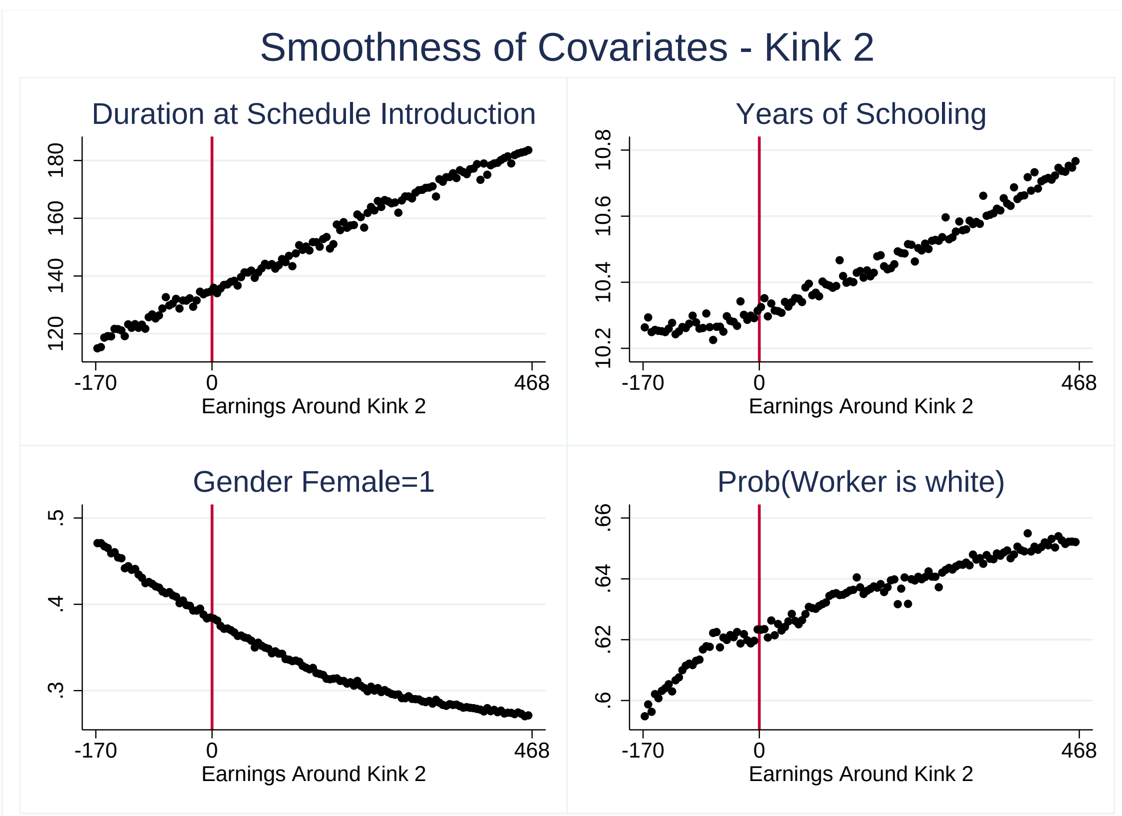
The graph displays how the prob. of reaching MER in the year evolves around the kink. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks.

Figure 13: Prob. of Layoff Around Kink 2



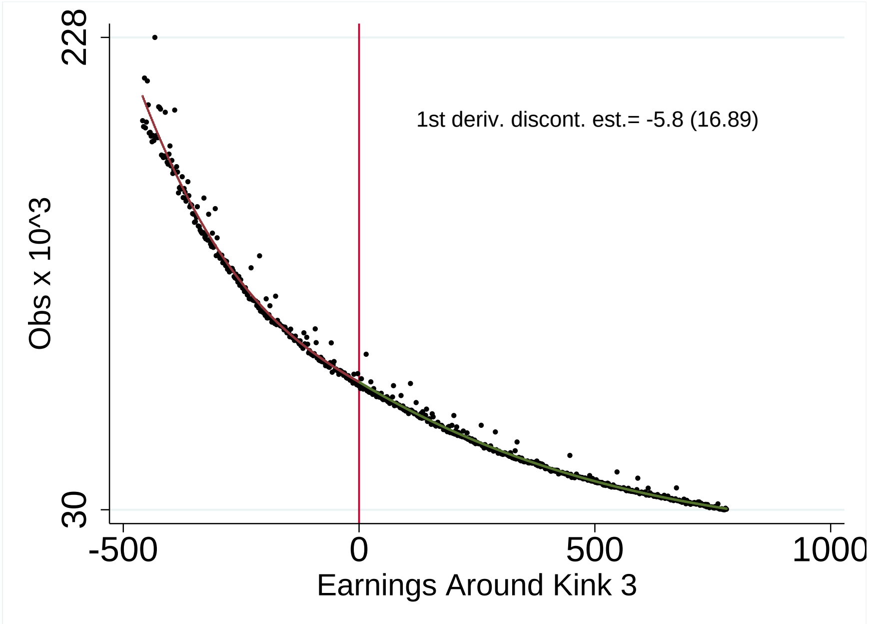
The graph displays how the prob. of layoff in the year evolves around the kink. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks.

Figure B14: Covariates Around Kink 2



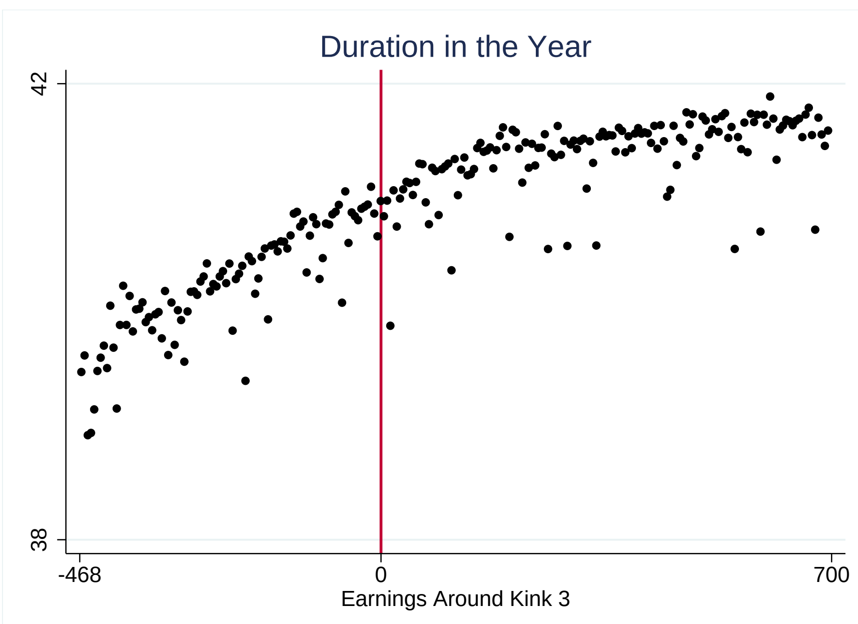
The graph displays how pre-determined covariates evolves around the kink. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks.

Figure B15: Density of Wages the Around Kink 3



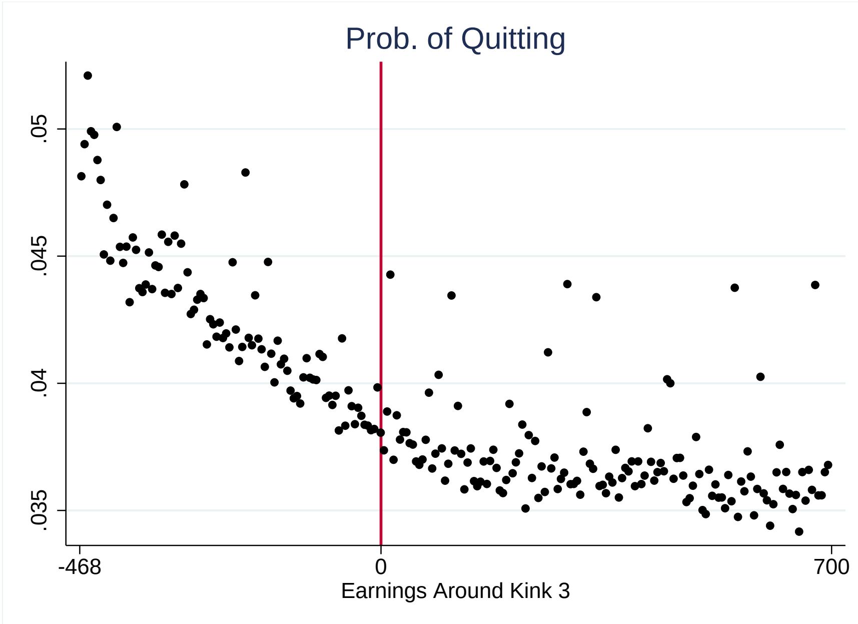
The graph displays how the density of earnings evolve around the kink. At each side of the kink, the density is approximated by the polynomial which minimizes the Akaike Criterion. The graph also displays the test statistics for the slope change of these polynomials at the kink. The sample is composed of data from all years from 2007 to 2012. See the text for details.

Figure B16: Employment Duration Around Kink 3



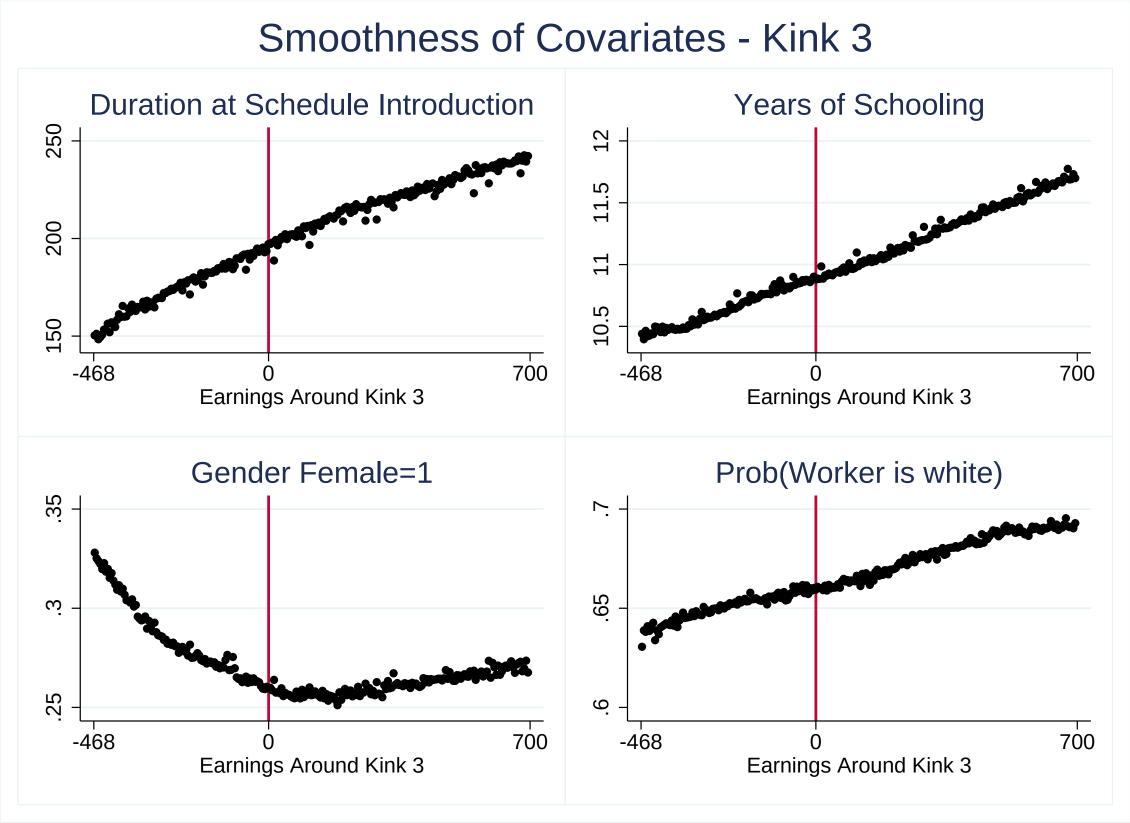
The graph displays how employment duration in the year evolve around the kink. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks

Figure B17: Job Quits Around Kink 3



The graph displays how the prob. of quitting in the year evolves around the kink. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks.

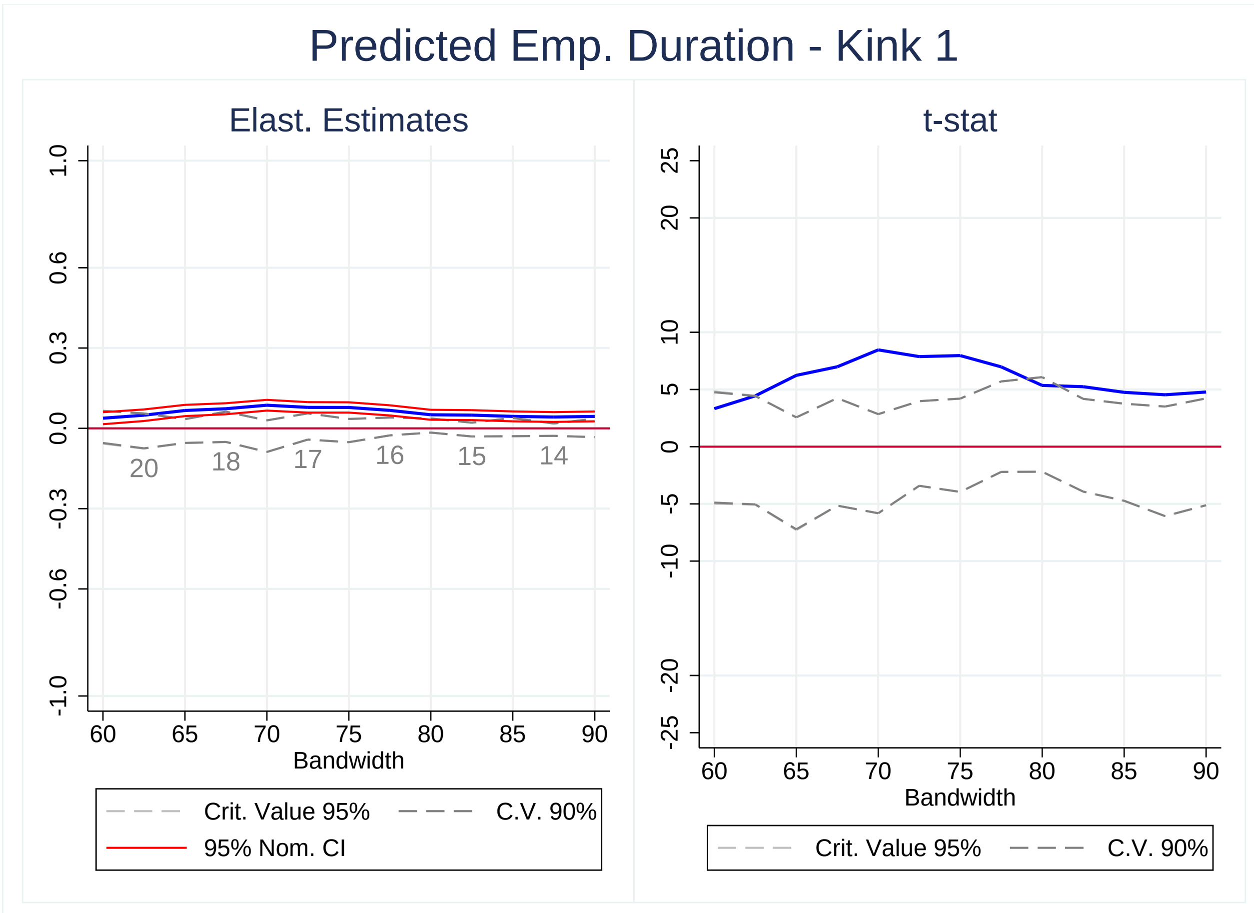
Figure B18: Covariates Around Kink 3



The graph displays how pre-determined covariates evolves around the kink. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks.

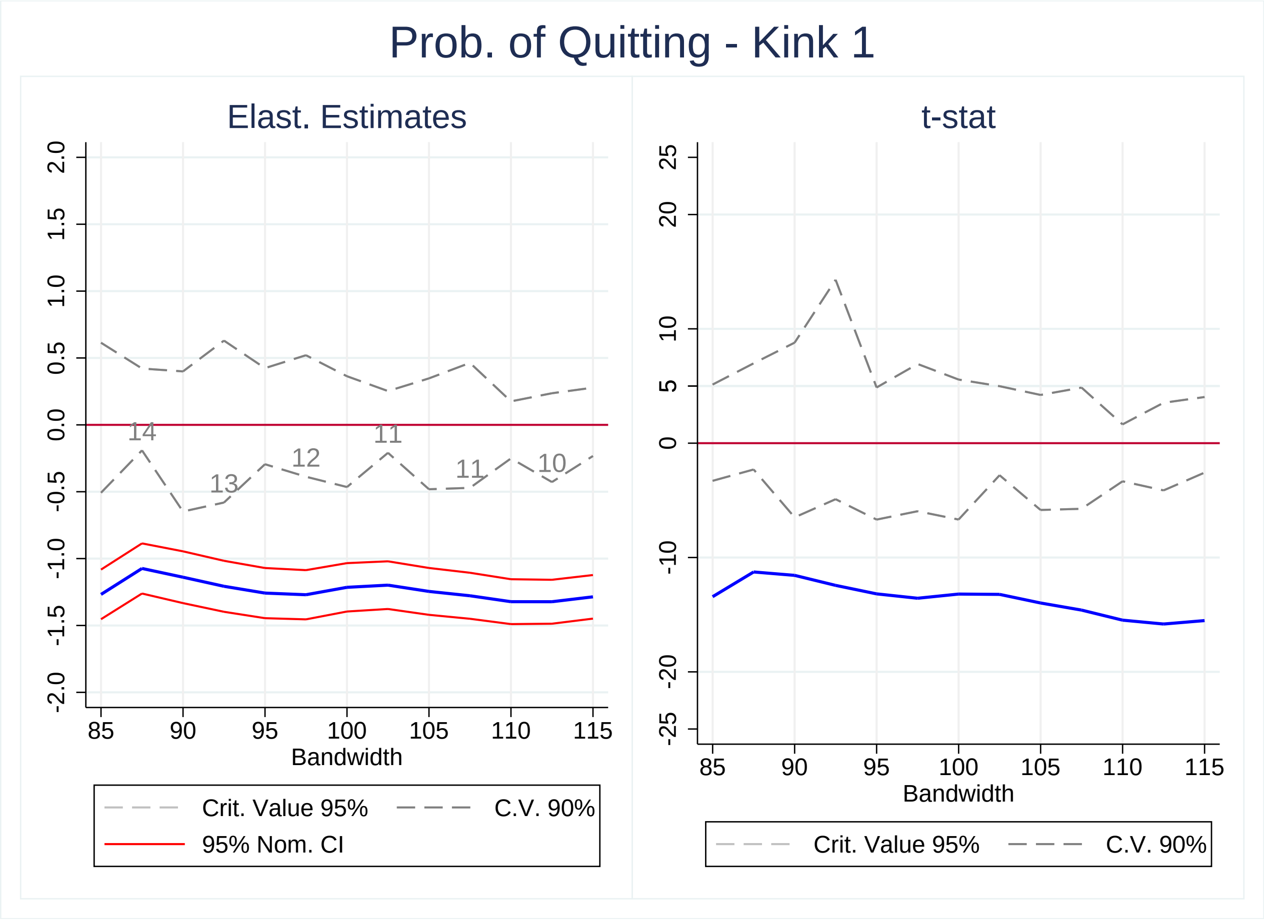
B.6 Complementary Results - Estimates

Figure B19: RKD estimates with varying bandwidths and Permutation Test Critical Values - Kink 1



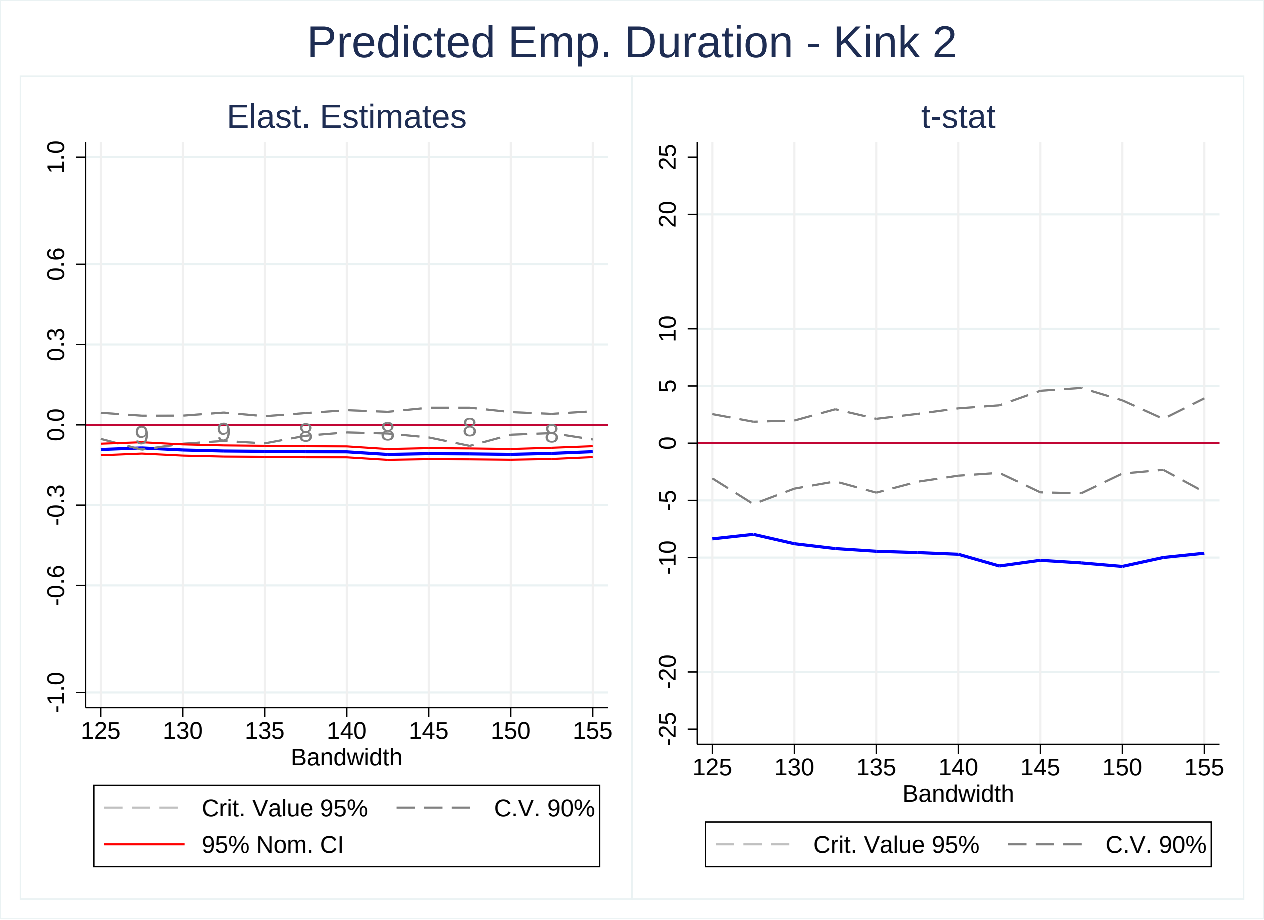
The graph displays elasticities estimates and t-statistics based on a local linear regressions around kink 1 for varying bandwidths (blue line), and nominal confidence intervals for these estimates (red line). The gray lines display the critical value in a two-sided test for rejecting the null hypothesis of zero effect, based on permutation tests along as many as possible placebo points located between the minimum wage and R$4000 (2012 prices). The gray number displays the number of placebo points for this test, according to the LLR bandwidth. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks. Standard errors are clustered at the firm level.

Figure B20: RKD estimates with varying bandwidths and Permutation Test Critical Values - Kink 1



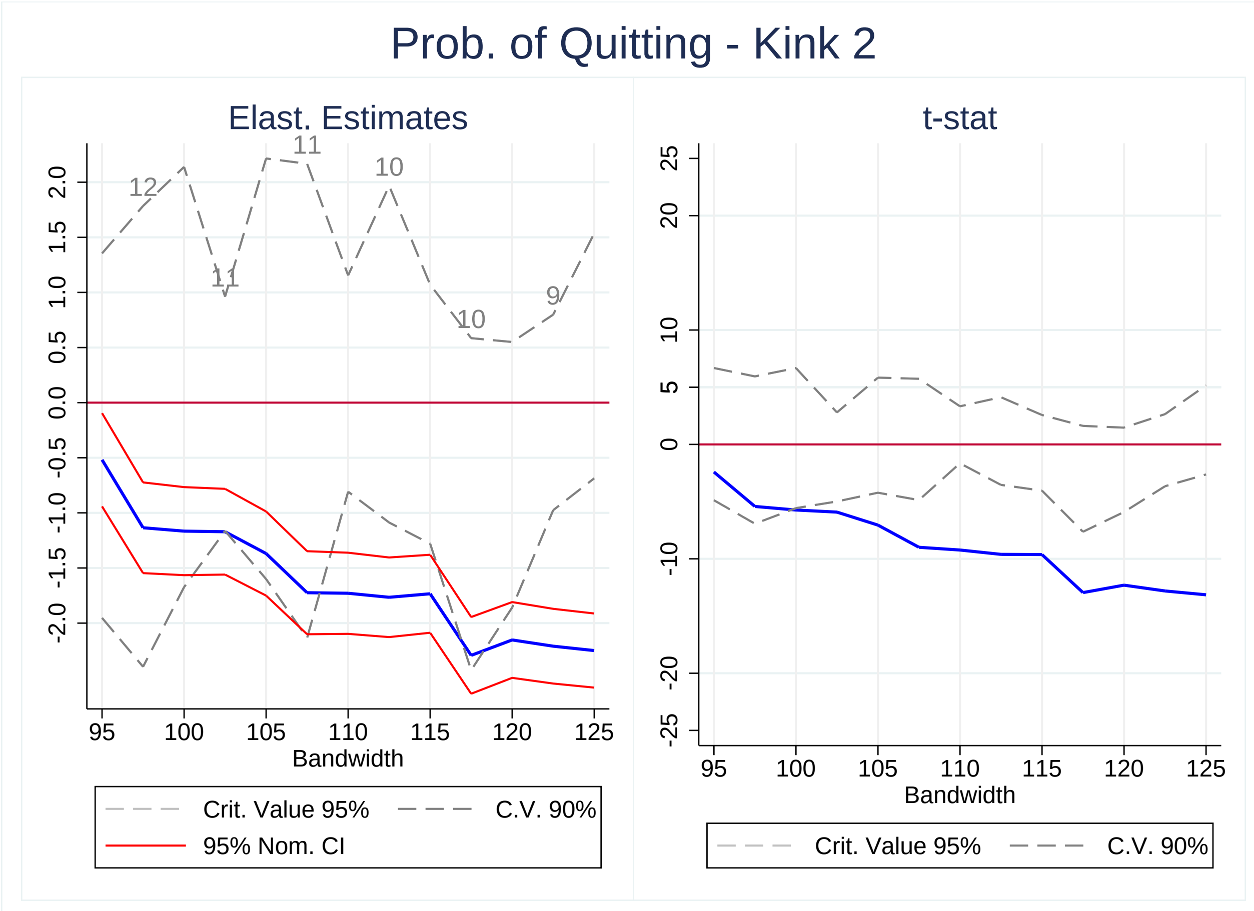
The graph displays elasticities estimates and t-statistics based on a local linear regressions around kink 1 for varying bandwidths (blue line), and nominal confidence intervals for these estimates (red line). The gray lines display the critical value in a two-sided test for rejecting the null hypothesis of zero effect, based on permutation tests along as many as possible placebo points located between the minimum wage and R$4000 (2012 prices). The gray number displays the number of placebo points for this test, according to the LLR bandwidth. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks. Standard errors are clustered at the firm level.

Figure B21: RKD estimates with varying bandwidths and Permutation Test Critical Values - Kink 2



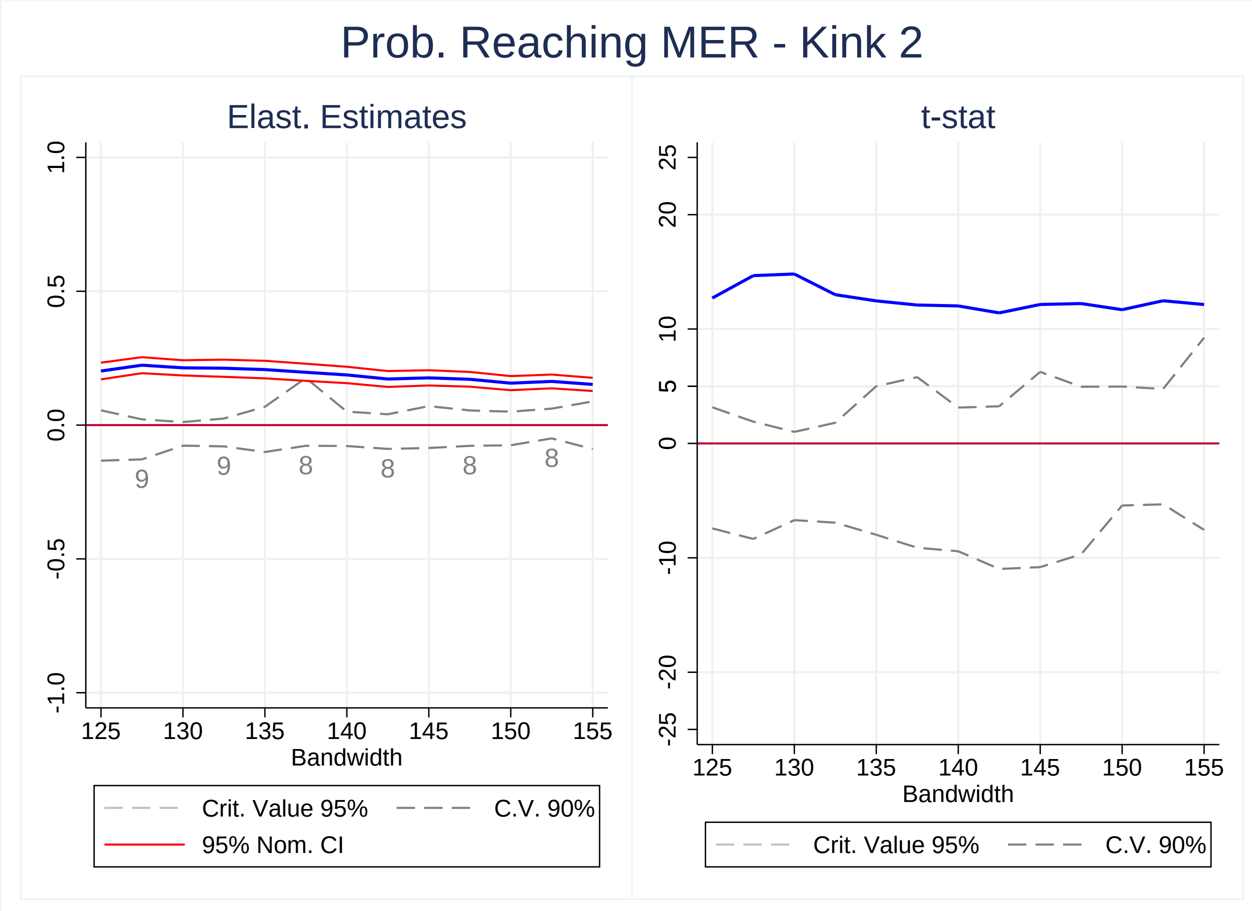
The graph displays elasticities estimates and t-statistics based on a local linear regressions around kink 1 for varying bandwidths (blue line), and nominal confidence intervals for these estimates (red line). The gray lines display the critical value in a two-sided test for rejecting the null hypothesis of zero effect, based on permutation tests along as many as possible placebo points located between the minimum wage and R$4000 (2012 prices). The gray number displays the number of placebo points for this test, according to the LLR bandwidth. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks. Standard errors are clustered at the firm level.

Figure B22: RKD estimates with varying bandwidths and Permutation Test Critical Values - Kink 2



The graph displays elasticities estimates and t-statistics based on a local linear regressions around kink 1 for varying bandwidths (blue line), and nominal confidence intervals for these estimates (red line). The gray lines display the critical value in a two-sided test for rejecting the null hypothesis of zero effect, based on permutation tests along as many as possible placebo points located between the minimum wage and R$4000 (2012 prices). The gray number displays the number of placebo points for this test, according to the LLR bandwidth. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks. Standard errors are clustered at the firm level.

Figure B23: RKD estimates with varying bandwidths and Permutation Test Critical Values - Kink 2



The graph displays elasticities estimates and t-statistics based on a local linear regressions around kink 1 for varying bandwidths (blue line), and nominal confidence intervals for these estimates (red line). The gray lines display the critical value in a two-sided test for rejecting the null hypothesis of zero effect, based on permutation tests along as many as possible placebo points located between the minimum wage and R$4000 (2012 prices). The gray number displays the number of placebo points for this test, according to the LLR bandwidth. The sample is composed of data from all years from 2007 to 2012. Duration is expressed in weeks. Standard errors are clustered at the firm level.

Table B8: Elasticity of Employment Duration to Benefit Level - Bandwidth Selectors

Bandwidth Selector	b.w.	Estimated Elasticity	s.e.	Robust C.I.
First Kink				
FG linear	24(24)	-0.83	(.03)	[1.52,1.98]
CCT linear, no regularization	2(8)	1.94	(.88)	[1.12,4.87]
CCT linear	2(8)	1.55	(.9)	[0.68,4.5]
FG quadratic	51(51)	-2.12	(.04)	[-0.26,0.1]
CCT quadratic, no regularization	8(22)	1.08	(.58)	[0.39,2.75]
CCT quadratic	7(21)	0.69	(.67)	[-0.08,2.6]
Second Kink				
FG linear	30(30)	1.03	(.07)	[0.44,1.61]
CCT linear, no regularization	6(26)	-5.71	(.81)	[-6.38,-3.06]
CCT linear	6(26)	-6.92	(.82)	[-7.6,-4.25]
FG quadratic	132(132)	-0.43	(.03)	[-0.27,0.04]
CCT quadratic, no regularization	17(35)	12.49	(.73)	[10.26,13.44]
CCT quadratic	18(35)	11.27	(.66)	[9.24,12.22]
Third Kink				
FG linear	151(151)	-0.08	(.01)	[-0.16,-0.02]
CCT linear, no regularization	44(109)	-0.48	(.06)	[-0.68,-0.4]
CCT linear	26(99)	0.07	(.13)	[-0.34,0.18]
FG quadratic	283(283)	-0.13	(.01)	[-0.52,-0.38]
CCT quadratic, no regularization	62(148)	-1.04	(.14)	[-1.44,-0.87]
CCT quadratic	59(144)	-0.07	(.15)	[-0.49,0.11]

Note: The table displays estimates for the elasticity based on the estimated slope change for each variable at each of the three kinks using equation (14). CCT robust confidence intervals are displayed in the right column.

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